Scott H.

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

An independent reader-supported journal addressing the philosophical, historical and sociological problems of the natural sciences, i.e., the physical, biological and formal (mathematical and logical) disciplines.

Editorial priorities: To demonstrate the usefulness of the Marxist world view in the practice of science. To help develop further the principles of dialectical materialism and Marxist theory of knowledge in their interaction with advancing natural science. To help deal with the social problems that scientists face in their professional activities.

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Instructions to Contributors: Contributions should be clearly related to the editorial priorities stated above. Though *Science and Nature* adheres generally to a mainstream approach to the philosophical problems of natural science based on the writings of Frederick Engels, contributions will be considered that seek to correct, develop further or dispute this orientation.

For future issues, submissions will be considered in these categories: 1) Articles or essay reviews, normally not to exceed 5,000 words; 2) Book reviews (up to 1200 words); 3) Communications (relating to previously published matter), up to 1200 words; and 4) Letters to the editor, usually limited to 300 words. Acceptance of unsolicited contributions will depend on a complex of considerations and may involve editing for clarity or length.

Manuscripts must be neatly and legibly typed or printed, double spaced on 8 1/2x11 inch paper, 10 or 12 characters per inch, with a margin of at least 1 1/2 inches on the left side. The word count must include notes, references, and captions (with an equivalent word count allowed for space to be occupied by figures, which should be line drawings). Manuscripts produced by word processor should be transmitted via modem if possible.

Footnotes (numbered consecutively) should be used sparingly, and only for substantive material that would be distracting if included in the body of text. Citations in the text will follow the "author-date" system described in detail in *The Chicago Manual of Style* 13th edition (Chicago & London: Univ. of Chicago Pr., 1969), ch. 15. For examples, see the article on pp 78-89 of this issue.

LETTERS TO THE EDITOR

Does this journal have a "class" angle?

I teach medical radiation science for students studying to become diagnostic x-ray technicians. I have been pleasantly surprised to find that the kind of ideas raised in *Science and Nature* arouse more interest among the technical students, predominantly working-class and minority, than among radiology residents (M.D.s) or among the upscale wealthier students in my physics courses at Northeastern University. For example, some students found your perspective on "Contradiction in Wave-Particle Duality" [S&N #2] quite edifying after struggling through the conventional presentation of the "dual-natured photon" given in the assigned, authoritative physics text.

I'd like to see this as a question of social "class" but am inclined to think that your compact format and concise presentation (free of statistical jargon and standard science-journal graphics) play as much a role in getting students to relate to the articles as does their social background.

Robert R. Montgomery
Program Director, Radiologic Technology
North Shore Community College
Beverly, Mass 01915

What Marxism Is All About

I am very impressed with your publication. It has been an ideal "find" after a long search for a periodical that attempts to connect all the sciences under one unified philosophy.

I wish to subscribe and acquire all the back issues.

Chris Barter
Albany CA 94706

On Axiomatics versus Dialectics in Quantum Mechanics

Max Robinson's paper was a useful reminder of the inadequacies and contradictions in the conventional interpretation of quantum mechanics that is used to support all kinds of mystical notions ["Is Quantum Mechanics a Scientific Theory?", S&N #6], but he was properly criticized for his confusing remarks on philosophy of science [Motz, Talkington, *ibid.*]. I wish to expand on some critical comments made there, as the basis for providing a concrete example to support them.

My main concern is with philosophical problems raised by Robinson's claim that there are only "differences in detail" between dialectical materialism (Engels, Lenin) and "scientific" materialism or realism (Popper, Russell, Bunge). There is, however, a fundamental difference in that one view insists nature is dialectical whereas, in the other view, this concept is strongly attacked.

For example, Bunge recently attacked the claim of universality for Marxist dialectics, asserting that the laws governing thought bear no relationship to the laws of nature since the former are man-made and the latter are not [*Scientific Materialism*, Reidel 1981]. Thus, the impression is left that the laws of thought emerged in some mysterious but unspecified process, and that concepts of matter are largely divorced from matter itself. This is a strange position for one who goes to great (and commendable) pains to defend materialism *per se*. For Bunge, a former Marxist, (formal) logic is now *a priori* and is applicable to any field of study, which leads to his heavy emphasis on the axiomatic approach. This account ignores the historical development of logic (What would Bunge make of multivalued logic?), and gives a universality to formal logical principles which he would deny to dialectics. The Bunge critique needs to be addressed seriously by Marxist scientists and I hope such a critique will be developed in the pages of *Science and Nature*.

Consistent with his emphasis on rigorous formal logic, Bunge advocates axiomatics as primary to the scientific method, particularly in quantum mechanics. But axiomatic formulations always take place after a breakthrough has been made in some area of science, and have no direct bearing on the process of scientific discovery itself. To suggest that axiomatic formulations are useful for the further development of science is misleading and, in fact, the opposite may be closer to the truth.

A case in point concerns quantum mechanics and Dirac's axiom that position and momentum operators do not commute, i.e., that $pq-qp=ih/2\pi$ follows from the Uncertainty Principle of Heisenberg. Recently a remarkable paper by graduate student Joseph Godfrey (Notre Dame) has shown "how to obtain the Dirac Commutator from the London-Weyl formalism."! [*Phys Rev Letters* 16 Apr 1984]. Godfrey uses Weyl's projective geometry to deduce the Dirac axiom, treating the geometrical properties of space as being in part determined by the particle used to probe that space. The physical notion of a particle's interaction with space is dialectical in the sense that a particle's motion affects the local geometry and the resulting distortion of the local geometry affects the particle's subsequent motion, leading to a profound interconnection between the position and momentum of a particle. In other words, where Dirac postulated the axiom, Godfrey has deduced it from more basic principles of physical relations. This new development in physics could certainly not have been made on the basis of Dirac's axiom, and thus provides a clear demonstration of not only the inadequacy of Bunge's attempts to axiomatize quantum mechanics but also the general weakness of strict dependence on formal logic alone.

Godfrey comments at the end of his paper: "One can only wonder if Einstein might have recognized here some confirmation of his views on quantum mechanics." I think Einstein would indeed regard Godfrey's work as confirmation of his view that quantum mechanics is not complete. Recently it has been claimed that the Aspect experiments on photon correlation [*Phys Rev D* 14:1944; 1976] validate orthodox quantum mechanics as opposed to hidden variable accounts, but this interpretation has been challenged by Marshall [*Phys Letters* 3 Oct 1983] who demonstrates that one class of local hidden variable theories gives very good agreement with Aspect's experimental results.

Incidentally, I do not think that Robinson has treated the Uncertainty Principle adequately. He seems to regard as "myth" that the measuring process interferes drastically with the measured system on the microscopic level. It seems to me that the logic of Heisenberg's microscope experiment is unassailable (the device is purely classical) and must lead to the conclusion that it is not possible to determine simultaneously with arbitrary accuracy the position and momentum of particles. It does not place restriction on the accuracy with which position or momentum may be determined independently. Robinson does not seem to realize this since his own proposed experiment [*Can. J. Physics* 47: 983; 1969] was criticized by Ballantine [*ibid.* 47: 2417; 1969] because it involves an initial determination of velocity and a subsequent determination of position.

Stanley Jeffers
Physics Department
York University (Ontario)

Editor's addendum: Godfrey has since noted [*Phys Rev Letters* 5 Nov 1984 p1857]: a) that Lloyd Motz had previously deduced the Dirac operator from the even more basic concept of Weyl's principle of gauge invariance [*Nuovo Cimento* 69: 95; 1970]; and b) that E. Santamato's independent deduction, from Weyl's geometry and Nelson's stochastic quantum mechanics [*Phys Rev D* 29: 2126; 1984], was likewise more successful than his own. It certainly seems, however, that the Jeffers interpretation gives added physical significance to Godfrey's derivation.

On Dialectics and the Logic of Nature

To postulate a logic in nature: this is a categorical outrage! Jeff Coulter in his critique of Engels fully supports (from a "revolutionary" standpoint of course) the often repeated empiricist attack on the dialectics of nature. The concept of a dialectical logic, Coulter complains, "confuses propositions in their relations to each other with their subject matter as such," but this of course is Kantian idealism, for if relations are not intrinsic to the "subject matter" whose objective reality we reflect in our minds, then they can only be introspectively induced, metaphysical postulates applicable *a priori*. The old story ... poor matter is the damsel in distress until Logic, with its shining armour of Categories, comes to the rescue! On this ruling class absurdity, "revolutionary" praxis and conservative positivism speak with one voice.....

Dialectics do not require the helping hand of consciousness before they can become a reality; on the contrary, nature is dialectically prior to man, as being is to thinking. And in making this assertion, Marxism has broken decisively from the exploitative tradition of thousands of years of abstract philosophical thought. It has not only replaced metaphysics with dialectics, but has freed dialectics from Hegelian mysticism: it has presented for the first time to the world a critical and revolutionary concept which, as Marx puts it, "lets nothing impose upon it" -- the dialectic in its consistently rational form. A dialectic which can only exist *in* human society because it existed before it.

-- John Hoffman, *Marxism and the Theory of Praxis*. NY: Intl. 1975 pp 181,69.

Kovalevskaia Lived More Than One Kind of Revolution

BEATRICE LUMPKIN
Mathematics (Retired)
Malcolm X College (Chicago)

A REVIEW ESSAY. Ann Hibner Koblitz, *A Convergence of Lives, Sofia Kovalevskaia: Scientist, Writer, Revolutionary*. Boston: Birkhauser 1984, 305 pp.+plates. $19.95.

THIS BIOGRAPHY of Sofia Kovalevskaia reads like a good novel. When you come to the end, you want more. And so you go back over the pages to absorb more of the rich, full, exciting and tragically short life (1850-1891) of the 19th-century's greatest woman mathematician.

As the title promises, Koblitz covers much more than the contributions to mathematics for which Kovalevskaia is best remembered today. In her time, she was also famous as novelist and revolutionary. The very term "convergence" suggests a unified whole, rather than three separate personalities struggling for ascendance in one person.

The biographer worked from a wealth of material. Kovalevskaia wrote autobiographical novels and kept correspondence files. Family members and friends kept diaries. Many Soviet historians have written on the subject. But most of this material has not been translated into English. Koblitz studied the Swedish as well as Soviet sources and faults the Kennedy [1983] biography of Kovalevskaia for omitting the Swedish sources.

My concern with the Kennedy biography *Little Sparrow* is not just a wrong date here or there, but something political. Kennedy [p ix] attributes Kovalevskaia's genius to the "genes of her inheritance," especially of her maternal grandfather, the German astronomer F.I. Shubert. To Koblitz's credit, she avoids such reactionary genetic explanations and does not make of her hero a "superwoman." Rather Koblitz describes the movement which produced many notable Russian women scientists, leaders and writers, of whom perhaps Kovalevskaia is best known.

Koblitz writes: "Sofia's achievements were impressive but so were those of her friends -- Suslova, Lermontova, Bokova-Sechenova, Evreinova and others" [pp 7-8]. Sofia was only one of a whole circle of radical women who broke away from their gentry traditions to become "independent-minded, dedicated, talented professionals of one sort or another."

And what a time that was for science and revolution! The 41 years of Kovalevskaia's short life spanned the bourgeois unification of Italy, the U.S. Civil War, the Third Republic in France, Japan's unconsummated revolution of 1868, the first printing of the theoretical works of Marx and Engels and the founding of the First International. Inside of Russia there were revolutionary situations in the late 50's and early 60's and the Peasant Reform of 1861 -- an incomplete but important first step toward abolition of serfdom in Russia [cf. Smirnov 1965; 217f]. As modern capitalism was just beginning to expand in Russia, the prophetic death knell of capitalism had already been sounded in the heroic Paris Commune of 1871.

Kovalevskaia felt the impact of these progressive upheavals as a child, witnessing the Peasant Reform and growing up with the story of her favorite uncle's wife who had been executed by her serfs in retaliation for her unbearable cruelty. As an adult, she herself participated in the Paris Commune, serving as a nurse for six weeks.

The intellectual life of the liberal gentry fully reflected all the progressive trends of Europe and America. The works of Darwin were embraced for their materialism and faith in progress, a world view which seemed to promise an end to the tyranny of religion and autocracy. There was a general conviction that the spread of knowledge, especially scientific knowledge, would hasten the day of revolution. Moreover, it was generally believed that science was well on the way to solving all of the world's problems, hunger, poverty, disease [p 57].

Learning calculus from the wallpaper

What led Sofia, despite incredible difficulties, to devote herself to the most abstract of sciences and become the first woman in modern times to win a doctorate in mathematics -- the first woman since the Renaissance in Italy to hold the chair of a major university? (There should be another book on these Renaissance women as well as one on Hypatia, the Egyptian algebraist who held a chair in philosophy at the University of Alexandria.)

Kovalevskaia grew up in a time of great change. Rebellion was in the air. Through her family she had access to some books and instruction, sometimes through subterfuge. For example, she learned to read at six by tricking those around her to tell her one letter at a time. Later her intellectual appetite was aroused by scientific discussions with her uncles and a happy accident (shortage of wallpaper) caused her room to be papered with printed notes of calculus lectures. She studied the pages for hours, but the formulas were incomprehensible. Still, in later years, it seemed she knew her calculus lessons in advance.

Sofia Kovalevskaia enjoyed the friendship of Darwin, Helmholz, Mendeleev, Mechnikov, most of the leading mathematicians of her time, notable cultural figures (Dostoevsky, Turgenev, Chekhov, Grieg, George Elliot), and utopian socialists (Herzen, Chernyshevsky). But some of these valuable contacts came only after she broke through the absolute prohibitions barring women from European universities. This involved literally escaping from the confines of the family estate by arranging a fictitious marriage with Vladimir Kovalevsky. He was one of a number of radical, idealistic men willing to make this kind of sacrifice for the cause of women's equality.

Such a desperate move was necessary then because a single woman could travel only on her father's passport and a passport was needed both for internal and external travel. The young "couple" moved to St. Petersberg and Sofia inspired Vladimir to study also.

Both Sofia and Vladimir studied intensively in St. Petersburg. Since women were not permitted to even audit university classes, sympathetic men accompanied Sofia to class, shielding her from the eyes of any possible government inspector. Soon Sofia decided that medicine was not her calling because it was mathematics that she loved with a passion. Her passion for mathematics is reminiscent of a statement by a 20th-century Afro-American mathematician, Marjorie Lee Browne: "I always, always, always loved mathematics!" [Kenschaft 1981; 593].

To continue their studies, the "pair" moved on to Vienna where there were good opportunities in paleontology for Vladimir. But Sofia did not find mathematicians willing to accept a woman student. True to his convictions, Vladimir put Sofia's professional needs first because women, so long suppressed, needed additional opportunities to catch up (19th century affirmative action). They moved to Heidelberg where Sofia studied physics with Kirchoff, physiology with Helmholtz, and mathematics with Konigsberger and DuBois-Reymond. In Heidelberg Sofia's apartment became the haven for other women scholars who had fled Czarist Russia in search of freedom and education, considered then as almost one and the same thing.

The reader may wonder how these students survived and where their bread and butter came from. Koblitz shows that they lived on Sofia's allowance from her father's estate. Although enough for one couple, spread among a group of refugees it was so little that at one point the women thought of hiring out as maids. Still they were living off the proceeds of semi-feudal exploitation. Although the Reform of 1861 had legally freed the serfs, in that landowners could no longer sell serfs as chattels, the peasants were forced to pay for their freedom and to buy their land from the landowner. Sofia, herself, in her book, *A Russian Childhood*, describes the hopes, confusion and disappointment which followed the ceremonial reading of the Reform proclamation which gave no land and left the corvee in place [Kovalevskaya 1978].

Twenty years after the reform, one-seventh of the serfs were still "temporarily bound" and land under peasant tillage was greatly reduced [Smirnov p 224]. In 1877, 79% of the land was still held by the nobility. As late as 1905 they held 62%, with only 15% of the land belonging to peasants [Lenin 1908]. This type of background, so vital to understanding the milieu in which Kovalevskaia lived and struggled, is largely omitted by Koblitz. (The land question is distorted by Kennedy who otherwise supplies much interesting material.)

In 1870 the Kovalevskys moved on to Berlin, armed with recommendations from Heidelberg professors. Karl Weierstrass, the 19th century mathematical giant, was not ready to accept a female student, but he did not close the entry door completely. Instead he gave Sofia a series of problems to solve. To his surprise, a week later she returned with solutions not only correct but original. Still the university refused to admit a woman, so Weierstrass taught her as a private student. He often said she was his most talented student. Their long, mathematical association ended only with Kovalevskaia's premature death. Indeed Weierstrass and his sisters, single all of their lives, became a second family for Sofia, many years their junior. Rumors suggesting any other type of association between Sofia and Karl appear totally unfounded.

Taking Part in Paris Commune

On March 18, 1871, the people of Paris "stormed the heavens" and declared the Paris Commune [Marx 1871a; 530]. Their Manifesto is quoted by Marx [1871b; 494]:

> The proletarians of Paris, amidst the failures and treasons of the ruling classes, have understood that the hour has struck for them to save the situation by taking into their own hands the direction of public affairs.

Anyuta, Sofia's revolutionary older sister, and Anyuta's French Communard husband played important roles in the governance of the Commune. Both Kovalevskys spent April 5 to May 12 in the service of the Commune, with Sofia nursing the many wounded. To get there, the Kovalevskys crossed the Seine to Paris in a row boat, under a hail of fire. Yet Koblitz spends only a few tantalizing pages on what was surely an unforgettable experience for the Kovalevskys and all posterity. Sofia planned to write the story herself, but if she did so before her fatal bout with pneumonia, the document has not yet been found.

On May 12, at Anyuta's urging, the Kovalevskys left for Berlin, thinking the Commune safe for the time being. But on May 22, through a treacherous conspiracy between Thiers, head of the bourgeois French government, and Bismarck's army, Paris was invaded. The insane fury of the treacherous French capitalists who teamed up with Bismarck was unleashed against Paris, Communards or not. Tens of thousands were mowed down and many buried alive [Marx 1871b; 526f].

Sofia and Vladimir returned to Paris, found Anyuta well but in hiding. Anyuta's husband was in jail. The Kovalevskys stayed long enough to smuggle Anyuta out of France and to arrange her husband's escape from jail. Karl Marx himself helped Anyuta relocate in London.

Koblitz' only explanation for the establishment of the Commune was the patriotism of the National Guardsmen of Paris who would not allow Thiers to disarm the defenders of Paris while Bismarck was camped at the city's gates. For whatever reason, Koblitz does not touch on the true content of the heroic Commune, pointed out by Marx:

> Its true secret was this. It was essentially a working class government, the produce of the struggle of the producing against the appropriating class, the political form at last discovered under which to work out the economical emancipation of labor [1871b; 502f].

Twenty years after the Commune, Frederick Engels added:

> Of late, the Social-Democratic philistine has once more been filled with wholesome terror at the words: Dictatorship of the Proletariat. Well and good, gentlemen, do you want to know what this dictatorship looks like? Look at the Paris Commune. That was the Dictatorship of the Proletariat [1891; 460]

It is only in the above context that we can understand Koblitz' sole quote from Lenin (or Marx): "The Commune is the first attempt by the proletarian revolution to smash the bourgeois state machine..." [p 104]. Otherwise the statement, out of context, can lead to a distorted picture, of hammer-wielding people smashing the marble halls of Versailles.

Vladimir wrote to his famous brother Alexander that "from April 5 to May 12 we lived happily in the Commune of Paris" (p 105), Koblitz could have provided the background for Kovalevsky's statement by citing some of the many reforms of the Commune. Marx [1871b; p 502] noted that "the commune made that catchword of bourgeois revolutions, cheap government, a reality by destroying the two greatest sources of expenditure -- the standing army and state functionarism." Public service had to be done at workmen's wages. The church was disestablished from the state and required to depend on private contributions. Education was made accessible to all and prostitution was outlawed, reforms in which Anyuta was involved. Judges were made elective, responsible to the people and subject to recall. Indeed the Commune won the support of most of the middle class of Paris by easing their burden of debt.

Other reforms included the abolition of nightwork for journeymen bakers, prohibition of the many fines levied by employers and the reopening of closed workshops and factories by associations of workers. "In fact", observed Marx [1871b; 511], "for the first time since the days of February 1848 the streets of Paris were safe, and that without any police of any kind."

After the Commune, Sofia spent some time in Zurich. There were frequent separations from her husband and the marriage still platonic. She returned to Berlin to write her doctoral dissertation and prepared three papers, any one of which would have been suitable. The problem remained of finding a university willing to grant a degree to a woman -- she was still a private student of Weierstrass because Berlin would not accept a woman student. They decided to try the University of Göttingen which agreed, after some pressure, to award the doctorate in absentia (*summa cum laude*).

Meanwhile husband Vladimir had also obtained his doctorate, as did two of the women who had lived with Sofia. Returning to Russia the group had great hopes of finding suitable positions. But their hopes were frustrated by the stone wall of prejudice against women scientists plus the additional prejudice against Russians who were German-trained. A five-year interlude occurred with Sofia and Vladimir becoming a real married couple, plunging into speculation with Sofia's inheritance and participating in the mad social whirl of St. Petersburg life. As could have been predicted, the speculations failed and Sofia's inheritance was entirely lost.

From the start, Vladimir had failed to play the part of the fictitious husband who helps his wife escape, then drops out of her life. Instead he considered Sofia and her sister his "family". After some years he thought of divorce but the attraction of "his family" was too great. There was genuine affection between the two but it was only after seven years that the marriage was "consummated" and a daughter was born.

After a five-year absence from mathematics, Kovalevskaia had to fight her way back. Refused the right to take the masters examination that was required for teaching positions in Russia, she determined to write as many mathematical papers as possible, "in order to keep up our women's reputation" [p 148]. She was living in Paris, working again, when news reached her of Vladimir's suicide. Financial ruin, the possibility of prosecution, and failure to win recognition for his important contributions as a founder of evolutionary paleontology had been too much for him to bear.

Academic Recognition Comes at Last

Not until 1883 did Sofia get a university position. Through efforts of another student of Weierstrass, Gösta Mittag-Leffler, she received a one year, unpaid appointment at Stockholm. Then followed a five-year paid position and, finally, a lifetime chair in Mechanics. As noted before, Kovalevskaia thus became the first woman to hold a chair in a European university since the Renaissance in Italy. She also became an editor of *Acta Mathematica*, an international mathematics journal founded by Mittag-Leffler. Somehow she found time to plunge into a literary career, too, and published her widely acclaimed autobiographical novels.

In the midst of many researches and plans for new books, Sofia ignored a bad cold, travelling across Europe in January, then meeting her classes despite the illness. Pneumonia developed. At the age of 41, Sofia Kovalevskaia died, leaving a shining example of struggle for equality and scientific achievement.

Kovalevskaia's contributions to mathematics are outlined by Koblitz all too briefly. Helena Pyclor's [1984] review of Koblitz' book in *Science* complains that too little space is given to Kovalevskaia's mathematics, nor are the differences in the approach of Russian and German mathematicians explained. But Pat Kenschaft [1984], reviewing for the Association of Women in Mathematics, comments that biographies of mathematicians tend to lack detailed explanations of mathematical achievements because mathematics is so highly specialized. For a more technical exposition of the mathematics, Koblitz refers the reader to an analysis by the Soviet historian of mathematics, P.Y. Polubarinova-Kochina (translated by Ann's husband Neal) in the appendix of *A Russian Childhood* [Kovalevskaya 1978; 231-249]. There is also a very readable article for mathematicians on "Sonya Kovalevsky" by Karen D. Rappaport [1978].

The philosophical issues involved in the differences between the German and Russian schools of mathematics at that time are of some interest. Kovalevskaia was an emissary of the Weierstrass school of the arithmetization of analysis (calculus and allied subjects). Arithmetization provided a rigorous, logically precise foundation for calculus, free of idealist considerations. But it did so only by sidestepping the question of motion and development.

A Philosophical Perspective

Weierstrass used purely static considerations [Boyer 1968; 608], avoiding the question posed by Zeno in the race of Achilles with the tortoise: does Achilles ever catch up to the tortoise? Or can a variable which approaches its limit ever attain it?

Augustin Cauchy, preceding Weierstrass in the effort to put calculus on a rigorous, logical foundation, still included an appeal to the intuition of continuous motion in his definition of the limit, using phrases such as "successive values," "approach indefinitely," and reducing the distance from the limit to "as little as one wishes" [Boyer 1949; 272].

The Weierstrass school, using what we today call the delta-epsilon method, eliminated the need to resort to quantities "infinitely small" which mystically appear and disappear. But they did so only by banishing the intuitive, geometric understanding of calculus that arose out of the study of physical motion. It was the same need for logical rather than mystical explanations that led Karl Marx in England to write his important works on mathematics [Gerdes 1985; 20].

Kovalevskaia was able to bridge the gap between the German and Russian schools because much of her work was in mathematical physics. She was well received when she brought the new Weierstrass methods before Russian mathematicians in an invited talk in St. Petersburg.

Kovalevskaia's first three works were theoretical. Charles Hermite said of her first paper that it would serve as the point of departure for all future work in partial differential equations [p. 241]. It is now known as the Cauchy-Kovalevsky theorem because, unknown to Weierstrass or his pupil, Cauchy had formulated a similar theorem, but for first order. Kovalevskaia had generalized it to order "r" and had used a simpler proof. Later works of Kovalevskaia, although in fields of applied mathematics, still used techniques of the Weierstrass school.

Perhaps the most spectacular award crowning Kovalevskaia's work was the Prix Bordin of the French Academy of Science. Of 15 papers submitted, all anonymously, one was so outstanding that the amount of the prize money was raised from 3,000 to 5,000 francs. The winning paper was Sophia's work on the motion of a rigid body about a fixed point. And the very next year she was elected a corresponding member of the Russian Academy of Science which changed its constitution for the express purpose of allowing the membership of Kovalevskaia.

Koblitz succeeds in tracing Kovalevskaia's personal development and gives a vivid picture of the awesome obstacles she overcame to become a professional mathematician and win a lifetime academic chair. Her literary work, although described only briefly, also comes across as a vital part of Kovalevskaia's life. While not enough material is supplied for a true understanding of her revolutionary convictions, still Koblitz has made a fine contribution in describing the *convergence* of the mathematical, literary and political components of Kovalevskaia's life.

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Women and Science

SOME CAUTIONARY TALES

On the last page of her autobiography, Payne-Gaposchkin lovingly transcribed the lines

Knowing that Nature never did betray
The heart that loved her.

If Nature never betrayed Cecelia Payne-Gaposchkin, male scientists certainly did. Beginning with the pathbreaking dissertation (*Stellar Atmospheres*, 1925), which applied powerful new physical concepts such as ionization, Harlow Shapley directed the course and content of her research. Payne-Gaposchkin found that hydrogen was by far the most abundant element in stellar atmospheres, but this result did not fit with Shapley's views and she was forced to disavow it. Her second monograph (*Stars of High Luminosity*, 1930) was suggested by Shapley. As she studied the distribution of supergiant stars, Payne-Gaposchkin found evidence of the absorption of light by dust in space but, again, this did not fit into Shapley's model, and "I record with shame that I allowed myself to be persuaded, to adduce evidence of faint blue stars at low [galactic] latitudes, and to express the opinion that interstellar absorption was at most a minor factor in the study of stellar distribution" (p. 168). A spectroscopist by training and temperament, Payne-Gaposchkin was forced by Shapley to abandon this line of work for photographic photometry because it fitted in with his master plan for the Harvard Observatory.

The life of Cecilia Payne-Gaposchkin has much to teach us concerning problems women face in the scientific community. It is also a story of strength and courage as well as a cautionary tale. May her autobiography and future scholarly studies based upon it prove a guide and an inspiration to both women and men in science.

-- John Lankford reviews *Cecelia Payne-Gaposchkin: An Autobiography and Other Recollections*, ed. by Katherine Haramundanis (Cambridge Univ. Pr. 1984). *ISIS* 76: 80-83; May 1985. Abridged excerpt.

Late in her [Cecelia's] life and early in my career, I attended an international astronomy meeting at the National Academy of Sciences at which she was present, and one evening found myself helping her fix her zipper in the ladies' room. Impulsively I took the opportunity to ask her many questions concerning her experience as a woman in a scientific field dominated by men. Oh, no, she replied to each of my questions, being a woman had made no difference.

But the next evening she sought me out as we were socializing in the Great Hall before the banquet. "You know those questions you asked me last night?" she asked. "Well, I decided that I gave you all the wrong answers." Then she proceeded to describe many of the difficulties that had plagued her throughout her career at Harvard. Her autobiography, published after her death, tells a tale of disappointment after disappointment, of opportunities denied. One of the most brilliant astronomers of her time, Payne-Gaposchkin was never permitted to work on astronomy's significant problems and never elected to the National Academy of Sciences.

-- Astronomer Vera Rubin, "For Women in Science, a Fair Shake Is Still Elusive." *Science 86* July issue pp 58-65.

What about the fantasies of a scientist wife?

Jerome Karle met his wife Isabella at Michigan University where he got his PhD in 1943. He is now chief scientist for work on the structure of matter at the Naval Research Laboratory. She is also a chemist working there. Jerome was one of the winners of the 1986 Nobel Prize for Chemistry. Isabella's contribution to the Nobel-Prize winning research is said to have been considerable.

Dr. William L. Duax at Buffalo said that for 15 years scientists did not appreciate what had been done in the 1950s and 1960s by Dr. Karle and Dr. Herbert A. Hauptman, who shared the prize -- not until Isabella Karle immersed herself in the mathematics of the research on three-dimensional molecular structure determination and pointed out to others its potential applications in the understanding of crystals and the development of drugs.

When one of their daughters, Louise Hanson, a chemist at Brookhaven National Laboratory, heard the news, she said her only regret was that her mother was not also honored with the Nobel Prize. "My mother worked very closely with my father all these years," Mrs. Hanson said. "If it were not for her, it would have taken much longer for the methods that he and Herb Hauptman developed to be accepted."

Jerome was on a plane returning from West Germany to Washington when the pilot announced the news of the Nobel Prize. Asked if he had ever thought he would win a Nobel Prize, he said, "I think there are very few serious scientists who don't fantasize about it at one time or another."

-- John Noble Wilford, *New York Times* 17 Oct 1985. Adapted.

ADDENDA: The Royal Swedish Academy of Sciences cited Hauptman and Karle "for their outstanding achievements in the development of direct methods for the determination of crystal structures." Since past Nobel awards in crystallography have been mostly for determining the structures of particular substances, the committee must certainly have had in mind the useful applications of this method. In the condensed version of Jerome Karle's Nobel lecture the work of Isabella Karle on applications of the theory is noted [p.842] and her name appears on 12 of 55 references cited (from 1949 to 1985). -- *cf. Science* 232:837-843; 16 May 1986.

The more things change, the more they stay the same

...the progress of women in science over the past two and a half decades has been enormous. From 1960 to 1985, women earned more than 56,000 doctorates in science and 1,000 in engineering, increasing their share of doctorates from 8.1 percent of the total in 1960 to 30 percent in 1984. But even while their ranks have been swelling, their career opportunities have lagged substantially behind those for men.....The gains over the last two decades have occurred in a climate of legally mandated educational

opportunities, supportive changes in society's view, and favorable political backing. But a change in this climate -- even toward neutrality -- could slow women's reach toward equality.

-- Betty Vetter, Director, Commission on Professionals in Science and Technology and AAAS Office of Manpower. *Science 86* July pp 62-63.+

Should feminists support motherhood?

The most urgent problem facing modern American women is reconciling the demands of childbirth and child rearing with those of earning a living. For working mothers, conditions are onerous and getting worse. Most women do not work for pin money; they need to hang on to their jobs to buy groceries and pay the rent.

Women take a nose dive when they become mothers. Pregnant workers are routinely fired; others are defined as "new hires" when they come back to work after childbirth and lose seniority rights; and large numbers of new mothers fail to find affordable child care and are forced to take a third-rate job with short hours close to home. Overall, women lose 20 percent of their earning power in the immediate aftermath of childbirth. U.S. women still earn 64 percent of a man's wage, as they did in 1939.

Many of the battles fought by feminists in the 70's were important. We did manage to break down barriers and open up the marketplace. Women gained new access to jobs, education and credit. But as we dressed for success in our business suits and little string ties, we tended to forget that 90% of women choose to have children at some point in their lives and that women would remains seriously handicapped in the workplace unless we established a new system of family supports.

Easing the lives of working mothers, insuring a better start in life for children -- these are among the central problems of our age. Put them at the top of the feminist agenda, and the movement will have a new lease on life.

-- Sylvia Ann Hewlett, economist and author, most recently of *A Lesser Life: The Myth of Women's Liberation in America* (*NYT Science Times* 17 June 86).

How the cards got stacked against women

ENGELS: The pairing family is the form characteristic of barbarism, as group marriage is characteristic of savagery and monogamy of civilization. Unless new social forces came into play, there was no reason why a new form of family should arise from the single pair. But these new forces did come into play. The domestication of animals and the breeding of herds developed a hitherto unsuspected source of wealth and created entirely new social relations.

But to whom did this new wealth belong? Originally to the gens (clan) without a doubt. But at the threshold of authentic history we already find the herds everywhere separately owned by heads of families, as are the artistic products of barbarism (metal implements, luxury articles and finally, the human cattle -- the slaves).

For now slavery has also been invented. The family did not multiply so rapidly as the cattle. More people were needed to look after them; for this purpose use could be made of enemies captured in war, who could also be bred just as easily as the cattle themselves.

Once it had passed into the private possession of families and there rapidly begun to augment, this wealth dealt a severe blow to the society founded on pairing marriage and the matriarchal gens. According to the division of labor within the family at that time, it was the man's part to obtain food and the instruments of labor necessary for that purpose. He therefore also owned the instruments of labor, just as the wife owner her household goods. Therefore, according to the social custom of the time, the man was also the owner of the new source of subsistence, the cattle, and later of the new instruments of labor, the slaves.

Thus in proportion as wealth increased it made the man's position in the family more important than the woman's. In the end, the ancient mother right had to be overthrown: for the reckoning of descent in the female line and the matriarchal law of inheritance were substituted the male line of descent and the paternal law of inheritance. The overthrow of mother right was the world historical defeat of the female sex. The man took command in the home also; the woman was degraded and reduced to servitude; she became the slave of his lust and a mere instrument for the production of children. This degraded position of the woman, especially conspicuous among the Greeks of the heroic and still more the classical age, has gradually been palliated and glossed over, and sometimes clothed in a milder form; in no sense has it been abolished.

The establishment of the exclusive supremacy of the man shows its effects first in the patriarchal family. Its essential features are the incorporation of unfree persons and paternal power. Among the Romans, the word "family" (*familia*) did not at first refer to the married pair and their children but only to the slaves. *Famulus* means domestic slave and *familia* is the total number of slaves belonging to one man. The term was invented by the Romans to denote a new social organism whose head ruled over wife and children and a number of slaves, and was invested under Roman paternal power with rights of life and death over them all. Marx noted that the modern family "contains in germ not only slavery but also serfdom [and] contains *in miniature* all the contradictions which later extend throughout society and its state."

Marxist prescience from the past century

In bourgeois society the marriage is conditioned by the class position of the parties and is to that extent a marriage of convenience, often turning into the crassest prostitution -- sometimes of both parties, but far more commonly of the woman, who only differs from the ordinary courtesan in that she does not let out her body on piecework as a wage worker, but sells it once and for all into slavery. Sex love in the relationship with a woman becomes and can only become the real rule among the proletariat -- whether this relation is officially sanctioned or not. But here all the foundations of typical monogamy are cleared away. Here there is no property, for the preservation and inheritance of which monogamy and male supremacy were established.

The peculiar character of the supremacy of the husband over the wife in the modern family, the necessity of creating real social equality between them and the way to do it, will only be seen in the clear light of day when both possess legally complete equality of rights. Then it will be plain that the first condition for the liberation of the wife is to bring the whole female sex back into public industry, and that this demands that the characteristic of the monogamous family as the economic unit of society be abolished.

Monogamy arose from the concentration of considerable wealth in the hands of a single individual -- a man -- and from the need to bequeath this wealth to the children of that man and of no other. For this purpose, the monogamy of the woman was required, not that of the man. The coming social revolution will reduce to a minimum all this anxiety about bequeathing and inheriting.

Having arisen from economic causes, will monogamy then disappear when these causes disappear? The position of men will be very much altered. But the position of women, of *all* women, also undergoes significant change. What will certainly disappear from monogamy are all the features stamped upon it through its origin in property relations; these are, in the first place, supremacy of the man and, secondly, the indissolubility of marriage.

But what will be new? That will be answered by a new generation: a generation of men who never in their lives have known what it is to buy a woman's surrender with money or any other social instrument of power; a generation of women who have never known what it is to give themselves to a man from any other consideration than real love.

-- Frederick Engels 1891, *The Origin of the Family, Private Property and the State* (NY: International 1972) pp 117-145. Abridged excerpts.

STILL PART OF THE SYSTEM: Justice Thurgood Marshall, in a separate opinion..., said: "I fully agree with the Court's conclusion that workplace sexual harassment is illegal," but he said the Court had not gone far enough. He said employers should be held liable whenever supervisors sexually harassed their subordinates, "regardless of knowledge or any other mitigating factor," following the Equal Employment Opportunity Commission guidelines adopted several years ago. The commission and the Justice Department, in their joint brief, repudiated the commission's guidelines on broad employer liability. [NYT 6-19-86]

Kovalevskaia Fund Established by the Koblitzes

Ann and Neal Koblitz [author of the biography reviewed in this issue, and her husband] held discussions in Hanoi on setting up a Kovalevskaia Fund to aid women in science in Vietnam and other developing countries who face formidable barriers to participation in science and technology. Agreement was reached with Vietnamese officials, educators and Women's Union activists on concrete Fund projects including: 1) Two annual Kovalevskaia Prizes (named for the great 19th century mathematician, writer, socialist and feminist), in books and money for other scientific purchases, for women scientists chosen by the Vietnamese. 2) An annual travel grant to an American woman scientist to spend a month lecturing in Vietnam on mathematics, biology or other applied science field. 4) An international seminar on Women and Science in Developing Countries to be held in Hanoi Jan 8-10, 1987, with transportation paid for ten participants from other countries of Southeast Asia and two from Africa and Latin America. In the U.S., the idea of the Kovalevskaia Fund has met with a very favorable reaction from many people, and there have been a few generous donations.

-- Bulletin of the U.S. Committee for Scientific Cooperation with Vietnam, Neal Koblitz, editor. Bulletin address: c/o Judith L. Ladinsky, 101 Bradley Bldg., 1300 University Ave., Madison WI 53706. Abridged.

Reflections on Brecht's "Galileo"

JAMES M. LAWLER
Philosophy Department
SUNY/Buffalo

In notes to his play *Galileo*, Brecht wrote:

> The bourgeois single out science from the scientist's consciousness, setting it up as an island of independence so as to be able in practice to interweave it with their politics, their economics, their ideology. The research scientist's object is pure research; the product of that research is not so pure. The formula $E=mc^2$ is conceived of as eternal, not tied to anything. Hence other people can do the tying: Suddenly the city of Hiroshima became very short-lived. The scientists are claiming the irresponsibility of machines. [1]

Brecht's play on the life of Galileo is a moving and thought-provoking examination of the moral responsibility of the scientist which goes to the roots of the atomic age. The play was originally to have opened in New York on December 7, 1947. But Brecht was brought before the Un-American Activities Committee and hounded out of the country because of his Marxism. Not unlike Galileo, Brecht was persecuted because he challenged the reigning dogmas of his age. Those dogmas continue to dominate American social life and continue to threaten the earth with annihilation.

Brecht's play probes the issue of the responsibility of the scientist for the welfare of mankind. Unchecked by this sense of responsibility, unchecked by a view of the connection between the progress of science and the progress of mankind, science can become a great force for destruction.

And yet for Brecht it is not so much a matter of adding to science a sense of moral responsibility, as though morality were some kind of external check, as though morality were something unscientific and science something that by itself is amoral. It is more a matter of seeing the moral grandeur of science itself and of developing to the fullest its moral implications, i.e., of recognizing the liberating potential of science. So the play opens with a paean to the "new age" in which science would free mankind from thousands of years of misery and superstition.

Such progress does not however come about in a straight line. The scientist is a specialist whose livelihood depends on others. In order to be able to carry on his fundamental scientific projects Galileo sinks to petty fraud and pandering to the commercial and military interests of the time. "No one's virtue is complete: Great Galileo liked to eat." The play, and the moral dilemma of the scientist, develop in this tension between the possibilities which science offers for the betterment of the laboring majority of mankind and practical conditions in which science must develop as an instrument for the advancement of the private interests of a privileged minority.

Brecht's Galileo attempts to straddle the fence in this matter, serving local interests as a kind of tactical concession to his personal needs while aiming in the long run at the overthrow of the system of ignorance and backwardness

which lies behind such abasement of his integity. Galileo naively believes that pure truth will triumph by its own intrinsic light. He does not immediately grasp the implications which truth, even the remote truth about the heavens, has for the practical lives of people on this earth--people whose privileges in life depend on keeping the majority of mankind in ignorance.

The powers that stand to lose by this truth refuse to look through Galileo's telescope, and a young monk, in defending the Church's decree, inadvertently explains why: "What would my people say if I were to tell them they were living on a small chunk of stone that moves around another star, turning incessantly in empty space, one among many and more or less significant?...In that case they would say, no one is watching over us. Must we, untaught, old and exhausted as we are, look out for ourselves?" And Galileo sardonically replies, "You're right; the question is not the planets, but the peasants of the Campagna."

Scientific truth cannot make its way unless the scientist does battle with the powers that stand to gain by continuing the old ways of thinking. Galileo taught that the earth was in motion, and discovered that this movement ultimately threatens social structures that seek to perpetuate themselves as immobile absolutes. In such a battle, what power does the scientist have? Overwhelmed by the odds, Galileo recants.

Brecht dramatically argues that the progress of scientific truth does not occur in a vacuum. It is intrinsically tied with the progress of humanity, and to the extent that the vast majority of mankind labors to enrich a few, then to that degree will science itself be hampered, bound and distorted in its own development. While science has a responsibility to contribute to the liberation of mankind, mankind, by freeing itself from its servile labor, will ultimately liberate science and the scientist. The responsibility of the scientist for human progress is at the same time, in a fundamental way. the responsibility of the scientist for science itself.

If Galileo announced the movement of the earth, Brecht himself effectively conveyed ideas about the motion of social life. He turned the telescope of historical science, of Marxism, on the contradictions of social life. He magnified the range of that science with the power of his art, revealing the horrific obstacles which private interests erect against human progress, culminating during the writing of this play both in the frenzy of Naziism as a desperate attempt to stop the clock of history, and in the atomic incineration of Hiroshima and Nagasaki.

Consequently, Brecht brought down on himself the condemnation of the modern church with its anti-communist dogmas and hatreds. The play and its ideas are not out of date, if only for the fact that one of the unrepentant perpetrators of Brecht's Inquisition, who wraps himself in the flag of morality, is today President of the United States.

Reference

[1] Ralph Manheim and John Willett, eds., *Berthold Brecht, Collected Plays*. New York: Pantheon 1972, vol 5 p 220. Citations from the play are found in this volume.

The Pope, on the *spirit* of Marxism:

Unfortunately, the resistance to the Holy Spirit.....finds in every period of history and especially in the modern era its external dimension, which takes concrete form as the content of culture and civilization, as a philosophical system, an ideology, a program for action and for the shaping of human behavior.

It reaches its clearest expression in materialism, both in its theoretical form -- as a system of thought -- and in its practical form -- as a method of interpreting and evaluating facts -- and likewise as a program of corresponding conduct. The system which developed most and carried to its extreme practical consequences this form of thought, ideology and praxis is dialectical and historical materialism, which is still recognized as the essential core of Marxism.....

This is the striking phenomenon of our time: atheism. The order of values and the aims of action which it describes are strictly bound to a reading of the whole of reality as "matter." Though it sometimes also speaks of the "spirit" and of "questions of the spirit," as for example in the fields of culture or morality, it does so only so far as it considers certain facts as derived from matter. According to this system, matter is the one and only form of being.....

It can be said therefore that materialism is the systematic and logical development of that "resistance" and opposition condemned by St. Paul with the words: "The desires of the flesh are against the spirit."

-- From John Paul II's encyclical, "The Lord and Giver of Life," which was "brimming with references to Satan and to the coming of the third millenium" [NYT 5-31-86].

ROME, June 1 -- Vatican officials said today that Pope John Paul's sharp attack on Marxism in his latest encyclical did not preclude new diplomatic openings to Eastern Europe or exchanges with individual Marxists.....Joaquin Navarro Valls, the chief Vatican spokesman, said the Pope's views on "Marxism as a doctrine" did not preclude "a practical approach to current problems.".....

[Mr. Navarro added] that the Pope's encyclical reflected his concern not only with Marxism, but also with the spread of more secular attitudes in the world. "In some ways, secularism is a much greater challenge now than Marxism," [he] said.....

Vatican officials said that paradoxically, the Pope's firm statement in his encyclical against "dialectical and historical materialism, which is still recognized as the essential core of Marxism," could ease the way for practical initiatives by making clear that there has been no softening of the Vatican's opposition to Marxism.

-- E.J. Dionne, special to the New York Times [6-2-86]

COMMENT: Though embedded in an anti-Marxist diatribe based on religious idealism, the Pope's description of Marxist philosophy is itself surprisingly objective. This surely reflects lessons from bitter argument with the Liberation Theologists who led the way to widespread adoption by Catholics of useful Marxist materialist concepts. One may read the encyclical as an angry protest over the way "secular" materialism continues to move Catholics toward Marxist positions on world issues that mightily involve humanity and the satisfaction of its natural "desires of the flesh." No doubt the leadership given by Marxists in the world struggle for peace and against imperialism has also contributed much to this shift of consciousness within the church as without. How much the official Vatican "explanation" portends in the way of "practical initiatives" for world peace and social justice remains to be demonstrated.

It's worth noting that early this year a papal scientific advisory group recommended "banning the placement and testing of all weapons in outer space," declaring that "it is essential to prevent a spiral of competitive deployment of weapons in space." [NYT 1-24-86] Then, in an address to the Pontifical Academy of Sciences shortly before issuing the new encyclical, John Paul II urged international accords to insure peaceful use of space. Though carefully avoiding any specific reference to Reagan's Star Wars plan, the Pope said: "It is my hope that by means of joint agreements and commitment, all governments will promote the peaceful use of space resources for the sake of the unification of the human family in justice and peace." Speaking on the topic of the academy meeting -- the use of satellites to help developing countries improve their agriculture and other resources -- the Pope also said that proper use of science should "make it possible to feed the whole human family." [NYT 6-21-86]

Key question: Does the Vatican's professed desire for dialogue with Marxists envision joint efforts to meet the needs of humankind, or is it simply a deceptive effort to take over leadership of the peace and liberation movements and subvert them to its own purposes? There is obviously a struggle within the Vatican over this question and the final answer may not be decided yet. But where the Pope stands personally seems clear if we may judge from his actions such as efforts to suppress Liberation Theology, implacable hostility to the Sandinista government and now the latest encyclical. His seems to be a Reagan-type game plan that risks destruction of the world. And this would certainly not be the first time that idealism has provided a mask for brutal reaction!

For what light they may shed on a question so important (philosophically and otherwise), I append two more Marxist views. [L.T.]

Resurrecting the struggle against Satan?

Tom Foley, *People's Daily World,* 10 June 1986

POPE JOHN PAUL II on May 30 issued a new encyclical. According to press reports, it denounced Marxism and materialism, especially dialectical and historical materialism. The New York Times commented that the 141-page encyclical was "brimming with references to Satan."

It would be unfair to judge something merely by press reports about it or by excerpts that may have been taken out of context. Capitalist propaganda has often used exactly those method to try to smear Marxism. So the encyclical as a whole will have to be studied before coming to any firm conclusions about it.

But there are certain things that can be dealt with right away, such as the definition of materialism. This word comes from the term "matter." As Lenin often stressed, for Marxists "matter" is objective reality that exists independently of us and is reflected in our minds. Materialists, therefore, are people who recognize the existence of objective reality.

However, now we begin to confront a problem. How can anyone object to this? All of modern science is based on recognizing that objective reality exists. Scientists may have the most widely varying personal philosophical beliefs. But in actual practice they are all materialists and could not be scientists otherwise, even those scientists who are members of the clergy. Objective reality is that 2 plus 2 equal 4, and so on.

All of us benefit from this scientific, materialist approach. Perhaps the vast majority of us would not even be here without the discoveries of modern medicine, for example. The scientific, materialist approach has been one of the greatest and most positive liberating forces in the history of humanity.

The unfortunate fact remains, however, that there were always powerful groups whose basic material interests were against liberation and they still exist today. These are not the ancient slaveowners and feudal lords of yesteryear but the capitalists who run the giant monopoly coporations. Scientific materialism is being used today to explain how they exploit working people, so that the working people can organize and mobilize to end this exploitation.

The Communists are in the front ranks of this struggle, but of course there are many others taking part as well, including religious believers. The impact of Marxism in recent decades has been very powerful, especially among the clergy who identify and work with poor, working people. Nowhere has this been more true than in Latin America where "liberation theology" was born.

Liberation theology to some extent borrowed directly from Marxism and used Marxist categories within a Christian framework to analyze and condemn exploitation, to place the church on the side of the workers, the poor and oppressed. It has been perceived by the Vatican as a serious threat and for some time now there have been harsh warnings coming from Rome about the alleged "Marxist contagion." Certain Brazilians were predicting that the next encyclical would be aimed at all exponents of liberation theology and would be entitled "Shut Up!" in Latin.

There is nothing above, beyond or apart from objective reality and neither is the church. The struggle that is going on in the world between the forces of liberation and the forces of exploitation and oppression is taking place within the church as well as outside of it.

The church emerged in ancient, slave-owning society and existed through feudal and capitalist society. It also exists today in those countries where socialism has been established and it is not being suppressed or persecuted. It is becoming more and more clear to many in the church that it is not wedded to the continued existence of capitalism.

But to those who feel the exact opposite, these people have to be condemned. They are dangerous. This raises the question of a widening gap between elements in the church as one becomes more reactionary and estranged from the others. And theologically, by stressing the philosophical division between "spirit" and "matter," the risk is run of lurching down into what is a disguised form of Manichaeanism rather than Christianity, that Manichaeanism which has been one of the most powerful undercurrents in Christianity. For in Manichaeanism, there are two equal Gods, one Good, the other Evil, one Spirit, the other Matter. Victory means physically annihilating Evil, Matter.

I look forward to reading the encyclical.

Left Catholics and the dialogue with Marxism

There are two substantially different tendencies among Catholic leaders who support socialism.

The first is the search for an alternative to scientific socialism through the construction of different variants of "Christian socialism." This is no more than a veiled form of political anti-communism, since it rejects the existing socialism advocated by the revolutionary working-class movement. In effect, "Christian socialism" is either a variant of social-democratism or the political ideology of so-called neo-capitalism. It is hostile to the Marxist outlook, and acts as its adversary in the field of political ideology.

The second is the tendency to remove the competition against Marxism to the philosophical sphere, allowing for dialogue and cooperation in the political field. Exponents of this tendency are trying to find a motivation for such cooperation on the basis of the Catholic world outlook. A variety of philosophical conceptions is being constructed, containing some progressive social ideas, as well as new interpretations of the Christian philosophy of man and social ethics.....Many Left Catholics stress that in the modern socialist-oriented world this is the only way to preserve Catholicism.

-- T. Jaroszewski, Socialism and the Left Catholic interpretation of Christian humanism. *World Marxist Review* Aug 1974 pp 98-106. Excerpts.

The Explanatory Power of Marxism in the Social Sciences

MICHAEL PARENTI
Political Science
Brooklyn College CUNY

A Marxist understanding of social problems is crucial for many of the problems faced by natural scientists in their professional community and as citizens in general. Moreover, the same Marxist outlook that illuminates social problems is useful to the natural scientist in dealing with conceptual problems and the theory of knowledge. Parenti, author of a recent book on the mass media (Inventing Reality), has an unusual knack for illuminating Marxism itself. This paper is a lively and penetrating analysis that makes a good introduction or refresher on the subject. It was first given at a AAAS workshop on The Usefulness of the Marxist Outlook in Science (Annual Meeting, New York 24 May 1984). Editor.

MARXISM has been accused--often by persons who have never studied it--of being "simplistic" and "unscientific." Actually, Marxism is not a science in the positivist sense, formulating narrow hypotheses and testing for predictability, slicing up the rich tapestry of reality for microscopic examination. It is a science, or social science, that shows us how to conceptualize, systematically and systemically, moving from the particular to the larger forces (and back again), from surface appearances to deeper, broader things, so better to understand both the specific and the general, and the relationship between the two.

Despite the way Marxism has been misrepresented and attacked, and despite the relative lack of opportunity to pursue Marxist studies (and the penalties often attached to such pursuits), a growing number of intellectuals are reading Marxist literature and doing research from a Marxist perspective. Marxism has been dismissed as an obsolete 19th century "doctrine," yet it retains a compelling contemporary quality, for it is less a body of fixed dicta and more a method of looking beyond the appearances of social reality to the forces that shape social relations and history. Besides providing a brilliant explanation of how capitalism works, Marx helps us to understand what capitalism is in its essence. And to do that he had to pierce the facades of such everyday concepts as "commodity," "labor," and "capital."

This is why, despite the passage of time, Marx's *Capital* is still relevant in a way that Adam Smith's *Wealth of Nations*, for all its engaging content, is not. Unlike Smith, Marx realized that what capitalism claims to be and what it actually is are two different things. To understand capitalism's economics, one first had to strip away the appearances presented by its ideology.

It occurs to me that if Marx and Freud have anything in common, it is their notion that human experience is profoundly influenced by forces far removed from immediate discernment. For Freud, it was the individual's past years; for Marx, it was the imperatives of political economy and social history. To the question of whether Marxism is scientific, one might respond that it depends on what is meant by "scientific." The positivist task, which in the name of empiricism often rivets its attention on immediate factualness, is quite a different undertaking from the Marxist attempt to penetrate appearances in order to see the inner qualities and moving forces of things. As Marx noted: "All science would be superfluous if outward appearances and the essence of things directly coincided." Indeed, perhaps the reason so much of modern social science is superfluous is because it settles for the rigorous tracing of outward appearances.

Marxists understand that capitalism is not just an economic system but a political and cultural one as well, an entire social order. Marxism is a holistic science in that it recognizes, rather than denies, the linkages between various components of the social system. When we study any part of that system, be it the mass media, criminal justice, corporate finance, Congress, defense spending, racism, sexism, the family, the medical profession, transportation, housing, overseas military intervention, elections, or whatever, we will see how that part reflects, within its own particularities, the nature of the whole, and how, in its specific and unique dynamic, it serves and is shaped by the larger social system -- especially the system's overriding class interests.

But our major news media and academic departments of our colleges and universities teach us otherwise, presenting reality as a scatter of events and issues or as something divided into distinct subjects that ostensibly bear little relation to each other. These events and subjects are then treated in narrow, limited ways that fail to give us any critical insights into larger systemic causalities or into the particulars themselves. From a Marxist perspective, life is no longer an unrelated string of happenings, an endless series of imponderables, as it is for mainstream scholars and news pundits. Consider a few specific political phenomena:

Intervention in Central America. For mainstream critics, U.S. foreign policy is all foolishness and confusion. "Why do we always end up supporting the oppressors?" The question is treated rhetorically. Were it to be answered with a Marxist analysis it would be discovered that U.S. policy is not foolish. It may be dangerous and unjust but it serves the moneyed class interests very well, those interests that know how to be well-served, at the expense of the American people and other peoples in the world. Because the reasons given in support of U.S. interventionism (defending democracy, guarding our national security, etc.) can be shown to be false, this does not mean the policy itself is foolish, nor that the policymakers are confused.

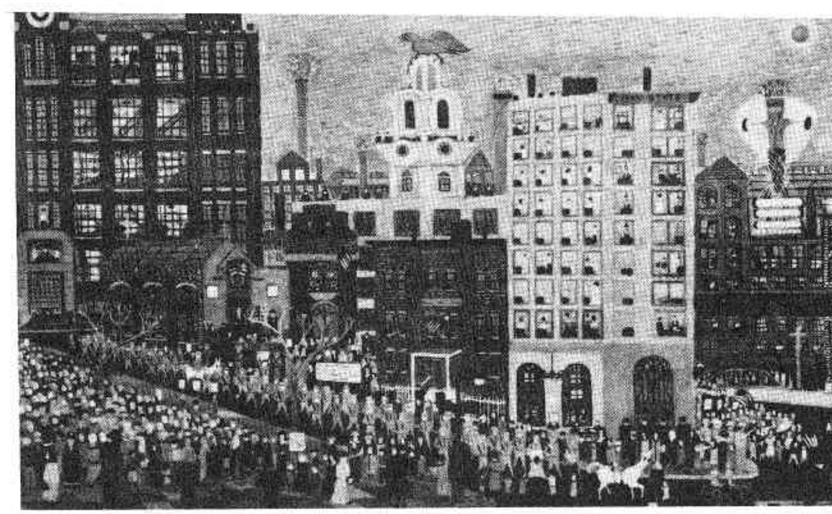
Nazism. For the anti-Marxists, Nazism just came upon the world like an aberration, a group of goose-stepping maniacs bent on murder and conquest. Certainly the Nazis were that, but what else must be said about them? What of the class interests they served, and the class interests they attacked once they came to power? Nazi nuts are always around. The question is who financed them and who used them for what ends? Who propelled them to the national stage and provided the means for their accession to power? Did not the irrational appeals of the Nazis serve the rational ends of the giant cartels and the business class?

Racism. For the liberals, racism is just a bad set of attitudes held by racists. Again there is little analysis of the social and class context which makes racism functional. Instead we are treated to discussions of whether the problem we face is "race" or "class," as if the two concepts are in competition and are mutually exclusive of each other. Those of us who think with Marx know that as class contradictions deepen and come to the fore, racism doesn't become less important but more important as a factor in the class struggle.

The conventional social science literature is riddled with such false questions: Is it cultural or economic? Or is it all a matter of individual psychology? And so forth. As if things can be compartmentalized so neatly. For instance, is an automobile an economic artifact or a cultural one? For bourgeois thinkers the world is an Aristotelian place in which all things are either "A" or "not-A." What is missing is any sense of how analytically distinct phenomena may be empirically interrelated and may actually gather strength and definition from each other.

Marxist social scientists generally do not accept the assumptions made by mainstream social science that social norms and values are self-generating, self-sustaining, consensual forces working upon us like disembodied Parsonian spirits. Rather such norms are usually mediated through institutions and are the products of institutional interests and class power relations. Marxists also do not accept the prevalent view of institutions--especially the more highly articulated structures such as the church, the army, the university, the press, the business conglomerate, the government--as just "being there," as it were, with all the innocence of mountains. Research done

Ralph Fasanella, *Lawrence 1912.* Oil on canvas 21x35.
From the traveling museum exhibit *Disarming Images* sponsored by the Bread and Roses Cultural Project of the National Union of Hospital and Health Care Employees AFL-CIO. Available as poster: Suite 1905, 330 West 42nd St., New York NY 10036. (212) 947-1944.



by Marxists, or by persons using a Marxist orientation, has revealed that far from being neutral and independent bastions, the major institutions of society are tied by purchase and persuasion, by charter and power to capitalist interests.

The business class exercises direct decision-making power through ownership and directorship over most of our "independent" institutions. They usually control the finances, budgets, and the very property of the institutions, a control inscribed into law and enforced by the police powers of the state. Their power extends to the managers picked, the policies set, and the performances of employees, including those--or especially those--who need to be censored for dissenting notions. Many social roles, then, are not "just the games people play"; they are resources of power defined and fixed by class-dominated institutions that are in turn linked to each other by the overriding imperatives and needs of the capitalist system.

If conventional social science has any one dedication, it is to ignore and deny the linkages between social action and the overriding imperatives of capitalism, *avoiding any view of power in its class dimensions, and any view of class as a power relationship.* For the conventional researchers, power is seen as highly fragmented and fluid, and class is nothing more than an occupational or income category (to be correlated with voting habits, consumer styles or whatever), not as a *relationship* between those who own and those who labor for those who own. In the Marxist view there can be no such thing as one class, no serfs without lords, no slaves without masters, no workers without capitalists, and vice versa. More than just a sociological category, class is a *relationship* to the means of production and to social and state power. This idea, so fundamental to Marx and to an understanding of social policy, is avoided like the plague by conventional social scientists who prefer to concentrate on everything else but class realities.

It is remarkable that political scientists, for instance, have studied the Presidency and Congress for over a century without saying a word about capitalism, without so much as a sidelong glance at the imperatives of the capitalist political economy that plays such a crucial role in prefiguring the political agenda, the national and military budget, the domestic program, and the policies of war and peace. It is remarkable that social science is cluttered with community power studies that seek to find out "who governs?" (not *what* governs?), treating particular communities as oddly isolated, autonomous entities, and particular issues the same way. These investigations are usually limited to the immediate interplay of policy actors, with nothing said about how the issues and the communities link up to larger power formations.

Conservative ideological preconceptions regularly prefigure the research strategies of those who think without Marx. In political science, for instance:

§ The relationships between industrial capitalist nations and third world nations are described as (a) "dependency" and "interdependency" and as fostering a mutually beneficial "development" and "stability" rather than (b) an imperialism that exploits the land, labor and resources of the weaker nations for the benefit of the favored classes in both the industrial and less-developed worlds.

§ The United States and other "democratic capitalist" societies are said to be held together by (a) a consensus of values that reflect some approximation of the common interest, or an interest-group pluralistic interplay that adheres to "the rules of the game," and not by (b) class power and domination.

§ The fragmentation of power in the political process is supposedly indicative of (a) a fluidity and democratization of influence rather than (b) the pocketing and structuring of power in unaccountable and undemocratic ways.

§ The inculcation of conventional political values and beliefs are described as (a) political "socialization" and "learning," and are implicitly or sometimes explicitly treated as a growthful, desirable process, rather than (b) an indoctrination that limits and warps critical democratic perceptions.

Mind you, in each of the above instances, the mainstream academics arrive at (a) not as a research finding but as an assumption that requires no critical analysis, and one upon which research is then predicated; at the same time they ignore the evidence and research that has been produced in support of (b).

By ignoring the moving forces and larger structures that exercise an influence over social behavior, the conventional social science can settle on surface factualness, on the false concreteness of trying to explain immediate actions in immediate terms. What is habitually overlooked in such research (and in our news reports, our daily observations and sometimes even our political work and political struggles) is the way seemingly remote forces may determine our experiences. Capitalism and its various institutional arrangements affect the most personal dimensions of our everyday life experience in ways not readily evident. A Marxist approach helps us see connections to which we were previously blind, to relate effects to causes, to replace the arbitrary, the accidental and the mysterious by the regular and the necessary.

Maybe one detailed example might suffice. A very compassionate friend of mine once trained as a nurse. In training, she had three patients to care for and she enjoyed every moment of chatting with them, tending their needs and lifting their grateful spirits. She felt she had a real knack for the work and she threw herself into it with much devotion and high spirit. But when put on a hospital ward she had 25 patients and could not keep up with their calls for assistance. She became anxious and then irritable and convinced that patients were expecting to be pampered. Soon she learned to ignore certain requests and found herself speaking sharply to sick people. The patients saw her behavior as a deficiency in her personal temperament, yet she was the same dedicated and conscientious person she had been before, with the same human nature as before.

You don't have to be a Marxist to know that people's behavior will change in different social settings. However, what might be overlooked in this story is that the hospital was controlled by a board of directors who drew huge salaries and extracted large profits for the corporate shareholders. (Most hospitals are run on a private profit basis, contrary to the common impression, and even the "non-profit" ones are usually milked by the top surgeons, directors and pharmaceutical and hospital supply companies.) So there had to be cutbacks in staff and one nurse on an entire floor would have to do. *Thus the interpersonal experiences of nurse and patient were deeply affected by forces not directly visible to either.*

To repeat: an essential point of Marxist analysis is that the social structure and class order (and the class struggle) prefigure our behavior in many ways, generating forces that may be intimately experienced even if remote from the immediate scene.

Is Marx relevant today? Only if you want to know why hospitals are often understaffed, dangerous, unpleasant, expensive places, and why the politician you voted for fails to serve your interests, and why your city is going bankrupt and public services are declining while the banks get richer and richer, and why U.S. forces and the CIA find it necessary to police so many regions and populations throughout the world and at home, and why there is a capital surplus but so many jobless people and so many things that need to be done, and why third world nations produce such vast riches while their people remain so poor, and why the media report things the way they do.

Marxism has explanations for all these things (some of which may not be perfectly complete and without need of redoing). Not all Marxists speak with one voice and one mind, but they know how to draw, rather than deny, connections between immediate experience and the larger structural forces that shape that experience. And this is why, for all the slander and calumny, the misrepresentation and suppression, Marxist scholarship endures and grows. It has coherence, relevance and a superior explanatory power.

> *An exercise for the reader.* Take a moment to think over the questions raised by Parenti in terms of your own experience in science . In what ways have you seen the profit motive result in misuse or corruption of science? Think how many times the damaging effects from capitalist manipulation of science, though "not directly visible," were later brought to light by accidental revelations or by courageous "whistle blowing." [Editor.]

Scientific socialism: the viable alternative

The forces operating in society work exactly like the forces operating in Nature: blindly, violently, destructively, so long as we do not understand them and fail to take them into account. But when once we have recognised them and understood how they work, their direction and their effects, the gradual subjection of them to our will and the use of them for the attainment of our aims depends entirely upon ourselves. And this is quite especially true of the mighty productive forces of the present day. So long as we obstinately refuse to understand their nature and their character -- and the capitalist mode of production and its defenders set themselves against any such attempt -- so long do these forces operate in spite of us, against us, and so long do they control us, as we have shown in detail. But once their nature is grasped, in the hands of the producers working in association they can be transformed from demoniac masters into willing servants. It is the difference between the destructive force of electricity in the lightning of a thunderstorm and the tamed electricity of the telegraph and arc light; the difference between a conflagration and fire in the service of man. This treatment of the productive forces of the present day, on the basis of their real nature at last recognised by society, opens the way to the replacement of the anarchy of social production by a socially planned regulation of production in accordance with the needs both of society as a whole and of each individual. The capitalist mode of appropriation, in which the product enslaves first the producer, and then also the appropriator, will thereby be replaced by the mode of appropriation of the products based on the nature of the modern means of production themselves: on the one hand direct social appropriation as a means to the maintenance and extension of production, and on the other hand direct individual appropriation as a means to life and pleasure.

-- Frederick Engels, *Anti-Duhring* (NY: International 1939) pp 305 f.

COUNTRY MATTERS

Summer nights a lit
candle is waxed fast
by Welsh farmers to
a tortoise's back

then lowered by string
down to a slate cave
where a hare hides its
brown fur, wet eyes.

The mist off the field
is green as a leek;
he waddles in deep,
his wax wand erect

casting wobbling light
his rear leg tethered
to the held breath of
night, bleak stars and air.

How like a wise man,
ponderous with care --
tortoise and candle
out hunting hare.

Kenneth Rosen
in *Light Year 84*
Robert Wallace, editor
Bits Press, Cleveland 1983
ISBN: 0-933248-02-4

A Scientist of Conscience

EDWARD LEE COOPERMAN, 1936-1984

NUCLEAR PHYSICIST Edward Lee Cooperman, professor and former chair, Dept. of Physics, California State University at Fullerton, was assassinated at work in his office on 13 Oct. 1984. He was a Corresponding Member of the World Federation of Scientific Workers, member and former chair of Task Force on Scientific Aid to Developing Nations of the Southern California Federation of Scientists, national chair of the Committee for Scientific Coooperation with Vietnam, and principal advisor for the UNESCO/UNDP Vietnam Project.

No one has yet been able to compile a complete list of all the activities in which he was engaged, nor of all the projects he was conducting. This was partly because of the extent of his work and partly because of his own self-effacement.

His Vietnam and UNESCO projects included many forms of ecology, agriculture, epidemiology, andrology, dentistry, chemistry, theoretical and experimental physics, mathematics, and law. He worked to convert the destruction of war into construction for peace as, for example, converting mosquito-infested bomb craters into fish farms. He worked to ameliorate the consequences of war. Ed arranged to make the expertise of Vietnamese scientists available to the U.S. veterans who sued Dow Chemical and the U.S. Government for damages resulting from Agent Orange, and to environmental groups concerned about toxic waste (dioxin) in such places as Times Beach, Mo. His work on Agent Orange was so effective that there was immediate speculation that it was the reason for his assassination.

He worked for peace and international cooperation, firm in the conviction that "cooperation in science leads to relaxation of international tensions." He worked for normalization of relations between the U.S. and Vietnam.

Probably his most dangerous activity -- considering the climate of terror in the expatriate Viet Nam community and the estimated 10,000 to 20,000 assassins from the CIA's Phoenix program who were given immigration priority -- was his help in secretly arranging for the repatriation of former high ministers in the South Viet Nam regime.

He was aware of the risks he incurred by his humanitarian work, especially after the end of the war, and more particularly after Ronald Reagan's 1981 order authorizing domestic CIA operations. It is inexpressibly sad that this gentle man who hated firearms and violence with an equal passion should have felt compelled to arm himself. He was very concerned, especially after the assassination of Vietnamese associates, but he expressed no fear. Rather, he treated the topic jocularly in conversation with colleagues and staff.

His mild manner belied his deep personal courage. During the Viet Nam war, for example, after James Register had been killed in People's Park in Berkeley, and after the police attacked the demonstrators at San Francisco State College during Ronald Reagan's tenure as governor of California, Ed was photographed by the press standing alone, facing the Tactical Squad of the police with their face masks and truncheons, his arms outstretched as he tried (unsuccessfully, it turned out) to protect the students and the university from the police.

Ed was especially sensitive to the threat of a repeat performance of the human tragedy of Viet Nam in Central America, and was turning his attention to that troubled area at the time of his murder.

-- Roger Dittman, Physics, Calif. State Univ. at Fullerton, *Sci. World* 29: 22-23; 1985. Abridged.

Wounded Knee: 1890-1973

I fear to see
where your body burns
leaving red
snakes of smoke
over the hills
like rainbows
arched between the ages
back to back --
past and prophecy
assume nothing
but harmony
in the gut --
startled to find
a bullet.

Will they let your hair dry
till its clay caves and obsidian shadows
pin you silent to the ground?
Will they crack your longbones
and scatter fragments marrow-sucked
to untrustworthy futures?
Will your fingers bleed
on the rotating blade
that scratches LIBERTY
onto your Indian-head?

Jeanette, Jancita, Yellow Thunder, Anna Mae
Jeannete, Jancita, Yellow Thunder, Anna Mae

Wendy Rose
The Halfbreed Chronicles and Other Poems
Los Angeles: West End Press 1985

The Search for Quantum Reality

Report on a conference: *New Techniques and Ideas in Quantum Measurement Theory*. New York Academy of Sciences, New York City, 21-24 January 1986.

Completeness or incompleteness? That's the key question which underlay the sometimes heated discussions at this gathering of today's "activists" in quantum mechanics. The phrasing of the conference title, with its emphasis on new techniques as well as new ideas, also no doubt attracted both speakers and audience disposed toward the further development of quantum mechanics. And the signal in the keynote address by Princeton's Eugene Wigner was an explicit message: quantum mechanics is incomplete. But relatively few of those present seemed conscious of the fact that all the reports of really new developments in themselves lend substance to Einstein's thesis that quantum mechanics is incomplete as a theory of microworld phenomena.

For example, papers by Anthony J. Leggett (U. Illinois, Urbana) and others argued that physical phenomena such as superconduction and tunneling reveal quantum effects (e.g., superposition) on a macroscopic scale [1]. This argument brought an indignant protest from the floor: "I thought we came here to talk about quantum mechanics." Also from the floor came a conciliatory explanation, to the effect that new ideas and new experiments were in order even though they departed from standard quantum doctrine. These two opposing tendencies persisted throughout the conference, inescapably reminding one of Thomas Kuhn's concept of *incommensurability*; clearly there was difficulty of communication here between the old and new mind sets concerning quantum interpretation. One participant commented that there were all sorts of semantic problems, with people talking through one another. To this observer, however, the misunderstandings seemed really due to philosophical differences of interpretation. How else account for the vehemence of disputes over the theoretical implications of experimental results accepted as objective by everybody! The meaning of quantum mechanics is clearly at issue and the physics community is split on this important question.

Wheeler poses the paradox. John A. Wheeler (Texas U. Austin) discussed three eras in the development of physics: The first era, characterized by Galileo's parabola and Kepler's ellipse, was a physics of motion but with no explanation of the motion. Second is the era of Newton's particle dynamics, Maxwell's electrodynamics, Einstein's geometrodynamics and modern chromo and string dynamics, with a physics of dynamical law but without explanation of the law. The third era, to come, will be one of deepened understanding of nature, especially of quantum mechanics.

"How can we understand the quantum principle?" he said, confessing, "I haven't any idea." And he stressed this basic ignorance several times. For example, after drawing a diagram of the poorly understood Bohm-Arahanov phenomenon (a phase shift that occurs in the twin-slit experiment when a

magnetic field is applied perpendicular to the plane of the slits), Wheeler said: "We not only have to learn what a quantum is but also what spacetime is."

Wheeler also seemed open to further development when he said that the quantum today seems "strange" but, when finally understood, will seem "simple, beautiful, inevitable," and we will exclaim "Oh, how could we have been so stupid." But Wheeler went on to assert that "determinism is out, quantum mechanics is in," thus failing to distinguish between the material effectiveness of quantum mechanical formulas and the idealist Copenhagen interpretation that denies causality at the microlevel. This confusion, shared by the majority present, tends to close the door of the mind against taking the materialist approach to a resolution of the mysteries.

The same confusion is apparent in the statement by one participant that "There are strange things in nature. The paradoxes are real and we have to live with them." No one was questioning the strangeness or the reality of the paradoxes but to accept the idea of living with them forever is to accept the death of physics.

The new arising within the old. Many papers revealed a striving to open that materialist doorway to deeper knowledge, though with varying degrees of consciousness concerning the nature of the conceptual revolution involved. For this observer, there were three papers of particular significance and clarity. One was primarily experimental. P. Grangier reported solid results, obtained with G. Roger and A. Aspect at Institut d'Optique Théoretique et Appliquée, Orsay, France, demonstrating conclusively that optical interference phenomena are properties of the individual photons rather than of interaction between photons [2]. Grangier, in the discussion, refused to draw philosophical conclusions from this experimental report. Later, in personal conversation, he confided that he thinks the photon is a quantum object that is sometimes a piece of the particle, sometimes a piece of the wave, depending on how you looked at it. (The implications of the experiment seem much more profound to this observer who has previously argued that the particle aspect of the photon is more fundamental than its wave aspect [3].)

The two other striking papers were more conceptual in nature. In one, Fritz Rohrlich (Syracuse U.) argued for a concept of quantum reality that accepts the existence of demonstrated quantum mechanical phenomena such as quantum blurring (uncertainty) and the possibility that this can be explained in terms of a more detailed structure that has so far escaped our observation. Rohrlich argued that such a postulate is very much in the spirit of Einstein's view that conventional quantum mechanics is incomplete, though adding that he did not know of even a suggested experiment that would provide justification for such a detailed structure.

The other key conceptual paper was by Franco Selleri (Università de Bari, Italy) who argued that today we are free to search for causal connection in quantum theory because all von Neumann-type theorems have been proved obsolete and, further, that the recent experimental tests of Bell's theorem (on the EPR paradox) may not prove Einstein wrong on local causality but simply demonstrate that some false assumptions were involved in the interpretation of results.

The paradoxes are sharpened. Informal taped interviews revealed wide disparities in perceptions of what was actually taking place at the conference. Most interviewees were experimentally inclined, and not all with equal grasp of the theoretical implications in the experiments reported. Two responses have been selected to show the wide disparities. The first is representative of the scientist with a purely technological orientation:

> This meeting has been great fun. There are several things interesting and new about it. There's the stuff that Leggett and Chakravarty [SUNY Stony Brook] were talking about. Interference between macroscopically different states, measured in squid magnetometers. It involves really difficult calculations--with the complication of quantum calculating, which is strange and mysterious, versus ordinary thermal activation. The fact that the experiments seem to bear out the calculations is interesting not only conceptually, in that it indicates quantum interference between macroscopically different states, but also because it is a really hard problem -- especially the problem of introducing friction into quantum mechanics, which people working in the area hadn't been able to solve.
>
> The problem with interferometry neutrons is also interesting in that the experiments are extremely beautiful. On esthetic grounds, it's quite lovely to do experiments like that. I don't think one learns new physics here, because existing theory tells you how the experiments should come out. Only people on the fringes of the subject would suggest the experiments could come out any other way. But the fact that these former gedanken experiments are being done in the laboratory is very beautiful. It's a peculiar art form that only highly trained professional people can appreciate. To some extent that's also true of 20th century music that only other composers can appreciate.
>
> As meetings like this go, the orthodox point of view is highly represented. I've been to other meetings that have been highly nonstandard and quite bizarre. There's a remarkably high percentage of extremely sensible people here. The evidence against Einstein's incompleteness claim is as strong as ever. This meeting has not changed that one way or another. Since Bell's theorem, there has been no compelling argument for the incompleteness of quantum mechanics.

The second response presented here is more or less at the opposite pole in epistemological approach:

> This field has attracted intelligent physicists since Bell's theorem on inequalities provided the basis for experiments and thus brought these matters out of the realm of speculative philosophy. That was a real leap forward. It startled some thinking people to realize that experiments could be addressed to the problems. It's true that this has only sharpened the paradoxes. You're talking to a disciple of Einstein: my own work is to try to make quantum physics describe his kind of reality rather than the Bohrian point of view. There are lots of points of view here, none of them experimentally refutable. All of these possible interpretations are sitting on a par, waiting for something to show us what the interpretation of quantum mechanics must be.

Judging from the conference papers, however, a goodly number of physicists had not been "sitting" or "waiting" but rather trying actively to help resolve the contradictions that beset quantum mechanics. Siding with them were the many participants who indicated in one way or another that they find Bohr's mystification to be conceptually offensive.

And one participant confided off the record that he thinks the Copenhagen noncausality interpretation to be not only reactionary but ideologically dangerous in today's world, and he considers his efforts toward a rational quantum mechanics to be the most important work he can do right now. I emphatically agree with this viewpoint on the politics of physics today. It is worth remembering what Ernst Fischer wrote (Austria 1937) in the heat of the struggle against Nazism:

> The ideology of fascism, concocted out of garbage and filth, was prepared for by the penetration of a stifling mysticism into the natural and social sciences. [4]

It really seemed that this conference helped substantially toward moving the physics community away from the dangers of that mystical ideology. Though the struggle is only beginning, Daniel Greenberger and other members of the Academy who helped organize this conference deserve the thanks of all for their contribution. [L.T.]

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[1] See Arthur L. Robinson, "Testing Superposition in Quantum Mechanics." *Science* 231:1370-72; 20 Mar 1986.

[2] "Experimental Evidence for a Photon Anticorrelation Effect on a Beamsplitter; a New Light on Single Photon Interferences." *Europhysics Letters* 1 (Jan 1986). Also see Arthur L. Robinson, "Demonstrating Single Photon Interference." *Science* 231:671-2; 14 Feb 1986.

[3] Lester Talkington, "Contradiction in Wave-Particle Duality." *Science and Nature* 2: 19-23; 1979.

[4] Ernst Fischer, "Capitalist Barbarism and Socialist Culture." *Communist International* Dec 1937 p962. Quoted in Maynard Solomon, *Marxism and Art.* Knopf 1973 p270. See also Paul Forman, "Weimar Culture, Causality and Quantum Theory, 1918-1927: Adaptation by German physicists and Mathematicians to a Hostile Intellectual Environment." *Historical studies in the physical sciences* 3: 1-115; 1971.

Erratum. Reference [4] was given incorrectly in *S&N* #2 p65.

From Natural Philosophy to Dialectics

It went without saying that the old natural philosophy -- in spite of its real value and the many fruitful seeds it contains -- was unable to satisfy us.....It contains a great deal of nonsense and phantasy, but not more than the contemporary unphilosophical theories of the empirical natural scientists...that there was also in it much that was sensible and rational is beginning to be perceived now that the theory of evolution is becoming widespread.....In his primordial slime and primordial vesicle Oken put forward as biological postulates what were in fact subsequently discovered as protoplasm and cell. As far as Hegel is concerned, in many respects he is head and shoulders above his empiricist contemporaries who thought they had explained all unexplained phenomena when they had endowed them with some power -- the power of gravity, the power of buoyancy, the power of electrical contact, etc., or where this would not do, with some unknown substance: the substance of light, of warmth, of electricity, etc. The imaginary substances have now been pretty well discarded, but the power humbug against which Hegel fought still pops up gaily, for example, as lately as 1869 in Helmholtz's Innsbruck lecture.....In opposition to the deification of Newtom...Hegel brought out the fact that Kepler, whom Germany allowed to starve, was the real founder of the modern mechanics of celestial bodies.....[Yet] natural philosophy, particularly in the Hegelian form, was lacking in that it did not recognise any development of nature in time, any "succession," but only "juxtaposition." This was on the one hand grounded in the Hegelian system itself, which ascribed historical evolution only to the "spirit," but on the other hand was also due to the whole state of the natural sciences at that period. In this Hegel fell far behind Kant, whose nebular theory had already indicated the origin of the solar system, and whose discovery of the retardation of the earth's rotation by the tides had already proclaimed its extinction. And finally, to me there could be no question of building the laws of dialectics into Nature, but of discovering them in it and evolving them from it.

But to do this systematically and in each separate department is a gigantic task. Not only is the domain to be mastered almost limitless; over the whole of this domain natural science itself is also in such a mighty process of being revolutionized that even people who can devote the whole of their spare time to it can hardly keep pace.

-- Frederick Engels (1885) *Anti-Duhring* (NY: Intl. 1939, 2nd preface, 16-17).

Nonstandard Analysis: A Revolution Under Way?

MARTIN DAVIS
Courant Institute
New York University

AN INTERVIEW WITH LESTER TALKINGTON. The subject matter of this interview is a paper, "Nonstandard Analysis," by Martin Davis and Reuben Hersh (University of New Mexico) that appeared in Scientific American June 1972. It deals with a new system of analysis created by the late Abraham Robinson, a logician at University of Toronto. Some main features of this system are summed up in a diagram and table on the facing page .

Q. After reading the paper that you wrote with Hersh I made a note to myself saying: "This entire discussion is in a delightful dialectical style." Were you conscious of using dialectical thinking?

A. I have certainly read Marx and Engels, and wouldn't be surprised if Reuben has too. I suppose it's bound to have had some influence. But if you ask what I think about all that now you will get a pretty negative reaction.

Q. Well, I'm concerned here strictly with philosophy of science.

A. That's what I mean. Some things Engels wrote on mathematics in *Anti-Duhring* are pretty terrible.

Q. Maybe we could come back to this later. It seems to me that making the infinitesimal once again respectable is a good example of development in which calculus history goes in an upward spiral and back to where it was, though at a higher level.

A. You can certainly put it in that scheme quite happily.

Q. But you haven't thought of it that way?

A. I don't see any special advantage to thinking of it that way but, sure, one certainly understands the idea of an infinitesimal now in a way that was hardly possible in the earlier centuries -- and then in the 19th century the whole idea of the infinitesimal was negated, as you might put it in your scheme.

Q. So now we have a negation of the negation. Not long ago I was told sternly not to think of a derivative as a ratio of infinitesimals. Now I am told that I can. Isn't this a revolution?

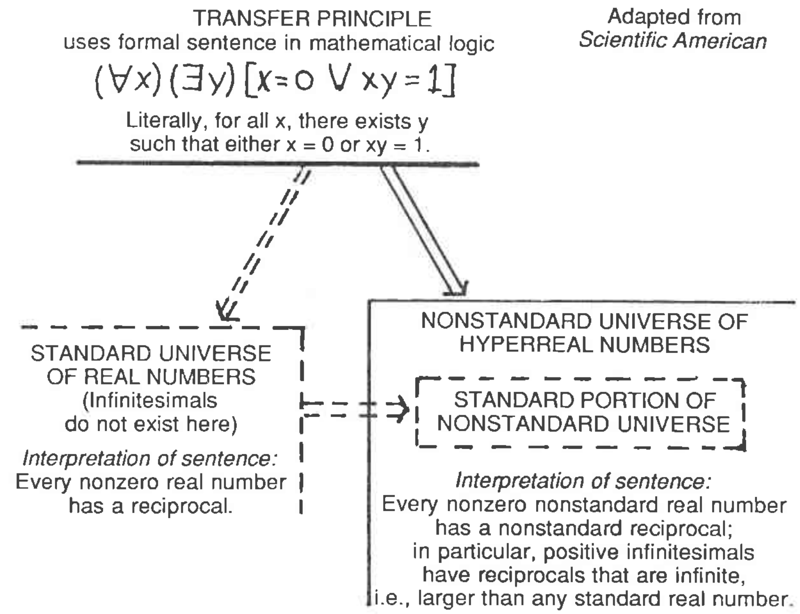
A. Yes, or think of it as differing from such a ratio by an infinitesimal.

Q. As I understand it, in the hyperreal number system you can manipulate infinitesimals arithmetically or even algebraically.

A. Absolutely. What was really so clever of Robinson was to see how the ideas of model theory in mathematical logic could give you an extension

Principles of NONSTANDARD ANALYSIS applied to calculus

Adapted from *Scientific American*



EXAMPLE FROM CALCULUS

THE FALLING STONE PROBLEM: Time $t = 1$; Position $s = 16\,t^2$

<u>Weierstrass (standard) analysis</u>

Δt is a positive real number.

Set $t_1 = t + \Delta t$.

$s_1 = 16\,(t_1)^2 = 16\,(t + \Delta t)^2$

$= 16\,t^2 + 32\,t\,(\Delta t) + 16\,(\Delta t)^2$

$\Delta s = s_1 - s = 32\,t\,(\Delta t) + 16\,(\Delta t)^2$.

$\Delta s/\Delta t = 32\,t + 16\,(\Delta t)$.

Given any positive real number ε, however small, we choose $\delta = \varepsilon/16$.

Then, for all $\Delta t < \delta$,

$\Delta s/\Delta t - 32\,t = 16\,(\Delta t) < 16\,\delta$

$= 16\,(\varepsilon/16) = \varepsilon$.

So, instantaneous velocity

$= \lim_{\Delta t \Rightarrow 0} \Delta s/\Delta t = 32\,t$.

<u>Robinson (nonstandard) analysis</u>

dt is a positive infinitesimal number.

Set $t_1 = t + dt$.

$s_1 = 16\,t^2 + 32\,t\,dt + 16\,(dt)^2$.

$ds = s_1 - s = 32\,t\,dt + 16\,(dt)^2$.

$ds/dt = 32\,t + 16\,dt$.

Since dt is infinitesimal, so is 16 dt.

Since 32 t is a standard real number, instantaneous velocity

= standard part of $ds/dt = 32\,t$.

(Note that in nonstandard universe the ratio of infinitesimals is a standard real number.)

of the number system to include infinitesimals and yet all the usual formal manipulative rules remain true. Not only algebraic. You can do trigonometry, anything you want, just as in dealing with ordinary numbers.

Q. You have the basis for manipulation at both ends of the scale?

A. Right. Abraham Robinson showed how you could embed the ordinary real number system in a larger structure, the hyperreal number system with infinite real numbers as well as infinitesimals. When you are working within the hyperreal system you don't even have to know you're there. In fact, you can't recognize infinitesimals or infinite numbers as such. The means for characterizing them are not present, which is what makes it possible to carry out all the ordinary manipulations. The trick is in moving in and out of the hyperreal universe. You can step outside and then say, "Oh, yes, those were infinitesimals I was working with. In this subject people distinguish between internal and external concepts. The infinitesimal is an external concept. As long as you're in the nonstandard universe, you can't distinguish an infinitesimal from other hyperreal numbers, but outside you can.

Q. Is this an example of different levels of manipulation?

A. I wouldn't put it that way. The two structures, I would say, are at complete equality with one another. You move back and forth from one to the other by means of something called the transfer principle. This principle says of any statement that can be written in a suitable formal language of symbolic logic, if it is true in one structure, it is also true in the other. If you want to be fanciful about it, imagine beings that inhabit the nonstandard universe who can talk to beings in the standard universe by telephone only if they use this particular language. And there's no way of saying in that language what an inifinitesimal is. If an inhabitant of the standard universe should say, "I think you live in a nonstandard world because youv'e got infinitesimals there," the response would be, "I don't see any infinitesimals here. What's an infinitesimal?"

Q. I have wondered if the infinitesimal isn't some sort of primitive, something that in the end you just have to accept rather than try to justify its existence logically.

A. But Robinson did justify its existence logically. That's exactly the nature of his achievement, to show that there is a systematic procedure by which you can replace argumentation that uses infinitesimals with argumentation that doesn't, getting your proof either way.

Q. How much has nonstandard anaylysis been used? How widely is it accepted in the mathematical world?

A. Not very widely. There are only a handful of enthusiasts, including an important school of nonstandard anaylsis in Strasbourg, France. But most traditional analysts tend not to have even learned it. Certainly it has not become a mainstream subject. There's basically the problem of articulation with mainstream mathematics. The students have to move up into advanced calculus that's taught the usual way, because that's how the system is set up.

Q. In the paper you state that "in a real sense we already knew what instantaneous velocity was before we learned [the Weierstrass] definition; for the sake of logical consistency we accept a definition that is much harder to understand than the concept being defined." Doesn't this imply that in the end mathematicians and physicists must surely come to accept the new Robinson definition because it is simpler and easier to understand?

A. I think so myself. But that's for history to determine.

Q. Is the resistance purely inertial, or is there also some ideological hostility involved?

A. It's complicated. I haven't found anybody who thinks that nonstandard analysis is wrong or invalid. Any hostility stems from a feeling that it's not going to help with our problems. You can say: "Look what Robinson and Bernstein did with the invariant subspace problem." And they say: "Well, Robinson is a very smart man. If he'd looked at the invariant subspace problem without nonstandard analysis, he probably would have solved it anyway." How can you answer that? He certainly was a very smart man. As I say, history will tell -- when young people come along who don't have this big investment from having learned analysis in the usual way.

There's a very interesting man at Princeton named Ed Nelson -- everybody agrees he's a topflight mathematician -- who is absolutely sold on nonstandard analysis. In fact, the Strasbourg school is very much the result of his influence. He has some definite philosophical concerns that you might find interesting. He's what I would call an ultra-positivist, refusing to make any kind of existential statements. Now there are a good many mathematicians who would philosophically rather not think of infinitesimals as something really existing but simply as concepts they can use and get valid results. For Nelson, however, it's not just the infinitesimals that do not exist; even real numbers do not exist for him. In one of his talks he said that physicists have long since given up on objective reality and only mathematicians continue to cling to it. So his way of expressing nonstandard analysis is ultra formalistic; there's nothing to it but just a new way of manipulating symbols.

Q. He solves the problem of two worlds by making them both nonexistent?

A. Right. But whether anyone can do the deep things Nelson does without at some level thinking existentially is another question. Afterwards you can always write up the results of the research in a purely formal way.

Q. That seems to me an important distinction. In other words, you think that the discovery of new things in mathematics has to have a materialist basis?

A. Yes, I think that's right. Archimedes left us a wonderful example. He anticipated the differential and integral calculus by solving problems that involved calculating areas for regions bounded by complicated curves such as parabolas. The way he first computed the areas was by the use of physics. For a parabolic segment, he imagined a thin body having this shape and computed the center of gravity using levers.

Q. He used levers literally or figuratively?

A. Figuratively. But then, after solving the problem as one of mechanics, he was enough imbued with Greek rigor to not regard this as a legitimate proof. So he proved it all over again by what he called the method of exhaustion which, in modern terms, is basically integration. For a long time there was a mystery because only the proofs by exhaustion were known. It's easy enough to see how these proofs worked if you already know the answer, but we had no clues as to where the answer had come from. Then somebody found the manuscript by Archimedes titled "Method" in which he told how he got the answers.

Modern mathematicians could learn from this example. There are some lecturers who will produce a beautiful rigorous exposition without a hint as to where the ideas come from, nothing about the usual rough and tumble of developing new ideas, some of which are not so precise, and all of which have been worked over in complicated and amorphous ways. The normal proof comes only at the end.

Q. I think that's often true in physics, too. Getting down to historical origins is important for understanding a mathematical or physical law.

A. It certainly adds a dimension. But it's partly a matter of taste. Some people find that going back to the origin of an idea helps them see it in a useful way. Others find that a distraction because connections are known now that weren't seen then, and things that once required complex and cumbersome calculations can now be done on a more conceptual basis. Some mathematicians work comfortably in a historical framework; others don't.

Q. Are there limits to Robinson's approach? I'm thinking about the requirement of formal logic in going back and forth between the standard and nonstandard discourse.

A. If you like, the transfer principle is a limit but it also gives the method its power. Without this limit, nonstandard analysis would be trivial, you would not be able to play upon the interrelations of the two systems. But using the principle skillfully is a new craft in itself. You learn what you can and cannot express in this formal language. You have to be tricky. Since you can't express infinitesimals directly, you sometimes have to replace a sentence with a weaker one which can be expressed in the language. Then, after the transfer, you find you can do something at the other side which will get you back what you want. That's the kind of manipulation that practitioners of the art learn to do.

Q. Do you need a new transfer sentence in mathematical logic for each new problem? Say, for each time you formulate a problem in calculus?

A. Not at all. Once you have established the transfer principle you are free to work algebraically in any way you choose within the nonstandard universe. For instance, to take an example from high school algebra, you can solve solve simultaneous equations or quadratic equations involving infinitesimals. Of course, if your problem involves some mathematical principle for which no transfer sentence exists, then you turn to the art of using this language.

One of the persistently amazing things about mathematics is the power of a formalism. It shows up in Robinson's contribution too. Formalisms always have a greater range of validity than the conditions under which they were derived. Somehow, by the time a truth has been written down in symbolic form, it has captured more than the conditions that produced it. This remarkable power is something that physicists can use freely; they can let themselves go, and not have to worry so much about the consequences as mathematicians do, because there's always the bedrock of experiment at the other end for a check on their work. We mathematicians don't have that check so we have to be a little more cautious.

Q. In your paper, for an example of "infinitesimal reasoning," you quote L'Hôpital's axiom that "two quantities differing by an infinitesimal can be considered equal," which means, as you point out, "the quantities are at the same time considered to be equal to each other and not equal to each other!"

An "abacist" (right) competes against an "algorist" in a 16th-century print

Doesn't this dialectical contradiction still hold in the standard world even though it has been resolved in the nonstandard world?

A. No, in the standard world there is only one infinitesimal and that has the value zero, so the symbols have only a formal meaning as the limit of a quotient. So you go over to the nonstandard world for a quotient of infinitesimals.

Q. You deal so fluently with concepts such as dialectical contradictions that I am puzzled. Why is it that I recognized the application of dialectical materialist principles in your paper with Hersh yet you do not feel they are any kind of guide in your conscious work?

A. The way I'd put it is that the so-called laws of dialectics are schemes or patterns that apply to all kinds of discourse. But I don't think they are particularly useful as a guide to making discoveries, and they're certainly not a useful guide in establishing their validity. You can talk about transforming quantity into quality from now to the end of time, but the real problem is to understand what happens in the process you're describing, what's happening in, say, the laws for a physical change of phase. I don't think dialectics is the least help for that.

Q. Engels, and many since, have said that you have to understand the science, the particular discipline itself, in order to think about it, dialectically or otherwise. Our journal is devoted to showing how materialist dialectics help, especially in the discovery process. Isn't it possible that in your creative work you may be using some of the ideas of Marx and Engels even unconsciously?

A. Who knows what I do unconsciously! It's certainly true that everything keeps changing and that the way things change is that they cease to have certain properties so that, when you're looking at that particular property, you see a contradiction. You say: "Look, what was this way has become that way." In Hegel's idealist view, things did not exist, they only had properties, so that seeing change as the alteration of properties was very natural. In that sense, everybody thinks dialectically. But this is not the use of laws in the same way as using the law of gravitation to predict an eclipse or the laws of logic to carry out a deduction. I see dialectics rather as a scheme that can be applied after the fact. And anybody can use such a scheme. If the Moonies wanted to use dialectics, they could write all their doctrines in dialectical terms.

Q. But not in dialectical *materialist* terms.

A. That's true.

Q. Dialectics by itself has no content. It's like mathematics.

A. I grant the point. But the union of the materialist outlook with the so-called dialectical laws can still be applied to any material system of things.

Q. That's what both Engels and Lenin said, that scientists in their work become spontaneous dialectical materialists, without knowing it. That's because these principles do reflect the processes of change in nature.

A. As far as I can see, the laws do not say anything more than that we live in a changing world. I don't see how they can help us look for the underlying laws that govern these changes, which is what's important.

Q. Lots of Marxists think they do in the social realm. Dialectical materialism was invented by Marx and Engels while they were studying the process of material change in society, and this philosophy has helped to further the process of social change. By the way, what were the absurd things you found in *Anti-Duhring*?

A. Engels was about 100 years behind the times on mathematics. For example, he said preposterous things about imaginary numbers. As far as he was concerned they existed as outright contradictions in mathematics itself.

Q. But didn't he joke about this later, referring to the "unrecognized genius of a mathematician" who complained in a letter to Marx about Engels' "wanton attack on the honour of $\sqrt{-1}$"? That was in the second edition of *Anti-Duhring*.

A. Engels' "joke" about $\sqrt{-1}$ is really not very funny. The "genius of a mathematician" evidently took the trouble to point out that, given the fact that "every negative magnitude multiplied by itself gives a positive square," this does not imply that "It is a contradiction that a negative magnitude should be the square of anything..." [*Anti-Duhring*]. It merely implies that a negative number cannot be the square of another negative quantity, so that $\sqrt{-1}$ can be neither positive nor negative (nor zero). But this is no more a contradiction than is the fact that many Americans are neither Democrats nor Republicans.

Q. Let's face reality. Neither Marx nor Engels (nor Lenin) was a mathematician or a natural scientist. They left a lot for us to do in these areas. Our journal is trying to do something about this. We want to show how the Marxist world view can really help scientists deal with the philosophical problems of their professional work. Give us a chance.

A. I don't question that any rational statement about the world, changing as it is, will fit into a dialectical scheme as well as a materialist one. But the critical question is whether anything is gained by consciously fitting things into a scheme of dialectics.

Q. I think most Marxists would agree with you that conscious retro-fitting is not the answer. That's what Lenin called "empty dialectics" [*Notebooks* p280]. But didn't you just agree that you yourself may be using dialectics unconsciously? Now how do you know that your conscious awareness of dialectics is not contributing unconsciously to your mathematical creativity?

A. You are right. By the very definition of "unconscious" I cannot claim to know (consciously) that I am not making unconscious use of dialectics or, for that matter, of the doctrine of transubstantiation. Again I want to say that Engels' errors and his "joke" about $\sqrt{-1}$ are not very funny.

At this point, both parties agreed to let the matter rest. For those interested in learning more about nonstandard analysis, Prof. Davis recommends Jerome Keisler's "Elementary Calculus" (Boston: Prindle, Weber & Schmidt 1976).

ADDENDA. (1) The troubled state of calculus was the subject of a recent special meeting of concerned educators at Tulane. Participants agreed that current curricula and teaching methods are a drag on both faculty and students (and, thereby, on the science and engineering professions), and that change is made difficult by entrenched textbook interests. However, judging from a two-page report in *Science News* (5 Apr 1986) and five letters from teachers and students (7 June), the possibility of simplifying the concepts of calculus by the conceptual approach of nonstandard analysis was not mentioned at the meeting.

(2) On rereading the interview it seems significant that Prof. Davis consistently referred to "dialectical laws," as in a formal context, while I referred to "dialectical principles," a concept more in keeping with the view of dialectical logic as inherently informal [cf. S&N #4: 77f, #5: 3-5]. [L.T.]

On Engels and the Dialectics of Calculus

We can now understand Engels' remark that the variable quantity introduced motion into mathematics, and with it dialectics and hence the calculus. This should not, however, be understood in the sense that mathematics before Descartes was undialectical. Engels himself observed that even the simplest mathematical relations show primitive dialectical structure. Indeed, multiplication originated as an abbreviated addition, division as an abbreviated subtraction, and we learn to consider a subtraction as an addition, a division as a multiplication [*Dialectics of Nature* 1940, pp 198f]. This last is already a clear case of a development in opposites, hence of a dialectical development, and it is certainly not a little game in which we amuse ourselves by calling John Peter, and Peter John. It is a mighty movement from the primitive to the complicated, from the simple to the difficult, from positive to negative, from integer to fraction, from rational to irrational, from real to imaginary, from the simple abstract images of real relations to the profound abstractions of modern mathematics, which yet are images of objective relations.

What Engels meant by his remark is that dialectics entered into mathematics in full force, in a formulation which does not use the more primitive dialectics of formal logic. Indeed, the foundations of the calculus, with its infinitesimals, will always remain a field where the laws of dialectics can be studied in great detail, so that even the most minute elements can be seen in a clear light. No wonder that it has been a favorite field for dialectical philosophers, such as Leibniz, Hegel and Marx.

-- DIRK STRUIK, "Concerning Mathematics." ***Science and Society vol I, pp 86f.***

It is of course clear that a mathematician, facing a problem, is not going to say, "Now I have to use dialectical materialism." He will get better results if he says, for example, "Now I have to use Lebesgue integration," or, "Now I have to get myself a drink." Philosophy does not replace scientific technique; it can, however, do much to clarify it. Personally, I always keep a dialectical kind of materialist approach in the back of my mind, whether I think of the difficulties of Cory Aquino's presidency, the relation of Marx to Thoreau, or the transfomation of mathematics in the days from Euclid to Newton.

Engels was not "100 years behind" even if he slipped up on $i=\sqrt{-1}$. Engels made more such blunders in his life, and we can learn even from those. The general position on math in *Anti-Duhring* is sound, as Aleksandrov correctly claims.* As for that $i=\sqrt{-1}$ business, when you consider the position of both Marx and Engels on math in the 1870s, don't forget that they were in England; except in algebra and geometry, England was far behind the Continent. They had no opportunity to talk to leading mathematicians, only to Engels' friend Samuel Moore who was just an intelligent layman. And there were enough *authorities* in England (such as the astronomer Airy, 1801-1892), who found $i=\sqrt{-1}$ so fantastic that they would not touch it ("pure imagination").

Do not ask me what I think of nonstandard analysis. I have not yet studied it. But, for me, it shows that, since Robinson *et al.* have every right to philosophize today about the foundations of the calculus, we cannot blame old Marx for worrying about them in the 1870s.

-- DIRK STRUIK, personal communication 1986.

*** A.D. Aleksandrov, "Mathematics: Its Essential Nature and Laws of Development."** ***Science and Nature*** **No. 3 pp 22-42.**

Applying Dialectics: An Example in Theoretical Biology

IRVING ADLER
North Bennington VT 05257

It appears that some general regulatory agent is at work producing common spiral forms in plants as disparate as sunflowers and pines.But what is this agent?...Why has the mystery of phyllotaxis not been adequately explained by now? Perhaps, [as suggested by] Gerald Alexanderson, chairman of the math department at the University of Santa Clara and a Fibonacci scholar, it is because, "biologists don't know enough math and mathematicians don't know enough biology." -- Braun [1986].

We have some news for Prof. Alexanderson, about a mathematician who studied his biology. -- Editor

Adler's mathematical analysis of phyllotaxis is "the most important breakthrough since the Bravais brothers [1837]" -- Jean [1983]

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Note: This is primarily a mathematical exposition. While one might wish that the philosophical content were more readily accessible, the effort of digging it out will be rewarded. For some, a quick scan of the historical matter (in larger type here) may provide motivation to tackle the mathematics itself, including the meaty appendices. Editor.

1. The botanical phenomenon of phyllotaxis

As far back as Leonardo Da Vinci it had been observed that the leaves are arranged in spirals around a stem. Similar spiral patterns are found in the florets of a pineapple, the seeds of a sunflower, and the scales of a pine cone. The laws governing these arrangements are known as Phyllotaxis. The scientific study of phyllotaxis began over a century and a half ago, initiated by Schimper [1830], Braun [1831], and L. and A. Bravais [1837]. My own contributions [Adler 1974, 1975, 1977a,b] will be discussed here to provide an example for the role of dialectical thinking in the discovery process.

Leaves around a stem are typically arranged in one or more parallel helices. The case of many helices is easily related mathematically to that of a single helix on which the leaves are located at equal distances like beads on a string. Where the leaves occur on a single helix, this helix is called the *fundamental spiral* [Fig. 1]. The angle of rotation around the central axis of the stem between successive leaf points along the fundamental spiral, measured the short way around, is called the divergence angle d.

The fundamental spiral is generally not conspicuous since, as it winds around the stem, the nearest neighbor to a given leaf may be, not the next one along the fundamental spiral, but one that is farther along. This is possible because, while the farther leaf is higher on the stem, it may be enough closer horizontally to more than compensate for the greater vertical distance. For example, in Fig. 1, leaf points 2 and 3 are nearest neighbors to leaf point 0 though point 1 is next to point 0 along the fundamental spiral.

The spirals that are conspicuous are those determined by joining a leaf (or scale, floret, etc.) to its nearest neighbors [Fig. 2]. These spirals are called *conspicuous parastichies*. [The latter term is from the Greek for parallel rows. Editor.] If the neighboring scales touch each other, they are called *contact parastichies*. There are two sets of conspicuous parastichies, one set going up to the left and the other set going up to the right. If the number of conspicuous parastichies going up the left is *m*, and the number going up to the right is *n*, the plant is said to have (m,n) phyllotaxis. On a typical pine cone, for example, the phyllotaxis is (5,8). On a pineapple it is usually (8,13). In a sunflower head it may be (34,55), (55,89), or even (89,144).

2. *Some mathematical background*

To describe the principal facts about phyllotaxis and the problems they posed, I will introduce certain mathematical concepts that were needed for stating the problems and finding their solutions.

Simple continued fractions. A simple continued fraction has the form

$$a_0 + 1 / [a_1 + 1 / [a_2 + 1 / [\dots a_i + 1 / [\dots \dots] \dots]]]$$

where the a_i are integers, $a_0 \geq 0$, and $a_i \geq 1$ for $i > 0$. Every positive real number can be expressed as a simple continued fraction. Successive approximations to the value of the simple continued fraction are given by the principal convergents obtained by usonly the first terms $a_0,\dots,a_n$ for n = 0,1,2,... and deleting the remaining terms. Each such principal convergent yields a fraction in lowest terms, p_n/q_n. Consecutive principal convergents are related by the recurrence relations,

$$p_n = a_n p_{n-1} + p_{n-2},\; q_n = a_n q_{n-1} + q_{n-2}$$

The Golden Section. If a line segment is divided into two parts so that the ratio of the whole line to the larger part is the same as the ratio of the larger part to the smaller part, then this ratio is called the *golden section*. If we use the smaller part as unit of length and let x be the length of the larger part, then $(x+1)/x = x/1$. It follows that $x^2 = x + 1$. and the golden section is the positive root ß of this quadratic equation.

$$ß = [1 + \sqrt{5}] / 2. \quad ß^2 = ß + 1 = [3 + \sqrt{5}] / 2.$$

$$ß^{-2} = 1/[ß + 1] = [3 - \sqrt{5}] / 2 = .382 \text{ approx.}$$

A well-known example of the golden section is the ratio of the diagonal to the side of a regular pentagon [Fig. 3].

The Fibonacci Sequence. In the year 1202 Leonardo Fibonacci of Pisa, in his book *Liber Abaci*, had a puzzle problem about a population explosion of rabbits, and found the solution to the problem in the sequence, 1,1,2,3,5,8,..., where each term of the sequence beginning with the third term is the sum of the two terms that precede it. Thus, the next term after 8 is 5+8=13, the next one after 13 is 8+13=21, etc. This sequence is now known as the *Fibonacci sequence*:

If F_n designates the n'th Fibonacci number, the definition of the sequence can be stated briefly as follows: $F_1 = F_2 = 1$; $F_{n+1} = F_n + F_{n-1}$ for $n \geq 2$.

Tying things together. There are important links that connect the Fibonacci sequence to the golden section and to the simple continued fraction expansion of the golden section:

a) The ratio of consecutive terms of the Fibonacci sequence F_{n+1}/F_n approaches ß as a limit as n increases to infinity.

b) The simple continued fraction expansion for ß is the simplest such fraction where all the terms a_i are equal to 1.

c) The principal convergents of this simple continued fraction are F_{n+1}/F_n. Other sequences analogous to the Fibonacci sequence can be formed by starting with any two numbers a and b and generating the rest by the same addition rule that generates the Fibonacci numbers, viz., add the last two to get the next one: a, b, a+b, a+2b, 2a+2b,... Examples that are relevant to phyllotaxis are 1,3,4,7,11,,...; 1,4,5,9,14,...; 2,5,7,12,19,.... Such sequences are called generalized Fibonacci sequences.

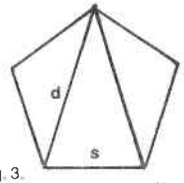

Fig. 3.
In a regular pentagon, d/s = ß, the golden section.
A math dictionary says that division of a line segment d into lengths s and d-s is often used since "it is considered pleasing to the eye." Editor.

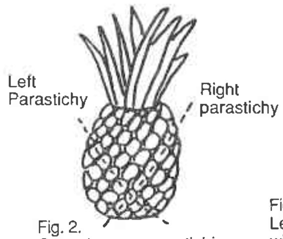

Fig. 2.
Conspicuous parastichies in a pineapple.

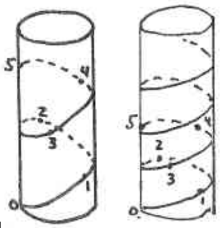
Fig. 1.
Leaves on a fundamental spiral with d = 2/5. Note that if the coils of the spiral are compressed, leaf 3 may be nearer to leaf 0 than leaf 1, though it emerges later and is higher on the spiral.

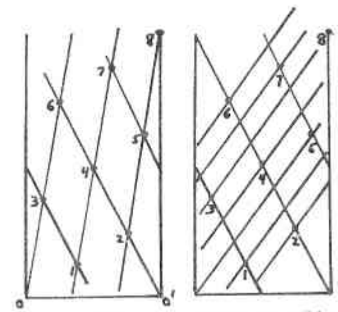
(2,3) is visible. (2,5) is not visible.

Fig. 4. d = 3/8.

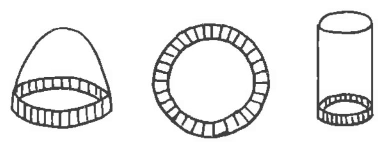
Zone of cone ⇒ Zone of disc ⇒ Zone of cylinder

Fig. 5

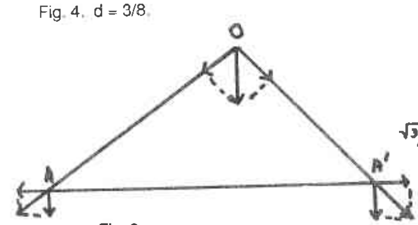
Fig. 6.
Schwendener's Force Diagram.

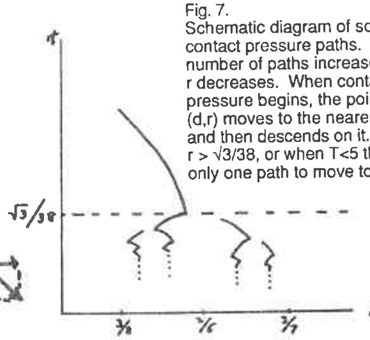
Fig. 7.
Schematic diagram of some contact pressure paths. The number of paths increases as r decreases. When contact pressure begins, the point (d,r) moves to the nearest path and then descends on it. When r > √3/38, or when T<5 there is only one path to move to.

3. *Facts to be explained*

The three principal facts about phyllotaxis for which explanations were sought are:

1) In nearly all species (about 96%), as a plant matures the divergence angle d, expressed as a fraction of a turn around the stem, converges rapidly to $d = ß^{-2} = .382$ turns appr., where ß is the golden section. Expressed in degrees, d = .382 (360°) = about 137°.

2) Again in nearly all plants, the numbers m and n that express the phyllotaxis are consecutive terms of the normal Fibonacci sequence. (There are exceptional cases in which the numbers are consecutive terms of one of the simple generalized Fibonacci sequences.)

3) As a stem with normal phyllotaxis grows to maturity, its phyllotaxis changes with the passage of time in a sequence of qualitative leaps from (1,2) to (3,2) to (3,5) and so forth, until the phyllotaxis characteristic of the mature stem of that plant is reached. On a sunflower head these qualitative leaps are also evident in the spatial location of the phyllotaxis. For example, if the numbers of left and right spirals near the rim are 55 and 34 respectively, part way in towards the center of the sunflower head there is a sudden transition to 21 and 34 spirals respectively.

4. *Key concept: The visible opposed parastichy pair*

The first problem I had to deal with was exploring fully the connection between the divergence angle and the phyllotaxis for any given constant divergence angle. This was a purely mathematical problem of properties of a point-lattice on a cylinder, where each lattice-point represents one of the biological units involved (leaf, scale, floret,, etc.). The Bravais brothers (1837) had obtained a partial solution to the problem. To obtain a complete solution, I introduced a new concept that had been overlooked by earlier investigators. The new concept was that of a *visible opposed parastichy pair*.

To see the significance of this concept we must examine some simple properties of a cylindrical point-lattice. As a reminder of the biological object it represents, I shall always refer to a lattice-point as a leaf. The leaves are numbered in the order of their appearance, starting with number 0. Slit the cylindrical surface along a line through leaf 0 parallel to the axis of the cylinder, and then unroll the surface on a plane to form its plane development. We then have leaf 0 appearing in two places, on the left and on the right of the plane development. Repeat the plane development in parallel strips indefinitely to the right and to the left. Then the cylindrical point-lattice becomes a plane point-lattice with each leaf appearing infinitely many times.

If, in one strip, we draw from leaf 0 a straight line going up to the left that joins it to any other leaf m, and extend this line indefinitely, then this line is (the plane development of) a left parastichy and contains the leaves 0, m, 2m, 3m, and no others besides multiples of m. If we draw the line through leaf 1 parallel to this parastichy, we get another parastichy containing leaves 1, m+1, 2m+1,... Similarly, for each other positive k<m, there is a parastichy that joins k, m+k, 2m+k,... We obtain in this way a set of m parallel left parastichies determined by the leaves 0 and m, with the property that every leaf lies on one of them, and the leaves on each one are at equal intervals. In the same way, by first joining leaf 0 with any leaf n by a line going up to the right, we determine a set of n right parastichies with the same properties. For any arbitrary pair of numbers m,n>0, we can obtain in this way a set of m left parastichies and a set of n right parastichies. We call them the *opposed* parastichy pair (m,n). In general, in an arbitrary opposed parastichy pair, there need not be a leaf at each point of intersection of a left parastichy and a right parastichy [Fig. 4]. In the special case where there is a leaf at every intersection of a left parastichy with a right parastichy, we call the opposed parastichy pair *visible*. In the case where all the leaves are on one fundamental spiral, if (m,n) is a visible opposed parastichy pair, then m and n are relatively prime (that is, their greatest common divisor is 1).

Further discussion of the role of visible opposed parastichy pairs is given in Appendix I.

The role of continued fractions. It can be proved mathematically that a conspicuous opposed parastichy pair is necessarily a visible opposed parastichy pair (see Appendix I). This implies that the simple continued fraction expansion of the divergence angle d is intrinsically related to the phyllotaxis that a stem may display. This fact was sensed intuitively by the earliest investigators of phyllotaxis. However, in the absence of an explicitly defined concept of "visible" opposed parastichy pair, and a full exploration of its implications, the exact connection between phyllotaxis and the simple continued fraction for d was not understood. This gap in the knowledge about the properties of phyllotaxis had some interesting consequences. On the one hand, some, like Braun [1835], impressed by the fact that the simple continued fraction for $ß^{-2}$ was the simplest one possible, thought of it as a kind of Platonic form that Nature strives to copy. On the other hand, Thompson [1952], unimpressed by such idealistic mysticism but not aware of the actual connection between phyllotaxis and continued fractions, denied that there was any. Sachs [1887] whose books on botany were standard reference works for decades, went even further, and rejected the entire mathematical theory of phyllotaxis as "a sort of geometrical and arithmetical playing with ideas." However, the problem of phyllotaxis didn't go away, and people continued to try to solve it.

5. *More on mathematization*

Surfaces. The phenomenon of phyllotaxis is observed on several different kinds of surface. The surface may be approxiately a *cylinder*, as on a pine cone, a pineapple, the trunk of a palm tree, and a mature stem. It may be a *disc*, as in the head of a sunflower, or in a transverse section of the growing tip of a stem. Or it may be a surface of revolution that is approximately *parabolic*, as in an asparagus tip or an artichoke or, in general, the growing tip of any stem. The first investigators of phyllotaxis [e.g., Schimper 1830, Braun 1831], studied the phenomenon as it is displayed on a mature stem and, consequently, viewed it as occurring on a cylindrical surface. The third generation of investigators [e.g., Richards 1948, Snow 1962], convinced that the origin of the phenomenon should be sought in the growing tip of the stem, began to study the tip via transverse sections and consequently, pictured phyllotaxis as a phenomenon on a disc. However, a disc and a parabolic surface are easily transformed into a cylindrical surface by means of appropriate formulas. In my own investigation I chose to return to the cylindrical picture for reasons explained below.

Parameters. Where there is only one fundamental spiral, the leaves emerge on a stem one at a time at approximately equal intervals of time. The time interval between consecutive leaves is called the *plastochrone*. It is convenient to use the plastochrone as unit of time. The first parameter we need is the time T, measured in plastochrones from the time that the first leaf emerges. If the leaves are numbered in the order of their appearance, starting with leaf 0, then leaf T emerges at time T, and the number of leaves present at time T, when T is a whole number, is T+1.

Phyllotaxis may take the same form on cylindrical surfaces of different sizes. Since the size of the surface is not relevant to the phyllotaxis displayed on it, we eliminate it by taking the girth of the cylinder as unit of length. The resulting cylinder is then said to be *normalized*. On a normalized cylindrical surface, the distance on the cylindrical surface between leaf number i and the one that precedes it along the fundamental spiral, namely leaf number i-1, can be resolved into a horizontal component and a vertical component. The horizontal component is the divergence angle between leaf i-1 and leaf i, and is designated as d_i. The vertical component is called the *rise* between leaf i-1 and leaf i, and is designated as r_i. If the d_i are the same for all i and the r_i are the same for all i, then the subscripts may be dropped, and the state of the phyllotaxis system is determined by the ordered pair (d,r).

On a disc, the parameter d_i also appears as the divergence angle between leaf i-1 and leaf i. Another parameter, the *plastochrone ratio*, is defined as follows. Let μ_i be the distance of leaf i from the center of the disc, and μ_{i-1} the distance of leaf i from the center. Then R_i, the plastochrone ratio for the pair of leaves i and i-1 is μ_i/μ_{i-1}.

Since phyllotaxis on a surface of revolution is studied via disc-like cross-sections, the same parameters d_i and R_i occur there. However, the zone on a surface of revolution between leaves i-1 and i is approximately conical. If this zone is slit and unrolled on a plane it becomes only part of a zone of a disc. To make it a complete zone, it must be stretched by an amount that depends on the angle $Ø_i$ between an element of the zone of the conical surface and its axis. This leads to the notion of *equivalent plastochrone ratio* obtained from the observed plastochrone ratio by multiplying by a correction factor.

6. The normalized cylindrical representation

The variety of surfaces on which phyllotaxis may be seen is a complicating factor. Another complicating factor is the fact that on the growing tip of a stem the older regions of the stem have a greater girth than the younger ones, and older primordia are generally larger than younger ones and grow faster. All these complications can be eliminated by replacing each surface by its normalized cylindrical representation obtained in the following way: Normalize each horizontal zone of the surface between consecutive leaves by the appropriate mathematical transformation into a zone of a cylinder with girth equal to 1, and then stack the zones on top of each other to match the sequence of the zones on the original surface. This replaces all the different surfaces that are possible by one standardized idealized surface that can represent them all. The transformations that produce the normalized cylindrical representation of a surface of revolution are shown diagramatically in Figure 5. The formulas that effect the transformations are these:

Disc to cylinder: $r_i = \ln R_i / 2\pi$.

Surface of revolution to disc: $\ln R_i = \ln R_i' / \sin Ø_i$, where R_i' is the measured plastochrone ratio, and R_i' is the equivalent plastochrone ratio.

Surface of revolution to cylinder: $r_i = \ln R_i' / [2\pi \sin Ø_i]$

7. Quantitative change and qualitative change

As mentioned earlier, one of the facts to be explained is the sequence of leaps in the phyllotaxis of a stem as it matures. On the sunflower, for instance, the phyllotaxis is highest near the rim, and is lower for regions closer to the center, with sudden transitions from one phyllotaxis to another with decreasing distance from the center. Richards [1951] and Coxeter [1961] derived formulas linking the qualitative changes in phyllotaxis to related quantitative changes. In doing so, both first assumed that the divergence angle was $ß^{-2}$. Richards, using the disc picture of phyllotaxis, connected the phyllotaxis with the plastochrone ratio. Coxeter, using a cylindrical picture, connected it with the rise. The two formulas are, of course, equivalent. However, they give only a discrete set of values of the rise or plastochrone correlated with the discrete values of the phyllotaxis. These values were identified as being the measures obtained when the left and right conspicuous parastichies are orthogonal. These formulas have the virtue of suggesting that qualitative leaps in phyllotaxis to higher parastichy numbers are the result of a continuous decrease in the rise (or of the plastochrone ratio). However, the formulas have the following defects: 1) They are based on assuming that the divergence angle is $ß^{-2}$ and therefore leave unanswered the principal question of why and how the divergence angle converges to this value; 2) They do not give the values of the rise (or the plastochrone ratio) at which the transition from one phyllotaxis to another takes place, and offer no explanation of why the change takes place. Both of these defects are overcome in the contact pressure model developed in Adler [1974, 1977].

8. Earlier theories of phyllotaxis

The first studies of phyllotaxis were made on mature stems where the leaves were not in contact with each other. The first theory advanced to explain the spiral arrangement with divergence angle of 137° was a teleological one: God arranged the leaves this way in order to maximize the amount of light each leaf receives. After the appearance of Darwin's theory of evolution, the same unproved assumption was advanced clothed in a new theoretical metaphor: the observed arrangement was a product of natural selection which perfected the fitness of the plant by maximizing the amount of sunlight received by the leaves. In 1875 Wiesner claimed to have proved the relationship experimentally by using an artificial tree with litmus-paper leaves. Since then botany textbooks have referrred to the alleged maximization of light with divergence angle of 137° as "Wiesner's law," though a careful reading of Wiesner [1875, 1902, 1903] shows that he did not prove his "law" but only that upper leaves partially shade the lower leaves.

Many objections that can be raised against the theory that Fibonacci phyllotaxis is an evolutionary adaptation that maximizes the amount of light received by the leaves of a plant:

1) As indicated, the alleged maximization of light remains unproved.

2) It is not established that maximization of the light received gives a plant a competitive advantage since a) some plants that display phyllotaxis do not thrive in bright sunlight and b) plants have developed other means such as phototropism for increasing the amount of light received.

3) Some plant organs that provide the best examples of Fibonacci phyllotaxis, such as pineapple or sunflower florets and pine cone scales, do not involve either leaves or partial shading of each other.

4) An "explanation" that assumes that the plant's genes, developed by evolution, determine the phyllotaxis of the plant really explains nothing unless it explains why and how the growth process of the individual plant produces the Fibonacci phyllotaxis.

When the attention of investigators shifted from the mature stem to the growing tip of the stem where the embryo leaves (leaf primordia) first emerge, three new theories were proposed.

a) Schwendener [1878] proposed that, as primordia grow while in contact, the contact pressure pushes them apart, and that this mechanical force accounts for the convergence of the divergence angle to 137°.

To support his contact pressure theory, Schwendener developed a theoretical argument based on addition and resolution of mechanical forces. He pictured the leaf distribution as an arrangement of equal circles in the plane development of the surface of a cylindrical stem, Fig. 6.

Comparing the broken line AOA' to a roof, he argued that contact pressure operates like a vertical force pressing down on the roof at O. To determine the consequences of this force, he first resolved it into components in the directions OA and OA', the forces being transmitted to A and A' respectively. Then he resolved the forces operating at A and A' into vertical and horizontal components, thus obtaining a horizontal force at A pointed left and an equal horizontal force at A' directed to the right. He concluded that these forces push A and A' apart, compelling the distance between A and A' to grow while, at the same time, the vertical force on O pushes O down toward AA'.

He used two separate methods to examine the consequences of these conclusions: a) a series of ruler and compass constructions, and b) a simulation of the compression of the roof by means of a mechanical device. His final conclusion was that the divergence angle alternately decreases and increases, with narrower and narrower swings, and converges to 137°. Although this conclusion turned out to be correct, he had not succeeded in proving it because of a fallacy in his underlying argument. The foundation of his argument was the statement that A and A' are pushed apart by horizontal forces of equal magnitude and opposite directions, but he overlooked the fact that A and A' are one and the same point. Two forces with equal magnitude and opposite direction acting at the same point add up to zero, and a zero force produces no movement. Thus his conclusion that AA' grows in length remained unproved.

b) Richards [1948, 1951] proposed that the growing tip (a region free of primordia) and each primordium secrete a chemical inhibitor that diffuses outward in the growing tissue. As the tip of the stem grows away from the last primordium that had emerged, the region between them becomes distant from both. In consequence, the concentration of the inhibitor in this region falls steadily. A new primordium emerges, according to this theory, at the place where the concentration falls below a certain threshold value. This rule, it was alleged, places the new primordium 137° away from the last previous one to emerge.

At the time I became interested in this problem, most botanists accepted the Richards theory as established fact. However, I kept an open mind as I read all the relevant papers critically, searching for evidence and proof that might support any of the theories. My first observations were these: A) The theories seemed to follow the fashions. In the immediate post-Darwin period, the most popular theory was one based on natural selection. When mechanical explanations were popular, due to the obvious successes of mechanics in industry and astronomy, a mechanical explanation was sought. I determined to distinguish fad from fact and wish from accomplishment by a separate evaluation of the reasoning behind each proposed theory. B) The inhibitor in the proposed inhibitor theory was purely hypothetical. There was no chemical evidence for its existence. But this by itself did not invalidate the theory. My principal objection to the theory was that it was very *undialectical*; it assumed that the divergence angle between two consecutive leaf primordia was determined at the time that the later of the two emerged, and that *it remained fixed forever* after that. This supposed constancy of the divergence angle with the passage of time seemed very unlikely in a growing stem. It seemed to me that since such parameters as the girth of the stem, the vertical distance between consecutive primordia, and the distance between the centers of primordia in contact are constantly changing, that it is far more likely that the divergence angle is also constantly changing. The problem, as I saw it, was to determine how, *while all these change take place*, convergence of the divergence angle to a limit takes place nevertheless.

c) A third theory said that, as the growing tip of the stem grows away from the last primordium to emerge, a new primordium emerges in the first space that is big enough to accomodate one [Snow 1962]. Snow, like Richards, expressed his theory vaguely in words and never determined rigorously what the assumptions imply. (Richards called his theory a field theory, but never produced any field equations.) There were also several other theories not significant enough to merit attention here.

9. Developing a new contact pressure model

The contact pressure theory seemed a likely candidate for development since a)growth undoubtedly produced such a pressure on primordia in contact, and b) it was a theory of continuous change more in keeping with the continuous change evident in the growing stem. A statement by Schimper pointing to the *internal contradiction* in the way neighboring leaves affect each other seemed to provide the clue that was needed. Freely translated, Schimper said that, as leaf primordia grow, while there is a tendency for them to grow as far apart from each other as possible, they are also under compulsion to grow closer to each other. The clue provided by Schimper's dialectical insight became the basis for my reformulation of the contact pressure theory in a form that permitted its development with full mathematical rigor. The argument goes like this: A primordium and its nearest neighbor, which may not be in contact initially, will grow until they make contact. After that, as they continue to grow, the distance between their centers tends to increase. But this increase cannot continue indefinitely, since the primordia are confined to a finite space. Therefore a certain maximum distance is attained. I assume that, after this, the distance remains maximized though it is still variable.

Thus the assumptions on which my model is based are: (l) That leaf primordia emerge at equal distances on a fundamental spiral. (The basis for this assumption is left as a separate problem to be solved.) (2) That the minimum distance between primordia is first maximized at some time T_c, and remains maximized for $T > T_c$. 3) That r is a decreasing function of time.

Questions to be answered. We have already noted that as more and more primordia emerge on a stem, the phyllotaxis rises for a while to higher and higher numbers, e.g., from (1,2) to (3,2) to (3,5) to (8,5), etc. In each change, the lower of the two numbers is replaced by the sum of the two. The result is that, as the number of primordia increases steadily and the stem grows continuously, there is a sequence of qualitative changes in the phyllotaxis. This fact raises three important questions:

I. What are the underlying continuous changes that drive the process forward?

II. How do these continuous quantitative changes become transformed into successive qualitative changes?

III. Why does the change from one phyllotaxis to the next follow the addition rule that the lower of the two numbers is replaced by their sum? Since this addition rule generates the Fibonacci sequence, an answer to question III would help explain the occurrence of the Fibonacci sequence.

Answers supplied by the new model. Question I was answered in this way for primordia on a cylindrical surface: One continuous change on the stem is a steady decrease of the r of the vertical distance between successive primordia to the girth of the stem. This decrease of r occurs when the girth grows faster than the vertical distance between successive primordia. If the plant surface is disc-like or parabolic, and the appropriate transformation is used to produced its normalized cylindrical representation, then the corresponding value of r is easily calculated. In these cases it can be proved that r steadily decreases as the plant grows. A second continuous change that takes place as the plant grows is the movement away from each other of the centers of neighboring primordia until the distance between them is maximized. With further growth, this minimum distance between primordia continues to change, but can remain maximized. The interaction of these two tendencies, decrease of r and maximization of the minimum distance between primordia, provided the answer to question I. (A third quantitative, though not continuous, change is the steady increase in the number of primordia.)

To obtain answers for questions II and III, leaves were numbered in the order of their appearance, starting with zero, and propositions were proved as follows: If the two leaves nearest leaf zero are leaves a and b, then the phyllotaxis is (a,b). If the distance between leaf 0 and the nearest of the two is maximized, then leaves a and b become equidistant from leaf 0. Then, if maximization of the minimum distance between leaves continues as r decreases, leaf number a+b approaches leaf 0 until all three leaves, a, b and a+b, are equidistant from leaf 0. With a further decrease of r, if a<b, leaf a+b comes nearer to leaf 0 than leaf a, and the phyllotaxis thus changes to (a+b, b). For more details on this investigation, see Appendix II.

10. The phase-space diagram

What happens when the minimum distance between leaves is maximized is best visualized by means of a diagram in the relevant phase space. The state of the phyllotaxis of a stem is given by two variables: d, the divergence angle, and r, the ratio of the vertical distance between consecutive leaves on the fundamental spiral to the girth of the stem in a cylindrical model. If there is no contact pressure, these two variables are independent of each other. The state of the system may be represented by a point P in the (d,r) plane. If the condition of maximization of the minimum distance between leaves has been attained, so that there is contact pressure, the variables d and r ceae to be independent. Then, as r decreases, d becomes a function of r. In fact, P is then constrained to move on a circle whose equation is given by the condition that, if p and q are the leaves nearest leaf 0, then p and q are equidistant from leaf 0. P moves on this circle until leaf p+q also becomes as close to leaf 0 as leaves p and q. Then, as r continues to decrease, P is constrained to move on a new path, the circle whose equation is given by the condition that leaves p+q and q are equidistant from leaf 0. For example, when leaves 2 and 3 are the leaves nearest leaf 0, the point (d,r) must lie on the semicircle

$$(d - 1/5)^2 + r^2 = (1/5)^2, \; r>0 .$$

When leaves 3 and 5 and the leaves nearest leaf 0, the point (d,r) lies on the semicircle

$$(d - 7/16)^2 + r^2 = (1/16)^2, \; r>0 .$$

The transition from (2,3) phyllotaxis to (5,3) phyllotaxis takes place at the intersection of these arcs. Before the transition, d is increasing. After the transition, d is decreasing. The path that P follows is a zig-zag path made of arcs of smaller and smaller circles. P oscillates back and forth as it descends, each oscillation covering a smaller range of values of d, so that d converges to a limit. This zig-zag path is called a *contact pressure path*. Note that this contact pressure path shows in detail exactly how, while the divergence angle is continuously changing, it converges nevertheless to a limit.

The fact that, under contact pressure, progress to higher phyllotaxis proceeds by the addition rule from (p,q) to (p+q,q) goes a long way toward explaining Fibonacci phyllotaxis, since the Fibonacci sequence is generated precisely by this addition rule. However, q and p+q will be consecutive terms of the Fibonacci sequence only if p and q were already consecutive terms of the sequence. Under what conditions can we guarantee that p and q themselves must be consecutive Fibonacci numbers? The answer to this question is supplied by the following theorem: If $r > \sqrt{3} / 38$ or $T < 5$, then the phyllotaxis can only be either (1,2) or (3,2). In either case, the two terms of the phyllotaxis are consecutive terms of the Fibonacci sequence.

The zig-zag shape of a contact pressure path suggests the following metaphor to describe what happens under contact pressure. There are many vortices in the (d,r) plane, each of which is a contact pressure path. Only one path begins high up in the plane where r is greater than $\sqrt{3}/38$. At lower levels in the plane more and more contact pressure paths begin, so that the diagram looks roughly like Fig. 7. When contact pressure begins, that is, when the minimum distance between leaves is maximized, wherever the point P may be in the (d,r) plane at that moment, it is immediately drawn to the nearest attainable vortex. (Attainable means able to be reached by a change in d that increases the minimum distance between leaves.) The, as r decreases, the point P descends into the vortex.

The final result achieved in the rigorous investigation of this model is that 1) if maximization of the minimum distance between leaf primorida is achieved before $r < \sqrt{3} / 38$, or before leaf number 5 emerges, then the phyllotaxis is always given by consecutive Fibonacci numbers; 2) as r continues to decrease, the divergence angle behaves as predicted by Schwendener, alternately increasing and decreasing with smaller and smaller swings, and converges to β^{-2}. Moreover, if maximization begins late, the model shows convergence of the divergence angle to the exceptional values sometimes observed, so that the model serves to explain both the general phenomenon and the exceptions.

11. What the other models can do

It was still necessary to deal with the rival theories of Richards and Snow. This was done by constructing an explicit mathematical model for each of these theories, previously formulated vaguely in words, in order to deduce rigorously their implications. The result was a proof that, while each of these theories can explain why the divergence angle converges to a limit, neither of them can explain why the limit should be precisely β^{-2}. Each permits that limit to be any number in a continuous interval of values. This does not mean, however, that the theories of Richards and Snow have no role to play in a complete theory of phyllotaxis. The contact pressure model does not attempt to explain the initial placement of leaf primordia. It only explains how the initial placement changes with the passage of time as more primordia emerge and r decreases. The Richards and Snow theories each provide a possible explanation of the initial placement of the leaf primordia, and thus might serve as a complement to the contact pressure model.

The success of the contact pressure model as an ontogenetic model does not exclude the possibility of developing a valid phylogenetic model to explain the survival value of a leaf distribution in which the phyllotaxis is of the form (a,b) where a and b are consecutive Fibonacci numbers. Such a phylogenetic model has been constructed by Professor Roger Jean [1983] based on a concept of entropy defined for this context. There is still much more work to be done, both experimental and theoretical, before there is a complete theory of phyllotaxis.

This outline of the thinking that went into the development of my contact pressure model illustrates how dialectical principles can be useful in the process of scientific discovery. Three dialectical principles in particular played a role in the development of this model. 1) In a growing organism, all parameters are constantly changing. Hence, an explanation of the origin of the phenomenon of phyllotaxis seemed more likely to be found in the continuing process rather than in some single event whose effects are assumed to remain unchanged thereafter. 2) The key to change in a developing system should be sought in some internal contradiction in that system. In this instance the key idea that had to be analyzed and developed was Schimper's insight that leaves, scales or florets on a stem tend to grow apart while at the same time they tend to grow toward each other. 3) Continuous quantitative change can be the underlying cause of qualitative leaps. In this instance, two quantitative changes, the decrease in r and the steady increase in the number of primordia, combined to produce at intervals such sudden changes of phyllotaxis from one level to another as from (3,5) phyllotaxis to (8,5) phyllotaxis.

Appendix I. Contractions and extensions of a visible opposed parastichy pair

If (m,n) is a visible opposed parastichy pair and m>n, the pair (m-n,n) is called its *contraction*. If n>m, the pair (m,n-m) is its contraction. Any visible opposed parastichy pair (m,n) has only one contraction, either (m-n,n) or (m,n-m). When m amd n are relatively prime, we ultimately get by successive contractions an opposed parastichy of the form (t,t+1) or (t+1,t), where the two terms of the pair differ by 1. A question that has to be answered is whether or not these successive contraction of (m,n) are also visible. Going in the other direction, if (m,n) is a visible opposed parastichy pair, (m+n,n) is called its *left extension* and (m,m+n) is called its *right extension*. Note that a visible opposed parastichy pair has *two* extensions. If an opposed parastichy pair (m,n) is the final result of k successive extensions of a visible opposed parastichy pair (p,q), we call (m,n) *an extension of order k* of (p,q). A question that has to be answered is whether an extension of any order of a visible opposed parastichy pair is necessarily visible. These questions are answered by the following sequence of theorems proved in Adler (1974), assuming, without loss of generality, that the fundamental spiral is a right spiral:

I. The contraction of a visible opposed parastichy pair is visible.

II. If (m,n) with m,n>1 is a visible opposed parastichy pair, then (m,n) is an extension of some order of a unique visible opposed parastichy pair (t,t+1) with t>1.

III. The opposed parastichy pair (t,t+1) is visible if and only if $1/(t+1) \leq d \leq 1/t$.

IV. When the opposed parastichy pair is visible if and only if $x/y \leq d \leq z/w$, where x/y and z/w are in lowest terms, then its left extension is visible if and only if d is in the left segment [x/y, (x+z)/(y+w)], and the right extension is visible if and only d is in the right segment [(x+z)/(y+w), z/w], where the left and right segments are obtained by inserting the mediant (x+z)/(y+w) between x/y and z/w. A consequence is that the higher the order of the extension of a visible opposed parastichy pair, the smaller the range of values of d for which the extension is visible.

V. It follows that the opposed parastichy pairs that are visible for a given constant value of d are fully determined by the simple continued fraction for d. In fact, since 0≤d≤´, there is an integer t≥2 such that $1/(t+1) \leq d \leq 1/t$, and every visible opposed parastichy pair (m,n) is an extension of some order of (t,t+1), and the sequence of left and right extensions required to obtain (m,n) from (t,t+1) is determined by the terms of the continued fraction for d. [See Adler 1977a, p 66.]

Appendix II. Steps in the proof of the result of Section 10

The answer to questions II and III was obtained by proving the following sequence of propositions:

1) If p and q are leaves nearest leaf 0, and the minimum distance between leaves is maximized, then the distance from leaf 0 to leaf p equals distance from leaf 0 to leaf q.

2) The distance to a leaf nearest leaf 0 is the denominator of a principal convergent of the simple continued fraction for d.

3) (p,q) is a conspicuous opposed parastichy pair if and only if p and q are leaves nearest zero.

4) If (p,q) is a visible opposed parastichy pair with divergence angle d, and both p and q are denominators of principal convergents of the simple continued fraction for d, then they are the denominators of *consecutive* principal convergents.

5) A conspicuous opposed parastichy pair is visible.

6) If p and q are leaves nearest leaf zero, then they are the denominators of consecutive principal convergents of d.

7) If p and q are leaves nearest leaf 0, and, as r decreases, leaf s descends to where it too becomes a leaf nearest leaf 0, then p, q and s must be denominators of consecutive principal convergents of d. If they are designated q_{n-2}, q_{n-1} and q_n respectively, then $q_n = a_n q_{n-1} + q_{n-2}$. Moreover, if all are equidistant from leaf 0, then $a_n = 1$ and $q_n = q_{n-1} + q_{n-2}$.

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On the Origin of Language and Consciousness

JACINTHE BARIBEAU
Dept of Psychology
Concordia Univ. (Montreal)

A Review Essay.

Vietnamese philosopher Trân Duc Thao, in the book *Investigations into the Origin of Language and Consciousness* [1], presents his own materialist model for the role of signs and signals (semiotics) in the emergence of *homo sapiens* from the primate state. His model seeks to integrate current theories of child development psychology with the findings of anthropology on the relations between tools, work activity, social relations, language, and the objective ontogeny of consciousness of self, thus producing what he calls "the semiotics of real life." An added benefit from his endeavor is the introduction to English-language readers of the dialectical materialist tradition established by Soviet anthropologists A. Spirkin and V.P. Iakimov [2]. This impressive undertaking has a weakness to be discussed here: Thao's failure to free himself fully from his previous preoccupation with the idealist concepts of Husserl's phenomenology, so akin to Sarte's existentialism [3]. Since Thao's work has been quite independent of mainstream anthropology, his original theoretical approach has a freshness which warrants our attention. His writing combines mastery of scientific psycho-linguistics and anthropology with the finesse characteristic of phenomenological analysis.

In the three chapters of this book, the author attempts to apply a dialectical materialist method to the investigation of consciousness as it is objectively experienced in three aspects of its relation to the external world: 1) the indicative gesture as the original form of consciousness; 2) the development of the instrument and the birth of language; 3) the origins of the Oedipal crisis.

The first chapter of Thao's book is deeply indebted to Spirkin's anthropological analysis of "gestural indication". This act of pointing to an object was, according to Spirkin, the crucial initial moment which allowed pre-hominids to develop a progression of linguistic signs. From there, Thao gives an erudite anthropological analysis of the origins of primitive pre-linguistic signs, proceeding from the indicative gesture, to the development of self-recognition and self-reference in the process of reciprocal interaction and recognition of/with others. The next phase is the act of "echoic representation", Thao's original anthropological application of the Marxist theory of self-consciousness [4] to the concrete context of pre-hominid tribal daily activities and interactions. He then hypothesizes further stages of development leading to qualitatively different "flashes of consciousness", which he terms sporadic cognizance, individual cognizance and collective cognizance, and which constitute the praxical basis for the ideality of consciousness.

Analysis by levels

Another original contribution of Thao here is his demonstration of the correspondence of these three anthropologically-derived stages of cognizance to the concrete experience of consciously apprehending an object. At the first level, the real object appears as a given from the external world in its sensory image, as existing outside consciousness and independently of it. At the second level is the concrete perceptual experience itself, where the sensory image is "projected" unto the object, giving it meaning based on experience. It is thus perceived as an image of that first-level image, or the image of itself in itself.

At the third level appears, in a relatively confused form, the social image where the subject sees himself as others see him in a structure of reciprocity of actions and as others see the external world. Here the act of perception has its own image in the gesture of the others with whom the subject identifies, so that this image of himself which he finds in the others presents itself as within himself. According to Thao the phenomenological method concentrates its attention on the second level, the "experienced" image. It systematically ignores the first level of the "real" image and is never able to grasp the third social image because of its solipsist perspective. Here Thao uses scientific anthropological evidence to dispose of the phenomenological claim that introspection is the essential basis of knowledge of the psyche.

One problem with the first chapter is Thao's use of terms such as "flashes of consciousness" and "tendential images projected by internal gesture" (p. 25). Here Thao's attempt at a materialist analysis stumbles at the difficulty of translating concrete "experienced" aspects of representation and meaning into materialist neurophysiological terms. An example is this passage (p. 20):

> "In fact, the projection which constitutes this image starting from the outlined movements of the animal, is actually produced by the 'tendency' of these movements..., the psychic image has a tendential reality, so to speak,...it remains strictly non-material."

Though Thao's concept of the "tendential image" is elaborated from Spirkin's theory, he seems to overlook essential notions of Spirkin's theory based in the neurophysiology of the second signalling system. These notions are essential to understand fully the material neural basis of linguistic images and representation.

Since the language of the neurophysiology of higher nervous processes provides alternative scientific formulations of greater clarity to refer respectively to "consolidations of incidental association" or to "inner speech" [5], one may wonder why Thao kept the ambiguous references to "tendential images" in his text. It must be that the author had some reservations in using purely neurophysiological terms. I suspect that such reservations, which are common to many philosophers dealing with the difficult definition of mental images, have something to do with the fear of biological reductionism, a fear which might be accentuated in Thao's case by his phenomenological background. Perhaps the apparent "idealism" involved in such analyses of the "tendential image", etc., Thao terminates this chapter by paying his dues to Lenin's materialist analysis of mental representations [6], and equating it to his own interpretation.

Recapitulation of consciousness

In the second chapter, concerned with the development (ontogeny) of individual consciousness, Thao's materialist anthropological analysis of the development of the indicative gesture in pre- and early hominids provides an analytical grid against which he objectively traces the interaction of the three levels of reflection in the formation of the first operations of meaning in the consciousness of the human child. Here, Thao gives a highly complex analysis and integrative reinterpretation of Piaget's observations on the development of inner speech and thought in the child [7].

The ontogenetic phases proposed by Thao are the following: 1) The indicative sign (14 months) accompanied with the word (sentence); 2) the first signs of representation (14-18 months) where the child is capable of an enduring image of the object in its absence; 3) the developed indicative sign (13-18 months); 4) the signs of "syncretic" representation (16-17 months), where syncretic refers to the confused alternating between two representa-ions: the developed gesture imitating the motion of the object and the indicative gesture of the object (the "this here"); 5) the deferred imitation as an insistent syncretic sign of representation of the motion of the absent object; 6) the functional sentence, from its elementary forms to developed types; 7) the disengagement of the form and the birth of the name.

To support his phylogenetic theory of the syncretic sign as a turning point in the appearance of truly conscious representation, and in the absence of anthropological data on that specific anthropological era where syncretic signs are hypothesized to take place *(Homo Faber Primigenitus)*, Thao skillfully and ingeniously combines evidence from observations of children by Piaget's disciple, Gouin-Décarie, and her non-Piagetian interpretation of such findings [8]. This is done in parallel with an elaborate phylogenetic analysis of the slow differentiation from the signaling and signifying gestures of pre-hominids to the semiotics of *Homo sapiens* taking form in the tool-making process. Step 1: the development from natural instruments used by apes in the presence of the object of biological need to the preparation of instruments by anthropoids with a consequent generalized sensori-motor image of the instrumental function, in parallel to the necessary formation of the "indicative gesture". Step 2: the phase of adaptation and formation of habits linked to prepared tools in the genus *Praehomo* entails the capacity for "sense certainty" that comes with cognizance of the indicative sign (*Australanthropi*). Step 3: the elaborated instrument (Kafuan) requires a representation of the absent object of a biological need and leads also to Step 4: a syncretic representation of the instrumental shape and later to Step 5: the production of the shape of the useful part of the instrument (*Olduvian*). Step 6: finally, the production of an instrument with representation of its total shape is defined as a tool, thus marking Chellean man and the emergence of genus *Homo Faber (Pithecanthropus)*.

The following excerpt shows how creatively Thao integrates evidence from two fields of science to understand the development of thinking in the last phase, the formation of sentences: the use and making of tools taking place in the context of task-oriented social interaction brings about the sentence as the necessary consequence of the communication involved in such a process (pp. 73-74):

> Tool production implies the shaping of the whole of the raw material according to a total typical form... While the Olduvian chopper only requires from 5 to 8 cutting strokes on both sides of the edge, the Chellean biface requires several dozen well-ordered strokes, and for each stroke, the exact striking place, the direction and the force of the motion. Here the worker must be able to indicate to himself a series of operations which presupposes the differentiation of the verb..."

Enter Oedipus

The third part of the book is an essay, "Marxism and Psychoanalysis on the Origins of the Oedipal Crisis". Trân Duc Thao criticizes and reconstructs the hypothetical beginnings of the Oedipal triangle in its pre-Oedipal stage by developing the theory of the socio-historical forms of individuality. Thao's socio-historical model traces the Oedipal crisis to the social progression from what he calls animal "jealousy" to the suppression of "zoological individualism", as a condition for the formation of the first cohesive social group which is essential for the beginning of human production. According to Thao, the next stages develop with the transition from the communalization of women to the pairing family, involving the "reawakening of jealousy" and the emergence of the Oedipus complex.

In opposition to Freudian theory of the Oedipus complex, which essentially bases itself on descriptive notions of concrete biological facts such as instinct and pleasure, Thao attempts to grasp its concrete socio-historical determination.

Thao however does not criticize nor question the validity of the Freudian concepts of the unconscious and of the Oedipal formation or structure in the unconscious psyche. Simply assuming the existence of the Oedipal structure in the unconscious as a given, he attempts to find its socio-historical determinants and to support it with facts from anthropology. From Freud's point of view the Oedipal conflict is born of the abstract opposition between "Desire" and "Social Law." From Thao's point of view the Oedipus complex originates in the dialectical contradiction, historically determined, between two laws: on the one hand, the primitive law of the communalization of women which, in the amorphous and undifferentiated state of the first human society during the Chellean era, guaranteed, by the strict interdiction of jealousy, the necessary unity and solidarity for the beginnings of tool production. On the other hand there is the new law of the pairing marriage, imposed by the development of household industry in the Mousterian epoch. The old communal right to sexual freedom without restrictions becomes a hindrance to the development of productive forces and loses all social justification, now merely appearing as a simple individualistic claim. Such a claim in other circumstances could have been limited to particular cases without leaving any trace in heredity. Thao argues, however, that the coincidental circumstances of what he calls "the biological tragedy of woman", where gait changes to vertical locomotion, the consequent pelvic inadequacies, and the ensuing high mortality rates of women during that period, aggravated undisciplined jealous competition for the few remaining female partners, especially among the youth.

The contradictions of the two laws assumed the anguished form of sharp conflict between generations: older male-Fathers, experienced hunters and paired with female-Mothers, versus the young "bachelor-Sons," less experienced at hunting and often given the task of protecting the female-Mothers and children while the rest of the tribe was gone on expeditions. According to Thao, this generation conflict developed into a gigantic social generation conflict, as the competition for female-Mothers (since most females were mothers a a very young age) became embodied in what Thao calls "the language of real life". This introduced pre-linguistic contradictions at the unconscious level of language between sexual desire for women and for Mothers when almost all women surviving the 'biological tragedy' were necessarily playing a highly valued Mother role. Here Thao does not define by what mechanisms the translation of social relations becomes codified into an unconscious structure even less materially defined, which would have the property of being passed on through hereditary mechanisms to the next generations, in the form of pre-linguistic mental structures. At this point Thao's hypothesis is purely speculative and seems to endorse the idealist notion of Jungian archetypes and to rely on a highly Lamarckian understanding of evolution in a rather intrepid way. The author uses a very self-assured tone in proposing such a hypothesis to the point that he does not even warn the reader of the speculative character of such hereditary mechanisms and of the questionable factual nature of his anthropological interpretation.

Moreover, Thao proposes this hereditary mechanism to account for the perpetuation of a complex of unconscious feelings in the unconscious of today's child and today's adult neurosis. Thao's definition of the unconscious is the key to this remarkable reconstruction. He defines the unconscious as "the sedimented residue of the language of the transcended stages of human development" (p. 195). This definition of the unconscious appears very abruptly in the last pages of the chapter without further elaboration. The text would have gained very much if the author had used the conditional tense more frequently.

Another criticism is that Thao overemphsizes the evidence from family interactions to support his theory of the origins of Oedipus. This theory does not attempt to account for the psychopathological and clinical facts essential to the development of Freudian Oedipal theory.

One important weakness in Thao's psychoanalytic interpretation is his argument that the Oedipal complex is a deviation or impasse in anthropogenesis and that the social relations of the time allowed for a second more "healthy" solution in parallel to the "Oedipal" solution to the generation conflict. Thao describes the healthy way, as the "Path of affectionate identification without rivalry". As a support of this hypothesis, Thao proposes the "ambivalence" of social relations which developed in the endogamic community of the Mousterian period. Forgetting that such "ambivalence" is far from demonstrated, and just postulated in the first argument on the Oedipal relations, Thao develops a second hypothetical interpretation: on the one hand, owing to the lack of women, the "Sons" found themselves sexual rivals of the "Fathers", but on the other hand, because the communal economy has remained dominant, the immediate communal relation would have maintained between them an identificatior without jealousy, following the tradition "*inherited*" from the original Chellean community (p. 196). Thao continues his speculation with overcertain statements such as: "The same was *undoubtedly* true for the Mothers" (p. 196) who, on the one hand, because of their age, appeared as objects of desire to the Sons but, on the other hand, as mothers responsible for the fireplace and guardians of precious provisions for the community, could not fail to elicit respect in the Sons. Thao goes on, concluding "and it was undoubtedly that respectful identification of the Sons with their social parents which initiated them into the practice of developing personal

relationships". Here Thao places the anthropogenetic birth of "personal" relations, defined as the first "intersubjective" relations, so central to the development of human consciousness.

This speculation however makes very little sense without the central postulate that "healthy" attitudes of respect were "inherited" from the original Chellean community. Thus this second and parallel "healthy" social structure is also inherited and can only be as difficult to accept as the first Oedipal one by the non-psychoanalytic and critically-minded reader. Though Thao does not say it, the reader may wonder if a human's mental structure for "personal" relations is also inherited.

Both types of relations, Oedipal and healthy, may have taken place in reality, but why would Thao need to postulate such an improbable and fancy mechanism as hereditary transmission of unconscious structures to explain their alleged presence in today's societies?

To conclude, this book provides the elements of a creative answer to this old idealist-materialist conundrum concerning the origins of consciousness: if human consciousness presupposes representations, and if this consciousness emerges first with production using tools, and if the production of tools itself pressupposes representation -- that is, an image in the mind of the producer of what is to be produced -- then the conditions for the origins of human consciousness already presuppose the very form of consciousness which they are supposed to explain. Thao breaks this circle by asking the question in another way, compatible with the historical materialist account: by asking if representation, as an essential precondition of consciousness, itself has its genesis in still more elementary forms of pre-representational consciousness; by proposing that the latter existed prior to the fully human forms of production, and prior to the use of tools. These proposals are indeed extremely thought-provoking and will certainly open new avenues for the anthropological investigations of consciousness.

Notes and references

[1] Translated from the French by D.J. Herman and R.L. Armstrong. D. Reidel (Boston Studies in the Philosophy of Science). x, 214 pp. Originally published as *Recherches sur l'origine du language et de la conscience*. Editions Sociales, Paris, 1973.

[2] Iakimov, V.P., *The Origins of Man*, Moscow (1964). Spirkin, A., *The Origin of Consciousness*, Moscow (1960). (Thao lived in France for some years, where he also brought these Soviet authors to the attention of French readers.)

[3] In the 1950s, Thao wrote several seminal studies on Husserl and Marx including *Phénoménologie et Matérialisme Dialectique* (1951). In the 1960s, he tried to develop a phenomenological method, purifying it as much as possible from Husserlian idealism, in order to integrate it with materialist dialectics. The present book represents an about face by the author after having dismissed the possibility of such a goal. Reflecting Spirkin's theory of the origins of consciousness and language, and the semiological and linguistic studies derived from the model of Ferdinand de Saussure (*Cours de Linguistique*, Payot, Paris, 1915), Trân Duc Thao moved towards an elaboration of a truly dialectical semiology through criticism of Husserlian phenomenology.

[4] The historical materialist theory of self-consciousness is based on the social relations of reciprocity; in the activity of collective labor, the workers point out to each other the object of their common efforts. Each is thus alternatively, or even simultaneously, the giver and the receiver of the indication, both the one who guides and the one guided. In other words, each sees in the other a being similar to himself, making the same gesture, and it is precisely because he sees himself in the others that the enduring image of the social environment allows him, when alone, to take the point of view of these others who are his other self in order to point out the object to himself.

[5] Sokolov, E.N., *Inner Speech and Thought*, New York: Plenum (1972). (Sokolov provides a scientific neurophysiological analysis of the material basis of inner language.)

[6] In *Materialism and Empirio-criticism* (p. 51) sensation is defined as the simplest form of consciousness: "it is its immediate connection with the external world." In "*Philosophical Notebooks*" (p. 182) Lenin further explains that "Knowledge is the brain itself in its motion of thinking." Thus knowledge is not just a simple physio-chemical movement. It is a most complex cerebral neurophysiological movement taking the forms of signifying gestures and linguistic signs which are shaped by and reflective of the human forms of social interactions.

[7] Piaget, J., *The Child's Construction of Reality*, London: Routledge and Kegan Paul (1976). (Translated from *La construction du reel chez l'enfant*. Neuchatel: Delachaux et Niestle, 1937.)

[8] Gouin-Decarie, T., *Intelligence and Affectivity in Early Child-hood*, International University Press, New York (1964). (Translated from *Intelligence et affectivite chez le jeune enfant; etude experimentale de la notion d'objet chez Jean Piaget et de la relation objectale*. Neuchatel: Delachaux et Niestle, 1962.)

About Pavlov and consciousness

Since the broad question of tool-making and speech has long been of interest to Marxist psychology, it is useful to [mention] the seminal work of Pavlov upon which many more recent studies of the question have been built. Pavlov's work on animal physiology eventually led him to regard human speech, which he termed "the second signaling system," as a qualitative human attribute, different from anything possessed by animals:

> **When the developing animal world reached the stage of man, an extremely important addition was made to the mechanisms of the nervous activity. In the animal, reality is signalised almost exclusively by stimulations and the traces they leave in the cerebral hemispheres... This is the first system of signals of reality common to men and animals. But speech constitutes a second signalling system of reality which is peculiarly ours, being the signal of the first signals.**

These "signals of signals" enable humans to link together multiple sensory stimuli and subsume them by unitary verbal signals or "synmbols" which are the key to conscious life and the human psyche. The connections arising on the basis of words introduce a new principle of neural activity, that of *generalization* and *abstraction* from reality, Pavlov commented:

> **For man, the word is just as real a conditioned stimulus as any other that he has in common with the animals, but at the same time the comprehensiveness of words is such that they cannot be compared either quantitatively or qualitatively with the conditioned stimuli of animals. As a result of an adult man's previous life experience, words are connected with all external and internal stimuli that reach the cerebral hemiscpheres, words signalize and stand for all these, and can therefore evoke all the actions and reactions in the organism that the stimuli themselves produce.**

This higher level of signalling interacts with and subordinates the lower, shared with the animals, and permits humans consciously to modify their behaviour. Pavlov's achievement was to show that these generalised signals imparted a new quality into human orientation to the surrounding world -- namely, a more profound reflection of reality whereby its essential features and relations could be distinguished and systematized.

-- Charles Woolfson, *The labour theory of culture; a re-examination of Engels's theory of human origins*. Routledge & Kegan Paul 1982 pp 67-68.

The State of the Art of AI

UNLIKE the U.S. [chess] championship, in which a deathly silence reigns, this tournament is filled with conversation, occasional laughter, the rattle of keyboards and a continuing microphone commentary by adjudicator Michael Valvo, a flamboyant computer consultant and international chess master from Sedona, Ariz. "A weak move by black. The king is still too exposed and the doubled pawns on c5 and c6 continue to hamper the defense." Nearby a member of the Cray Blitz team exclaims to no one in particular, "That's funny, I thought it would play king to f3." An international master can still spot flaws in computer chess and programs still surprise their creators.

Throughout this final round of play it has been obvious that Hitech has the advantage over its rival; early in the game Cray Blitz has fallen into a zugzwang, a critical position from which any conceivable escape involves either a bad move or a loss of material. In this case Cray Blitz has been forced to structure its pawns badly. Hitech continues to exploit its advantage. By midnight it is all but over. ... The Cray Blitz team asks adjudicator Valvo for permission to resign. He suggests two more moves: if the Cray Blitz position is no better by then, the team may resign. It is not and they do. Hitech is North American champion and de facto king of computer chess. Although Cray Blitz is the official world champion (...and does not have to defend [the title] until June), Hitech's win, along with its three other tournament victories, is impressive. Hitech is almost certainly the world's strongest chess-playing computer.

-- A.K. Dewdney, Computer Recreations. *Scientific American*, Feb 1986 p 13.

ALL THE contenders have a level of prowess that lets them overmatch all but the top rank of human chess players. But they still make occasional blunders that mark them as computers. It was such a blunder that undermined Hitech in the final game, according to one of its operators, Murray Campbell of Carnegie-Mellon.

Playing a Queen's Gambit Accepted, Hitech proved blind to a passed pawn developing in an unexpected configuration. Its operators saw the problem coming, but Hitech did not recognize the passed pawn until it was too late to prevent it.

A drawn out ending followed, with Cray Blitz winning in 60 moves. Hitech needed only a draw for the championship. But it didn't know that.

-- *The New York Times* 17 June 1986.

AFTER its first two games in a five-game tournament, Cray Blitz, the defending champion, was in trouble. It had just lost a game to a lightly regarded opponent. But a mid-tournament correction -- the removal of four lines in a 28,000-line computer program -- saved the day and the title.....

Cray Blitz's initial problems stemmed from four lines that program developer Robert Hyatt, a graduate student at the University of Alabama in Birmingham, had inserted after testing some parts of his program on a VAX minicomputer and finding an apparent weakness in the way the computer evaluated pawn movements. But when the modified program was run on a

Cray supercomputer, which is fast enough to allow a much deeper search than a VAX, the effect was not unlike "putting glue on the bottoms of all the pawns," says Hyatt. That change probably led to the losses at last year's North American championship and in this tournament. But after the offend - ing lines were removed, Cray Blitz started playing like a world champion again. "The difference in its play was striking," says Hyatt.

-- I. Peterson, Blitzing to win at computer chess. *Science News*, 21 June 1986 p 391.

Varying viewpoints on artificial intelligence

PRO. A more fundamental error is the assumption that artificial intelligence is a dead-end street and that machines can never think. Never mind that this conclusion was reached by an organic processor that is funamentally no different than a silicon processor, or that this point has been argued repeatedly since the thirties and settled to the satisfaction of most computer science researchers.

-- Duane Vore, Letters. *PC World.*

CON. Labeling a medical diagnosis program "intelligent" is like calling a dictionary a poet. Just like its predecessors, *integrated* amd *user-friendly*. *artificial intelligence* is this year's buzzword. The technology-hungry market is gullible enough to believe promises with little basis in fact.

-- Alan Cooper, Letters. *PC World.*

REALIST. And now Borland introduces Turbo Prolog, the natural language of Artificial Intelligence.....you'll be able to design your own expert systems.....Think of Turbo Prolog as a high-speed electronic detective. First, you feed it information and teach it rules. Then Turbo Prolog "thinks" the problem through and comes up with all the reasonable answers -- almost instantly.

-- From an advertisement in *Byte* June 1986.

MARXIST: The modeling of certain aspects of thought activity [by computer] is no less impressive in its results than the modeling of perception and memory. At present there are machines that can perform such intellectual operations as proving geometrical theorems, translation from one language to another, or playing chess.

Cybernetic machines [computers] are extremely effective for modelling the characteristically human ability of formal logical thought. But human consciousness is by no means confined to such thought. It has a dialectical flexibility and accuracy in solving problems that is not conditioned by any rigid system of formal rules.

We must remember that man's ability to think is shaped by his assimi - lation of a historically accumulated culture, by his education and training... The richness of a man's inner world depends on the richness and diversity of his social connections. Therefore, if we wished to model the whole human consciousness, its structure and all its functions, it would not be enough to reproduce only the *structure* of the brain. We should have to reproduce the logic of the whole history of human thought, and consequently repeat the whole historical path of human development and provide it with all its needs, including political, moral, aesthetic and other needs.

-- *Fundamentals of Marxist-Leninist Philosophy.* Progress: Moscow 1982 p 96.

COMMENT: When thinking of artificial intelligence, It's best to put the emphasis on the "artificial." [L.T.]

INTELLIGENCE

explosion!
of mind and thought --
then the deed!
in ancient Greece
two millenia now past
the great god *Doubt* emerged
to challenge priest-kingship's
superstition and human sacrifice
belief in spirits-invisible
behind every unknown --
gods become human!
ridiculous unpredictable
erratic snappish laughing
merely humans on a grander scale

Doubt!

a Socratic distillation
for which he drank
hemlock
for his impiety
for this he died
not for overthrow of older gods
but for enthronement
of a new and strange god
incorporeal
but cerebral --
Doubt!
even today
Socratic dialogues
after more than two millenia
still practice that art
to prove *true* is not enough
prove it *not-false* as well!
doubt it!
the inception
of science
of proven perceptions
of the observable
the provable
let the mind sharpen
focus and never lapse!
ache!
press to limit
force cerebrum

spark, synapse!

Richard Cloke, *Earth Ovum*
Cerulean Press 1983.
ISBN: 0-917458-10-5

On the Foundations of Behavior: Marxist Approaches in Psychology

CLAUDE BRAUN
Psychology
Université du Québec à Montréal

DESPITE the well known fact that people disagree on the definition of the science of psychology, there is general agreement that the discipline comprises natural as well as social science components. Whether defined as the science of behavior or of mental life, psychology includes the investigation of both. There are problematics (problem formulations) in psychological research where social phenomena are either irrelevant or can be safely ignored. For example, the behavior of one neurotransmitter of one synapse of a flatworm conditioned to photic stimulation is a purely natural-science phenomenon in psychology. In human psychological research, however, most problematics do not allow methodologically, and even less theoretically, for discarding the social parameters. In fact, virtually no behavior of humans can be adequately theorized without taking into account the social mediation as well as the biological basis for its development within the individual .

In the essay, "The Part Played by Labour in the Transition from Ape to Man," Engels [1876] was one of the first to propose explicitly a three-way interaction between natural selection, social labor and ontogeny. Engels understood, for example, that the individual or ontogenetic development of language cannot be thoroughly explained unless direct links are established between it and natural selection and social labor. Engels realized that there is a hereditary apparatus for language in the human brain and that this apparatus is unable to function without social mediation. He went even further in realizing that its very function is none other than social exchange, with the particular ecological pressure of social labor acting as a primary determinant of its emergence within phylogeny.

This theoretical framework of Engels, whereby the articulation between natural and social science is drawn out to enrich our understanding of individual behavior, particularly in its development, is the inspiration for the present essay. More precisely, our objective here will be to bring out the largely unknown early attempts of Marxist research psychologists to interpret the three fundamental interconnected processes of psychological causality, namely phylogenetic, social and ontogenetic. These are considered fundamental because, aside from physical causality, they yield the most relevant explanatory and predictive power for the process under investigation, namely, psychological or learned behavior of the organism.

1. *Oparin's theory of anticipatory reflection*

In Oparin [1957], the Nobel laureate and Marxist biologist proposed a theory of the origin of life in whch he postulated that anticipatory reflection is a fundamental and universal property of living matter. The term refers to the universal property of genes of being pre-programmed to activate organismic motion patterns in isomorphic sequence with environmental sequences -- programs which are in fact "remembered" by the genes -- such that all life forms appear to actually "anticipate" sequences of future events. The notion of the "biological clock" is practically synonymous with the concept of life itself. Life forms tend to last because they tend to be selected when they endure. Increased durability of a life form necessarily supposes a machinery to assure the prolonged adaptability of the organism. Similarly, all life forms must deal with space. Organisms either displace themselves, parts of themselves or their offspring. Programs become integrated into organisms' genetic codes which deal more effectively, more systematically with such variables as terrain, distance, motion etc. Let's consider a final and most fundamental, universal aspect of mnestic (memory) representation within all forms of life, namely, gravity. All known terrestrial organisms are subject to gravity, and are adapted to it. A weightless terrestrial organism living in a weightless ecology would be a markedly different type of living being from anything we know. What is meant by mnesis of gravity? Virtually all life forms "remember" which direction is "up" and which is "down". This is not necessarily because they are endowed with eyesight (a later phylogenetic development). It suffices that they be endowed with weight receptors and a means of processing that information. In the case of small aquatic animals, magnetic orientation may suffice without recourse to gravity. Moreover, virtually no organism (even the most primitive) is indifferent to the vertical stratum constituting its ecological niche. It must somehow recognize how far "up" or "down" it is supposed to go. To recognize its stratum, it must have within itself some kind of model or memory trace, a mechanism capable of comparing the percept with the trace, and a decision-making mechanism.

A disciple of Oparin, the neurophysiologist Anokhin, as early as 1962 extended the notion of anticipatory reflection by specifying its form in psychic phenomena. Its essential characteristics are, he wrote: 1) its ontogenetically-acquired nature or neuronal conditionability; 2) its connective variability and plasticity; 3) its signaling properties which enable it quickly to compress slow sequences into brief codes. Anokhin devoted detailed chapters and essays to the elucidation of the continuity of genetic codes and neuronal signaling, thereby establishing a basis for understanding the emergence of primitive behavior. These ideas are presented briefly in Anokhin [1969], more elaborately in Anokhin [1975, in French].

2. *Spirkin's gestural theory on origin of language and abstract thought*

An important Marxist anthropological tradition began with Alexander Spirkin in the Soviet Union in the 1950s and radiated throughout France via translations and through a Vietnamese student of Spirkin, Trân Duc Thao, who resided several years in France and published frequently in organs of the French Communist party before moving on to Vietnam in the 80s.

By clever use of etymology, Spirkin showed that any word in any language, no matter how abstract, can be traced to a concrete object. In several languages, "calculation" contains a root referring to the stones used in primitive cultures to carry out arithmetic. "Matter" in Greek refers to fire wood, etc. Years before the American Sign Language was taught to a chimpanzee, Spirkin had been training apes to use expressive signs. He concluded from his experimentation that the passage from Pavlov's first signaling system (natural animal language) to the second (abstract communication by means of arbitrary signs) must include an intermediate phenomenon, namely," gestural indication". Pointing to something in view, to elicit a visual localization in an interlocutor, is the creation of a "tendential image". The "indicative gesture" is the most primitive incidence of a sign containing within itself a distinction between the external and internal, between the mental representation of the physical sign ("signifiant") and mental representation of the referant or external object ("signifié"), wrote Trân Duc Thao [1975]. A number of implications for a theory of the origins of consciousness are drawn in this essay as well as in Trân Duc Thao [1984].

3. *Vygotsky's cultural-historical theory of psychic development*

In the 1930s, a young philologist turned psychologist stood out in the Soviet Union as a representative of the historical-materialist theoretical approach to psychological phenomena. Rather than seeking to establish a bio-materialistic and experimental basis for a theory of psychology as had the dominant Pavlovian school, Lev Vygotsky theorized about the development of behavior in the context of its richly interconnected phylogenetic, ontogenetic, and historical determination. Vygotsky's ideas became the catalyzer for a school of thought which came to be termed the "cultural-historical" school of psychology. Vygotsky's most illustrious disciples included the neuropsychologist Alexander Luria and the theorist Alexei Leontiev. Not surprisingly, the former (neuropsychologist) came to be known in the west rather than the latter, while in the Soviet Union, the latter (theorist) enjoyed a wider audience and an even more illustrious reputation than the former. One of the few sources of Vygotsky's works in the English language is *Thought and Language* [MIT Press 1962].

Vygotsky proposed that whereas psychic control develops bottom-upward in the neuraxis, it operates top-downward later in life as it becomes mediated and regulated by language. The cultural-historical school of Soviet psychology considered language the royal route by which to understand consciousness, and in turn unconsciousness, rather than the other way around as in psychoanalysis. Vygotsky stated that human infants are born genetically programmed as social beings. He recognized, contrary to the dominant thesis proposed at the time by Piaget, that the child's speech does not proceed from egocentric or autistic speech to social speech but from social speech (imitative and self-regulatory) which is overt and expanded (according to known rules of grammar) to an abbreviated and internal form of speech. Up to that time, no distinction had been made between internal (mental) and external (acoustic, written, etc.) speech. Vygotsky and his students discovered through experimentation and observation that external speech becomes markedly condensed during the process of internalization according to specific rules of grammar. Luria further observed this process and reported that internal speech loses its nominative or "thematic" function and becomes more centered upon what is "new" and what "has to be done," it becomes "rhematic".

We can suppose that this linguistic property is also characteristic of the language structure of dreams, though to our knowledge this has never before been proposed. Still other approaches are more likely to inform us about the origins of the unconscious than the pseudo-clinical approach traditionally used in psychoanalysis. The conditioned visceral response approach to the study of the human unconscious, Uznadze's [1967] experimental approach to the unconscious attitude sets, and the cybernetic approach are examples of alternative approaches.

Marxist research psychologists recognize that the human unconscious develops via historically-framed conscious activity, i.e. practice. In this respect they oppose the psychoanalytic concept of the unconscious as a base from whch consciousness emerges primarily via sublimation and other mechanisms of transformation of fundamental instinctual drives.

Marxist scientists, not unlike other scientific psychologists, study many other unconscious phenomena besides the Freudian *lapsus linguae*, neurotic symptoms and dreams which, contrary to the above, can be observed systematically. The following section provides an example of such a line of investigation developed by a pioneering Marxist psychophysiologist.

4. *Bernstein's systems theory of historically-based movements*

In 1926, the Soviet physiologist of motor action, Nicholas Bernstein, published his *General Biomechanics*. Twelve years before the famous Norbert Wiener publication, Bernstein formulated some basic principles of self-regulatory systems and the role of feedback in the regulation of man's voluntary movement. In particular, he was interested in culturally-determined movements (which become automatized through learning), such as hand writing. The intricate complexity of the motor activity involved in handwriting, the unconscious expressive structure which underlies our written production, is recognizable as a style whether a person writes with mouth, hands or feet. Anokhin proposed the notion of *functional system* as a metatheoretical concept which applies to such phenomena in all physiological systems, i.e., providing redundancy and flexibility. Respiration, for example, is normally taken care of primarily by the diaphragm in human males. If this particular organ fails, the intercostal muscle will step in to do the job. Finally if both of these fail, the larynx will be used to gulp air and feed the lungs.

Bernstein knew that the complexity of human motor activity cannot be explained by learning theory alone. He drew extensively from biology and genetics to put together a systems model of complex action. He recognized for example the pertinence here of Oparin's concept of anticipatory reflection. But his own specific substantive contribution to the field was to introduce new cyclogrametric methods, probability theory and other complex mathematical methods into the theory of central motor control. He proposed that central motor systems are organized into hierarchical levels with each level characterized by its own degrees of freedom. External obstacles or interferences, which impinge upon the execution of a motor program, will call into action, if sufficiently secondary, reactive (passive) accomodation. However, more primary disturbances will elicit essential motor accomodation, active adaptation, including reprogramming during operation.

5. *Luria's three-tier theory of brain development*

Vygotsky's student, Alexander Luria [1973], produced the first extensive developmental neuropsychological theory in his book *The Working Brain*, providing an integration of clinical, developmental and experimental neuropsychology based on original research results as well as the work of others (primarily Soviet colleagues and collaborators). Luria's main theoretical contribution to neuropsychology was to integrate into a harmonious and extremely detailed empirically-supported theory the connections between the interpenetrating phylo- and ontogenetic and historical determinants of brain development. The integration, particularly of the latter aspect, into a concrete and extensive neuropsychological model of brain function and development remains unparalleled to this day.

The non-Marxist world is particularly unaccustomed to paying attention to this aspect of brain development even though it is an indispensable key to the understanding of its origins.

Let's consider a few general concepts and some examples provided by Luria. The brain can be envisioned as a system composed of functional subsystems which develop behaviorially and anatomically according to a basic sequence and are disposed in an interactive hierarchy composed of 3 tiers. The first tier consists of a unit for regulating tone or arousal. It is a reticulo-cortical (mesial) system whose functional integrity is a condition for the development of the next system. The second unit serves to obtain, process and store information. It is located in the posterior (post-Rolandic) area of the cortex. The third unit's function is to program, regulate and verify mental activity. It is the last to attain maturity and is located in the anterior (pre-Rolandic) cortex.

The historical dimension of the first unit or system is that attention arousal and orientation to stimuli can be regulated, and in some cases essentially defined, by language and cultural interchange. Vygotsky and Luria were the first to provide concrete experimental demonstrations of this phenomenon in normal and impaired children and adults. An individual who cannot generate brain tonus or pay attention obviously cannot memorize experience and is unable to plan and organize his life on the basis of stored experience. Thus, historically-determined social life is essential for the functional and anatomical integrity of even the first tier of brain organization. Vygotsky and Luria, following Engels, recognized that man is an animal genetically programmed for cultural acquisition of behavior.

Cultural-historical determination in the development of the second tier of the human brain was demonstrated in numerous ways by Luria. A good example is Luria's "law of progressive lateralization" which states that the higher the perceptual function, the more likely it is to be lateralized within one or the other cerebral hemisphere. The prime factor of the emergence of such brain lateralization, Luria explains, was labor. Though there are a few exceptions, animals are generally not lateralized for perceptual functions.

The third tier of the human brain, the frontal lobes, also contains mechanisms that could not develop without an intimate relationship between the individual and human civilization. Let's consider the single most important example: one reason that humans are able to accomplish prolonged complex mental tasks is because they have a prop, namely language. Language, internal speech in particular, supports memory, provides implicit and rapid means of categorization, helps organize plans into sequentially meaningful strategies. Inner speech is a condensed rhematic form of speech. It functions not only as a mental prop, but as a prototype for the building of neural connections of the tertiary zones of the frontal lobes.

6. Wallon on the origins of emotion and of social life

The most important historical figure in French Marxist psychology, Henri Wallon, developed in the 1930s a sophisticated theory of early emotional development. Despite its importance, none of his works have been translated into English and his discoveries remain totally unknown outside

continental Europe. Unfortunately, this forum does not permit a detailed exposition of his theory, so the wrong can only be very partially righted here.

Psychophysiology up to this day has interpreted emotion as a corporeal "alarm" or "arousal" response (early protagonists included James, Cannon, Lindsley, Papez and Maclean). Similarly, radical behaviorism (Skinner and his followers) views emotion as an epiphenomenon barely worthy of scientific investigation, effectively preventing any serious attempt to understand the origins of human behavior.

Wallon was the first to realize that the function which unifies the vegetative and muscular manifestations of rudimentary emotion in the child is posture. He noted that visceral and proprioceptive functions mature much earlier than gestural and exteroceptive functions in man. The neonate has active labyrinthic and cervical reflexes, equipping it to orientate and assume postures, even rudimentary body attitudes. The first emotional category, pleasure/displeasure, is formed in the neonate via the visceral system, especially in nutrition, and in the proprioceptive system, especially in the handling of the infant (in other words, perception of events in visceral and muscular tissue and in the joints). Wallon gave muscular and visceral tonus a fundamental and primordial role in development. For him, the early bodily emotion of the child is programmed to occur in interpersonal relations. In the 30s, Wallon was already using cybernetic-like concepts to describe two such types of behavior. The first, essentially emotional, is the posturo-visceral response which proceeds by "irradiation, diffusion, amplification, recruitment, conjugation and catharsis". This first emotional differentiation in neonatal development is that between well-being (sensory dilation accompanied by saccadic visceral and eventually cathartic gestural recruitment, i.e., laughing) and malaise (sensory contraction accompanied by the same recruitmemt, *i.e.*, crying). The second type of response, the exteroceptive, is a sequential, goal-oriented closed circuit. The child reaches out, apprehends an object and closes the loop by bringing it to his mouth. Wallon demonstrated that social or interpersonal stimuli are the most powerful of emotional triggers (easily conditionable and difficult to extinguish). In addition, the child's interlocuters attribute social meaning to the child's emotional responses (verbal labels, evaluative judgements, role modeling, aversive and appetitive conditioning, etc.) such that the child incorporates these meanings into his own identity.

Wallon stated that the human infant is pre-programmed to mature emotionally via interpersonal stimuli. Powerful mechanisms of this bio-social development of emotion described by Wallon include mimicry, contagion, entrainment -- behaviors that humans share with innumerable other species. Another interesting early bio-social emotional response expressed by posture is the child's reaction to the appearance of an adult ("reaction de prestance") -- a strong favorable bodily response to the presence of a familiar figure and a similarly strong negative response to the presence of a stranger. A further bio-social mechanism of emotional development is the use of dramatization by infants. The main condition for the appearance of this behavior is the development of the child's susceptibility to reinforcement of it. Dramatization occurs in the presence of others, not in solo. It nourishes the child's narcissism and contributes to the development of an ability to role play and thereby to differentiate one's self from one's emotional states. Further, this process is a necessary condition for the child to eventually gain control over the immediate components of its emotional responses in order to inhibit or activate them. These emotional processes contribute to the formation of the child's identity and to the development of subtle cognitive processes such as sentiment, morality, aesthetics, etc. Wallon wrote "subtle emotions always contain a lie" (dramatization role-playing, etc.). Higher-order social-cultural influences on emotion were also of interest to Wallon. Social rules of politeness, conventionalities and decorum provide a process for selection of emotion responses and for fixation of certain expressive automatisms. They further charge emotional responses with culturally specific meaning.

For all these concepts Wallon provided a wealth of negative examples from pathology, based on insights from his practice and research as a medical pathologist. Finally Wallon developed a theory of stages of emotional development but space does not permit its exposition here.

A eulogy by Piaget [1975; 180] recognizes the extent of Wallon's contributions, as in the following comment:

> There are two forms of representation, truly distinct, and exactly mutually complementary, which have interested Wallon and myself: the figurative form, which engenders the image on the basis of imitation and which proceeds originally from the postural function; and the operative form which starts with motor schemata and ends, much later as Wallon had correctly argued, in the operations of thought. [Tr. by C.B.]

7. Leontiev on the origins of activity and consciousness

In the analysis of behaviorial development, Marxism attributes greater importance to the objective than to the subjective. Several essays in Leontiev's *Problems of the Development of the Mind* [1981] illustrate this point. Though some of these essays date back to the 1930s, they seem to have been written yesterday.

Leontiev basically explained phylogenetic development via the general concept of *adaptation*. He explained historical progress by means of the general Marxist category of practice. And he introduced his own concept of ontogenetic development. The particularity of individual human development, he wrote, consists of *appropriation* of the objectively (primarily historical) given, of its transformation into motivated, personalized *subjectivity*, and in turn of *objectification* of the latter in social practice.

Following Marx's lead in the *Theses on Feuerbach*, Leontiev [1978] in *Activity, Consciousness and Personality* sought to develop a complex empirically-based theory of human action. The two essential specific qualities of human action, stated by Vygotsky, are: 1) its instrumentality, its embeddedness in a world of previously (historically) transformed objects; and 2) its conscious motivational nature. Leontiev specified that "need" is an activity of internal receptor stimuli; initially, "needs" do not know their objects. Only disclosure of the object of need, its representation, produces a motive. In the tradition of Rubinstein, Leontiev rehabilitated consciousness as a legitimate and, further, an essential primordial ingredient of the development of human behavior. Aware that the introspectionist concept of consciousness is limited by its ignorance of the social determination of every aspect of consciousness, Leontiev also realized the emptiness of the early psychophysiological interpretation of consciousness as a synonym of wakefulness. The clear-cut sense in which the term consciousness is used by Leontiev corresponds to the mental prerequisite for the existence of a subject. The subject consists of a double interchange: first, of the organism with its environment and, second, of the detailed realization by the organism of the first relation. In other words, only a subject knows that he or she knows. This function presupposes, of course, Pavlov's second signaling system.

Leontiev also further developed the philosophical import of Lenin's concept of reflection. This last example of Marxist theory of origins extends somewhat beyond the scope of the present essay, but will serve as the basis for some open-ended concluding comments as food for further thought. Leontiev fully realized that psychologists today are uniquely equipped to substantiate the extraordinary foresightedness of Lenin's choice of the word reflection as the best term by which to combine the ontological and epistemological essences of knowledge. Reflection is a bona fide philosophical category, anticipating the modern concept of information understood as a universal property of matter which assumes specific forms in physics, biology and psychology.

In physics, any cause-effect relationship (i.e., real connection between discernible entities) consists at the same time of an information exchange. On the "effect" side of the connection, any new and relatively stable property may be characterized as reflection. Thus reflection is portrayed here as a particular aspect of the general phenomenon of information exchange, namely, cumulation or appropriation of information (in the sense of objective acquisition of a new property)

It has been realized for some time that systems far from equilibrium can generate information, order and structure. Systems involving periodic cumulation of information were among the first to be studied in astronomy. Numerous types of random cumulation have been studied more recently. Physical systems, particularly those involving so-called "statistically long range" phenomena, manifest strong correlations between past and future as well as betweeen the diffeent parts of a system. Such systems have been described in the turbulence literature [Kolmogorov 1941]. Geological time frames present similar characteristics in the "landmarks" provided by rock strata whose statistical distributions show intermittent change but without stable time periods. Long periods without much change are separated by short, eventful bursts of change. This phenomenon is a general property of nonlinear dynamical systems called "multiplicative chaos" [Kahane 1985].

Though intermittent cumulation of information is a relatively well-known property of numerous physical systems, controversies over the laws governing closed systems complicate the issue. The second law of thermodynamics, or entropy, for example, is believed by some to exist only in theory, by others to apply only to the universe as a whole, and by several to be devoid of any truth whatsoever [Lovejoy 1985].

If, however, the law of entropy does exist for the universe as a whole, then Brillouin [1962] was correct in proposing an opposite law, of negentropy, as the most general distinctive feature of the biosphere. Indeed, the law of entropy postulates that cumulation of information in a closed system is fortuitous, tending to simple randomness (devoid of the possibility of new structure). This, of course, is the opposite of what can be easily observed in the evolution of life forms.

In biology, information exchange involves the specific way in which genetic matter selectively accumulates particular information. The principle which determines the selection of specific information and the discarding of other potential information is natural selection. Cumulation of information in genes, or biological reflection, manifests the property referred to previously as "anticipatory reflection." The expression summarizes the dialectic of life-and-death unity in the general sense, and of the natural-selection process specifically. To the extent that life forms code, within their genes, determinate messages for their own future development, these messages in the past selectively sustained life and were in turn selectively sustained by life. However, the limitation which inexorably imposes itself on information cumulated in lower biological forms is that such cumulation is locked into the process of speciation. Though biological reflection undergoes (throughout speciation) cumulation that, in the long run, is of a sublative rather than merely disjunctive nature (evolutionary stumps or dead-ends are examples of disjunctive cumulation in the short run), the possibilities of significant and varied cumulation within each species is minimal in species not endowed with a central nervous system. In light of the Hardy-Weinberg concept of species' gene pools, it is obvious that genetic information does not cumulate (or does so only minimally) during the ecological stability of a species, but rather cumulates in qualitative leaps during the relatively brief and abrupt mutational phase of the appearance of a new species. Thus, the general rhythm of biological reflection appears as a complex periodicity, rather than the tendential randomness of physics. The general property of biological reflection, which tends intermittently to increase its cumulative complexity through the process of speciation, may form a unity of opposites with the general property of reflection in physics.

In psychology, reflection assumes once again a specific form. It appears here as an intensive process of cumulation of information, essentially embodied not in the genes but in complex brain activity still to be identified in its full specificity. This process of cumulation of information, at the human level, consists for Leontiev in the formation of the subject, a theme to which he devoted many papers but which would take us too far away from our topic.

Conclusion

A common theme underlying this essay is the recurrence in Marxist writings of the 1920s and later of ideas which foreshadow or parallel the more recent so-called "contemporary" systems theories developed originally by Von Bertalanffy, Weiner and Ashby.

Though Von Bertalanffy has been working on general systems theory since the 1930s, a comprehensive presentation was not published until his *Outline of General Systems Theory* [1958]. An explicit application to psychology appeared in 1967 with his *General Theory of Systems: Applications to Psychology*. Weiner published his *Cybernetics* in 1948, and Ashby his *Introduction to Cybernetics* in 1956.

Marxist philosophy has been late in coming to terms with the indispensability of such systems concepts, particularly for the epistemology of interdisciplinary sciences (psychology, ecology, linguistics, etc.) [cf. Blauberg et al. 1977]. In the Marxist scientific tradition, however, there has been no such reluctance to propose and develop the "systems" concept, enriching it and compensating for its tendency to focus on closed and static systems with a new tendency to focus on open and changing systems.

Marxism has indeed contributed integrative and coherent theoretical models for understanding phenomena at a level compatible with the sophistication of the specialized sciences. The present essay has been an attempt to

demonstrate the truth of this claim with regard to a particular specialized science, modern psychology.

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Dear Friend:

Nicaragua is a poor country. After freeing itself from the yoke of the Somoza dictatorship, it is trying to pull itself up by its bootstraps. It is making heroic efforts to raise the standard of living of an impoverished people and to open the door of educational opportunity to a population formerly kept illiterate and undeducated.

A key role in this effort is played by the Universidad Nacional Autonoma de Nicaragua which shares the responsibility of preparing a new generation of teachers, technicians, and administrators to carry out the program of economic and educational expansion. But the work of the University is hampered by a severe shortage of supplies. Our committee was organized to purchase and ship to the University some of the educational materials it needs. Our first shipment to the University included 1280 pounds of paper, 10 typewriters, 396 art slides, 100 audio cassettes, 3 wall maps, and miscellaneous office supplies, all of which had been specifically requested.

The University has since requested language laboratory repair parts and print shop supplies, for which we must now raise $15,000. Please contribute as much as you can to show your support for the Nicaraguan people's efforts to help themselves. All contributions are tax deductible. Make checks payable to Aid to UNAN.

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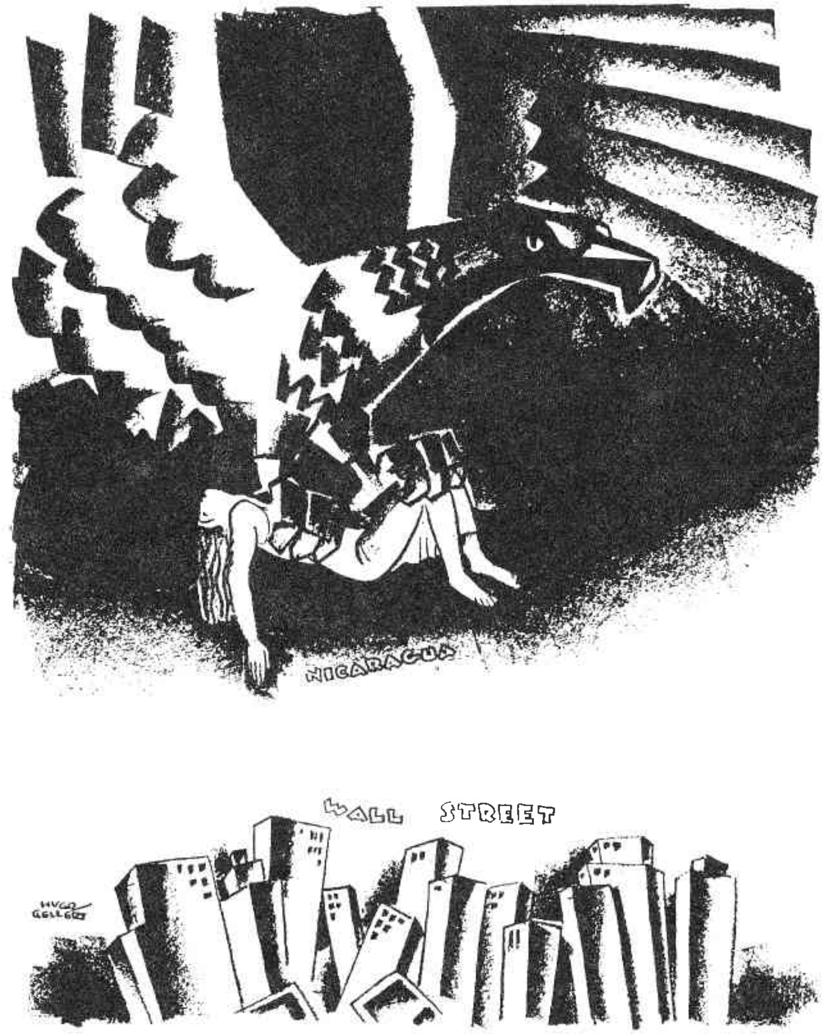

Hugo Gellert [1892-1985], *New Masses* 1928.
Nicaragua in the claws of imperialism.

Is the Creative Process Rational?

LESTER (HANK) TALKINGTON
SCIENCE AND NATURE

IF YOU consider how the concepts of the mad artist and mad scientist have become bywords of our culture, then you can see that it is not frivolous to ask whether creativity is a rational process. As a matter of fact, philosophers of science have actually contributed to the idea that scientific creativity is irrational. Hence, the nature of the creative process is a highly relevant question for all scientists.

To propose a Marxist answer to this question, I must begin by defining the terms and phrasing the question in a form more amenable to philosophical analysis. First, by *creativity* I mean the process of innovative discovery or invention that occurs in the development of scientific knowledge. Second, by *rational* I mean that some form of *logical* thinking is involved in this process. But what form of logic applies to the process of creative discovery and invention? In philosophy, this form of logic is known as *induction*, and *inductive logic* is the form of reasoning from the parts to the whole, from the facts to their theoretical representation, from the known to the unknown, and so forth. But, as we shall see, there is a great deal of controversy and mystery concerning the nature of inductive logic.

Defining the problem

In the general sense, induction means something quite different from deduction. After one gets a new idea (a *conjecture, hypothesis*, or *theory*, depending on how ambitious the idea is), then deduction can help test the validity and consistency of the new idea, help explore its implications. But no one has ever generated a truly new idea by deductive logic alone. Actually, the creative process involves a dialectical interplay of induction and deduction together. But, since deduction is well understood as a highly rational form of logic, the emphasis here will be on the problem of understanding induction.

An excellent description of induction, in the sense described above, has been given by a chemistry professor:

> Research is like looking for a string in a room that is pitch black. You can grope around for a while, but when you find the right string, suddenly the whole room is illuminated [Ternay 1984].

Thus induction involves some kind of qualitative leap in thought. Just about everybody agrees on this point. But that's where agreement ends on the nature of induction. At this point I will phrase more precisely the question to be addressed here:

> *Is the creative generation of new ideas a logical process that follows the laws of scientific induction?*

However, the question put that bluntly opens up a whole can of worms: "What laws are you talking about?" asks the typical scientist, quite unaware that there is supposed to be some specific kind of logic in his creativity.

"What kind of induction are you talking about?" asks the philosopher, very much aware that there is no philosophical consensus on how induction works or even if such a thing as induction actually exists.

For example, Karl Popper [1962, 53] maintains that scientific induction is a myth. For two generations, his teachings have equated scientific creativity with irrationality, even portraying scientific discovery as an unscientific act:

> There is no such thing as a logical method of having new ideas, or a logical reconstruction of this process. My view may be expressed by saying that every discovery contains "an irrational element," or "a creative intuition".....I am inclined to think that scientific discovery is impossible without faith in ideas which are of a purely speculative kind, and sometimes quite hazy; a faith which is completely unwarranted from the viewpoint of science.....a theory of induction is superfluous. It has no function in a logic of science [1934; 32,38,315].

One wonders if Sir Karl is not tossing out the scientists along with their inductive thought processes. And I think few scientists would agree with his recent pronouncement implying randomness in scientific research:

> The actual procedure of science is to operate with conjectures: to jump to conclusions -- often after one single observation.....How do we jump from an observation statement to a *good* theory?...by jumping first to *any* theory and testing it, to find out whether it is good or not [1962; 53,55].

Unfortunately, Popper is not a lone voice in philosophy of science. His ideas have been widely influential. For example, historian-philosopher Thomas S. Kuhn, who strongly opposes Popper on many philosophical issues, expresses agreement on the key question of induction:

> Neither Sir Karl nor I is an inductivist. We do not believe that ...theories, correct or incorrect, are induced at all. Instead, we view them as imaginary posits, invented in one piece for application to nature [Kuhn 1970a, 12].

In Kuhn's [1970b] historical analysis of scientific revolutions he compares the discovery process to puzzle-solving [pp 35-42] but lists induction among "vexing problems" [171] and cannot delve into the logical mode of creative reasoning to any greater depth than this:

> Scientists usually develop many speculative and unarticulated theories that can themselves point the way to discovery....Only as experiment and tentative theory are together articulated to a match does the discovery emerge and the theory become a paradigm [61].

Note that Kuhn has nothing to say about how the speculative process occurs. He does not even offer us his own speculations.

Then there's Paul Feyerabend, apostle of "epistemological anarchism," another critic of Popper who nevertheless goes along with him on this prime philosophical issue. Feyerabend takes the position that "science could not exist" without what he calls "counterinduction," arriving thereby at Popper's same dead end by concluding that scientific discovery "may be irrational" [1975; 36,175].

Where Popper and his followers go wrong is indicated in this comment by a physical chemist, the Nobelist Nikolai N. Semyenov [1972]:

> 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 [scientist] inmates whose sole aim is to disrupt it.

Admittedly, the concept of induction these philosophers reject actually deserves to be rejected. The prevailing models for induction -- from the familiar Mill's Methods to the most sophisticated computer programs of probabilistic "induction" -- are actually deductive and mechanistic in nature, i.e., they are analytic rather than synthetic [cf. Mackie 1967; 340], and may be compared to procedures for differential diagnosis. Their usefulness comes mainly after data has been collected to test a conjecture or hypothesis that was previously generated by true induction.

What Karl Popper and his followers advocate in lieu of induction is the so-called hypothetico-deductive model of scientific thinking which ducks entirely the question of how the hypotheses originate. Most scientists, if they think about the matter at all, probably go along with some variant of this. If asked how they originate new creative ideas, they will probably attribute this to "intuition" or "insight." For instance, Einstein [1973] said of his own discoveries and inventions:

> There is no logical path to these laws; only intuition, resting on sympathetic understanding of experience, can reach them.

And even the Marxist handbook *Fundamentals* goes along with this formulation, though providing a good materialist discussion of what intuition implies:

> How do conjectures arise? Why does one particular idea and not another occur to the scientists? The reply to these quite reasonable questions is that one cannot ignore the concept of *intuition*...The history of scientific discoveries abounds in legends about the incidents that are supposed to have sparked off brilliant intuitions. We have all heard of "Newton's apple," and "Mendeleyev's dream", and so on. But while not denying the possibility of such incidents, we must see behind every such case of intuition the effort of human thought, its constant and stubborn search for a solution to the problem it has posed. [1982; 185f]

Clearly, to speak of intuition is to confess that the mental processes of induction are not understood, and to leave open the question as to whether these processes are subject to rational analysis. Philosophy has reached this dead end as the result of Popper and others making induction a dirty word.

Hence, the real "problem of induction" stems from the established view that inductive thought processes must somehow satisfy the same criteria of formal proof as in deductive logic. This is a self-defeating requirement since it assigns to deductive logic the impossible task of inducing new ideas. For example, Engels railed at the "inductionists" for whom "induction is an infallible method," pointing out that "Induction and deduction belong together as necessarily as synthesis and analysis", then going on to express agreement with Hegel's thesis that "the inductive conclusion is essentially a problematic one!" [1954; 228].

Part of the problem stems from the traditional tendency of philosophers to ignore the crucial role of practice in determining truth values. Even when they remember that formal deduction is empty of content except to the extent that the premises reflect practice, few philosophers seem to grasp the fact that the inductive process acquires meaning only if it reflects *continually* the results of the investigatory process.

Still and all, I think the main stumbling block has been the lack of a suitable model for the specific mental processes involved in the generation of new scientific ideas. Until that is achieved, inductive reasoning will remain "the glory of Science...the scandal of Philosophy" [Broad, 1926].

I propose to remedy this defect. I begin by noting that induction starts with asking questions of nature. The form of questioning is that of experimentation and systematic observation. The answers that nature yields are the raw material for the induction process. Hence, induction must be primarily concerned with reasoning about *content*, whereas deduction is concerned primarily with the *form* of reasoning. As a corollary difference, noted before, induction is synthetic whereas deduction is analytic. I wish to find a more concrete and detailed description of how the inductive synthesis takes place in the mind of the scientific investigator. I find my clue in statements such as these (emphasis added):

> A physical law defines a *connection* between some characteristics of certain phenomena or one phenomenon. [Alexandrov 1983]

> At the theoretic level the object is reflected in its *connections* and laws, which are discovered not only by experiment but through abstract thinking. [*Fundamentals* p174]

The clue is found in the word *connection*. That's what I see as the essence of the research process -- trying to find new concepts and laws that show *connections* between phenomena. It has gradually dawned on me that scientific induction is simply the process of *search* for these connections. From this basic idea, I have developed the following more elaborate definition of induction:

> *The inductive mode of thought can be characterized specifically as a mental search for a connecting concept which brings new theoretical unity to a group of empirical and/or theoretical objects. Since empirical objects can enter the induction process only in the form of concepts (ideas or questions about their potential meaning), the induction itself consists of discovering or postulating some concept that provides a unifying relationship between the lower-level concepts. Proof of the validity of a new higher-level theory cannot be established by the inductive process but must be accomplished independently.*

Support for the above definition is provided by Hofstadter who, discussing creativity in a wider context, expressed something like the same idea about connectivity.

> Nothing is a concept except by virtue of the way it is connected up with other things that are also concepts. In other words, the property of being a concept is a property of connectivity, a quality that comes from being embedded in a certain kind of network and from nowhere else.....surely the happy choice of the right concept at the right time is the essence of the creative [1982; 18f].

This basic idea, that new knowledge comes through establishing some kind of connectivity or relatedness, appears quite often in the scientific literature, at least implicitly. Here are a few examples (emphasis added):

> *Biology*: The point...is that science does not merely consist of making observations but also of *ordering* them in relation to each other [Howard 1981].
>
> *Geophysics*: The complexity of physicochemical processes encountered in natural phenomena...is such that great progress has usually come from widely directed observations and studies of their *interrelationships* (rather than from a more directed effort at understanding the physics involved) [Runcorn 1982].
>
> *Biomedical*: Molecular genetics, our latest wonder, has taught us to spell out the *connectivity* of the tree of life in such palpable detail that we may say in plain words, 'This riddle of life has been solved' [Delbruck 1969].
>
> *Philosophy*: The aim of science is not things themselves...but the *relations* between things; outside these *relations* there is no reality knowable.....Experiment teaches us *relations* between bodies; this is the fact in the rough; these *relations* are extremely complicated....In sum, the sole objective reality consists in the *relations* of things [Poincaré 1905; xxiv and 1907, 125, 140].

Poincaré, of course, was talking about connections between "sense impressions" because his *conventionalist* outlook obscured the intermediary role of concepts in human thought. Einstein [1934] made a different error by confusing the concept of connections with the mechanistic views of the positivists:

> It is, of course, universally agreed that science has to establish connections between the facts of experience, of such a kind that we can predict further occurrences from those already experienced. Indeed, according to the opinion of many positivists the completest possible accomplishment of this task is the only end of science.
>
> I do not believe, however, that so elementary an ideal could do much to kindle the investigator's passion from which really great achievements have arisen. Behind the tireless efforts of the investigator there lurks a stronger, more mysterious drive; it is existence and reality that one wishes to comprehend.

One may interpret Einstein's words as a defense of the principle of causality against the views of positivists such as Mach who argued for eliminating the *concept* of causality in favor of the mathematical notion of functional relation. In the actual practice of science, causality provides one of the strongest and most convincing forms of connection, the discovery of which requires imaginative penetration into the essence of natural relationships. Seen in this light, the search for connections may not have as much "mystery" as Einstein desired but it provides ample motivation for the passionate investigator.

Nobelist Peter Medawar opts for intuition against induction on much the same basis as Popper, yet, when he discusses the research process itself, it seems to be induction as I have defined it:

> We describe and annotate the phenomena when they are made to take place under certain well-defined and well-regulated conditions. In the meanwhile *we begin to form opinions about the nature of the causal mechanisms at work and the relationship of the phenomena to others, and only critical experimentation can discriminate between them.* [1969; 38, emphasis added.]

The informal nature of induction, as opposed to the rigid standards of demonstrative reasoning with formal logic, is discussed by mathematician

George Polya [1954], who uses the term *plausible reasoning* for what I have defined as *inductive reasoning* (perhaps because in mathematics the term induction has a specialized meaning):

> Anything new that we learn about the world involves plausible reasoning, which is the only kind of reasoning for which we care in everyday affairs.....The standards of plausible reasoning are fluid, and there is no theory of such reasoning that could be compared to demonstrative logic in clarity or would command comparable consensus ....[p.v] Let me observe that the two kinds of reasoning do not contradict each other; on the contrary, they complete each other.....[p.vi] The examples of plausible reasoning...may throw some light upon a much agitated philosophical problem: the problem of induction. The crucial question is: Are there rules for induction? Some philosophers say Yes, most scientists think No.....[The question] should be treated differently...with less reliance on traditional verbalisms, or on new-fangled formalisms, but in closer touch with the practice of scientists. [p.vii]

The fluidity so natural to inductive reasoning, as described by Polya, is what permits the element of the accidental to enter frequently into the discovery process. Far from unusual is a discovery reported in *Science* under the headline "Trail of Ironies...Sloppy chemical synthesis by an illicit drug producer has led to important insights into the basic cause of Parkinson's disease" [Lewin 1984]. Another type of fortuitousness is found in the anecdote about one of the co-discoverers of the Legionnaire's bacterium, who said that the finding was made only after he took a second look at some samples months later. That second look, he said, was prompted by the embarrassment he felt for his employer, the Centers for Disease Control, in its failure to solve the epidemic [Altman, 1982]. Then there are the stories about flashes of insight at unexpected moments or even in dreams. All this is possible because the trained scientific mind is able to continue the mental search for a new *connective* concept while going about other activities *in or out of the laboratory*.

The formulation of induction as a search for connections is also implicit in the role of Gestalt perception or pattern recognition in the process of creative discovery [cf. Kuhn 1970b]. The role of connective patterns in playing chess, discussed by Hofstadter [1983; 20], provide an example:

> A brilliant chess move, once the game is over and can be viewed in retrospect, can be seen as logical, as "the correct thing to do in that situation." But brilliant moves do not originate from the kind of logical analysis that occurs after the game; there is no time during the game to check out all the logical consequences of a move. Good chess moves spring from the organization of a good chess mind; a set of perceptions arranged in such a way that certain kinds of ideas leap to mind when subtle patterns or cues are present. The way perceptions have of triggering old and buried memories underlies skill in any type of human activity, not only chess. It is just that in chess the skill is particularly deceptive, because after the fact it can all be justified by a logical analysis, a fact that seems to hint that the original idea *came from* logic.

I have my own metaphorical description of how pattern perception works in the inductive process, based on the way jigsaw puzzles are solved. In my metaphor, however, there would be no clues from a picture on the puzzle box, or else such clues would be deceptive because of mental preconceptions. Moreover, there would be many missing jigsaw pieces (representing empirical data, experimental design, lower-level theory, etc.) and those on hand would be somewhat plastic, taking on different shapes (meanings) according to the changing conceptual interpretations of the investigator in the puzzle-solving process. The goal, of course, is to find and connect up enough pieces to form a meaningful, self-consistent pattern. As the investigator's mind plays around with the possibilities (making *conjectures*), the connections between pieces would undergo transformations (changes of interpretation) so that the same puzzle pieces fit together into different patterns, acquire different meanings, become relevant or irrelevant -- all in one kaleidoscopic process.

More than one investigator, of course, may be working concurrently on the same puzzle, in teams or independently, so that different and conflicting patterns of interpretation may be discerned at one and the same time. The discovery process may proceed in erratic jumps as more and more puzzle pieces are found and fitted into place, with a prolonged effort often required before an overall coherent pattern emerges. Sometimes this overall pattern will emerge suddenly through a perception of connections that makes for a qualitative leap in the coherence of the mergent pattern.

This metaphor of the puzzle-solving process, if it has any validity as an analogy, makes clear the fact that induction of a new theoretical pattern is not "invented in one piece" as Kuhn (above) conjectured. Quite the contrary, science in general is a social process in which many individual puzzle pieces must be fitted into place before it is possible for an overall pattern to be perceived. In the search process, the investigators develop a heightened consciousness about where to look for missing pieces of empirical informa - tion and how to intepret their theoretical connections. (Hence, the oft-noted frequency with which the same discovery is made more or less simultan - eously by more than one investigator.) This heightened consciousness, usually referred to as intuition, is actually the essence of the inductive process and it is what makes possible the qualitative leap in pattern recognition, the Gestalt perception that constitutes the discovery itself.

It should be obvious that this form of heightened consciousness derives not from any occult faculty but simply from the intimate hands-on experience of "working with the material." Acquiring this faculty is no different, in principle, from the way skills are developed in other realms of human activity -- in the arts, in politics, in sports, and so on. The training of scientists involves not only soaking up the accumulated facts of scientific knowledge but much drilling in abstract thought, manipulation of concepts, etc. This training provides the foundational consciousness that is heightened in the discovery process. Once this consciousness is understood in terms of the search for connections, there is nothing really mysterious about it.

Naturally the development of my concept of induction has also been an inductive process. Though I do not remember exactly when or how the moment of discovery came, I do know that it represented fruition of some years of groping. For example, the excerpts on plausible reasoning from Polya (above) come from a book that has been on my shelves for years, with some of the excerpted passages underlined by me at an unremembered time. Yet only on picking up the book at the later stages of writing this paper did I come to realize how much he anticipated my ideas on the informal nature of inductive reasoning. And I have no way of knowing how much Polya may have influenced me in coming to this model of induction.

On the other hand, I can be pretty certain that I was greatly influenced by familiar passages in the Marxist classics emphasizing that "Science has always in some way or another attempted to reveal the connections between phenomena" [*Fundamentals* 1982; 99]. Very suggestive is this example from

Lenin's *Philosophical Notebooks* [1961; 178f] that concerns how "notions" or "ideas" are created:

> The formation of (abstract) notions and operations with them *already* includes idea, conviction, consciousness of the law-governed character of the objective connection of the world.....the simplest *generalization*, the first and simplest formation of *notions* (judgments, syllogisms, etc.) already denotes man's ever deeper cognition of the *objective* connection of the world. [Emphasis in original.]

And Engels is even more suggestive concerning the scientific research process as a search for connections:

> The perception that all the phenomena of Nature are systematically *interconnected* drives science on to prove this systematic *interconnection* throughout, both in general and in detail [1939; 43f, emphasis added].

Engels did not, however, link the perception of connections with the process of induction. He seems to have accepted the then prevailing mechanistic formulation of induction due to Francis Bacon and John Stuart Mill (the Mill of Mill's Methods). As a result, while he considered both induction and deduction as components of the dialectical mode of thought, Engels mostly used the undifferentiated term *dialectics* when referring to the mental processes I define as induction. For example, Engels [1954; 17] refers to dialectics as "the science of universal inter-connection". But don't we need a precise and useful Marxist definition of the inductive process, as a *specific part* of dialectics? After all, the deductive process, known in exhausting detail, is the dialectical opposite of induction, not of dialectics itself.

Nor is this in any way just a matter of convenient terminology. Engels says that dialectical reasoning must base itself on the concept of universal interconnection in order to avoid getting "entangled in the one-sidedness of metaphysical thinking" [1954; 167]. In other words, the inductive search for connections implies acceptance of the materialist concept of the *unity* of the world (as well as its existence independent of consciousness). Without a conscious view of the world as a single connected *whole*, the scientist is limited in his/her ability to find new unifying concepts; it is too easy to fall into the metaphysical trap of constructing theoretical absolutes concerning arbitrarily isolated phenomena, ignoring relevant connections with other phenomena. This metaphysical tendency, prevalent in science today, may be one of the biggest sources of confusion concerning the essential nature of induction as the search for conceptual connections.

It must be emphasized that there can be no prescriptive rules for the kind of inductive logic that I define here, that is, no rules with the character of formal deductive logic. There can be only informal and suggestive heuristic principles based on mankind's accumulated experience concerning the existence of connectedness and the form it takes. It is in this sense that the laws of dialectics provide useful heuristics for the scientific investigator, placing induction and deduction within a more comprehensive framework of the laws of conceptual reasoning. The dialectical principles that sum up the laws of change and development (applying to nature, to society and to human thought) were summarized by Engels [1954; 62] as follows:

> The law of the transformation of quantity into quality and vice versa; The law of the interpenetration of opposites; The law of the negation of the negation.

This is not the place to discuss these laws further except, first, to emphasize that they cannot be applied mechanically in the induction process and, second, to give my opinion that political activism is a good way for the scientist to acquire hands-on experience and deep understanding of these laws, an understanding which can then be transferred to scientific work, enabling the investigator to ask more penetrating questions of nature. Since the literature contains many examples of scientists using dialectical reasoning spontaneously, it seems reasonable to think that conscious and deep understanding of dialectics would enable scientists to be even more effective in the discovery effort. One scientist who thinks so is Nobelist Semyenov [1978; 27]:

> 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 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.

Even more specific on the role of dialectical materialism in the discovery process is this comment from physicist/historian J.D. Bernal [1935].

> The crises of modern science appear in the first place as intellectual difficulties arising from new and apparently incompatible discoveries. The resolution of these crises, that is, the process of bringing them into harmony with the general movement of thought and action, is a task for the Marxist scientists of today and tomorrow.....We have through dialectical materialism a greater comprehension of whole processes, which before were only seen in their parts. But it is not only in these general, almost philosophical, aspects of science that Engels' work is of value. In everyday work, those who take the trouble to follow Engels' hints find themselves more able to grasp the detailed *connections* of special investigations. The function of dialectical materialism is not to take the place of scientific method, but to supplement it by giving indications of directions in which hopeful solutions may be looked for. [*Emphasis added.*]

At this point I wish to give proper credit to two seemingly improbable philosophers who have nevertheless made distinct contributions to the theory of induction presented here. The first of these philosophers is David Hume, the 18th-century empiricist who sought to undermine belief in the objectivity of knowledge. Despite his skepticism, Hume took the materialist position that all reasoning concerning matters of fact is based on the relation between cause and effect, which he referred to as a "necessary connexion" and thus, it seems, became the first to emphasize this central aspect of the inductive process [cf. Black 1967; 170]. Hume also recognized other sources of connection "between the different thoughts or ideas of the mind," including resemblance (analogy) and contiguity in time and place [Hume 1748; Sect. III]. One can also agree with Hume in his insistence that no absolute truths are to be gained through inductive inference, only regretting that his skepticism did not end there.

The second of these philosophers is the 19th-century William Whewell whose contributions were more profound. If I had encountered his work earlier, this paper might never have been written or else it might have concentrated just on his comprehensive view of induction, anticipating in great detail as it did the idea of induction as a process of establishing the conceptual connections between empirical data. I give the following sub - stantial excerpt because it is a good exposition of ideas that I achieved through lengthy struggle on my own:

> *In Discovery a new Conception is introduced*.....I conceive that Kepler, in discovering the law of Mars's motion, and in asserting that the planet moved in an ellipse, did this;-- he bound together particular observations of separate places of Mars by the notion, or, as I have called it, the *conception*, of an *ellipse*, which was supplied by his own mind. Other persons, and he, too, before he made this discovery, had present to their minds the facts of such separate successive positions of the planet; but could not bind them together rightly, because they did not apply to them this concept of an *ellipse*.....
>
> To discover such a connexion, the mind must be conversant with certain relations of space, and with certain kinds of figures.....To hit upon the right conception is a difficult step; and when this step is once made, the facts assume a different aspect from what they had before; that done, they are seen in a new point of view; and the catching this point of view, is a special mental operation, requiring special endowments and habits of thought. Before this, the facts are seen as detached, separate, lawless; afterwards they are seen as connected, simple, regular; as parts of one general fact, and thereby possessing innumerable new relations before unseen. Kepler, then, I say, bound together the facts by superinducing upon them the *conception* of an *ellipse*; and this was an essential element in his Induction. [Whewell 1860 pp. 253f]

Whewell goes further to elaborate a hierarchy of inductions in science. Not only did he see inductions "tying together" the facts in the formation of new ideas but he also saw how inductions themselves tend to coalesce, coming together to form a unified coherent theoretical structure as a *consilience* of inductions whose independent derivations reflect a fundamental unity of the theoretical structure itself [cf. Bynum *et al*, 1981; 75].

Now I want to show you how the thinking of Engels and Whewell on the nature of induction run very much parallel. Here is one of the notes left by Engels in the folders for his unfinished *Dialectics of Nature* [1954; 222f]:

> We have in common with animals all activity of the understanding: *induction*, *deduction*, and hence also *abstraction*.....*analysis* of unknown objects (even the cracking of a nut is the beginning of analysis), *synthesis* (in animal tricks), and as the union of both, *experiment* (in the case of new obstacles and unfamiliar situations)... -- hence all means of scientific investigation that ordinary logic recognizes -- are absolutely the same in men and the higher animals. They differ only in degree...of development.....On the other hand, dialectical thought -- precisely because it presupposes investigation of the nature of concepts themselves -- is only possible for man, and for him only at a comparatively high stage of development. [Emphasis in original.]

Note that Engels sees induction as a part of conceptual thinking because it leads to abstraction -- though he places it at a lower level than dialectical thought which involves forming concepts about "the nature of concepts themselves." Does this not run parallel to Whewell's emphasis on the role of concept formation in the inductive process and his view that a *consilience of inductions* brings a higher level of theoretical understanding? I don't want to stretch this analogy too far. The point is simply that both men were thinking of induction in terms of concept formation as a dialectical process. (Whewell's dialectics, though conscious, were pre-Marxist.)

Whewell's rather modern ideas on the theory of knowledge came from studying the history of actual science. This was how he came to realize that the inductive process of scientists does not resemble the generalizing arguments of such logicians as John Stuart Mill, who was Whewell's 19th century contemporary and opponent on many philosophical issues including induction.

You may wonder why Whewell's strikingly original ideas on induction are not better known today. It seems that, in his own time, Whewell was recognized as a historian of science but his philosophical ideas got a cool reception [cf. Blanché 1965]. Logicians such as Mill complained that he had changed the definition of induction (just as I am trying to do), and otherwise strayed off the beaten path. His critics, for one thing, evidently did not want the problem of induction solved in a manner that would recognize the true subjective creativity of the scientist -- no more than would the logical empiricists and positivists of our own century. Another important factor in Whewell's eclipse was his religious idealism. Freethinkers were suspicious because of the theological setting in which his contributions were presented.

And his idealism could be pretty blatant. For instance, Whewell [1860; 196] criticized Newton's Rules of Reasoning for implying that "inductive propositions are to be considered as merely provisional and limited, and never secure from exception" by additional study of phenomena. Newton's caution on this matter seems pretty reasonable today, considering that the anomalous precession of Mercury's perihelion (discovered in 1859!) was explained only by Einstein's general theory of relativity. Yet Whewell [pp. 196f] was cheeky enough to protest against Newton's scientific caution in these strongly idealist terms:

> What man of science can suppose that we shall hereafter discover exceptions to the universal gravitation of all parts of the solar system?.....both the universality and the rigorous accuracy of our laws are proved by reference to Ideas rather than to Experience, a truth which perhaps the philosophers of Newton's time were somewhat disposed to overlook.

No doubt it was this kind of idealist dogmatism, together with Engels' limited definition of induction, that prompted his sketchy note dismissing Whewell out of hand:

> The whole swindle of induction (is derived) from the Englishmen; Whewell, inductive sciences, comprising the purely mathematical (sciences), and so the antithesis of deduction invented. Logic, old or new, knows nothing of this. All forms of conclusion that start from the individual are experimental and based on experience...[1954; 227]

Why such a negative assessment of Whewell? If, as I claim, Whewell's ideas were moving in the same direction as Engels', how do I explain this charge of "swindling"? Well, I think it could easily be a case of mistaken identity. By this I mean that Whewell's idealist enthusiasm for infallibility made it easy to lump his ideas in with those of his antagonist John Stuart Mill, the empiricist logician who was busy helping lay the foundations for today's logical positivism. Perhaps the reason Engels did not winnow out the useful core concepts beneath Whewell's idealism is that Engels had not defined induction for himself in any specific and concrete way. It is not sufficient to say with Engels that induction is part of dialectical logic or that conclusions "that start from the individual are experimental and based on experience." We still need to know the specific characteristics of the mental process for making that leap from the facts to the theory. Whewell, despite his egregious idealism, had really penetrated to the essence of inductive thought as the search for conceptual connections.

Now, in a closing summary, I wish to demonstrate how much there is to be said for defining induction as the search for conceptual connections:

1) It demonstrates that induction has a logic of its own, a rational logic that conforms to the fluid and even subconscious mode of reasoning so characteristic of scientific discovery -- without requiring appeal to an undefined intuition or resort to mysticism.

2) It reflects the actual practice of science and moreover provides the scientist with a useful new heuristic guide for thinking about the creative aspects of the research process.

3) It takes account of the crucial role of practice -- which is outside of formal logic -- and this explains why hands-on participation in research plays so important a role in the discovery process (along with background knowledge, overall experience, and style of thinking).

4) It shows why a successful induction always has the character of a qualitative leap in the movement of thought, bringing knowledge to a new higher theoretical level -- precisely what deductive reasoning can never do.

5) It accepts the fact that inductive concepts (conjectures, hypotheses, theories) are always to some degree tentative or contingent, subject to negation in whole or part from further empirical experience.

6) It is an open-ended model for the inductive process, subject to refinement and further articulation -- offering to philosophers and logicians a juicy new bone to chew on, at least to those who have any taste for a rational solution to the problem of induction.

7) It gives further evidence of how the Marxist outlook contributes to creativity in any social activity -- whether the innovation is in developing a scientific theory, painting a picture, writing a book or giving leadership to the people's liberation movement. Marxism, properly understood and applied, helps to discover and develop the particular logic of any such activity.

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Productive memory and connectedness

The imagination is dialectical in so far as anticipating the future is a form of *productive remembering.*

In the work process there is at first an imitation of useful objects found in nature, say a wedge to become an axe. But gradually the object placed before the eye as a model becomes remembered, an inwardly imagined one; the worker improves, refines the crude model, adapts it more and more closely to his purpose, discovers more advantageous combinations, unrealized possibilities. What was once a material model now appears to the worker as an ideal original image, an *idea* of the thing to be made. The possible as still unformed reality, the stuff that dreams and gods and tools, phantoms and inventions, myths and mechanisms are made of, is the never-ending material of the imagination. The trinity of the creative imagination is there from the beginning. To imagine what has not yet been objectified, what is not yet present; to combine things which are not yet mutually related, to join them together and to establish an interaction between them; to draw what is to be from that which is remembered, to overstep the inadequate here-and-now, to make what has never yet been seen, conceived of or noticed *creatively* visible, conceivable and conscious -- that is the imagination's three-fold manner of working.

-- Ernst Fischer, quoted in Maynard Solomon *Marxism and Art*, Knopf 1973 p272.

On Big Bang "Creationism" and Marxist Methodology in Science

See what the editor stirred up this time. *In S&N #6 we had a reader's query about the conflict between Marxist materialism and the "big bang" theory of an expanding universe. In answering, two major difficulties of that theory were discussed: first, it depends completely on an unproved assumption that observed redshifts arise only from receding motion of galaxies; second, the theory lacks physical plausibility because it is based on a mathematical singularity, found in the equations of Einstein's general relativity, that is interpreted in terms of an explosive origin for the universe. This "cosmic singularity" implies original conditions that are impossible to achieve according to known physical laws and some scientists doubt whether Einstein's equations provide the proper theoretical model for an early universe. The theory was termed "creationist" because it has given rise to so much mystification in the scientific community as well as in the public at large concerning the "creation" of the universe. Since this very complex subject is of great importance for the Marxist theory of knowledge, it is good to have these strongly dissenting letters and the occasion for the editor to develop further his iconoclastic views .*

TWO READERS HAVE THEIR SAY AND THE EDITOR RESPONDS:

Your position, that the *big bang* theory smacks of "creationism," is incon - sistent with a Marxist approach to science and a distinct disservice to *Science and Nature*. [See Talkington S&N #6 pp 3-5]

As you say, all knowledge is relative, and so it is in astronomy. In relation to the origin and development of the universe, our knowledge is skimpy indeed, restricted mainly by our still limited and inadequate technology. This paucity of data makes things difficult in formulating a scientific theory. Nevertheless, Marxists (and all scientists worth their salt) are obliged to formulate theories to explain what we know about real, existing objects or phenomena. Lack of enough data is no excuse for avoiding theory, though it does oblige us to expand our knowledge and put our theories to the test. New data may prove a theory valid, show that it needs revision, or indicate that it may have to be rejected as entirely false.

The general acceptance of the redshift phenomenon is not a consequence of the expansion of the universe. Rather, the expansion provides the most plausible and coherent explanation we have for the existence of the redshift, which can be measured and varies with the distance. There is always room for doubt that the redshift is connected with the expansion of the universe, but doubt alone does not mean that the theory should be rejected. The theory can be rejected only when data is acquired which cannot be explained by the present theory.

You argue that the *big bang* theory lacks plausibility. True enough, cosmic singularity and the *big bang* are theoretical constructs based on the notion of the expanding universe. It is a theory in which mathematics plays an exceptionally important part. The estimated moment of the *big bang*, 20 billion years ago, is extrapolated from what is known about the universe's present rate of expansion. Since we cannot go back in time to that event, and since there is little likelihood with present scientific knowledge that we can find direct evidence of the cosmic singularity (although data for the *big bang* is accumulating), we must resort to theoretical construction based on what we know now.

Your rejection of the *big bang* theory because it does not originate in what you call "physical thinking" smacks of empiricism. Though there is still great controversy and endless debate over the nature of the cosmic singularity, which may seem physically implausible given our present state of scientific knowledge, the *big bang* remains the most plausible explanation for what we know about the nature of the universe. To equate the *big bang* theory with "creationism" is unfair as well as inaccurate. Clearly, the former relies exclusively on physical process (albeit understood in theoretical terms) while the latter attributes the creation of the universe to God. While matter cannot be created or destroyed, things made from matter can -- and that includes the universe.

Gerry van Houten
221 Milverton Boulevard
Toronto Canada M4J IV6

Concerning your comment on the *big bang* [Talkington S&N #6], it is important to distinguish the scientific aspects of this theory from various philosophical conclusions being drawn. Let us first briefly review the scientific aspects.

The most widely accepted model of the early universe, the so-called *big bang* model, postulates that our present observable universe has evolved from an explosion some 10 to 15 billion years ago. In the earliest stages of this explosion about which one can speak with any degree of confidence, the matter of the universe was heated to more than 10 billion degrees Kelvin (the center of a typical star is only some 10 million °K).

For a universe of infinite spatial extent (the so-called "open" universe), the location of this primoridal explosion was everywhere, and not at any one spatial "point" [Weinberg 1977 p 5]. At temperatures of 109°K, the universe must have consisted of photons (quanta of electromagnetic radiation), protons, neutrons, electrons and positrons. Atoms or even atomic nuclei could not have existed at such extreme temperatures. As the compressed matter of the universe expanded and cooled, first the lighter nuclei and later atoms began to form, the latter only after several hundred thousand years had passed. Synthesis of heavier elements came still later, when galaxies and stars began to form.

The *big bang* model has gained such wide acceptance in the scientific world because its predictions have been experimentally verified: 1) It predicts a redshift in the optimal spectra of distant galaxies that is proportional to distance (Hubble's Law); this is observed. (The expansion of the universe causes this redshift. The 1922 solutions of Einstein's field equations by Soviet mathematician A.A. Friedman predicted an expanding universe seven years before Hubble verified it.) 2) The predictions of this model for the abundances of the lighter nuclei have also been verified. And 3), it predicts an observable "relic" of the primordial explosion -- a uniform background of microwave radiation with wavelength distribution for a temperature of 3°K -- which has been experimentally confirmed.

This is not to say that the *big bang* model has been conclusively proved [cf. NAS 1982 p 95]. Some problems remain (e.g., the horizon, flatness and smoothness problems [cf. Guth and Steinhardt 1984]). But, it is safe to say, the *big bang* model, suitably modified, rests on rather firm scientific ground and is certainly far from speculative.

The philosophical problems arise from extrapolation outside the domain of what is scientifically understood (as presented briefly above). Such extraphysical formulations are found even in the writings of highly respected cosmologists. For example, when Silk, et al. [1983] say,

> According to the *big bang* theory, the universe began as a singular point of infinite density some 10 to 20 billion years ago and pulsed into being as a vast explosion that continues to this day.

the phrases "the universe began" and "pulsed into being" imply that there was a Beginning to the universe and, hence, before that there was Nothing. But there is no scientific basis for such a statement. It is sheer speculation to extrapolate backwards in time from the state of matter at extremely high temperatures, to assert or imply that there was no universe before that time.

A quasi-physical justification for such speculation is also attempted in a recently proposed modification of the *big bang* model:

> If grand unified theories are correct in their prediction that baryon number is not conserved, there is no known conservation law that prevents the observed universe from evolving out of nothing. The [new] inflationary model of the universe provides a possible mechanism by which the observed universe could have evolved from an infinitesimal region. It is then tempting to go one step further and speculate that the entire universe evolved from literally nothing. [Guth and Steinhardt 1984 p 128].

Here at least the authors admit that the idea of the universe evolving from literally nothing is just speculation, which means it has no basis in science. Even assuming that no known conservation law of physics forbids this, it certainly does not follow that the universe did evolve from nothing; the basis for such a belief can only be found in idealist philosophy.

Others have proposed similar ideas and attempted to give them an air of physical reality [e.g., Guth and Steinhardt 1984 p 128]. Though such attempts have not gained acceptance in the physics community, it is disturbing to see them taken seriously. For if the whole universe can come from nothing, that is, without a prior cause, then why not any part of it? Why would any phenomenon need have a cause? Clearly, such speculations are deeply anti-scientific. Today's ideological crisis of capitalism creates an atmosphere in which such idealist speculations flourish in all areas of science, but cosmology seems to be particularly plagued with them. Consider the recent treatments of the *big bang* model by Weinberg [1977] in *The First Three Minutes*, and by Trefil [1984] in *The Moment of Creation*. Both books are serious, factually correct and well-written, yet their titles lend themselves to idealist interpretation (and Trefils even concludes with a brief excursion into religion).

The idealist business of making extraphysical extrapolations from scientific knowledge is hardly new, of course. In Lenin's time, the discov - eries of the electron and of radioactivity were used by philosophical idealists as a platform from whch to launch attacks on the materialist outlook; later, the Heisenberg Uncertainty Principle played a similar role.

There have also been reactions against *big bang* cosmology, sometimes intemperate as well as negative, and again not based on any scientific evidence. In the negative category are the attempt by Hoyle [1982] to refute the big bang model on a theological basis, as well as the Talkington [1983] deprecation of the model based in part on an interpretation of philosophical materialism. And Alfvén's ridicule of the *big bang* model was, as noted by Ginzburg [1981] rather extreme.

In these negative reactions there are two tendencies, both bad: 1) the attacks on materialist philosophy based on scientifically unwarranted extrapolations from scientific knowledge; and 2) attacks on science based on prejudice and, unfortunately, sometimes based on an incomplete under - standing of materialist philosophy [cf. Ginzburg 1981 pp 40-56, on the latter].

John Pappademos
Department of Physics
University of Illinois at Chicago

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But the Editor Looks at the Universe from a Different Frame of Reference

On top of everything else, these letters have impugned my philosophical morality. Am I really "bad" or merely "unfair" in criticizing the *big bang* model as "creationist" and "soft" science? [Talkington 1983]. In the following remarks, I try to make clear, at least implicitly, where I agree or disagree with their criticisms. But I am mainly concerned with presenting an entirely different view of the problem, putting the problem in a different perspective.

To begin, one could say that the concept of an expanding universe was born, if not in sin, at least as the product of a forced union between the

observational data and a particular solution of Einstein's field equations, with Edwin Hubble officiating at the positivist ceremony. Hubble's mode of reasoning is clear when he acknowledges that the data fit better with a nonexpanding universe:

> The assumption that red shifts are not velocity shifts is more economical and less vulnerable, except for the fact that, at the moment, no other satisfactory explanation is known. In the present status of observations and theory, [to account for something other than Doppler effects] we may evidently choose between a curious, small-scale [relativistic] universe and a new principle of physics. [Hubble 1936 p 626].

In pursuit of a relativistic expanding universe, Hubble made numerous *ad hoc* revisions of his model, including an adjustable parameter for spatial curvature in a closed universe, to avoid accepting the possibility of some presently unknown "principle." As one historian put it,

> The numerous adjustments and compensations that Hubble had found necessary to make in his efforts to save a relativistic, homogeneous and expanding model of the universe from the contradictory implications of his data suggested [to Hubble] that the model might be a "forced interpretation" of the evidence. [Hetherington 1982; 57]

The result of this "shotgun" match is today's velocity-distance paradigm, or standard model, in which the Doppler interpretation of observed redshifts determines distances and provides the foundation for our *big bang* cosmology with expanding universe. Without the interpretation as velocity-distance, our standard cosmology would have no *logical* basis, which goes a long way to explain why astrophysicists and cosmologists today are so quick to reject any questioning of the Doppler interpretation. (Usually they don't refute, simply reject.) So strong is the ideological grip of Hubble's vision that most astronomers today are literally unable to "listen" when the ruling paradigm is questioned in any way. Holcomb [1970] is quite frank about this:

> There is also a well-accepted correlation between redshift and apparent brightness of galaxies, so *any attempt to argue that galactic redshifts are not a true indication of their distance would not be taken seriously* [emphasis added].

I have never learned by what consensus process a phrase such as "well accepted" becomes transformed into scientific law, but Holcomb's prediction on the behavior of cosmologists was to prove accurate. When mathematician I.E. Segal gave a paper showing that, if Hubble's relativistic model is bypassed and correlations are made directly from the raw data for redshifts and observed magnitudes, the Hubble linear relation is rejected statistically while a square law is accepted [*cf.* Nicoll and Segal 1975]. Holcomb proved correct on the temper of the community, judging from this news report:

> [Segal's] assertion was greeted by the assembled astrophysicists with a chill as cold as intergalactic space. After the formal close of the session, a heated argument ensued between Segal and several prominent astrophysicists over a number of points, including whether the galaxies whose redshifts are known are a fair sample for statistical purposes. [Thomsen 1975]

This report also points up one of the more glaring contradictions of *big bang* cosmology: the doubt acknowledged by cosmologists about the sample of galaxies used, in strong contrast to the absolute certainty about the conclusions drawn from this sample. Hubble also expressed doubt about his sample since it refused to fit his model. (This doubt about the sample may be reflected in Pappademos' cautious use of the term optimal in asserting, above, that observations bear out predictions by the *big bang* model of "a red shift in the optimal spectra of distant galaxies proportional to distance," since this result is really obtained from a hand-picked sample of galaxies.) Segal, rather than hand pick his own, used a sample considered by cosmologists to be favorable for justifying the Hubble expansion [*cf.* Segal 1980].

Perhaps the contradictions of the velocity-distance paradigm show up most blatantly in the "Hubble constant," a parameter relating the redshift of extragalactic objects to their distances. The value of this parameter has been changed so frequently and by such considerable amounts, with each new system of selecting the sample of galactic redshifts and each new assumption concerning how to measure extragalactic distances, that one wag suggested renaming it the "Hubble variable" [Proctor 1973]. No satisfactory method of measuring or estimating these distances has been found: "Essentially we have failed," says an astronomer who has worked on the problem for over 20 years [Sandage 1984].

Some brand new contradictions in the velocity-distance paradigm were revealed by the 1960 discovery of quasars (quasi-stellar objects) characterized by sharply defined starlike nuclei and very high redshifts (up to 3.5 times the velocity of light, in the Doppler sense). Other startling features include highly variable redshifts and even redshifts that differ from place to place within the same object, the latter being interpreted in the standard model as quasar components moving apart at "superluminal" speeds. In the velocity-distance paradigm, quasars thus become exotic and mystifying objects, segregated in remote parts of the universe, with energy output difficult to explain in terms of known physics. But these difficulties are all associated with the Hubble linear relation of redshift to speed and distance. If this requirement is relaxed by simply assuming that not all cosmic redshifting involves the Doppler principle, quasars might cease to be such special objects and could take their natural place in a spectrum of active galaxies (Seyfert, etc.) which they otherwise resemble [*cf.* M. Burbidge and Lynds 1970].

Adherents of the standard model tend to be very intolerant of such heretical ideas. When Arp [1971] developed tangible evidence for several cases of a physical association between a quasar and a normal galaxy of lower redshift, thus contradicting the rigid distance/redshift relation of the Hubble model, the debate became vigorous indeed [*cf.* Field *et al.* 1973]. The antagonism to Arp's ideas has reached the point where astronomers openly discuss denying him access to the telescopes [Waldrop 1982], and a protest against Arp and others being excluded from symposium participation was reported with derision:

> In arguing that there is an almost overwhelming case for the reality of noncosmological redshifts in quasars, G. Burbidge hints darkly that others who favor his view have not been invited to speak at the symposium and closes with the declaration that a revolution is upon us whether we like it or not. In the papers immediately before and after his, [evidence is given that quasars] do have cosmological redshifts. [Osterbrock 1980]

Today neither side is giving any ground in the controversy that essentially revolves around the same contradiction that preoccupied Edwin Hubble originally:

> MAJORITY VIEW: There is no known hypothesis besides Doppler shifts that is both consistent with the laws of physics and able to explain the large redshifts of galaxies.....If discordant redshifts truly exist, then the known laws of physics do not apply to some galaxies [Bahcall 1973].

> MINORITY VIEW: [We] have no physical theory which will explain this phenomenon, and this is treated, by many, not as a challenge but as an objection to the evidence.....However, the evidence is there, and if we are really searching for the truth, we ignore it at our intellectual peril [G. Burbidge 1981].

As is to be expected, the scientific community includes a sizable group of materialist skeptics who are looking for more definitive answers:

> MIDDLE VIEW: [P]lausible as our current picture of quasars may be, there is some chance that it is entirely wrong and a good chance that it is wrong in some particulars.....Rarely, if ever, is a large body of data collected and suddenly explained at a stroke by an inspired theory. So it is likely to be with quasars. [Osmer 1982]

> *Have we the right picture for the physical universe?*.....the basis for the standard relativistic cosmology is substantial but hardly definitive. It is not surprising therefore that there has been on-going discussion of possible alternatives.....Interpretation of the observed galaxy redshift as the simple Doppler effect has been questioned by many people on many grounds. [Peebles 1981]

The responsibility of any scientist for developing (and defending) a given theory is relative, depending on the nature of the information about the phenomenon and on insight concerning how to connect this up with existing knowledge. On the other hand, the responsibility of Marxists for exposing idealism is absolute, particularly so when a scientific theory provides the basis for mystifying the public. In the case of cosmology, the contradiction between empirical knowledge and the mathematical theory supposed to represent this knowledge has proved to be a boon for the mystifiers who work both inside and outside the scientific comunity.

If Weinberg can use the standard model confidently to describe conditions one-hundredth of a second after the *big bang*, why is it less "scientific" for other scientists, using the same mathematical model, to discuss their ideas of what happens at time zero when the density and temperature are both infinite. The only practical way to plug up this opening for "creationists" and other mystifiers is to acknowledge openly the basic contradiction in the model, even though such materialist candor takes away all the media appeal and lets the public in on the secret that the whole theoretical structure may be subject to change anytime in an unknown direction and to an unknown extent.

The main thrust of Weinberg's work is to gloss over this contradiction and thus contribute to the mystification. An instance is his [1984;21] description of *big bang* itself:

> In the beginning there was an explosion. Not an explosion like those familiar on earth, starting from a definite center and spreading out to engulf more and more of the circumambient air, but an explosion which occurred simultaneously everywhere, filling all space from the beginning, with every particle of matter rushing apart from every other particle.....it matters hardly at all in the early universe whether space is finite or infinite.

That's poetic but hardly theoretic. I could not find, even in his Mathematical Supplement, how he reconciles this with the cosmic singularity. The question nowhere addressed is how, according to the laws of thermodynamics, all space ("everywhere") and all matter got compressed to a temperature, in his version, of 10^{11} °C. or higher.

Now I don't mean to throw out all the work on cosmology to date, for I have no doubt that much of it reflects to some degree some aspect of the reality of our universe. But I do think that the ideological bent in this effort has distorted the results in a predictable direction. It is no accident that Weinberg, on the first page of his preface, discussing his motivation for a book on the early universe, asks us: "What could be more interesting than the problem of Genesis?"

The Marxist approach to cosmology would be, it seems to me, to start by acknowledging that we do not now and may never have any basis for precise (absolute) knowledge of any beginning or whatever. Like Lenin's electron, this subject is inexhaustible and, as he adds, "nature is infinite... it infinitely *exists*" [1908 p 262]. A materialist cosmology would not give so much credence to a vast theoretical structure erected on the sands of changing interpretations, but would give more attention to the dialectical processes of birth and death that are observed right now in the universe around us:

> 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]

This cosmic process of death and rebirth is described more concisely by a Soviet philosopher: :

> The latest discoveries of astronomy show that the cycle of matter in the Universe does not cease for a single moment. In some regions of cosmic space matter and energy are dispersed, in others they are re-concentrated, giving rise to new celestial bodies. Soviet scientists have established that new stars are still being formed, and not merely single stars but entire groups (associations) of stars. [Afanasyev 1980; 50f]

Now I have the notion that the observed cosmic background radiation and abundances of elements, supposed to support the *big bang* theory, may well find a more materialist explanation in the process described above, which occurs constantly at many levels in the realm of moving matter. For example, though astrophysicists have barely begun to understand the vast energetic processes in the center of our own galaxy, the explorations now under way may someday provide a better materialist basis for building theoretical models of the early universe. Meantime, let us consider the contradiction of a cosmology that offers a more precise picture of the origin of the universe, a process well removed from direct observation, than it can offer for the origin of our own galaxy, where actual observations are changing our concepts almost daily.

I don't mind acknowledging that my ideas involve a bit of speculation that goes beyond our firm empirical knowledge. Contrary to some mecha - nistic materialists, I think such speculation is a necessary part of the creative discovery process in science. In the end, the usefulness of any speculation depends not on its logical rigor but on how much its insights prove to help unify and expand our knowledge.

I will close by pointing out the one aspect of Marxist methodology that has most informed my approach to the problems of cosmology. The em - phasis here has been on the contradictions to be found within the standard model because, in my opinion, the discovery of contradictions opens the way to new modes of thinking about a problem. The search for contradictions helps the investigator in many ways: 1) to maintain sensitivity to the ambiguities of knowledge and reduce the likelihood of being taken in by a prevailing orthodoxy or momentary fad; 2) to help assure a many-sided approach to a problem and avoid the narrow metaphysical approach that has led many a scientist into a theoretical deadend; and 3) to put the finger on untruths that exist within any theoretical structure and thus avoid complacent acceptance of the too-easy consensus that *explains away* challenging new evidence.

A scientist should learn to think in terms of contradictions, look upon any study as a search for contradictions, and lovingly treasure each contradiction discovered along the way. (Most of the content in this discussion of the *big bang* model is based on a file labeled "Contradictions in Cosmology," my 30-year collection of notes, reprints and clippings on anything anomalous found in the scientific literature or the popular press.)

The search for understanding through contradictions seems to me central to the mode of thought we call dialectical logic. Good materialist judgment is needed, of course, in using this type of logic. But we get this sort of judgment by combining scientific knowledge of specific subject matter with Marxist understanding of the general dialectics of development and change. Thus we learn to articulate theory with practice in science, just as in social and political activities. This *dialectical* logic is what lifts the outlook of the scientific materialist to the higher level of consciousness of the *dialectical* materialist.

Lester (Hank) Talkington

POSTSCRIPT: I must also mention still another form of empirical evidence indicating that observed quasar redshifts may not be related to the supposed motion of an "expanding" universe. In mathematical cosmology, to allow for the supposed increase of photon wavelength caused by expansion of the universe, it is standard practice to include a factor of $(1+z)^{-2}$ where $z=v/c$ is the ratio of the supposed Doppler recession v to the speed of light c. In studies of Faraday rotation of the polarization of radio signals from quasars, however, it was found that the factor $(1+z)^{-2}$ has a "watering down" or "diluting" effect on the observed positive correlation between detectable rotation measure and optical absorption, with both the rotation and the absorption evidently occurring in the same intervening cloud system [Kronberg & Perry 1982; Welter, Perry & Kronberg 1984]. These authors simply remove the offending factor without offering any explanation for its "diluting" effect on their empirical results. Obviously, their finding calls into question the relativistic formulation of quasar red shifts. The most economical explanation for the effect is simply that the supposed relativistic "expansion" of photon wave length does not occur at all or not in anything like the magnitude indicated by equating observed quasar red shifts with recession velocities. Who knows how many more important instances of evidence against a simplistic expanding universe concept will be uncovered through reinterpretation of present knowledge by those who dare to question the standard model!

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The reification of reality

Reality is for people who cannot deal with drugs.

Drugs are for people who cannot deal with physics.

Physics is for people who cannot deal with reality.

-- Scribblings (in three different handwritings) found on men's room wall in physics department at a large university [LT].

How Ideology Relates to Natural Science, with Examples from Geology and Cosmogony

DAVID W. SCHWARTZMAN
Geology and Geography
Howard University

MOHSIN SIDDIQUE
1634 Montague St.N.W.
Washington DC 20011

RESPONSE BY LESTER TALKINGTON

THE RELATION of ideology to the natural sciences is an important issue on which the variety of competing views ranges from claims of their absolute dichotomy to their identity. It is our contention that ideology and natural science are inseparably interpenetrated but irreducible to one another. They are opposites in dialectical unity. We believe that the source of the ideological penetration of the natural sciences is not limited to obvious political influences on scientists such as militarism, racism and sexism [cf. Rose and Rose 1976] and to philosophical interpretation of theory, but is found right in the core of science in the production of knowledge as an inseparable influence. This latter aspect of ideology has not been generally recognized--it goes beyond the usual Marxist definition of ideology.

It has long been accepted in Marxist analysis that the social sciences are intimately bound to ideologies that represent class interests, and that this crucially affects the theories. With respect to the natural sciences, the relation to ideologies is not as obvious. Some Marxists [e.g. Konstantinov et al., 1974] maintain that ideological factors usually function at the level of philosophical interpretation of theories of the natural sciences but imply that the normal production of knowledge is relatively free of ideological defor-mation because ideologies in their view reflect class interests, directly or indirectly, and it is nonsense to talk about "bourgeois and proletarian math-ematics or chemistry". We agree with this position with respect to class interests, but we think the view is inadequate for explaining the depth of the ideology/science connection.

Another accepted result of Marxist analysis is that a scientific community can have its own body of ideology. Parekh [1982] points out, based on a close reading of Marx, that every social group, and not merely classes as such, adopt systematically biased ideological concepts reflecting their com-plex place in society. Talkington [1981] also emphasizes this point in relation to science (our differences with his approach are pointed out below).

Let us define what we mean by ideology: ideology is a system of views and ideas reflecting the lived relations of people with their world (including, of course, the collective of scientists), and functioning therefore as a material force in society, embodying itself in institutions which tend to reproduce the dominant ideology. Poulantzas [1978] provides a lucid discussion of ideology in this general problematic. In particular, following Althusser, he develops the concept of ideology of the dominant class in capitalist society as functioning to "fix men's real relation to their conditions of existence in the form of an imaginary relation" by "hiding the real contradictions." Ideologies derivative from scientific theory--and all modern ideologies have some influence from science (even Creationism!)--in this context will function as a technology of ideas/concepts, analogous to technology functioning as applied science. Thus, such ideologies as Social Darwinism will be useful in the reproduction of the dominant world view just as technology functions in the production of commodities and reproduction of the means of production.

Marxist ideology, derivative from the scientific theory of historical materialism, is of course the main contender against bourgeois ideology in our day and age. The ideologies of the scientific community in the modern era have reflected directly and indirectly the class struggle and its expression in ideological struggle on a global scale. The generation of scientific theories occurs in the context of the ideological struggles in the scientific community, struggles which are inseparable from controversies arising from the dialectical development of scientific knowledge. We argue here that a neglected aspect of this development, from the point of view of philosophical analysis, is the interpenetration of science and ideology in the core of the "productive process" of science. The finiteness of scientific theory facing the qualitative infinity of nature is an inevitable outcome of this productive process.

It is through the scientists' world view, a finite complex of ideological and scientific concepts, that objective reality is reflected and new theory is generated, elaborated and tested. Since no theory ultimately proves to be complete, in the sense of encompassing all the qualitative infinity of the world, it inevitably reaches an impasse in its self-reflection. There is always an unknown, a depth to the real outside the comprehension of any theoretical structure; this is an ever-present boundary condition in the history of a science, shifting with the continual revolutions of its theoretical and experimental apparatuses. This finiteness of theory generates an ideological quality which interpenetrates with the ideological effects of class struggle simultaneously with the interpenetration of ideological and scientific concepts. The "internal" generation of an ideological quality to theory, alongside the "external" can be described as the process of "ideologisation" of science, an historical process to be understood in its concrete aspects by the science of the history of the sciences. Although the internal ideological quality is, unlike the external, not a reflection of class interests, we label it ideological because its effects are so similar to the classical Marxist concept of ideology. We will now proceed to discuss the typology of this interpenetration of science and ideology, as well as their irreducibility as dialectical opposites.

In contrast to science, ideology does not produce knowledge of the objective world, which of course includes society and its ideological apparatuses (e.g., church). This quality of ideology was emphasized by Althusser [1969, 1971]. Implicit in this concept is that ideologies are closed in themselves; unlike the sciences, ideologies are not open ended by their own means of comprehension. This is what we think Althusser means when he says ideologies have no history. Ilyenkov's [1977] phrase "dogmatised image" is particularly apt as a description of ideological conceptions. Macherey (1978) put it this way: "Ideology's essential weakness is that it can never recognize for itself its own real limits". It is in this sense that ideology is an "opposite" to science, more precisely, to that aspect of scientific development which deepens our knowledge.

Yet scientific theories do reach their limits, become ideological (a faith, a dogma) as an inevitable process of scientific development. As Althusser [1970] put it:

> ...ideology not only lies in wait for science at each point where its rigor slackens, but also at the furthest point where an investigation currently reaches its limits.

For example, the continued adherence by sections of the scientific community to the theoretical framework of Newtonian physics during its crisis in the late 19th and early 20th centuries (and even now by a tiny group of dissenters!) has all the characteristics of belief in religious dogma. To be sure, Newtonian physics is certainly still useful in everyday life (except in some of the everyday life of the physicist, e.g., nuclear physics). This usefulness is simply the result of the validity of equations of Einsteinian physics reducing to those of Newtonian physics for velocities much less than the speed of light. The meanings of mass, time and space in the reduced equations are in a theoretical sense still profoundly different, as pointed out by Kuhn [1970].

Thus, the science/ideology connection has both external and internal dimensions. The external relations are more easily seen as contamination of scientific theories by, say, theological conceptions (as in Jastrow's interpretation of big-bang cosmology as the act of the deity). Indeed, on the level of philosophical interpretation of theories, as well as the institutional materiality of science, we find unmistakable penetration by ideology via religion, bourgeois philosophy and political direction. But it is the internal aspect which is most disturbing for those of us who accept the progressive character of scientific advance, in the sense of a deepening of knowledge. Today's scientific theory, which moves to a fuller and deeper knowledge of the world is tomorrow's ideological dogma. This paradox is closely related to the paradox of relative truth (and its core of absolute truth) recently discussed by Narskii [1979]:

> If absolute truth...is to be singled out of the composition of some given relative truth as its "core", which will not be canceled by the future development of cognition, and if, further, we take account of the fact that it will never be possible, either today or in the future to establish with absolute accuracy the boundary between this "core" and its "envelope," then one asks what that "envelope" is from the standpoint of exact epistemological identification, i.e., what the remaining part of relative truth is. The answer that it is another relative truth will not work here, because it merely enlarges the "core", shifting its boundary somewhat closer to the periphery, but at the same time facing us with the same question once more. Also unsatisfactory is the answer that, outside the confines of the absolutely true "core", one always finds errors (lies) because this would imply an interpretation of relative truth as the sum of absolute truth and lies. That is not only untrue in application to special cases of truth, as found in court or on the athletic field, but is erroneous in general, for the very relativity of truth would then resolve to falsity, and the process by which absolute knowledge grows, would resolve to simple summation.

Narskii holds that this paradox is entirely soluble by a deeper understanding of the status of relative truths (through the dialectical relation of relative and absolute truth). The growth of absolute truth in scientific knowledge consists of its deepening as a whole, of the ascension from the abstract to the concrete, in the constant transformation of its conceptual object. As Ilyenkov [1982] put it:

> The specific and characteristic feature of theoretical assimilation (as distinct from mere empirical familiarity with facts) is that each separate abstraction is formed within the

general movement of research towards a fuller and more comprehensive, that is concrete, conception of the object. Each separate generalisation (according to the formula "from the concrete to the abstract") has a meaning only on condition that it is a step on the way to concrete comprehension of reality, along the way of ascending from an abstract reflection of the object in thought to its increasingly concrete expression in the concept..

Talkington [1981] maintains that scientific theories consist of an "operative" part which is objective and an "interpretive" part which is ideo - logical. The latter roughly corresponds to the "external" aspect referred to above, while the "operative" part corresponds to the "internal" aspect of theories. This argument reduces the objective side of the theory to empirical observations and mathematical operations, approximately the Smirnov [1970] "empirical stage of knowledge" as distinguished from the theoretical which is explanatory of the empirical connections. This view appears to us as empiricist, reducing the core of theory to an operationalist mechanism. A mathematical formulation of theory is just as ideological as some popular interpretation, particularly when the theory reaches its limits. Moreover, even the "empirical" is profoundly informed by theory as argued in many recent critiques of operationalism and empiricism (Hawkins, Feyerabend, Bunge). We do agree with Talkington that the interpretative side of theory is more transparently ideological, but the relative opacity of the operative part presents the real challenge to scientists and philosophers to understand its interpenetration with ideology.

An aspect of the contradictory character of the science/ideology relation is the shifting positive/negative influence of ideologies on the development of scientific theory. Just as these theories in their limits become ideological, ideological concepts can anticipate, in embryonic form, scientific theory and can provide critical stimulus or ammunition to scientific development even at times far removed from the ideology's birth (e.g., ancient Greek materialist atomic theory) [1]. Yet in itself the system of particular ideological conceptions is utterly empty of knowledge production. We maintain therefore that, though in science's ascent to deeper knowledge it is irreducible to ideology, it is impossible to guarantee the definitive isolation of scientific truth from ideological conceptions at any historical moment, including the case of matural sciences far removed from their pre-scientific past (e.g., modern physics) but in science's ascent to deeper knowledge it is irreducible to ideology. The absence of this guarantee leads some authors who sympathize with Marxism to identify science as an ideology (e.g., the praxis school; and see Dickson (1979) for a stimulating essay). The extreme of the relativist position is found in Feyerabend's anarchistic philosophy of science (*Against Method*). On the other hand, errors arise from the absolutist position as well. Althusser in his strong defense of the autonomy of scientific practice has tended to see it threatened from without by ideologies but has failed to treat the interpenetration in any depth or to recognize the necessary inseparability at every stage of development [2]. Further, he retreated from his characterization of materialist dialectics as the theory of the sciences to a position that Marxist philosophy is a practice of political-theoretical intervention, without its own object or possibility of development as a metascience [see Althusser 1976, Schwartzman 1975]. Glucksmann [1974] has pointed out the weakness in this position of absolute externality of theoretical and ideological practice [3]. Ironically, Althusser in his absolutist position finds

himself in the company of the neopositivist A.J. Ayer and the neo-Thomists in their insistence on the need for the de-ideologisation of natural science.

Ideologies vary greatly, of course, in their relations to science at any given time; this is determined most directly in the case of the social sciences by the class interests informing the ideology; working class (Marxist) ideol - ogy as a revolutionary material force in society is guided by the science of society, historical materialism, but as an ideology itself does not produce knowledge of the social formation. Yet, on the terrain of political struggle, this ideology has an increasingly vital role in forcing the direction of research programs of science (e.g., cancer research, environmental crisis).

The histories of geology and cosmogony (origin of the solar system) are particularly rich in the interplay of science and ideology. Geology can be said to have emerged as a science in the Huttonian-Lyellian revolution. Hutton used the ideological conception of an endlessly cyclical, balanced Earth without a beginning as a weapon against creationism and its attendant catastrophism. This notion is in part derived from analogies to agricultural practice and the physiology of humans (Hutton's M.D. Thesis at Leyden was entitled "The Blood and Circulation in the Microcosm"). He later wrote:

> We are...thus led to see a circulation in the matter of the globe, and a system of beautiful economy in the works of nature. This earth, like the body of an animal, is wasted at the same time that it is repaired. It has a state of growth and augmentation; it has another state which is that of diminution and decay. This world is thus destroyed in one part, but it is renewed in another.....we have the the satisfaction to find that in nature there is wisdom, system, and consistency.....The result therefore of our present enquiry is that we find no vestige of a beginning, no prospect of an end [quoted, McIntyre, 1963]

This was a retreat in one sense from the conception of the world with a natural beginning (e.g., Kant's cosmogony, which broke with Newtonian ahistoricism) but contributed to the emergence of the crucial concept of the knowability of the past from a knowledge of present processes operating (e.g., erosion, volcanism) and its translation into the scientific methodology of modern geology (uniformitarianism or, better, "actualism"). Hutton's retreat was carried further by Lyell, whose systematic formulation of the doctrine of uniformitarianism had a critical influence in Darwin's concept of evolution by natural selection on an Earth with a sensuously inconceivable age. Lyell made a double retreat; he acknowledged Darwinian evolution only in the last editions of his Principles of Geology.

Indeed, the battle of the Uniformitarianists and the Catastrophists, detached from Biblical chronology [4], was complex and uneven in its development, as Stephen Gould [1977] has lucidly described. Modern geology has tended towards a synthesis of uniformity and catastrophe and the issue is not settled, as Gould [1979] argued concerning the episodic nature of biologic change, relating gradualism to the conservative side of bourgeois ideology of the 19th century. Thus, we can recognize the rough outlines of the process by which the ideological sources available to Hutton, a Scottish farmer/doctor/geologist, were raw materials that entered into the theoretical foundation of geology, a theory that bears its ideological birthmarks two centuries later. The argument over the relative weight of uniformity and catastrophe in global geological theory still reflects the old controversies, but in its critical self-consciousness modern geology has moved far from ideological determination, at least from this source. Some geologists today fear, however, that plate tectonics, the most recent revolution in geology (and enormously fruitful), is now reaching its limits (becoming ideological!) as a conceptual framework.

Cosmogony has its own curious and uneven history. It emerged just before the French Revolution, with Count Buffon's catastrophic theory of the solar system's origin by impact of a comet with the sun and the decisive rejection of Biblical chronology. The Kant-Laplace nebular hypothesis [5] depicted the solar system's origin in a strikingly modern form, though of course in a highly speculative model. Its great flaw, the concentration of angular momentum in the sun (instead of the planets) set the stage for the revival of collision theories in the late 19th and 20th centuries. James Jeans' theory of tidal disruption of the sun by a passing star, successfully explained, on the face of it, the distribution of angular momentum, and thus dominated cosmogony this century up until recently, though his physics was woefully inadequate in accounting for planet formation. Its great popularity is probably due to its effective revival of anthropocentrism. Academician Schmidt [1958], also founder of a school of cosmogony, put it this way:

> The Jeans hypothesis lasted longer than any of the other 20th century hypotheses. The reason for its popularity was not its scientific value (it had none).....but because it was the most acceptable to the idealist, religious philosophy predominating in bourgeois society.

This view is supported by the following extracts from the highly influential work by Eddington [1929]:

> The solar system is not the typical product of development of a star; it is not even a common variety of development; it is a freak.....By elimination of alternatives it appears that a configuration resembling the solar system would only be formed if at a certain stage of condensation an unusual accident had occurred. According to Jeans the accident was the close approach of another star casually pursuing its way through space.....Even in the long life of a star encounters of this kind must be extremely rare.....I should judge that perhaps not one in a hundred millions of stars can have undergone this experience in the right stage and conditions to result in the formation of a system of planets.....I do not think that the whole purpose of the Creation has been staked on the one planet where we live; and in the long run we cannot deem ourselves the only race that has been or will be gifted with the mystery of consciousness. But I feel inclined to claim that at the present time our race is supreme; and not one of the profusion of stars in their myriad clusters looks down on scenes comparable to those which are passing beneath the rays of the sun.

The concept of uneven development of the sciences and their ideological interpenetration is essential to an understanding of the formation of scientific theories such as cosmogony, which can be seen as a "premature" theory in the 18th, 19th and early 20th centuries (i.e. dominantly ideological). It could not advance beyond a series of ad hoc hypotheses for lack of knowledge concerning stellar processes and evolution. Laplace was the Greek materialist philosopher and Jeans the idealist, basing their theories on Newtonian physics to be sure. Modern cosmogony has emerged as a systematic global theory drawing on astrophysics, geochemistry, planetology, etc., and significantly incorporating a synthesis of Laplacian historical uniformity with catastrophism (recent evidence for a supernova trigger in the collapse of an interstellar dust cloud [6]). But cosmogony will only advance from its ideological geocentrism, tending to stress uniqueness of our planetary system

rather than some probable lawful regularities governing planetary system formation, with the discovery of extrasolar planets in the coming decades.

The interpenetration and irreducibility of ideology and the sciences, we maintain, can best be approached through the categories of materialist dialectics and historical materialism. Yet dialectical materialism, as the theory of the sciences with its own relatively autonomous mode of production, has its own history of ideological penetration. The classical tradition reiterated today with citation of text and repetition of metaphorical formulations has reached its ideological limits, while the attacks from bourgeois ideologies continue in different forms of penetration (no dialectics of nature, only human practice, e.g., praxis school; mechanistic materialist reductionism). This requires giving priority to the development of materialist dialectics as a living combative theory of the sciences, as a metascience uniquely rooted in all the sciences.

NOTES

[1] Cf. Thomson [1955]. Also, see A. Clegg [1979] on the complex of technology/ideology of craftsmen as the basis for experimental science, Althusser [1969] on ideology as the raw material for birth of science.

[2] Lecourt [1975] of the Althusserian school approaches this position in his essay on Foucault's The Archeology of Knowledge: "No longer to stress unilaterally the autonomy of the history of the sciences, but to mark out at the same time the relativity of that autonomy" (p.205).

[3] Glucksmann's position is shared by Larrain (1980).

[4] In 1664, with literal interpretation of the Bible, Archbishop Usher "computed" the time of Earth's creation as 9:00 AM, Oct. 26, 4004 B.C.

[5] Note that "a detailed comparison with Kant's theory should be made: it would show that theological reasoning has still a great place in Kant's work, whereas it has none in Laplace's" [Merleau-Ponty 1977].

[6] Studies of meteorites have revealed anomalies in isotopes of several elements suggestive of additions to the primitive solar nebula shortly before the formation of the planets.

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Critical Comment by Lester Talkington

A problem of "commensurability" arises in formulating a response to the Schwartzman/Siddique critique of my model for ideology in natural science. They state the issues in terms of Althusser's "structuralist" viewpoint, which has a conceptual basis that is truly worlds apart from my Marxist-Leninist viewpoint. In the first place, they portray science and ideology as "inseparably interpenetrated...opposites in dialectical unity," and thus make ideology a form of anti-science. This Althusserian concept of ideology, as the absolute opposite of science, is usually justified by quoting Marx and Engels who, in their struggle against bourgeois and petty bourgeois ideology, emphasized its nagative aspects. It was Lenin [1902] who redefined the term and gave us its present more generalized usage in which the character of an ideology depends on its content -- revolutionary, reactionary or whatever. To adopt this Leninist usage is much more than a simple matter of individual preference since, for example, it involves the question of whether or not Marxist ideology is scientific.

The Althusserian orientation of Schwartzmann/Siddique leads to basic divergences between us on other crucial questions concerning the nature of scientific knowledge. For one, they follow Althusser in the tendency to depersonalize science, i.e., to treat the production of scientific knowledge as if it were something coming off an assembly line rather than being the result of an intensely personal/social process carried on by highly conscious subjects (human beings!). In this respect, Althusser's ideas resemble neo-positivism, which also seeks to eliminate the subjective from the scientific process. Though these authors criticize Althusser for his positivist tendency to "absolutism," they follow him in the positivist denial of the subject/object dialectic. This tendency shows up in their phrases such as "the productive process of science" and "a technology of ideas/concepts." Even when they acknowledge the presence of scientists in the process, they put this only in collective terms of "the scientific community," never in terms of the individual scientist and his/her subjective thought processes. Note the use of the plural here: "It is through scientists' world view...that objective reality is reflected and new theory is generated, elaborated and tested."

How much more realistic is the many-sided Marxist-Leninist formulation: "One could say that [ideology] is a unity of individual and social consciousness.....the individual's consciousness, while retaining its characteristics, links up with social consciousness and in a sense expresses it" [Fedoseyev 1978].

The other side of their coin, so to speak, is the Schwartzman-Siddique tendency to personify ideology (as anti-science), endowing it with relative autonomy and independence so that scientists seem like marionettes manipulated by an ill-defined but potent thing (ideology), "functioning...as a material force in society...to fix men's real relation to their conditions of existence." This transformation of ideology into an actor is the logical outcome of the Althusserian fiat that puts ideology on a par with science, as its "dialectical opposite." Only in such an artificial context can any sense be made of their statement that "ideology does not produce knowledge of the objective world." In the Leninist context, science is only one of several processes by which people gain knowledge of the objective world. Ideology may influence such processes but in itself never "produces" anything:

> Ideology expresses and orients human consciousness within the system of relations and natural interconnections, and provides a set of initial values and tenets which influence the behavior and way of life of social classes, groups and individuals. The concepts and ideas which make up an ideology become a man's convictions and take an active part in shaping his attitude to to all vital phenomena and events in the world. [Fedoseyev 1978].

Taking the view that scientists are the actors, I proposed a base-and-superstructure model for the scientific process [Talkington 1981], where the *base* consists of the objective or operational components while the *superstructure* comprises the ideational or ideological elements of a theoretical structure.

Here, *operational base* refers to the objective, empirical components of a theory -- for example, the elements which go into the "methods and procedures" section of a scientific report, with the primary purpose of assuring reproducibility of the results. The operational elements include laws, equations and exemplars that show how the theory attaches to reality, how it can be applied to solve problems. For example, anywhere in the world Einstein's $E = mc^2$ will be recognized as the relativist description of operations for calculating the potential energy of matter (in designing nuclear weapons, etc.). Similarly, Maxwell's equations and Newton's laws are associated with objective procedures, embodied in engineering formulas, tables and sample problems.

By *ideological superstructure,* I mean all the elements of conceptual interpretation -- statements about the meaning of operations, what kind of entities are being manipulated in the experimental apparatus and in the equations used to massage data, etc. This is the side of theory where basic arguments occur, such as the famous Bohr-Einstein debate, where all the ambiguities persist concerning the nature of the photon or the electron, the nature of evolutionary processes, disease processes, or stellar processes. No theory can be useful in the search for new knowledge without giving play to this subjective interpretational side. Though the scientific method works well in eliminating the subjective factor from the operational base, this is neither possible nor desirable for the ideological superstructure where passionate debate is just as necessary as the tradition of scientific integrity.

Obviously, the ideological superstructure in my model is completely internal to the process of science and, hence, in no way corresponds, even roughly, to the external aspect of science as charged by Schwartzman and Siddique. True, the superstructure does provide a window through which outside concepts are brought into scientfic theory; hence, mine may well constitute the first concrete proposal for a cognitive mechanism by which social ideas can be smuggled into natural science.

Similarly, while the operational base certainly includes the empirical content of science, this has nothing whatsoever to do with philosophical empiricism or operationism, as Schwartzman and Siddique charge, since these philosophical systems seek to minimize the role of the subjective while my model gives full play to the creative interpretational aspects of science through the ideological superstructure. In particular, my model treats a mathematical equation as an objective tool for scientists which, in itself, is no more ideological than a laser or a centrifuge; ideology enters in considering how such tools are used in social processes.

There is, of course, constant interaction between operational base and ideological superstructure as the entire theoretical structure develops and changes through practice, thus neatly comprehending the theory/practice dialectic. Clearly, the complexities of this dialectical model would never fit into the narrow Althusserian conceptual scheme.

Significant, too, is the difference in the way the two models treat Marxist ideology. Schwartzman and Siddique must beat around the bush, saying that "working class (Marxist) ideology as a revolutionary material force in society is guided by the science of society, historical materialism, but as an ideology itself does not produce knowledge of the social formation." In my model no hedging is needed and one may come right out and say that Marxist ideology is scientific in nature, based as it is on Marxist scientific philosophy (historical and dialectical materialism are, of course, part of the ideological superstructure.)

Another fundamental difference between our two approaches relates to their point about ideology penetrating to the very core of science: they draw a parallel with Narskii's discussion of relative truth, its core of absolute truth, and the dialectical relation of absolute and relative truth. It seems more helpful to conceive of scientific knowledge, not as having a "core" of absolute truth, but rather as a dialectical interpenetration of relative and absolute truth, an intertwining of truth and error rather than the interpenetration of science and ideology claimed by the authors. This analysis has led me to realize that the basic error of the Althusserian approach must be its neglect of the relative/absolute dialectic, with a consequent confusing of science with pure truth, of ideology with absolute error. Many statements by these authors would make much better sense if rephrased with this dialectic in mind:

AUTHORS SAY: The interpenetration of science and ideolgy.
REPHRASED: The interpenetration of truth and error in science

AUTHORS SAY: It is impossible to guarantee the definitive isolation of scientific truth from ideological conceptions at any given historical point.
REPHRASED: It is generally impossible to separate completely the truth from the error in a given theoretical structure.

AUTHORS SAY: A mathematical formulation of theory is just as ideological as some popular interpretation, particularly when the theory reaches its limits.
REPHRASED: The mathematical basis of a theoretical structure usually becomes just as useless as the interpretive superstructure when the theory is used outside the limits of its validity.

Finally, the way in which Althusser's "structuralist" approach acts as a conceptual straitjacket is nowhere more evident than in his ahistoricism, typified by the statement that "ideologies have no history." Schwartzman and Siddique do not challenge this statement but merely interpret it to mean that an ideology is a closed system, not open to self-correction and self-development in the way that science is. (Presumably, this interpretation reflects the kind of autonomy they ascribe to ideology.) But when they apply Althusserian theory to actual science, things do not turn out exactly as theory predicts. In their very interesting account of geology's origins, for example, it turns out that ideology does have a history, a development, and even an end. While they remark that theoretical geology still bears the ideological birthmarks left by Hutton two centuries earlier, they also state that "in its critical self-consciousness modern geology has moved far from ideological determination, at least from this source."

In fact, their example tends to support my thesis that scientific ideology is a part of the theoretical superstructure, and thus subject to self-correction in the same process that characterizes the development of scientific knowledge. This is the process described by Engels [1894]:

> *It is self-evident that where things and their interrelations are conceived, not as fixed, but as changing, their mental images, the ideas, are likewise subject to change and transformation; and they are not encapsulated in rigid definitions, but are developed in their historical or logical process of formation.*

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Another Marxist View of Structure in Theory

Each theory is complex in structure. For example, two parts may be distinguished in physical theories: formal calculations (mathematical equations, logical symbols, rules, etc.) and a "substantive" interpretation (categories, laws, principles). The structure and treatment of this "substantive" part of theory are connected with the scientist's philosophy and with definite methodological principles of approach to reality. *Dictionary of Philosophy*, M. Rosenthal and P. Yudin, eds. Moscow: Progress 1967 p 449.

[Unfortunately, this notable passage does not appear in new 1984 edition; see review of *Dictionary of Philosophy*, this issue. Editor.]

Concern Over the Militarization of U.S. Science

David Dickson, *The New Politics of Science*. New York: Pantheon 1984, 404 pages $22.95

DAVID DICKSON is European correspondant for *Science* (journal of the American Association for the Advancement of Science) and was 1978-1982 Washington correspondent of *Nature*, the comparable British journal. His recent book, *The New Politics of Science*, presents a very good in-depth commentary on recent fundamental changes in patterns of control over the activities of U.S. science. In the present phase of the global scientific and technological revolution (STR), these changes, some of which are discussed by Dickson, have significance far beyond the confines of the scientific establishment. Although he has been accused of Marxist formulations, it seems more correct to consider him a liberal who is deeply concerned over the growing anti-democratic and militaristic direction in which U.S. science is being forced.

Dickson divides the history of U.S. science policy since World War II into three phases. In phase I, roughly 1946 to the mid-1960s, federal research budgets grew steadily, rising from 0.3% of the GNP in 1946 to 0.8% by 1956. During this period most federally-supported research was controlled by the Department of Defense (DOD). With the 1957 launching of Sputnik, the U.S. space effort was accelerated and in 1958 the National Aeronautics and Space Administration (NASA) was created. Basic research in universities, which before the war had received hardly any federal funding, now got generous federal support (increasingly through the the National Science Foundation (NSF) which, though established in 1950, played only a small role for a number of years.)

In phase II, approximately 1966 to 1976, under Presidents Johnson and Nixon, U.S. research and development (R&D) expenditures leveled off. In fact, Federal basic research support in real terms (i.e., after correcting for inflation) actually declined by 10% between 1968 and 1971, and then stayed practically constant for the next four years. In this period, U.S. corporations cut their spending on basic research drastically, concentrating instead on efforts likely to produce an immediate payoff. Federal spending on basic research also declined relative to applied research as pressures from the environmental movement and anti-Vietnam war movement brought federal support for projects with social relevance

Phase III, which began in the late 70s and continues to the present, is marked by less and less emphasis on socially relevant research, a rising emphasis on and support for basic research (but in areas like physical science and biotechnology which have military or industrial application). For example, in the four years of the Carter administration (1976-1980), funding for basic science rose by 10.8% in real terms. Reagan's budget requests for basic research funding have grown even more rapidly (almost all administered by the DOD.)

Dickson is deeply concerned with the decision-making process involved in the setting of national priorities for directions of R&D. As he puts it, he tries to show that "decisions ranging from the broad allocation of scientific resources among competing areas of basic science, to the detailed application of scientific results to market-determined needs, are increasingly concentrated in the hands of corporate, banking, and military leaders, assisted by those in other sectors, such as universities, whose political allegiance lies with this class in practice, if not in principle" [p.5]. In a detailed and carefully documented argument, he shows that except for a modicum of democratic input during phase II (c.1966-1976), planning for U.S. science has been and is based almost exclusively on military needs and corporate profit. This is true of U.S. science policy both in phase I (the post-World War II period until about 1966) and phase III (the late 70s till now), with some qualitatively new features in phase III (see below).

The question is not, says Dickson, whether basic science should be left alone, to go in whatever direction the scientists' fancy dictates; it is, rather who should control it, because it always has been and always will be subject to controls. Should it be responsible to the private sector (mainly big corpo - rations), or should it heed the voice of the popular organizations.

In an early chapter, Dickson examines how under Carter, and even more so under Reagan (phase III), science policy has become characterized by: 1) an explicit recognition of the dominant role of the private sector in determining the agenda for public funding of science; 2) the application of public-funded research is left to the private sector and the profit motive; 3) a major change of attitude in the scientific community is encouraged and expected, namely, that the notion of scientists as independent scholars unconcerned about practical use of their results is replaced by the view that the overriding concern must be the enhancement of the competitive position (read "profitability") of U.S. industry and the push for military strength.

Elsewhere, Dickson explores the new features of the university-industry relationship. He points out that, although corporate funding of university research is hardly new, what is new is the intensity of the corporate interest in university research and in particular the new stress being placed on the importance of basic research to industry. For hi-tech industries like micro - electronics and biotechnology the traditional distinction between basic and applied research tends to get blurred. The result is that universities' basic research efforts are increasingly molded to meet corporate needs. Dickson goes on to delineate a number of harmful effects resulting from this qualitative and quantitative change in university-industry relationships. These include pressures on the universities to emphasize the areas of corporate interest at the expense of the humanities, a deemphasis on research areas reflecting social concerns (urban studies, black studies, environmental studies, alternative energy sources, etc.), the misuse of public-supported institutions to benefit certain corporations, dangers to the academic tradition of free and open communication of research results and techniques, and a number of others.

In his chapter on "Science and the Military", Dickson describes very well how the structure of U.S. scientific research and its goals have been put to the service of the Pentagon in the last several years to an extent that is unprecedented in peacetime, with a a growing net of restrictions on the freedom of scientific exchange imposed in the process. Today the U.S.

government spends twice as much on military research and development (R&D) as it does on R&D for all other social goals put together. Along with this expansion of military R&D has come an increase in Pentagon's control over the direction and freedom of U.S. basic research

Between 1980 and 1983, DOD funding of basic research (mainly in the universities) increased by nearly 30% in real terms, while support for basic research from all other federal agencies increased by only 4.5%. Taking into account also the fact that the biggest increases in funding by the other agencies have been in the mathematical, physical science, and engineering fields particularly important for strengthening military technology, the directions of this trend become quite clear. Dickson discusses in some detail the ominous effects of this militarization of "basic" and applied research: the removal of science from democratic control, the imposition of restrictions on the freedom of scientists (including the tradition of free and open communication of results), etc. The brief period in the late 60s and early 70s, during which partial successes were achieved in subjecting science to some degree of social control, has given way to the trend to increased military control over basic and applied science, supposedly in the interests of "national security." Witness the National Academy of Sciences' "Corson Report" which acknowledges the Pentagon's right to restrict the dissemination of basic research results when it deems appropriate. And, despite a number of protest actions, universities have lately agreed to accept this unprecedented degree of control over basic research.

Since Dickson's book appeared, a 1985 Harvard report *[Chronicle of Higher Education*, January 9, 1985] has revealed a rapid erosion of our traditional academic freedoms as military and corporate influence envelop American university life. Furthermore, this increasing military control over R&D is not of "academic" interest only, as Dickson makes clear; it is an important factor in fueling the arms race.

That military concerns have an important influence over the conduct of research is hardly news, of course; this has a tradition going back to the earliest origins of science itself. And in the U.S., as Dickson points out, military support of science has a tradition going back to the War Department's sponsorship of the Lewis and Clark expedition in 1804. The history of the top institutes of technology, such as MIT and Cal Tech, going back to World War II, shows that they were built largely with DOD funds. But, since World War II and especially since the late 70s, the military-science relationship has intensified to an unprecedented degree, and promises to accelerate if the arms race is escalated to outer space.

Dickson devotes a chapter to discussing the role that science plays as a weapon of U.S. foreign policy and the consequences thereof on the other capitalist countries, the socialist countries, and especially the less developed countries (LDCs). In Dickson's view, the central importance of science in the global economy has become a main concern of U.S. foreign policy in its efforts to secure a global enviornment conducive to the steady expansion of the U.S. economy. This has meant increased use of the U.S. hegemony in science to undermine the ability of other nations, developed or not, socialist or capitalist, to compete economically on equal terms. This, he says, is accomplished by draining key scientific and financial resources (the present influx of European venture capital attracted by high U.S. interest rates is an example of the latter, the "brain drain" of the former), and by limiting the

Galileo is startled by the change in the earth's surface
Honoré Daumier 1867. Lithograph.
From *Art Against War* by Bruckner, Chwast, Heller. Abbeville Press, 1984.

opportunity for independent maneuver (e.g., using hi-tech export controls to bend other countries to the U.S. political will).

But the heaviest impact of U.S. hi-tech policy, says Dickson, is on the LDCs. The imperialist nations, driving for maximum profit and minimum competition, have forced Asian, African, and Latin American countries into excessive dependence on foreign supplies. This is reflected in the fact that the LDCs together, with three-fourths of the world's population, spend less than one twentieth of the global R&D budget (and even the small amount of research done in the Third World is frequently on projects whose nature is foreign controlled, unrelated to pressing social needs of the local country).

The greater part of Dickson's chapter on the use of science as an instrument of foreign policy is given over to the history of the U.S. actions in this area since World War II, starting with the story of how the nuclear arms race was made inevitable when the U.S. presented conditions to the UN Atomic Energy Commission that were impossible to use as a basis for a system of international control of atomic energy, and ending with discussions of the Reagan administrations' refusal to sign the 1982 Law of the Sea treaty, or to support plans for an International Center for Genetic Engineering and Biotechnology approved in principle under UN auspices in 1983.

Starting with the publication in 1962 of Rachel Carsons' *The Silent Spring*, and coinciding (not accidentally) with the Black Liberation struggles and antiwar movements of the 60s and early 70s), there was a period of widespread demands for greater social control of science, as a result of the recognition that science had been misused to the point that it posed grave dangers to the survival of humanity. This movement eventually led to the passage of new laws (such as the National Environmental Protection Act of 1969) and setting up of new institutions (such as the Occupational Safety and Health Administration (OSHA)) for greater social control over application of science and technology. This phase of science policy, Dickson suggests, was characterized by the "democratic approach" to technological decision-making. Under Carter, and especially Reagan, this approach, according to Dickson has been supplanted by the "technocratic approach" (decision making by an elite of "experts"). The control of the NSF by a committee of scientists, the National Science Board, exemplifies the latter approach. Most of the rest of the book discusses various techniques used to limit the impact of demands for greater democratic controls over science and its applications, despite an apparent opening-up of decision making. Among these techniques was the technology assessment movement, leading to establishment by Congress of the Office of Technology Assessment (OTA) in 1972. According to Dickson, the OTA has tended to reinforce the technocratic over the democratic style of decision making.

Another example of a technocratic technique used to head off demands for greater democracy is provided by the National Institutes of Health which, through development of the recombinant DNA guidelines and the activities of its Recombinant DNA Advisory Committee, have managed so far to ensure that research using recombinant DNA techniques has not caused the damage to humans or the environment that was initially feared, and in the process has maintained firm control over the decision-making process.

A tendency of increasing significance in recent years is the attachment of the label "scientific" to regulatory activities affecting new technologies. Thus, requirements are made, supposedly on objective scientific grounds, for

"proof" and "certainty" of damage to the environment (e.g., by acid rain) before regulatory action can be taken. It is by surrounding such decisions (which are really political) in what Edmund Husserl described as a "cocoon of objectivity" that science is used as legitimation.

Dickson's final (and shortest) chapter, entitled "Toward a Democratic Strategy for Science", can hardly be taken seriously as an agenda outlining directions for social change. It amounts to little more than a pious plea for more democratic input into science policy decision-making. It mentions various groups in society which have, at various times and in various ways, addressed the issues of democratic controls over science policy. Mentioned are Ralph Nader's Health Research Group, various labor unions, and local groups such as the California Agrarian Action Project and the New Mexico Solar Energy Association, Environmentalists for Full Employment, and the Campaign for Economic Democracy. But there is no mention of electoral action, the peace movement, movements of oppressed minorities, women's movement, etc. And perhaps the biggest omission of all is the role of the socialist countries, which today account for over 40% of world industrial production and, in the USSR alone, over one fourth of the total number of scientists in the world (yet having less than 6% of the world's population). Contrasting the U.S. and the USSR with regard to democratic participation in the formulation of national science policy would have added greatly to Dickson's study.

Perhaps the strongest feature of Dickson's book is its thoroughly documented proof of the unprecedented degree to which the "military-industrial complex" has taken control of science in the U.S. and, in the process, is steadily restricting traditional freedoms, curtailing the development of science in the LDCs, etc.

Dickson thoroughly demolishes the notion that science is "objective" and above politics, showing that the question is not *whether* science is controlled, but rather *by whom* is it controlled, and in whose interests. Furthermore, he shows the extent to which distinctions between "pure" (or "basic") research and "applied" research have become blurred; the STR has reached the point where a basic scientific discovery becomes applied to technology and production in an incredibly short period, whether for military or peaceful use. Thus the increased funding by the Pentagon of university research, for example, is not limited to classified research involving applications of fundamental principles, but includes what is normally called "basic" research.

Several weaknesses in the Dickson analysis must, however, be noted. Although Dickson seems to be clear enough on the predatory, capitalistic nature of the U.S. economy, his is a static, non-Marxist view. The profound crisis of world capitalism and the growing ascendency of world socialism have escaped him and, as a result, he tends constantly to overrate the strength of U.S. capitalism. This leads to serious errors of fact, such as downplaying the decline of U.S. preeminence in science, and ignoring the crisis of U.S. education. He is also completely off base in suggesting that efforts to improve U.S. science teaching is motivated by the desire to reestablish the authority of science and thu limit the disruptive effect of public criticism.

Dickson does correctly note certain factors which operate temporarily to keep the U.S. in the forefront of world science such as the "brain drain" (luring of top scientific talent from Europe and hundreds of thousands of the best students of the Third World), and the attraction of European venture capital, lured by high U.S. interest rates, for financing of R&D. These factors, however, cannot be counted on to operate forever.

Dickson's ideas on strategies for democratizing science policy are weakened by his persistent failure to recognize the role of mass movements as agents of social change. This gives the whole chapter a pessimistic cast. The peace movement, including the unprecedented refusal since mid-1985 of many leading scientists to work on the Strategic Defense Initiative ("Star Wars"), is one example of an important development with great potential for effecting a democratization of science policy. Furthermore, it is amazing that Dickson neglects to include Afro-Americans among the various groups affected adversely by present U.S. science policies. No analysis can be adequate that neglects the pervasive effects of racism in every aspect of U.S. ruling-class policy.

The negative qualities in Dickson's work, however, should be viewed as minor flaws in what is, on the whole, a valuable contribution to the struggle to make science into a force for improving the condition of man rather than destroying him.

John Pappademos
Physics Department
University of Illinois at Chicago

6,500 scientists say no to Star Wars

Scientists and engineers Monday delivered to Congress pledges signed by 6,500 of their colleagues vowing to boycott Star Wars research.

Rep. George E. Brown Jr. (D-Calif.), accepting the pledges at a Capitol Hill news conference, declared that "at a time when the Reagan administration has been seeking to purchase legitimacy for the SDI (Strategic Defense Initiative), this boycott announces that the majority of the nation's top academic scientists are not for sale."

University of Illinois physicist John Kogut, organizer of the boycott, said the pledge has been signed by 3,700 science and engineering professors and senior researchers, including 15 Nobel laureates.

Signers comprise 57 percent of the combined faculties of the top 20 physics departments in the country and 2,800 graduate students and junior researchers. Heading the list are Harvard, Cornell, Caltech, Princeton, MIT, University of California at Berkeley, and Stanford. To make the pledge meaningful, only researchers likely to be offered a part of the estimated trillion dollars in Star Wars money were solicited -- physicists, engineers, computer scientists, chemists, astronomers, and mathematicians.

The pledge condemns Star Wars as "ill-conceived and dangerous." A shield to defend the U.S. population, it warns, "is not technically feasible," while a limited system "will only serve to escalate the nuclear arms race by encouraging development of both additonal offensive overkill and an all-out competition in anti-ballistic missile weapons.

"The program will jeopardize existing arms control agreements and make arms control negotiations even more difficult than at present. The program is a step toward the type of weapons and strategy likely to trigger a nuclear holocaust."

-- Tim Wheeler, *Daily World* 5-15-86.

Marx and Mathematics in Mozambique

Paulus Gerdes, *Marx Demystifies Calculus*. Translated by Beatrice Lumpkin. Minneapolis: Marxist Educational Press, 1985. xiv+129 pp.

The birth of capitalism, at the end of the Middle Ages in Europe, gave rise to many new mathematical problems associated with further development of navigation (and consequently of astronomy), and with new developments of technology and mechanics. The uniqueness of these problems consisted primarily in that they required mathematical study of problems involving motion. From a theoretical point of view, it was time for mathematicians to resolve the age old "paradox" of Zeno: Can motion of a body be realized by following a sequence of the positions of this body at rest?

In response to the new requirement, the mathematical constructs of variable and function and the calculus of Newton and Leibniz were developed. Though the techniques of differentiation and integration were found useful in solving new and old problems involving motion, there was considerable debate among mathematicians on the theoretical foundations of the calculus. The main issues debated were the precise nature of differentials and the proper means of arriving at the derivative of a function. This dispute lasted well over 200 years after the invention of the calculus. In the late nineteenth century, while exiled in London, Marx studied the calculus and decided to enter into the debate.

Engels [1885] was the first to mention the existence of mathematical manuscripts left by Marx and that Marx had not only studied the calculus but also developed some original ideas concerning its foundations. Dirk J. Struik [1948] was the first to write in English about Marx's theoretical ideas on the calculus. Paulus Gerdes [1983], on the centennial of Marx's death, was the first to offer a popular account of portions of Marx's *Mathematical Manuscripts* and their significance. Thanks to Beatrice Lumpkin's lucid translation from the Portuguese, we now have an English edition of Gerdes' *Marx Demystifies Calculus.*

The book under review is an extended version of a paper presented by Gerdes in March, 1983 at Eduardo Mondlane University in Maputo, capital of Mozambique, at a seminar on the current significance of Marx's works. Gerdes states that his purpose is to give "an introduction to the contents, methods and significance of Marx's *Mathematical Manuscripts*" [p xi]. He actually focuses on Marx's philosophical ideas concerning the differential calculus. Gerdes describes how Marx came to study the calculus, which texts Marx studied, Marx's critique, and his contributions to the debate on the foundations of the calculus. Gerdes also attempts to demonstrate how a knowledge of Marx's *Mathematical Manuscripts* or, more specifically, of Marx's dialectical method, can contribute to development of new methods of learning and teaching mathematics.

In 1858, to deepen his analysis of political economy and the elaboration of its principles, Marx began a study of algebra and moved from there to a study of analytic geometry and then differential calculus. Much of Marx's mathematical investigation took place during times of illness or recreation. Among his manuscripts are two research papers, one on the concept of the differential and the other on the concept of the derivative.

The textbooks in which Marx studied the calculus were written under the direct influence of the great mathematicians of the seventeenth and eighteenth centuries, specifically Newton, Leibniz, Euler, d'Alembert and Lagrange. (Marx was not in touch with professional mathematicians and was unaware of Cauchy's work on the calculus and limits.) As Gerdes points out, the textbooks that Marx read gave contradictory and confusing treatments of the subject. Marx classified these treatments into three groups; the mystic, the rational, and the algebraic. They were associated with Newton-Leibniz, Euler-d'Alembert, and Lagrange, respectively.

His analysis led Marx to two principal questions [p 20]:

1. *Is the derivative based on the differential or vice versa?*

2. *Does the differential remain a small constant, or does it tend to zero, or is it equal to zero?*

Gerdes states that Marx criticized the then-used methods of deriving the derivative of a function because none took account of the dialectical nature of motion and change to which a function is subjected in the process of differentiation. The fundamental flaw that Marx found in these methods was that the derivative of a function was always present before the actual differentiation occurred. In Marx's opinion the derivative had to be developed, not merely separated algebraically from the function. For each method the justification was that it produced the correct results, not that the means of arriving at these results were mathematically sound. In the "mystic" method (Newton and Leibniz), the differential is an infinitesimal quantity which is "juggled away" or "forcibly suppressed" at an arbitrary moment to reveal the derivative. In connection with this, Marx objected to the use of algebraic techniques without any justification for handling these non-Archimedean numbers. (This criticism by Marx was not answered until twenty-five years ago with the development of nonstandard analysis by Abraham Robinson. [See interview with Martin Davis, this issue.])

Marx considered the Euler-d'Alembert method, which he termed "rational," to represent an important contribution because in it infinitesimal quantities, dx and dy, were replaced by finite increments, Δx and Δy. As Gerdes quotes Marx, this removed "the veil of mysticism from differential calculus" [p 37]. The rational method, Marx opined, was justified in the use of algebraic techniques to operate on Δx's, since they were ordinary numbers. Aside from the fundamental flaw mentioned earlier, there were two other criticisms Marx had of this method. The first was that the quotient of differentials, dy/dx, appeared suddenly without justification at the end of the process of differentiation . Second, Marx criticized Euler's conception of the differential as being equal to zero, holding in suspicion the vague geometric interpretation Euler gave to these zeros. To overcome the obvious problem of division by zero in the quotient 0/0, Euler developed a special set of rules for calculating with "zeros in the geometric sense" [p 41]. Marx, however, considered the numerator and denominator of a differential quotient, dy/dx, to be "inseparably bound together" [quoted, p 42].

In the "algebraic" method (Lagrange), differentials and their quotients were avoided. Instead, since the derivative of a function was present in its power series expansion as the coefficient of Δx, the derivative was defined as such. Marx [quoted, p 46] commented that this method "...freed itself from anything resembling metaphysical transcendence." Lagrange believed that $f(x + \Delta x)$ could be expanded in a power series and, therefore, the derivative could be distinguished. However, Lagrange did not prove that all functions

could be represented as a power series nor that the possible infinite sum really existed. For this lack of rigor, Marx criticized the method.

Engels wrote that Marx allowed the variable of a function to "really vary" while others always represented this variation as "a sum of two quantities, but never a variation of a quantity" [quoted, p 48]. We will follow a slightly modified example that Gerdes provides to illustrate Marx's method and his use of the dialectical notion of "the negation of the negation."

$y = f(x) = x^3$

x varies from x_0 to x_1 (the first negation)

and y varies accordingly from y_0 to y_1.

$\Delta y/\Delta x = (y_1-y_0)/(x_1-x_0) = (x_1^3-x_0^3)/(x_1-x_0)$

$= (x_1-x_0)(x_1^2+x_1x_0+x_0^2)/(x_1-x_0)$

$= x_1^2+x_1x_0+x_0^2$ (provisional derivative).

x returns from x_1 to x_0,

$x_1 = x_0$ (negation of the first negation).

$x_1^2+x_1x_0+x_0^2 = x_0^2+x_0x_0+x_0^2 = 3x_0^2 = 3x^2$ (definitive derivative).

Note that, in contrast to the other methods, the derivative is developed here without having to discard unwanted terms. Gerdes, in describing Marx's method, uses the term "provisional derivative" for denoting the expression Marx obtained immediately before differentiating the function. The term is a substitute for Marx's "preliminary derivative" to represent the actual development which takes place in the process of differentiation, as the translator states [p.x]. In Marx's method, the independent variable varies away from its starting point. That is, x becomes distinct from x_0, and this transformation represents the first negation. Afterward, the difference quotient in Δy and Δx is taken, and the provisional derivative results. Following this, x returns from x_1 to x_0 and, in so doing, negates the distinction between x_1 and x_0. The outcome of this dialectical process of "the negation of the negation" yields the development of a qualitatively new function, the derivative of the function in question. Marx called this sublated form of the initial function the "definitive derivative." It is "the provisional derivative reduced to its absolute minimum value" [p 49]. The differential is equal to zero precisely at the moment that the independent variable returns from $x_1$to x_0.

That was Marx's answer to the second question that he posed on the foundations of the calculus. Gerdes returns us to Marx's first question. Marx rejected both Leibniz's and Euler's view of the differential. For him, the quotient of the differentials, dy/dx, was neither a ratio of "infinitely small quantities" nor a "geometric ratio of two zeros." In Marx's method, when x returns from x_1 to x_0, $x_1 = x_0$, and $y_1 = y_0$. So,

$$\Delta y/\Delta x = (y_1-y_0)/(x_1-x_0) \Rightarrow (y_0-y_0)/(x_0-x_0) = 0/0 .$$

The ratio, 0/0, loses its arithmetic meaning since it appears at the moment when the independent variable returns to its original position and a new function results, the definitive derivative. That is to say, the moment when the derivative is determined, a qualitative leap occurs from algebra to the calculus. Marx thus defined 0/0 as the "symbolic equivalent" of dy/dx [p 57]. The ratio 0/0 replaced by dy/dx yields

$$dy/dx = 3x_0^2 = 3x^2 .$$

Here, I believe, Gerdes could have been clearer by indicating that Marx posits the differential as an operator, a sign for taking the derivative. For Marx, the quotient of differentials denoted a process which is to be performed or which has just ended. This point is somehow lost in the discussion of the term "symbolic equivalent" [p 57], though Gerdes makes it later [p 75]. It also seems that Marx viewed the calculus as an "algebra" consisting of numbers and differential signs (analogous to the fact that nonzero rational numbers are operators, as in one-eighth of 16, and together with ordinary multiplication form an algebraic group.) As Gerdes points out, Marx uses differentials to derive the formula of the derivative of the product of two functions u and z in x [p 59].

In discussing the significance of Marx's *Mathematical Manuscripts*, Gerdes places it and the general development of the notions of function, continuity and differential and integral calculus in historical context. He argues that Marx's discoveries were independent, in some instances represented rediscoveries, and in other instances they anticipated conceptual and philosophical developments which were to occur later. Gerdes also discusses the class of functions to which Marx's method applies.

Note also that Marx's method of differentiation is essentially different from Cauchy's Δ-method as stated in modern textbooks. Unlike Cauchy's Δx tending to zero, or x_1 approaching x_0 infinitely close, for Marx $x_1 - x_0 = 0$ when $x_1 = x_0$. Gerdes points out that Marx's algorithmic method differs from the Cauchy-Weierstrass limit concept which only provides "a pragmatic criterion to *verify* whether or not a given value is really a limit . . ." [p 78].

In what way does Marx's method shed light on the philosophical paradox of Zeno to which I referred at the beginning of this review? A focus of Marx's investigation was on the exact moment when the calculus arises out of the underpinnings of algebra. For Marx, the genesis of the derivative had to be in the movement of the independent variable. He implicitly rejected the notion of Newton and Leibniz that the derivative could be computed under the assumption of the infinite divisibility of space and time. Similarly, Marx rejected the "rational" method's confusion of the nature of Δx with Euler's non-intuitive notion of "zeros in the geometric sense." Lagrange, who avoided the issue, did not demonstrate the derivative as a development of the motion of a point. In Marx's conception of differentiation, when x returns to its original position $\Delta x=0$ objectively and, using the language of Cauchy, the limit dy/dx is attained [p 82]. Thus, for Marx, motion could not be captured algebraically; it manifested itself in the transition to the calculus. Marx pinpointed this transition through his application of the notion of "the negation of the negation." Motion results when a body is in a place and is not in it. This is what Marx expressed in his conception that x moves away from x_0 and returns to x_0 Motion is thus realized as a dialectical transformation of a function resulting in its "definitive derivative." In this sense, Marx attempted to infuse the theory of the calculus with the very idea which was the motive force behind its invention, namely motion.

In Gerdes' penultimate chapter, he states that "[a]pplication of dialectics improves the quality of teaching..." [p 89]. His concern is how students can learn to think dialectically in mathematics. Those of us who teach and are interested in progressive social change share in this preoccupation. Gerdes proceeds to give parts of a kind of Socratic dialogue in which he engages his students in an attempt to differentiate the square root function. After several exchanges among the students and between them and Gerdes, they "discover" an approach to the problem and successfully differentiate the function using Marx's method. In his discussion, Gerdes considers that the "discovery" came about when the students decided to use, apparently unknown to them, the negation of the negation. Not having been present for that lesson, I wonder if Gerdes considers this the moment when the "discovery" occurred because the negation of the negation was used. Pinpointing when the creative moments take place in the classroom seem to me to be a complex and uncertain research project. Moreover, I wonder what sort of image Gerdes' students had of the square root function and whether they conceptualized the derivative as a tangent or as some other representation. Neither of these questions are answered in Gerdes' dialogue.

Gerdes then goes on to provide four examples of the use of negation of the negation in solving problems in elementary algebra, geometry, trigo - nometry, and in the calculation of limits. These examples are illustrative of dialectics in mathematics but not of how to build a mathematics curriculum to teach students to think dialectically.

The concern that Gerdes has about the learning and teaching of mathe - matics is a serious one. We know from his other writings [Gerdes, 1985, 1981a, 1981b, 1980] that he has much to offer on the subject owing to his praxis in Mozambique. Regrettably, his insights are not manifest in the final chapters of this book.

This popular account of how *Marx Demystifies Calculus* is a worthwhile book, filling a void of available and accessible interpetations of Marx's *Mathematical Manuscripts*. Gerdes brings together a large number of references from four languages. Parts of the book could be used as material in a calculus course. Perhaps this could lead to more discussions on applications of Marxism to the teaching and learning of mathematics in general and to the development of new paradigms of instruction.

Arthur B. Powell Jr.
Rutgers University

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Particle physics and philosophical idealism

Andrew Pickering, *Constructing Quarks: A Sociological History of Particle Physics.* University of Chicago Press 1984, xii + 468 pp. $30.

J.E. Dodd, *The Ideas of Particle Physics: An Introduction for Scientists.* Cambridge University Press 1984, x + 202 pp. $44.50 (paper $11.95).

EACH of these authors has worked in particle physics and each book in its own way really constitutes a condemnation of what is happening in particle physics. Pickering is clearly conscious of this, Dodd is not.

Dodd gives a straightforward presentation of the standard model: "We believe that we have a very firm idea of the basic material particles from which all observable matter is made" [vii]. Pickering, however, is much concerned with the ambiguities of that knowledge and highly critical of a naive realism using retrospective "appeal to the reality of theoretical constructs to legitimate scientific judgments when one has already decided which constructs are real" [7].

Dodd's history of particle physics is like a series of tableaux, very weak on the transitions from one stage to another in the development of today's concepts. Pickering's history is more process. His method of historical criticism emerges clearly in this description of "how consensus on the standard model was achieved":

> By summer 1979 all the anomalous storm clouds which threatened the standard electroweak model had been dispelled to the satisfaction of the high energy physics community (if not, perhaps, to the authors of the anomalous data). The mutants had been slain. Weinberg, Salam and Glashow shared the 1979 Nobel for their part in the achievement of a "new orthodoxy."
>
> In 1977 many physicists had been prepared to accept the null-results of the anomalous Washington and Oxford experiments and to construct new electroweak models to explain them but, in the wake of the 1979 SLAC experiment E122, the Washington-Oxford results came to be regarded as unreliable even though there had been no change in their intrinsic status (no data had been withdrawn; no fatal flaws in the experimental practice of either group had been proposed). What had changed was the context within which the data were assessed. Particle physicists *chose* to accept the results of the SLAC experiment, *chose* to interpret them in terms of the standard model (rather than some alternative which might reconcile them with the other results), and therefore *chose* to regard the Washington-Oxford experiments as somehow defective in performance or interpretation.
>
> Thus the development of electroweak physics in the late 1970s diverged markedly from the adversarial image of theory and experiment implicit in the "scientist's account." [Abridged excerpt, 300f.]

And thereby the particle physics community stands accused of gross idealism. According to Pickering, very little if any of the particle theory developed in the last 25 years need be accepted as reality. He sees it instead as mathematical dogma presented to the public as a grand unification of all the basic forces except gravity, another "congenial" compromise within the high energy physics community based on an amalgam of two mathematically unrelated models. Dodd, of course, accepts the whole kit and kaboodle.

Each of these books aims to help other scientists grasp the essentials of particle physics. Pickering goes into greater depth over a longer period of particle history and demands a great deal from the reader, with sections that assume some prior knowledge of gauge theory. It's worth the effort because of the drama in Pickering's argumentative discussions of the theoretical cross currents that have buffeted high energy physics in recent decades.

Dodd's book is not a bad reference work for the particle physicist but it is not suitable for other scientists because there are too many terms used without sufficient explanation. It is also marred by minor errors: p. 31, par. 2, it was Schwinger, not Dyson, who with Feynman and Tomonga showed how to remove the infinities; p. 35, col. 2, the necessity for gravitons to have spin 2 is because of tensor theory in general relativity, not because of the attractive force; p. 43, col. 1, parity is preserved in transition between states for photon and electron together, but not for individual electron states; p. 78, col. 2, the example of stellar evolution is incorrect because a heavy star in the transition to burning helium goes from blue giant on main sequence to a red super giant. Since neither book deals substantially with the mathematics of group theory, most readers even with some training in physics will probably still feel left on the outside, but looking in with more comprehension than before.

Finally, I believe that Pickering makes an excellent case for his thesis. Though today particle physicists are still in a state of euphoria over the supposed discovery of the W± and Z° intermediate bosons, there is also a pervasive feeling that the whole theoretical structure is quite shaky and may collapse any day from some new anomalous observation that cannot be fitted into the standard model, or from the failure to observe some phenomenon predicted by the model. Group theory has come to dominate the thinking of particle physicists to such an extent that they have lost touch with reality; there is nothing palpable to grasp because a group can tell them nothing about particle dynamics, only about symmetry relations. The whole field may be seen as a new kind of scholasticism concerned with how many gauge fields can dance inside a baryon.

Lloyd Motz
Astronomy (Emeritus)
Columbia University

ADDENDA. 1) Another critique of particle physics may be found in Shrader-Frechette [1977] who argued that the discipline was in a state of Kuhnian crisis presaging a move to the stage of extraordinary science and the emergence of a new paradigm. Hendrick and Murphy [1981], finding "danger" in this argument, sought to demonstrate that the standard model is alive and well, and that, since no alternative model has yet won the day, we have no way of knowing whether current particle physics is in a state of crisis.

2) Some interesting comments on the contradictory state of the scientific method today can be found in the exchange of letters by Sullivan [1984] and DeWitt [1984] occasioned by the DeWitt [1983] article on quantum gravity in *Scientific American.*

3) We need a good bibliography on the inner contradictions of high energy physics today. Contributions and suggestions from readers are always welcomed. *Editor.*

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On the Origins of Human Nature

Lewis Henry Morgan, *Ancient Society* (facsimile edition). Tucson: University of Arizona Press 1985. 560 pp, index. Paper $14.95.

IN SCIENCE, as in politics, some major problems of our time revolve around the question: What is human nature? This question is at the heart of the stormy controversies over sociobiology, over IQ and "hereditary" intelligence, over "creationism" versus evolution, and so forth. The strife over these questions reflects the underlying fact that our society is wracked by antagonistic relations along sex, race, class, nationality, religion, and other divisions.

Are such antagonisms inevitable and inherent in "human nature"? Some would answer yes to this question. Marxist historical materialism provides a firm theoretical basis for answering no! Marxist theory helps reveal the historical origins of the antagonisms, providing a truly comprehensive and many-sided understanding, not only of the possibility, but also of the historical necessity of overcoming such antagonisms.

Historical materialism deals with the forces of social change that have carried mankind from its pre-antagonistic beginnings in primitive communal life to our present predicament, and that will carry us forward to a new level of communal life. A giant step in the elaboration of historical materialism was the book, *The Origin of the Family, Private Property and the State,* written by Frederick Engels after the death of Karl Marx. This classic Marxist work was based to a great extent on the empirical researches and materialist theorizing of an upstate New York lawyer whose deep interest in the life of the Iroquois led to his adoption into that Indian nation.

This review will deal mainly with Morgan's contribution to the underpinnings of historical materialism as developed by Marx and Engels. It is hoped that readers will be thereby inspired to turn (perhaps anew) to the works by Morgan and Engels that, for many, have opened new vistas of Marxist understanding.

Lewis Henry Morgan was born in Aurora, New York, in 1818, the same year as Karl Marx. He graduated from nearby Union College in 1840, practiced law in Rochester for a time, and served several terms in the New York state legislature in the 1860s. But law and politics were not enough for Morgan. His five major anthropological and ethological studies established him as a scientist of the first rank, not only in America but around the world.

In 1851 he published *League of the Ho-dé-no-sau-nee,* or Iroquois (Rochester: Sage), an enthnographic account of Iroquois culture which remains important to this day. Morgan followed this in 1868 with *The American Beaver and His Works* (Philadelphia: Lippincott), which continued the ethological interest in the beaver manifested by such writers as James Burnet a century earlier. In 1871 the Smithsonian Institution published Morgan's *Systems of Consanguinity and Affiliation* , with an ethnography of kinship terms derived from questionnaires sent around the world and from Morgan's own field trips. The evolutionary themes of that book were elaborated in *Ancient Society*, published in 1877 by Henry Holt in New York (the reprint under review is "a direct photographic reproduction of the corrected 1878 edition"). A planned fifth part of *Ancient Society*, which became too lengthy for inclusion in this volume, was published by the Smithsonian in 1881 (the year of Morgan's death) as *Houses and House-Life* *of the American Aborigines*. It constitutes the foundation of what later would be called proxemics, the scientific study of the relationship between physical space and social structure.

Morgan received widespread scientific recognition even in his lifetime and within the United States. He was elected to the National Academy of Science and was president of the American Association for the Advancement of Science for the year 1880. Morgan's significance, however, cannot be assessed simply in terms of the scientific honors he received or the fate of his works in subsequent bourgeois scholarship. Even the crudest of vulgarizers have found it necessary to acknowledge the significance of *Ancient Society* for the development of historical materialism, and thereby for the scientific conception of the world. Let us focus on that contribution.

Marx and Engels had provided a preliminary characterization of the primordial condition of humanity in the *German Ideology*. A brief look at that manuscript will disclose the richness as well as some important lacunae in the content of their 1840s anthropology. They pointed out that the mode of production manifests the "mode of life" of humanity [1]. This "mode" comprises on the one side social relations, i.e., the cooperative production of means for the satisfaction of needs, and on the other side species-being, i.e., the reproduction of humankind as species. Thus the "mode of life" incorporates two aspects: subsistence of the individual and the continuation of the collectivity [2]. The trajectory of social development is schematized as a series of four stages corresponding to the social division of labor on the one hand, and to the forms of property on the other: the earliest stage was that of the patriarchal tribe; the next was that of the ancient city-state; the third was the feudal stage; and last was the bourgeois stage [3]. It seems that there are two demands placed on a dialectical theory of society. One is to disclose the internal relations of poiesis and praxis (production and its relations) which make up the "mode of life" and the social form itself. The other is to reveal the relations between the social form and its environment, both the natural environment which bounds it in space, and the system of other social forms which precede and follow it in historical time. These two demands are interrelated, in the same way that "inner" and "outer" mutually condition one another. The division of labor, property, etc. revealed the internal relations of the several forms. The representation of the stages of social development likewise disclosed the external relations.

For present purposes, it suffices to note that, for Marx and Engels in the 1840s, the first stage encompasses the social forms preceding Hellenic society of the 6th century B.C.E., and supposes that the primordial state of humanity was antagonistic, insofar as youth and women were exploited by patriarchs. This is an undifferentiated conception of "primitive society."

This preliminary understanding represented, of course, an historical refinement of the "four stages" theory of societal development which had been sketched by the writers of the Scottish Enlightenment. As Meek has reminded us, this theory, promoted by Montesquieu, Smith, and other 18th century thinkers, was an anticipation of historical materialism [4]. But only an anticipation. For instance, in *The Wealth of Nations*, Adam Smith had distinguished four stages or "periods" in the development of property and of the means of subsistence, pointing out that "nations of hunters" made up "the lowest and rudest state of society." While we would question whether pre-antagonistic society was characterized by a "hunting" mode of subsistence [5], Smith was nonetheless correct in holding of each adult in that society that

"he maintains himself by his own labor." There was personal but not private property, with social equality, so that "in this state of things there is properly neither sovereign nor commonwealth." But this gives way, in Smith's account, to a "second period of society, that of shepherds, [which] admits of very great inequalities of fortune." While we would question whether the earliest antagonistic social order is characterized by a "nomadic mode of subsistence," Smith was correct in pointing out that this form generates hierarchy: "There is no period accordingly in which authority and subordination are more perfectly established" [6].

In the late 1850s Marx revised his and Engels' preliminary anthropological conceptions. He recognized the existence of an "Asiatic mode of production" and an associated "Oriental social formation" which preceded the ancient city-state [7]. Nonetheless, Marx and Engels' characterization of the primordial condition of humanity remained undifferentiated. But all this became much more concrete after Marx read *Ancient Society*, which was no later than early 1881 [8]. He abstracted extensively from Morgan's book [9]. Marx died March 14, 1883, before he was able to complete his study of Morgan. Engels used Marx's abstracts as well as Morgan's book in preparing the Origin (1884). Thus *Ancient Society* directly influenced the anthropological thought of Marx as well as that of Engels, and was indirectly influential on Engels' thought through Marx's notebooks as well.

In *Ancient Society*, Morgan had indicated the correlation between four characteristics of the pre-antagonistic social order, namely between the means of subsistence, the forms of government, the forms of the 'family', and the inheritance of property. These characteristics each constitute the topic for one of the four parts of Morgan's book. Two of these characteristics, forms of the 'family' and forms of government, disclosed the internal relations of the *jus gentilicum* which made up the several pre-antagonistic social forms. Three of these characteristics, forms of government excepted, provided a remarkable differentiation of the primordial society. Much attention has been paid to the topic of internal relations and the various social formations of "primitive society." Let us focus our attention on the relations which structure the system of pre-antagonistic social forms. Morgan called the earliest form of society Savagery; it was comprised of three successive periods of stata, "Lower," "Middle," and "Upper." His next form of society, Barbarism, was also comprised of three periods. (The "Upper Status of Barbarism" was followed by Civilization, identified by the appearance of writing.) We will consider Morgan's characterization of primordial society only at the level of the dichotomy between "Savagery" and "Barbarism."

"Savagery" and "Barbarism" were defined in terms of the characteristic subsistence patterns (first simple appropriation of the natural product, then humanity's active trans-formation of nature and the enhancement of natural productivity) as well as in terms of the characteristic mating patterns or "family" (first group mating, then pair mating) [10]. The first term in both characteristics thereby articulates with the data of primate ethology [11].

Marx, as well as Engels, appears to have concurred in this dichotomy of the pre-antagonistic social order. In his *Ethnological Notebooks* [pp 108f], Marx had followed Morgan in acknowledging that "the Horde would break up into smaller groups for subsistence; it would fall from promiscuity into consanguine families, the first 'organized form of society'" [12]. These forms of group mating, added Marx, were "widely prevailing in the Status of

Savagery" and, further along, that "the syndyasmian [or `pairing'] family became a constant phenomenon in the Lower Status [i.e., the first stage] of Barbarism" [p 126]. For completeness sake, it should be noted that Morgan states that "the classificatory [i.e., consanguine and pairing] and the descriptive [i.e., the monogamous forms of the "family"] yield nearly the exact line of demarcation between the barbarous and civilized nations" [13].

Marx not only followed Morgan in delineating the characteristic mating forms of the genus of "Savagery" and that of "Barbarism," but regarding the characteristic inheritance patterns as well. The "first great rule" of inheritance, which obtained during "savagery," was the distribution of the decedent's effects among the *Gentiles*, i.e., among members of the gens or clan [14]. The "second great rule of inheritance," which obtained during "Barbarism," was the distribution among *agnatic* kindred, i.e., through male descent alone, to the exclusion of the remaining gentiles [15]. The "third great rule of inheritance" gave "property to the children of the deceased [patriarchal] owner" [16]. (This rule obtained, of course, subsequent to the emergence of civilization, i.e., emergence of the antagonistic social order.)

The correlation of these characteristics of the pre-antagonistic social order, and its resultant differentiation, can be illustrated as follows:

	"Savagery	"Barbarism"
Mode of Subsistence	Food Gathering	Food Production
Mating Patterns	Group Mating	Pair Mating
"Rules" of Inheritance	Gentile	Agnatic

Morgan has thus revealed the unity of the primordial condition of humanity. But this is not an abstract, an undifferentiated unity. It is the unity of difference, whereby the complex actuality of the pre-antagonistic social order becomes the object of scientific inquiry at a theoretically appropriate level of discourse.

Morgan's writings have begun to receive renewed attention during the past decade, after a half-century of deliberate neglect if not vilification in the West. They have been assailed on "methodological grounds" by positivism, but at last their theoretical significance is being acknowledged. This reprint, then, is testimony to the renewed attention. Perhaps it can be the stimulus for still further attention.

Ancient Society should be read...and re-read. No less an authority than Engels himself put it this way in his 1891 preface to *Origin*.

> [Morgan's research] has the same importance for anthropology as Darwin's theory of evolution has for biology and Marx's theory of surplus value for political economy.

Gordon Welty
Wright State University
Dayton OH 45435

Notes and References

1. Marx-Engels, *Collected Works*. New York: International 1975, v, 31.
2. In these two aspects, Marx and Engels not only reflect their prior understanding, e.g., in the *Economic and Philosophical Manuscripts of 1844*, but also anticipate their later formulations, e.g., the 1884 preface to the *Origin of the Family, Private Property and the State* (New York: International 1972).

3. *Collected Works* v, 32-35 and 64-74.
4. See Ronald Meek, *Studies in the Labor Theory of Value* (London: Lawrence and Wishart 1972), piii.
5. Marx, Engels, and today's ethnography reject the suggestion that early humanity subsisted on the "fruits of the hunt": cf. Engels *Origin* p 88; Lawrence Krader, ed., *The Ethnological Notebooks* of Karl Marx (Assen: Van Gorcum 1972), p 99; Sally Slocum "Woman the Gatherer" in R.R. Reiter, ed., Toward an Anthropology of Women (NY: Monthly Review Press 1975), pp 36-50; Eleanor Leacock, Myths of Male Dominance (New York: Monthly Review Press 1981), Ch. 7, "Women's Status in Egalitarian Society"; Elizabeth Fee, "Woman's Role in Evolution" (review of Nancy Tanner's On Becoming Human), Science and Nature No. 5, 1982, pp 20-29; and Pat Shipman, "Scavenging or Hunting in Early Hominids." Amer. Anthropologist 88:27-43;1986.
6. Adam Smith, *The Wealth of Nations*, Book V, in Glasgow Edition of the *Works and Correspondence of Adam Smith* (Oxford: Clarendon Pr. 1976) ii, 689f and 712-713.
7. See the 1857-1858 "Formen" section of the Grundrisse and the 1859 preface to *Contribution to the Critique of Political Economy* [*Coll. Works* xvi, 465-477].
8. The date can be inferred from Marx's draft letter to Vera Zasulich where he alludes to Morgan (cf. Marx-Engels *Werke*, Berlin: Dietz Verlag 1982, Bd. 19, S. 386).
9. These abstracts now available in *Ethnological Notebooks*, op cit., pp 97-241.
10. Regarding subsistence, see Robert Braidwood and C.A. Reed, "The Achievement and Early Consequences of Food-Production" in *Cold Spring Harbor Symposia on Quantitative Biology* v 22 (1957) pp 21-22. Regarding group mating, see *Ancient Society* p 446: "The consanguine and punaluan families represent the substance of human progress through the greater part of the period savagery." Regarding pair mating, see *ibid.* p 460: "The great advancement of society indicated by the transtion from savagery into ... barbarism, would carry with it a corresponding improvement in the condition of the family." See also Engels *Origin* p 138, as well as Viktor G. Afanasyev *Marxist Philosophy* (Moscow: Progress 1980) pp 277-27.
11. On group mating among primates, see, for instance, Linda `Scott, "Reproductive Behavior of Adolescent Female Baboons in Kenya." in Meredith Small, ed., *Female Primates* (New York: Alan R. Liss Inc. 1984), p 96. On susbsistence patterns and tool use among primates, see Hilary Box, *Primate Behavior and Social Ecology* (London: Chapman and Hall 1984), p 146ff.
12. Morgan also referred to "the first organized form of society" (*Ancient Society* p 418).
13. Ancient Society p 397; cf. *Ethnological Notebooks* p 106, and George Peter Murdock *Social Structure* (New York: Macmillan 1949), p 100.
14. *Ancient Society* p 525; *Ethnological Notebooks* p 128.
15. *Ancient Society* p 531; *Ethnological Notebooks* p 130.
16. *Ancient Society* p 544; *Ethnological Notebooks* p 135.

The Politics of Sex in Medicine

Elizabeth Fee, ed., *Women and Health: The Politics of Sex in Medicine*. Farmingdale, NY: Baywood, 1983.

This collection of 11 articles, which previously appeared in the *International Journal of Health Services*, is the best available book on the subject of women and health. Several of the studies contribute substantially to developing a Marxist critique and revolutionary practice in health care. Other articles are grounded in liberal bourgeois or radical feminist perspectives, reflecting the diversity of views within the women's movement. The excellent preface and introductory article by Elizabeth Fee provide an overview of the various currents represented in the book.

Health care facilitates capital accumulation in three ways. First, many illnesses which sap the productivity of labor can now be cured or ameliorated. Second, medicine is an important ideological prop for the ruling class in the maintenance of the domestic tranquility and social stability needed for production and profit. Since Bismarck's introduction of health insurance for workers in 1883, health care has been used by the ruling class to cushion some of the most savage aspects of industrialization and forestall more radical working class demands. Finally, the medical care industry has itself become an important field for investment and profit-making.

During the past fifty years the medical industry has enormously expanded its ability to fulfill these tasks. The real, if exaggerated, technical achievements of twentieth century medicine have provided the basis for the progressive socialization and commodification of functions previously carried out by women within the household. The reproduction of labor power, including birth, child raising and protection, care for the sick, and comfort for the dying was historically the province of women. The capitalist integration of this previously non-commodity sphere has involved profound changes in the role of women in health care and in bourgeois society more generally. Forced from their previous role as unalienated (though oppressed) use value producers whose commodity relations were largely mediated through husbands and fathers, 20th century women have fully entered commodity production both as wage laborers and consumers.

Until the turn of the century hospitals were largely reserved for the dying poor. The development of a few effective therapies which required hospital facilities and organized nursing services made hospitalization attractive for the non-destitute. Gradually, small nursing homes and hospitals arose which assumed much of the care of the sick and elderly, thus removing a major barrier to women's participation in the work force. As in many of the other industries which arose from the progressive socialization of household work (*e.g.*, food service, childcare and education) medical care was controlled by men who supervised a largely female labor force. For most of this century health care production consisted mainly of nursing and housekeeping and was widely scattered in small hospitals and offices. Within the workplace racial and ethnic stratification further divided the workforce, with underpaid white nurses perceiving their status as fundamentally dissimilar from that of even worse paid minority nursing aides and housekeepers. The charity orientation of hospitals and the immediacy of patient contact fostered a service ethic among hospital employees which was encouraged by the paternalistic supervisory practices of physicians and administrators. For all these reasons, health workers' unions were virtually non-existent until the mid 1950's, and wages were substantially below those of workers in other sectors.

Over the past 30 years medical interventions and care have become increasingly capital intensive and technical in nature. The small doctor's office and community hospital have given way to the medical center or hospital corporation controlling thousands of beds. Increasingly, hospital work involves manipulation of a machine or piece of paper rather than human contact with a patient. In addition, the civil rights and women's movements have broken down the legitimacy of a paternalism already compromised by the open pursuit of profits. As a result, the health care work force more and more assumes the subjective and objective characteristics of an industrial proletariat. An understanding of sexism is especially important here since

hospital employees are 85% women (many of them minorities), and 15% of all U.S. women wage earners work in the health care sector.

Two chapters in Fee's book address the issues of women in the health work force. Carol Brown's *Women Workers in the Health Service Industry* is (along with a chapter in Vicente Navarro's *Medicine under Capitalism*) the best in description and analysis of these issues produced by the left. A superb combination of statistical compendium and sociological insight into sex roles in hospitals, this essay should be required reading for all concerned with health care. Weaver and Garrett give a carefully documented and detailed description of the racial and gender stratification of health workers, which provides an important foundation for further analytic work.

Women are disproportionately represented in health care not only as workers but also as "consumers" (though women live longer than men, an issue seldom addressed by left analysts). Women are hospitalized in acute care settings 40% more frequently than men (and in nursing homes twice as often), make approximately 2/3 of all adult visits to physicians, and usually accompany children during pediatric visits. Most of the women's health movement activism and analysis has focused on women as patients. This emphasis, as Fee points out in her lucid introduction, has produced some valuable insights but also reflects the middle class predominance in the women's movement and the failure of Marxists to articulate an analysis of medicine as industrial production. These failings aside, the women`s movement has become the most effective force in demystifying medicine, challenging the dominance of physicians and medical ideology, and asserting the rights of people to control their own bodies. It has fostered a continued active questioning of obstetrical and gynecologic technology. Examples of such questioning included in the book are a study of the frequency of induction of labor and an article on vaginal cancer due to DES. Both essays effectively demonstrate how the ceding of control of medicine to the (male) medical profession has resulted in the medical abuse of women. However, they remain at the level of description and fail to situate the problems in an understanding of the capitalist imperatives which produce the destructive social relations of medical care. We need deeper such critiques of gynecologic technology, and similar analyses in other areas of medical technology (many of which profoundly affect women), to lay bare the undercurrent of profitability, descrimination, and repressive social control.

Two chapters deal more explicitly with medicine's role in reproducing bourgeois ideology. Linda Gordon reviews the professionalization of the birth control movement between 1920 and 1940, and its evolution from a popular demand of the left and feminists to instrument for social control. Start, Flitcraft and Frazier describe how the medical profession has at once medicalized and denied the problem of wife battering and family violence. They illustrate eloquently the ideological distortion of medical services and the resulting inability to deal with problems which do not fit into the individualistic and mechanistic models guiding bourgeois medical practice. While they describe this enforcement mechanism of patriarchy in great detail, class analysis is largely absent. Both essays are useful case studies providing insights into the medical mechanisms of social control.

Finally, two essays deal with the social construction of medical knowledge and ideology. Karl Figlio details the history of Chloriosis, a common 19th century "disease" (mainly diagnosed in young women) which has since disappeared. Figlio eloquently shows how this diagnosis was constructed to deflect "criticism from concrete working conditions onto abstract generalities and personal responsibility for illness," and was rapidly abandoned as it lost this ideological usefulness. Richard Lewontin's critique of the biocentric view of health, longevity and social inequality, "Sociobiology: another biologic determinism," is a superb refutation of the view that inequalities between the sexes (and races) are due to genetic differences. He shows the poverty of the scientific basis for such views, and lays bare the real ideological foundations.

Women and Health is an excellent book, reflecting both the strengths and weaknesses of the work of the left in this field. Though several of the chapters deal with rather narrow and specialized topics, and would benefit from a broader analytic perspective, the editor's introduction and preface provide the needed context and overview. Description of medical ideology, paternalism, and social control is effectively presented, while political-economic understanding and analysis of the interactions of class, race, and gender remain underdeveloped. If you are going to read one book about women and health, this is the one.

Steffie Woolhandler MD
Public Health, Boston University
David U Himmelstein MD
Harvard Medical School

Toward Women's Liberation

Ruth Bleier, *Science and Gender: A Critique of Biology and Its Theories on Women*. Pergamon Press, 1984. 220 pp.

Modern science seeks knowledge about nature mainly for use in the form of technology; the actual uses to which it is put are determined by those who hold power in society. Furthermore, science as we know it is practiced mostly by economically privileged, university educated, white males who are either themselves members of the ruling class or share its interests. None of this is news to progressives -- except that the equation of specifically *male* power with science often is omitted from Marxist analyses and the equation of *ruling class* male power from feminist analyses.

Bleier tries to avoid these pitfalls but, when writing about science, abstracted and reified, she sometimes ignores what an intellectually and socially circumscribed and limited group of people scientists are, and how little their interests reflect those of most men as well as women. And this creates problems, since this book is not about science in general but about the ways in which scientists have made the gender roles and expectations of our particular society appear as though they were "natural" outcomes of inherent biological differences between women and men.

Bleier's is one of a number of books by feminists that describe the social construction of gender and its scientific rationalizations, but whereas most earlier such books -- including those I have helped edit -- have been collections of articles by several authors [1] and have therefore inevitably had gaps and inconsistencies, she has put together a coherent monograph. The

book is well-written and accessible to nonspecialists, yet sophisticated in its analysis. It is full of information, clearly presented, and is useful for teaching chapter by chapter as well as rewarding to read in its entirety.

In the Introduction, Bleier discusses the context-dependence of science and sketches the history of biodeterminist explanations of sex and race differences since the mid-nineteenth century while stressing the political functions of racist and sexist science [2]. Next is a chapter exposing flaws in biodeterminist explanations of human behavior, especially sociobiology, and two chapters criticizing theories that draw on the brain, hormones, or genes as "causes" of differences in women's and men's roles. Then come two chapters discussing sex roles and divisions of labor in different societies and current disputes among feminists as well as anti-feminists about the existence of egalitarian societies as opposed to a universal subordination of women [3]. The next to last chapter, "Sexuality, Ideology, and Patriarchy," is an attack on the presumed "naturalness" of heterosexuality and on the toll homophobia and sexual violence take of women. In the final chapter Bleier describes her visions for a science that is more aware of context and respectful of complexity, process, and change, less ready to focus narrowly and dichotomize, a science practiced by people who try to understand and acknowledge biases imposed by culture, class, race, and gender.

Because it is an ambitious book that criticizes what has been and projects what should be, this review will concentrate on a few issues where I think Bleier does not go far enough. My most general criticism is that the book is unclear about the ways nature, science and technology are related. Bleier sometimes seems to accept the conventional view of science as distinct from technology [4] . Though she criticizes definitions of science as "objective, transcendent, neutral, and value-free," she characterizes its "ideas and theories" as "efforts to describe and explain the natural world; that is, reality." And, though she grants that scientists filter that reality through a socially conditioned consciousness, I miss an on-going insistence that science is a very specific kind of knowledge about nature. Rita Arditti, feminist and scientist, urges that we stop talking about "science" and always refer to "science-and-technology" or "science/technology", awkward as that may be. Science is an effort to learn about nature and "reality" not just in order to understand them better, but to use that understanding to solve practical problems. Therefore, the question is always: who is learning and understanding, in whose interest and for what purposes.

Joseph Needham, in his essay "Human Law and the Laws of Nature" (1951), explored (as he has done so often) the question why modern science developed in Europe although the Chinese at that time had a much more highly developed technology and understanding of natural processes than had Europeans [5]. He suggests that one precondition was the Judaeo-Christian assumption of a lawfulness inherent in nature because God put it there. This points to the curious relationship between nature and modern science and technology in which scientists are said to "discover" (uncover?) natural laws that are then put to use. It also points to an odd relationship between science and gender, because during the time in which science began to codify laws of nature, our present notions about gender also became codified in laws of science as well as laws of society. And such was European thinking about "laws" of nature that it was possible in Switzerland, as late as 1730, for a rooster to be tried, convicted, and sentenced to die by burning for the crime of laying an egg! This not only shows the extent to which our tradition merged natural law with human law, but also exemplifies the absurd lengths to which it could go to legislate and enforce gender differences.

Compare this, for example, with the acceptance of cross-gender dressing and other social activities, including marrying people of one's own sex and rearing children together, by several groups of Western and Plains Indians until the early 1800s -- before systematic white contact and religious proselytizing [6]. To these people gender roles were entirely separate from biological sex, a very different perception from that of biodeterminists who ignore the variety of ways in which women and men function in different societies in order to prove that stereotyped norms of behavior are inevitable consequences of biological sex differences.

This brings me to another question I have about Bleier's book, this one about her critique of biodeterminism. She criticizes biodeterminists for trying to derive social structures from biology, pointing out that this is not only politically and socially oppressive, but scientifically wrong -- bad science. But I miss an analysis of why this is so. The omission leads her to conclude, I believe falsely, that the claims of sociobiologists to establish the biological roots of social behavior are problematic only with reference to people, so that the same reductionist analysis can be used successfully to understand animal behavior. I find the critique by Lewontin, Rose, and Kamin more consistent and convincing [7]. These authors argue that biodeterminism and reductionism are flawed in principle because they ignore the differences in complexity at different levels of organization and the changes in quality as well as quantity that accompany transitions between levels in either direction. (The levels of organization in this instance extend from molecules to cells, organs, organisms, and their groupings into societies.) Hence, the properties found at one level cannot be derived from those observable at other levels. One cannot explain the behavior of people or animals by studying their hormones or genes, their brains or their nervous systems; similarly, one cannot predict how hormones or genes will act within cells, tissues, or organisms by knowing their molecular properties.

I think that we need some such analysis to explain *why* biodeterminist reasoning cannot come up with right answers. Otherwise, we are stuck with the assertion that it is wrong because it goes against common sense, daily experience, and/or political conviction. (This is not to sneer at these criteria of acceptability, but why not include analytical arguments as well?) Bleier's failure to see the problem in terms of differences in levels of complexity also leads her to personalize the brain and cerebral cortex. They are said to "produce" behavior, and to "construct" or "transmit" "the body of ideas and values that constitutes our culture." But only people do those things, not organs (not even brains!).

One of the things I like is that Bleier openly expresses her opinions and does not hide behind pseudo-objective statements and descriptions. This is especially true in the last chapter where she juxtaposes "patriarchal science" with "feminist visions" and poses the question thus:

> Having defined the problem as nothing less than an enveloping patriarchal consciousness and a more than 4000-year-old patriarchal civilization that has ordered social behaviors, forms of social organization, and systems of thought, including science, how can we view the possibilities and directions for change?

Stressing the cultural origins of scientific descriptions, she points out:

> Hierarchies, relations of domination, subordination, power, and control are not necessarily [why 'necessarily'?] in nature but are part of the conceptual framework of persons bred in a civilization constructed on principles of stratification, domination, subordination, power and control, all made to appear natural.

Therefore, change is possible. However, these two statements, with which I agree, are followed by generalizations about a reified, male science which specifies "who does science," defines the doer in stereotypically masculine terms, and denies "that science is or even can be objective, transcendent, neutral, and value-free." I strongly disagree with this reification of science. Science specifies, defines and claims nothing; scientists do. And scientists do not encompass all men, nor do they all represent "the male mind," whatever that is. They are a very particular, tiny minority among men and a much tinier minority among women. They share less a "male mind" than the interests of the ruling class and a Euro-American, upper class, white consciousness.

Feminist critics of science must not only have the immodest ambition to "transform ideological bases for our Western civilization and for women's place in it;" we must make the production of knowledge, including scientific knowledge, a more accessible enterprise. An elite science will never provide tools for a life that is respectful of the needs of nature and of people. So, although I agree with much that Bleier says, I want in addition an explicit commitment that feminist scientists must work together with other community activists to generate the questions and develop the political and scientific strategies that can get at the answers we need in order to steer "Western civilization" off its deadly course. This will require the participation of many different kinds of people -- women and men -- who have been systematically excluded from producing science and technology, and whose needs the science and technology we now have cannot serve.

Ruth Hubbard
Cellular and Developmental Biology
Harvard University

Notes and references

1. For example, see Ruth Hubbard and Marian Lowe (eds.), *Genes and Gender II: Pitfalls in Research on Sex and Gender*. NY: Gordian Press (1979); Ruth Hubbard, Mary Sue Henifin, and Barbara Fried (eds.), *Biological Woman* -- The Convenient Myth. Cambridge, MA: Schenkman Publishing Co. (1982); Marian Lowe and Ruth Hubbard (eds.), *Woman's Nature: Rationalizations of Inequality*. NY: Pergamon 1983.
2. An interesting collection of turn-of-the-century writings on sex differences has just appeared in the same series as Bleier's book. See Louise Michele Newman (ed.), *Men's Ideas/Women's Realities: Popular Science, 1870-1915*. NY: Pergamon 1985.
3. A contribution to this discussion of particular interest to Marxists is the last essay (before her untimely death in June 1984) by anthropologist Lila Leibowitz, "Origins of the Sexual Division of Labor" [in Marian Lowe and Ruth Hubbard (eds.), *Woman's Nature: Rationalizations of Inequality*. New York: Pergamon (1983), pp. 123-147].
4. On the history and inappropriateness of this distinction, see the excellent discussion by Steven Rose, The Limits to Science." *Science for the People* 16 (6): 16-21; Nov 1984.
5. Reprinted in Joseph Needham, *The Grand Titration: Science and Society in East and West*. London: Allen and Unwin 1969 pp 299-331.
6. See Evelyn Blackwood, "Sexuality and Gender in Certain Native American Tribes: The Case of Cross-Gender Females." *Signs* 10: 27-42; 1984.
7. R.C. Lewontin, Steven Rose, and Leon J. Kamin, *Not in Our Genes: Biology, Ideology, and Human Nature*. New York: Pantheon 1984.

Human Behavior: Dialectical-Materialist Approaches

Steven Rose, ed., *Against Biological Determinism.* London: Allison & Busby, New York: Schocken, 1982. 184 pp.

Steven Rose, ed., *Towards a Liberatory Biology.* London: Allison & Busby, New York: Schocken, 1982, 161 pp.

Political Affairs Conference. "Psychology and Changing Human Nature -- A Marxist Approach." *Political Affairs,* Vol. 60, No. 11.

Although science poses as being objective, without bias, and able to approach problems dispassionately, science develops in a social milieu and basically is supported and promoted because it can serve the economical and ideological interests of the economic system wherein it exists. In addition, women and men who work as scientists have developed in a social background; they are products of their environment. Each of us inevitably comes to science with political, economic and social biases that reflect our position in society and our acceptance or rejection of the mores of the status quo (as a rule we remain unaware of our own prejudices). It is true, of course, that (as reflections of the reality around us) facts are facts, providing they have been properly ascertained: under certain conditions of pressure, temperature, etc. pure water is a liquid; atoms combine by sharing electrons to form molecules; and all eukaryotic cell nuclei contain desoxyribose nucleic acid (DNA). Whatever the economic and political orientation of a scientist, these must be accepted as facts. It is in the interpretation of facts, and in the extrapolation and generalization beyond known facts that the biases come into play.

In the history of science, the biases have been and remain most striking when biologists speak of human evolution and abilities, especially when they extrapolate from what they observe regarding animal behavior to interpret human behavior or the human condition.

As a case in point, the appearance in recent years of E.O. Wilson's *Sociobiology* [1] and Dawkin's *The Selfish Gene* [2] have engendered heated controversy because of the blatant reductionist views projected. Although, as pointed out by Masters [3], all of those who follow Wilson and his reductionism are not necessarily conscious racists, elitists or male chauvinists, nevertheless those who do subscribe to such anti-social views have rushed to use Wilson's conclusions in support of their own ideologies. As one might expect, there also is an "unconscious" permeation of such views among those we might classify as being on the "liberal" side of racism (showing patronizing attitudes towards "undeveloped" peoples), of elitism (we must try to help the "lower classes") and of male chauvinism (we like "girls"; it's nice to have good-looking secretaries and technicians).

That sociobiology and similar ideas are useful in support of the status quo is shown by their enthusiastic propagation in the mass media. The Establishment appreciates every opportunity to bolster beliefs in the inferiority of working people and in the unchangeability of human nature. Also it welcomes the fostering of divisive ideas that pit "inferior" minorities against WASPS, men against women, etc. But for every action there is a reaction (in this case perhaps it is better to say: for every reaction there is an action!). Anti-capitalists, and especially dialectical materialists are interested in upsetting the status quo and in changing human nature to develop a dominance of cooperative socialist attitudes to replace the aggressive competitiveness of capitalist ideology. Inevitably, sociobiology has aroused counteraction and widespread discussion.

Two discussion meetings have considered these matters, and it is of interest to compare and contrast them. The first was held in the spring of 1980 at the University of Padua in Bressanone, Italy. It was initiated by a manifesto, originating from some biological workers in England, which pointed to the reactionary reductionism of sociobiology in interpretations of human behavior, and regretted that "ranged against reductionism appear to be mainly the forces of a reactionary and incoherent idealism..." It was proposed to explore the dialectical-materialist approach to these important questions. From this there arose a sponsoring Bialectics of Biology Group, which arranged the Bressanone conference, entitled "The Dialectics of Biology and Society in the Production of the Mind." About 50 persons attended the conference, mostly from England and Italy, all working in professional institutions of advanced capitalist societies. A wide spectrum of professions was represented, from linguistics to molecular biology. Of the papers presented, 21 have been published in two small paperback volumes edited by Steven Rose: *Against Biological Determinism* and *Towards a Liberatory Biology.*

The second conference, held October 1982 in New York City with the title "Psychology and Changing Human Nature: A Marxist Approach," was organized by the Marxist journal *Political Affairs*, where the proceedings have now appeared. The format of this conference included three main papers, each followed by two critical discussion papers and general discussion. Most of the speakers and discussants were professional persons in psychology and psychotherapy from academe, hospitals or private practice. All discussants and two of the main speakers were from the United States, while the third major speaker was the director of the Institute of Psychology of the U.S.S.R. Academy of Sciences in Moscow.

At Bressanone the formal presentations were less rigorously organized than in New York, as if the detailed subjects under consideration depended solely on the personal interests of each individual participant. The talks, however, were planned to fall into place within six half-day topics, listed in the introduction to both volumes as: 1) Know your enemy; reductionism as meta-theory; 2) Systems, machines and dialectics; 2) Evolution, organism and environment; 4) Neurobiological explanations and human action; 5) The material basis of consciousness; and 6) Where does our behavior come from? The two volumes are respectively devoted to "...the philosophical, political and ideological challenge to biological reductionism, and its transcendence by way of systems or dialectically based theories" (*Against Biological Determinism*) and "with exploring the building of a new biology...based on non-reductionist premises" (*Towards a Liberatory Biology*).

The New York meeting on the other hand, being a one-day affair, dealt with only three topics: 1) A Marxist approach to the human mind; 2) Current psychological practice in the United States; and 3) A discussion of Soviet psychology in the building of socialism -- each topic represented by a single speaker and two discussants.

The basic aim of both conferences was similar: to explore dialectical relationships between the genetic makeup of individuals and the shaping of their minds by their varying socializing experiences.

Many of the Bressanone papers are of great interest and importance. Lesley Rogers, discussing ideology in medicine, is clear on the weaknesses of reductionist thinking. Martin Barker, discussing the reactionary uses of reductionism (*e.g.*, in interpretations of the "innate inferiority" of women, of blacks, of the poor, etc.) shows that we "cannot be satisfied with an answer solely in terms of misuses of otherwise innocent biological investigations." The paper by Hilary and Steven Rose analyzes bourgeois criticisms of reductionism and dissects weaknesses in some of the more dogmatic critiques from the left; then attempts a non-dogmatic Marxist dialectical interpretation that does not deny the value of reductionism, but rather rejects reactionary bourgeois utilization of the method. In summary they say:

> A new science as it develops will not destroy the genuine insights of reductionism, nor its power as a methodological tool...nor will it endeavor to cut humanity adrift from our biological natures. The strengths of reductionism will be incorporated into a post-reductionist science, its limitations and errors transcended...[k]nowing as variables what bourgeois science sees as constants, emphasizing the historicity of objects and the reality of discontinuities, is to work [for]...a transformative theory of the natural world.

Ruth Hubbard ("The Theory and Practice of Genetic Reductionism...") examines concepts of heredity by "genes". She emphasizes that DNA is not isolated in the cell or the organism, but that its synthesis (replication) anew requires the mediation of enzymes (i.e., proteins), that the total DNA is not involved in the production of RNA, and that the "message" of the RNA itself may be modified before the syntheses of amino acid sequences. Hence the reductionist genetic dogma that "genes" (*i.e.*, base sequences in the DNA molecule) control the production of specific amino acid sequences in the synthesis of protein molecules seems to be on shaky ground. A similar approach is taken in a paper by Mae-wan Ho and Peter Saunders ("Adaptation and Natural Selection..."). These authors emphasize that the Weismannian concept of the isolation and independence of the germ plasm (DNA in modern terminology) is untenable since the DNA interpenetrates with its surrounding internal environment, and this in turn is influenced both directly and indirectly by the external environment. They conclude that, considering what Lamarck actually said (in contrast to what he is said to have said!), and considering the state of biological and biochemical knowledge when Lamarck wrote during the early years of the 19th Century, a dialectical neo-Lamarckian interpretation of heredity and nature cannot *a priori* be excluded.

At the New York meeting, Irving Crain discussed "A Marxist Approach to the Human Mind." The Marxist literature on the subject is largely from Soviet sources, particularly the seminal works of Vygotsky, Luria and Leontiev, and Crain's paper includes a survey of their works. Vygotsky, for example, concluded from his studies that the development of the mind of a child was not merely the passive absorption of patters of behavior exhibited by its parents other human associates. Instead the development was the result of a dynamic interchange between the child and people in its environment, with an important aspect being the child's social (*i.e.*, class) position in society. Luria studied the peoples of Uzbekistan and Kirghizia in the early '30's. He found that hard-working peasants who had not yet been much influenced by the new socialist regime, in contrast to local people who had been so influenced, and those who were deeply involved in the socialist reorganization of the region, actually *thought* differently. Even colors and optical illusions were perceived differently. After reviewing some idealist conclusions of various western psychologists, Crain concludes that "a Marxist psychology places human activity at the center of all development -- social and individual. Instincts, genes, biology, temperament...are transformed by socially and historically determined human activity, continuously creating qualitatively new needs, new motives, new goals."

A second paper given in New York was by Alan Schreiber on "Current U.S. Psychological Practice." The prevailing view of U.S. psychologists and psychotherapists is that human nature is fixed and immutable, based on our animal ancestry. He contrasts this with the Marxist view: "Psychologists have interpreted human nature in various ways: our goal is to change it." Stress is placed on the value to the capitalist Establishment of the prevailing U.S. psychological theories in controlling and manipulating the general population politically and economically (as in commercial advertising).

Boris Lomov ("The Role of Soviet Psychology in Building Socialism") began by reviewing and contrasting pre-revolution (Russian) and post-revolution (Soviet) psychology. The continuing role of Marxist philosophy in the development of Soviet psychology is documented. Persons are biological creatures, but also they are social beings. A true picture of human mental phenomena must take both of these into consideration. A conception of human behavior centered strictly on biology is inadequate. It is necessary also to take account of societal influences on behavior and mind. Schreiber's statement that "our goal is to change human nature" is carried a step farther: "if we would like to change human nature we must change social relations."

In his paper at Bressanone, Giacomo Gava ("Hierarchical Structures and Structural Descriptions") points out that there are various levels of language, of which ordinary language is the oldest, most basic, and most easily understood. Philosophic language (as other levels) grew out of ordinary language. There are psychological languages, neurophysiological languages, etc. These "higher" language levels usually are sufficiently complex and specialized that they can be difficult to understand for those familiar only with other levels. To a lay person using ordinary language, they become more or less incomprehensible .

Unfortunately, at Bressanone the participants aimed their discussions primarily towards each other as fellow intellectuals and professionals. The published proceedings will be very esoteric for the general reader, and therefore will be read mainly by a limited academic audience. All the speakers undoubtedly considered themselves to be dialectical materialists -- some of them clearly Marxist. This reviewer, however, failed to detect any real class approach to the subjects under consideration. Nowhere is there any clear mention of the effect on the human mind and on human attitudes and behavior of the class position of individuals. There was no obvious mention anywhere of the labor movement or of class consciousness, despite the fact that Marxists -- both the founders and the practiioners -- in their social criticism of capitalism and of its superstructure emphasize the central role of the working class in the changes necessary to develop a socialist system and a socialist personality.

In contrast, the New York conference, without probing less discerningly into the problems of the development of the mind, was aimed at a much broader audience, and its proceedings will be read with understanding and sympathy not only in academic and professional circles, but also in workers' educational groups and in the homes of non-professionals. The class nature of the development of the mind is clearly spelled out: "This means that if we would like to understand a personality we must study the position and role in the system of social relations of the individual" (Lomov, on the role of Soviet psychology).

At both conferences the correct view was expressed (and demonstrated during the talks) that dialectical materialism is a philosophical method of thinking and not a cut-and-dried dogma that inevitably leads anyone who uses the method to absolute truth, down to the last dot on the last i. Differences among the participants surfaced both at Bressanone and New York. At Bressanone, for example, some speakers (*e.g.*, Steven Rose in *Towards a Liberatory Biology*) seemed to feel that reductionism in general, if initially it was progressive in liberating thought from feudal restrictions, now has mainly become a fetter in bourgeois science, while others (*e.g.*, Scazzochio) concluded that reductionism is both inevitable and proper in scientific and dialectical thinking, and is incorrect only when carried to extremes (as when the effect of testosterone on the behavior of roosters in the barnyard is cited as justification for male chauvinism in humans). Similarly at New York, Schreiber, whose main paper was a critical review of current non-dialectical practices in U.S. psychotherapy, was taken to task by respondent Lefkowitz for equating social consciousness with individual consciousness.

It is unfortunate that the language and lack of sharp orientation of many of the participants at Bressanone will make their proceedings virtually inaccessible to many potential readers. Nevertheless, there is much to challenge our thinking and our orientations in the proceedings of both conferences, and they bear reading and re-reading.

Charles C. Davis
Department of Biology
Memorial University of Newfoundland
St. John's, Newfoundland, Canada A1B 3X9

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Marxist Dictionary of Philosophy in New Edition

Dictionary of Philosophy, Ivan T. Frolov, editor. Translation edited by Mura Saifulin and the late Richard R. Dixon, from 4th Russian edition (Moscow 1980). New York: International 1984. 464 pp. $8.95.

Near my left elbow as I write is a well-thumbed and much-marked *Dictionary of Philosophy* (1967 edition, edited by Rosenthal and Yudin). For many years I have depended on its terse relevance for Marxist insight on the concepts behind some word or phrase. Hence, the new Frolov edition was welcome indeed. Here is how the new edition stacks up against the old:

1) There is a marked improvement in the writing. A majority of the entries have been rewritten to achieve brevity and more graceful expression. There is also extensive updating to reflect developments in philosophy around the world: new entries and omissions as well as rewrites. The typography is easier to read despite the smaller type size. And the paper quality is better.

2) In relation to the particular philosophical problems of natural science, however, not all the changes are for the better. For one thing, there is an obvious de-emphasizing of natural science and the important problems of scientific cognition. For instance, the 1967 entry for *Science* dealt primarily with the nature of scientific knowledge and the role of philosophy therein, while the 1984 entry, somewhat longer, deals almost entirely with the social role of science, giving only the briefest mention of philsophical aspects. This tendency is also evident in the new entry for *Materialism, Natural-Historical*, which omits entirely the following pregnant comment from 1967 on the cognitive limits of scientific materialism:

> Its limitations become most apparent in periods when scientific theories are revolutionized. At such times it is unable to explain the new facts of knowledge that contradict established notions. For this reason the difficulties of interpreting new scientific facts often lead scientists to abandon their spontaneous materialist convictions in favour of idealism. True philosophical generalization of the conclusions arrived at by specialized sciences can be achieved only from the standpoint of dialectical-materialist philosophy.

Another criticism is that the new edition ignores some philosophical problems plaguing us today in the west; there are no entries for sociobiology, feminism or scientific realism. I was glad to find an entry on *Structuralism* that, while reflecting the Soviet preoccupation with the methodology of the systems approach, is properly critical of the Althusserian distortion:

> It is characteristic of structuralism to focus on describing the actual state of the objects under investigation, to reveal their intrinsic timeless properties, and to establish relations between facts or elements of the system under investigation. Departing from the set of facts observed initially, structuralism proceeds to reveal and describe the inner structure of the object.....However, the widespread introduction of structural methods in different spheres of knowledge has given rise to futile attempts to raise structuralism to the status of a philosophical system and, as such, to oppose it to other philosophical systems, particularly Marxism. These attempts, ignoring as they do the cognitive limits of structuralism as a concrete scientific method, are absolutely unwarranted and have been criticized by Soviet scholars and foreign Marxist philosophers. Marxist philosophy counterposes the methodological principles of dialectical analysis to [its] anti-historical approach to structure and [its] rejection of inner contradictions as the source of development and change of the object's structure.

I have also found two errors that warrant comment. For one, under *Theory and Practice* the new entry says:

> Marxist philosophy regards practice not as the sensuous subjective experience of the individual and not as an experiment of the scientist, etc., but as the activity of people to sustain the existence and development of society.....

It seems wrong to pose scientific experimentation thus, purely in terms of individual activity, since the requirement of reproducibility of experimental results is one of the important factors making science a social process (an "activity of people to sustain the existence and development of society"). Furthermore, the above formulation contradicts not only the 1967 entry but also the 1984 entry for *Experiment* which says that "Experiment is an aspect of man's social and historical practice."

Another instance of error is one which I believe originated with Engels himself. Under *Logic, Inductive* both editions say essentially the same thing:

In the history of logic there was also another conception of the subject matter of inductive logic, limiting its tasks to analyzing logical criteria for verifying scientific assertions within the framework of the hypothetico-deductive method. This conception was formulated in the 19th century by W. Whewell, a British logician, and has become widespread in the modern logic of science.

As a matter of historical fact, this was not the approach of William Whewell to inductive logic. [On the possible origin of Engels' mistaken interpretation, see Talkington, "Is the Creative Process Rational?", S&N this issue.]

Returning to the positive side, I present two new entries which seem very much on the button. One is the concept of *Reduction* which is discussed first in terms of methodology (data reduction, etc.) and then in terms of the philosophical problem of reductionism:

Making reduction absolute leads to reductionism, a concept that it is possible to completely reduce higher phenomena to lower, basic phenomena. Although higher forms of the development of matter arise from lower forms and retain them in a sublated form, they are not reducible to them. Reductionism may be seen in mechanism, the tendency to consider the psychic only as a result of physiological, etc., processes and to biologize the phenomena of social life. In neo-positivism reductionism is manifested in the tendency to "free philosophy from metaphysics" and to reduce scientific knowledge to propositions about sensations or to physical experiments and measurements.

One might add that the discovery of underlying principles of nature has no relation to reductionism in the philosophical sense. For example, molecular biology has provided the technology to solve many medical problems, but this in no way reduces biomedical science to the laws of molecules.

I would also score a big plus for the entry on *Scientism* which is treated as a concept that absolutizes the role of science. Then anti-scientism is discussed as absolutization at the opposite pole:

Modern bourgeois culture has produced various trends of anti-scientism, some of which claim that science's potential for solving the key problems of human existence is limited, while its extreme varieties assess science as a force hostile to the true essence of man. Consistent anti-scientism regards philosophy as something basically different from science, which, it holds, is purely utilitarian, and is incapable of rising to the understanding of the genuine problems inherent in the being of the world and man. While upholding the principles of a scientific approach to any ideological, philosophical, social or humanitarian problem, and rejecting the anti-scientist attempts at downgrading the role of science, Marxism-Leninism is equally opposed to vulgar scientism with its disregard for the place and function of science in the system of culture, and the interrelation between different forms of culture.

In conclusion, *The Dictionary of Philosophy*, in its Frolov edition, continues to be a useful reference tool despite some deficiencies on natural science. (Could these represent an over-reaction to the Lysenko business?) The philosophical content of most entries is not affected, as a reading of the entries for *Materialism* and *Dialectical Materialism* will convince you. But don't try to borrow my review copy; it now stands beside the old edition, both at my elbow in easy reach when I need to clarify my thinking on any sort of philosophical matter -- from *Absolute and Relative, the* -- all the way to *Zeno of Elea* (he of the paradoxes).

Hank Talkington
Science and Nature

Astronomy and Many-Sided Marxism

Dieter B. Herrmann, *The History of Astronomy from Herschel to Hertzsprung*. Translated and revised by Kevin Krisciunas. Cambridge University Press. 300 pp, $24.95.

THIS BOOK concerns the development of astronomy from 1780 to 1930, *i.e.,* the period of transition to modern astrophysics from classical astronomy which, to the end of the 19th century, was primarily concerned with positional and distance determinations. As such it fills an important gap in the history of astronomy. This particular book is of much greater significance, however, since it is written by a Marxist whose distinctive approach to science and its history is evident on every page. This is not a mere chronology of events or a series of disconnected accounts of the contributions by great men who lived during this historical period. Rather, astronomy is seen as developing in its complex interrelationships with society and its modes of production.

He goes beyond such well known relationships as the important role of astronomers in the determination of longitude and the consequences of this for exploration and colonization. An example is his discussion of the development of astronomical photometry:

Lighting technology also developed in the nineteenth century. This innovation was of the greatest significance for the development of capitalist production relationships, above all with the implementation of night work and the marked exploitation of manpower and the use of machines. Simultaneously, the production of all kinds of lamps (where success was achieved) was an extraordinarily profitable business which later grew to become a giant industry. By the first half of the nineteenth century London, Paris and Berlin were lit up by gas lamps. Because it had become practical to "make night into day," comparisons between the luminosities of the new artificial lamps and the natural light sources, especially the Sun, were an obvious result; here technical and astronomical photometry met. Numerous physicists and astronomers actively concerned themselves with the production of tools for the measurement of light, and many photometers from those years were discussed in textbooks on astronomical photometry as well as in standardized works of lighting technology.

In Herschel's day (1738-1822) the universe was largely regarded as immutable. The positivist Comte (1798-1857) had confidently declared that the chemical constitution of the sun must forever remain unknown to mankind and that concern with such questions was a waste of time. Classical astronomy rested firmly on the basis of Newtonian mechanics and its practitioners resisted application of the new physics in their domain.

The opening of the 20th century marked the founding of the new discipline of astrophysics by young physicists and chemists who lacked the benefit of a classical training in astronomy. They discovered the energy generation mechanism in stars and the Hertzsprung-Russell diagram which indicated clearly that we live in an evolving universe. Though Marx and Engels lived in the period just prior to the birth of astrophysics, Herrmann points out that

Marx and Engels attributed the greatest significance to the prevalence of evolutionary thought in the natural sciences. Engels especially occupied himself with the manifestation of evolution of heavenly bodies. He saw important elements for the dialectical-materialistic interpretation of nature in the suppositions and hypotheses which continually entrenched themselves in the course of research. The numerous related notes in his fragment *Dialectics of Nature* are impressive proof of this, as these solely scientific points of view found their place in the Marxist philosophical theory of evolution.

This is a book of high scholarship covering a fascinating period, recommended for anyone interested in astronomy or the history of science, and for assigned reading in college level courses.

Stanley Jeffers
Dept. of Physics
York University (Ontario)

Toward Overcoming Underdevelopment

QUIPU: Revista Latino Americano de Historia de Las Sciencias y la Tecnologia. Official journal of the Latin American Society for the History of Science and Technology.

QUIPU fills a gap in the literature of the field. So far no other periodical addresses the evolution of science and technology in the region. Already in its second year, QUIPU is a great success, with more than two thousand fragmentary edition of Georgius Agricola, now held in Peru. Enrique Beltran surveyed the history of science in Latin America; Jose Leite Lopes presented a personal view of the past fify years of Brazilian physics; Jose Lopez Sanchez described the first scientific relations between Cuba and Mexico; Elaine Pennini de De Vega addressed the reception of Darwinian ideas in Cuba; Luis Pyenson discussed the use of science as a means of imperialist penetration in Argentina; Dirk J. Struik compared the early colonial sciences of North America and Mexico; among others. [Quipu is the name for the type of knotted cord used as a mnemonic device bu the ancient Peruvians to record history and so forth.]

It is easy to perceive why one should request libraries and individuals to subscribe to QUIPU. For one thing, the history of science is especially important in Marxist philosophical studies of science. There is also another reason. We should all do our best to make sure that an intellectual enterprise of such importance will not fold becaue of the economic conditons plaguing Latin America. QUIPU deserves our support. The one-year subscription for individuals is $15, for institutions $30 (two years, $25 and $55). Write QUIPU, Aptdo. Postal 21-873, 04000 Mexico D.F. MEXICO.

Regis Cabral
Department of History
University of Chicago.

Constructionism and Reductionism in Biomedicine

William Coleman, "The Cognitive Basis of the Discipline: Claude Bernard on Physiology." *ISIS* 76: 49-70; 1985.

The object of physiological inquiry, announced the pioneering Claude Bernard (1813-1878), is not the nature of life but rather the experimental "determination" of vital phenomena. Though such phenomena are notoriously complex and labile, the physiologist (like any other scientist) must assume the regularity of the laws of nature as a condition for scientific discourse. Thus, exemplary in his positivism, Bernard agreed with Kant on the need for a mechanistic but not reductionistic biology. As a "critical teleologist" he assumed the organism to be an integrated set of diverse organs acting in a harmonious way, so that the problem becomes one of how to gain access to the complex whole: "if we learn to recognize and control the physiochemical condition of a vital action, we shall [thereby] have learned to dominate a phenomenon of life. From such knowledge flows the capacity to call forth again this and other vital phenomena and to establish the relations between them, the latter being the supreme aim of physiology," i.e., both its theoretical and practical basis.

Bernard's programme thus started with the *reduction* of vital phenomena to their simplest components, but only as the necessary first step to *construction* of a better picture of life processes through scientific under-standing of *relationships* within the complex whole. I don't know of a better explication of the dialectical interplay between the two opposing aspects of scientific investigation: the *reduction* of a system to its elemental components as the basis for *construction* of a useful theory for the organic whole.

Historian William Coleman goes on to show us how Bernard carried out this programme with unswerving dedication. Up to his time, medicine had been primarily a contemplative science with clinician and pathological anatomist as passive observers, increasing the precision of their diagnostic abilities but, in effect, facing the vital phenomena without any effective means for intervening, for "dominating" the natural processes. Bernard also saw the biology of his time as a passive observational science, with at most the ability to predict an event; only experimental science, *active* science, could dominate nature. One of Bernard's ideological targets was the Liebig notion that the character of chemical transformations occurring within the living organism could be discovered by measuring the chemical input and output of the body. This approach was false, Bernard held, because it was seriously incomplete and it exaggerated the role of chemical analysis at the expense of physiological reality, for which the necessary framework is the living body.

Bernard did not reject the wares offered by academic physicians and the expanding chemical community. Instead, he appropriated them as needed, and added others, in creating the new science of experimental physiology. His trinity of methodological procedures embraced *vivisection*, in which anatomy and surgery joined to provide direct access to vital activities;

chemistry, now working closely with analysis of inner structure and function of the living body; and *histology*, promising to provide a new deeper level of insight beyond that of organs such as lung or liver. In all such studies, observation and description were merely a prelude to directed intervention in Bernard's call for an exhaustive scientific engagement with life itself.

I have emphasized here passages that help illuminate the problem of reductionism. But there is much more, and all worthwhile, in this insightful scholarly review of Claude Bernard's fruitful career.

ADDENDUM: Garland Allen points out that Bernard was not the first to emphasize the use of experimentation in physiology or medicine, but was a pioneer in emphasizing that a holistic physiology can also be experimental. In reacting against the extreme reductionism of the Berlin medical materialists, Bernard thus emphasized a holistic and organizational role in physiology that the former school had missed.

Fitting mathematics to an empiricist formula

Philip Kitcher, *The Nature of Mathematical Knowledge.* Oxford University Press 1983. x+287 pp, index. $25.

HIGHLY ORIGINAL is the theory of mathematical knowledge now proposed by Philip Kitcher. He tells us it breaks with tradition in three ways: 1) by rejecting mathematical apriorism in favor of mathematical empiricism, 2) by giving a central role to the community of mathematicians in the development of their knowledge, and 3) in stressing the importance of understanding the history of that development for grasping the meaning of mathematics and how it changes. Unfortunately, Kitcher's empiricist interpretation severely limits the value of his contributions concerning the social and historical aspects of mathematics.

For example, he says that "my theory of mathematical knowledge traces the knowledge of the contemporary individual through a chain of prior authorities, to perceptual knowledge acquired by our remote ancestors" [p7], by which he means (explicitly) that sensuous perception has played no role in the development of mathematics since, say, Babylonian times. So much for the perceptions of great mathematicians who have interacted with practical problems in, say, the development of calculus. In fact, Kitcher devotes the whole last chapter to a history of mathematical analysis which contains no hint that this development could have been influenced by its practical usefulness in the world outside of the mathematical community.

Kitcher does not see the development of mathematics as a dialectical process of interaction between theory and practice, with qualitative change growing out of this process. Instead, he proposes a gradualist model for change and a concept of mathematical practice as something quite formal and static, consisting of five components: a language, a set of accepted statements, a set of accepted reasonings, a set of questions selected as important, and a set of metamathematical views (standards of proof, etc.). His formulation represents a one-sided logical empiricist derivation from Thomas Kuhn's notions of practice, with Kitcher rejecting outright the Kuhnian concept of paradigm (in which ideological battles are implied):

> To suppose that the science of a time is to be regarded as multi-faceted is not to endorse the idea that the history of science must reveal discontinuities, or that changes in some components of the science are so fundamental that those changes should be hailed as revolutionary.....I wish to salvage the notion of a practice and jettison the concept of a paradigm [pp162f].

In Kitcher's world view, theory is a very shadowy thing. The term theory appears only once in the index, in the entry for his own gradualist "Evolutionary theory of knowledge". While his empiricism is on the materialist side ("mathematics is about structures present in physical reality"[p107]), it nevertheless shares with idealist empiricism (from Berkeley to Mach to Carnap and company) the overriding tendency to idealize the role of sensory experience, to underestimate the role of conceptual experience and scientific abstraction, and to ignore or blur over the theory-practice dialectic.

On the other hand, Kitcher's truly venturesome book has been properly hailed for its critical insights concerning the philosophy and history of mathematics, breathing new life into an area dominated by idealistic *apriorism*.. As Lorraine Daston says [*ISIS* 75: 717-21; 1984]:

> More dramatic is the possibility Kitcher offers of explaining why mathematics should happen to fit the world so neatly, the very existence of applied mathematics being a perpetual miracle to the Platonists. If mathematics derives ultimately from our experience, more specifically our experience of physical operations idealized, then we need not invoke prearranged harmony to map that idealization back onto the world.

A Marxist may agree with much of his penetrating critique, yet regret that the alternative offered would put mathematics into an empiricist straightjacket.

For a Marxist view covering many of the same questions, the reader is referred to A.D. Aleksandrov [S&N No.3 pp 22-42].

Madness in His Method?

Richard P. Feynman, *"Surely You're Joking, Mr. Feynman!" Adventures of a Curious Character* (as told to Ralph Leighton and edited by Edward Hutchings). New York, London: W.W. Norton 1985. 350 pp, index $16.95.

DON'T EXPECT to learn any physics here. This is the Nobel Laureate, taped over the years in a convivial story-telling mood, sharing his delights in bongo drums, the opposite sex, and humanity in general -- to the delight of scientists and non-scientists alike. Each story has its own unique twist, mostly quite humorous, often with the joke on Feynman himself. But sometimes the twist is that of a scalpel laying bare some aspect of our society's sickness. Caustic indeed is his account of trying to help a state curriculum committee choose the textbooks for public education: "Judging Books by Their Covers."

There is a brief glimpse of today's tragic dichotomy between science and philosophy of science in Feynman's remark on "the guys from the [philosophy] department being particularly inane" [p 232]. And in general this book seems barren as far as any formal philosophical outlook. Nevertheless, some of his anecdotes are pregnant with meaning for philosophy of science. For example, he deals several times with aspects of his own method, which I think of as *putting practice into theory*.

> They would tell me the general problem they were working on, and would begin to write a bunch of equations.
> "Wait a minute," I would say, "Is there a particular example of this general problem?"
> "Why, yes; of course."
> "Good, give me one example." That was for me; I can't understand anything in general unless I'm carrying along in my mind a specific example and watching it go. Some people think in the beginning that I'm kind of slow and I don't understand the problem, because I ask a lot of these "dumb" questions: "Is a cathode plus or minus? Is an anion this way, or that way?"
> But later, whem the guy's in the middle of a bunch of equations, he'd say something and I'll say, "Wait a minute!! There's an error! That can't be right!"
> The guy looks at his equations, and sure enough, after awhile, he finds the mistake and wonders, "How the hell did this guy, who hardly understood at the beginning, find that mistake in the mess of all these equations?"
> He thinks I'm following the steps mathematically, but that's not what I'm doing. I have the specific physical example of what he's trying to analyze, and I know from instinct and experience the properties of the thing. So when the equation says it should behave so-and-so, and I know that's the wrong way around, I jump up and say, "Wait! There's a mistake!" [pp 244f.]

Note that this method does not depend on practical experience alone but also on the proper use of that experience in the evaluation of theory, something sadly missing from many papers appearing in academic journals.

His most important statement he saves for the last: a plea for scientific integrity, defined as "a principle of scientific thought that corresponds to a kind of utter honesty -- a kind of leaning over backwards.....the idea is to try to give all of the information to help others to judge the value of your contribution; not just the information that leads in one particular direction or another." [p 341]

Feynman properly contrasts this approach to what happens in advertising and concludes that scientific integrity operates at another level. He believes this integrity should also apply to the scientist in communicating with the lay person. But it always begins with the integrity of the individual scientist:

> The first principle is that you must not fool yourself -- and you are the easiest person to fool. So you have to be very careful about that. After you've not fooled yourself, it's easy not to fool other scientists. You just have to be honest in a conventional way after that. [p 343, emphasis added]

And he gives illuminating examples where integrity came into play, where it didn't. Worth getting the book from the library just to read this last essay, "Cargo Cult Science."

> ADDENDUM: As a member of the Presidential Commission investigating causes of the Challenger explosion, Feynman demonstrated not only his deep insight in physical problems but also his contempt for scientists and engineers who "fool themselves." Considering his sensitivity to fakery on social problems, one could hope he would play more of a public role in the struggle for peace. While he shows no signs of being a Marxist, he is also clearly no wishy-washy liberal by temperament, and hence might provide great leadership in the scientific community if he widened the scope of his social concerns and acquired more fundamental understanding of the mess we're in.

Towards Historical Materialism in Physics

Augusto Garuccio and Franco Selleri, "On Dialectical Materialism and Quantum Mechanics." *Critica Marxista* 11: 304-318; 1973. In Italian.

Reviewing a collection of Soviet papers on quantum mechanics published in Italy, Garuccio and Selleri comment on the wide divergence in the interpretations of quantum mechanics among Soviet scientists and philosophers of science, all of whom consider themselves to be dialectical materialists. The authors are also struck by the general lack, in these papers, of a historical approach to the cognitive problems involved (an important criticism which could be applied to most of modern physics).

> If, [they say,] as seems evident to us, physical theories represent a profound interweaving of irreversible cognitive content (true ideas about parts of the world that are incomplete but correct for what they specify) with content that is historically conditioned and therefore arbitrary with respect to nature (introduced more or less consciously by the researcher in the creative process), then, in order to form a synthesis of the valid cognitive content, one must assume the more exacting task of global criticism of scientific theories in order to distinguish that which represents the external world from that which is arbitrary. To weed out the parts of a theory that are historically conditioned, it is therefore essential to study their historical development, with the connections and reciprocal conditionings between science and society, along the lines of [Soviet Academician] Kedrov's point of view.

On another front, Garuccio and Selleri analyze the philosophic foundations of the Bohr-Fock variant of the Copenhagen interpretation to find that an important role is played by the special definition used for *physical reality*, a concept which refers to current theory and experimental results ("the image of objective reality which physicists have been able to construct for themselves at a given moment in history"), rather than to the independently existing objective physical world. The basis for this definition, they find, rests not in the obvious fact that physical reality is more complex than any theory can comprehend but, rather, from accepting the idea that microscopic reality "manifests itself at the macroscopic level in some way that is completely divergent from its true intrinsic nature." Thus, for example, "the contradiction between wave and corpuscle does not derive from the true nature of the electron but only from its means of manifesting itself, conditioned by instruments of measurement at the macroscopic level." Here again, the lack of a historical approach is evident in the failure to differentiate between the material reality of the subject under investigation and the *relative* reality of prevailing physical concepts.

This critique dates from 1973, of course. The question now is to what extent it still applies. How much of a trend is there among Soviet physicists and philosophers toward a materialist and *historical* interpretation of quantum mechanics in opposition to that of Bohr-Fock?

In a broader sense, the same question must be asked of all Marxists in the world's physics community. No approach to any scientific problem can be considered truly Marxist if it is not historical. Consider how the ideas of Thomas S. Kuhn, despite his philosophical errors and anti-Marxism, continue to epitomize the historical approach to physics in a unique way, and how slow the Marxists have been to winnow out the idealist chaff from the materialist grain of his formulations. This is not a healthy situation.

QUOTE UNQUOTE: "It is just twenty years since *The Structure of Scientific Revolutions* appeared in its deceptively modest original format. Ever since, that book has remained the focal point for passionate debate....The influence of the book shows no signs of having run its course....Kuhn himself has qualified and modified his original scheme, and he has attempted, with limited success, either to redefine or withdraw his notorious term 'paradigm'; but he has found that his original ideas have become such common property that even he can no longer reshape them at his own will." -- Remarks by Frederic L. Holmes, president, History of Science Society, on presenting the Sarton Medal to Thomas S. Kuhn, October 1982 (ISIS 74: 247; 1983).

Reflecting on Einstein and Mach

D.P. Gribanov, et al. *Einstein and the Philosophical Problems of 20th-Century Physics*. Moscow: Progress 1983. 508 pp, index. (Imported Pubns., Chicago.)

There is more satisfying philosophical discussion here than in any other Einstein centenary volume I have seen. In one way or another, consciously or not, our 18 Soviet authors struggle with the peculiar mix of scientific materialism and Machian idealism which characterized all of Einstein's work. Their papers reflect paradoxes in the thinking of Einstein who often cited the works of Mach and acknowledged their "profound influence" on him as a student, especially in relation to the relativity of physical knowledge, yet expressed his dislike for "the basic positivistic attitude" and the "philosophical prejudices" as manifested in Mach's rejection of atomic theory. Because of space limitations, I will comment briefly on a sample of only three papers.

K. Kh. Delokarov provides intriguing quotes from (positivist) physicists, mathematicians and philosophers to the effect that relativity theory is indeed built on epistemological premises that coincide with Mach's ideas. While Delokarov also seeks to prove the "incompatibility" between the theories of knowledge of Mach and Einstein, one may well ponder on Einstein's background and question whether his philosophical and physical ideas, or even his methodology, are quite so free of contradictions as Delokarov and others imply here.

Mathematician A.D. Alexandrov proffers a stimulating probe of relativity theory. He contends that "any sort of physics disappears" unless the structural properties of space-time itself can be defined theoretically, and that present relativity theory provides no answer for this problem. Then he offers a Riemannian definition of space-time as the possible basis for constructing a new relativity theory. What's refreshing here is that relativity theory is seen as open to change and development.

An excellent historical review of "The Einstein-Bohr Controversy" by S.V. Illarionov treats the debate as a dialectical process extending over decades. But the author ends on a nondialectical note of frustration and resignation concerning Einstein's conception of the unity of the world:

> Few scientists now hope to find a general [unifying] principle, fewer still hope to do so by a flight of inagination, but the search for unity is one of the most important motive forces of modern science. The main trend in physics [today] is finding unity through experiment rather than formulating a general principle as a free invention of the intellect.

Let's stop right there, Comrade Illarionov, and ask each other why anyone should simply accept the prevailing metaphysical dichotomy between theory and experiment with not even a hint of new or emergent efforts to bridge the gap and establish the *unity* of these opposites. Can anyone imagine that the next great unifying advance in physics will come without interpenetrating breakthroughs in both theory and experiment? Perhaps both sides have contributed to establishing this present dichotomy: Einstein's rather one-sided emphasis on "free invention" does tend to imply some some degree of independence from empirical results; while his opponents, by their empiricist emphasis, tend to make experiment independent of theory.

The virtue of this volume is that it stimulates philosophical discourse on the paradoxes in the Einstein *oeuvre*. One need not agree with a single one of the conclusions to gain a great deal from studying it.

The translation by Sergei Syrovatkin is remarkably good, with only a very few spots where his ear for English was not tuned finely. It was good to find the helpful subject index, so often missing in Soviet scholarly books. My one criticism is the failure to identify the editor(s) of the volume. Who should get the credit for organizing this stimulating volume? And whom should I scold over the defensive or apologetic note in the preface addressed to philosophers in the West who still believe "that Soviet philosophers take a negative attitude to the theory of relativity which is allegedly incompatible with dialectical materialism." Let us get on with the still unresolved problems of modern physics and see how much Marxist philosophical principles can contribute toward their resolution. No doubt this volume could have contributed more toward that end if the philosophical analyses had been more consciously directed toward helping resolve some outstanding contradictions in Einstein's legacy. [L.T.]

Mystifying Science: Department of Machism

It is, however, equally true that the effect of Einstein's work, outside the narrow specialist fields where it can be applied, was one of general mystification. It was eagerly seized on by the disillusioned intellectuals after the First World War to help them in refusing to face realities. They only needed to use the word `relativity' and say `Everything is relative,' or `It depends on what you mean.' Relativity formed the basis of the work of many popularizations of the mysteries of science.

The physical theories of the twentieth century are no freer than those of earlier centuries from influences derived from idealistic trends from outside science. For all their symbolic and mathematical formulations they still embody much of the flight from reality that derives ultimately from religion, now more and more clearly concerned to provide a smoke screen for the operation of capitalism. The influence of the positivism of Ernst Mach on the theoretical formulations of modern physical theories was a predominating one. Most physicists have so absorbed this*postitivism* in their education that they think of it as an intrinsic part of science, instead of being an ingenious way of explaining away an objective world in terms of subjective ideas. This was brilliantly exposed almost at the beginning of the period by Lenin in his *Materialism and Empirio-Criticism;* but the mystifications of theoretical physics have still continued, and it will take many more years of argument and experience, including political experience, before the logical basis of physics is cleared of the ideas that have nothing to do with the material world.

-- John D. Bernal, *Science in History*. MIT Press 1971 iii, 746.

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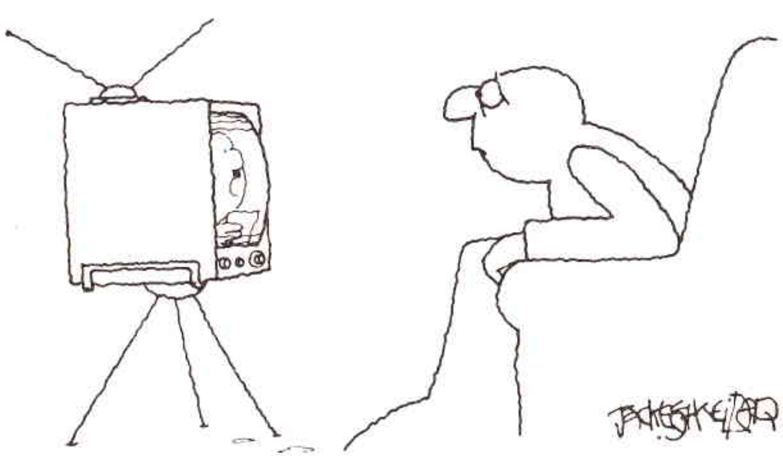
Marx on science and alienation

On the one hand, there have started into life industrial and scientific forces, which no epoch of the former human history had ever suspected. On the other hand, there exist symptoms of decay, far surpassing the horrors recorded of the latter times of the Roman empire. In our days everything seems pregnant with its contrary. Machinery, gifted with the wonderful power of shortening and fructifying human labour, we behold starving and overworking it. The new-fangled sources of wealth, by some strange weird spell, are turned into sources of want. The victories of art seem bought by the loss of character. At the same pace that mankind masters nature, man seems to become enslaved to other men or to his own infamy. Even the pure light 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. This antagonism between modern industry and science on the one hand, modern misery and dissolution on the other hand; this antagonism between the productive powers, and the social relations of our epoch is a fact, palpable, overwhelming, and not to be controverted. Some parties may wail over it; others may wish to get rid of the modern arts, in order to get rid of modern conflicts. Or they may imagine that so signal a progress in industry wants to be completed by as signal a regress in politics. On our part, we do not mistake the shape of the shrewd spirit that continues to mark all these contradictions. We know that to work well the new-fangled forces of society, they only want to be mastered by new-fangled men -- and such are the working men. They are as much the invention of modern time as machinery itself.

-- Karl Marx, speech at anniversary of the People's Paper, London 1856
Marx-Engels *Collected Works* xiv, 655f (New York: International 1980)



"The trouble is not in your set — the president actually said that."

Basic Bookshelf on Marxism in the Natural Sciences

The Fundamentals of Marxist-Leninist Philosophy, F.V. Constantinov *et al.*, editors. Moscow: Progress 1982 (Imported Pubns.,* $9.95).

A complete handbook, giving systematic treatment of dialectical materialism and the theory of knowledge. Very accessible and highly useful to scientists.

Reader in Marxist Philosophy, Howard Selsam and Harry Martel, editors. New York: International 1963, $4.95.

Handy sourcebook of selections from Marx, Engels and Lenin, introduced to provide coherent coverage of topics in Marxist materialism and the dialectical method.

Marx, Engels, Lenin on Dialectical Materialism. Moscow, Progress 1977

As a publisher's note says, the (unnamed) compilers "have confined themselves to the task of collecting the most important statements of the classics of Marxism-Leninism on dialectical materialism." Very useful despite lack of subject index.

Science in History, J.D. Bernal. MIT Press. 1971, 4 vols. paper $30.

Physicist J.D. Bernal had the help of other great British Marxists in this pioneering historical survey of science from Marxist standpoint. Rewarding insights abound, especially where Bernal rips away the mystical veil from modern physics to perceive its continuing state of flux and the probable need for "a far more radical revision of the relativity and quantum theories."

Frederick Engels, Anti-Duhring. NY: International 1966.

This polemic, against a reformist philosopher now otherwise forgotten, gives the first rounded formulations of the Marxist approach to philosophical problems of the natural sciences. Engels' responses to Herr Duhring's critique of dialectical materialism has a contemporary ring because the issues haven't changed that much.

----- *The Dialectics of Nature.* NY: International 1940. $3.50 paperback.

Even where the science of his time has lost its relevance, Engels' philosophical analysis retains its vitality. Not to be studied as infallible but as a source of amazingly fertile ideas from the unfinished notes of the first Marxist to work the fields of natural science.

----- *Ludwig Feuerbach and the Outcome of Classical German Philosophy.* New York: International 1941. $1.25, paper.

Provides clarity on how Hegel's philosophy relates to that of Marx and Engels.

V.I. Lenin, *Materialism and Empirio-Criticism.* NY: Intl. 1978. $2.95 pbk.

Because Bogdanov and others preached Machist idealism as updated "Marxism," Lenin spent most of 1908 studying physics in the British Museum reading room to write this effective defense of Marxist materialism and theory of knowledge.

----- *Philosophical Notebooks.* Moscow: Progress 1961 (Imported,* $5).

In 1914-15, while World War I raged, Lenin found time to study Hegel and left notes in the margins with illuminating insights that reveal his development as a dialectician.

A.D. Aleksandrov, A.N. Kolmogorov, M.A. Lavrent'ev, eds., *Mathematics: Its Content, Methods, and Meaning.* S.H. Gould, tr. ed. MIT 1969 $27.50.

The mathematical world was deeply impressed by the 1954 publication in Russian of this triumph in communication with the non-mathematician based on Marxist understanding of mathematical concepts and their development in relation to society. It has substantial interest for the mathematician too.

Stephen J. Gould. Collections of popular essays (*Ever Since Darwin; Hen's Teeth and Horse's Toes; Panda's Thumb; Flamingo's Smile)* and scientific books *(The Mismeasure of Man*; *Ontogeny and Phylogeny*, etc.).

Though seldom more explicitly Marxist than a passing reference to Engels, Gould's works provide exemplars for the Marxist mode of thought in biological investigation.

Science and Nature, Lester Talkington, ed. Back issue contents, pp 156f.

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