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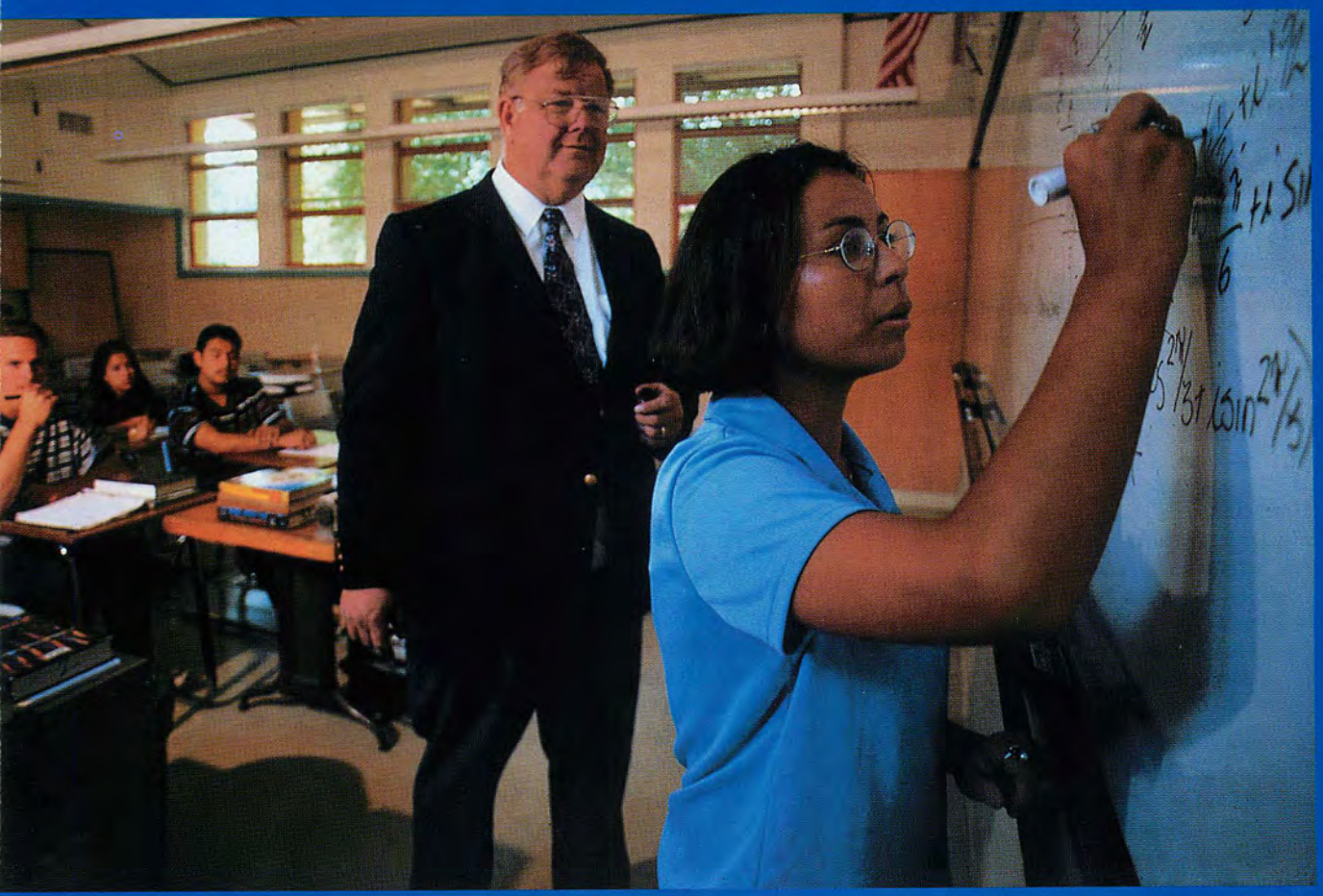
P. 67

The Real Calculus vs. What You Learned



- Challenge to Gravitation Theory
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Chandra X-ray Observatory image of the remnants of Cassiopeia A supernova, courtesy of NASA and Chandra Science Center

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21st CENTURY SCIENCE & TECHNOLOGY

Vol. 12, No. 3 Fall 1999

Features

Courtesy of V. Voeikov

Beyond molecular biology: The discoverer of mitogenetic radiation, Alexander Gurwitsch, reviews the early years of his research, and his reductionist critics, in a newly translated 1943 article (page 41). Here, Gurwitsch and his wife, Lydia.

On the cover: High school college prep course in calculus. Photograph by Lara Jo Regan/ Liaison Agency. Cover design by Rosemary Moak.

News

Departments

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Return to the 'American System' of Calculus

Readers may be surprised to learn that there exists an "American System" of calculus, as there exists an American System of economics—neither being the brand sold in today's classrooms. Yet, despite that consumer fraud known as a modern university education, the fact is, that the anti-Newtonian and anti-Lockean philosophy of the true inventor of the calculus, Gottfried Wilhelm Leibniz, was the *credo* of the leading republican thinkers who engineered America's freedom from the British crown.

It is to Leibniz, philosopher, scientist, statesman, and universal thinker, that we owe our legally protected "inalienable rights" to "life, liberty, and the pursuit of happiness."[1] ("Happiness" for Leibniz did not mean what the vast majority of today's tragically miseducated citizens would assume—the satisfaction of material or libidinal wants—but, rather the realization of the true potential within every single member of the human species, that which distinguishes every human being from a mere beast, that is, the potential for exercise of the creative powers of the mind, or what is referred to in the Mosaic and Christian tradition as the "divine spark" of Reason.)

And it is to the same Leibniz and his followers on both sides of the Atlantic, that we owe modern science. An alien view, that modern science is based on the concept of man as merely a more sophisticated beast, capable only of receiving and processing sense perceptions ("information"), is the one that prevails today: It permeates nearly everything we read, and even think, about science. The view is false, and its pursuit can lead only to error, not only in matters of natural science *per se*, but also in economics and politics, as contributor Philip Rubinstein addresses in the Pedagogical section of this issue.

Our Anti-Newtonian Roots

In America, such universal intellects as the Pennsylvania colony's James Logan (1674-1751), Harvard's Professor John Winthrop (1714-1779), and Benjamin Franklin himself were partisans in the battle for the *real calculus,* and the scientific method that guided Leibniz in its creation, as against Isaac Newton's fraud. Indeed, it was only in America that Leibniz, hounded throughout Europe in his later years by a British disinformation campaign, could get a fair hearing for some years to come.

Thus, in a 1727 letter to New York Governor William Burnet, James Logan wrote:

"'Tis certain the world was obliged only to Leibnitz for the publication of that method, who was so fair as to communicate it in a great measure to [Royal Society Secretary] Oldenburg in 1677, when Sir Isaac was so careful of concealing his, that he involved it in his Letter [of] 1676 in strange knotts of Letters, that all the art & skill of the universe could never Decipher. . . . And yet foreigners have generally been so Just as to pay all possible deference to Sir Isaac as an Inventor, tho' till his Publication of the Principia in 1687, they never had anything of it from him."[2]

Though the memory of James Logan is itself victim of our all-pervading cultural decline, this universal intellect, self-taught in Latin, Greek, Hebrew, and four modern languages, and a lifelong student of the physical sciences, was a secretary to William Penn, and later mayor of Philadelphia and lieutenant-governor of the Pennsylvania colony. It was Logan who, along with Harvard's John Winthrop, steered Franklin into his researches into elec-

tricity, for the explicit purpose of disproving one of the shibboleths of Newton's worldview: the doctrine of forces acting in an *empty space.*

In chapter two of his still unpublished polemic against Thomas Hobbes and the British ideologues, Logan targets Newton, and counters Newton's doctrine with the thoroughly modern (we might even say *avant-garde*) conception of a space filled with electricity, which is responsible for motion:

"And if there be no heresy in mentioning it in the present age, why may we not venture to question the reasonableness of asserting a vacuum as indispensably necessary to the continuance of motion? The argument may indeed hold in relation to all such bodies, the matter of light excepted, as our senses are formed to take coginicance of, but shall we from thence presume to judge of all the kinds of subtile matter that space may be filled with? Can we be sure that there is no electric or elastic medium that instead of obstructing or retarding motion may be the very means of continuing it?

"Can we say an exhausted receiver is a vacuum because the air is drawn out of it, while at the same time we see it filled with light, the matter of which in the true nature of things and on a just estimate of them, tho' not according to our apprehensions, may possibly be a more essential substance than the earth or stones we tread on?"

Logan's extensive library was open to the members of the young Franklin's study circle, the Junto, in Philadelphia, and the two were fast friends. Forty years later, in 1766, when Franklin made his trip to Germany, he was feted by the circle of Leibniz devotees at Göttingen University, led by Abraham Gotthelf Kästner (Gauss's astronomy professor) and geologist and librarian Rudolf Erich Raspe, who were just then combing the Leibniz archives in Hanover to revive and publish the master's works. Franklin was remembered in the British crown territory of Hanover for his prediction that the American colony, its population doubling every 25 years, would soon be free of Britain, and for his electrical demonstrations.

That was the program of American science, and the only true foundation of our modern science. Leibniz's is the "American System" calculus. If you were educated some other way, there is still time to "get with the program." Such effort would prove to be its own reward, as well as an act of patriotism toward one's nation and its founders.

—Laurence Hecht

Notes

1. See Philip Valenti, "The Anti-Newtonian Roots of the American Revolution," *Executive Intelligence Review,* Vol. 22, No. 48 (Dec. 1, 1995), pp. 12-31; and, Graham Lowry, *How the Nation Was Won: America's Untold Story 1630-1754* (Washington, D.C.: *Executive Intelligence Review*, 1988).
2. Valenti, p. 25.

   In his next letter to Burnet, Logan asserts that the still-now persisting, historical fraud of Newton's "invention" of the calculus was no more than a giant political ploy by the forces behind George I of Hanover (today Windsor) to discredit Leibniz, who was in line for the British prime ministership by virtue of his close advisory relationship to the Electress Sophie. (Leibniz was fired by the almost illiterate George I, immediately after the death of his mother, Sophie, in June 1714. Queen Anne died one month later, making George I the King of England.) Speaking of the near deification of Newton in England, and his wish that Leibniz and Sophie had come to power there, Logan writes:

   "He [Newton] is, however great, but a man, & when I last saw him in 1724 walking up Crane Court & the stairs leading to the [Royal] Society's Room, he bent under his Load of years exceeding unlike what they have Represented him two years after as in body. 'Tis but reasonable to expect a declension elsewhere, so that for his own honour as well as the Nation's, to which he has been a very great one, had he & Queen Anne both been gathered to their Ancestors by the year 1710, before that fierce, unnatural Dispute broke out between him and Leibniz, *which I always believed, was blown up by the forces of the Society in opposition to the house that had so long employ'd Leibniz. . .*" (Valenti, p. 25; emphasis added).

With this issue, we welcome back Editor Laurence Hecht, who was released on parole July 13, after spending nearly six years as a political prisoner in Virginia's prison system. Hecht's 33-year sentence was part of a nationwide witch-hunt against leading political associates of Lyndon H. LaRouche, Jr., three of whom are still in Virginia prisons.

Letters

The Non-pollution of NO

To the Editor:

Frank Cornell, in the article "The Foibles of Air Pollution Research" [Spring 1999, p. 4], wonders if removal of sulfur and nitrogen from fuel would not make catalytic converters superfluous. The answer is that without them, the residual hydrocarbons would still cause harmful ozone levels, as a result of photochemical reactions involving hydrocarbons and natural levels of NO_x in the atmosphere.

However, tailpipe NO_x is not the problem it is made out to be. Nearly all tailpipe NO_x (about 90 percent nitric oxide, NO) is from nitrogen fixation ($N_2 + O_2 \rightarrow 2NO$) at the very high temperatures during combustion, rather than from fuel. Nitric oxide should not be classified as a pollutant because it is colorless, and evidence is lacking for harmful health effects. In the bloodstream, it has been shown to play a beneficial role in erectile dysfunction. It decreases atmospheric ozone levels by the rapid reaction: $NO + O_3 \rightarrow O_2 + NO_2$.

Nitrogen dioxide is certainly not "one of the most dangerous pollutants known to man." It is a weak oxidizing agent, and about 10 percent as toxic as ozone, per a Delphi review of the data. Atmospheric levels now meet questionable health standards.

William B. Innes, Ph.D.
Upland, Calif.

The writer, now retired, is a chemist and an expert in atmospheric pollution.

Black Holes and Biology

To the Editor:

I greatly enjoyed the Summer 1999 issue of *21st Century.* Every article was about interesting, cutting-edge topics, and every article went into detail and depth totally missing from "mainstream" science magazines.

I believe there may be an intriguing, very speculative link possible between two articles from the summer issue,

which at first glance appear to be totally unrelated. (1) The biology article by V. Voeikov ["The Scientific Basis of the New Biological Paradigm"] brings up the point that there is some qualitative and complex difference between living systems and non-living systems. (2) The entropy/black hole article by M. Rabinowitz ["Entropy of the Universe and Little Black Hole Manifestations"] speculates that certain observable phenomena at the macro level may be manifestations of black hole physics at the micro level.

(3) What if we make one little creative leap here: Perhaps living cells themselves are macro, biological manifestations of micro black holes. That is, would many of life's strange, complex, field-oriented interactions take on a very interesting light if cells (or pivotal structures within individual cells) were to be viewed as kinds of "accretion spheres" surrounding micro black holes? This view creates interesting speculative connotations and implications for thinking about cells, organisms, and life processes in general, which do not immediately seem to be without merit.

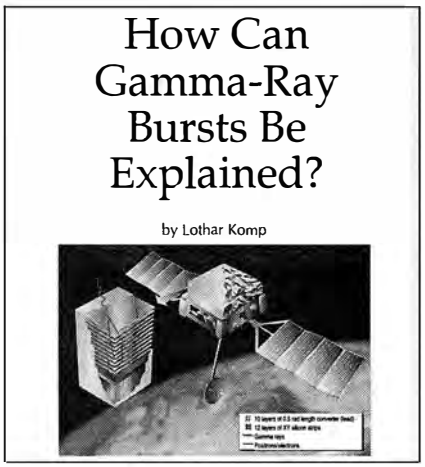
Mark Warrian
Oak Lawn, Ill.

The Latest on Gamma-Ray Bursts

To the Editor:

I was disappointed to read your article in the Summer 1999 issue, "How Can Gamma Rays Be Explained?" [by Lothar Komp, p. 48]. It was an extremely well written, interesting, and informative article. However, it was outdated! I noticed that this article was originally printed in 1998. It was disappointing to see that you were careless enough to give your readers outdated, old information.

I would like to inform you that, despite the article's 1998 puzzling over whether gamma-ray bursts are paralleled in other wavelengths, gamma-ray bursts have been observed optically in *real time* (not just an afterglow) by the ROTSE (Robotic Optical Transient Search Experiment) in Los Alamos, N.M., on Jan. 23, 1999. GRB 990123 peaked at a magnitude of 8.9 (it could have been viewed with binoculars) and lasted for approximately a minute. It had a red shift of 1.6. It took 10+ billion light years to reach us



and was located somewhere near the constellation Bootes.

This kind of information is not just of interest only to astrophysicists; it was of interest to me, a 17-year-old girl, attending high school in Hawaii. Please give this information out to your readers!

Andrea Sabino
Honolulu, Hawaii

The Editor Replies

We are happy to have high schoolers critically reading *21st Century!* Although some of our articles may not have up-to-the-minute data, we aim to convey the kinds of ideas, or ways of thinking about science, that have a timeless importance, even if some of the particular predicates change.

Jacques Pradel Responds On Simplifying Radiation Risk

French radiation expert Jacques Pradel here responds to a letter in the Summer 1999 issue, by John Cameron, radiation specialist and Professor Emeritus at the University of Wisconsin, who commented on Pradel's Viewpoint article, "Let's Simplify the Concept of Radiation Risk," which appeared in Spring 1999, p. 12.

To the Editor:

I am rather astonished by Dr. Cameron's misunderstanding of my Viewpoint.

(1) I do not think I have written that the no-threshold linear law was a fact. I do not believe personally that it represents a real risk.

(2) I think the epidemiological studies will always be able to show that the probability for an effect of small doses is extremely weak. But the antinuclear fundamentalists will never agree to consider this probability, as weak as *it* may be, as "insignificant."

(3) I did not write that very small doses have an effect. I simply said that, even if we admit this linear no-threshold law, the effects should be compared to those resulting from living at elevations that are a few meters higher, which seems to me to be a rather reassuring comparison for people.

(4) It is curious to say we cannot use cosmic rays or radon concentration as references, because they vary too much with time and space, and then to propose instead to use the "natural dose"; this dose originates from different causes, mainly radon, and thus itself varies.

(5) Readers of this magazine probably use airplanes rather frequently, but experience has taught us that making comparisons of radiation using the dose received during an air travel is not always very convincing for the general public, which does not use airplanes frequently and may consider it as a risky way of travelling.

(6) My paper aims to show how we must give references with comparisons that are understandable by the public and not only by the experts.

I am not very familiar with the "BERT approach" [the radiation comparison unit proposed by John Cameron] but it does not seem to me that it brings in new elements, compared to already in use practices of comparing natural and artificial radiation.

I would appreciate it if Dr. Cameron would be able to make the International Committee on Radiation Protection and other such institutions accept the fact that there is no risk for doses lower than 0.2 gray. Unfortunately, there is still work to be done to accomplish that, and further, to convince the public that this is the case.

I do not think that natural background, which I have often used, is the best reference, because it is a little bit too complex. I prefer, instead, to use one of its components, such as altitude or radon's average concentration in houses, because I think it is more convincing.

This is what I wanted to explain.

Jacques Pradel
Paris, France

The U.S. Food Quality Protection Act Is Worse Than the Delaney Clause

by Dr. J. Gordon Edwards

The unfortunate wording of the 1958 Delaney Clause has encouraged unscientific assumptions about testing foods for carcinogenicity and has fostered irrational beliefs about food and chemicals. The 1996 Food Quality Protection Act, however, is far worse.

Let me begin with the problems of the Delaney Clause. In 1958, Representative James J. Delaney entered a clause into the food additive provisions of the Federal Food, Drug and Cosmetic Act. The intent was to permit only toxicologically insignificant amounts of additives in our food supply. Both Congress and the Department of Health, Education, and Welfare construed the Delaney Clause as specifying that an *insignificant* amount of chemicals, including carcinogens, could be legally permitted to occur in human foods. (They did not interpret the Clause to require *zero* amounts.

Section 408 required that raw foods or produce conform with tolerance levels established by the Federal Food, Drug, and Cosmetic Act. If the levels were higher than the permitted legal "tolerance," the raw food would be considered "adulterated," and could not be sold.

Section 409 dealt with the intentional addition of chemicals to processed foods. This section included the Delaney Clause, which stated: *"No additive shall be deemed to be safe if it is found to induce cancer when ingested by man or animal, or if it is found, after tests which are appropriate for the evaluation of the safety of food additives, to induce cancer in man or animals."* (21 USCS, Section 348, page 280)

The Clause began with: *"No additive shall be deemed to be safe if it is found to induce cancer when ingested by man . . . ."* Obviously, it was impossible to determine a cause of human carcinogenicity unless long-term, strictly regulated tests were performed. In such tests, all activities of a large series of nearly identical, same-sex humans, would have to be carried out; half of the isolated humans would have to daily consume huge doses of a test chemical, while the other half would have none. If, after months or years on such diets, the "test" humans developed cancer, but none of the "controls" developed that same sort of cancer, it could be hypothesized that the tested chemical might have caused cancer in the "test" humans.

Stuart Lewis

Can we have an assured food supply when decisions on pesticides are made so capriciously and unscientifically? Here, Gordon Edwards at a Washington, D.C. press conference on the benefits of DDT.

Such tests have never been performed, and obviously never could be performed; therefore, that part of the Delaney Clause was meaningless.

That first sentence in the Clause continued, however, with the words: *". . . or if it is found, after tests which are appropriate for the evaluation of the safety of food additives, to induce cancer in man or animals."* The most important word there is *"appropriate."* There has never been any broad agreement on how to decide which animal tests are "appropriate" for accurately determining cancer risk.

Can doses thousands of times greater than any available outside of the lab cages be considered "appropriate" for accurately determining "cancer risk"? Can intubation of poisons directly into the stomach be considered "appropriate" dosages? What about dosing neonates before birth? Should massive doses be fed daily, or two or three times each day?

Almost none of the feeding "experiments" that have been performed on birds or mammals could be considered "appropriate" for the determination of "cancer risks" for free-living animals, and the results certainly could not be extrapolated to humans living normal lives!

What happened is that "test animals" were routinely fed "maximum tolerated doses" (meaning that any increase in the amount of chemical ingested would be fatal). Animals ingesting maximum tolerated dosage can barely stay alive, and the high dosage causes the death of body tissues. As a result, there is a proliferation of new cell divisions, during which numerous mutations naturally occur. The development of tumors or cancers is therefore increased, but those mutations were *not* directly caused by the tested chemicals.

When Rep. James J. Delaney proposed his Clause, the medical definition of "cancers" was that they were malignant, invasive growths that frequently metastasize, and may kill the host. Tumors, on the other hand, were *benign* swellings or lumps. Some might become malignant, but most do not. Often such tumors disappear after the massive overdoses of test chemicals are halted.

EPA Head Redefines 'Cancer'

In October 1975, Russell Train, an attorney who was then Administrator of the U.S. Environmental Protection Agency, redefined the medical term, "cancer." He said that tumor-causing and cancer-causing substances are synonymous, and ruled that *"For pur-*

poses of carcinogenicity testing, no distinction should be made between the induction of tumors diagnosed as benign and the induction of tumors diagnosed as malignant." At the EPA, even chemicals that caused benign "tumors" would therefore be subject to regulation under the provisions of the Delaney Clause.

The Council for Agricultural Science and Technology, a consortium of more than 30 scientific organizations, observed that *"Classifying as carcinogens all chemicals that cause tumors . . . greatly overestimates the 'cancer risk.'"* Obviously, tests for tumor-causing agents were not *"appropriate for the evaluation of the safety of food additives to induce cancer in man or animals,"* as required by the Delaney Clause. Furthermore, some chemicals, including selenium and vitamin A, are *anti*-carcinogenic at low levels, but become carcinogenic at higher levels.

Another problem with cancer testing was that rats, which have been the most common test animals, were later found to produce a special protein (Alpha 2U Globalin) that makes them especially prone to tumors and cancers. In 1991, even the EPA pointed out that humans lack that protein, which the EPA said could "invalidate thousands of tests of pesticides, preservatives, additives, and other chemicals that have been banned on the basis of producing tumors in rats in laboratories." Those tumors, the EPA said, "are a species-specific effect inapplicable to human risk assessments" and "are not relevant to human risks from those chemicals."

All such information was ignored by promoters of the Delaney Clause. Rep. Delaney himself was upset at the destructive interpretation of the Clause. He had sought to require that decisions about pesticides be based on the results of "appropriate" tests. He assured me that he intended that the rodents would be fed only small amounts of the chemicals, which was why he specified that "appropriate" tests be required. Delaney was especially dismayed by the common belief that if *any* level of a chemical caused cancer in rodents, that chemical must be banned "because of the Delaney Clause."

Stuart Lewis/EIRNS

The EPA's "risk cup" may jeopardize America's fruit and vegetable supply.

From Bad to Worse

In 1988, the EPA sought to make the Delaney Clause more workable by permitting insignificant, harmless amounts of pesticide residues in processed foods. The EPA termed this a *de minimis* standard. In 1992, the pro-environmental 9th Circuit Court of Appeals in San Francisco, destroyed the EPA's efforts to proceed rationally. Evidently, the members of the Court failed to notice the words *"appropriate tests"* in the Delaney Clause! As a result of decades of failure to carefully read Delaney's specific wording, the Delaney Clause was rejected. Congress then replaced it in 1996 by much more potentially destructive legislation, the Food Quality Protection Act.

The Food Quality Protection Act (FQPA) gives the EPA even greater power to harm American food producers and suppliers. This remarkable mandate states that the EPA may ban a pesticide, *unless* it believes "there is reasonable certainty of no harm" from the total amount of that pesticide in the aggregate of food, water, or residential use. The law required a reassessment of all existing pesticide tolerances between 1996 and 2006. This was to be done by a Tolerance Review Assessment Committee (TRAC), which would reassess potential limits for human exposure to pesticides. Unfortunately, the TRAC contains some non-scientists, from environmental organizations, which may make reasonable decisions difficult.

The FQPA will consider potential effects of each of thousands of chemicals in food and liquids available to humans or other life forms. By August 1999, the EPA was to complete its analyses of 3,000 pesticides, and establish tolerance levels. At the top of the list are the organophosphate and carbamate insecticides. Those categories include about three-fourths of the insecticides needed to protect American crops, upon which our balance of trade is dependent, as well as our nutrition.

Perhaps it is not surprising that the director of the EPA's pesticide programs said that one way to implement the Food Quality Protection Act would be simply to revoke all insecticide tolerances and start over! Hopefully, she was being facetious.

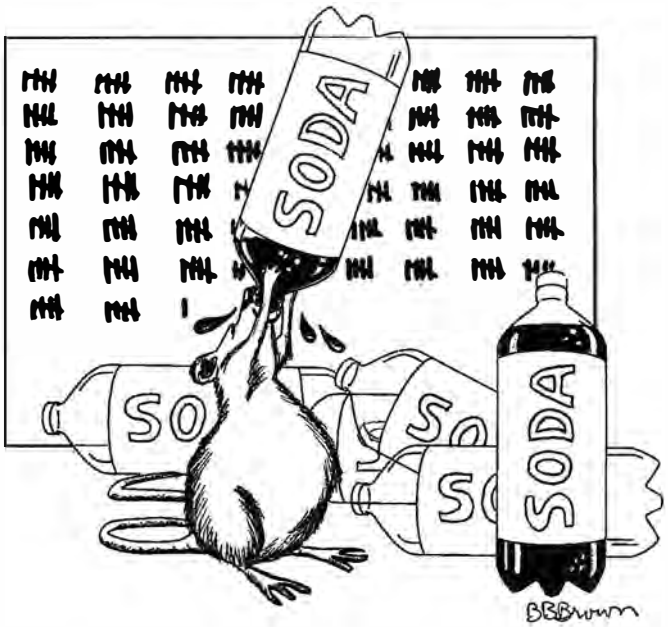
An Unreasonable Methodology

The EPA intends to *estimate* a dietary risk for each pesticide (and also *estimate* non-dietary exposures). After all of the estimates, from all sources, are combined, the EPA will decide upon a total level of risk that it considers acceptable. This level is termed the "risk cup," which cannot be legally exceeded. When the "risk cup" for any pesticide is full, the EPA says, no additional uses of that chemical can be approved unless others are removed from the "cup." To further complicate the process, the EPA will assign every pesticide to one of three poorly differentiated "groups," each containing various poorly differentiated "classes" of chemicals.

The method used to establish the amount of each pesticide permitted to

be used will be based on the EPA's *"reasonable certainty of no harm."* The EPA has seldom been considered as "reasonable," but this agency will now regulate the nation's health, welfare, and agriculture, based on a vague interpretation of "reasonable certainty."

But what must the EPA be "reasonably certain" about? The answer the agency gives is that it is about *"harm."* Unfortunately, the EPA definition of "harm" only leads to further confusion, and may cause catastrophic results in the food supply. Unspecified categories of "harm," will surely include "harm" to the physical environment, the ecosystem, and to all forms of life. With the EPA making decisions on what constitutes "harm," the failure of American agriculture, forestry, medicine, and most businesses rests in the agency's hands.



The case of the ban of saccharin, was one of the few times that the public was told, how much of the chemical rodents were fed before they developed tumors, and therefore people could compare that to the number of cans of soft drinks a person would have to drink daily (800 daily for life!), in order to ingest a comparable amount of saccharin. As a result, most Americans objected to the ban.

Even if the EPA could provide a "reasonable" definition of "harm," further difficulty centers on their definition of "no." The word *"no,"*, as in *"no harm,"* also potentially threatens the nation's well being. The agency cannot realistically equate "no" with "zero" as a standard of measurement, because that is unattainable, so what low level of certainty *will* the EPA utilize? It could settle on *any* level, but the standard decreed by the EPA must obviously be less stringent than "no" or "zero." How will the amount of harm be measured?

What About Natural Pesticides?

Because of propagandists' fears of even legal traces of pesticides in food, a few of them have trumped up what they refer to as "organic farming." But organic farmers will also be hit by the FQPA. Disciples of organic farming do not often use synthetic chemical compounds to control pests, but they may use *natural* poisons (such as chlorine, arsenic, cyanide, fluorines, sulphur, and mercury) and poisons derived from plants (nicotine, rotenone and pyrethrum). Those poisons should all be banned by the EPA if they violate the FQPA definitions.

Then there is the issue of *natural* pesticides. Dr. Bruce Ames, world-renowned biochemist at the University of California, has pointed out that edible plants often contain natural pesticides making up to 5 percent to 10 percent of the plant's dry weight. "We are ingesting in our diet at least 10,000 times more, by weight, of natural pesticides than of man-made pesticide residues," Ames has shown.[1]

These levels of natural pesticides can be enhanced by genetic engineering of plants. Biotechnicians have recently altered some food plants genetically, with the amount of natural poisons in their leaves becoming so high that pests are killed simply by eating the engineered plants. Others have experimentally incorporated *Bacillus thuringiensis* into the plant's genetic makeup, where it kills insects just as quickly as if the plants had been physically treated with *Bacillus thuringiensis* insecticide.

The EPA is now considering banning genetically altered plant varieties, by simply classifying the individual *plant* as an "insecticide." Can the EPA decree that there is "reasonable certainty of no harm" resulting from the presence of such poisonous plants in the fields? Will strict limits be placed on the numbers of such plants that the EPA will permit in each field?

May we assume that the EPA chose its words deliberately, aware that FQPA activists could employ *"reasonable," "harm,"* and *"no harm"* in any way they desire? If their intentions were not malicious, would not the EPA have avoided such serious misinterpretations? For example, the agency could have stated, "no significant (or demonstrable) danger of causing serious harm to *non-target* organisms."

Odd actions and untenable decisions continue at the EPA. Can Americans be assured of a safe and adequate food supply when we remain at the mercy of this capricious, monolithic organization?

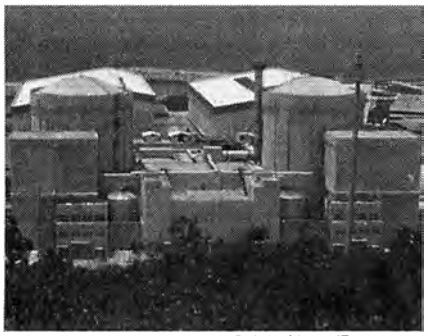
Gordon Edwards, Emeritus Professor of Entomology at San Jose State University in California, has taught biology and entomology there for more than 50 years. He is a long-time member of the Sierra Club and the Audubon Society, and is a fellow of the California Academy of Sciences.

Notes

1. See Bruce Ames's articles: "Environmental Pollution and the Causes of Human Cancer," *21st Century*, Summer 1990, p. 38, and *Science* magazine, Vol. 236, pp. 271-280.

EDITOR'S NOTE

For more on the Delaney Clause, and how it curtailed the development of food irradiation, see this issue's Nuclear Report, p. 24.



Gabriel Liesse/Framatome

China's Guangdong Nuclear Power Station has two 985 MWe pressurized water reactors, built jointly with Framatome.

CHINA'S NUCLEAR-GENERATING INDUSTRY PLANS RAPID GROWTH

The China Nuclear Industry Corporation announced Sept. 9 that China has shifted its nuclear industry from military to civilian production over the past 20 years, and now combines military and civilian nuclear production in a diversified economy. As reported by Xinhua News Service, the nuclear sector has set up many enterprise groups and developed more than 1,500 civilian products. The Qinshan Nuclear Power Station in east China's Zhejiang Province and the Daya Bay Nuclear Power Station in south China's Guangdong Province are now operating, and construction is ongoing for plants in Qinshan, Ling'ao, and Lianyungang. By early next century, China's nuclear generating capacity is expected to reach 9.2 million kW. By 2010, Chinese experts say that the nation's generating capacity could reach 20 million kW (about that of Germany today), doubling by the year 2020 to 40 million kW.

TRANSPLANTED BETA CELLS SHOW PROMISE AS CURE FOR DIABETES

Tissue-culturing technology developed in the bioreactor for use in space, is showing promise for growing insulin-producing beta cells that can be successfully transplanted in patients with diabetes. The bioreactor, which simulates on Earth some of the effects of the microgravity of space, has been able to support three-dimensional tissue growth for studying disease, and also for growing cells that can be the basis for "replacement parts."

In 1997, NASA and the Juvenile Diabetes Foundation signed an agreement for joint work, including bioreactor research to cultivate and transplant beta cells in diabetics. Based on the success of the ground-based experiments, a Technology Transfer Agreement was signed with the VivoRx Company in Santa Monica, Calif., to develop a method for the implantation of cultured human islet cells, using treated seaweed membrane encapsulation. The seaweed allows insulin and glucose to diffuse back and forth, so the transplanted cells work as an artificial pancreas, and allows the cells to be accepted by the diabetic patient's immune system. Once transplanted, the cells secrete the appropriate amount of insulin for regulating the body's blood sugar levels. The results in human volunteers have been promising, and the technique is now undergoing the third level of testing by the Food and Drug Administration.

'TRANSMUTATION: THE ERA OF NUCLEAR ENERGY IS JUST BEGINNING'

If the problem of nuclear waste were posed to youth as a problem soluble by the application of advanced transmutation technologies, there would be a rush to take part, Prof. Reinhard Odoj told a forum in Bonn, Germany, Sept. 14, titled "Transmutation: The Era of Nuclear Energy Is Just Beginning." Odoj, who heads Germany's program for the chemical separation of nuclear waste, discussed his team's recent breakthroughs in separation technology, which will greatly facilitate the transmutation process. The other major speaker, Helmut Boettiger, developed the idea that the era of harnessing the nuclear binding forces was just beginning, and that by attempting to throttle it, Germany's red-green government was "reactionary." The forum was sponsored by the Science Attachés Club of Foreign Embassies in Bonn and the publishing house Dr. Boettiger Verlag. Boettiger is well known as the publisher in Germany of books by Lyndon LaRouche, as well as books challenging environmental catastrophe theories (including those books published by *21st Century*).

Cyane Lowden, Science Museum of Virginia

A fourth-grader gets some help building a small house in the "If I Had a Hammer" program at the Science Museum of Virginia in Richmond.

'IF I HAD A HAMMER': TEACHING MATH BY BUILDING HOUSES

A carpenter who realized that he could do on the job the same complex geometry problems that he could not do when he was in math class, initiated a program to teach children math, art, and architecture as they build a house. Called "If I Had a Hammer," the program is now sponsored nationally by Home Depot, and hosted at museums around the country. Its founder, Perry Wilson, based the program on his own experience, when, after failing fifth grade, his father took him out to the backyard to build a tree house.

TYSON FOODS LAUNCHES PLAN TO MARKET IRRADIATED CHICKEN

Tyson Foods, one of the largest poultry providers in the world, announced Sept. 20 that it had signed an agreement with the Titan Corporation to use its electron-beam irradiation technology on selected poultry items, as "part of Tyson's strategy of providing its customers the highest quality and safest product in the industry." Tyson and Titan are aiming for a market test in spring 2000. Tyson joins the largest and second-largest ground beef producers in the United States, Iowa Beef Packers (IBP) and Cargill Foods, which have already made agreements with Titan to use its state-of-the-art irradiation facility under construction in Sioux City, Iowa. The final regulations permitting beef irradiation are expected to be announced in November 1999.

In its position statement, Tyson Foods says that it "only intends to offer 'irradiated' products as an alternative to some of our existing products for the benefit of those . . . consumers who make the informed decision to choose them. The long-term consumer demand and the ability of the market place to support the cost of this technology will be our, and we believe the industry's, keys to future expansion."

Dick Connelly

Irradiated poultry, processed by Food Technology, Inc., has been available for supermarkets since 1993. Here, a display at Carrot Top in the Chicago area.

SEN. DOMENICI REQUESTS GAO STUDY OF RADIATION STANDARDS

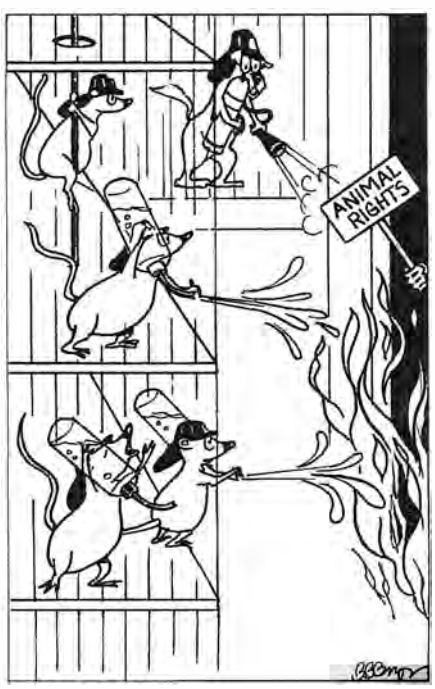
U.S. Senator Pete Domenici (R.-N.M.) asked the General Accounting Office July 21, to update its 1994 study on radiation protection in the United States, with emphasis on examining the validity of standards on which current policies are based. The senator said he was concerned about the cost impact of the so-called linear no-threshold hypothesis now in effect, which assumes that any amount of radiation, no matter how small, can cause health effects, such as cancer. "The linear no-threshold hypothesis is now questioned by many scientists and health professionals, who assert radiation doses below certain levels, a threshold, have no deleterious health effects at all. If radiation protection standards are unnecessarily restrictive, the impact on the costs of high-level waste disposal (such as Yucca Mountain), low-level waste disposal, power plant decommissioning, and decontamination, and DOE's environmental cleanup could be huge," Domenici said.

'WOULD THE GREEN MOVEMENT HAVE PULLED TVA'S PLUG?'

This is the title of a special report released by the Greening Earth Society, which examines the likelihood that the regional economic development the Tennessee Valley Authority brought to the impoverished people of the Tennessee Valley, starting in the Depression years, would be impossible today, under current environmentalist thinking. Society president Fred Palmer notes that what the TVA did to eliminate poverty, building power plants and dams, is "precisely the things the Kyoto Protocol is meant to forestall in the Third World where life today is much as it was in Appalachia during the Depression." For more information, contact the society at info@greeningearthsociety.org.

MEDICAL BREAKTHROUGHS JEOPARDIZED BY ANIMAL RIGHTS TERRORISTS

Americans for Medical Progress held a press conference July 21 in Washington, D.C., featuring prominent medical researchers, who documented how animal rights terrorism is threatening future medical breakthroughs. Richard W. Bianco, Director of Experimental Surgery at the University of Minnesota, described an attack on the University laboratories, April 5, 1999, which destroyed two years of Alzheimer's disease research. Credit was claimed by the "Animal Liberation Front," whose spokesman told the press, "We're proud of the courageous activists who have risked their lives." Cancer, Parkinson's disease, and other ongoing neurological studies were also ruined. The hooded terrorists kicked in doors, smashed medical equipment, spray-painted slogans on the walls, and manhandled laboratory animals. Later, members of the "Animal Liberation Front" released a videotape of the attack, which it claimed was sent to them in the mail.



IN MEMORIAM: COLONEL MOLLOY VAUGHN

A Man Whose Mission was Progress

by Paul Goldstein

On Aug. 31, one of the finest military officers the United States has produced was buried with full military honors in an impressive ceremony attended by more than 50 people, as Colonel Molloy Vaughn was laid to rest at Arlington National Cemetery.

Colonel Vaughn, 79, who was on the Editorial Advisory Board of *21st Century,* and had worked with the LaRouche political movement over the past 15 years, was bidden farewell with a 21-gun salute; a 75-person honor guard; eight pallbearers; a riderless horse; a caisson carrying his coffin; an 18-piece drum and bugle corps; and a three-star general representing the United States military and a grateful nation.

In his condolences to Col. Vaughn's wife and family, Lyndon LaRouche said, "On hearing of the death of Colonel Molloy Vaughn, nothing seemed more appropriate than to say what sprang to my lips then: As General of the Armies Douglas MacArthur said: 'Old soldiers never die; they just fade away.' In my opportunities to know him, as friend and collaborator, he typified the retired professional officer always at the call of duty when a fine job, calling for something extra, needed to be done. To my knowledge, he often did it, for no other reason than it needed to be done. For him, his career was a way of life, all the way to the end of it. I shall miss him very much."

A Sense of Mission

LaRouche's words appropriately identified the life of Col. Vaughn as one fully dedicated to the idea of a mission for his nation and for all nations.

To fully appreciate his dedication to his nation and life, it is useful to know a bit of his biography. "The Colonel," as he was known, was born in 1920 in Lewes, Delaware, where his father and mother ran the "Dixiedells Farm," a turkey farm. There, as a boy, Molloy became one of the prominent turkey farmers in the state of Delaware. He won the 4-H award for breeding broad-breasted turkeys and by the time he went off to college, he had left his family farm with 5,000 broad-breasted turkeys.

Stuart Lewis

Col. Molloy Vaughn in 1985, speaking on the Strategic Defense Initiative at a conference honoring space scientist Krafft Ehricke.

When World War II broke out, the young Molloy became one of the first ROTC commissioned lieutenants; he was first assigned to the coastal defense of the United States in the state of Washington. Eventually, he served under Gen. Douglas MacArthur in the Southwest Pacific, and he helped liberate the Philippines from Japanese occupation.

After World War II, Col. Vaughn chose a full military career, serving in a wide variety of positions, unlike most officers of his rank—positions that gave him an experience and command over so many facets of U.S. strategic policy, that one Pentagon official who met with this author and Col. Vaughn together commented, "I have never seen a military record like this before," and proceeded to salute the Colonel in a spontaneous gesture that surprised even him.

From being a nuclear testing officer in Bikini Atoll, to serving on an assignment at NORAD, to helping Saudi Arabia build an air defense system—these were just some of the varied parts of his career. Colonel Vaughn also participated in the Korean War and was at Dien Bien Phu as a military observer in 1954, when the French military collapsed against the Viet Minh.

1980s and 1990s

After he left military service, Col. Vaughn was given a secret assignment for the 1980 Reagan Transition Team, to make contact with Palestine Liberation Organization chairman Yasser Arafat and the leaders of all religious groups in Lebanon. The success of that mission enabled the United States to have a much improved set of relations with the PLO, and helped build the foundations for the present Middle East peace process.

During the 1980s, Col. Vaughn collaborated with the Schiller Institute and the *Executive Intelligence Review* in organizing support in Europe and Japan for President Reagan's Strategic Defense Initiative. In 1985, at the request of the Schiller Institute, he wrote a strategic plan on how to "Save Africa" from the devastation of a locust plague and the collapse of the African economies (see p. 12).

As a Christian and a citizen-soldier, Col. Molloy Vaughn represented the notion of "republican virtue and truthfulness" even beyond the call of duty. His selflessness, and his love of his family and country will be remembered by all. God bless this wonderful man.

(Continued on page 12)

IN MEMORIAM: REP. GEORGE E. BROWN, JR.

The Congressman Who Loved Science

by Marsha Freeman

The death on July 15, 1999, of California Democrat Rep. George Brown, at age 79, leaves a gaping hole in the deliberations on science and technology policy in the United States. Brown, who served 18 terms in the U.S. Congress, was an elected official with the rare qualities of fairness and integrity, as well as a technical background in industrial physics. He made his mark, particularly in space policy, as former congressmen Mike McCormack (D-Wash.) had done in nuclear and energy policy nearly 20 years ago.

Brown strongly believed that over the centuries, advancements in science and technology were responsible for the major advancements in society. This belief led him to become a champion for their cause.

The four years of Brown's tenure as chair of the House Committee on Science and Technology, from 1991-1995, were characterized by reasoned discourse, rather than partisan wrangling; an outreach to the science communities of other nations, including the failing Soviet Union; and an educational program for committee members and the public, which included hearings that brought scientists from many fields to brief policymakers on the major issues in science.

Rep. Brown became the most respected champion in Washington for adequate resources for science, especially since the 1994 "Conservative Revolution" onslaught against government spending. He led the campaign for the elimination of politically motivated pork barrel projects that denigrate the very process of rationally funding science.

Brown never abandoned his vocal support for the continuation of a visionary civilian space program, regardless of the mindless criticisms periodically levelled against NASA's programs, although he believed strongly in accountability from agencies for the resources they are allocated by the Congress. Overall, he believed in an activist role for the state, that must base itself, as he said recently, on the "provision of social welfare, and a sense of national identity."

Stuart Lewis

A 'Roosevelt Democrat'

Brown was a "liberal Democrat" in the best tradition of Franklin D. Roosevelt and John F. Kennedy.

His concern for civil rights began while attending the University of California at Los Angeles in the late 1930s, when he organized the first integrated campus housing in order to help break the color barrier. During World War II, Brown mobilized public opposition to what he believed to be the wrongful incarceration of Japanese-Americans.

After earning his degree in industrial physics, Brown was elected to the local city council; later, in 1955, he was elected as mayor of Monterey Park. During that time, he also became a full-time union business manager for the Engineers and Architects Association for the city of Los Angeles.

In 1958, Brown joined the California State Assembly, where he authored legislation to provide public employees with the right to bargain collectively, and once in Congress, in 1962, he fought for passage of what became the 1964 Civil Rights Act. He was present when President Johnson signed that historic legislation. He became an early opponent of the war in Vietnam, and voted against every defense spending bill during that war.

Like many liberal Democrats, Brown did not see the strategic and immense peace-making potential of President Reagan's Strategic Defense Initiative, and he strongly opposed that program. He also became a spokesman for many so-called environmental causes, possibly under pressure from his California constituents, although, in recent years, he moderated his support for such anti-technology efforts.

In an interview with *The New York Times,* published on March 9, 1999, Brown was asked if he were a scientist. "I'm not," he replied. "I'm a politician. I started out with a background in science. I do have a degree in physics and did a short stint as an engineer. I was interested in science before I even knew what science was. I used to read science-fiction magazines when I was a kid, stories about space travel, that led me—structuring my college training in math, physics, chemistry, astronomy—a little bit. I've been on the Science Committee in the House since 1965."

An 'Investment Budget'

If there is one thing any congressmen is loath to do, it is to venture into policy areas that are the venue of a committee other than the ones on which he serves. But in his constant, unrelenting battle with successive administrations and congressional oppo-

(Continued on page 12)

Col. Molloy Vaughn's Plan to Save Africa

On Jan. 14, 1985, Col. Molloy Vaughn presented a policy paper to the Fourth International Schiller Institute conference, held in Richmond, Va., on the topic of moving 25 million tons of food to Africa. We reprint short excerpts from his speech.

You should know why I am standing here today and why this task was given to me. An individual from the Schiller Institute flew out to California, and, on his busy schedule on the evening of New Year's Day, we had a working conference at dinner. Just before the main course was served, he gave me a figure: "How would you face the problem of 25 million tons of relief going into Africa? How would it be arranged for, and the planning get it there?"

When he said that, I lost my appetite for the meal. I am not a negative person, but everything that flashed through my mind in one minute's time were disaster scenes I'd seen from Indochina in 1953-1954, when I was one of the Americans selected to go to Dien Bien Phu to assist the French there. I saw the waste that happened there during the rainy season, when we had literally tens of thousands of tons being unloaded every day, and the parachutes rotted and the food was wasted and the blood plasma never got to Dien Bien Phu.

Then I thought about conditions that I have witnessed in the last 30 years: flood relief, when I helped collect things as a Boy Scout in Delaware to send to Pennsylvania because of the floods there, the famous ones in the 1930s.

These same problems continued wherever I went, as I got into the Middle East, and saw relief going into certain areas. We are a great nation for shipping vast quantities of relief, but when it gets there, we absolutely stagger the people. They cannot handle it, they are not organized to do so, and we feel that we have done our job, and we walk away.

My paper is a proposed guideline for successfully transporting the 25 million tons of food supplies into Africa.

I. Personnel to carry out task:

A. Use of a Berlin Air Lift-type operation (Navy, Army, Air Force, Merchant Marine active units—in uniform).

B. Use of Peace Corps organization (no uniforms).

C. Joint taskforce of nations (under direction of the United States).

D. Use of retired military to staff taskforce.

II. Formation of site survey teams for each country to provide required data for in-depth planning. This must come at the very beginning. . . .

14. Obtain all available climactic studies (rainfall, temperature, dry and wet seasons, sandstorms, and *locust cycles.* I have seen what happens when the U.S. or the U.N. does not get the airplanes or supplies there to destroy locusts. You have a number of hours in which you must hit the sides of the mountains where the locusts are buried before they hatch. When that erupts—you see the sand or dirt "boil"—you either kill them or you have a disaster. . . .

IV. Review all of above data and prepare Master Plan for each host country that has been selected to receive the proposed relief program. Secure host country acceptance of the Master Plan before you start to carry it out. . . .

With the implementation of the above guidelines, which would result in the successful receipt of 25 million tons of relief supplies in Africa, many of the vital projects could be started that are critical for the development and future industrialization of Africa.

These include a continental railroad—you would have a rail net installed that would tie these countries together. Also developed would be a power grid, which does not even exist in many countries over there now. . . a communication network. . . fuel pipelines—so important, and new airports.

Rep. George Brown

(Continued from page 11)

nents, to secure proper funding for science, Brown took the bold step of putting forward an alternative concept and program for the entire federal budget, which was certainly not within the jurisdiction of the Science Committee. In October 1997, Brown announced an "investment budget alternative," with the purpose of increasing national investments in activities that would promote long-term economic growth.

"Public investments in R&D, transportation, and human resources, together with financial initiatives such as a targeted tax cut and a reduction of the debt, will strongly contribute to economic growth over the next decade," Brown stated. His purpose, Brown said, was to invest a portion of federal revenues "in Federal programs that have the potential to enhance long-term productivity and economic growth."

Brown's interest in economic growth was not only domestic. In 1991, he traveled to Mexico, to establish an arrangement whereby Mexican debt owed to the United States would be put into a fund to be matched by U.S. contributions, and used for joint research on problems of mutual interest, such as health and water.

A 'Remarkable Individual'

Learning of Rep. Brown's death, on July 16, NASA Administrator Dan Goldin described him as "a remarkable individual." He stated that Brown's "professional training in industrial physics and his love of science made his tenure as chairman of the House Science Committee one of the most productive in our nation's history."

With Brown's death coming just a few days before Apollo's anniversary, Goldin referred to the long-term vision the Congressman possessed, stating, "as we celebrate the 30th anniversary of the Moon landing on July 20, we should never forget that it was the vision and support of leaders like George Brown that made it all possible."

Along with the social and moral issues that he clearly believed in, Rep. Brown was, "the Congressmen who loved science." Great projects, such as Apollo, are the legacy George Brown leaves, as an example to be followed by government leaders in the future.

AIDS Pandemic Raging Worldwide

by Colin Lowry

The spread of the AIDS pandemic continues to devastate the world population. According to the United Nations AIDS program report, released in December 1998, there are now 33.4 million people infected with the human immunodeficiency virus (HIV) worldwide. The number of new infections in 1998 increased by 10 percent in just one year, with 5.8 million people *newly infected.* In 1998, 2.5 million people died from the disease, making AIDS one of the top four causes of death in the world.

The continent hardest hit by the AIDS epidemic is Africa, where 12 million people have died since the epidemic began. In 1998, about 70 percent of all new HIV infections in the world occurred in sub-Saharan Africa. Most devastating is that half of these were in young people between the ages of 15 and 25. The nine countries of southern Africa have the highest HIV rates in the world. In Botswana, Namibia, Swaziland, and Zimbabwe, between 20 percent and 26 percent of the adult population is infected with HIV. With infection rates at these levels, and climbing, these countries will lose almost an entire generation to the AIDS epidemic by 2010.

As a result of the AIDS epidemic, large numbers of children are becoming "AIDS orphans." The UNAIDS report forecast that by 2010, there will be 40 million orphaned children worldwide.

Figure 1 shows the pattern of HIV infection globally, in terms of the proportional increase in HIV in each country, between 1994 and 1997, as estimated by the World Health Organization. In addition to Africa, There are alarmingly high rates of infection in Russia, India, and other locations in Eastern Europe.

In the first eight months of 1999, Russian Health Minister Yuri Shevchenko said, the reported number of cases of HIV infection increased by 70 percent. He said that most of these cases were residents of the Moscow region, and that the unprecedented growth in HIV infection was the result of the virus hitting "the circle of the capital's drug users.

Although the official number of HIV cases reported was 15,800, as of July 1999, Russian officials have warned that the actual number of cases is at least 10 times higher than that.

Figure 2 shows the world pattern of HIV and tuberculosis co-infection rates

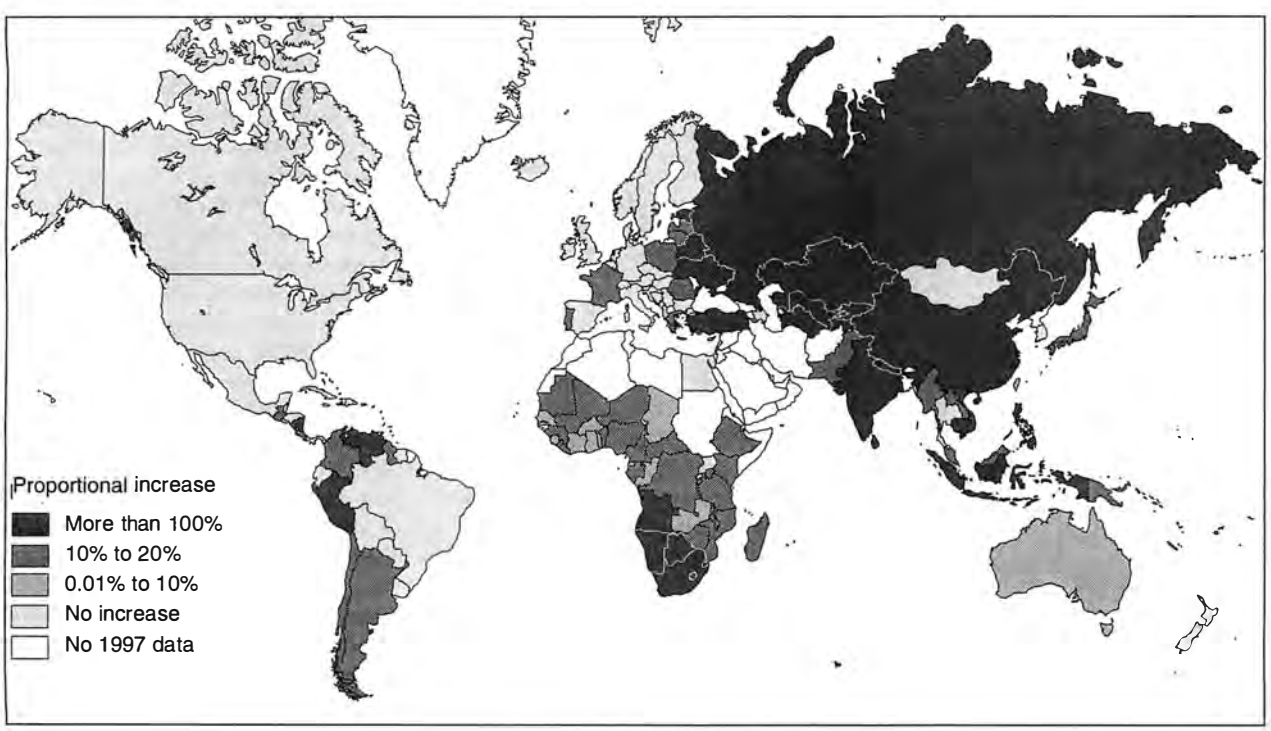


Figure 1
PROPORTIONAL INCREASE IN HIV PREVALENCE RATES, BY COUNTRY, BETWEEN 1994 AND 1997

Source: World Health Organization

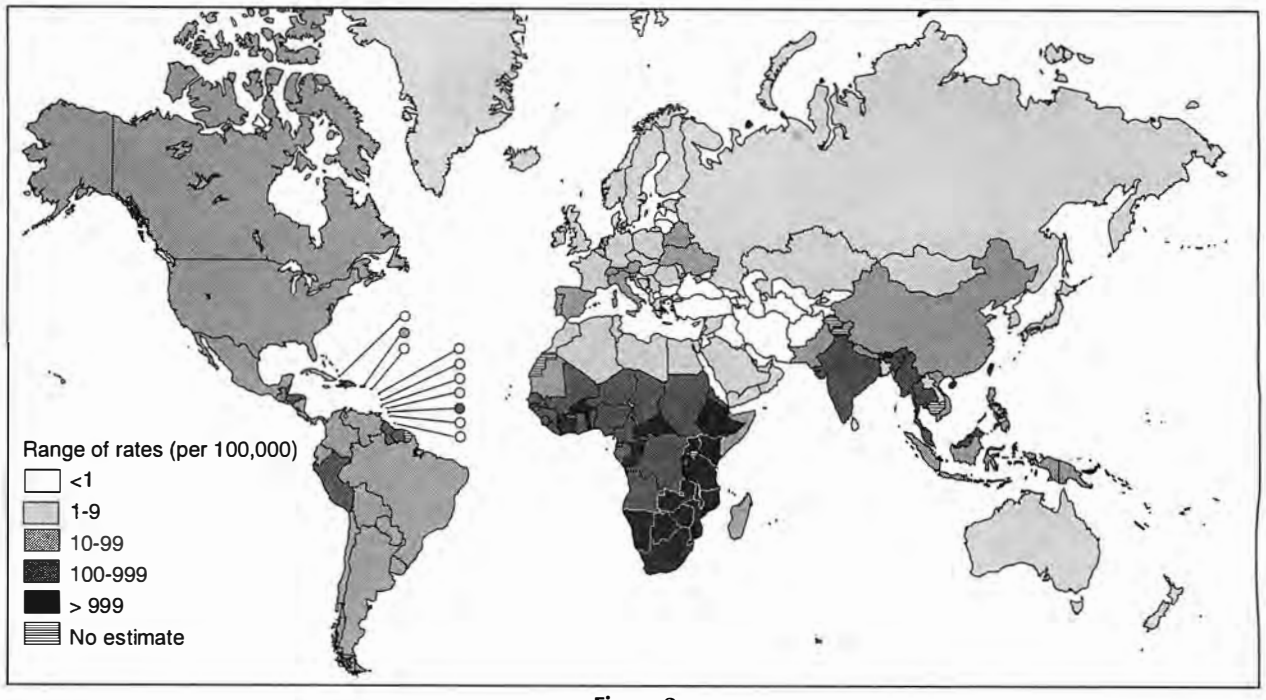


Figure 2
ESTIMATED CO-INFECTION RATES OF HIV AND TB, 1997

Source: World Health Organization

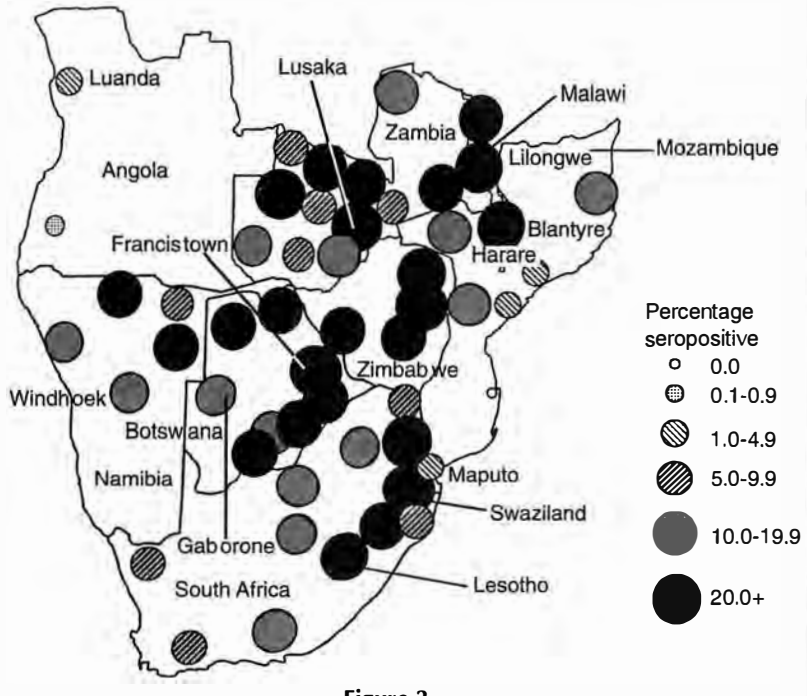


Figure 3
SEROPREVALENCE OF HIV-1 FOR LOW-RISK POPULATIONS IN SOUTHERN AFRICA

Source: U.S. Bureau of the Census

in 1997, as estimated by the World Health Organization.

On both maps, the continent of Africa stands out with the most extensive toll from HIV itself and from TB co-infection.

Figure 3 shows a map of southern Africa, with the percentage of seroprevalence of HIV-1 for low-risk populations (those people living in ways not considered dangerous or conducive to acquiring or transmitting AIDS) in various locations. In many places, more than 20 percent of the population is seropositive!

Here, AIDS is reversing any progress made in child mortality since the 1960s. In the coming decade, child mortality will triple with AIDS in many areas of sub-Saharan Africa.

At the 11th International Conference on AIDS in Africa, held in Lusaka, Zambia in September, the pandemic was termed a "global super-disaster." UNAIDS Executive Director Peter Piot said that the world community has failed to mobilize to fight the disease, and that half of all babies born in Africa are infected with HIV. There were 16,000 new HIV infections per day in 1998, Piot said, and more than 80 percent of the world's AIDS deaths have occurred in Africa.

AIDS VACCINE UPDATE

Catching HIV in the Act

by Colin Lowry

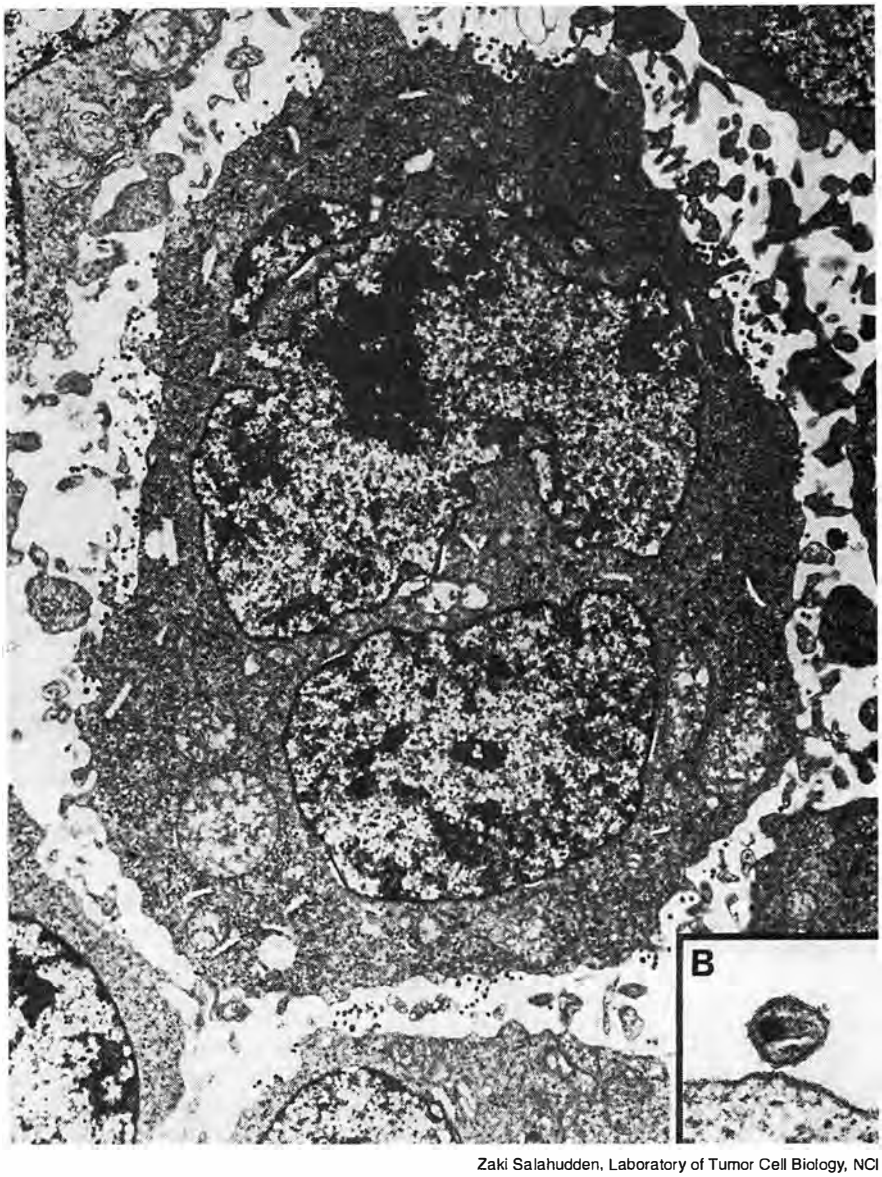
While the AIDS epidemic continues to spread out of control, efforts to develop a vaccine using traditional approaches have been stymied, leaving researchers at a crossroads.[1] The number of vaccines moving to clinical trials is at an all time low, and those in progress are not proving to be effective.

A clinical trial currently under way in Thailand, of Vaxgen's vaccine based only on the HIV coat protein gp120, was shown to be ineffective in stimulating antibodies that can neutralize HIV taken directly from patients (primary isolates). Because the gp120 vaccine was only partly effective in stimulating neutralizing antibodies against laboratory-bred viruses, there are serious doubts as to whether this clinical trial should proceed any further.

Two recent discoveries explain why the traditional vaccine approach has failed, thus opening a new strategy for vaccine development. Both discoveries have revealed the dynamic structure of HIV's coat protein in the process of binding to receptors on the cell surface, and have identified critical regions of the protein needed for gaining entrance into the cell. However, we are still a long way from clinical trials.

The first discovery was of the atomic structure of the coat protein gp120, complexed with the human CD4 cell surface receptor. This is the receptor to which HIV binds on immune cells, as a first step to infecting them. This structure was revealed in June 1998, through X-ray crystallography, and involved research groups from Columbia, Harvard, and Tulane universities. Their molecular "snapshot" showed that the binding region for the CD4 receptor was recessed in a deep cavity, out of sight of the immune system.

Once HIV binds to the CD4 receptor, this causes a shift in the conformation of the gp120 protein, which exposes the region that binds to the chemokine receptor, an event crucial to HIV's ability to fuse to the cell membrane. This two-stage process keeps the regions critical for gp120's binding and fusing to the cell membrane hidden from immune system surveillance until the last moment.



Zaki Salahudden, Laboratory of Tumor Cell Biology, NCI

A multinucleated leukocyte producing HIV particles. Large numbers of virus particles can be seen along the margin of the cell. Inset (bottom right) shows a mature virus particle, with the characteristic cylindrical core.

Using a traditional vaccine approach, gp120 protein would be presented in a static conformation to the immune system. This would create antibodies reactive to the protein, but these would not stop the virus from binding and fusing in

most cases, because none of the critical regions of the protein is exposed in the static conformation. Hence, the failure of the traditional vaccines so far to protect against infection.

From clinical studies, it is also well known that AIDS patients produce many antibodies to HIV gp120, but very few of these can actually neutralize the activity of the virus. The difficulty for the immune system is that its T-cells may be able to "see" the critical binding regions of gp120 only for as little as a few seconds, or minutes, before it is too late to stop HIV from entering a cell.

The key to designing an effective vaccine, would be to present gp120 in the active conformation it assumes during the two-stage binding and fusing process. How could this be done? A live virus vaccine of HIV has been ruled out, because of safety concerns that the vaccine could mutate and cause AIDS.

'Fusion-Competent' Vaccines

One of the major hurdles to developing an HIV vaccine using the coat protein gp120, is the fact that the outer regions of the protein mutate rapidly, and there are now six known clades (families) of HIV, with many different subfamilies and varieties, all with gp120 proteins of slightly different composition. This means that a vaccine made against one type of HIV may not be effective against another, if the gp120 varies between the two. However, from recent observations of the atomic structure, it is clear that the CD4 and chemokine binding regions are highly conserved among all HIV virus clades and subfamilies.

To overcome this problem, a team of researchers from Montana University and New York University Medical Center have devised a way to chemically freeze gp120 as it fuses to the cell surface. This complex of gp120 that is bound to CD4 and the chemokine receptor in the act of fusion to the cell membrane, was used as the basis for a new type of vaccine—a fusion-competent vaccine. To generate these complexes, one group of cells expressing gp120 was mixed with another group of cells containing human CD4 and chemokine receptor. The researchers then selected only those groups of cells that had fused their membranes together, and chemically "froze" these for use as a vaccine antigen.

These complexes were then injected as a vaccine into transgenic animals containing the human CD4 and chemokine receptor genes, which makes them susceptible to HIV infection. After vaccination, the animals were exposed to 24 different types of HIV primary isolates from 5 clades. Amazingly, the vaccine protected the animals from infection, stimulating the production of neutralizing antibodies against 23 of the 24 HIV types.

As a control, gp120 that did not fuse to cell membranes was used as a vaccine antigen, and the vaccinated animals were exposed to the same 24 types of HIV. This "static" gp120 vaccine failed to stimulate neutralizing antibodies, and did not protect the animals from HIV infection at all. This result proved the traditional "static" vaccine to be ineffective against HIV. The new, active presentation of gp120 in the fusion-competent vaccine design also appears to overcome the problem of having to make a different vaccine for every clade of HIV that exists in order for it to provide adequate protection.

The fusion-competent vaccine was made from only one clade of HIV, yet it protected against infection by 5 other clades; only 1 of the 24 types tested escaped detection by the immune system. This type of result has eluded all previous vaccine designs to date.

Superior Design Principle

There are still many technical problems that must be overcome before this type of vaccine could be used in human beings. However, these animal experiments have proven that the principle of the design is far superior to any other vaccine so far. Were the fusion-competent vaccines to be developed for human use, there would still be some questions as to its overall effectiveness, however. The problem *is that* these types of vaccines stimulate a neutralizing antibody response, which can defend only against viruses that are present in the blood or lymph system. HIV often enters the body by directly infecting immune cells, such as T-cells or dendritic cells found in mucous membranes.

To defend against this type of infection, the cellular branch of the immune system that recognizes infected cells must be activated. This cellular defense is activated when special T-cells ("killer T-cells") find viral proteins made by infected cells that are presented on a receptor on a membrane surface. One way to activate this cellular response would be to introduce the DNA for the viral coat protein into cells, so the protein would be produced and sent to the membrane like a flag telling the T-cells that the cell is infected. However, it is not known if the activation of cellular immunity is required for protection against HIV.

At the moment, the fusion-competent vaccine design may offer the best chance for developing an *effective* HIV vaccine. Much more research needs to be done before any new vaccines will be ready to test in clinical trials.

Notes

1. See *21st Century*, Spring 1998, "The Challenge of Developing an AIDS Vaccine," p. 76.

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INTERVIEW WITH SANDY THURMAN, WHITE HOUSE OFFICE OF NAT'L AIDS POLICY

We Need an AIDS Vaccine Now

After the United Nations AIDS report forecast in 1998 that there will be 40 million children orphaned by AIDS by 2010, President Clinton directed the Office of National AIDS Policy to lead a fact-finding tour of sub-Saharan Africa, to investigate the AIDS epidemic and the needs of orphans there. He also announced a $10 million relief fund to support AIDS orphans.

At the same time, in December 1998, the President introduced two initiatives aimed at increasing research into HIV vaccine development and prevention strategies. Sandy Thurman, Director of the Office of National AIDS Policy, commented, at a White House press conference in December, on the importance of developing an HIV vaccine: "Unless we find a vaccine to stop the spread of this disease, this epidemic stands to make the plague of the Middle Ages and the flu epidemic in the early part of this century absolutely pale in comparison to this pandemic."

After it carried out its fact-finding mission in Africa, the Office of National AIDS Policy released its report on AIDS in Africa July 19, 1999. Its recommendations called for increased support to prevention and treatment programs: specifically, $100 million for treatment and prevention, and another $10 million to support AIDS orphans in Africa. According to Thurman, the response to the report has been excellent.

Thurman testified before Congress this summer to explain the need for increased funding for AIDS programs in Africa, and said that the appropriations bill now under discussion has met with little Congressional resistance.

Sandy Thurman

Since the publication of the report, which is available at the Office of National AIDS Policy website (www.whitehouse.gov/ONAP), officials at the White House have been putting together a series of meetings with representatives from the business community, the pharmaceutical industry, and elected officials to discuss how to bring more forces into the fight against the AIDS epidemic.

What Is Required

Although it is good news that more activity and more funding is being marshalled to stop the AIDS epidemic, the programs suggested thus far fall far short of what is required to solve a problem of global proportions. Such a program was put forward throughout the early 1980s by the economist Lyndon H. LaRouche and his political organization.

In brief, LaRouche proposed a crash research program, modelled on the Apollo program, to find a cure and a vaccine, with a focus on optical biophysics research as essential for making breakthroughs in understanding how the virus works.

The LaRouche program included the construction of hospitals, clinics, and other medical infrastructure, and the application of standard public health and sanitation measures to stop the epidemic. In addition, LaRouche proposed universal testing to identify the infected, so that they could be treated.

In the case of Africa, the funds required to build adequate medical infrastructure would exceed $100 billion. Like any great project, the main ingredient to stopping AIDS is the political will required to put such a large-scale program into effect. Without that level of commitment, we will have a world disaster greater than that of the Plague in the Middle Ages.

—Colin M. Lowry

Sandy Thurman has been the director of the White House Office of National AIDS Policy since 1997. She has served on the Presidential Advisory Council on HIV/AIDS, and served as the executive director of AID Atlanta, a community-based, nonprofit organization that provides health and support services to people with HIV/AIDS. Before becoming the director of the Office of National AIDS Policy, she was the director of Advocacy Programs at the Task Force for Child Survival and Development at the Carter Center in Atlanta, Georgia. As director, she focussed on the global health concerns of children, including immunization programs and the eradication of polio.

Thurman was interviewed by Colin Lowry on May 25, and excerpts of her interview appear here. The full interview appeared in Executive Intelligence Review *magazine, July 2, 1999.*

Question: You have taken several trips to Africa as part of a fact-finding mission to investigate the AIDS epidemic. What were the directives for these trips, and what countries have you visited since December 1998?

The focus on Africa started in December on World AIDS day, when the President announced that he was setting aside $10 million to look specifically at the issue of children who are being orphaned as a result of AIDS. He did that in response to a USAID [U.S. Agency for International Development] report, which indicated that by the end of the decade, there will be more than 40 million children orphaned by AIDS worldwide, and more than 95 percent of these will be in sub-Saharan Africa. When you think about 40 million children, that is the equivalent of every child in the United States east of the Mississippi River.

At that time, the President directed me to undertake a fact-finding mission, leading a delegation to sub-Saharan Africa to look at programs that are working on the ground that we might be able to support, or expand, or replicate in some way. We are looking at what is working and what isn't, with respect to

children who are orphaned, and children at risk.

We did an initial site visit in February, and then took the Presidential delegation in March, visiting Zambia, Uganda, and South Africa. We chose those three countries because we thought that we would get the kind of contrast that we needed. Zambia has been doing some pretty exemplary work, but they have a huge AIDS crisis, with more than 20 percent of the adult population infected with HIV. Then we went to Uganda, to look at what a model prevention program looks like. Uganda is still standing as probably the most successful model that we have. In fact, they have been able to cut their HIV prevalence in almost half in 10 years time. Then, we went to South Africa, which has one of the fastest-growing epidemics in the world, and is in turmoil, with the changing of the government, and other factors. We chose to look at these three countries, to see if we could get a good sense of what was required, and what differences in leadership and support meant to the condition of the epidemic.

Question: Did this go beyond looking at just the orphan question?

Yes, we started, of course, looking at orphans, but you can't look at the impact of this epidemic on children without looking at the impact on women and families and communities as a whole. Our initial focus was on children, and how we could support programs for them. But, the fact of the matter is that in the initial report to the President, we focussed on the entire AIDS epidemic in sub-Saharan Africa, and the impact it is having there.

Question: Were you also looking at the medical infrastructure there, and what would be required to combat the epidemic?

Sure. The building of any kind of medical infrastructure in a developing nation is much bigger than just dealing with the AIDS epidemic. Obviously, the need for infrastructure is critical in those countries, which is why it's so important that we continue to invest not only in HIV prevention and care, but in health care development across the board. And, focus on expanding the amount of national budgets that are dedicated to health and social welfare. In many of these countries, they've been focussed on economic growth and trade, and spend only $10 per capita on health care for their populations. When you look at the cost of AIDS care, and, certainly, the cost of drugs, we know that these costs are much higher than what is allocated for health care.

Maggie Murray-Lee/UNICEF

An Apollo-style research program and massive health infrastructure development is required to stop the spread of AIDS. Here, a health center in Kolondieba, Mali.

Question: Will you be making specific recommendations to the President about the medical infrastructure required, and how the United States could be involved?

I made a preliminary report, and I am working on an expanded report that will be made public sometime in June, with more specific recommendations. But, in the preliminary report, we did not address the need for overall health care infrastructure development. The President is certainly sensitive to that, as he just went to Africa last year, and has developed a keen interest in Africa's development. We did address how the HIV/AIDS crisis fits into that larger context.

Question: The drug pricing question is a big issue. The best drugs to treat AIDS that are used in the West, such as the protease inhibitors, are just too expensive for the nations that need them the most. What kind of approach is your office taking to address this problem? Are you trying to work with the drug companies, to sell to developing nations at a reduced cost, or will the research agencies of the U.S. government act directly?

This is a multi-tiered issue. We are incredibly concerned, because more than 95 percent of the people around the world who are infected with HIV have no access to drugs at all. Certainly, there is a question on the pricing of drugs, and while I am delighted that some of the drug companies have dramatically reduced their prices to developing nations, the reduced prices are, in most instances, four times more expensive than the per-capita spending on health care in these countries. Even with the drug companies' efforts, there is a huge gap between what drugs cost and how we manage to get them to people who need them. I think part of this is looking at public-private partnerships, certainly working with the drug companies. But, I think that we are going to have to find some sort of balance, between the companies and the protection of their intellectual property rights, and what the needs of the people are. I think that the role of government is to help negotiate a balance.

Question: Could you describe the conditions on the ground in Africa? Do you think what you have seen is worse than what is portrayed by the UNAIDS report, or were you prepared for what you found?

I think it prepared us for what we saw there. But, there is no way the U.N. AIDS report, as overwhelming as the numbers are, can prepare you for the faces of people who are desperate and have no access to care. It reminds me, although the scale is much larger, of the early epidemic here. I first started working in this epidemic in 1983, and in those days we had nothing to give to people. All we could provide was palliative care, and support to people who were sick, and their families. So, going back to Africa reminds me of those times in *the* early 1980s; the scale is just 10 times larger, and it's really overwhelming to see.

On the other hand, what the report doesn't reflect is the reasons for hope, and the incredible work that is being done on the ground. People who have nothing are helping people who have even less. The community-based organizations, the women's groups, the peer-education groups that we visited on the ground there were really extraordinary. Those are the kinds of programs we need to focus on, and see if we can find ways to sustain them in an active way.

Question: Is the role of the United States now being discussed in detail?

We haven't detailed this yet. We are looking specifically at the HIV issue now. Obviously, in the broader context, health care infrastructure development is a huge part of this in all of these countries. We found that out working in immunization in international health for a long time. This isn't new; we fought these same battles when we were doing immunization, and polio eradication, and diarrheal disease, and we are facing all of the same things. If these countries are going to remain healthy, we have to understand the connection between health and economic well-being. You can't separate the two. Then again, I think we focus too much on the trade and investment side, and not enough on the health care and social services side, when we are looking at investing in these countries.

Cover page of the report, issued in July 1999, documenting the AIDS emergency in Africa.

Question: Regarding the orphans: How did the funding the President talked about work?

It's already been given out. We spent $7 million of the $10 million in sub-Saharan Africa. It was allocated from U.S. AID [Aid for International Development] directly to community-based organizations in Africa. I think that what's important to remember is that in Africa and the United States, the battle against HIV/AIDS will be won or lost at the community level. That's true in terms of prevention and in terms of care, particularly in Africa, where communities still have a strong family network. The old saying, that it takes a village to raise a child, is really true in Africa. The majority of the children being orphaned in Africa are being taken in by their extended families or by the community. But, now, the communities and families are reaching capacity, and we do see increasing numbers of children who are abandoned or living in orphanages.

India the Next Epicenter

Question: Late last year you went to India. Could you give an idea of what the situation is there in regard to the AIDS epidemic, and what they are discussing to stop the spread of the disease? The subcontinent could be the next epicenter of the AIDS epidemic.

I think it will be the next epicenter of the epidemic. Those who are in the know, think it will be, and the number of infections there in raw terms will exceed those in Africa, if we don't do something to stop the spread pretty quickly. I'm not sure that the percentage rate of infection will ever be quite as high as it is in Africa, but the number will be larger. The World Bank has invested $200 million in the AIDS program in India, to try and get a handle on the epidemic.

The Indian government has been engaged for several years in the development of an AIDS control plan, which is part of their agreement with the World Bank. They are working very closely with community-based organizations. I think India presents unique challenges. The vast majority of people live in rural areas in India, and are hard to reach. India is a very complex society, with all kinds of cultural challenges to dealing with this epidemic. Not that we haven't met those everywhere else; we've seen these in Africa and the United States, and this isn't new, it's just very different. We have to really be specialized with our approaches in India. There is still a lot of denial, there is still denial in the United States in some of the communities hardest hit by the epidemic.

Question: What are they trying to implement in India?

Education programs, and prevention programs. They are focussing at this point in time on prevention.

Question: The study in Tamil Nadu, documented in the UNAIDS report, shows that the rural population has a higher HIV prevalence than the urban population. This really doesn't fit the classic models of the spread of HIV in the United States. How do they explain this?

I don't think we know, and that's the challenge, why the epidemic has flipped profile there. The frightening thing about that statistic is that the vast majority of

the almost 1 billion people in India live in rural areas. So, if we see some trends indicating that the epidemic is going to be worse in rural areas, than in urban areas in India, we are really in trouble. We need to pay very close attention to that, and try to define why that is occurring. Again, that's where prevention and education is key. They have some good networks in place that they have used for maternal and child health, and for immunization, that will be helpful for HIV prevention as well.

Question: Has insect transmission been investigated there, considering that this is an area that is endemic for malaria?

Not that I know of. I think we ruled out insect transmission years ago. It hasn't come up in recent years. We have no evidence at all that HIV is transmitted in that way, so we are not focussing much attention on that anymore. People still ask about it though.

Question: The reason I ask is because of the Belle Glade, Florida episode in the mid-1980s. Drs. Whiteside and McLeod who investigated there, found that their data point strongly to insect transmission.

Again, I think we have to never rule anything out, as we are dealing with an epidemic that is fairly new. And so, we should be vigilant, and pay attention to all the facets of the epidemic.

Question: The infection rates in the last five years have been relatively stable, with 40,000 to 60,000 new cases added each year. I think people are taking a false sense of security from this, and seeing this as progress. What do you think about this? And, considering the lack of broad-based testing, what do you think the real figures might be?

Well, it's not progress. I don't think we know what the actual number of infections is. I think the 40,000 to 60,000 is a very educated guess, and I would be willing to bet on that. But, we certainly can't call that success. We have invested a lot in prevention, and we don't see a reduction in new infections. We see exploding epidemics in particular communities in the United States. What we see is a real shift in the epidemic to women, people of color, and to young people.

More than 50 percent of all new cases in this country are in people under 24 years of age. One in four of those is a teenager. So, it tells us that we aren't getting to young people early enough with the right information. I think we need to rethink our prevention effort, and we are in the process of doing that. We are working very closely with CDC [Centers for Disease Control], and need to look at where the epidemic is moving, and make sure that our prevention messages are appropriate for those communities. Your message to a 14-year-old in Harlem is going to be different than for someone who is Latino in East Los Angeles.

"We are currently wiping out every single development gain that we have made in the last two or three decades in Africa. In the next five years, we will see infant mortality double, as a result of this disease. We will see child mortality triple, and we will see life expectancies drop as much as 20 years."

I worry about that in this stage of the epidemic, when people are tired, battle-weary from 18 years of fighting this epidemic, when there is the misperception in a lot of the public that this epidemic is under control or over. They read the great headlines that we have dropping rates of AIDS deaths, but we don't have any decline in the rate of new infections at all. We are not winning the battle. It's great that we are staying level, but those rates need to be going down, not leveling off. We have to focus on making sure we are staying current, and retooling to keep pace with the epidemic. That is a challenge.

Question: Will you be working on programs to increase testing, now that the treatments are better? In the past, people would be very pessimistic, because there was nothing that they could do. Now, is this further incentive to try to increase testing for HIV?

Absolutely. CDC and community-based organizations are all encouraging people now to get tested. In fact, the President's council on HIV and AIDS has recommended to us and the President, to undertake a "get tested" campaign, and we are in the process of working with CDC on the development of that campaign. The bottom line of this is that we have to focus on prevention, again, since we know how to prevent the spread of HIV. We need to educate people to take responsibility for their own actions, and not make assumptions of others.

Question: We don't have a vaccine for HIV yet, but you have spoken about the importance of this. Hypothetically, if we did have a vaccine, what kind of strategy would you develop to use it, especially in the areas that need it the most, such as Africa and Asia?

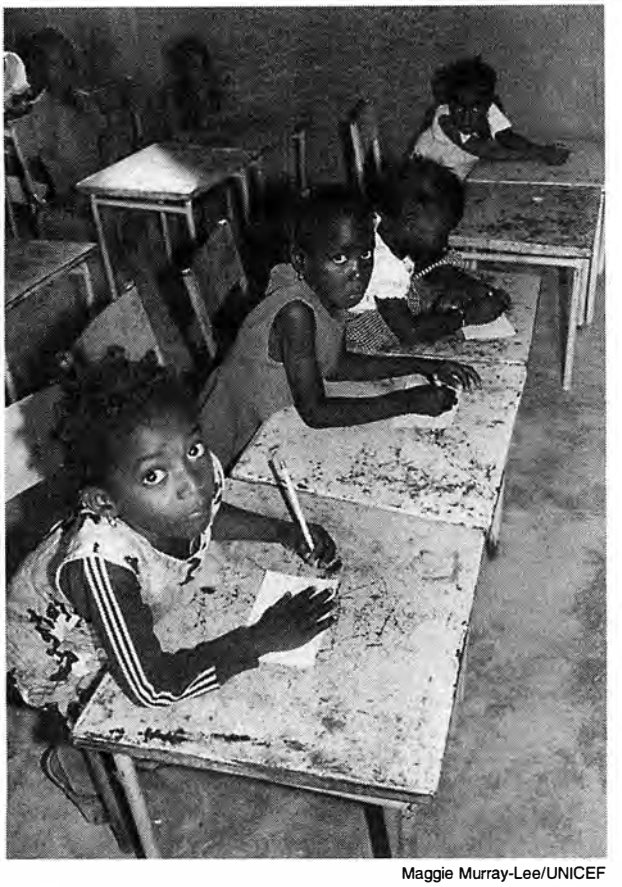
I think we're putting the cart in front of the horse, because we don't know what kind of vaccine we'll have. Our big challenge is to find a vaccine or vaccines, that are both cost-effective and easy to administer, so that we can get them out to the places that need them. Certainly, even if we had a vaccine today, that was both cost-effective and easy to administer, it would take us years, probably our lifetime, to stop the spread of this epidemic.

We have a perfect example in polio. We have had an effective vaccine against polio for 40 years, yet we still haven't eradicated it from the Earth. In fact, it's coming back. Although we have eradicated polio from the Western Hemisphere, we still spend $235 million a year in the United States immunizing our children against polio, and we will have to always do that until polio is eradicated from the face of the Earth. So, we have to understand, that even if we had a vaccine today, we are going to be dealing with this epidemic for the foreseeable future.

Question: In the case of polio, what has happened is that you have had a complete breakdown of medical infrastructure in the areas where it is returning, such as in the states in the trans-Caucasus and in other states of the former Soviet Union. What do you think about not falling into that same trap with HIV? What policies are the U.S. government and the international aid agencies going to have to shift to deal with this?

Well, I think that building up your infrastructure, not only where it never existed, but also where it is falling apart. When we look at the whole area of emerging infectious diseases, it's really important to focus on health care infrastructure. It keeps coming back to the same thing. It doesn't matter how good our drugs are or how cheap they are. If we can't get them to people who need them, they're really not going to do us any good.

People have a tendency, now that we have drugs available, to want to go buy drugs for everybody in the developing world. Even if we had them available, the challenge is that we can't get them to people who need them, we can't give them the care, and provide the kind of support. In doing that, if people don't take the drugs appropriately, and we have no way to monitor them, the bottom line is we create a worse problem than we have now, with drug-resistance problems.



Maggie Murray-Lee/UNICEF

By the year 2000, in nine sub-Saharan countries, one-fifth to one-third of all children under the age of 15 will be orphaned by AIDS. Here, a village pre-school class in Mali.

Question: Which we are seeing with drug-resistant tuberculosis in Africa and the former socialist bloc.

Sure, it's the same thing. All we are doing is adding one more awful thing onto the already awful situation we are seeing when we look at this epidemic. It just points out the weaknesses, or exacerbates the weaknesses, in an already weak health care system.

Question: Since you have been on the ground in Africa, what do you think the consequences will be for Africa and the world, if the current increases in HIV infections are not stopped?

The effect is devastating. It's absolutely devastating. We are currently wiping out every single development gain that we have made in the last two or three decades in Africa. In the next five years, we will see infant mortality double, as a result of this disease. We will see child mortality triple, and we will see life expectancies in the majority of sub-Saharan African countries drop as much as 20 years. In South Africa, in the next five years the life expectancy will drop from 60 to 40. In Zimbabwe, it's 65 to 39.

"We have to continue to try to put the epidemic in this country in the broader context of the global epidemic, and help both the public and the policymakers understand the importance of a U.S. leadership role in the fight against this epidemic worldwide."

In many of these countries, you have one in five adults infected with HIV. That's one in every five people you see walking down the street. Well, you can't sustain a healthy economy when you are carrying that burden of disease, and when you are losing your most productive citizens in the prime of their life—when they should be producing and purchasing to keep the economy going.

Question: They are also raising children. . .

And they are raising children. So, we have to look at not just the health implications, sort of the human cost, but we need to look at the economic cost. We need to look at the effect on the stability of these nations.

The countries currently involved in the conflict in the Congo, the seven armies involved there—it's estimated that the rate of infection in the military personnel is anywhere from 50 percent to 80 percent. Fifty percent for the Angolans, and more than 80 percent for the Zimbabweans. And that is scary. So, the implications are enormous, and if we don't learn something by our experience in the United States and in Africa, and our shared experience, that we can share with our friends in India, they're going to end up in the exact same boat.

So, there is pressure on us, to look at what we can do to turn this around. Then, after India, comes the newly independent states of the former Soviet Union; they've got a burgeoning epidemic there, and they are right behind India. So, we've got a lot of work to do.

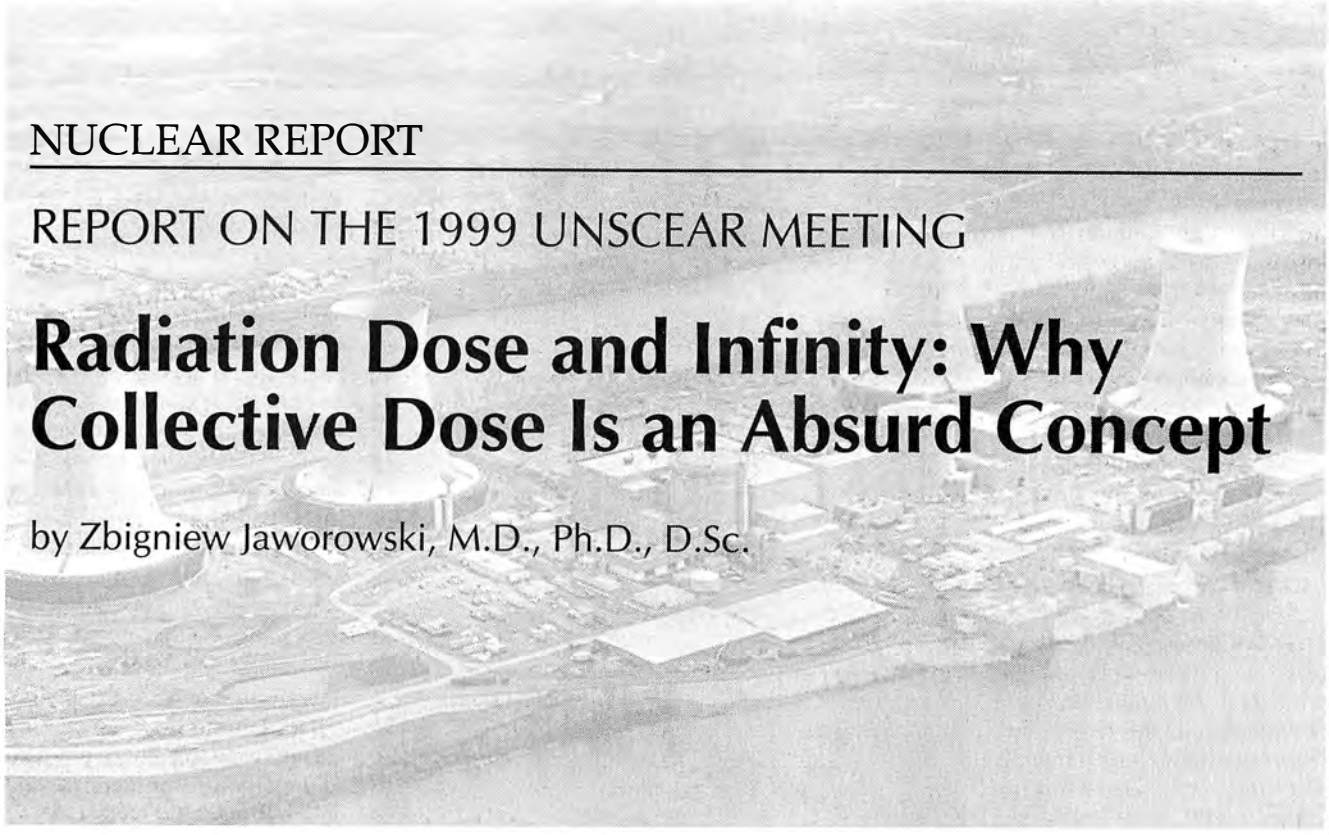
Question: What's next for you and your office?

We have to continue to try to put the epidemic in this country in the broader context of the global epidemic, and help both the public and the policymakers understand the importance of a U.S. leadership role in the fight against this epidemic worldwide. Historically, where we have led, other donors have followed. We have seen a leveling off in international support for the fight against AIDS. I think that's a bad indicator. So, our challenge here is to make people pay attention to what's happening in Africa, and help them understand what kind of implication that has—not only for us in America, but for the rest of the world as well. It's a tall order.

REPORT ON THE 1999 UNSCEAR MEETING

Radiation Dose and Infinity: Why Collective Dose Is an Absurd Concept

by Zbigniew Jaworowski, M.D., Ph.D., D.Sc.



Metropolitan Edison Co.

The United Nations Scientific Committee on the Effects of Atomic Radiation (UNSCEAR) was established by the General Assembly in 1955. The original task of UNSCEAR was to advise the General Assembly on the effects of nuclear test explosions, which had just entered their intensive phase. During the next four decades UNSCEAR studied also the effects of ionizing radiation from many other man-made and natural sources.

During the 1999 session of UNSCEAR, held in April in Vienna, Austria, UNSCEAR reviewed and corrected 11 documents prepared by its secretariat, including an epidemiological evaluation of radiation-induced cancers, DNA repair, and mutagenesis and the hereditary effects of radiation. These documents will be included in the comprehensive *UNSCEAR 2000 Report.*

One of the important chapters of this report will be a review of the local and regional effects of the Chernobyl accident. The general conclusion of the committee on these effects will be probably as in its draft documents: "Apart from the substantial increase in thyroid cancer after children's exposure [probably a screening effect—ZJ], there is no evidence of a major public health impact to date from the radiation exposure caused by the Chernobyl accident in the three most affected countries. No major increase in all cancer incidence or mortality has been observed that could be attributed to the accident."

The linear no-threshold hypothesis (LNT) was the subject of discussion during the 1999 session, as it has been each year since the 1994 publication of the "revolutionary" UNSCEAR report in which the evidence on radiation hormesis (beneficial effects of low radiation doses) was presented. This document intensified a worldwide discussion on LNT, which may change the theoretical basis of radiological protection, and its practical standards.

Integrating Out to Infinity

Among other subjects related to LNT, the Committee discussed the problem of collective dose and dose commitment. Offsprings of the linear no-threshold assumption, these concepts were introduced in the early 1960s. At the time, they reflected the main concern of induction of hereditary effects by nuclear test fallout. After almost four decades, collective dose and dose commitment are still widely used, although both the concepts and the concern should by now have faded into oblivion.

UNSCEAR first used the concept of "dose commitment" in 1962, and defined it as: "the integral over infinite time of the average dose rate in a given tissue for the world population, as a result of a given practice, e.g. a given series of nuclear explosions." Such integration requires some daring assumptions, or a superhuman omniscience on the population dynamics and environmental changes for all the eons of time to come.

In a more humble mood, UNSCEAR later introduced a "truncated" dose commitment, limited at will to 50, 500, 10,000, or many millions of years. However, the original "infinite" definition is still retained in the recent UNSCEAR documents.

The Absurd Assumptions

The collective dose commitment was first used by UNSCEAR in its 1972 report. In order to accept the definitions of dose commitment and of collective dose, one has to accept the following: (1) a linear no-threshold relationship between absorbed dose and risk to an individual; (2) additivity of risk (via additivity of dose) over the lifetime of an individual; (3) additivity of risk (dose) across individuals of the same generation; (4) additivity of risk (dose) across the lifetimes of individuals over any number of generations; (5) ex-

Zbigniew Jaworowski is a professor at the Central Laboratory for Radiological Protection in Warsaw, and a leading expert worldwide on radiation effects. A multidisciplinary scientist, he has studied pollution with radionuclides and heavy metals, and he has served as chairman of UNSCEAR.

pectation that late harm resulting from a dose accumulated over many years, or over generations (dose commitment) be the same as that after an instantaneous dose of the same magnitude; and finally, (6) expectation that late harm resulting from a given value of collective dose, or dose commitment, calculated for a large number of people who are exposed to trifling doses, be the same as that calculated for a small number of people exposed to large doses. (This opposes the common practice that holds that noxious agents can be diluted or dispersed so that they are below dangerous levels.)

In 1969, UNSCEAR advised that natural radiation should be a convenient reference for comparing dose commitments from man-made sources. However, over the three decades since it introduced the dose commitment concept, UNSCEAR has not followed its own advice. The collective dose commitment from natural sources for the world population, truncated to only 50 years (650,000,000 man Sv), was published for the first time in the 1993 *UNSCEAR Report*.

But why should we stop at 50 years, when for man-made radiation UNSCEAR estimates the dose commitments *over infinite time?* It is easy to calculate the individual dose commitment from the past exposures to natural radiation, for periods comparable to those used for man-made sources of radiation. For this calculation, one may assume that during the past several million years, the natural radiation dose rate was the same as is now, that is, 2.2 mSv per year.

Table 1 presents the values of individual truncated natural dose commitment for various periods since the appearance of some of our ancestors. One may compose a similar table for the collective truncated dose commitments for the global populations, integrated over the past generations, also given in the table. One may also calculate the future natural dose commitments of our descendants for tens or thousands of generations.

Each of us is burdened with these values of dose commitment. Do these values represent anything real, or are they just an academic fantasy? What are the medical effects of these enormously high doses?

Collective dose to the world population from nuclear dumping operations in the Kara Sea, truncated to the year 3000 A.D. has been estimated by an international study to be about 10 manSv (K. Sjöblom and G. Linsley, *IAEA Bulletin,* Vol. 40, No. 4, pp. 18-20, 1998). Let us explore the meaning of this value, which may be equivalent to 10 Sv for 1 person in 1 day (a lethal acute effect); or 10 Sv for 1 person in 1 year (chronic effect, for example, cancer); or 0.5 Sv for 20 persons in 1 day (chronic effect); or 0.00001 Sv in 1,000 persons in 1,000 years (no biological or medical concern); or 0.0000000000002 Sv per each of 5×10^9 persons now living and their descendants from 33 generations in 1,000 years (no concern).

Obviously, collective dose obliterates information of the patterns of dose deposition in space and in time, which are of major importance for estimating their biological effects, in terms of risk to humans. Individual doses cannot be additive over generations, simply because humans are mortal, therefore dose "dies" with an individual.

Similarly, individual doses cannot be added between individuals of the same generation, because we do not "contaminate" each other with a dose which we have absorbed. The presence of biological repair processes and the multistage process of cancer induction make linear addition of small contributions of individual dose, to estimate the associated risk of cancer occurrence, a highly unlikely procedure. Collective dose and dose commitment, therefore, cannot have any biological meaning.

The often-published large values of collective doses and collective dose commitments were derived from minuscule individual doses. For example, UNSCEAR calculated 100,000 man Sv from nuclear explosions during the past 54 years, 205,000 man Sv to the global population in the next 10,000 years from power reactors and reprocessing plants, 600,000 man Sv from Chernobyl fallout in the Northern Hemisphere till infinity, and 650,000,000 man Sv of natural radiation for the world population over 50 years.

Frightening—but Not Real

These large values, frightening as they are to the general public, do not imply that individuals or populations are harmfully burdened by nuclear explosions, nuclear power plants, Chernobyl fallout, or Nature. In fact, they provide the society with no relevant biological or medical information. Instead, a false image of the imminent danger of radiation, with all its actual negative social and psychosomatic consequences, is created.

If harm to an individual is trivial, then the total harm to members of his or her society over all past or future time must also be trivial, irrespective of how many people are exposed to natural or man-made radiation. It is hoped that in its future work, UNSCEAR might decide to apply Occam's razor to the collective dose and dose commitment concepts.

What Is UNSCEAR?

UNSCEAR is composed of the following member states: Argentina, Australia, Belgium, Brazil, Canada, China, Egypt, France, Germany, India, Indonesia, Japan, Mexico, Peru, Poland, Russia, Slovakia, Sweden, Sudan, the United Kingdom, and the United States.

The meetings of the Committee are usually attended by about 100 participants, including national representatives and their advisers, consultants, and observers. The impartial scientific reviews issued every few years by UNSCEAR maintained a very high scientific standard and became bibles for the students and legislators in the radiation field. UNSCEAR reports directly to the General Assembly—the only committee placed so

(Continued on page 25)

VALUES OF INDIVIDUAL TRUNCATED NATURAL DOSE COMMITMENT

Time since the appearance of	Years	Number of generations	Dose commitment (Sv)
Early modern *Homo sapiens*	130,000	4,300	286
Archaic *Homo sapiens*	400,000	13,300	880
Homo erectus	1,800,000	60,000	3,960
Homo habilis	2,400,000	80,000	5,280

Source for years: *The Cambridge Encyclopaedia of Human Evolution* (Cambridge: Cambridge University Press, 1994)

Food Irradiation: A Public Health Measure Long Overdue!

by James H. Steele, D.V.M., M.P.H.

Public health scientists have had an interest in food irradiation for more than 100 years. The first investigations occurred within a few years of the discovery of X-ray/short wavelength by the German physicist Roentgen in 1895. German and French scientists carried on studies to pasteurize food by radiation up to the war years, 1914. The problem at that time was that irradiated foods had an unacceptable taste. In 1915, the X-ray was reported to be effective in killing trichina cysts in pork meat. Later, the U.S. Department of Agriculture demonstrated that X-rays/short wavelengths of energy, could kill disease-causing organisms and halt food spoilage.

As food irradiation pioneer Dr. Edward Josephson pointed out in a recent review, food irradiation was the first entirely new method of preserving food since the thermal canning and pasteurization of wine, beer, and milk in the 19th century.[1] These methods of food preservation were all considered to be processes, but in 1958, under pressure from protesters, the Food, Drug, and Cosmetic Act designated food irradiation as an "additive." Scientific research has never found evidence to call radiation an "additive" that remained in food; this unfair designation has been used to keep the technology from being used.

In the United States, scientists at the Massachusetts Institute of Technology instituted the first studies of food irradiation in 1899. During the first half of the 20th century, many more studies were undertaken to learn how ionizing radiation could be used to provide more and safer foods to humanity on a worldwide basis. However, the paucity of suitable radiation sources and their high cost prevented the full benefits of irradiation from being realized for use in food and biomedical research.

Since 1950, many beneficial effects of ionizing radiation have been observed, and documented. In addition to its potential to reduce the incidence of food-borne diseases, food irradiation can inhibit post-harvest sprouting in potatoes and onions; disinfest fruits, vegetables, and grains of insects; delay ripening in fruits; eliminate pathogens, using sub-sterilization doses in meat, seafood, fruits, poultry, fruit juices, and vegetables; and, with sterilization doses, produce an array of prepackaged meats, poultry, and seafood, which can keep for years without refrigeration. In addition, irradiation can be used to eliminate pests such as the screw worm fly, which preys on cattle, the Mediterranean fruit fly, and the tsetse fly, by the release of sterile insects.[2]

Worries about nuclear weapons, combined with an anti-progress ideology, began to stymie food irradiation research after the war. Although there was now an adequate supply of gamma rays—the high-energy, short-wavelength rays given off by radionuclides—lawmakers became convinced by the anti-technology faction to control the development of nuclear technology for treating foods.

In 1958, when the Food, Drug, and Cosmetic Act was passed by the U.S. Congress, there were many unanswered questions: Would food be made radioactive? What would be the effect of this additional radioactivity above that of background upon humans? Would there be new toxic products formed in the irradiated foods? Would carcinogens be formed? Would there be excessive loss of nutrients? Would molecular fragments from packaging materials migrate

Philip Ulanowsky

Food irradiation eliminates 99.5 percent of all Salmonella, Listeria, and Campylobacter pathogens in chicken—without affecting freshness, taste, smell, texture, or wholesomeness.

onto the foods in amounts derogatory to the health of consumers? In the killing of pathogens, would new microbiological problems evolve? What radiation doses would be safe to use? What effect would radiation have on taste, odor, color, texture of the food?

Also, what adverse effect, if any, would result to the environment should there be accidents? What sources of radiation (gamma and machine) and what doses would be suitable for irradiation?

The U.S. Congress—with successful lobbying by well-known public figures in the movie and entertainment circles—convinced Congress to keep food irradiation under tight control. To do this, a legal fiction was created that ionizing radiation used to treat food is a "food additive." This part of the 1958 law, known as the Delaney Clause, assured that no irradiated food could be approved for consumption without a lengthy drawn-out procedure, thereby singling out and stigmatizing foods so treated, by requiring a long period for research and petition writing to the Food and Drug Administration (FDA) and the U.S. Department of Agriculture, and then many months or years for evaluation.[3]

Finding the Answers

After 1961-1962, when Ed Josephson was placed in charge of the Department of Defense's food radiation research and development program, the top priority was to try to sort out the diverse claims—pro and con—about irradiated foods. During his tenure as head of the program, the U.S. Army Medical Services completed studies for testing in rats, mice, and beagle dogs, using 21 foods representing all major food classes in the diets of Americans. In a June 1965 hearing by the Joint Committee on Atomic Energy, the Army Surgeon General submitted a statement that all foods irradiated at sterilizing doses up to 5.6 Mrad (56 kGy) using cobalt-60, or electrons at energies below 10 MeV, were wholesome—that is, safe to eat and nutritionally adequate.[4]

Nutritional assessments showed that the irradiation process was no more destructive to nutrients than other processes then being used commercially. It was also demonstrated that there were no toxic products formed in quantities that would be hazardous to the health and well-being of consumers.

The microbiological standard for irradiation-sterilized foods was to use a radiation dose sufficient to reduce a theoretical population of spores of *Clostridium botulinum* by 12 logs. This standard, recommended by the National Academy of Sciences/National Research Council Advisory Committee to the Army's program on food irradiation, was adopted. In the ensuing years, there was no record of any problem with possible *C. botulinum* survivors; although this has continued to be one of the anti-nuclear arguments against food irradiation. (See accompanying article, p. 28.)

Thousands of irradiated components of meals have been served to volunteers. In every respect, the irradiated foods have come through with flying colors. Irradiated foods have been eaten by astronauts on the Moon flight, and on many other space missions, by immune-compromised patients, and by military personnel in several parts of the world.

World Health Groups for Irradiation

Every conceivable possibility for harm has been carefully considered. None was found. Nor have any chemicals formed that are unique to food irradiation. In the meantime, irradiated foods have been approved by the health authorities in 40 countries.

Between 1964 and 1997, the World Health Organization (WHO), in concert with the Food and Agricultural Organization (FAO), and the International Atomic Energy Agency (IAEA), held a series of meetings of experts from many countries to assess the quality and safety of foods.[5] The latest meeting, in September 1997, recommended approval of irradiated foods without restrictions at all doses, up to the highest dose compatible with organoleptic properties. At each meeting, the internationally recognized health authorities have concluded that all irradiated foods are safe to eat without the need for further toxicological testing, at doses as high as taste would be acceptable.

In view of the foregoing, food scientists believe that the FDA and the USDA should follow the WHO/FAO/IAEA recommendation that food irradiation is a process.

Scientists have thought for three decades that the legal fiction designating ionizing radiation as a food additive, instead of a food process, unjustly penalized food irradiation, and helped

Radiation Dose

(Continued from page 23)

highly in the United Nations family—and is probably the best international scientific committee in the world.

The 47th session of UNSCEAR, in 1998, discussed the problem of the very existence of the committee. There are forces in the U.N. General Assembly who wish to dissolve the committee, including one "major nation" that is pushing for its dissolution. If UNSCEAR disappears, what nations will have enough scientific authority, and courage, to effectively oppose the opinions coined in less politically independent bodies, such as the self-perpetuating International Commission on Radiation Protection (ICRP), or the U.S. National Committee on Radiation Protection (NCRP)? In a world still full of nuclear weapons, and still undecided what to do with nuclear energy—a cheap and safe source of heat and electricity for millennia to come—radiation and radioactivity are obviously a hot political issue.

Fortunately, it seems that at least for the time being, the General Assembly dispelled this menacing cloud. By its resolution of Dec. 3, 1998, the Assembly decided to maintain the present functions and independent status of UNSCEAR, and endorsed the plans for its future activities.

delay its implementation for almost 30 years. On the other hand, during these years, the additive designation has stimulated those working in the field to perform at the highest level of good science, thus convincing the scientific community worldwide that food irradiation has an important role to play to combat hunger and disease.

When we look at the big picture, we find that we have essentially reached our objective in documenting that food irradiation is a safe and beneficial process. Now we need to "educate" government officials, as well as health workers, food processors, marketers, and the public, on the safety and advantages of food irradiation.

IAEA

Irradiation delays spoilage, without affecting taste or nutrition. Irradiated strawberries show no evidence of mold after being stored for 15 days, while the nonirradiated berries (left) are moldy. Wherever irradiated berries have gone on sale, consumers have preferred them, even if they cost a little more.

The Pasteurization Example

With approximately 9,000 people dying annually in the United States from food poisoning, and an estimated 30,000,000 cases of food infection each year, the time has come to use food irradiation more widely for the benefit of mankind.

Today in the application of ionizing radiation to protect the public health against foodborne pathogenic bacteria, public health officers face the same arguments that were voiced against pasteurization at the beginning of the century, and later against canned and frozen foods. In the history of pasteurization, many voiced disbeliefs of pasteurization's benefits for sanitation, nutrition, physical and bacteriological quality, public health and safety, and economics. Loss of hair, skin tone, and general well-being, as well as potency, were also alluded to. All of these mistaken beliefs are cited today against the irradiation of food.

Food irradiation is now recognized as another method of preserving food and ensuring its wholesomeness by sterilization or cold pasteurization, and has wide application worldwide. If it had been in place in the United States, recent foodborne disease outbreaks caused by *E. coli*0157:H7, which are found in food-producing animals, would not have occurred. If one attempts to tabulate tens of thousands of *Salmonella, Campylobacter, Yersinia, Listeria, and Escherichia coli* foodborne disease outbreaks related to poultry and meat, the totals exceed millions of human illnesses, over the course of the more than 40 years since the Delaney Clause established the travesty that gamma rays are a food additive. (Fortunately, the Congress did not redefine the entire electromagnetic spectrum, which encompasses all kinds of rays and waves.)

The Death Toll

How many thousands of deaths and illnesses could have been prevented if public health authorities had implemented food irradiation and educated the public as to its benefits, we will never know.

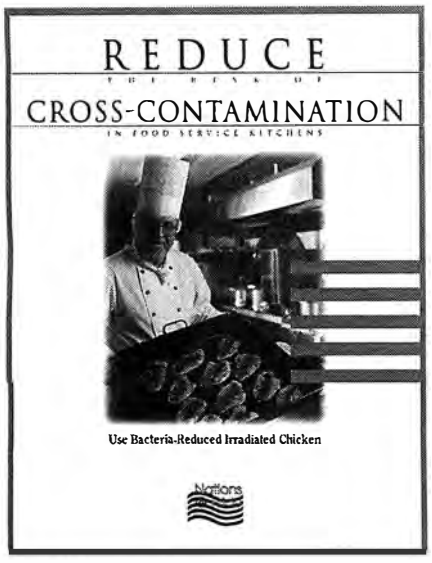
The morbidity and medical expense of meat- and poultry-borne diseases can be prevented, just as milk-borne disease can be prevented by pasteurization. All of the bacteria cited above can be present in unpasteurized milk, even though the U.S. Public Health Service Grade A standards require that milk be free of disease-causing organisms. Imagine the public outcry if the governments allowed the marketing of unpasteurized milk in which *Salmonella* were found, or *E. coli* virulent strains, or *Listeria* in soft cheese or Mexican-style cheese.

In 1984, the Secretary of Health, Margaret Heckler, endorsed food irradiation, after lengthy studies had proven its safety. If public health officers had spoken out then for the irradiation of foods that are known to carry pathogenic bacteria, events like the *E. coli*0157:H7 outbreaks from undercooked hamburger (3 deaths and more than 400 cases of illness) that occurred in the northwest United States in January 1993, could have been prevented.

Even today, no national or state local health authority is speaking out to require

Food Preservation Advances Increase Human Longevity

"The advancement of food preservation hygiene since the time of early civilizations has been marked by the increased longevity of man. In the 20th century, human mortality has had a constant decrease. The extension of human life and well-being is attributable to good public health practices, immunization of all children and adults, chlorination of potable water, sewage disposal of human and industrial waste, and food hygiene, including pasteurization. All have contributed to improved life and longer survival of human beings. The irradiation of food will further improve human health by the prevention of foodborne disease. . . ." —J.H. Steele and R. Engel, "Radiation Processing of Food," *JAVMA*, Vol. 201, No. 10, p. 1522 (1992)



Nations Pride Distributors, Inc.

Bacteria-reduced irradiated chicken is available now from Nations Pride, at the Food Technology plant in Mulberry, Florida. Formerly called Vindicator, Inc., the irradiation plant was organized and directed throughout the 1990s by Sam Whitney, whose dream was to use food irradiation as a means of eliminating foodborne illnesses. Whitney died of lung cancer in Sept. 1998.

pasteurization by irradiation of hamburger meat patties, of which some tens of millions are consumed daily. The same attitude and apathy exists in Europe, where *Listeria*-contaminated pork meat and other food caused the death of 63 persons in France, as reported in 1993. Since then, *Listeria* has become a serious public health problem in America.

One hesitates to ask who is in charge of the protection of the public health in these United States, or our neighbors in the Americas or Europe. The "anti" activist can always be relied on to oppose new technologies, and among them are powerful interests. Environmentalists, health food advocates, food processors, wholesalers, retailers, and producers—all for their own reasons are saying that the consumer is not ready, or does not want it, or is against it.

Consumers Want Irradiation!

But this is not true: The U.S. Department of Agriculture survey of consumer attitudes, and actual market tests by Susan Conley, say that 70 percent of the American public wants safe food and will accept food irradiation to ensure that this is so. The University of California survey by Dr. Christine Bruhn found Californians of the same mind. A University of Georgia survey went further, and found the consumer willing to pay more for irradiated food that would offer the same protection as pasteurized food. The consumer said the same in surveys by the Food Science departments at Purdue, Iowa State, and Kansas State universities. More recently, several national consumer surveys find the public seeking an opportunity to test irradiated foods.

Why have public health scientists not given the consumer the benefits of food irradiation?

Where were the national public health leaders who spoke for irradiation? The American Medical Association was among the few early supporters, as was the American Veterinary Medical Association. But the American Public Health Association was outspoken against food irradiation, and it opposed any discussion of resolutions supporting radiation.

The only academic support came from universities and colleges with food science and home economics departments. Strangely, some public health schools and medical colleges were afraid to support food irradiation, or spoke against it, calling it "dangerous" and "destructive." So-called health letters warned their readers against food processors, who would supposedly use irradiation to cover up failed hygiene.

The first top public health officials to speak out on the importance and value of food irradiation was James Mason, M.D., the Assistant Secretary of Health, HHS, in an editorial in *Public Health Reports,* Sept./Oct. 1999.[6] He concluded: "The bottom line on food irradiation is that the nation deserves to have—and should claim—the health benefit this technology will surely provide. We don't know how great that benefit will be—but we do know it will be significant."

Two years later, Philip R. Lee, M.D., the Assistant Secretary of Health, Director of the U.S. Public Health Service stated:[7]

"It is the U.S. Public Health Service's responsibility to use what we know to protect and improve the health of the public. Each modern food-processing advance—pasteurization, canning, freezing—produced criticism. Food irradiation is not different. It is up to leaders in the health professions to dispel the myths.

"The technology of food irradiation has languished too long already. Perhaps our nation has become dangerously complacent about the importance of public health measures. The current health care debate offers us both a mandate and an opportunity to increase the understanding of the importance of public health for ensuring personal health. If this message is lost, our efforts to advance and protect the nation's health will not succeed."

James H. Steele, a pioneer in food irradiation, is a former public health veterinarian with the U.S. Public Health Service, and has more than 45 years of global public health experience. Steele has held the position of Assistant Surgeon General of the USPHS, and is currently Professor Emeritus of the School of Public Health, University of Texas at Houston. This article was adapted from a paper Steele presented at a June 1999 conference on irradiated foods, sponsored by the Minnesota Health Department.

Notes

1. E.S. Josephson and H.A. Dymsza, (1999). "Food Irradiation," *Technology,* Vol. 6, pp. 235-238.
2. S.D. Baily et al., 1957. *Radiation Preservation of Food by the U.S. Army Quartermaster Corps* (Washington, D.C.: U.S. Government Printing Office).
3. Anonymous, 1958. "Federal Food, Drug and Cosmetic Act, as amended. 21 U.S. code 321,21 Code of Federal Regulations, part 121—Food Additives.
4. Statement on the wholesomeness of irradiated foods by the Surgeon General, Department of the Army, in "Radiation Processing of Foods." Published Hearings before the Subcommittee on Research and Development and Radiation of the Joint Committee on Atomic Energy of the United States, June 9 and 10, 1965.
5. The report of the 1964 meeting in Rome was published in WHO Technical Report Series, No. 316 (Geneva: World Health Organization, 1966). The report of the 1969 meeting in Geneva was published in WHO Technical Report Series, No. 451 (Geneva: World Health Organization, 1970). The report of the 1976 meeting in Geneva was published in WHO Technical Report Series, No. 604 (Geneva: World Health Organization, 1977). The report of the 1980 meeting in Geneva was published in WHO Technical Report Series, No. 659 (Geneva: World Health Organization, 1981). The report of the 1997 meeting in Geneva was published in WHO Technical Report Series, No. 890 (Geneva: World Health Organization, 1998).
6. J.O. Mason, 1993. "Food Irradiation—Promising Technology for Public Health," *Public Health Reports,* Vol. 108, No. 3, p. 402.
7. P.R. Lee, 1994. "Irradiation to Prevent Foodborne Illness, *JAMA,* Vol. 272, No. 4, p. 281 (July 27).

Scientific Answers to Irradiation Bugaboos

by Marjorie Mazel Hecht

The anti-food irradiation litany repeats the same objections to food irradiation, no matter how many times each of the points has been answered in full by scientific researchers. Here, we summarize for readers the objections and the answers, giving sources for further documentation.

Objection: Food irradiation creates unique radiolytic products.

Response: The July 1986 report of the Council for Agricultural Science and Technology (CAST), which reviewed all the research work on food irradiation, defined unique radiolytic products "as compounds that are formed by treating foods with ionizing energy, but are not found normally in any untreated foods and are not formed by other accepted methods of food processing."

The report states that "on the basis of this definition no unique radiolytic compounds have been found in 30 years of research. Compounds produced in specific foods by ionizing energy have always been found in the same foods when processed by other accepted methods or in other foods" (Vol. 1, p. 15).

Objection: Food irradiation causes vitamin loss.

Response. There is an extensive scientific literature dealing with food irradiation and vitamins over the past 45 years. In general, vitamins sensitive to heat are also sensitive to irradiation. It was found that vitamin retention is best when irradiation is carried out at low temperatures and in a vacuum (with the absence of oxygen) or when foods are dry. In general, scientists have concluded that "no significant adverse effects on the nutritional quality have been found in foods processed with ionizing energy relative to foods processed by conventional means" (CAST report, Vol. 1, p. 5).

One frequently cited comment by the opponents of food irradiation concerns the loss of vitamin C in irradiated potatoes. However, the specific finding is that the ascorbic acid in the potato shifts to dehydroascorbic acid. This change is "irrelevant from a nutritional point of view because dehydroascorbic acid has practically the same vitamin C activity as ascorbic acid," as reported in the CAST report (Vol. 1, p. 29).

Objection: The Food and Drug Administration ignored any adverse studies in their 1988 ruling on food irradiation.

Response: The FDA has stated that there was no substance to allegations that it did not include adverse studies in making its rulings on food irradiation. The FDA answered each specific point of criticism in a 33-page document published in the *Federal Register,* Vol. 53, No. 251, Dec. 30, 1988. In this document, the FDA denies a request for a hearing on objections to its 1988 rule authorizing the use of food irradiation for pork and other foods, stating that "none of the objections has provided the information necessary to justify a hearing."

Taking up the objections from food irradiation critics one by one, the FDA notes in each case that "a hearing will not be granted on the basis of mere allegations or general descriptions of positions and contentions."

The FDA states that its Task Group did examine "in detail those studies [noted by the critics] that appeared on their face to show adverse effects. The Task Group found that because of problems associated with diet or inadequate experimental design, any adverse toxicological effects reported in these studies could not be attributed to irradiation of the food."

Objection: Food irradiation caused polyploidy in India.

Response: Perhaps the most frequently cited objection to food irradiation by its opponents is based on a study by the National Institute of Nutrition in Hyderabad, India, in the mid-1970s that found polyploidy (chromosomal changes) in a small sample of malnourished children fed irradiated wheat.

Both an Indian government committee and an international committee determined that the conclusions of this study could not be supported by the data, that the study was faulty, and that the results were not reproducible. The Indian government committee found that "the frequency of 1.8 polyploid cells found in children eating freshly irradiated wheat was well within the normal range of healthy human beings" (as reported in the FDA statement in the *Federal Record,* Dec. 30, 1988, p. 53183).

Objection: Animals fed irradiated food developed abnormalities.

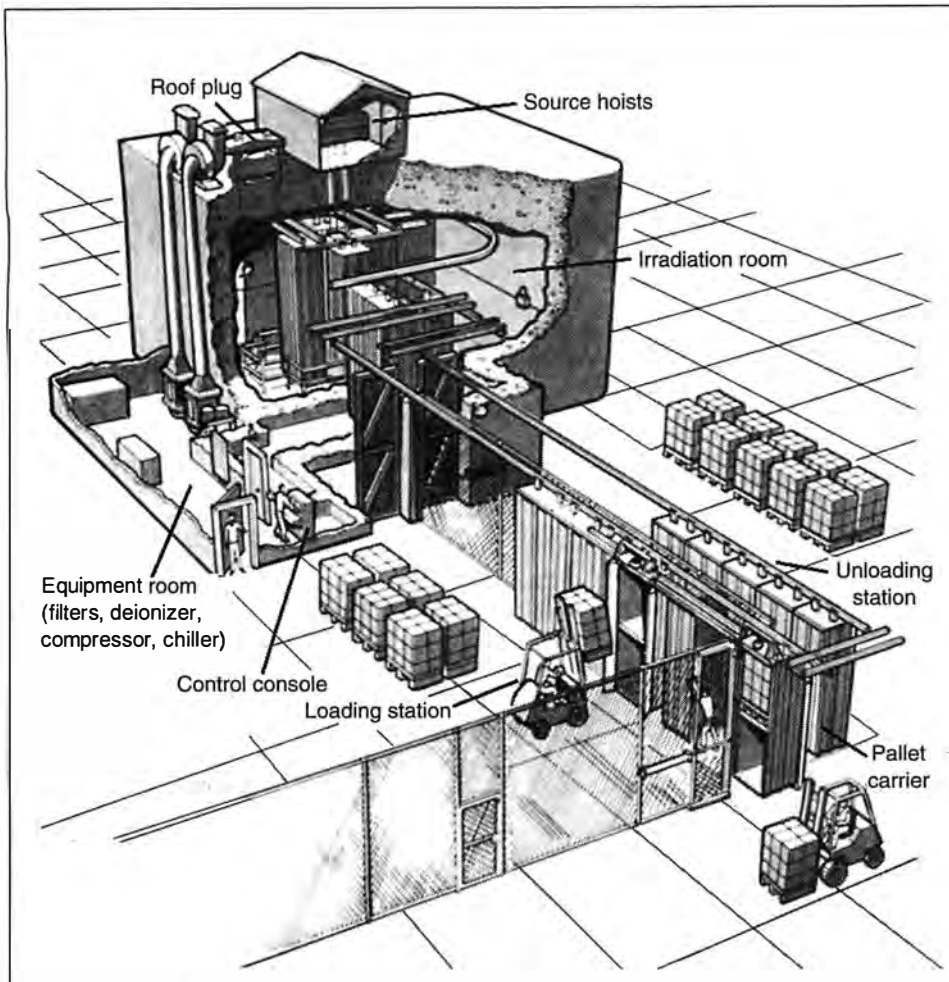
Response: In the Dec. 30, 1988 *Federal Record,* the FDA discusses the so-called adverse studies one by one, concluding that "studies with irradiated foods do not show adverse toxicological effects." Specifically, the FDA evaluated 1984 studies conducted with mice to test the safety of radiation-sterilized chicken, noting that the signs of kidney damage "were the result of the high protein content of the chicken diets rather than of the irradiation of some diets" (p. 53186).

As for studies with fruit flies fed radiation-sterilized chicken that had fewer progeny than a control group, the FDA noted that fruit flies are an "unreliable indicator of adverse reproductive effects" in human beings and that "mammalian data, which are more relevant to humans" do not show a pattern or trend indicative of adverse reproductive effects."

In a study of hamsters fed irradiated fish, which critics said showed chromosome damage, the FDA noted that "the proportion of cells with the *normal* number of chromosomes was greater in the hamsters fed irradiated fish than in those fed the control diet" (p. 53190).

Objection: Irradiation will allow an increase in aflatoxin production.

Response: The studies cited about aflatoxin production involve the addition of aflatoxin-producing organisms to wheat after irradiation. The FDA stated that "it had no evidence that would lead it to conclude that food irradiated and



SCHEMATIC OF A FOOD IRRADIATION PLANT

This pallet irradiator design uses cobalt-60 as a radiation source. The boxed product (chicken, fruit, meat, and so on) remains on the same pallet from the completion of packaging to delivery to the customer.

Food irradiation uses the ionizing radiation (or ionizing energy) from a decaying radioactive isotope, like cobalt-60, as its radiation source. The very short wavelength gamma rays penetrate inside solid particles and kill microorganisms by breaking down the cell walls or destroying the metabolic pathways of the organism, so that the cell dies. At higher doses, all microorganisms are killed, sterilizing the processed food.

Source: Nordion

stored under normal handling practices would show increased aflatoxin production" and that "infection with aflatoxin producing organisms would ordinarily occur before harvest" (as reported in the FDA statement in the *Federal Record,* Dec. 30, 1988, p. 53194).

As for concerns that malpractice in the storage of dry grains could lead to the growth of aflatoxin, the CAST report notes from the review of the research that "the available evidence [on the subject] indicates that treating grain with ionizing energy to control insects does not add to that hazard."

Objection: Botulism could grow undetected after irradiation.

Response: The research on this topic indicates that with medium doses of irradiation "there are no microbiological safety problems with moist foods, such as fresh meats, poultry, and fish . . . as long as these foods are stored and distributed near the temperatures of ice (36 to 41 degrees F) (2 to 5 degrees C) according to good manufacturing practice." The CAST report notes further that "As long as foods are refrigerated below 50 degrees F (10 degrees C), there is no microbiological safety problem for *Clostridium botulinum* types A and B" (Vol. 1, p. 32).

For *Clostridium botulinum* type E, however, which may be a concern with fresh fish, the CAST report notes that at doses of less than 2.2 kilograys, and particularly below 1.5 kilograys, "a diverse and active spoilage population remains to avoid toxin production by *Clostridium botulinum* type E" (CAST report, Vol. 1, p. 32). In other words, irradiation at lower doses reduces the microbial population but does not eliminate it, so that if irradiated fish is improperly refrigerated, it will begin to spoil and smell before the *Clostridium botulinum* type E could produce toxin.

Objection: Irradiation facilities will be a safety problem.

Response: Nuclear safety is one of the chief concerns mentioned by the groups opposing food irradiation. In particular, critics are concerned with nuclear waste disposal. This is a political problem, not a technical one. The technology of reprocessing nuclear waste, which eliminates 99 percent of it, is well known and used by other nuclear nations—but not by the United States. The technology also exists for long-term burial of waste in a vitrified form.

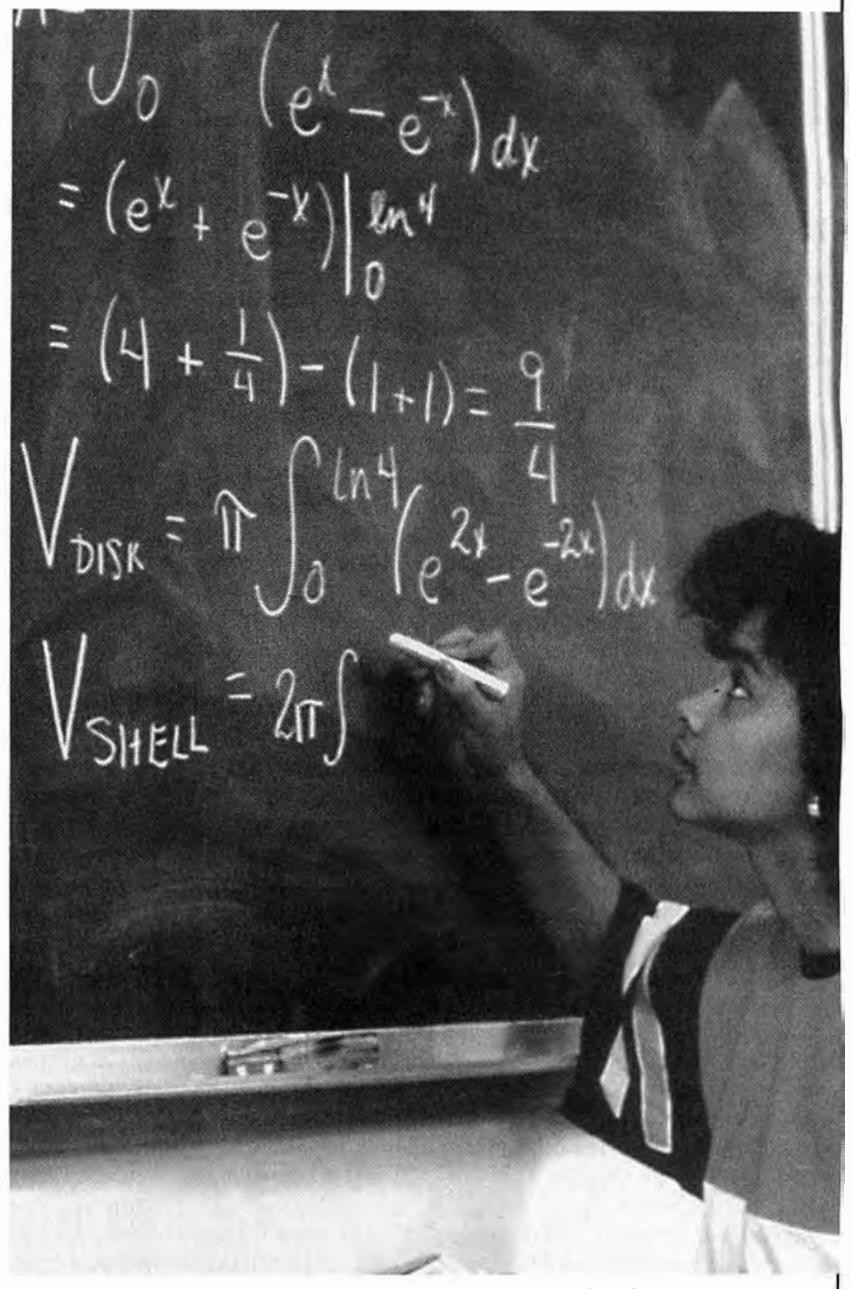
However, it seems certain that the food irradiation plants of the future will not use radionuclides for irradiation. They will use electron beam accelerators or X-ray sources to provide irradiation. This should eliminate opposition to food irradiation based on concerns with nuclear safety because no radioactive materials will be used.

The Real Calculus vs. What You Learned

HOW LEIBNIZ'S ORIGINAL CALCULUS HAS BEEN SUBVERTED

by Ernest Schapiro

A false version of the calculus, based on the Cauchy limit theorem, is now taught in the schools. To revive inventiveness in the physical sciences, students must learn the real creative breakthrough embodied in Leibniz's discovery of the calculus.



A student in calculus class. But what is she learning?—a set of rules whose discovery has been mystified by the Limit Theorem of Cauchy, or the actual method of invention utilized by Leibniz in discovering the calculus?

Invention of the calculus is one of the greatest discoveries. It permitted the solution to a wide range of mathematical problems by means of a newly invented language or metaphor. It was therefore a great creative breakthrough, and it is therefore entirely predictable that the process by which it was invented has never been properly taught to the millions of people who study the calculus. The basic concept of continuity, which for Gottfried Wilhelm Leibniz (1646–1716) was consistent with the notion of causality, is taught, but in such a way as to stand it on its head. Continuity, rather than being something fundamental, gets defined nowadays as something secondary to "sets of points."

I became interested in the origin of the calculus after hearing a lecture on the subject in Buffalo in 1978. I ordered a book mentioned by the speaker, Carol White, entitled *The History of the Calculus and Its Conceptual Development,* by Carl Boyer.[1] This book, in turn, cited *The Early Mathematical Manuscripts of Leibniz.*[2] When I came to New York City in 1980, I was able to get a copy of the Leibniz work through a company that searches for out-of-print books. After grappling with the book for a few weeks, I could get the main idea of what Leibniz was doing with series. I have been trying since then to figure out why Leibniz, and not other great mathematicians, such as Pascal, Fermat, and Huygens, made the breakthrough. I think the answer to the question requires understanding his philosophical method.

Leibniz, from his teens, was interested in metaphysics and scientific method. His dissertation at age 20, entitled "Dissertation on the Art of Combinations,"[3] concerned the mathematical analysis of complex statements into simpler ones. In the course of his work, he was forced to present his own definitions of commonly used words. In fact, the invention of the calculus was part of a program to enrich the language of Germany. His calculus itself was based upon new poetic metaphors, applied to previously unsolvable problems. He thus enabled everyone to conceptualize something which previously had been extremely difficult.

The Principle of Discovery

Leibniz proposed a project to represent all conceptions of mathematics, law, physical science, and morals by a sort of universal language, which would contain within itself the very principle of discovery. He described this as providing an increase in the powers of reason, comparable to the improvement of vision by the invention of the telescope. He called this the universal characteristic. Unfortunately, he could not enlist the collaboration of any scientists of his time.

However, to break the ground for this project, he developed rigorous definitions, definitions which contained within themselves, wherever possible, the element of causality. He insisted on the principle that the *predicate* is necessarily implied in the *subject*. This were true, whether the truths involved were contingent truths, or necessary, *a priori* truths. A *first truth* is one which predicates something of itself, or denies the opposite of its opposite. For example, *A is A,* or *A is not non-A*. These truths are called *identities*. All other truths are reducible to first truths by the aid of definitions or of concepts.

Leibniz gave as an example the, until then, axiomatic statement: "The whole is greater than the part." Here is how he proceeded:

> "The whole is greater than its part," could be proved by a syllogism, of which the major term was a definition, and the minor term an identity. For, if one of two things is equal to a part of another, the former is called the less, and the latter the greater; and this is to be taken as the definition. Now, if to this definition there be added the following identical and undemonstrable axiom, "Everything possessed of magnitude is equal to itself," i.e. $A = A$, then we have the syllogism:
>
> Whatever is equal to a part of another, is less than that other: (by the definition)
>
> but the part is equal to a part of the whole: (i.e. to itself by identity).
>
> Hence the part is less than the whole. QED[4]

As Leibniz remarked later, this proof was important, because without it, someone would be able to assert an exception to the axiomatic statement. Furthermore, from these considerations came the principle that the *predicate* or *consequent* inheres in the *antecedent*. He restated it as a principle of causality: Nothing happens without a reason. Leibniz wrote:

> In contingent truths however, though the predicate inheres in the subject, we can never demonstrate this, nor can the proposition ever be reduced to an equation or an identity, but the analysis proceeds to infinity, only God being able to see, not the end of the analysis indeed, since there is no end, but the nexus of terms or the inclusion of the predicate in the subject, since He sees everything which is in the series. Indeed this truth arises in part from His intellect and in part from His will, and so expresses His infinite perfection, and the harmony of the entire series of things, each in its own particular way.[5]

As an example of such an infinite series he gave the ratio of the side of the square to the diagonal.

Thus Leibniz's work in mathematics was one aspect of his philosophical program and grand design. He hoped that theological questions could be approached as rigorously as mathematics. In 1679, writing to John Frederick, Duke of Brunswick-Hanover, he said:

> But disputes are more customary than demonstrations in philosophy, morals, and theology, and most readers will have the prejudices about such a project that are usual about works dealing with these matters; for it will be thought that the author has merely transcribed and problematized, and is probably a superficial mind little versed in the mathematical sciences and, consequently, hardly capable of true demonstration. In view of these considerations, I have tried to disabuse everyone by pushing myself ahead a little further than is common in mathematics, where I believe I have made discoveries which have already received the general approval of the greatest men of the day, and which will appear with brilliance whenever I choose. This was the true reason for my long stay in France—to perfect myself in this field, and

From a portrait by Edelinck, David Smith Collection, as in Howard Eves, *An Introduction to the History of Mathematics* (New York: Holt, Rinehart and Winston, 1953)

The young Gottfried Wilhelm Leibniz (right), and Christiaan Huygens (1629-1695). Leibniz's association with the Dutch-born mathematical-physicist in Paris in 1672, set him on the path of discovery of the calculus.

to establish my reputation, for when I went there I was not much of a geometrician, which I needed to be in order to set up my demonstrations in a rigorous way. So I want first to publish my discoveries in analysis, geometry, and mechanics, and I venture to say that these will not be inferior to those which Galileo and Descartes have given us. Men will be able to judge from them whether I know how to discover and to demonstrate. I did not study mathematical sciences for themselves, therefore, but in order some day to use them in establishing my credit and furthering piety.[6]

Series and Differences

In the course of his work with identities, he noted the following case, whose implications had gone unrecognized.

Consider the series of increasing numbers

$A, B, C, D, E,$ and examine the differences

$$A + (B-A) + (C-B) + (D-C) + (E-D) = E$$
$$L \qquad M \qquad N \qquad O$$
$$E-A = L + M + N + O$$

This was identically true of any series of steadily increasing or decreasing numbers. He began to look at some simple series of numbers such as the series of the squares.

0 1 4 9 16 25
1 3 5 7 9

where the second row represents the differences between successive squares. He noticed that the differences of these differences were all 2.

He devised a table of numbers to represent the formations of sums and differences by a kind of shorthand.

1	1	1	1	1	1	1
1	2	3	4	5	6	7
1	3	6	10	15	21	28
1	4	10	20	35	56	84
1	5	15	35	70	126	210
1	6	21	56	126	252	462
1	7	28	84	210	462	924

Looking at this horizontally, any term is the sum of the series to the left just above it. That is, 10 = 1 + 2 + 3 + 4. Any term is the difference of two just below it and to the left. Furthermore, looking at the diagonals,[7] the terms provide the coefficients for the elevation for $x + 1$ to any power.

For example: $(x+1)^2 = x^2 + 2x + 1$
$(x+1)^3 = x^3 + 3x^2 + 3x + 1$

This has a geometrical interpretation. Thus, to take a square two units on a side and convert it into one with three units on a side, we add on a 1×1 square and two 2×1 rectangles to the original square (Figure 1).

The expression for $(x + 1)^3$ has a geometrical interpretation for cubes.[8] Leibniz's table was a way of representing series of numbers, because each row was constructed by taking the sum of the numbers in the row above, and this principle could be extended as far as one wished. Sums of sums were second sums and differences of differences were second differences. We will see that the notion of a derivative and a second derivative go back to the simple ideas of differences and second differences.

Leibniz looked upon series of numbers as analogous to contingent causal sequences, traceable to an original cause.

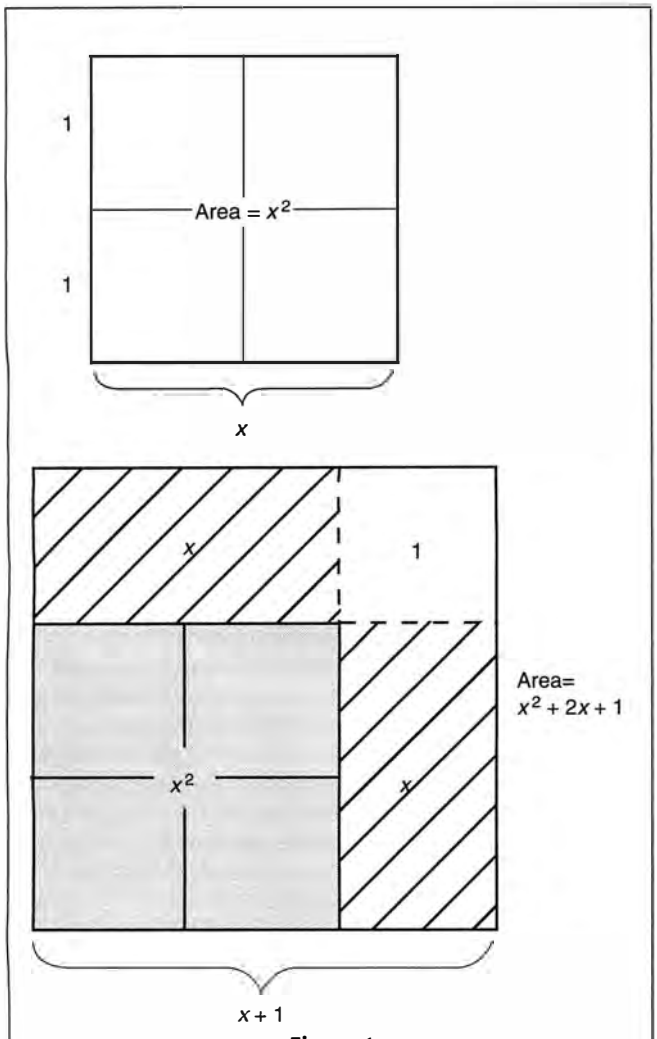


Figure 1

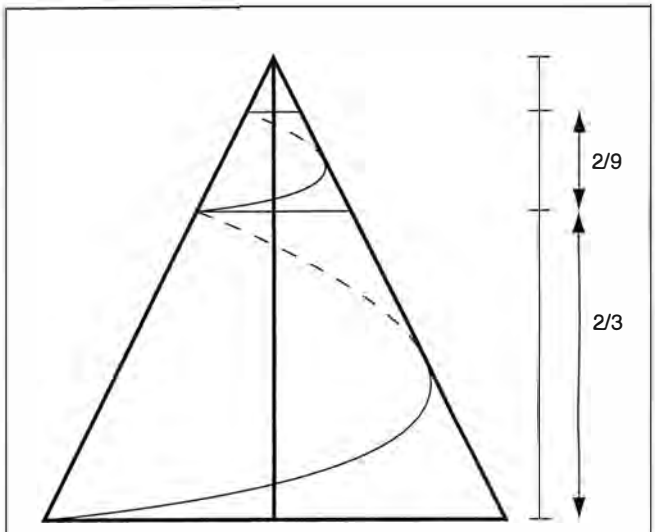


Figure 2

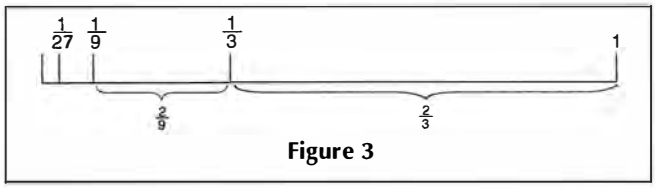


Figure 3

For example, he begins his essay "Art of Combinations," with a proof for the existence of God, based upon all motion in the universe, of necessity, having a first cause. The cause of the sequence may not be apparent on first inspection. However, the generative principle must exist, for nothing happens without a cause. A series of numbers represents a principle of causality. We have already seen how some series have a simple geometric interpretation, such as the series of squares or of cubes.

The series called the geometric series can be considered to represent self-similar growth, as in the formation of a self-similar spiral traversing the surface of a cone from the base to the apex, and always maintaining the same angle to the horizontal (Figure 2). Consider the series

$$1,\ 1/3,\ 1/9,\ 1/27,\ 1/81, \ldots$$

Thus, at the start, the entire height of the cone is yet to be traversed; hence we have 1. After the first turn of a spiral, one-third of the distance remains. After the second turn is completed, only one-ninth remains. Leibniz noticed something interesting about this series, using his new approach. The difference series of a geometric series is, itself, a geometric series. This follows from the self-similar geometry. Leibniz diagrammed the calculation by representing each of the terms as a length; all of these lengths took the same starting point (Figure 3).

Because the first term of the series is 1, and the last term is 0, the sum of all the successive differences between the terms of the series must also equal 1. The successive differences, however, also are a geometric series with the same ratio as the original series!

Leibniz told his colleague Christiaan Huygens, in Paris in 1672, that he had achieved these interesting results with this new principle. Huygens put his young friend to the test, asking him to find the sum of the following continuing series:

$$1 + 1/2 + 1/6 + 1/12 + 1/20 + 1/30 \ldots .$$

Leibniz recognized this series as being the difference series of another series (series *A*, below); and, this allowed for its sum to be readily determined. Here is how he worked it:

$$\text{series } A = 1,\ \frac{1}{2},\ \frac{1}{3},\ \frac{1}{4},\ \frac{1}{5},\ \frac{1}{6},\ \frac{1}{7} \ldots$$

$$\text{series } B = \frac{1}{2},\ \frac{1}{6},\ \frac{1}{12},\ \frac{1}{20},\ \frac{1}{30},\ \frac{1}{42}, \ldots$$

Leibniz was aware that series *A* was not convergent, that is, its sum was infinity, not a particular number. Therefore, he cut series *A* off after *n* terms. This means that there are $n - 1$ terms in the *B* series of differences. Leibniz discovered that the sum of these $n - 1$ differences is equal to $1 - (1/n)$. (The reason for this is found in the rule Leibniz discovered in his study of identities, mentioned earlier, that the sum of the differences

is equal to the difference between the first and last terms of the original series.) So, for example, consider the sum of series *B*, up through the third term, 1/12. This is the $n - 1$ term, so n would equal 4. Then the sum of series *B* up through this term should be $1 - 1/4 = 3/4$. Adding the three terms shows that it is, and the same holds for any term. Now, if we take the expression $1 - (1/n)$, describing the sum of series *B*, and consider it as n gets larger and larger, we see that $1/n$ gets very small. Thus the expression for the sum of series *B* approaches 1. This was the answer to Huygens's test.

$$1$$

$$\frac{1}{2} \quad \frac{1}{2}$$

$$\frac{1}{3} \quad \frac{1}{6} \quad \frac{1}{3}$$

$$\frac{1}{4} \quad \frac{1}{12} \quad \frac{1}{12} \quad \frac{1}{4}$$

$$\frac{1}{5} \quad \frac{1}{20} \quad \frac{1}{30} \quad \frac{1}{20} \quad \frac{1}{5}$$

$$\frac{1}{6} \quad \frac{1}{30} \quad \frac{1}{60} \quad \frac{1}{60} \quad \frac{1}{30} \quad \frac{1}{6}$$

$$\frac{1}{7} \quad \frac{1}{42} \quad \frac{1}{105} \quad \frac{1}{140} \quad \frac{1}{105} \quad \frac{1}{42} \quad \frac{1}{7}$$

Figure 4
HARMONIC TRIANGLE

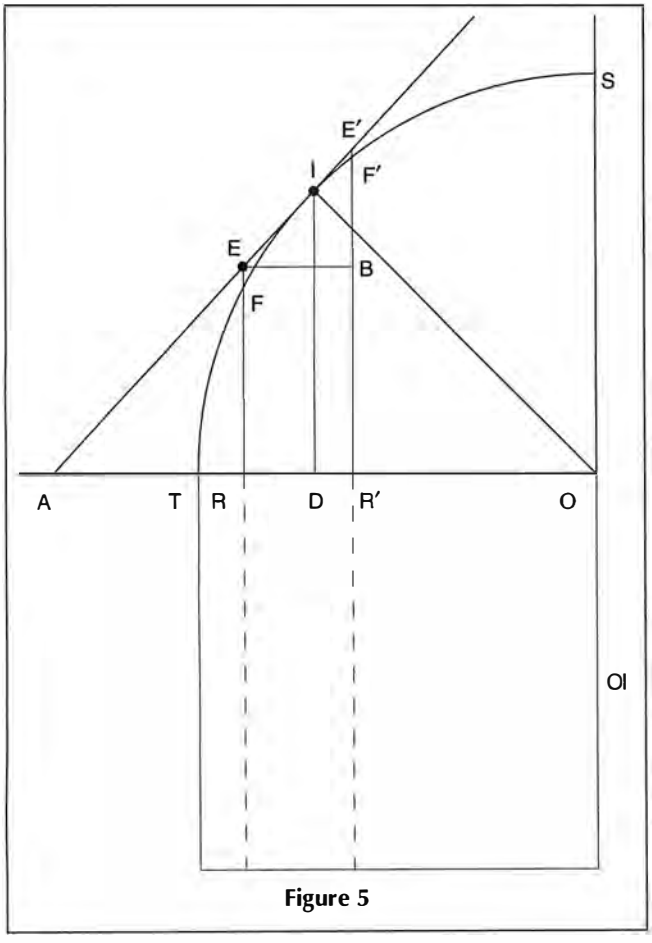


Figure 5

Leibniz saw that series of fractions, just like the series of integers, could also be derived, *ad infinitum,* from one another. He constructed another table, which he called the harmonic triangle (Figure 4). This was based upon the same rule, namely, that successive rows were composed of the difference terms of the previous row. (Thus, 1/2 is the difference between 1 and 1/2; 1/6 is the difference between 1/2 and 1/3; 1/12 is the difference between 1/3 and 1/4, and so forth.)

Leibniz began to think about how this approach, which was valid for integers and fractions, might also be valid for series of infinitesimally small numbers. We will soon see how that was applied.

Huygens was delighted by Leibniz's discovery. The particular series he had asked Leibniz to solve had already been worked out by Hudde. But the approach Leibniz took was original. Huygens asked Leibniz to study geometry, especially the determination of the areas of surfaces of revolution. Leibniz read the writings of Blaise Pascal. In particular he was fascinated by Pascal's solution to the surface of a sphere, conceiving of the sphere as produced by the rotation of a circle about an axis. Figure 5 is the diagram which Pascal constructed to represent the solution to the surface generated by rotating a quadrant of a circle about an axis. Pascal was able to transform the surface of the hemisphere generated by this rotation into a rectangle. This section represents a stage in Leibniz's efforts to develop the calculus; it does not embody the basic conception which he later achieved. If it is too difficult for the reader, don't worry too much, just go on to the next section.

In this figure, *OI* is a radius. The vertical strip with base *RR'* is actually of *infinitesimal* width. *I* is some point located vertically above the width *RR'*. *EB* is equal to *RR'*. *EE'* is the tangent to the circle at the point I. By the tangent, we mean a line touching the circle at one and only one point. Then we can show that the little infinitesimal triangle, *EE'B*, and the triangle *OID* are similar. (The line *ID* divides right triangle *AIO* into triangles *IAD* and *OID*, which are similar to each other, as well as to triangle *AIO*. That is, they have the same three angles, and therefore their sides are proportional, or, in Leibniz's description, they are indistinguishable apart from their size. *EE'B* and *IAD* are similar because their sides are parallel. Because *IAD* and *OID* are similar, so are *EE'B* and *OID*.)

Based on this similarity of *EE'B* and *IOD*, Pascal concluded that $EE' \times DI = RR' \times OI$ (the radius), and that this relationship must hold for each vertical infinitesimal strip! To find the surface for the entire hemisphere, we need the surface generated by rotating the quadrant about the *OR'R* axis. Each vertical strip or *sinus*, such as *RR'FF'*, when rotated about the base, will generate a circular band upon the hemisphere of arc length *FF'*, that is, an arc length very close to the length of the tangent *EE'*. Pascal then said, that if we were to take the entire quadrant as divided up into these infinitesimally thin vertical strips, then

$$\Sigma\, EE' \times DI = OI^2,$$

where Σ denotes a process of summation. We get OI^2 on the right side, because *OI* is being multiplied in succession by

each of the lines *RR'*, from *O* out to *I*, and their sum is also *OI*.

But what is the product $EE' \times DI$? It is the area of a cylinder of approximate radius *DI* and height *EE'*, provided we also multiply by 2π. We say *approximate radius,* because *DI* lies between the two diameters of the little cylinder, *RE'* and *R'E'*. The total surface of the hemisphere is obtained by summing up all of these little cylinders. Since the two radii are not exactly equal, that is, *RE* and *R'E'*, these are not perfect cylinders. This was justified, because as the vertical strip gets thinner and thinner, the tangent line *EE'* comes closer and closer to being equal to the arc of the circle *FF'*. Therefore, the area of the infinitesimal cylinder becomes equal to the area of the infinitesimal circular band on the surface of the sphere generated by rotating the quadrant around the axis *AO*. It gives the result: 2π times the radius squared. Notice that what we were also doing was to construct a rectangle of base equal to the sum of all the *RR*'s and of constant height *OI*. Because we were summing the *RR*'s all the way out to the end, the rectangle is, in this case, a square. This is illustrated by the strips placed vertically below the line *OA*. Thus, we have, in fact, been converting the surface of the sphere into a plane area, in this case a square.

Now Leibniz was suddenly struck by the observation that this method, which Pascal had limited to the sphere, could actually be used for any surface of revolution. In this case, the plane area would be constructed as before by taking the normal (perpendicular) to the curve at a given point on the curve. Whereas, in the case of the sphere, the normal was always the radius of the circle, in the case of some other surface of revolution, say a paraboloid, the normal would be of varying length. However, one could still derive the characteristic triangle for the curve at each point and erect below that point, as before, a perpendicular, not to the curve but to the axis of rotation below the curve, and of length equal to the original normal to the curve. One then had the difficult task of summing all the rectangular strips.

Generating a Curve

Leibniz spent some time working out solutions based upon this new approach, which, it turns out, was also being utilized by Barrow, Newton's teacher. Although this method used the tangent to the curve, it was not until 1676 that Leibniz began to use the method of differences to derive tangents. In that year he made a crucial breakthrough, when he realized that the determination of the tangent to the curve could be obtained very easily by use of the principles he had already been applying with series of integers and series of fractions. He also realized that, because determining the tangent to a curve was equivalent, as we shall see, to finding the successive differences of the curve, then, because finding areas of surfaces involved a process of summation of a series, it amounted to an inverse tangent problem. That is to say: Given a function or curve, determine that second function for which the first function or curve was the tangent. If this sounds very complicated, just take another look at the arithmetic and harmonic triangles. Recognize again, that summation, and the taking of successive differences, are the inverse of one another. The principle is, in fact, childishly simple—but only a great creative genius was able to see its application, as we are about to demonstrate.

Leibniz saw that the characteristic triangle, *BEE'* used in Pascal's calculation of the sphere, reflected not just the property of the curve at that point, but, of necessity, the process of generation of the entire curve, of which the point was only one moment. Therefore, he looked at the process governing the generation of the curve from the same standpoint from which he had looked at the formation of all other series.

Consider the parabola with equation $y = kx^2$ (Figure 6). This equation for the parabola, and the equations of other conic sections, were already known at this time, and Leibniz was reading about them in the works of Descartes. The line from x_0, passing through $(x_1 y_1)$, and reaching the vertical line on the right is the tangent to the parabola. The line from $(x_1 y_1)$ to $(x_2 y_2)$ is a chord of the parabola.

The tangent in this parabola can be represented by its slope. For instance, the slope of the first line can be represented by

$$\frac{y_1 - y_0}{x_1 - x_0}.$$

This line is the tangent at (x_1, y_1).

The slope of the line joining the points (x_1, y_1) and (x_2, y_2) is

$$\frac{y_2 - y_1}{x_2 - x_1}.$$

We thus can think of the tangent as being the first of the series of such lines connecting point (x_1, y_1) with a series of

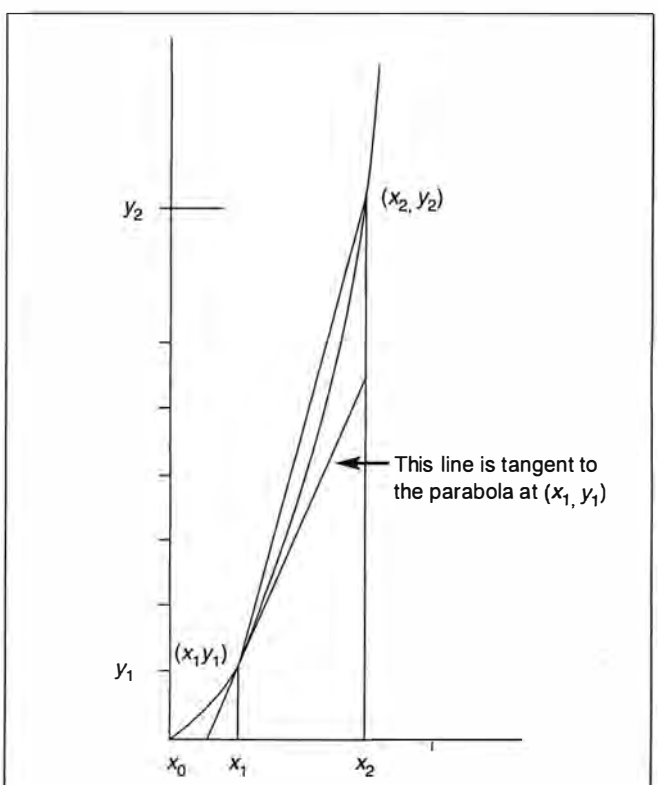


Figure 6
THE PARABOLA $y = kx^2$


Two leading opponents of the school of Descartes, the mathematician-philosophers Pierre de Fermat (left, 1601-1665) and Blaise Pascal (1623-1662), laid the foundation, through their work in number series and geometry, for Leibniz's discovery.

points further up along the parabola. Because it is the first such line, it connects the point (x_1, y_1) with itself. Leibniz saw that the successive values of the slopes of these lines formed a series, and that, if he could determine their rule of formation, then he could deduce the value of the series at the starting point. He looked at the successive differences of these values of the slope by the following simple calculation. By the known equation of the parabola,

$$y_1 = kx_1^2$$

Then, if $x_2 - x_1 = dx$,

$$y_2 = k(x_1 + dx)^2$$

(also by the known equation of the parabola).

Then, if $dy = k(x_1 + dx)^2 - kx_1^2$,

$$dy = y_2 - y_1 = k(x_1^2 + 2x_1 dx + dx^2) - kx_1^2$$

Note that dy and dx denote hypothetical changes in y and x; we are conducting a thought experiment for which Leibniz was prepared to provide full justification.

Then what was the value of the slope of the curve at the point x_1y_1? Taking the ratio, we get

$$\frac{dy}{dx} = k(2x_1 + dx)$$

Here Leibniz introduced his principle of continuity. He conducted a thought experiment. His principle stated:

> In any supposed transition ending in any terminus, it is permissible to institute a general reasoning in which the final terminus may also be included.[10]

The use of these new kinds of numbers, he compared with the successful use of the imaginary numbers:

> It will be sufficient if, when we speak of infinitely great (or, more strictly, unlimited), or of infinitely small quantities (i.e. the very least of those within our knowledge), it is understood that we mean quantities that are indefinitely great or indefinitely small; i.e., as great as you please, or as small as you please, so that the error that anyone may assign may be less than a certain assigned quantity. Also, since in general it will appear that, when any small error is assigned, it can be shown that it should be less, it follows that the error is absolutely nothing; an almost exactly similar kind of argument is used in different places by Euclid, Theodosius, and others; and this seemed to them to be a wonderful thing, although it could not be denied that it was perfectly true that, from the very thing that was assumed an error, it could be inferred that the error was nonexistent. Thus, by infinitely great and infinitely small, we understand something indefinitely great, or something indefinitely small, so that each conducts itself as a sort of class, and not merely as the last thing of a class. If any one wishes to understand these as the ultimate things, or as truly infinite, it can be done, and that too without falling back upon a controversy about the reality of extensions, or of infinite continuums in general, or of the infinitely small, ay, even

though he think that such things are utterly impossible; it will be sufficient simply to make use of them as a tool that has advantages for the purpose of the calculation, just as the algebraists retain imaginary roots with great profit. For they contain a handy means of reckoning, as can manifestly be verified in every case in a rigorous manner by the method already stated.[11]

In other words, we can include the case of $dx = 0$. As we will see, this approach of Leibniz evoked howls of protest: "How can you divide by zero?" Here is another formulation Leibniz gave of this principle:

> If in a given series one value approaches another value continuously, and at length disappears into it, the results dependent on these values in the unknown series must also necessarily approach each other continuously, and, at length, end in each other. So in geometry, for example, the case of an ellipse continuously approaches that of a parabola, as one focus remains fixed and the other is moved farther and farther away, until the ellipse goes over into a parabola when the focus is removed infinitely. Therefore, all the rules for the ellipse must of necessity be verified in the parabola (understood as an ellipse whose second focus is at an infinite distance.) Hence, rays striking a parabola in parallel lines can be conceived as coming from the other focus, or tending towards it.[12]

(Remember that when a light source is placed at one focus of an ellipse, the light is reflected back to the other focus. When a light source is placed at the focus of a parabolic mirror, it is reflected out in parallel rays; when parallel rays strike a parabolic mirror, they are reflected back through the focus of the parabola.)

Leibniz's solution is based upon the method of hypothesis, of a thought experiment in which a universal principle is invoked. As we shall see, it is this method of hypothesis to which his adversaries objected. Through the method of hypothesis, he had brought into existence a new kind of number, denoted by a metaphor, dy/dx, which has permanently enriched our language. Even his most bitter adversaries have been forced to adopt the metaphor in doing their calculations, although they have tried to mystify the way it was invented.

Once the rules of obtaining the tangent for a particular type of function or curve are worked out, the rest is child's play. For example, the derivative or tangent to the exponential x^n is $nx^{(n-1)}$. Leibniz also deduced the derivatives for first derivatives, namely, the second derivatives. In this, he was entirely unique; tangents for certain curves had already been discovered, but no one had worked out, or even conceived of, second derivatives. The science of wave motion, and much of mathematical physics, requires the second derivative.

The Principle of Continuity

Textbooks of calculus describe this tangent-determining process as equivalent to finding the derivative, or dy/dx, at the point. However, rather than utilizing the principle of continuity, they make continuity itself a secondary idea, one that is deduced from sets of points. The tangent is referred to as a limit obtained as one approaches, but never quite reaches, the point. This is in contradiction to Leibniz, who stated clearly that the end-point or terminus of the process must be included in the process. The Leibnizian approach enables us to see the growth process in the curve. By the principle of continuity, we can and must relate changes in the discrete to changes in the continuous manifold where causality is located. For example, the difference series for cubes shows us how cubes grow by adding on squares, lines, and points. The calculus, for the first time, helps us to hypothesize what must be going on in the continuous manifold between the moments when the new singularities pop out, that is, when the new layers are added onto the faces of the cube.

All growth processes generate a series of numbers. These series, in turn, are a means of describing the original process. As we remarked earlier, Leibniz saw that the inverse tangent calculation could be used to determine surfaces and areas. This amounted to simply determining what the series of numbers must be for which the first series constituted the first differences. This very easy approach gave Leibniz solutions to very difficult, or hitherto unsolved, problems. For example, Archimedes worked out a very tedious solution to the area under a parabola; his method is called the method of exhaustion, and well it might be, because it is so tedious! As we shall see, Leibniz's method makes use of his new language to solve the problem almost instantly.

Consider the series of strips of infinitesimal width, dx (Figure 7). Then the area of the rectangular strip of height kx^2 and the width dx is kx^2dx. Now, since these strips form an increasing series, it must be that there exists a second series for which they are in turn the differences. How did Leibniz figure out what that series is? Very simple: just take the inverse of the difference-forming process. The series of cubes has its differences in the form

$$(x + dx)^3 - x^3 = 3x^2dx + 3xdx^2 + dx^3.$$

When dx is made infinitesimally small, then, because dx^2 is incomparably bigger than dx^3, and incomparably smaller than dx, this reduces to $3x^2dx$.

Therefore, for the parabola, $y = kx^2$, the function

$$\tfrac{1}{3}kx^3$$

gives the series kx^2dx as its difference series. By making the rectangles infinitesimally narrow, their sum gives an increasingly close approximation to the area under the curve. Remember Leibniz's original discovery, that the sum of any series of differences equals the difference between the first and last terms of the second series, which gives rise to those difference. Therefore, the sum of the differences kx^2dx is equal to the value of $\frac{1}{3}kx^3$ at the right-hand endpoint, minus its value at the left-hand endpoint. Today this is called the *definite integral*.

The Limits of Courant

Having been through this demonstration of Leibniz's method, you may be thinking that surely it excites admiration among today's mathematicians, and is taught and used as a model for students. Wrong! All you need to do is to

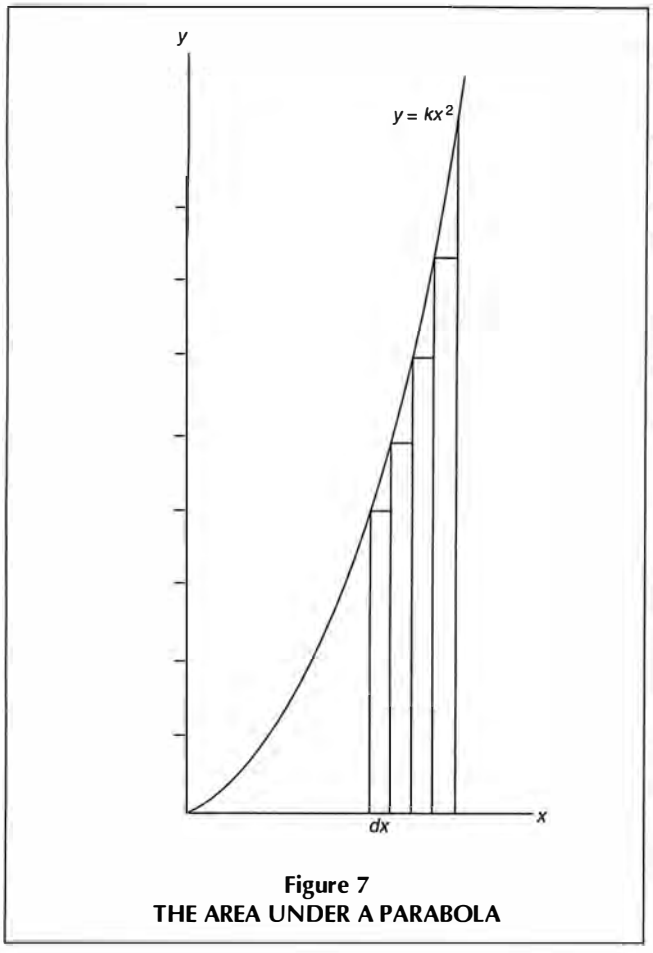


Figure 7
THE AREA UNDER A PARABOLA

examine the vicious slanders and distortions in the following commentaries on Leibniz, which, like iron filings in a magnetic field, point along the controlling lines of force. Let us look at the famous textbook *What Is Mathematics?* by Richard Courant, who was director of the prestigious Institute for Mathematical Sciences at New York University. He writes of Leibniz:

> His achievement is in no way diminished by the fact that it was linked with hazy and untenable ideas which are apt to perpetuate a lack of precise understanding in minds that prefer mysticism to clarity.

And further:

> In the mathematical analysis of the seventeenth and most of the eighteenth centuries, the Greek ideal of clear and rigorous reasoning seemed to have been discarded. "Intuition" and "instinct" replaced reason in many important instances.[13]

Leibniz is misrepresented, and his concept of continuity is omitted, in a later section of Courant's book entitled, "Leibniz' Notation and the 'Infinitely Small.'" Courant there reduces Leibniz's powerful metaphor, *dy/dx,* to a "symbolic notation," so as to leave out the underlying idea. Courant even implies that Leibniz really meant the same thing as he:

> Leibniz's attempt to "explain" the derivative started in a perfectly correct way with the difference quotient of a function $y = f(x)$,
>
> $$\frac{\Delta y}{\Delta x} = \frac{f(x_1) - f(x)}{x_1 - x}$$
>
> For the limit, the derivative, which we called $f'(x)$ (following the usage introduced later by Lagrange), Leibniz wrote dy/dx, replacing the difference symbol Δ by the "differential symbol" d. . . .

After insisting that we can avoid the problem of dividing by $dx = 0$, if and only if we resort to the "limiting process," Courant attacks Leibniz:

> Mystery and confusion only enter if we follow Leibniz and many of his successors by saying something like this: "Δx does not approach zero. Instead, the 'last value' of Δx is not zero, but an 'infinitely small quantity,' a 'differential' called dx; and similarly, Δy has a 'last' infinitely small value dy. . . ." Such infinitely small quantities were considered a new kind of number, not zero but smaller than any positive number of the real number system. Only those with a real mathematical sense could grasp this concept, and the calculus was thought to be genuinely difficult, because not everybody has, or can develop, this sense.[14]

Courant's criticism is basically that which Leibniz's work encountered from the time that it first appeared. However, the replacement of the *principle of continuity* by the idea of *limits* was codified in the 19th century by Augustin Cauchy, and this is the view espoused by Courant. Cauchy was deployed against Leibniz and his entire tradition of Continental Science. Cauchy's approach is the one taught in today's mathematics classes all over the world. It is responsible for mystifying the calculus and making it so difficult to learn, especially the differential calculus.

Carl Boyer, author of *The History of the Calculus and Its Conceptual Development,* was a student of Courant. He is outraged at the idea that Leibniz's description represents physical reality. He denies that instantaneous velocity at a point, represented by the tangent at the point, actually exists. Rather, he says, the instantaneous velocity is the limit which the average velocity, referred to above by Courant, approaches as the intervals get small enough:

> Inasmuch as the laws of science are formulated by induction on the basis of the evidence of the senses, on the face of it there can be no such thing in science as an instantaneous velocity, that is, one in which the distance and time intervals are zero. The senses are unable to perceive, and science is consequently unable to measure, any but actual changes in position and time. The power of every sense organ is limited by a minimum of possible perception. We cannot, therefore, speak of motion or velocity, in the sense of a scientific observation, when either the distance or the corresponding time interval becomes so small that the minimum of sensation involved

in its measurement is not excited—much less when the interval is assumed to be zero. . . .

This difficulty has been resolved by the introduction of the derivative, a concept based on the idea of the limit. In considering the successive values of the difference quotient $\frac{\Delta s}{\Delta t}$ [distance over time —ed.], mathematics may continue to indefinitely make the intervals as small as it pleases. In this way, an infinite sequence of values r_1, r_2, r_3, . . . r_n, . . . (the successive values of the ratio $\frac{\Delta s}{\Delta t}$) is obtained. This sequence may be such that the smaller the intervals, the nearer the ratio r_n will approach to some fixed value L, and such that by taking the value of n to be sufficiently large, the difference $|L - r_n|$ can be made arbitrarily small. If this be the case, this value L is said to be the limit of the infinite sequence, or the derivative $f'(t)$ of the distance function $f(t)$, or the instantaneous velocity of the body. It is to be borne in mind, however, that this is not a velocity in the ordinary sense and has no counterpart in the world of nature, in which there be no motion without a change in position.[15]

On Leibniz's principle of continuity, Boyer says:

. . . when called upon to explain the transition from finite to infinitesimal magnitudes, he [Leibniz —ed.] resorted to a quasi-philosophical principle known as the law of continuity. We have seen previous applications made of this doctrine by Kepler and by Nicholas of Cusa. The latter may have influenced Leibniz in this respect, as well as in the philosophical doctrine of monads.[16]

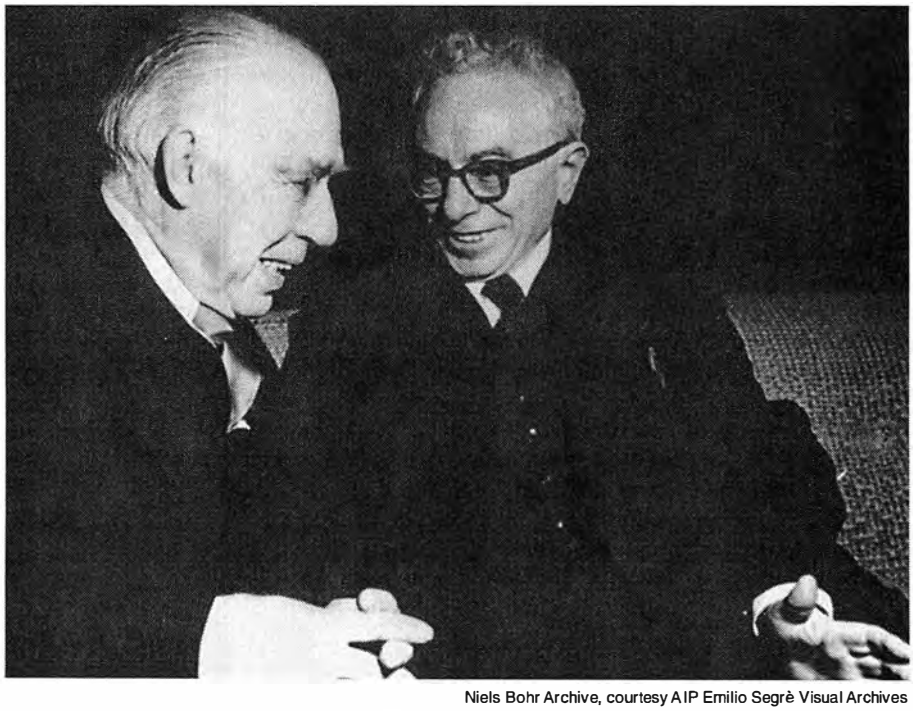
Later Boyer says:

Leibniz justified the limiting condition by the law of continuity, whereas mathematics has since shown that the latter must itself first be defined in terms of limits. In this manner of thinking Leibniz seems still to be striving to make use of a vague idea of continuity which we feel we possess and which had bothered thinkers since the Greek period.[17]

Leibniz vs. Cauchy Empiricism

The above description of the Cauchy method by Boyer introduces the empiricist outlook, an outlook that culminated in the mind-destroying New Math of the 1970s. All hypothesis-formation is eliminated. The student is forced to go through pages and pages of definitions of sets of points and axioms of the number system before he even learns about the derivative. Bertrand Russell, the person philosophically responsible for the New Math, had an intense dislike for Leibniz because of Leibniz's assertion of universals. Does knowledge depend, as Russell said, upon induction from particulars, or, do universals exist? Continuity is a universal. So is substance. The empiricist says: "Can you prove there is something real that you can call continuity? Relative to what?" Leibniz successfully and hubristically introduced the idea of continuity into physics and mathematics. He described it as

a principle of general order which I have observed. . . . This principle has its origin in the infinite, and is absolutely necessary in geometry, but it is effective in physics as well, because the sovereign wisdom, the source of all things, acts as a perfect geometrician, observing a harmony to which nothing can be added. This is why the principle serves one as a test or criterion by which to reveal the error of an ill-conceived opinion at once, and from the outside, even before a penetrating internal examination is begun. When the difference between two instances in a given series, or that which is presupposed, can be diminished until it becomes smaller than any given quantity whatever, the corresponding difference in what is sought, or in their results, must of necessity also be reduced, or become less than any given quantity whatever. Or, to put it more commonly, when two instances or data approach each other continuously, so that one at least passes over into the other, it is necessary for their consequences or results (or the



Niels Bohr Archive, courtesy AIP Emilio Segrè Visual Archives

***Boring and Boring-er**: Twentieth century physicist Niels Henrik Bohr (l.) and mathematician Richard Courant. Courant's desecration of Leibniz's discovery, quoted within, and Bohr's insistence on irrationality as the foundation of quantum physics have been two of the greatest contributions to the destruction of the Western tradition of scientific discovery in this century.*

unknowns) to do so also. This depends on a more general principle: that, as the data are ordered, so the unknowns are ordered also.

In the case of the tangent, the slopes, which are the unknowns, must yield the value at the point in question, i.e. the tangent at the point, when the data, that is, x and y become sufficiently close to the values of x and y at that point.

Leibniz directly discussed the nature of universals, such as continuity, in 1670, about two years before he began work on the calculus. He had been asked to write an introduction to a book by Marius Nizolius, written in 1553, called *On the True Principles of Philosophy, against Pseudo-Philosophers*. Nizolius, a nominalist, denied that a "universal is anything more than all particulars taken simultaneously and collectively, in Leibniz's words But, Leibniz pointed out, "if universals were nothing but collections of individuals, it would follow that we could attain no knowledge by demonstration—a conclusion which Nizolius actually draws—but only through collecting individuals, or by induction."

The nominalist says: "Induction from experience teaches us that if we put our fingers in the fire they will be burnt." But, without realizing it, Leibniz says, the nominalist is using

> the following universal propositions, which do not depend on induction but on a universal idea or definition of terms: 1. If the cause is the same or similar in all cases, the effect will be the same or similar in all; 2. the existence of a thing which is not senses is not assumed; and finally, 3. whatever is not assumed, is to be disregarded in practice until it is proved.

Thus, continuity is not merely something we infer on the basis of the observed proximity of a set of points. It works the other way. Because the universe obeys the principle of continuity, and because our mind, as part of the universe, obeys this principle, we can make inferences about the way successive points relate to one another, and about the way physical processes must work.

Leibniz made a useful reference to series in this essay when he said:

> Induction in itself produces nothing, not even any moral certainty, without the help of propositions depending, not on induction, but on universal reason. For if these helping propositions too were derived from induction, they would need new helping propositions, and so on to infinity, and moral certainty would never be obtained. By induction alone, we should never perfectly know the proposition that the whole is greater than its part, for someone would soon appear, and for some reason, deny that it is true in cases not yet observed.[18]

Thus, to explain the formation of series of numbers, Leibniz sought the process that generated the entire series. Beginning with the universal principle of identity, he was able to show how one series can be derived from another series. Also, with curves, he saw that there is a single process which generates the whole curve, but which is revealed at each very small interval of the curve. That is the true story of the invention of the calculus.

Ernest Schapiro, M.D., an organizer for Lyndon LaRouche's political movement, was a member of the biological holocaust task force, set up by LaRouche in 1974, and he co-authored two Executive Intelligence Review *Special Reports on the AIDS crisis, produced in the mid-1980s.*

Notes

1. Carl B. Boyer, 1959. *The History of the Calculus and Its Historical Development* (New York: Dover Publications).
2. J.M. Child (translator), 1920. *The Early Mathematical Manuscripts of Leibniz* (Chicago: Open Court Publishing Co.).
3. For the "Art of Combinations," see: Leroy Loemker (editor and translator), *Gottfried Leibniz: Philosophical Papers and Letters* (Chicago: Chicago University Press, 1976), p. 73ff.
4. Child, p. 30
5. Loemker, p. 265
6. For letter to the Grand Duke, see Loemker, p. 261
7. By diagonals are meant the slanted rows 1 1, 1 2 1, 1 3 3 1, 1 4 6 4 1. If you rotate the figure clockwise 45°, you see Pascal's triangle.
8. The reader can construct a three-dimensional model of this as a useful exercise.
9. Loemker, p. 73
10. Child, p. 147
11. Child, p. 150
12. Loemker, p. 447
13. Richard Courant, 1969 *What Is Mathematics?* (New York: Oxford University Press), pp. 398–99.
14. Courant, p. 434
15. Boyer, pp. 6-7
16. Boyer, p. 217
17. Boyer, p. 218
18. Loemker, p. 129

Twenty Years of Mitogenetic Radiation: Emergence, Development, And Perspectives

A translation of the great Russian biologists' 1943 review of the discovery and development of mitogenetic radiation.

Courtesy of V. Voeikov

Alexander and Lydia Gurwitsch, from a 1940s photograph.

by Alexander G. Gurwitsch
and Lydia D. Gurwitsch

Introduction

Twenty years' existence of a new discipline gives a substantial basis for critical review of its successes and failures. This is particularly true for mitogenesis, because such a review may throw light on a very peculiar, and, in our opinion, unprecedented chapter in the history of science.

The fate of mitogenesis is indeed very peculiar: A review by several authors, published in commemoration of the decennium of the discovery of mitogenetic radiation, appeared at the culmination point of the apparent success and acknowledgement of the new discipline.[1] Since then, the opposite trend has become more and

EDITOR'S NOTE

This 1943 article was translated from Russian by Dr. Vladimir Voeikov and Dr. Lev Beloussov, a grandson of the Gurwitsches. It first appeared in English as an appendix to the proceedings of the International A.G. Gurwitsch Conference, Sept. 28-Oct. 2, 1994, held in Moscow, titled Biophotonics: Non-Equilibrium and Coherent Systems in Biology, Biophysics, Biotechnology *(Moscow: BioInform, 1995). It was originally published in the Russian journal* Uspekhi Sovremennoi Biologii *(Advances in Contemporary Biology), 1943, Vol. 16, No. 3, pages 305-334. This translation has been edited in collaboration with Dr. Voeikov.*

A report on the second International A.G. Gurwitsch Conference, held in Moscow in September 1999, will appear in the next issue of 21st Century.

Editors' Notes from the 1995 Publication in *Biophotonics*

The editors of this volume found it appropriate to present, along with the conference papers, this review by Alexander Gurwitsch and his spouse and devoted assistant, Lydia Gurwitsch. It was written in the full swing of World War II, soon after the authors escaped from besieged Leningrad, where all the laboratory documents had to be left. Therefore, the bibliography is incomplete.

This review is of great scientific value, and still relevant. It is an exciting human document of a "little scientific tragedy," as Alexander Gurwitsch modestly noted in one of his papers. It is written with his whole heart. Gurwitsch demonstrates here self-criticism, pointing to his previous mistakes and delusions—a feature not frequently met among scientists. However, as regards the essence of his main discoveries, Gurwitsch strictly insisted on their objectiveness. He was disappointed by an inadequate and preconceived attitude of the majority of the scientific community.

Obviously, a scientist is free to accept or reject a certain concept based on the direct experimental facts, but the history of the problem, including the history of the emergence and development of theoretical ideas also means a lot. It seems that any reader, either completely unfamiliar with the problem, or even prejudiced against it, will be impressed by the sincerity of the authors' tone and by such an immense amount of a highly qualified and promising work made in this field within the first 20 years. Probably, he will become more open to the perspectives in this scientific field that are arising today.

The editors omitted about 10 percent of the original text; they hope to have succeeded in retaining and adequately reproducing the content and the style of the authors. Editors' notes appear in square brackets.

more evident: Interest and confidence in the new discipline have been gradually fading. Things have come to such a point that now the publication of the next, even modest collection of papers seems to be impossible. And the gravity of the current moment [the authors are referring to the wartime conditions of 1943] cannot be the only explanation for such a situation.

Meanwhile, we, being devoted to mitogenesis, are convinced that the real development of this discipline during the second decade of its existence was very fruitful. Its achievements have even exceeded the expectations of the broadest scientific circles. How could it happen that—under complete isolation and while seeming to fade on a worldwide scale—mitogenesis was undergoing a rapid and successful self-development? This question undoubtedly sounds like a challenge for anyone who seeks to reveal regularities in the general roots of evolution of scientific thought.

We are attempting to review here the overall relations between our laboratory and the scientific community. However, we would like to start from a general formulation of the reasons which led to the present situation.

The main reason for the hostility or, at least, the skepticism of scientific circles towards mitogenesis has been the absence of any connection of the discovered phenomenon to the already known facts, or rather, to their interpretation within the limits of the conventional concepts. Such a discovery can function psychologically like a bomb blast. One can hardly find in the history of biology similar cases of the unwillingness of scientific circles to accept the new fact. The second reason for skepticism, and the neglect that manifested itself later, but became dominating, was the non-classical character of the phenomenon discovered by mitogenetic methods. This conclusion can be illustrated by the words of the well-known physiologist Hill: "The new era would come for neural physiology, if the claims of the Russian authors were correct. . . ."

From the psychological point of view, such an attitude is easily understandable, although it is quite unworthy of science. Unfortunately, most of the researchers educated within the frames of a given set of theoretical concepts are unwilling to re-evaluate these habitual concepts, because giving them up is always an unpleasant task. These two motifs are, so to speak, natural. Conservatism is inherent in science and unavoidable.

However, along with the above-mentioned reasons, the attitude to mitogenesis was fatally influenced by some other circumstances, seemingly attendant and occasional. We mean the attempts to test our main assertions in other laboratories, both in our country and abroad. With the remarkable exception of the studies of a few authors who really contributed to the new discipline (among them we may mention Magrou and Magrou, Ziebert, Blacher, Wolf, and Zirpolo), all the other numerous tests—with either positive or negative conclusions—led the authors to express doubt. Some of them expressed severe criticism.

The latter came primarily from those authors whose aim is to test the existence of the phenomenon, but who do not precisely follow our recommendations with respect to methods and, in some cases, even deliberately violate them.[2]

Another reason for distrust of mitogenesis seems to be rather strange and forces us to suspect a certain unfairness of some representatives of the scientific community. Numerous reviewers from various countries, who never sought to carry out their own experiments and were often absolutely ignorant of the literature on the problem, are claiming that from year to

1. A.G. and L.D. Gurwitsch, 1934. *Mitogenetic Radiation.* Leningrad: The All-Union Institute of Experimental Medicine Publishing House. In Russian.

2. What we mean here is that our recommendation and restrictions are based on a firm empirical foundation. For example, our recommendation that the incubation period of a yeast culture should not exceed 2 hours, because the mitogenetic effects can not be detected after a longer period, was not taken into consideration by Schreiber and Nakaidzumi (1932). They tried to observe the effects after a 4-hour incubation period and certainly failed. In spite of the substantiated recommendation by Baron to use diluted cultures (no more than 200,000 cells/cm^2), Bateman and Kruechen (1934) used cultures 10 to 15 times more dense.

year the number of the negative results is increasing, while the number of corroborations of mitogenesis is decreasing. The discrepancy between such statements and the reality is so shocking that similar claims, if made on the matters of everyday life, rather than on scientific questions, would not go without punishment. Suffice it to say that, according to the last and obviously most complete review by Maxia (1940), several hundreds of confirming results coming from different countries could be opposed by barely a couple of dozen reports of negative results.

We abstain from elucidating logical and psychological grounds for such an unhealthy atmosphere around mitogenesis. In any case, the reference to the inherent conservatism of scientific thought would be wrong here. But it is important to say that we have never met in the scientific literature any motivated arguments revealing errors in our central statements or pointing to their physical unattainability. However, as shown below in more detail, our long labor has forced us to conclude that many of our initial suggestions and interpretations appear to be completely wrong. Therefore, it is not surprising that our theoretical interpretations of the experimental results have been changing during these 20 years. But we may declare with full confidence, that in all cases of recent tests of our initial experiments (even by the newcomers in this field), the results have always been confirmed.

From archives of L. Beloussov

Alexander Gavrilovich Gurwitsch (1874-1954)

Emergence of Mitogenesis And the General Trend of Reasoning

Many cases are known in the history of science in which erroneous assumptions have led to valuable results and even to discoveries. However, it is difficult to find any analogy to the emergence and development of mitogenesis. A long chain of theoretical considerations and conclusions, which finally led to the discovery of mitogenetic radiation, turned out to be a particular combination of successful and correct ideas, on the one hand, and quite erroneous speculations, on the other.

It would be aimless and tiresome to follow step by step our entire course of reasoning. We shall limit ourselves to the main stage of the path taken. However, we shall strictly preserve the reality, without reformulating the initial, immature or poorly substantiated reasoning according to our current logical consideration of the problem.

An inexhaustible interest in the miraculous phenomenon of karyokinesis was the starting point of the whole story. Having in mind purely cytological aims (shifting yolk platelets out of the animal parts of triton eggs), we employed an intense centrifugation of eggs. We were amazed by the resulting, chaotic picture of cleavage that disobeyed all rules and regularities and looked like an "occasional" event. Was it really so?

By a strange coincidence, just at that time, we became acquainted with a book on biometrics, containing an elementary account of probability theory. Using the elementary notions of normal, supernormal, and subnormal distribution, we could easily demonstrate on a number of subjects (and on onion roots in particular) that spatial distribution of mitoses obeyed purely random distribution. This result brought about the following chain of reasoning that turned out to be crucial for the further discovery of mitogenetic radiation.

Cell division is an "occasional" episode in a cell's life. Following the division of a parent cell, two sister cells may have quite different fates, even under completely identical life conditions. One of them may remain intact, while the other one may divide. From this, the following conclusion could be derived: An occasional event is the result of at least two mutually independent factors. To create a theory of the emergence of mitoses, we had to start with this dualistic concept. We know that such a formally correct conclusion from a largely unconvincing and biologically improbable statement led to the discovery of mitogenetic radiation. Only much later did it become clear that there are alternative explanations of the statistical distribution of mitoses, that are almost identical to the "occasional" one. It might be the result of a long succession of cell divisions, each having certain fluctuations of interkinesis duration. So it was biologically wrong to suggest the existence side-by-side of sterile and repeatedly dividing cells. However, if the initial idea was correct, that at least two independent factors were necessary for a cell division to occur, it would be

logical to assume that one of the factors is endogenous, that is, it coincides with the whole complex of the internal "ripening" processes (a factor of possibility), while another is exogenous, even if it originates in the same organism to which a dividing cell belongs (a factor of realization). Such a distinction of the two factors seems to be merely a theoretical construction; nevertheless, it found substantial support and had been introduced into science.

We point therefore to an example of a correct conclusion from the incorrect premise used in our argumentation. We shall see below that such a peculiar mixture of successful and erroneous conclusions occurs rather often.

After we had found experimentally that the impulses for cell divisions come from onion basement membranes, and after a long series of measurements and calculations, a very simple linear dependence between the surface area of meristem cells and the probability of cell division was revealed. The new, quite arbitrary and thoroughly inconceivable conclusions were derived from these facts. In spite of the physical naiveté of this reasoning, it provided the impulse for decisive experiments, instead of leading us down a blind alley.

Because, in those days, we were utterly seized by an unfortunate, preconceived notion of the complete "fortuity" of mitoses, and rejected any possibility of regular cycles of cell division, we suggested the following, quite artificial construction.[3]

Earlier we had demonstrated that the meristem cells grow exponentially. From this fact we drew the correct conclusion that cell surface growth has a strictly assimilative character. This conclusion is identical to an assumption that there exist two kinds of substances at the cell surface. The quantity of the first one (K) does not change with growth, and K occupies the same surface area during the whole growth period. The area occupied by the second component of the surface (A) is constantly increasing, because A is growing in an assimilative way. This suggestion would imply that during cell growth, the cell surface retains all of its properties. Thus, surfaces of small or large cells should differ only in continuous changing of the ratio of the variable parameter A to the constant parameter K. Now, what is the explanation of the simple dependence between the A/K ratio and the probability of mitosis?

Assuming that we are dealing only with an action of external factors upon the cell surface, we suggested first that those factors are similar to Haberlandt's hormones. In this case, the only plausible notion would be the following: The cell surface can be considered as a kind of mosaic in which K is a dispersed, and A is a continuous, component. It is clear that as A increases, the dispersion of K will increase as well. Hence, if K is permeable to a hormone but A is impermeable, the permeability of the cell surface to the hormone will gradually decrease. Under these circumstances, however, the relation between A and the division probability will be expressed by a fraction in which the A-variable is the denominator—that is, by a hyperbolic function. However, this dependence is in fact linear.

This discrepancy impelled us to reject the idea that division was induced by a hormone, and we moved further in the direction of risky and far-reaching speculation. The linear dependence can be expressed by a formula:

$$P = aK - A,$$

where P is the probability of a mitosis, and *a* is a coefficient.

It may follow from such a dependence that K is favorable but A is unfavorable for the action of an external factor. As A increases, more regions occupied by K are inactivated. but a certain residue of K retains its properties. One may suggest that K creates a regular mosaic which is gradually destroyed by continuously growing A. The fragments of the mosaic would lose their function under these conditions.

These considerations were followed by a very risky leap of thought. If the configuration of the K mosaic plays a decisive role, one may suggest that the perception of an impulse by the cell surface is based upon something which can be defined as a resonance. This suggestion leads to the following: The "factor of realization" which determines cell division is of an oscillatory nature, that is, it may have something to do with a radiation process.

It is difficult to understand now, how such a chain of arbitrary and physically rather naive reasoning could have led us to the valid result—the discovery of radiation. However, the first suggestion, that the cell surface is a decisive factor for perceiving mitogenetic radiation, seems to be true. This was quite clear at the very beginning, when the nature of the division factor was completely unknown. Such a conclusion could be directly deduced from the synchronism of cell divisions in various syncytia and polynuclear cells, as opposed to the high degree of randomness in the distribution of mitoses in most cell populations. On the other hand, the very idea of a resonance-like principle also contained a grain of truth, which could not be completely realized at that time.

Therefore, as now appears quite obvious, a reaction of the cell to one or several photons is possible only if the photon absorption triggers a chain reaction largely dependent upon the spatial arrangement of the molecules involved, that is, in general terms, upon the supramolecular order.

We shall now resume the review of our ideas, delusions, and researches that led us to the discovery of mitogenetic radiation. It is trivial to claim that knowledge is gained only after many errors and delusions. What may be instructive in our case is that blunders frequently intervened in the chain of our deductions, sometimes in its most crucial links. This happened repeatedly after the discovery of the phenomenon and in the course of its further investigation.

The First Experimental Results

After we had offered a risky suggestion that some form of radiant energy constitutes an exogenous factor, we ran into a number of problems and found ourselves in a rather miserable situation. Because the visible and infrared parts of the spectrum could be rejected, only two possibilities remained: either ultraviolet or some new, unknown kind of radiation. It was natural that we tried to investigate the first case.

As followed from our previous experiments, only an onion

3. It is clear now that the most simple and biologically plausible explanation of the linear dependence of division probability on cell surface area is that the duration of interkinesis increases in parallel with cell growth.

basement membrane could be a source of radiation. It seemed reasonable to suggest, in addition, that rays could travel along the whole axis of the root (not less than 10 to 12 cm long) towards the meristem. The latter could be reached only by a beam of parallel rays.

Strange as it may seem, the chain of our initial reasoning ended here, and we did not immediately reach the natural conclusion that at least part of this beam could be emitted from the root. Such a simple idea came, as a kind of a revelation, only a few weeks later, while I was walking; this may be qualified as one of an inconceivable number of cases of inconsistency and a lack of logical of thinking.

What came directly from the initial hypothesis was a rather doubtful idea to trace the spreading of rays in a bent system, in the hope that in this case some regions of the meristem zone would be "illuminated," while others would be "shadowed." A large series of experiments gave the expected results, that is, those calculated as a function of a root distortion. But we regret that we published them. Both the constructions and the assumptions are too complicated, and the results could be explained on the basis of quite different hypotheses. Experiments with frog cornea wounding performed at the same time had similar drawbacks. In the latter case, one extensive rounded wound was made by a heated needle and another wound, having a linear shape, was made at some distance from the first one. We suggested that the impulse to mitoses coming from the first wound is able to spread throughout the whole cornea, and that it will be at least partly screened by the linear wound. These experiments also gave some results which could be interpreted as positive. It is interesting to note that these doubtful considerations and results were accepted with wider sympathy (for example, by Wasserman) than the later data which were reliable and unambiguous.

Courtesy of Vladimir Voeikov

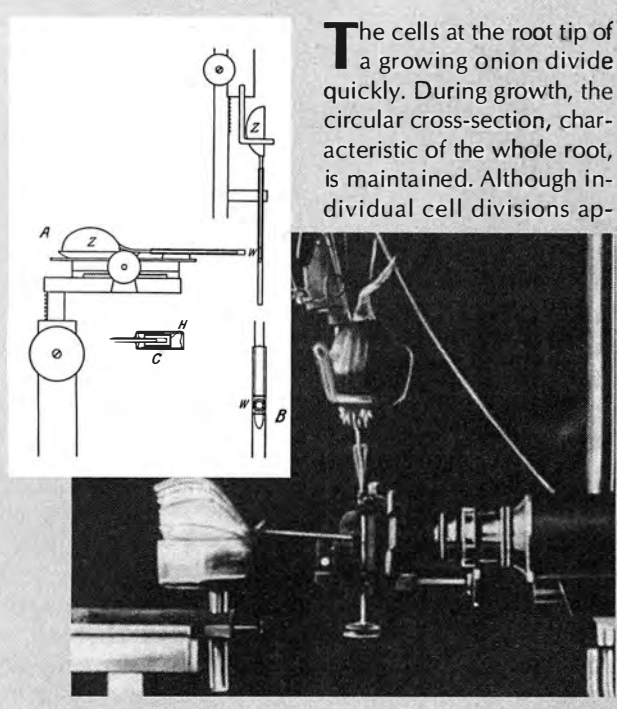
Some of the participants at the First International Alexander Gurwitsch Conference on Non-equilibrium and Coherent Systems in Biology, Biophysics, and Biotechnology, held at M.V. Lomonosov Moscow State University in Moscow, September 1994. In the first row are Prof. F.-A. Popp (third from left) and Prof. Lev Beloussov (fourth from left), co-chairmen of the conference. In the background is a statue of M. Lomonosov and the main building of the university, which he founded.

The Main Experiment

Even before we reached, at last, the conclusion that if the main hypothesis were correct, emission should come from the root's tip, we started to look for an appropriate detector. And the first successful idea, after so many unsuccessful ones, finally came. Only a set of cells which is capable of dividing normally but, at the same time, is sensitive to the action of an exogenous "realization factor" increasing the division probability, could be used as a detector. The result of such an action—that is, according to our hypothesis, irradiation—could be detected only in a comparison with a control set of identical cells not affected by the same factor. The well-known onion root could serve again as the most suitable subject for such a study.

Straight roots are completely symmetrical. Thus, they can be divided into two equal parts by any medial plane. The

Gurwitsch's Famous 'Onion Experiment'



The cells at the root tip of a growing onion divide quickly. During growth, the circular cross-section, characteristic of the whole root, is maintained. Although individual cell divisions appear to occur in an unordered, even random distribution, the number of divisions in all directions from the axis must nevertheless be approximately equal. The root would otherwise not have a cylindrical form.

Gurwitsch supposed that at least some of the cells must be emitting light that regulated the rate of division of the other cells; he proved it by means of the experimental setup shown here. The roots (W) of two onions (Z) were positioned perpendicularly, so that the tip of one root pointed to one side of the other root. He then examined under the microscope the second root, at the site facing the tip of the first root. He was able to establish a statistically significant increase in cell divisions there, compared to the opposite, "unirradiated" side. This effect disappeared when he placed a thin piece of window glass between the two roots, and reappeared when he replaced it with quartz glass! That meant that ordinary glass is opaque for mitogenetic radiation, while quartz glass is translucent. Hence electromagnetic radiation must be operative, and ultraviolet light in particular, since it passes through quartz, but is stopped by window glass.

Source: A.G. Gurwitsch, *Das Problem der Zellteilung* (The Problem of Cell Division), 1926

number of mitoses in both parts is almost equal. Although in different roots the total number of mitoses is in the range of 1,000 to 2,000, the difference between the two halves of the dicotyledonous plant is no more than 3+ to 5 percent. Even in two halves of the same section, the difference is only a bit greater. Therefore, the difference between an irradiated and a "shadowed" half caused by a unilateral, strictly localized irradiation should be quite distinct.

The first experiments immediately gave brilliant, decisive results. Further experiments confirmed the initial data. This requires discussion in more detail because the persuasiveness of even a single experiment appears to be much greater than one might initially suggest.

As radiation is detected from a root tip at a distance of several millimeters from the surface of a detector root, the rays, for purely physical reasons, should be directed at the normal. Thus, they should act only on that part of the detector surface that intersects the extension of the axis of the radiating root. Therefore, if the detector root is dissected into a series of longitudinal sections coinciding with this direction, the effect—that is, the excess of mitoses on one side compared to the other—should be expected to take place only within the medial and paramedial sections, and in any case should decrease on both sides of the medial plane.

At that time, we already knew that there are fluctuations in the number of mitoses, equally probable for both halves of the section. If the probability of an excess within one section is 0.5 , the probability for x successive sections should be $0.5\ e^x$. That means that if $x = 5$, this probability should be as small as 1/64. The following also confirms this reasoning: The probability of successive unilateral excesses of the number of mitoses in the absence of irradiation would be the same for both exactly medial and much more laterally prolonged sections. When, under medial irradiation, the excess is detectable just within the medial, rather than the lateral regions, the probability that the observed phenomenon will be of a purely occasional nature becomes very low.

Finally, the last and most convincing argument is that the excess can be observed within several medial sections of the induced side; this amount was several times greater than the level of statistical fluctuations in other sections. We see therefore that the result of even one experiment can be convincing.

After it was observed that the effect is not inhibited by inserting between the roots a thin glass plate (several dozen micrometers thick), but the effect disappears when a cover-glass was used, all doubts about the radiant character of the impulse were gone. It became clear that we were probably dealing with ultraviolet irradiation. Strict evidence of this was obtained much later.

Critical analysis of our interpretation of this undoubtedly real phenomenon should reveal the following point, which escaped us at that time, and escaped our critics, who doubted the experimental fact itself. If the assumptions that the irradiation is coming from an onion basement membrane and that we are dealing with ultraviolet are correct, it seems impossible that radiation emerging from the onion basement membrane could pass a distance of 10 to 15 cm,

through a meristem layer, down to a root tip in a medium rich in proteins.

We realized this problem much later, when we had already observed the phenomenon of "secondary" radiation under the influence of irradiation. The idea of secondary radiation came to us when we recognized that the effect of irradiation, that is, the excess in the number of the mitoses on the irradiated side, is spread along the meristem up to 1.5 to 2 mm from the area that was briefly irradiated with a point source. The diameter of this area did not exceed fractions of a millimeter. If an irradiated cell transmits the same impulse further, by way of secondary radiation, it is obvious that the photons reaching the meristem are generated not in the onion basement membrane but in the vicinity of this very cell which started to divide after absorbing the photon.

In a short time after our first studies had been published, various laboratories reported their own results, both positive and negative. Very soon the regrettable situation already mentioned in the introduction became obvious. With the exception of a quite reliable investigation of Magrou and Magrou (1927), which was a corroboration of the discovered phenomenon, both the positive results by Wagner (1927) and the refutations by Rossman (1928) and Moiseewa (1931), as well as many later studies, were of no significance.

We shall discuss these papers from the general point of view. Our opponents, of which Rossman is an example, usually claim that in several experiments that they performed, the radiation effect could not be reproduced. They opposed their results to at least several dozens and, later, more than 130 sets of experimental data which we have published. However, we pointed out that we published all of our results, which means that there was no lack of the effects observed. In spite of that, our opponents insisted that the described phenomenon does not exist at all. We consider it reasonable to ask the following direct question: How can the authors explain our numerous positive results—by a systematic error made by all of the authors, or by their dishonesty? This question has hardly any answer worthy of the attention of the scientific community. We consider that negative results may be used as a refutation of the positive ones only in some exceptional cases.

On the other hand, it is not difficult to explain the sources of the negative results. Usually,t hey came from methodological errors. For example, Rossman used roots of *Leguminosae* as a detector. This dicotyledonous plant obviously has a certain bilateral symmetry, even in a hidden form (instead of the required radial symmetry). He centered the roots with the naked eye, instead of using the horizontal microscope, and so on.

We pay such attention to these incidents because of their general importance, because they are equally applicable to further "refutations" of our data obtained with the use of biological detectors and also to some investigations in which physical methods have been used. At this point we finish what can hardly be qualified as "scientific polemics." We had to mention these sorrowful facts in our retrospective review only because we were often saddled with the reproach that we ignore weighty objections.

Corroboration of the Ultraviolet Nature Of Mitogenetic Radiation

The hypothesis that mitogenetic radiation belongs to the ultraviolet range was experimentally corroborated rather soon. First, it was found that a crystalline quartz plate is completely transparent to radiation, while even the thinnest gelatin plate is nontransparent. Second, Gleb Frank made the first spectral analysis of radiation from a biological source, which was muscle tissue. Finally, it was established, in collaboration with Frank, that the positive effect upon roots can be obtained from the spectrally dispersed UV from physical sources, if the intensity of emission is considerably reduced and the time of exposure is very short. Incidentally, the latter finding disproved an established view that the biological action of UV can only be inhibitory. It became evident to us (but, unfortunately, not to biologists at large) that the mitogenetic phenomena imply very special microevents, which can be neither corroborated nor disproved by employing UV of commonly used intensities and doses.

The second way to prove the identity of mitogenetic radiation with UV is to use purely physical methods which overstep the limits of our competence. Therefore, they will be mentioned only briefly here. Rajevsky's data, published previously, were not quite convincing from the standpoint of quantitative criteria. Later, he obtained additional quantitative data and thus removed any doubts.

Numerous attempts were made to use physical methods to detect mitogenetic radiation that were capable of reproducing the data obtained with biological detectors. Some of the favorable studies, however, could not be considered technically perfect. As well, several reliable, positive studies (for example, Frank and Rodionow 1932; Barth 1937) were criticized by authors who could not detect radiation. And again it was convincingly demonstrated (Barth 1937) that the negative results may appear because of the deficiency of the devices, but mostly because the authors neglected our recommendations and used unsuitable sources of radiation.

However, a substantial study by Krost and Peuchert, and the extensive, and largely acknowledged studies by Audubert, completely clarified the situation. Mean-spirited attempts of the latter author to conceal the truth and neglect the relationship of his data to our own are a manifestation of the grievous symptoms of modern scientific ethics, but they cannot change the essence of the matter.[4]

Further Development of Mitogenesis

The above-mentioned experimental findings were obtained while still in Simferopol [up to 1924] and during the first period of our work in Moscow [1924-1929]. Later, particularly because of the introduction of a new detector—

4. It is interesting that in some reviews of the discovery of mitogenetic radiation, even in favorable ones such as Huxley's, it is stated that I (A.G.) initially claimed to have discovered some specific "rays of life," and this notion is still widely used in low-pitched popular literature. It is scarcely necessary to say that this is pure fantasy on the part of the authors. A reasonable initial cautiousness in our first report, with some doubts as to whether the new phenomenon could be related to a poorly studied range of Lyman wavelengths (shortwave UV), was probably not adequately estimated. But one should be astonished that it is possible to ascribe such a childish idea as "rays of life" to me.

From archives of L. Beloussov

The Gurwitsch laboratory at Simferopol (1923-1924). Gurwitsch is first row, second from left; his wife is third from left.

yeast cultures (M.A. Baron)—the scope of our work increased tremendously. At the same time, the main lines of investigation began to branch off rather chaotically. Such a situation seriously complicates the main task of this review, namely, to attempt to present a systematic account of the development of the general idea. The rest of this account, therefore, will have a rather fragmentary character. Moreover, we have lost the opportunity to use our notebooks [those left behind in besieged Leningrad] and thus to recover the exact sequence of our thoughts and the motivations of the new trends in our work. However, we can say that we were guided by the following major considerations.

First of all, it was but natural to see whether mitogenetic radiation is a general biological phenomenon, and we still remember our excitement when we succeeded, while still in Simferopol, in detecting emission from certain animal tissues (amphibian tadpoles). But later, when it turned out that all the tissues were emission sources, the problem certainly became much more complicated and required new, extensive studies.

We would like to mention one incident which put forward the problem of emission from blood. When studying photon emission from early chicken embryos cultivated in physiological solutions outside the egg, we ascribed the initial negative results to the lack of blood circulation. We came to the idea of studying blood as a possible general source of radiation. However, it was established later that the negative results appeared to be caused by the presence of Bunsen burners for heating embryos in the near vicinity of the sample. The burners were found to emit much more intense UV than mitogenetic radiation.

The addition of blood to the range of experimental materials initiated a new trend in our studies that persisted for a long time. What was done later in this field was mainly the accumulation of occasional facts and the results of scientific curiosity, rather than of a well-defined and substantiated plan.

This applies mostly to the studies of blood radiation under the influence of various diseases. Many experiments of this kind had already been performed by L.D. Gurwitsch at the very beginning of the blood studies. In any case, one of the most important chapters in mitogenesis—the study of the "cancer extinguisher"—did not at all emerge as a link in a logically developing chain of events.

An extensive study of various plant and animal subjects yielded results that could not be adequately interpreted in the early period of our studies, and still cannot: Radiation could not be observed in all tissues. For example, it was not detected in parenchymous tissues characterized by intense metabolism (liver and kidney). So long as this fact remains enigmatic, one cannot interpret the mechanisms of radiation arising in living tissues. Up to now, therefore, there are no satisfactory solutions to the questions related to the universality of radiation and the conditions of its emergence in living systems.

Moreover, the main problem of mitogenesis, namely, the analysis of the basic mitogenetic phenomenon—of stimulation of cell division by UV photons—was for a long time con-

sidered ambiguous. It is obvious now that consideration of mitogenesis using the concepts of the usual classical optics was wrong, because the latter can be applied only to high light intensities and completely ignores quanta of light. We tried to analyze the results of our experiments with intermittent irradiation, as well as creeping effects and the mechanism of a continuous increase of intensity, and so on, assuming the concept of continuity of light. For example, in experiments in which the detector was irradiated with intermittent light, this treatment was considered a strictly rhythmic process. However, it now became clear that, with the rotation frequencies and angular values of sectoral slits used in the experiments, some of the slits did not let even a single photon through. [The routine method involved the insertion of a rotating disk with one or several windows between the radiation source and the detector.] It is obvious that any attempts to analyze the process of mitosis induction with inadequate equipment were doomed to failure.

It was very important to realize that the process of stimulating mitosis with mitogenetic radiation is based on the chain reactions that could be triggered by a single photon. A study of the chain reactions became possible only because of the discovery of the secondary radiation, and because of the results of studying processes developing in nonorganized systems (homogeneous solutions) after their irradiation. A study of these phenomena appeared to be so complicated that we were diverted for a long time from the main task—the elucidation of mechanisms of the mitogenetic effect.

But there was another reason for our excessively slow movement toward the solution of the main problem. For many years we could not establish whether mitogenetic radiation was really a specific factor that triggers cell division. To prove its specificity, one should arrest cell proliferation completely by the action of a certain factor *X* and then restore it completely by irradiating the detector from the outside.

Such a crucial experiment was first performed by Zalkind. It yielded the results that permitted us to move forward. The study was made on yeast cultures in a liquid medium. By adding a negligible amount of the so-called "cancer extinguisher" from the blood of a cancer patient, a complete, temporary arrest of cell proliferation can be achieved. After irradiation of the culture from the outside, proliferation was renewed and brought to its initial level. It was also demonstrated that the addition of the extinguisher, besides suppressing proliferation, inhibits mitogenetic emission from the culture itself. The extinguisher did not disturb any other conditions necessary for cell division, except for self-irradiation of the culture. Thus, it was found that external irradiation completely substitutes for self-irradiation of the culture in the process of initiating mitogenesis. These experiments have proven the specificity of mitogenetic radiation as a factor inducing mitosis in cultured cells which are ready for this event.

Only later did we establish that not only the impulse for a premature division, but also the development of the mitotic process, is controlled by mitogenetic radiation. A series of investigations of photochemical processes opened the possibility for rational analysis of the crucial role of UV photons.

We turn now to the recent investigations that clarify the mitogenetic action of UV photons. Irradiation of peptone solutions or of a mixture of amino acids (which should include at least one dicarbonic amino acid, that is, glutamate or aspartate) induces their polycondensation into peptide molecules. This conclusion is based on the susceptibility of these peptides to cleavage by pepsin that was revealed by the detection of a typical mitogenetic "peptide" spectrum after the action of a pepsin. To initiate the process in an amino acid solution, the photon energy should exceed 105 kcal/mol. This energy may be supplied either by a single photon with a wavelength not exceeding 270 nm, or by two photons. The energy of the first should be not less than 87.4 kcal/mol (326 nm), while the second can belong to the visible or infrared range with an energy limit of 18 kcal/mol, that is, around 1,500 nm. Such sharply limited energy requirements may be explained as follows.

Amino acid polycondensation requires cleavage of one hydrogen atom from the amino group of one amino acid and of the hydroxyl residue from another. The first event requires 87 kcal/mol, while the second requires 66 kcal/mol. Total energy expenditure for both processes is completely compensated by two exothermic processes: the formation of a water molecule and a peptide bond, CO-NH. The additional 18 kcal/mol, which is necessary for the process, represents probably the energy of activation. We established that from 326 nm, up to the short wavelength limit of a quartz spectrograph, effectiveness of the UV radiation depends exclusively upon the degree of UV absorption by the peptone or amino acids, rather than upon the photon wavelength.

Polycondensation may also take place under other experimental conditions. After addition of a very dilute liver extract to a mixture of amino acids or to a peptone, polycondensation occurs without UV irradiation by bright visible light, but not in the dark. It was found that diluted liver extracts irradiated with visible monochromatic light, emit UV with a wavelength roughly corresponding to the energy of two photons of the monochromatic light. In this case, the process of polycondensation is triggered only when the energy of the resulting UV photon exceeds the value mentioned above.

A detailed investigation of the energetic parameters of the mitogenetic action of UV has shown that they coincide precisely with those required for polycondensation. All of the above-mentioned facts remove every doubt that the mechanism of mitogenetic action of UV photons consists in, and is limited by, the stimulation of the processes of peptide synthesis.

In relation to the main mitogenetic effect, the cancer problem first attracted our attention long ago. An extremely low level of mitogenetic radiation in cancer patients' blood, because of the presence of the extinguisher, led us to look for the place of its formation. The mitogenetic analysis provided a definite answer: The extinguisher is a product of cancer cells and is obviously located in the superficial monofilm covering the cell, together with the specific enzymes: glycolytic, proteolytic, phosphatases, and probably some others. Their specific feature is that they bear negative charge, while the corresponding blood enzymes are charged positively. Both the extinguisher and the enzymes can be obtained by washing intact cancer tumors. Using spectral

analysis we obtained evidence that enzymatic activities of proteases and peptidases in cancer cells are located mostly epicellularly. That means that their substrates are derived from nutrient medium as well as from tissue elements located in close vicinity to the cancer cell. On the other hand, the intercellular splitting of proteins in the cancer cell is obviously minimal.

Such a peculiar localization of peptidase activity can determine an extremely parasitic character in cancer cells. Along with some other discoveries, the following fundamental fact that reveals the origin of cancer was discovered: It appeared that a number of tested carcinogenic substances emit mitogenetic radiation, while some similar chemical substances, including noncarcinogenic resins, lack such a capability (Kannegiser).

Taking into consideration the peculiarity of the mitogenetic state of cells exposed to a source of radiation for a long time, as well as the versatility of the action of UV photons upon cells, we may suggest that UV photon emission by carcinogenic substances plays an important role in their action.

The problem of the basic mitogenetic effect was studied for many years with ups and downs. It was typical of all of the important problems that we had been studying during the preceding two decades. We would not be surprised, if someone said that our school gives the impression of having no principal idea, or that it uses the trial-and-error method in research work. One should never forget, however, that mitogenesis did not have any relationships with the allied sciences, and that all of our results had a "non-classical" character. That is, they did not fit within the usual cytological, chemical, or physical concepts. We therefore had to propose new concepts for approaching problems, which seemed, at first glance, to have no connections among them. Later it turned out that these problems are interrelated. Sometimes it becomes possible to make a step forward in solving a certain problem only with the help of new data obtained in the neighboring field of science, which depends, in its turn, upon some other, often remote, field. After successful achievements in one direction, further progress may be delayed or even blocked for a long time, until new steps in some other direction open the way.

The present status of mitogenesis is, to a considerable extent, determined by the method itself, which appeared to be really miraculous because of its sensitivity. Mitogenesis implies microprocesses in both the organized and non-organized systems. These microprocesses are "non-classical" in the sense that, on one hand, they do not allow easy extrapolation into the realm of macroscopic events and, on the other hand, they seem to proceed independently of the latter. However, they are usually shadowed by macroscopic processes that can be easily studied by the classical methods.

Each section of mitogenesis is supposed to be a narrow gateway into an immense new field. Let us analyze some of them.

One of the main achievements was the discovery of so-called "degradational irradiation," described in the following brief history. Studying the mitogenetic process of corneal epithelium, Yu. N. Ponomareva came across the fact that irradiation of the enucleated frog eye results in an increase of the number of mitoses only 20 to 25 minutes after enucleation. It was found, at the same time, that the enucleated frog eye did not itself emit during the whole refractory period, and that restoration of its own radiation occurred just at the time when its epithelial cells recovered their ability to react to external irradiation. At first, she [Ponomareva] could not find the explanation for this finding.

Investigations of the effect of cooling the cornea revealed a phenomenon that seemed to be quite paradoxical: a very strong mitogenetic effect was observed when a freshly enucleated cornea, being cooled down to 2° to 5°C, was subjected to external irradiation. Trying to explain such a strange finding—that cooling is a factor that favors the effect of the external irradiating source—we came to the conclusion that under the condition of rapid and intense cooling, the corneal tissues start to irradiate themselves, though this suggestion seemed to be rather unlikely.

This somewhat improbable suggestion was confirmed in the very first experiment: A burst of radiation under such a treatment lasted for about 5 minutes. This fact had innumerable consequences in our further investigations. Here again the work was of the same irregular and scattered character as in the other fields of mitogenesis. The research bifurcated immediately into two lines: First, much attention was directed to purely phenomenological aspects of the newly discovered phenomenon; its universality and the specificity of the emission spectra had nothing in common with the spectra of the same organs and tissues under physiological conditions. Second, attempts to provide a theoretical interpretation for these unexpected events were undertaken, and led us to some new, and again non-classical concepts, such as "non-equilibrium molecular constellations" (NMCs). The latter was, in fact. a logical jump, because it was based on intuition, which gave us a feeling that NMCs constitute the main apparatus for performing quite varied processes that can be considered the basic manifestations of life.

These findings were followed by a series of experiments which, at first glance, had nothing to do with the preceding ones, but whose results exceeded all of our initial expectations, nevertheless. It turned out that negligible deviations in metabolic processes and a wide variety of excitations of biological subjects induced degradational radiation differing in its spectral pattern and in the character of evolution of the spectra. The results convinced us that, first, an important and even a major reevaluation of what we mean by the term "protoplasm" should be made, and, second, new methods for classical physiology should be proposed for the purpose of revealing mostly intimate functional relations among various systems and organs, avoiding the usual techniques.

The spectral analysis opened really unlimited perspectives in quite different areas. Initially it was used by Frank only for demonstrating that mitogenetic rays really do belong to the UV range. But after we obtained—mainly to satisfy our curiosity—the first rather sharply outlined spectral band, the spectral analysis became an autonomous field of investigation. There was a great temptation to apply it in some other fields that otherwise would not attract our attention, for example, in nervous system research.

We did not expect that, by performing a spectral analysis of

nerve fiber emission, we would be able to demonstrate a qualitative variability of excitation processes. Nevertheless, the very first, timid experiment appeared to be the starting point for the development of one of the most fruitful and important areas of mitogenesis. As a result, we came to the following major conclusions: (1) the emitting substance of the nerve elements is at the same time the target of nerve excitation; (2) the analysis of mitogenetic phenomena makes it possible to penetrate into the essence of the molecular substratum of excitation processes.

In light of new data concerning degradational radiation, the following concept of neural excitation was formulated (A.A. Gurwitsch): An excited substratum consists of NMCs which form a three-dimensional "continuum" within the brain cortex. Even under physiological conditions, the emission of the elements of the neural system has the character of degradational radiation, that is, it arises as a result of continuous disintegration of NMCs just after their formation. According to these views, the substratum for neural excitation is a continuously oscillating system. Such a concept made it possible to create a rather coherent system interpreting the various properties of neural excitation.

We shall finish our review with a brief account of a large number of investigations with great prospects, and a very peculiar path of development.

Starting from our earlier data about the disappearance of blood radiation that correlated with various deviations from a normal physiological state, our collaborator S.N. Brainess suggested that grave physical exhaustion should also be accompanied by a temporary suppression of radiation. His investigations completely supported this idea. As a development of this idea, it was suggested that blood radiation should also be suppressed in the depressive psychical state which, according to Brainess, has some similarity to tiredness. On the other hand, he supposed that in maniacal states, the intensity of blood radiation should be higher than in the normal state. These speculations were completely supported experimentally.

Then, Brainess took a rather risky step. He attempted to cure patients in depressive physical states by injecting a small amount of blood from a maniacal individual. The result was positive.

The spectral analysis of "maniacal" blood radiation (the latter was not only intensive, but emitted mitogenetic rays for a long period after the blood was taken) revealed a single narrow spectral band in the 229 to 230 nm range. According to our previous data, this band corresponded to the fluorescence spectrum of amino groups liberated because of oxidative deamination of amino acids present in blood serum. Hence, "maniacal" blood is characterized by its increased capacity for deamination, while "depressive" blood, on the contrary, by a sharp decrease of this capacity.

It was of considerable theoretical interest that the therapeutic effect of a single injection of a small amount of "maniacal" blood into a depressive patient appeared to be rather prolonged. Suggesting that the therapeutic effect of the "maniacal" blood injections is directly linked with its increased irradiation rate, the author took the following step, although there was only a weak theoretical basis for it: A sample of citric blood from a depressive patient was irradiated with a mitogenetic source and injected back into the patient. This procedure had a good therapeutic effect. The next step was substitution of irradiation of blood for that of serum and then of irradiation of amino acid solution, which also demonstrated the healing effect after injection into patients. The latter procedure was based on the previously discovered remarkable fact: An irradiated amino acid solution itself became a source of mitogenetic rays acting for many hours—almost a whole day. It retained a therapeutic effect when injected at any time within this period. This led to a suggestion, that the injected active substance, which was analogous to the oxidative deaminase enzyme, was capable of self-reproduction with the organism. Such an idea—more intuitive than logically grounded—became an initial point for a new, extensive branch of research into mitogenesis.

[Here the authors discuss further the results obtained in the studies of self-reproduction of oxidative deaminase activity and of activities of various other enzymes in aqueous amino acid solutions irradiated by mitogenetic rays. The more detailed, up-to-date consideration of this highly exciting and puzzling problem is presented in the paper by A.G. and A.A. Gurwitsch, "The Problem of Autocatalysis (Autoreproduction) of Some Cyclic Compounds from Lower Amino Acids," *Enzymologia,* Vol. XX, pp. 1-16, 1958.]

Let us now summarize the complicated development of investigations and the present-day status of mitogenesis. One might forgive and forget its intricate, error-laden history of two decades if, at present, our major results and conclusions were really regarded as trustworthy. However, according to a viewpoint that dominates in scientific circles generally, the situation is unsatisfactory. Our own viewpoint is quite different, although we would certainly like the situation in this field to be much better than it really is. However, our motivations differ from the reasons of our opponents. As we have often mentioned, we cannot point to any case of a refutation of any of our positive experiments, that is, those demonstrating the existence of mitogenetic radiation. Thus, we have the right to object to a skeptical and distrustful attitude toward our data.

What seems to be absolutely unsatisfactory, and even hopeless, is the completely isolated position of the science of mitogenesis among other, neighboring sciences and the resulting fruitlessness of both our data and our theoretical constructions.

We might consider it rather natural and, at the beginning, even tolerable, if our colleagues, while remaining skeptical with respect to the theories derived from mitogenetic facts, accepted our experimental results as real. These facts could then be introduced into the everyday usage of the firmly established disciplines and, even if the conclusions drawn from them by specialists might differ considerably from our own, that would probably affect only our personal ambitions, rather than the interests of science. In any case, we would be satisfied that some fundamental problems within various disciplines should be seen in the light of new discoveries.

However, just because the acknowledgement of our results would lead to an inevitable break with habitual concepts, scientific circles prefer to abstain from accepting them. Actu-

ally, the hasty phrase of Hill, who we mentioned above, is a good illustration. As mentioned, mitogenetic spectral analysis of active nervous tissue brings new concepts to the fore, which are incompatible with the poor idea of the electric character of nerve excitation. The unwillingness to break with these deep-rooted concepts is understandable and pardonable. What is completely inadmissible is to justify one's own quietism by claiming, as Hill does, that "all of this seems to exist only in the fantasy of the Russian authors."

We cannot accept the facts of the rejection of our methods—methods that might be an inexhaustible source of new data. Nor can we accept an attitude of indifference to our results, which forces us to extend our studies into some fields in which we feel ourselves mere amateurs. No doubt, the specialists are able to treat the same problems on a higher scientific level. However, because they show no interest in our research, we have to do this job by ourselves, being satisfied if the data obtained are at least trustworthy and if the related theoretical concepts do not contradict those established in other disciplines. Even when our statements seem to be incompatible with generally accepted views, this cannot be a reason for the automatic rejection of the facts.

Our last task will be to outline the near, and probably more distant, future of mitogenesis. In this context the following question arises: Does mitogenesis remain as an unresolved problem, as a kind of scientific enigma, or, on the contrary, are the foundations of mitogenesis sufficiently elucidated to enable its use as a new, highly valuable concept applicable to a variety of fields in the natural sciences? Certainly we cannot draw boundaries between these alternatives. We can only speak of one of them predominating.

We suggest that the second part of the question—the use of the mitogenetic method—will prevail and will gradually push into the background the first, the question of mitogenesis itself.

Within the limits of the purely mitogenetic problem we can raise only two questions: What is the origin of weak UV emission from homogeneous and organized systems, and what are the mechanisms of the mitogenetic effect, that is, of the control of cell division? We find it warrantable to claim that the main question of the origin of UV emission during chemical reactions is elucidated to such an extent that further work will flow in a common physico-chemical channel, and thus UV emission will no longer remain a specific enigma for this field of science.

The situation with degradational radiation seems to be much more complicated. Of course, our explanation of this phenomenon is simply a preliminary construction in need of further development. We suggest that in the near future it will be the central, and perhaps even the only, problem of mitogenesis. It has already become a foundation for new concepts related to the main problems of biology—a field theory.

As for the the main mitogenetic effect—the induction of cell divisions by one or several photons—the striking parallelism between the action of weak UV of mitogenetic intensities upon amino acids, peptides, and whole cells, leaves no doubt that the mitogenetic effect of UV is identical to that of splitting (oligo)peptide (amino acid) molecules with a high probability of detaching hydrogen from the amino group. This process triggers the peptide synthesis. Why such elementary reactions lead to cell division is certainly one of the most important and difficult biological problems, and mitogenesis plays its role here.

However, the initial and intermediate links of the radiation-stimulated process, the intermittent irradiation, the transformation of stimulation into inhibition, and the striking disproportionality between the number of absorbed photons and the number of initiated mitoses, all remain obscure.

As we have mentioned, mitogenetic methods disclose microevents that cannot be extrapolated, as a rule, to macrosystems. This can throw doubt upon the prospects of studies in this direction. We believe that this area should be highly fruitful and that the discovery of microevents may be of great importance. First, these events may serve as signals of the existence of other microprocesses escaping direct observation. Application of our methods to the study of neural processes appeared to be fruitful for the most *part.* Here the role of mitogenetic radiation may be similar to that of "action currents," the latter also being the signals of something happening. Because mitogenetic effects show extreme sensitivity, and especially quantitative variability, they have an advantage over electrophysiological methods. The mitogenetic method permits analysis of the subject and the process of excitation. Just as the familiar form of spectral analysis became the foundation of modern concepts of atomic and molecular structure, mitogenetic radiation—being a signal of molecular processes—provides the possibility of deeper penetration into the properties of the excited biological substrata.

Only the future development of science will show whether the wall separating classical and mitogenetic physiology will crumble.

The same can be said of the mitogenetic analysis of the cancer problem. Here, also, we have no connections with classical oncology, except for some interest in the extinguisher phenomenon for cancer diagnosis. We consider these results less important than other mitogenetic data related to carcinogenesis.

An even more extensive field for mitogenesis has been opened by the discovery of degradational radiation. The amazing sensitivity of the degradational spectra, reflecting even minute influences upon living systems or particular organs, provides new means for elucidating the functional interrelations between different systems. If, indeed, an irritation (excitation) of a certain system A results in changes of the spectral composition of system B, the latter being, at first glance, quite independent of A, we should claim that the two are interconnected, even if this can not be registered by routine methods. But even in those areas where such connections are detected by classical methods, the use of degradational methods may disclose some new regularities.

Development of this boundless field belongs to the future. Mitogenetic methods should be in common use in physico-chemical studies. Yet, the technique for detection of free radicals, which we have elaborated, has been almost completely neglected up until now, although the appearance of free radicals is not just a mere signal; sometimes this event is important for understanding the mechanism of a process. This concerns, in particular, the analysis of those enzymatic processes

From archives of L. Beloussov

Gurwitsch's laboratory in Moscow in June 1948. Gurwitsch is second from right, first row. His daughter, Anna, is third from right.

in which only the final products, but not the intermediate ones, are known. This is true even for such a simple system as, for example, urease + urea. Resonance illumination of this system with mitogenetic rays made it possible to detect the existence of free radicals such as the carbonyl group (=CO). This may certainly be a determining factor for understanding the whole process.

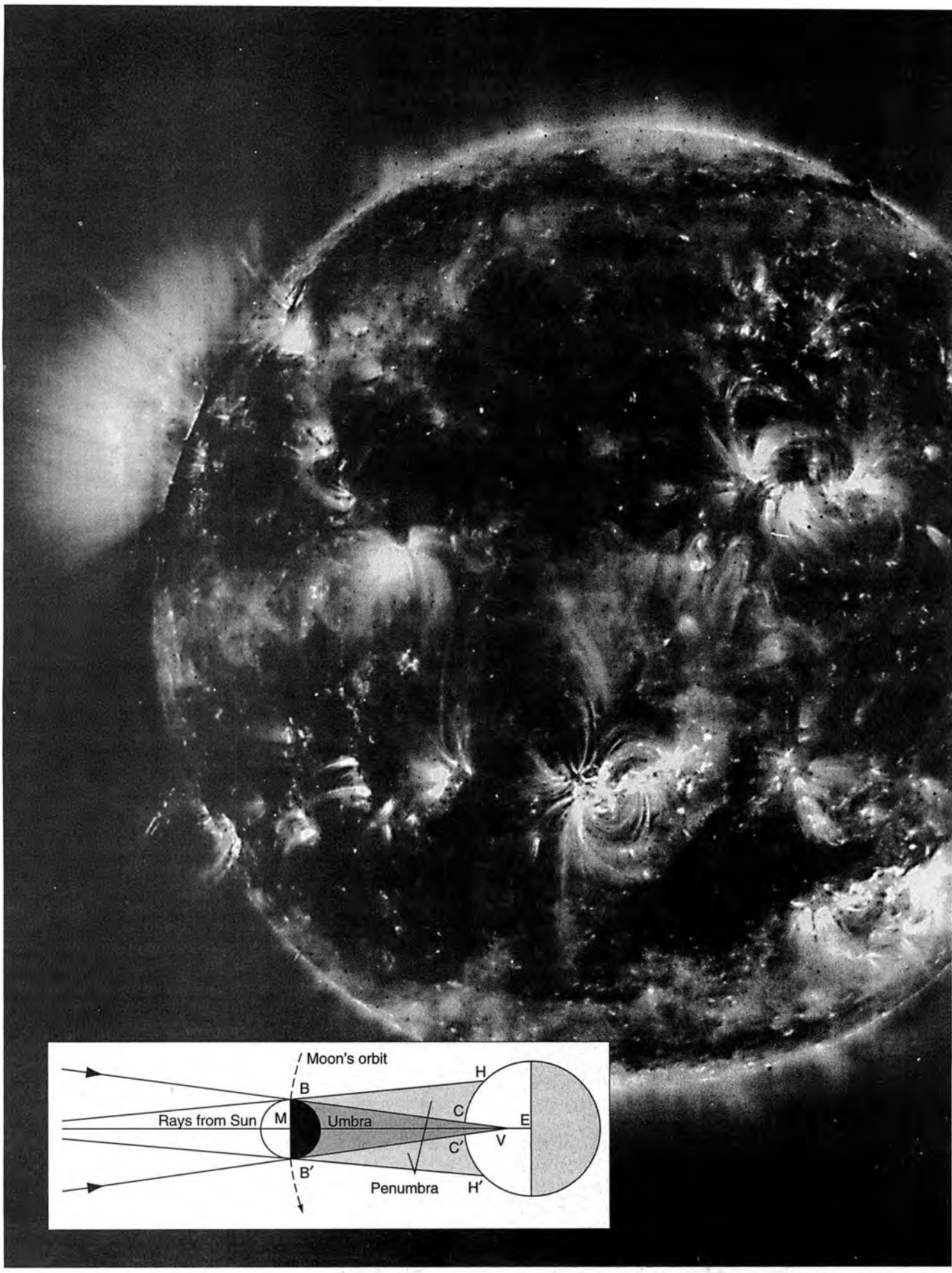
One should keep in mind that we are probably dealing here with the main course of the enzymatic reaction, rather than with some negligible microevent. Such a conclusion is corroborated by the following: Mitogenetic analysis does not reveal radicals as such, but only those radicals that are excited by photons. It is appropriate to mention here that in some cases, even very weak UV illumination is enough for the detection of free radicals by our method of resonance scattering. Because the probability of photon absorption by a short-lived free radical is very low, the number of radicals excited by external irradiation will comprise a negligible part of the whole.

To summarize, the sphere of application of mitogenetic methods in biology appears to be almost inexhaustible. Obviously it would be ridiculous to make any scientific prognosis here, in light of the complete disregard for our results. But we made it a rule to evaluate the results of mitogenetic investigations regardless of predominant opinions.

It would be useless to enumerate all of the spheres in which mitogenetic methods can be applied. The general progress and expansion of the application of mitogenesis—which has already given us, and should give in the future, a new biological horizon—seems to be very important.

We would like to point out at least that mitogenetic methods permitted us to reveal the existence and importance for living systems of chain processes and of a regular nonequilibrium arrangement of molecules. Until now, these two concepts, although they are completely alien to classical biology and cytology, have been necessary for understanding the main biological processes. It is extremely important to take these concepts into consideration. Moreover, after they obtain general recognition, many conventional concepts will be looked upon in the light of new discoveries and will be subjected to reconsideration.

At the same time, this review, although incomplete, brings us to the following general conclusion: If further development of the idea of the mitogenetic phenomenon and the attitude of science toward it were to become normal, "mitogenesis" should become completely dissolved into the realm of related disciplines. The very term "mitogenesis," as the name of a specific discipline, should disappear, as its role would have been played out.


Moon's orbit
B
H
C
Rays from Sun
M
Umbra
E
V
C′
B′
Penumbra
H′

Abnormal Physical Phenomena Observed When the Sun, Moon, And Earth Are Aligned

by Prof. Shu-wen Zhou
Department of Physics, Huazhong University of Science and Technology

Contrary to accepted theories of gravitation, the three-body alignment occurring at solar and lunar eclipse produces a measurable, abnormal effect on force and time measurements.

NASA/SOHO

EDITOR'S NOTE: *Professor Zhou contacted* 21st Century *shortly after our publication of the work of Maurice Allais in the Spring 1998 issue ("Michelson-Morley-Miller: The Coverup"), informing us of his independent researches verifying the conclusions of Allais that the theory of gravitation should be developed. This article is his summary of several reports, both published and unpublished, covering more than a decade of experimental work.*

During the period when the three bodies—the Sun, the Moon, and the Earth—are in an approximately straight line, there appear some inexplicable anomalies of physical character. These include: an unusual force of horizontal oscillation, strange changes in the pattern of grain sequence in crystals, changes in wavelength of emission spectra, and changes in the rate of speed of atomic clocks. Since 1987, my collaborators and I have been conducting numerous observations and analyses of these strange phenomena.

The solar system is a vast laboratory, storing many secrets that can not easily be interpreted. Beginning in the 1950s, scientists noticed some inexplicable mechanical phenomena which occur on the Earth when the Sun, the Moon, and the Earth line up approxi-

◀*The Sun as captured by the Extreme Ultraviolet Imaging telescope during the Aug. 11, 1999 total eclipse. Inset, diagram of the lineup of the Sun, Moon, and Earth, during a solar eclipse.*

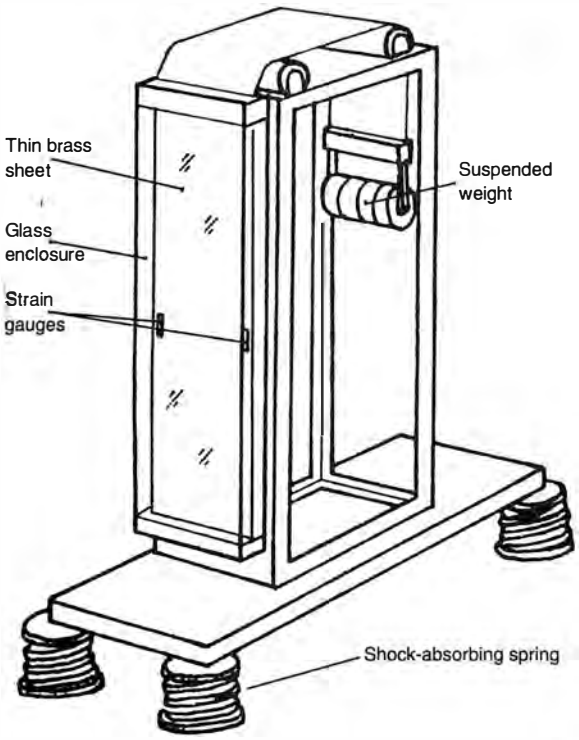


Figure 1
THE FORCE SENSOR
Drawing of the device used by Professor Zhou and his colleagues in China to detect an anomalous horizontal force during solar eclipse.

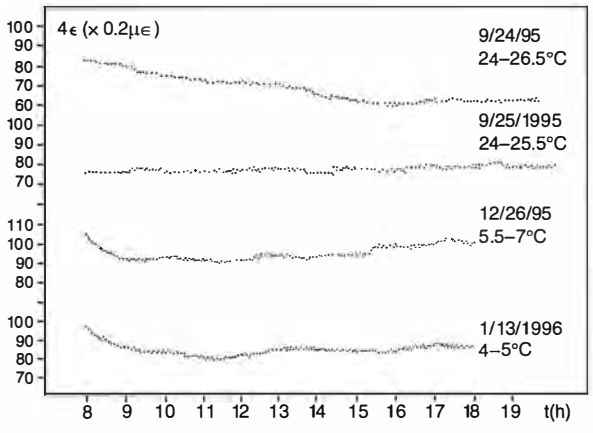


Figure 2
THE STRAIN CURVES (4ϵ-t) ON NON-ECLIPSE DAYS
The measured value of the strain, or horizontal force exerted on the vertical brass sheet in the apparatus shown in Figure 1 is 4ϵ. On ordinary days, the curve plotting strain against time is fairly constant.

mately along a straight line. These are generally called "gravitational anomalies." First, Maurice Allais discovered the azimuth change of a paraconical (modified Foucault) pendulum during a solar eclipse. Then, also during a solar eclipse, Liuquan Wang found an unusual inclination of a clinometer (an instrument that measures angles of elevation or inclination), as if a horizontal force of 10 to 20 μg was applied to the instrument. Thereafter, T. Kuusela found an abnormal inclination from the plane of a torsion pendulum during a solar eclipse. E.J. Saxl discovered that a relative change of magnitude 10^{-4} in the partial cycle of a torsion pendulum occurred during a solar eclipse. Yet, others found that the change for the entire cycle was zero.

The following is a summary of some of the research on these very exciting physical phenomena that I and my colleagues carried out in China.

Abnormal Horizontal Force Detected During Eclipse

An apparatus is set up to detect the horizontal force in the air (Figure 1). The main part of the device is a thin brass sheet (1.5 × 0.3 × 0.000095 m), mounted with its lower end fixed and its upper end freely supported by rollers and pulled tight by a suspended weight, so that the brass sheet remains flat. The vertical part of the brass sheet is surrounded by a glass enclosure to avoid the effect of changes in the barometric pressure. A strain will be produced when the sheet is affected by a horizontal force. To measure the strain, two strain gauges are fastened to the sheet and also wired to a strainometer. This apparatus is set upon shock-absorbing springs to isolate it from any ground vibration. An electromagnetic interferometer is placed in the same laboratory. The maximum change of room temperature within a 12-hour period is 1° C.

An eclipse of the Sun took place on Oct. 24, 1995, in the city of Kun Ming, China. It began at 10:22 (Beijing time), reached its maximum at 11:46, and had the second contact at 13:18. The maximum of the eclipse was 0.73 when the Sun was at an altitude of 60°.

On ordinary (non-eclipse) days we conducted repeated comparative experiments, the results of which are presented in Figure 2. The measured value of the strain we found is 4ϵ. The figure shows that the curves are smooth and continuous.

During the eclipse period of Oct. 21 to Oct. 28, while we were observing the brass sheet apparatus, we obtained a very different result (see Figure 3). During the five days (Oct. 23-27) spanning the occurrence of the eclipse, the curves change suddenly from their original smoothness, with the change occurring right on the points of 09:00, 10:42, 15:00, and 17:12 for all five days. Between 15:00 and 17:00, ϵ oscillates greatly with an amplitude that is partly symmetrical to the amplitude on the day of the eclipse. These curves do not manifest any sudden changes or oscillation before Oct. 23, or after Oct. 28.

We conclude from our observations, therefore, that at certain times during the three-body alignment, the brass sheet is affected by a horizontal force of oscillation. The magnitude on a unit mass of sheet is 10^4 μg (where g is approximately equal to 10 m/s^2), which is 10^2 times the tidal force. The cause of the force cannot be attributed to temperature changes, nor to an electromagnetic disturbance, nor to ex-

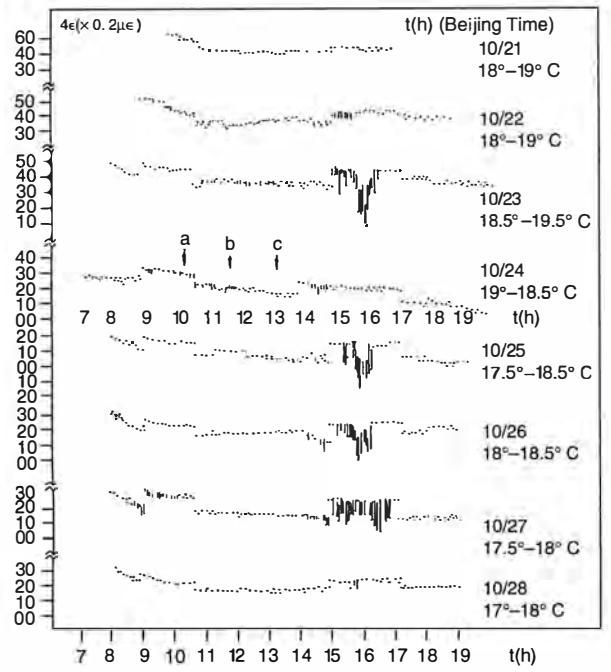


Figure 3
THE STRAIN CURVES (4ε-t)
DURING ECLIPSE PERIOD
(OCTOBER 21-28, 1995)

During the eclipse period, the curve of strain plotted against time showed radical oscillations. The arrows a, b, c show, respectively, the times of onset, maximum, and second contact of the solar eclipse.

ternal vibration, nor to fluctuation of barometric pressure, nor to the tidal effect, because the tidal effect should be on the order of 10^2 μg. The phenomenon we have observed is anomalous; its implications are yet to be explored.

Effect of Eclipse on the Casting of Lead-Tin Alloy

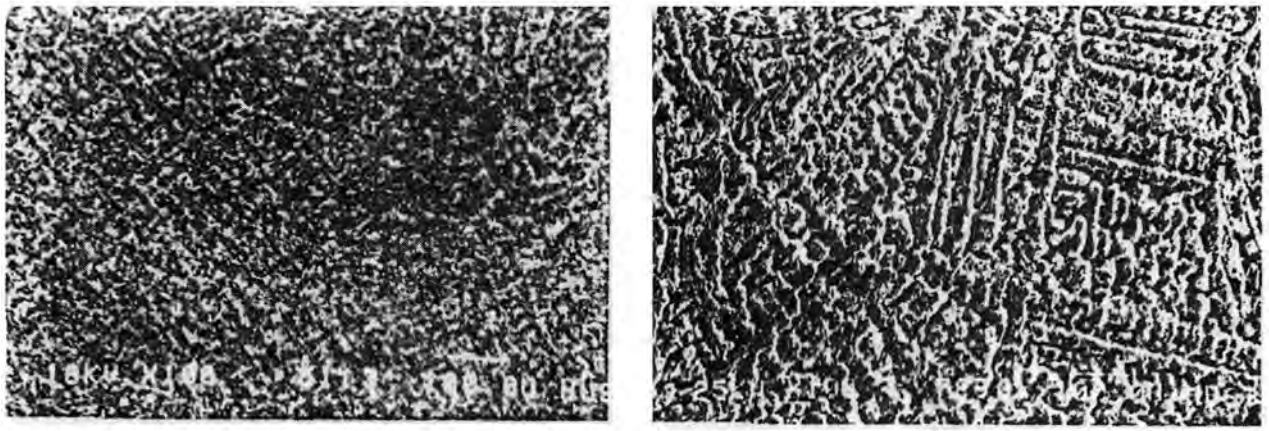
Another solar eclipse took place on Dec. 24, 1992, whose maximum in Harbin City, China, was 0.61. Our experiment was conducted at an indoor temperature of about 10° C.

Before the beginning of the eclipse, we melted the lead-tin alloy (Sn 55 percent, Pb 45 percent) into a liquid state, and maintained it at the melting point temperature. During the eclipse, we cast a group of samples using the Pb-Sn liquid alloy. Under the same conditions, in the days after the eclipse, we also cast another group of samples.

The two groups were photographed and enlarged 100, 300, and 600 times, respectively, by an electron microscope, in order to examine their metallographic patterns. In Figure 4, photo (a), from the control group, shows a random grain distribution, while photo (b), from the eclipse samples, shows a sequenced grain distribution. The conductivity of the two groups was also tested, and the results indicated that the conductivity of the eclipse sample was 5 percent higher than that of the control group. It should be pointed out that during the casting of the two sample groups, we did not exert any influence on either group, and thus the sequenced grain pattern of the eclipse sample should be attributed only to the effect of the solar eclipse.

Abnormal Changes in Emission Spectra

It is well known that the spectral wavelengths of elements on the Earth have proven constant by all the tests in the past. They can be altered in the universe only by the gravitational and Doppler effects. For instance, spectral wavelengths of the



(a) (b)

Figure 4
REFLECTION ELECTRON MICROGRAPHS (100 X)

Experiments conducted in a laboratory in Harbin, China, showed that the grain distribution pattern of crystallized samples of a lead-tin alloy was altered during the solar eclipse of Dec. 24, 1992. Photo (a), from the control group, shows a random grain distribution, while photo (b), of a sample of alloy crystallized during the eclipse, shows a sequenced grain pattern.

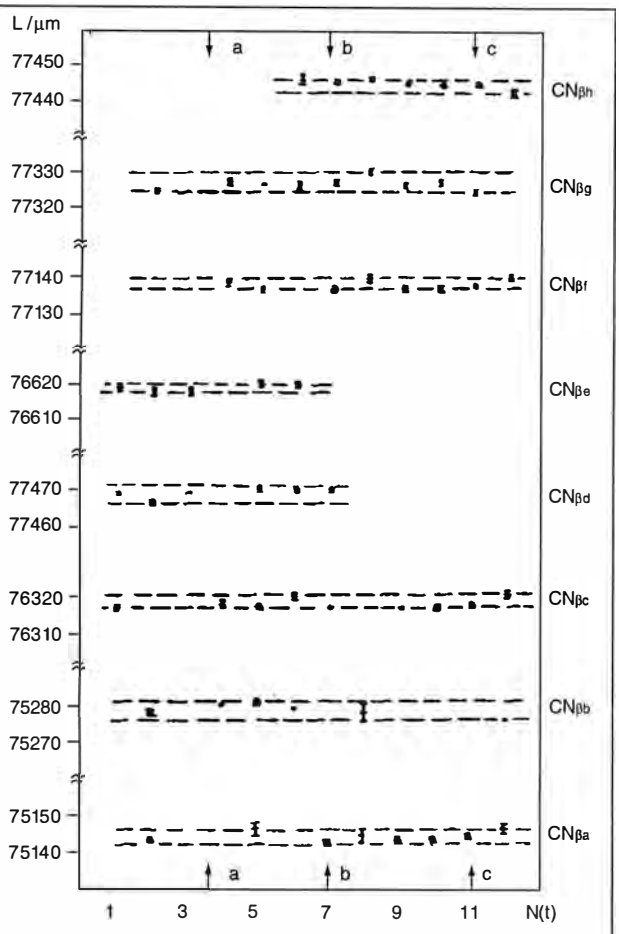


Figure 5
L-t CURVES OF THE COMPARISON SPECTRUM
When L, *the distance between two spectral lines, is plotted against time* (t), *under ordinary conditions, the curve is flat, showing no time-dependency.*

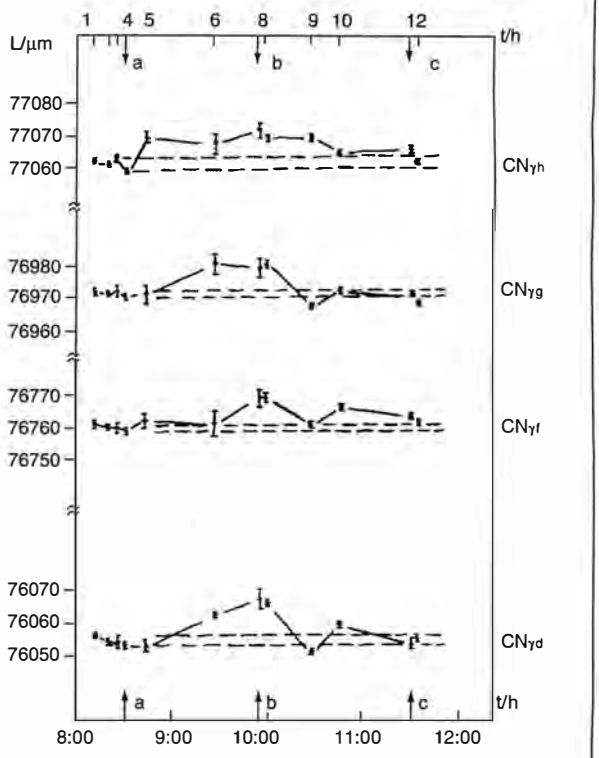


Figure 6
PART OF THE *L-t* CURVE OF THE ECLIPSE SPECTRUM
The line spacings for the emission spectrum of CN, and of a number of elemental spectra tested, were shifted from their normal zone (shown by region between horizontal dotted lines) during the solar eclipse of Sept. 23, 1987. Arrows a, b, c mark the onset, maximum, and second contact of the eclipse.

solar surface compared to those of the Earth's surface show a relative shift of 10^{-6} magnitude. However, during the solar eclipse of Sept. 23, 1987 (an eclipse with a maximum of 0.86), we found a relative change of 10^{-4} in the magnitude of the spectral wavelength.

Six different models of spectrum analyzers were placed in laboratories in different areas. They photographed the emission spectra of H, D, Ca, CN, Ni, Ti, and so on. The spectra photographed, we should point out, are of a light source inside the laboratories, not the solar spectrum. The spectrum analyzers are of ordinary kind, with a dispersive power of 4 to 8 angstroms/mm.

To measure spectral line spacings, an instrument such as the Abbé comparator, which has a low accuracy, is widely used in different countries in the world. We succeeded in using a laser comparator, which automatically measures spectral line spacings. The uncertainty of a laser comparator in measuring a standard ruler is smaller than ±0.4 micrometers.

The results of our measurements indicate that spectral line spacings that are measured on any usual day remain unchanged within the average error range, while a relative change of 10^{-4} magnitude took place for all the spectral line distances of H, D, Ca, CN, Ni, Ti, and so on, photographed during the solar eclipse at different laboratories. It can be reliably stated that it is the solar eclipse which causes the relative shift of 10^{-4} magnitude in the spectral wavelengths.

Take the case for CN, for example, as its test results are shown in Figures 5-7. L, measured in micrometers, represents the distance between two spectral lines. $CN_{\beta g}$ and $CN_{\beta g'}$ and so on, represent any two given spectral lines measured. Arrows a, b, c mark the time of the beginning, the maximum, and the second contact of the solar eclipse. Figure 5 shows the distance between two spectral lines (L) plotted against time (t), which is a spectral comparative curve. The graph demonstrates that *L* and *t* do not have any influence upon each other. Figures 6 and 7 show the *L-t* curve of the eclipse spectrum, which demonstrates that the distance of spectral lines keeps equally within the error range before and after the eclipse, but changes during the solar eclipse. When the eclipse reaches its maximum, the distance band varies far beyond its normal zone.

Spectroscopy has a long history and there are many laboratories equipped with sophisticated spectrographs in various countries of the world. Why haven't they found that the solar

eclipse causes changes in the spectral line distance of emission spectra, while we have done so using only a spectrograph of low dispersive power? The reasons are, first, that during the solar eclipse most researchers paid a lot of attention to the Sun's spectrum and perhaps ignored the indoor light spectrum. Second, even if they did study the indoor light spectrum during a solar eclipse, they could not find changes using the traditional Abbé comparator.

Abnormal Effect of Eclipse On Rate of Atomic Clocks

It has been previously reported that the solar eclipse has an influence on atomic clocks placed at two different locations and observed by sky-wave comparison. This is, essentially, the influence of the day-night effect caused by the eclipse upon the ionosphere, which is understandable.

We have discovered that the solar eclipse has various influences upon atomic clocks at two locations observed by ground-wave comparison, upon atomic clocks in the same laboratory but different azimuth by direct comparison, and upon atomic clocks at two locations compared by clock transportation by airplane.

We have analyzed atomic clock time comparison (ground-wave) data of 16 LOC chain recorded from 1987 to 1993 by the U.S. Naval Astronomical Observatory, and found an obvious impact of the solar and lunar eclipse on the data. Figure 8 presents a chart of time comparison by the U.S. Naval Astronomical Observatory at LC7970. LP, LT, SP and ST represent, respectively: partial lunar eclipse, total lunar eclipse, partial solar eclipse, and total solar eclipse. The vertical dashed line indicates the time of a solar or lunar eclipse. As the chart shows, during the eclipse, when all three bodies are aligned, the curve appears in the shape of a peak or valley with a fiercely changing gradient. We can also see from the chart that the tendency to change is not limited to the period of optical effect of the eclipse, but extends over the entire period of the three-body alignment, approximately.

During the partial solar eclipse of Dec. 24, 1992, using seven atomic clocks, we conducted some direct comparisons with the same atomic clock in different positions and did so at the

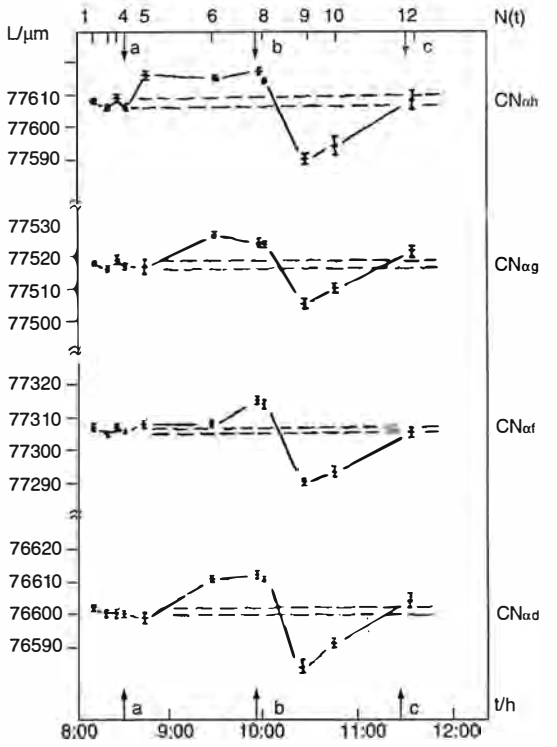


Figure 7
OTHER PART OF THE *L-t* CURVE OF THE ECLIPSE SPECTRUM

Line spacings for another part of the emission spectra of CN gas, as measured during the eclipse of Sept. 23, 1987, at laboratories in of China.

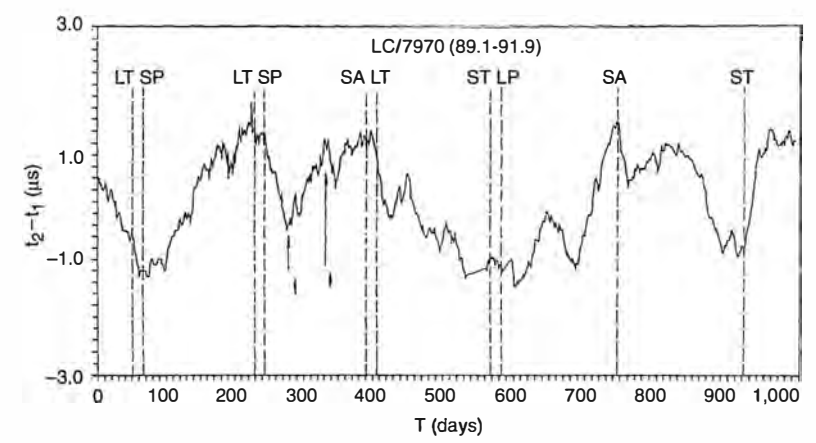


Figure 8
VARIATION OF THE DAILY TIME DIFFERENCE WITH DATE SERIES AT LC7970 FROM 1989 TO 1991

LT = Total lunar eclipse *ST = Total solar eclipse*
LP = Partial lunar eclipse *SP = Partial solar eclipse*

The graph shows the time difference (t_2-t_1, measured in microseconds) for two atomic clocks as measured by the U.S. Naval Astronomical Observatory (LC/7970), over a period of 1,000 days from 1989 to 1991. The vertical dashed lines indicate the time of lunar and solar eclipses. Superimposing these on this graph of time differences shows that the maxima and minima of the time differences generally occur at the time of Sun-Moon-Earth alignment.

same time in the four cities of Harbin (magnitude 0.61), Chang Chun, Beijing (0.40), and Wuhan (0.20). We also carried out comparisons using clocks transported by planes flying among the three cities of Harbin, Beijing, and Wuhan.

Figure 9 demonstrates the results of a direct comparison between two cesium clocks positioned in different azimuths, but in the same laboratory. The dotted line indicates the maximum of a solar eclipse. Lines AB and CD have a constant gradient showing, therefore, that the difference of the two clock rates is constant. The gradient of line BC gradually changes, indicating that the alignment of the three bodies causes a change in the difference of the two clock rates. This relative change of the time difference between two cesium clocks is 2.4×10^{-12} magnitude.

A direct comparison between two rubidium clocks in the same laboratory in the city of Chang Chun is demonstrated in Figure 10. Analysis of the graph shows that a relative change of time difference between the two rubidium clocks reaches 3.6×10^{-8} magnitude during the solar eclipse. The effect occurs not only on the day of the solar eclipse, but during the period of the three-body alignment, approximately.

Figure 11 demonstrates the time comparisons of clock transportation by plane between Wuhan and Harbin. Line AB shows the comparison between the flying clock and the ground clock in Wuhan; Line BC shows the flying clock from Wuhan to Harbin, affected by the solar eclipse in Harbin, then returning back to Wuhan. Line CD shows the comparison between the returned flying clocks to Wuhan and the ground clock in Wuhan. When the relativity effect is deducted, the relative change of the two clocks' time difference reaches 7.6×10^{-12} magnitude during the three-body alignment.

In conclusion, we realize by the direct comparisons and by clock transportation comparisons, that under usual circumstances the atomic clock rate is very stable. It is, however, affected by the three-body line up. The length of time of the influence is not limited to the period of the solar optical effect, but falls within the period of the three-body alignment.

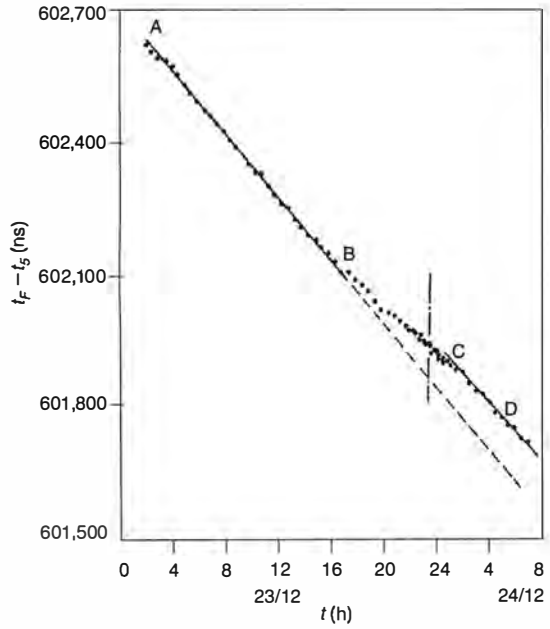


Figure 9
TIME DIFFERENCE CURVE OF DIRECT COMPARISONS BETWEEN TWO CESIUM CLOCKS IN HARBIN

Tests on two cesium atomic clocks positioned in different directions, but in the same laboratory, in Harbin, China, showed an increase in the time difference occurring around the time of the Dec. 24, 1992, solar eclipse (vertical dotted line on graph). Straight lines AB and CD show that the rate of change, measured in nanoseconds, of the time difference between the two clocks is constant in the non-eclipse period, but becomes irregular around the time of the eclipse.

Discussion

Our observations and research involve several specialized areas of science, such as mechanics, metallurgy, spectroscopy, and time comparison of atomic clocks. Experts in these areas have offered their technical support, for which we are wholeheartedly grateful.

To summarize our findings: During the period of three-body alignment, an unusual force of horizontal oscillation takes place. This period has various effects upon the grain arrange-

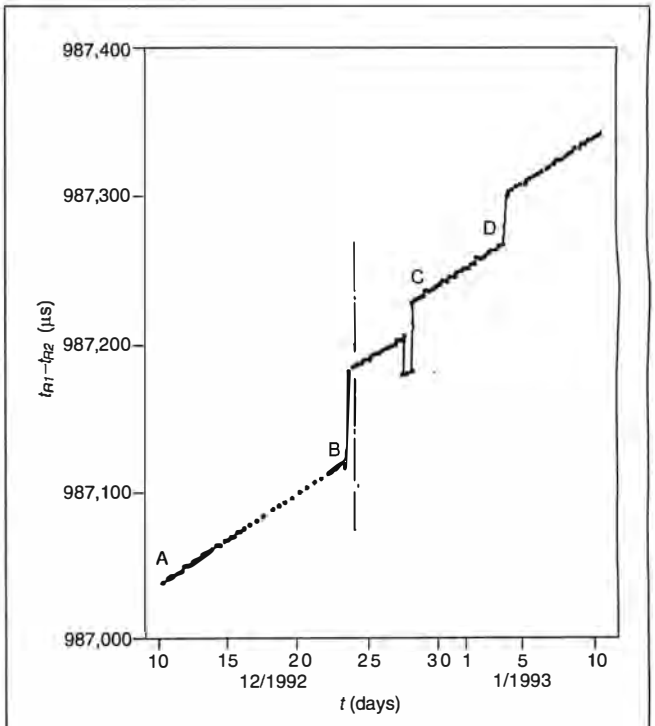


Figure 10
TIME DIFFERENCE CURVE OF DIRECT COMPARISONS BETWEEN TWO RUBIDIUM CLOCKS IN CHANG CHUN

The time difference observed between two rubidium clocks in the same laboratory in Chang Chun, China, showed sudden changes around the time of the Dec. 24, 1992, eclipse (vertical line).

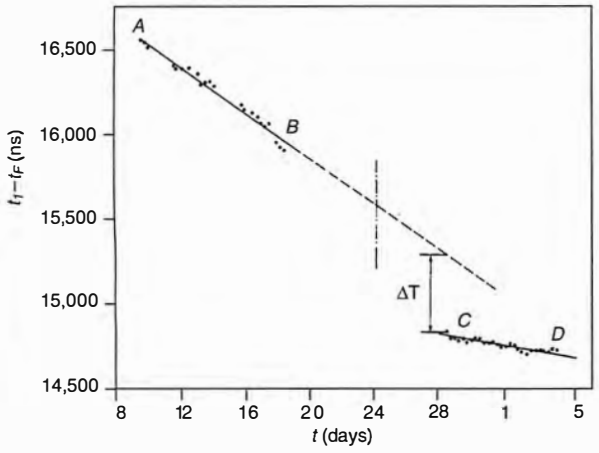
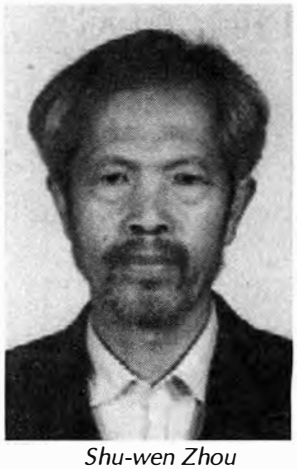


Figure 11
CLOCK TRANSPORT COMPARISONS BETWEEN WUHAN AND HARBIN DURING SOLAR ECLIPSE
The Theory of Relativity predicts that a difference in time-keeping will be found between stationary and flying clocks. But even when this predicted difference is deducted, the relative time difference between a clock carried in an airplane and one kept on the ground was found to reach 7.6×10^{-12} magnitude when the flying clock was carried to a city, Harbin, where the solar eclipse was maximum, and returned to Wuhan, where the comparison clock was kept.

ment of a lead-tin alloy, upon the spectral wavelengths of atoms or molecules, and upon the speed rate of atomic clocks. The uncertain mechanism contained in these effects is not yet explained. All of the abnormal physical phenomena tell us, in different ways, that there are many secrets, yet undiscovered, which occur during the period of the three-body alignment, and which require further exploration. The most important work at present is to repeat our experiments and tests.


Shu-wen Zhou

Shu-wen Zhou is in the Department of Physics, Huazhong University of Science and Technology, Wuhan, China. This article was translated into English by Ms. Haiou Liu.

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Predictions Are Always Wrong

by Phil Rubinstein

Of late, in explaining to the American population Lyndon LaRouche's forecast of an economic collapse, and related political consequences, we often have to face the impact of an irrational belief in linearity, most directly with respect to the sense of time. This occurs in the form of, "Can you predict. . .?", "Can you tell us when. . .?", or "Your prediction was wrong; it didn't happen," and so on.

Of course, no "prediction" was made, but rather a *forecast,* based on certain physical economic parameters, but whose timing, and even outcome, depend on many complex and unpredictable events, not the least of which is the individual citizen's decision to act, or not to act, in such a way as to help avert an otherwise unstoppable collapse.

The difficulty which the typical American confronts in thinking about LaRouche's economic forecast reflects a view of space-time that is one of a linear extension, with space as a filled-up box, and, in effect, no concept of time, because time can exist only as *change, action, becoming.* It is precisely this linearity that simplifies language to dumbness, reduces music to noise, and makes all science and geometry of the post-Kepler period incomprehensible.

Aristotle's Influence

It is no accident that one can find a nearly completely modern expression of this standpoint in Aristotle's *On Interpretation.* Aristotle says first, in section III: ". . . verbs by themselves, then, are, nouns and they stand for or signify something. . . . [T]hey indicate nothing themselves but imply a copulation or synthesis, which we can hardly conceive of apart from the things thus combined." And then: "We call propositions those only that have truth or falsity in them."

Were this only the ancient outlook of a discredited Aristotle, no problem would ensue but, in fact, this is the root of the thoroughly modern outlook of Russell, Frege, Carnap, and so on. In fact, *On Interpretation* could be a handbook for information theory. Although Aristotle, like his modern followers, recognized that the thoroughly deterministic outlook that follows from this contradicts the actual choices made by human beings, his resolution of this paradox is to introduce mere *contingency,* a kind of randomness, which is allowed to the empty future.

The Future Determines the Present

The reality is best grasped by taking an approach rooted in physical economic planning. Begin with a moment in history defined by a resource level that is determined by an existing science and technology. A horizon can be hypothesized at which the social cost of resources usable at that level of technology would lead to a critical degeneration, or inability to maintain capital or labor. That crisis defines a necessary present deployment of advanced technologies to create new scientific breakthroughs. This, however, requires greater density of use of resources, labor, and so on; thus, the horizon is changed.

Take the example of the progression: fossil fuel, nuclear fission, then nuclear fusion. Our present resources may be stretched to extend the horizon, but that merely worsens the crisis. If we choose to accelerate the use of fission energy, the demand on existing resources *uses up* those resources more rapidly. If we mobilize a crash effort to achieve fusion, the rate of usage increases.

Thus, *the future* is changed *for present action* at each step. The problem then becomes to determine the actual activity required in the present. As this occurs, the relationship between *now* and *the future* is constantly altering: That also alters all other activity, allocation of resources, labor, and so on. In this way, the present is itself an incommensurable. It is a perfect example of non-constantly changing action. It is this subjectivity that lies at the root of understanding physical space-time as something of constantly changed activity of *a* multiply connected *type, in* the sense of Leibniz.

From this standpoint, one can see that not only is the future *causing* the present, but that implicit in any hypothesis of this type is an inversion that is asymmetric. As the forecast is made, it immediately brings us to a new concept of the path of action itself. The relationship of past, present, and future is altered. So, let us never be caught in Aristotelian conceptions of "the future."

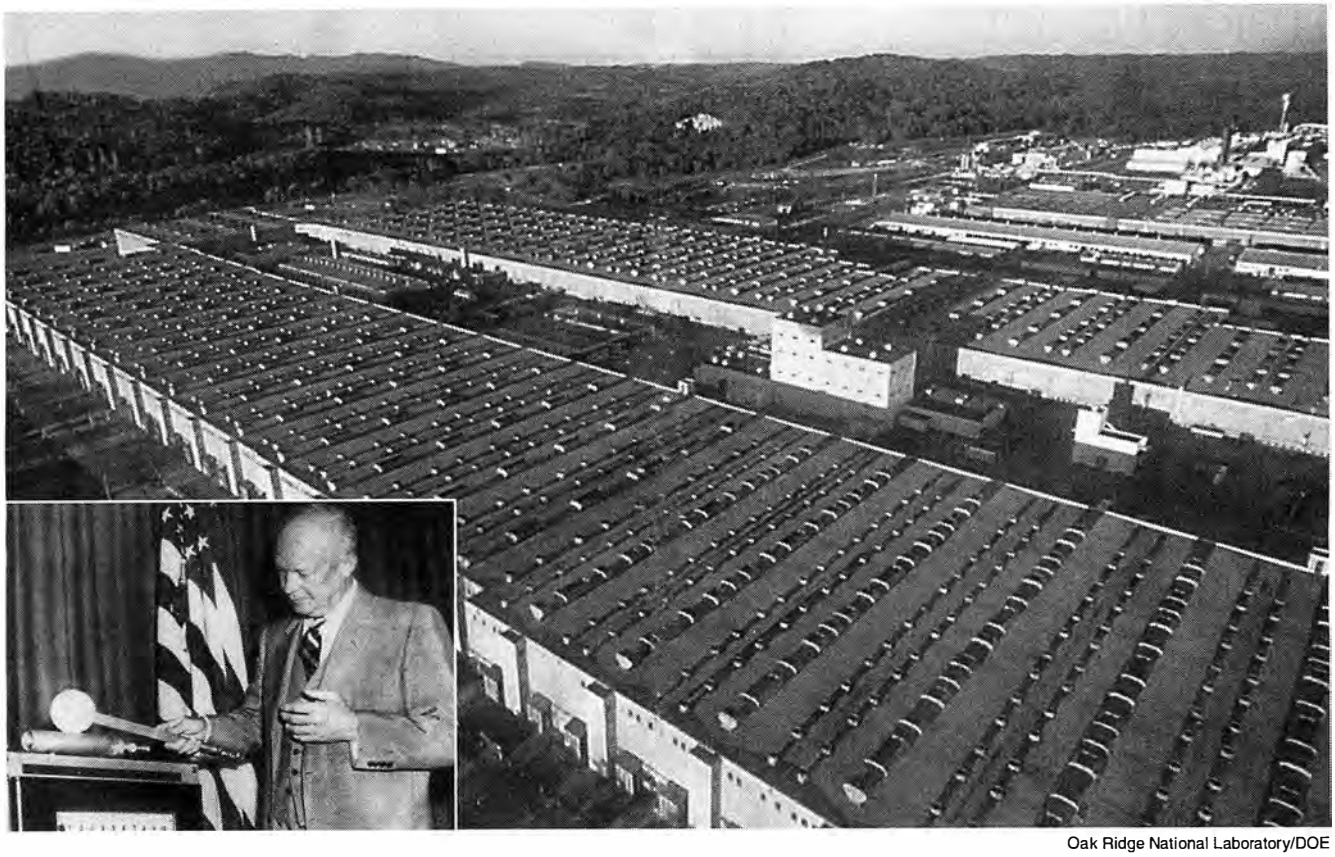
Implications for Language

This also has implications for language, such as the fundamental role of the subjunctive, and in physics, such as non-relativistic relativity and non-statistical quantum theory.

Let us look at the grip of fixity from the standpoint of language. Despite often best intentions, our own thoughts are dominated by the tyranny of names, propositions, states of affairs, facts which are either true or false. For is not every statement either true or false?

It is often best to revert to "the Philosopher," Aristotle, who says simply in *On Interpretation* (part of his study of language) that, "verbs by themselves, then, are nouns, and they stand for or signify something, for the speaker stops his process of thinking and the mind of the hearer acquiesces."

Perhaps more remarkable than the bold statement that "verbs by themselves, then, are nouns," is Aristotle's recognition that this denuding of language ends thought, and forces submission. Is this not the simplest description of non-cognitive political organizing, or the folly of conceiving a human economy as if it were moved by animal lusts and drives? We might call this the



Oak Ridge National Laboratory/DOE

"The future is changed for present action at each step": The U.S. Manhattan Project during World War II was the world's largest industrial project ever, mobilizing vast national resources. It built the world's largest building in 1943 (above), for isotope separation, requiring more energy than the nation had ever before produced. But within a decade of the Manhattan Project, nuclear fission was creating electric power for civilian use. The first nuclear fast breeder reactor generated electricity in Idaho in 1951. Inset: President Eisenhower opens the Shippingport Nuclear Plant in Pennsylvania in 1957.

"name it and nail it" approach to thought; the search for the right slogan or formalism.

Aristotle finishes here by noting: "for even the infinitives 'to be,' 'not to be,' and the participle 'being' are indicative only of fact if and when something further is added. They indicate nothing themselves, but imply a copulation or synthesis."

Now consider the *space-time* such thought implies: There are things which have names, which by a strange act, or occult connection, are joined. Statements about them are true or false. In fact, there is no action, they simply rest in space, and there is thus no time, no true change. In reality, this world is one of things and properties. Since, in this view, all facts are either true or false, there is no real succession, and eternity is simply the present.

It is from this standpoint that the monstrosity of *absolute space and time* derives, an empty, fixed box full of coupling and joining (achieved by straight line forces).

Leibniz's Alternative

To leave such a world requires a complete change, not simply in language, but in the way the mind's eye works. In the *Metaphysical Foundations of Mathematics,* Leibniz notes: "Space is the order of coexisting things, or the order of existence for things which are simultaneous." And slightly above that: "If a plurality of states of things is assumed to exist which involve no opposition to each other, they are said to exist simultaneously."

These two statements, taken together, can be seen to open an entirely different conception of physical space-time which focusses universal activity on a real *succession of change.* Now, the totality of space is altered when action introduces something incompatible to previous order, and it is that which introduces real time as a changed space. Thus, all of the space-time is truly changed and the primacy of facts is altered. Leibniz (as perhaps can be investigated at a later time) develops this idea of relative space-time in the Clark-Leibniz letters, including the notion of increased power.

From this altered view—the view as instituted by Nicholas of Cusa, and expanded, from which all true science flows—comes a conception of curvature which is rooted in the alteration of all space-time. Such a change begins in the way we think. This allows us to recognize in changes in the small, the curvature of space-time, since it is the succession which is the reality, not an arbitrary alteration in a flat box. We can now reject nouns and properties (and perhaps even John Locke's property), in our language, and in our mind as well.

Phil Rubinstein is a National Committee member of the National Caucus of Labor Committees, the activist philosophical organization founded by Lyndon LaRouche in 1968.

Japan Pushes Forward in Fusion Research

by Marsha Feeeman

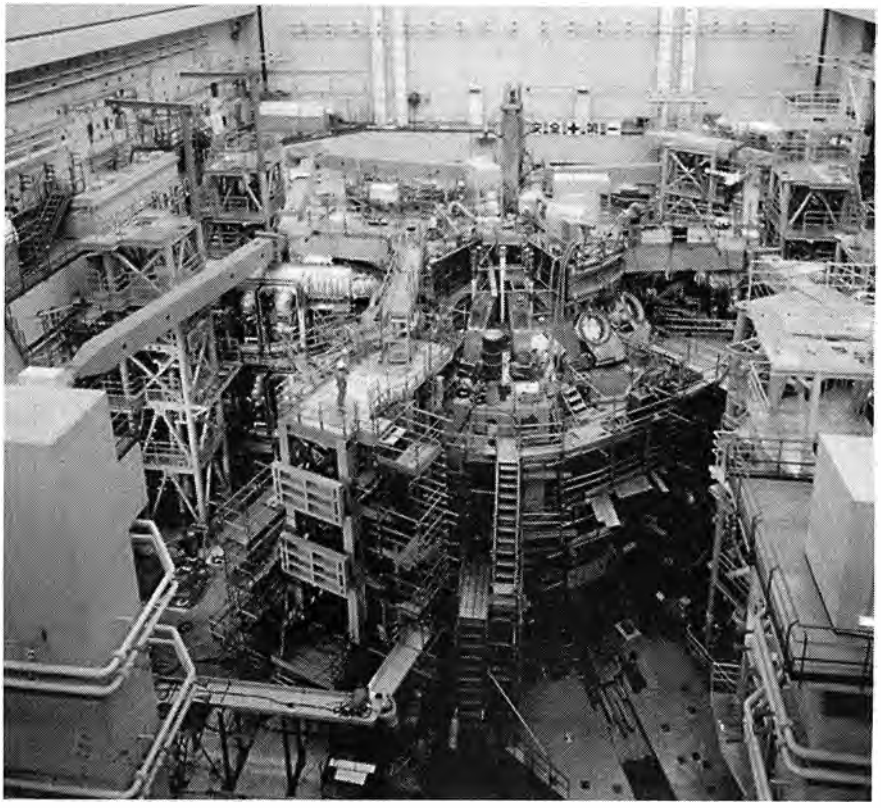
A consensus has been reached in Japan that fusion energy must be developed "as a responsibility of the present generation for the next generation." Japan's commitment to fusion and the future is in great contrast to that of the United States, which has emasculated its thermonuclear fusion program over the past decade, claiming that it is not worth spending the resources to develop this potentially inexhaustible supply of energy, and to the Russian fusion program, which has suffered the same devastation as the rest of science in Russia.

A review of the Japanese fusion program plan was presented by Dr. Nobuyuki Inoue, chairman of the Nuclear Fusion Council of Japan, to a July 23, 1999, meeting of the U.S. Department of Energy's Fusion Energy Sciences Advisory Committee (FESAC), held in Snowmass, Colorado.

Because Japan has to import the bulk of its energy resources, he explained, "Japan has a long-range strategy for nuclear energy development to solve the energy problems of the 21st century," and "Japan intends to develop fusion as a viable option as a future energy source."

Inoue explained that the core program to develop fusion in Japan is multifaceted, including continued experiments using the JT-60 tokamak, completion of an advanced tokamak study, research in future reactor technologies, and consideration of alternative and innovative new fusion concepts. The next step, the Japanese believe, is to feed the results from the current phase into the design and construction of the International Thermonuclear Experimental Reactor (ITER), which has been under development by the international fusion community since 1978.

Japan already has a broad-based research and experimental program for fusion development, which includes facilities for not only the tokamak geometry, but also a mirror fusion device, reversed field pinch experiment, and helical field facilities. Japan also has an active program in inertial confinement fusion, using lasers.



Kiyoshi Yazawa

Japan's JT-60 tokamak, as it looked in construction.

Cost 'Not of Concern'

Inoue made clear that while the estimates are that the ITER tokamak facility will cost in the range of $5 billion, the cost is "not of concern" to the government of Japan. Although the Japanese government hopes that the cost will be shared, it is proceeding apace to formulate proposals for ITER to be "hosted" by Japan, he reported.

Over the past year, the United States decided to terminate its participation in the ITER program, because of draconian budget cuts imposed by the Congress. But the Japanese, Europeans, and Russians have continued their joint design and engineering studies. Inoue said that Japan recognizes that "domestic reasons are regarded to have caused a change in U.S. policy on ITER," bearing little on the scientific merits of the program. "Japan expects the U.S. to rejoin the ITER activities," he stated.

When asked about the difference between the American and Japanese fusion programs, Inoue pointed to the broad-ranging consensus in Japan for fusion development, with "heavy involvement of industry," in addition to the university and laboratory programs.

A Broad Consensus

As he explained, this consensus did not come about as if by magic, but through a serious commitment to educate various government bodies and organizations on the importance of fusion energy.

In the Japanese Parliament, there is a group of more than 100 members of the

(Continued on page 66)

Japan's Tokamak, JT-60U, Scores Advances

by Elisabeth Pascali

Scientists at the Japanese Atomic Energy Research Institute (JAERI) have just reported some significant breakthroughs on their main experimental reactor, the JT-60U. JAERI issued a press release in August, stating that they had achieved "quasi steady sustainment for a high performance reversed shear plasma. . . where the high fusion performance of $Q_{DT}^{eq} \sim 0.5$ was sustained for ~0.8 secs. . . [see figure]. A new technique of stored energy feedback control by using neutral beam injection power was applied for maintaining the stored energy constant slightly below beta limits. Occurrence of a beta collapse limited the duration of the high performance when the magnetic pitch minimum q_{min} in the current profile decreased down to ~ 2."

Q_{DT}^{eq} is the ratio of the energy produced by the reactor (using deuterium-tritium (D-T) fuel) to the energy input, and is an indication of the efficiency of the fusion process. A Q_{DT}^{eq} of 1 for a fusion reaction means breakeven. The JT-60U had already achieved a Q_{DT}^{eq} of 1.25 back in 1998, but only for 0.01 seconds. In the world of magnetic confinement fusion, the success of an experiment is generally measured in milliseconds, because the plasma current density and pressure, so far, has been attained through induction. The attainment of stable energy output at Q_{DT}^{eq} of 0.5 for 0.8 seconds is far beyond what other experiments have been able to achieve.

U.S. Retreats from Fusion

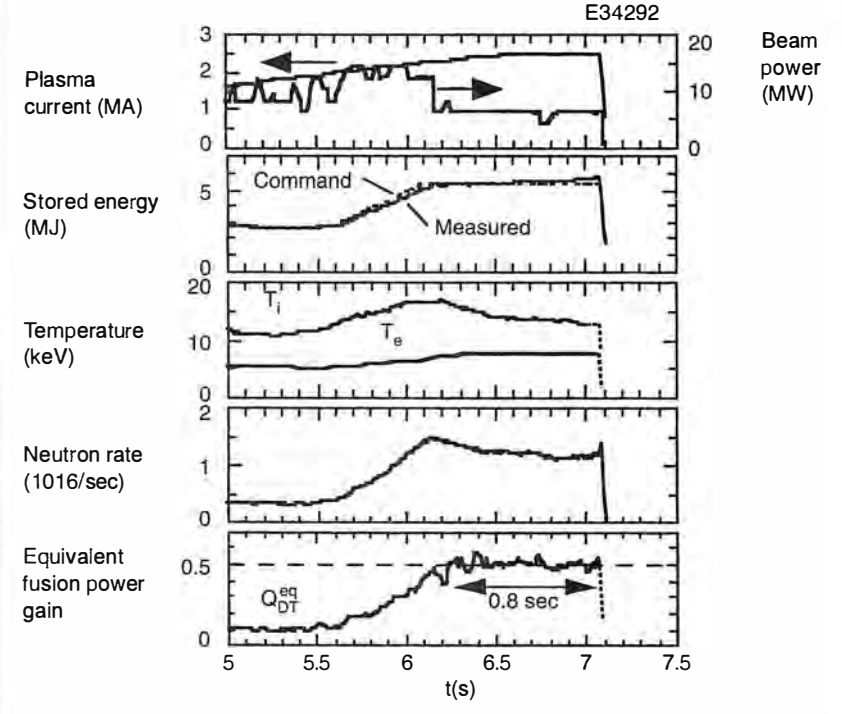
Contrary to the United States, Japan has been aggressively and optimistically experimenting in fusion research. The United States, at least for the moment, has decided to stop all funding for the International Thermonuclear Experimental Reactor (ITER) in the year 2000, and has shut down some of its main experimental programs, such as the Tokamak Fusion Test Reactor (TFTR) at the Princeton Plasma Physics Laboratory.

Fusion, of course, is the most common source of energy in the universe. The key to understanding how to reproduce the natural reactions that fuse isotopes of H into He (among other reactions) that take place every day in the Sun, and other stars, was discovered in a terrestrial laboratory in the 1950s, with the invention of the hydrogen bomb. Since that time, there has been some effort to master the fusion process for commercial energy production by several nations. However, this research has been hampered, especially in the United States, by an irrational fear of "nuclear technology," and now, more recently, of "unbalancing the country's checkbook."

In fact, the opposite is true. It is only

MAJOR PLASMA PARAMETERS

Plasma current	2.4 MA
Energy confinement time τ_E	0.8 sec
Central ion temperature ($T_i(0)$)	13 keV
Central deuterium density $n_D(0)$	3.7×10^{19} m^{-3}
Fusion triple product $n_D(0)\tau_E T_i(0)$	3.92×10^{20} m^{-3} s keV
Equivalent fusion power gain Q_{DT}^{eq}	0.5



SUSTAINING EQUIVALENT FUSION POWER GAIN OF 0.5 FOR 0.8 SECONDS

Here is the profile of a quasi steady state for high performance reversed shear plasmas obtained by the JT-60U, which obtained a high fusion performance of Q_{DT}^{eq} for about 0.8 seconds. JAERI expects to extend the high performance duration using neutral beam injection power.

Source: JAERI, Report No. 53, Aug. 11, 1999

the development of science and its technological applications, like fusion, that drive the economy forward.

Two Fusion Directions

Two directions in fusion research have been pursued. One idea, is to take solid crystals of deuterium and tritium (isotopes of hydrogen) and ignite them through compression, using lasers or particle beams. This is called inertial confinement fusion, and it is similar to the technology used in the hydrogen bomb. The largest ongoing experiment of this type is the National Ignition Facility (NIF), now in construction at Lawrence Livermore National Laboratory.

The other idea is to heat tritium and deuterium gases, which are confined by a magnetic field, into a plasma that is hot enough and at a high enough pressure for fusion to take place. The tokamak, or toroidally shaped plasma, which was first built by the Soviets in 1968, is the most successful type of such magnetic confinement fusion.

In 1990, there was an international commitment made by the United States, Japan, the European Union, and Russia to build the first commercially viable fusion reactor, called the International Thermonuclear Experimental Reactor (ITER). This was an ambitious program, because until this point, the main experimental reactors have been achieving power outputs at the 100s of kW range for short periods of time; whereas, the ITER is planned to produce 1,000 MW over a sustained length of time.

In 1998, the research and development phase of the ITER was extended by three years, but the United States has decided to participate only for one more year. Now, the Japanese are planning to host the actual construction of ITER, and they consider the JT-60U as a precursor to it.

New JT-60U Technologies

The most exciting thing about the new Japanese work on JT-60U is that it was achieved with two significant technological changes, which promise even better results in the near future. The first was the modification in June 1997 of the divertor to a *W*-shape pumped divertor. The divertor helps to remove impurities and lower energy particles from the plasma, thus maintaining the plasma's organization. JT-60 researchers have found that this new *W*-shape divertor geometry allows the plasma to achieve a transition to high mode, known as H-mode, with 30 percent less power. H-mode is a more highly organized state of the plasma, which leads to longer confinement times and higher density and temperature than those achieved with the normal low mode.

The other technological change which allowed the JT-60U to achieve longer confinement times is a neutral beam power injection system. The neutral beam adds power to the plasma in a controlled way, aiding in its organization. Some of the beams are directed in a tangential manner, thus adding to the spinning (helicity) of the plasma. The JT-60U team expects to use neutral beam injection to stabilize and improve the confinement time in the future.

Self-organizing Beltrami Vortex

That the fusion researchers are finding that the H-mode can be more easily sustained with the reversed shear configuration and the use of the neutral beam injection is very interesting. They are effectively approaching the minimum energy state of a self-organizing Beltrami vortex, as specified by fusion researcher Daniel Wells in the 1960s, working at the University of Miami (Coral Gables).

If the Japanese were to combine the Ampère-Gauss-Weber-Riemann-Beltrami-Bostick-Wells tradition with their demonstrated technological capability, commercial fusion could be here faster than anyone now thinks.

Dr. Wells, who is on the scientific advisory Board of *21st Century,* demonstrated that a plasma will self-organize and contain itself if the plasma currents are organized in the Beltrami "force-free" configuration. And this may indeed correspond to the zero-force observed by André-Marie Ampère.[1] If the Japanese were to combine the theoretical understanding of the behavior of plasmas of the Ampère-Gauss-Weber-Riemann-Beltrami-Bostick-Wells tradition,[2] with the technological capability that they have already demonstrated, commercial fusion could be here faster than anyone now thinks is possible.

Notes

1. For a discussion of the Ampère experiments, see Laurence Hecht, "The Atomic Science Textbooks Don't Teach, *21st Century,* Fall 1996, p. 21.
2. The work of Winston H. Bostick is discussed in "The Plasma Focus Fusion Device" by Charles B. Stevens, *21st Century,* Sept.-Oct. 1988, pp. 37, and in Bostick's article, "Morphology of the Electron, Photon, and Neutron: The Plasmoid Construction of the Superstring," *21st Century,* Winter 1990, p. 58 and its sequel, "How Superstrings Form the Basis of Nuclear Matter," Winter 1990, p. 66. The development of Daniel Wells's fusion work is discussed in his article "How the Solar System Was Formed," *21st Century,* July-Aug. 1988, p. 18.

Fusion Research

(Continued from page 64)

ruling Liberal Democratic Party, which supports fusion and ITER activities. Inoue reported that leading members of the Diet, or Parliament, meet occasionally with leading scientists in the fusion program, to discuss issues relating to the decision to host ITER in Japan.

As early as 1991, the Diet had passed a resolution on joining the ITER project in the design stage, noting that joining the project "is duly our responsibility for succeeding generations." The resolution also stressed that Japan should participate in international collaboration, "while noting the importance of promotion of domestic research and development capabilities in this field."

The Japan Federation of Economic Organizations, or Keidanren, consisting of a group of leaders in the industrial and financial community, have, in a recent reported titled, "Second Proposal Toward Enhancing Industrial Competitiveness," strongly suggested "ITER as an item to promote as a cooperative project by industry, the academic community, and the government."

Unlike the United States, where the proposal to build virtually any industrial facility attracts vocal "environmentalists," who manufacture community opposition, in Japan, three local communities, in Hokkaido, Aomori, and near the JT-60 project in Ibaraki, have already stated their desire to host the international ITER project.

"Japan hopes to actively promote the ITER project as a most important item in fusion research," both at home and abroad, Inoue said.

Hydrocarbon Fuels Aren't Fossils

by Paul Sheridan

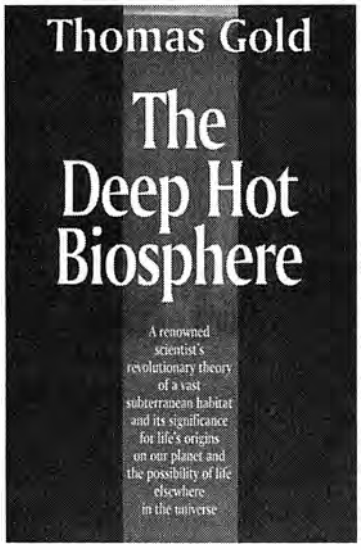
The Deep Hot Biosphere
Thomas Gold
New York: Copernicus, 1999
Hardcover, 235 pp., $27.00

The Deep Hot Biosphere is a culmination of more than 50 years of the life of its remarkable author, astrophysicist Thomas Gold, of Cornell University. Gold was a founding director for the Cornell University Center for Radiophysics and Space Research, chairman of Cornell's Department of Astronomy, and is the author of more than 280 papers in the areas of cosmology, zoology, physics, and astronomy.

Gold's thesis in *The Deep Hot Biosphere* is simple: Hydrocarbons have been in existence since the earliest times of the universe, and are part of the process of planetary formation. Their constituents, hydrogen and carbon, originated in the "primordial soup" from which Earth was formed. Earth's methane and petroleum, Gold says, are *abiogenic*—without biological origin.

Contrary to the currently promoted explanation, Gold says that hydrocarbons did not dissociate during these early times because of high temperatures of planet formation, as theorists claim. Current geological science, he shows, affirms that the temperatures were not high enough, especially when depth-related pressures are taken into account.

Gold contends that hydrocarbon sources can be found at great depths below the surface, not a few miles, but a few hundred miles. The deep-Earth sources of hydrocarbons are still working to this day, pumping tons of petroleum and methane gas up through the deep Earth's cracks and pores to the shallow sedimentary levels. It is here that drilling rigs access the upwelling that has been vertically dammed into reservoirs, Gold says. Hydrocarbons did not come from rotting prehistoric plants; they were here a few billion years before life occurred.



Gold discusses the latest space research information, much of which he discovered or proposed, which confirms that hydrocarbons are present on lifeless heavenly bodies such as moons, asteroids, comets, and, of course, the gas giants Jupiter, Saturn, Uranus, and Neptune. In fact, the blue coloration of planet Uranus is the result of methane, a so-called fossil fuel. As Gold comments, "I am sure there are no big stagnant swamps on Titan or Pluto."

To support the abiogenic theory, Gold notes several points:

(1) The geographical patterns that emerge from the oil fields, whether in the Middle East or Indonesia, all exhibit a correspondence to deep-Earth geological structure. This is in stark contrast to the haphazard deposition we find with surface life, and its subsequent fossils, which have never exhibited such patterns.

(2) Hydrocarbons from a particular oil field do not exhibit chemical changes as the depth of their extraction increases. But the fossils above them have constantly changing biological "signatures," which relate to their particular paleontological periods.

(3) Hydrocarbons are found in geographic areas where the amount of prehistoric life known to be at that location could never have provided the quantities of hydrocarbons involved. Most surface life is comprised of 90 percent water and 10 percent organic compounds. So, even if that 10 percent that is organic compounds had been entirely converted to "fossil fuels," it would not come close to the mass of hydrocarbons already extracted during the last 130 years.

(4) Because hydrocarbons are so consistent, the use of distinct trace metals can be used to identify their geographic origin.

(5) The existing petroleum reservoirs are refilling themselves—from the bottom! Gold explains: "The phenomenon of petroleum reservoirs that seem to refill themselves is widely reported, notably in the Middle East and along the U.S. Gulf Coast. I regard these occurrences as strong evidence for the deep-Earth gas theory."

The Carbon Case

Life as we know it is based on the chemical properties of carbon. Although there is discussion that silicon is another element that could provide a basis for life, carbon-based life is all that we have observed thus far. The origin, quantity, and duty cycle of carbon is thus fundamental to a complete understanding of life on Earth. As it turns out, certain chemical forms of carbon are also crucial to the preservation of life.

The land and ocean areas contain sedimentary rocks which have great quantities of carbon-based chemical materials called carbonaceous compounds. A full 80 percent of this material contains oxygen; for example, calcium carbonate, better known as limestone, is an oxygenated material. The other 20 percent that is not oxygenated, is comprised of the hydrocarbons—oil, coal, and methane. There is also a tiny fraction of not-yet-decomposed biological debris that is included in the carbon content of the sedimentary layer.

Carbonaceous compounds are also found in the atmosphere, mostly as car-

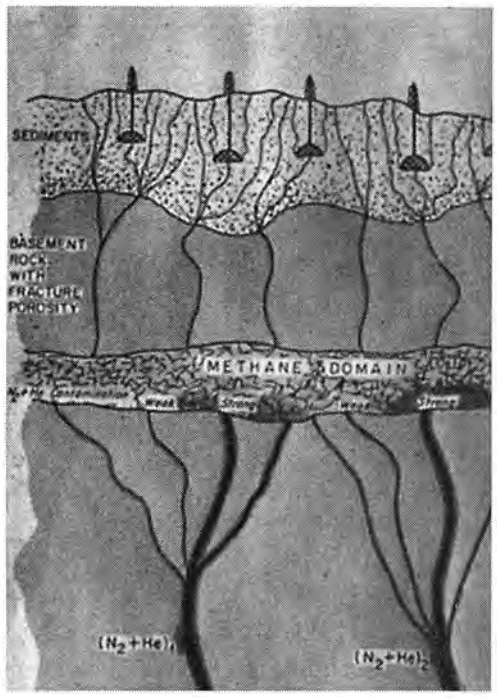


Figure 1
HELIUM TRANSPORT IN UPWELLING VOLATILES

Gold's schematic shows how the deep-earth gas theory would account for the helium association with methane. Helium produced by radioactive decay, at levels as deep as 300 km, is swept into the stream of upwelling nitrogen. At 100 km, designated here as the methane domain, nitrogen and helium mix with methane, and all three continue upward. The three gases arrive in the final fields with mixing ratios already determined.

Source: *The Deep Hot Biosphere*, p. 76

bon dioxide or methane. Together, atmosphere and the sedimentary layers of the land and ocean comprise what is called the atmospheric-ocean pool. The total amount of carbon in this pool is enormous, and the overwhelming majority of this "near surface enrichment" of carbon is in the sediments, not the atmosphere.

Venus and the Global Warmers

Environmentalists argue that this near-surface enrichment of carbon originated from the prehistoric atmosphere, and they promote the notion that the Earth's early atmosphere was very similar to that of Venus. Earth's carbon, they say, was "precipitated out" from atmospheric carbon dioxide into the atmospheric-ocean pool; absorption of carbon by prehistoric plants also occurred.

To hard-sell the global warming agenda, these theorists emphasize that Venus has vast quantities of the "greenhouse gas" carbon dioxide and, as a result, the temperature on its surface is about 700 degrees. However, these environmentalists usually fail to mention that Venus is 26 million miles closer to the Sun, or that its orbit is a near-perfect circle!

Unlike computer climate modellers or politicians with degrees in theology, Gold is an astrophysicist who has spent decades deciphering the details of how planetary bodies form. According to Gold, the general cosmic conditions that formed Earth and Venus were similar, but the devil is in the details. The early Earth was not characterized by the capture of gases from space, as was Venus. An indication of this is Earth's very low quantities of atmospheric krypton and xenon, compared with the rest of the solar system.

"Gold's theories are always original, always important, usually controversial—and usually right. It is my belief, based on 50 years of observation of Gold as a friend and colleague, that the deep hot biosphere is all of the above: original, important, controversial—and right."

—From the Foreword by Freeman Dyson, Institute for Advanced Study, Princeton

Gold also points out that if the carbonate rocks got their carbon from an early atmosphere, the deeper sedimentary layers should possess higher densities of carbonaceous compounds. If the carbon was "precipitated out" from an early atmosphere that was originally rich in carbon dioxide, then shallower rock specimens should show a successive decline of carbonaceous compounds.

The geological records prove otherwise, as Gold shows. There is no successive decline of carbonaceous compounds; the density is steady throughout geologic time. "The only sound explanation," Gold says, "is that atmospheric gases have derived mainly from outgassing of volatiles derived at depth from buried solid materials, not from an initial large atmosphere *acquired at the Earth's* formation or by later capture of gases from space."

More compelling, in my mind, is the issue of carbon-13. In the last decade, it has been proven that plants do not inhale carbon dioxide containing the heavy isotope C-13. The process of diffusion used by plants during respiration allows only the carbon dioxide containing C-12. Now, C-13 occurs in nature at a rate of just 1 percent. This means that if the hydrocarbons that were layed down over millions of years are the result of decomposing plant life, then these "fossil fuels" should show an absence of C-13. However, the samples of hydrocarbons taken from deep wells show no such isotopic constituency.

What is found is the original stellar nucleosynthesis constituency of 99 percent C-12 and 1 percent C-13. Gold cautions that a process of geological fractionalization, especially of methane, must be accounted for when discussing similar constituencies of the carbonate rocks.

The Helium Issue

Permeating every oil find throughout the history of the world, is the presence of outgassing helium. In fact, it is so plentiful at the well sites, that petroleum companies now use helium detectors as one of their oil prospecting tools, and commercial quantities of helium are piped, and repackaged for sale at well sites. Gold says: "The association of helium with hydrocarbons is probably the most striking fact that the biogenic theory ("fossil fuels") fails to account for, and therefore it has been for me of greatest interest."

Helium is inert, it does not react. It is not a member of the "primordial dozen."

(Recently, biophysicists determined that the stable nuclides that were the original minimum required for life to begin on Earth are hydrogen, carbon, nitrogen, oxygen, sodium, magnesium, phosphorous, sulfur, chlorine, potassium, calcium, and iron.) Plant life does not use helium for anything, and it is not derived from life. However, it is a fundamental product of stellar nucleosynthesis. It is also a known by-product of the radioactive decay of uranium and thorium. Both of these heavy nuclides are known to exist at great depth, about 200 miles down.

Curiously, helium is not found in meaningful quantities in areas that are *not* producing oil or methane. When the constituents of oil wells are examined for mixing ratios of helium, the data patterns are consistent throughout the world. Alone, helium does not possess the fluid pressures required to reach the surface in the manner observed.

The only way that such quantities and consistencies of helium mixing are possible, Gold explains, is by virtue of a deep source "carrier gas" such as methane. The depth of these sources is far below the penetration depths of surface life of their fossils. (See Figure 1.)

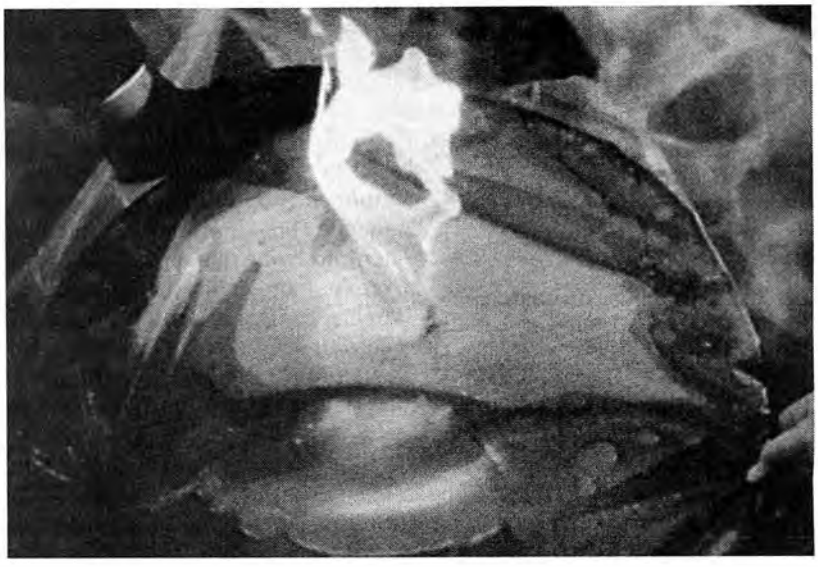


Figure 2
FLAME SUPPORTED BY GAS EMISSIONS OVER A WATER WELL IN THE SILJAN RING

Methane emissions in some places within the Siljan Ring of Sweden, where no "fossil fuels" are expected to be found, are strong enough to produce a flame. For this photograph, Gold covered a water well with a plastic sheet for a few minutes, then pricked the sheet with a pin and put a match over the hole. A flame shot up 30 to 40 cm, and then declined to 10 cm. Gold aborted the experiment after 10 minutes, when the plastic began to melt.

Source: *The Deep Hot Biosphere*, p. 112

The Diamond Evidence

Another item supportive of the abiogenic theory is the data Gold gathered from diamonds, which are a pure form of carbon. The temperatures and pressures required to form diamonds begin at depths of 70 miles. This far down, where the pressures are nearly 600,000 pounds per square inch, is far below the reach and survival of fossils.

Environmentalists and others claim that hydrocarbons cannot be created in the domains of such high temperatures; diamonds would disassociate there, they say, and, therefore, could not have possibly been created there. But such claims have failed to take into account the stabilizing affects of high pressure on temperature-related excitation. In any case, Gold has confirmed that between the interstitial spaces of the carbon crystals that comprise the diamonds, one finds hydrocarbons. The biogenic theory of "fossil fuels" has no explanation for this fact of nature.

The Siljan Ring Experiment

Another example Gold uses to illustrate the abiogenic theory is that of the Siljan Ring, a meteor impact structure in the central part of Sweden, near the city of Rattvik. Because the location is so far north, it is not considered a site where one would find an abundance of "fossil fuels." The interior of the impact structure has very few sedimentary rocks, as a result of the impact explosion. The interior also has a basement rock that is very thin.

In 1986, Gold and his Swedish and American colleagues drilled holes reaching nearly 5 miles down from the impact interior. The idea was to penetrate the lower crust, and possibly the upper mantle. At these depths, and in this location, no surface life that was decomposed over time could possibly have existed, which makes it an excellent choice for scientific *research* intended to test the abiogenic theory of hydrocarbon formation.

I emphasize "research" here, because the intention was not the large scale production of natural gas or crude oil. Despite this format, by 1991, the Siljan Ring experiment was producing 80 barrels of crude oil per day. These are not commercial quantities, but that was not the intention of the project; science was the intention.

The Russians have taken note of Gold's scientific findings; the major American petroleum companies have not yet done so. As of 1998, the Russians have more than 300 wells, drilled into the basement rock on the basis of the Siljan Ring experiment; all of which are producing commercial quantities of crude oil and natural gas.

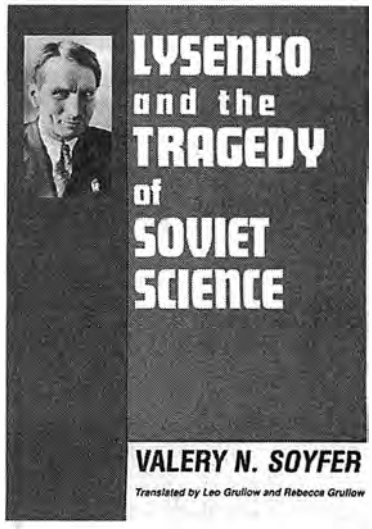
Using the knowledge and experience gained from Gold, the Russians have transferred their drilling technology to their former allies in Vietnam. So far, in what is called the White Tiger Field, they have drilled 20 wells into the basement rock. The Vietnamese are producing in excess of 6,000 barrels of crude oil per day per well, in an area in which the biogenic theory of "fossil fuels" maintains there will be no hydrocarbons.

It appears that the debate is over.

Paul Sheridan is an engineer and automotive consultant, based in Dearborn, Mich.

When Ideology Kills Science

by Denise Henderson



Lysenko and the Tragedy of Soviet Science
Valery N. Soyfer
(Translated by Leo Gruliow and Rebecca Gruliow)
New Brunswick, N.J.: Rutgers University Press, 1994
Hardcover, 379 pp., $39.95

The most telling statement by Dr. Valery Soyfer in *Lysenko and the Tragedy of Soviet Science,* comes when he describes how, after T.D. Lysenko's "fall from grace," his supporters and followers continue to bask in memories of the "good old days": "It made me think of the stories of Napoleon's soldiers," he writes, "who forgot the hardships of army service, the harshness of the tyrant, and the bitterness of defeat and retained only a sacred feeling of veneration for their commander."

Soyfer's comparison is no exaggeration. The damage done by Lysenko, as in the case of Napoleon, should not be forgotten. Indeed, one of the problems which arises when ideology supersedes *any* aspect of a society's life—cultural, political, economic, or scientific—is *the unwillingness* of those who were actors in the events to recognize that what cannot be tolerated is *the ideology itself, on the basis of which one has been making decisions, not merely an aspect of one's ideological system.*

For this reviewer, Soyfer's book has raised as many questions as it has answered, questions that can be pursued only through continuing research. The book is based on the historical record (Soyfer saw Lysenko's power and "charismatic charm" first hand, as a young student in the 1950s), as well as on personal interviews with those scientists and others directly involved in the Lysenko controversy, and is of great interest. It is a reminder of what serious research can contribute, even today, when such research is not considered necessary in our ever more "wired society," and its Internet full of "information."

Soyfer tells the story of Trofim Lysenko, the "agronomist" who became Stalin's favorite in the late 1920s and early 1930s—when the Soviet Union was in dire need of food and had experienced crop failures—by promising that he could transform non-living matter into living matter, that he could increase crop yields, that he could grow beets from yams, and other such absurdities. Of course, Lysenko falsified most of his results, something which not even the Politburo realized until the 1950s.

For whatever reason, Stalin bought into this, even though competent agronomists, including many who were trained before the Russian Revolution by associates of the great scientist Vladimir Vernadsky, were attempting to explain to Stalin that the nation needed to set up serious agricultural research stations, to develop new and more potent, more disease-resistant crops. They emphasized that all this would take time; that if such a program were to be followed over time, however, the result would be a much more advanced Soviet agriculture.

Lysenko vs. Frontiers of Science

The leading proponent of these ideas was Nikolai I. Vavilov, a student of the agronomists Dmitri N. Pryanishnikov and Gleb M. Krzhizhanovsky, both of whom had studied in Germany. Vavilov also apparently had ties to the OGPU, the predecessor of the KGB. Although his early expeditions to Afghanistan were as much military as they were agronomical, Vavilov seems to have been a universal thinker, who truly wanted to implement a program that would improve crop yields, crop quality, and so on, in the Soviet Union. Vavilov, his research institute, and his fellow researchers, thus became the first casualties of the power-hungry, uneducated Lysenko.

Lysenko could not have achieved his success, without the perfidious role played, in particular, by J.B.S. Haldane, the British scientist (who, like Lysenko, abhorred field laboratory work), who

Soviet Agriculture, May 21, 1937, p. 3, as reproduced in *Lysenko and the Tragedy of Soviet Science*

Lysenko, right, demonstrates the results of wheat crossing to Academicians N.I. Vavilov (left) and G.K. Meister (center).

Photo by N. Kalashniko and N. Kuleshov, in *Pravda*, Jan. 3, 1936, as reproduced in *Lysenko and the Tragedy of Soviet Science*

Stalin (top right) and other party leaders listen to Lysenko (left foreground) at a 1935 meeting honoring workers bringing in the best harvests.

praised Lysenko's "ideas" and "methods." Haldane wrote several articles for Western newspapers praising Lysenko, and Haldane's undeserved reputation thus gave Lysenko a certain amount of credibility in the West.

Another, and just as significant, victim of the Lysenko takeover of Soviet biology, was Alexander Gurwitsch, who developed the field of mitogenetic radiation. Although the significance of Gurwitsch's work on mitogenetic radiation had been well recognized in the 1920s and 1930s, Lysenko denounced work on the radiation of the cell as being anti-scientific and anti-Marxist—not to mention anti-Darwinian. Other Soviet officials and scientists apparently found means to protect Gurwitsch, perhaps because of his scientific reputation, but others in this field were not so lucky. Ervin S. Bauer died in a Soviet prison camp, and Alexander Chizhevsky spent 10 years in a Soviet prison camp.

In 1948, Gurwitsch resigned his position as head of the Institute for Experimental Biology, in protest over Lysenko's methods and control of Soviet science. By the 1950s, Gurwitsch's institute had been dismantled.

Soyfer's book originally circulated as a *samizdat,* an underground document, inside the Soviet Union. Consequently, although he refers obliquely to the fights which erupted around Gurwitsch and his institute, he does not mention Gurwitsch by name, referring only to some of Gurwitsch's more outspoken students who opposed Lysenko when the Institute for Experimental Biology was closed down in 1948.

After the war, some scientists, including some of Gurwitsch's collaborators, began to rebel against the oppressive control which Lysenko had over Soviet science. In 1956, Pyotr Kapitsa, a well-known Soviet scientist, began organizing scientific seminars at the Theoretical Physics Institute in Moscow, to try to bring true science to the fore once again. The discussion was to be on radiation genetics. And, despite an order "from the top" to cancel the seminar, Kapitsa insisted that it would go forward, and it did.

Perfidious Haldane

Given that Lysenko was taking aim, in both the Vavilov and Gurwitsch cases, at the frontiers of science, it is interesting to read Soyfer's citation from Haldane's obituary, written by Haldane himself years before his death:

"I consider Lysenko a very good biologist, and some of his ideas were right. . . . And I believe Soviet agriculture and Soviet biology were very unfortunate in that he was given so much power under Stalin. . . . I . . . am deeply convinced that if I had been made dictator over British genetics or English philosophy I would have played just as catastrophic a role."

Although in later years, Haldane admitted that his support for Lysenko had been an error, the damage had long since been done.

This issue cannot be looked at simplistically. One can only hypothesize about the reasons that the scientific research project proposed by Vavilov was rejected, when it would have made the Soviet Union self-sufficient in food production, instead of producing droughts so severe that Soviet Russia began looting Ukraine of every last stalk of grain, causing the famine there that killed millions.

In addition to the general political climate, I believe that the foremost reason for the rejection was that the methods Vavilov was proposing would have meant *not* collectivizing agriculture, but more of a U.S.-style of family-based farms, which from the time of Abraham Lincoln until recently, has been the most productive system of agriculture in the world. Vavilov's program was based on his travels to five continents—a trip which seems to have followed the route of Alexander von Humboldt, whom Vavilov had read as a student—during which he collected seeds of the most viable and productive plants from around

the world. Ironically, however, Vavilov supported collectivization and defended it both in public and in private.

Another reason for the demise of Vavilov, was the confusion created by the growing British control over the science of genetics. It is clear from another source (Zhores Medvedev's book, *The Rise and Fall of T.D. Lysenko,* written in the heat of battle against Khrushchev's and Lysenko's Virgin Lands policy in 1963), that either for populist or other, more serious reasons, Stalin opposed the use of genetics to create race science. The British camp—the Huxleys, Haldane, and others—in the 1920s and 1930s were transforming genetics into the science of eugenics, or so-called race science: the idea that the so-called different "races" of man exhibited different characteristics, and that therefore some "races" were superior and others inferior *by nature.*

Lysenko and the Tragedy of Soviet Science

Lysenko (center right) in the midst of a heated discussion during a visit of foreign scholars to the Institute of Genetics of the Soviet Academy of Sciences in the late 1950s.

One Lysenko supporter, V.N. Studitsky, wrote an article with subtitles including "Mendelist-Morganist genetics in defense of Malthusianism," and "Morganist genetics and fascism," (referring to the biologist Morgan). In another article, Studitsky wrote: "And, in fact, the followers of Virchow, Weismann, Mendel, and Morgan, talking of the immutability of the gene and denying the effect of the environment, are preachers of pseudoscientific genetics, which provided the base for the racist theory of fascism in capitalistic countries. World War II was unleashed by imperialist forces whose arsenal also included racism."

But, here is the problem: Vavilov himself—who by 1939 was in the *Lubyanka,* the Moscow KGB prison, was not talking about genetic engineering of human beings at all. Vavilov had never discussed such a thing, although a minority of those at his institute were, in

Who Was Trofim Denisovich Lysenko?

T.D. Lysenko was born on Dec. 30, 1898, in Ukraine. He worked in the fields as a child, and received no formal education until he was 13. After graduating from a local school in 1913, he attended a local horticultural school from 1913-1915.

In 1916, Lysenko attempted the entrance exams for the vocational School of Agriculture and Horticulture in Uman, Ukraine, but he failed the Scripture exam. The next year, he took the exams again and was admitted. According to Soyfer, Lysenko's reading, writing, and cultural background was weak.

The school was caught between the White Russians and the Communists during the civil war, and it closed down. In 1921, Lysenko went to Kiev to take make-up courses. Later that same year, he received an internship at a selection station, where he did some work on peas. In 1923, he published two articles on this work, which contained nothing of major importance.

According to Soyfer, the "world of books was alien to him." Lysenko knew no foreign languages, and scorned the world outside the Soviet Union. All his life, he believed that Soviets had nothing to learn from the "bourgeois science" of the West.

In 1925, Lysenko was hired as a junior specialist at the Ordzhonikidze Central Plant Breeding Experiment Station, which was directed by Nikolai Vavilov, who was rapidly becoming a world-renowned specialist in agronomy.

The Vernalization Fraud

There, with the help of his father, a farmer, he began to put forward his so-called theory of vernalization. This was neither new—it had been tried in the 19th century in the United States and Russia—nor particularly workable as a method. The idea was to be able to sow winter wheat in summer by soaking the wheat before planting it. It was a time-consuming and costly method, which did not in any way significantly increase wheat yields; therefore, it had been abandoned in the West. The Soviet Union was even less capable of implementing such a program, given its limited access to seeds, equipment, and so forth.

Nevertheless, Lysenko claimed that he and his father had done successful experiments on vernalization, and it soon became touted as one *of the* "quick-fix" solutions to the Soviet Union's failing crops.

Lysenko's Potemkin Farm

Lysenko was also given control of a government farm on the outskirts of Moscow, called Lenin Hills. This can

fact, attempting to apply genetics to humans. Vavilov's primary commitment, however, was to the development and advancement of agriculture for all the Soviet people. He never expressed any belief that any sort of genetic engineering ought to be applied to human beings. And unlike the charlatan Lysenko, he was a hard worker, who knew how to achieve results in his agronomic work.

Vavilov died in prison of dystrophy in 1941. For about a year before his death, an attempt was being made to move him to one of the new "scientists' prison camp," where imprisoned scientists could pursue research for the fatherland. But his health had already deteriorated to such a point that he died before he could be moved. This was just as World War II was breaking out and precisely at the point that the issue of food production was crucial for the Soviet Union.

Lysenko (right) and Khrushchev (second from right) at Lysenko's farm, Lenin Hills in 1955.

The Major Damage

The major damage done to Soviet science by the promotion of Lysenko was the eclipsing of the work of Gurwitsch. That is, if one were to accept Lysenko's "ideas," such as they were (ghost-written in the main by those more literate than he), then one would have to accept the idea that man is no better than a plant. For Lysenko believed in spontaneous generation of non-living matter into living matter, whereas Vernadsky earlier, and then Gurwitsch in more in-depth studies in the 1950s, was able to demonstrate that there is a distinction between living and non-living processes.

Of course, one must then go the further step to realize that there is also a distinction between *human* living processes, and other living processes, as Vernadsky and Gurwitsch, in the only be described as a "Potemkin Farm." That is, it was a farm which operated with the best of everything—conditions that no collective farm inside the Soviet Union could match. Even so, Lysenko was only able to demonstrate that his methods were a fraud.

The farm was used to keep the bellies of the Politburo full. It also was used for experimentation—but all of the experiments were tainted; they were not controlled in any careful way, and other scientists could not replicate them.

Although this Potemkin of a farm would not be investigated until 1965, its contributions to Soviet agriculture were, if anything, negative. Lysenko's theories on breeding were absurd, and he simply falsified results on the plant experiments and then claimed that his critics lied about their results.

The Reality of War

During World War II, the Soviet Union needed food, and Lysenko returned to traditional agricultural methods—or, at least, workable methods, given the limitations of collective farming and the previous destruction of qualified agronomists. For example, Lysenko proposed that peasants sow the eyes of potatoes to produce new potatoes. Although he claimed he had "discovered" this technique, it was a widely known and used method. Other such workable methods were "discovered" and used to try to provide as much food as possible under wartime conditions.

Suspicions began to arise against Lysenko after the war. In part, this was because one of his brothers became the mayor of a town in Ukraine under Nazi occupation. When famines broke out in Kazakhstan and Siberia, as well as European Soviet Russia after the war, there was a brief window of opportunity for an attack on Lysenko. This was led by Politburo member Zhdanov and his son, along with students and associates of Gurwitsch.

In August 1948, Lysenko led a counterattack, which forced Zhdanov to recant his criticisms of Lysenko. In the 1950s, in the post-Stalin era, political opposition to Lysenko again arose, and even Khrushchev criticized him. But, in the context of the continued cooperation with the United States, and the offers made under the auspices of U.S. Special Envoy Harry Hopkins to provide agricultural equipment to Russia, Khrushchev, who needed to improve crop yields, accepted the proposal for the Virgin Lands campaign from the semi-eclipsed Lysenko, and once again, Lysenko rose to ascendancy.

The Virgin Lands campaign continued until 1963, by which point the Soviet Union was once again importing grain. In that year, a grouping in the Politburo decided it was time to get rid of Khrushchev, for many reasons, including the fall in agricultural production. Lysenko was soon out of power.

Lysenko, still heading Lenin Hills Farm, died in 1976.

tradition of Western classical science, had both insisted.

As long as Lysenko held sway, the work of Gurwitsch could not flourish, and to this day, it does not get the hearing it should have. Further, because it is in the interests of British ideology to insist that man can be genetically determined like a race horse, or a genetically engineered cow, to this day, the real science of living and non-living processes is eclipsed.

Because of Lysenko's control over science, byzantine efforts were made to protect serious researchers like Gurwitsch and others. They were hidden in institutes and given nondescript titles, or no title at all. In the 1950s, a major push was made by a group led by M.A. Lavrentyev, which set up an Academy of Science in Siberia, so far away from Moscow that it was considered safe from Lysenko's control. Later, Khrushchev decided to intervene, and almost succeeded in shutting down the Siberian branch of the Academy of Science, which to this day is well known for its high level of scientific research.

Lysenko was also the author of the Virgin Lands campaign, in which in the 1950s, Khrushchev sent Russian teenagers from the city, who knew nothing about agriculture, out to till barely arable soil in Kazakhstan, an experiment which failed miserably. Lysenko also served as one of many links for Khrushchev into the World Peace Congress, one of the entry points through which Bertrand Russell, on behalf of the British oligarchy, undermined popular support for scientific progress in both the Soviet Union and the United States.

Notes Soyfer, Lysenko "was not reluctant to talk about peace . . . and he placed his people in the leadership of the movement. . . . In 1950, Glushchenko [a Lysenko protégé] became a member of the Soviet Peace committee, its presidium, and finally of the World Peace Council."

Soyfer, unfortunately, has not quite learned the lesson of the significance of this bureaucratic control over science. At the end of his book, he insists that scientific freedom is alive and well in the United States. As an emigré who was not permitted to work in his own country (he was stripped of his scientific degrees because he had supported Andrei Sakharov), and was able to come to the United States and continue his work on repair enzymes in higher plants, Soyfer may simply be blind to the larger reality, or unwilling to criticize it.

One recent example of the control of science here is the use of the press to slander unwanted research, as in the case of cold fusion. Cold fusion researchers Fleischmann and Pons, both well-known electrochemists, had to leave the United States to be able to continue their work, once the Establishment decided that cold fusion did not work.

And today, five years after Soyfer's book was written, the U.S. national laboratories are being directly confronted with the issue of their researchers' right to scientific freedom.

Overall, Soyfer provides the background to a crucial chapter in science history. We in the United States and Russia, as well as those in any country who are serious about harnessing science for the development of the common good, should learn the lesson, and begin anew to educate serious scientific cadre, and give them the freedom necessary for creative scientific work.

Origami and the Geometrical Imagination

by Susan Welsh

Origami: The Secret Life of Paper
CD Rom for Windows and Macintosh[1]
Salinas, California: Casady & Greene, 1998
$29.95

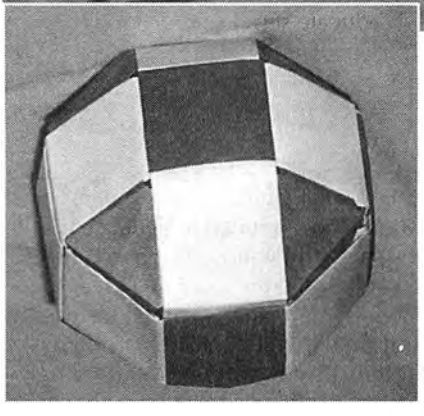
My son Jacob, who is nine, taught a geometry lesson to his third grade class last spring. Using the excellent "Platonic Solids" video and construction materials from Key Curriculum Press,[2] he had his classmates constructing the five platonic solids from cardboard polygons and rubber bands, while the teacher took a coffee break.

Jacob has loved geometry since he was two. (As a matter of fact, all children of that age do; the trick is not to let their natural love of block-building, their playful curiosity about the *shape* of things, be suffocated by our video-game culture!) It is no surprise, then, that Jacob also has become a lover of origami, the Japanese art of paper-folding. We don't have a television, so he has a good concentration span—when he chooses to—and will work for hours on a construction.

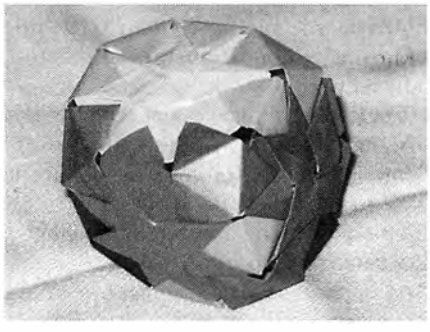
The CD Rom from Casady & Greene is a useful introduction to origami for beginners, and would be a valuable addition to a classroom collection of computer software. Through origami, one does not learn the sorts of factoids needed to pass the state Standards of Learning exams, but one can, with patience and perseverance, develop a quality that is of inestimable value: the geometrical imagination.

The CD Rom uses the device of a Japanese house, with various rooms to explore in which one can find beautiful examples of origami by some of the world's masters, along with biographical material on the artists, and an extensive bibliography. There are instructions, including videos, on how to make 12 origami figures, and a unit on making origami paper out of junk mail.

Most useful are the videos that show a person doing each step of the construction, alongside of diagrams and written instructions. Using all three together, the beautiful constructions fall quickly into place.



Above: Jacob exploring origami via this CD Rom. Left one of his geometric constructions made by paper-folding. The shape is a rhombicuboctahedron, according to a design from Unit Origami, *by Tomoko Fusè. Below: An octagonal star construction.*



Another nice feature is the overview the CD gives of the work of many different artists (although you are not taught how to make their models, some of which are exceedingly complex). Such an overview is difficult to get from books, each of which generally represents the work of only one artist.

Is it worth all the high tech, when you can get a paperback from the library and a pack of origami paper for $4? Yes, and no. An interested child, with an adult to help decipher the instructions at first, has no need to spend the $29.95 (assuming you already have the computer). An intermediate student of origami will exhaust the interest of this CD quickly, once the 12 models have been constructed. But for novices, whose parents or teachers find themselves "all thumbs," and who are baffled by the directions about "valley folds" and "squash folds," the set provides a good boost.

Notes

1. The CD can be ordered by phone (831) 484-9228, or via E-mail: c&g@sadyg.com, or the company's website: www.casadyg.com.
2. These are now going out of print; the company says there was no demand for the material. What a shame!

Sergei Korolev: The Soviet Wernher von Braun

by Marsha Freeman

Korolev: How One Man Masterminded The Soviet Drive to Beat America To the Moon
James Harford
New York: John Wiley & Sons, Inc., 1999
Paperback: 432 pp., $17.95

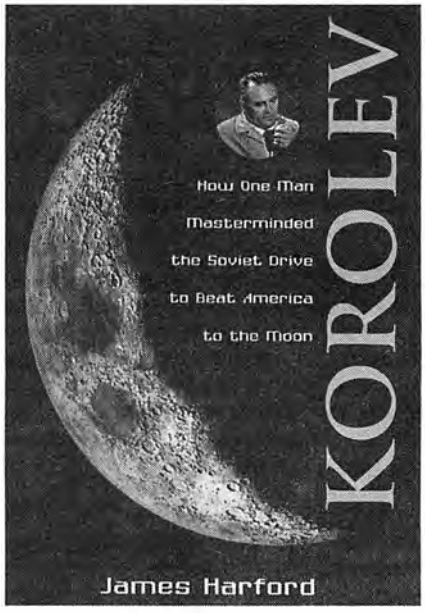
Throughout history, it has always been a mere handful of men who have had the ability to lead nations to realize humanity's greatest projects. Sergei Korolev was one such man. If not for his ingenuity, steadfastness of purpose, great personal sacrifice, and vision, it is not likely that there would have been any possibility for the Soviet Union to have accomplished its "firsts" in space.

James Harford's book is required reading to gain a proper perspective on the declining state of what is now the Russian space program. His biography of Korolev chronicles the remarkable accomplishments of a man who worked under the worst possible circumstances, demonstrating how the human creative spirit can triumph, even under conditions of the greatest adversity.

Harford's prologue opens with the following: "One day in the early 1960s, Sergei Pavlovich Korolev was looking at a newspaper photograph of Wernher von Braun, then being lionized in the United States for his part in the upcoming Apollo program. His comment, recalled Antonona Otrieshka, a staff assistant: 'We should be friends.' "

But Korolev died at the height of the Cold War, in 1966, so such a meeting was not destined to occur.

Just as the German emigré Wernher von Braun was a driving force in the United States for launching an Earth-orbiting satellite during the 1950s, so it was Ukrainian emigré Korolev's vision, tenacity, and commitment to that same effort, that enabled the Soviet Union to be the first to open the Space Age on Oct. 4, 1957, with the launch of the Sputnik satellite.



Like von Braun, as a young man, Sergei Korolev was inspired to dedicate his life to space exploration after becoming acquainted with the work of a great space pioneer: Hermann Oberth in the case of von Braun, and Konstantin Tsiolkovsky in the case of Korolev. While biographers differ as to when, or even whether, Korolev ever met Tsiolkovsky, who died in 1935, there is no question that, as Harford states, "Korolev began to build what Tsiolkovksy had conceived."

Korolev's life paralleled von Braun's in many ways. Both spent the 1930s working for brutal, totalitarian regimes; both were imprisoned by these regimes—the Nazis in one case, Stalin in the other. Both worked on projects of such strategic import, that, as distasteful as it was to each of them, they had to appeal to the leaders of both dictatorships for support, in order to make any progress.

They both began their careers in space development through serious study, participation in amateur rocket societies, and later, support from the military. Both died prematurely and both saw one overarching goal for mankind in space: trips by human beings to Mars.

Prelude to Space Exploration

Both the German and Soviet efforts had their roots in the experiments of enthusiastic young amateurs, *who in the* 1920s and early 1930s, were trying to turn the ideas of Tsiolkovsky and Oberth into hardware.

Before his 20th birthday, in 1926, Sergei Korolev moved to Moscow from Ukraine, to study at the prestigious Bauman Institute. As a youngster, he had become enamored of the new technology of flight, and was intent upon studying at the place where other young men were also designing the gliders and planes of the future. Because the Institute was so near to the Central Aero-Hydrodynamics Institute (TsAGI), as Harford relates, many students were also involved in projects there, and could gain the experience of taking their conceptual studies to practical reality.

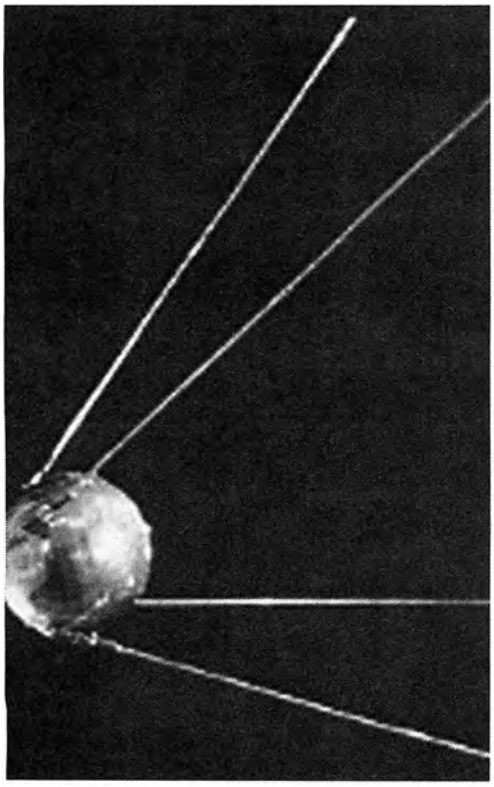
Interest among the technical community in Moscow was broadening to encompass "flights in Universal Ether," in addition to flights in the air, when Korolev arrived in Moscow. In 1927, the First World Exhibition of Interplanetary Apparatus and Devices was held in Moscow. Spaceplane models by the Latvian engineer F.A. Tsander (1887-1933), whom Korolev would meet four years later, and papers and concepts by Robert Goddard, Oberth, and Max Valier, were on display. Like Friedrikh Tsander, at that time Korolev thought that the major role for the rocket would be as an adjunct to aircraft.

By 1930, an amateur rocket experimental group had been established in Moscow, called the Group for Studying Reaction Propulsion (GIRD), which was led by Tsander. Korolev and Tsander, both working at TsAGI, devoted their after-hours and weekends to the experiment GIRD was embarking on, Harford

reports. Tsander's unsuccessful rocket design was tested by the GIRD staff in March 1933, while Tsander was dying of typhoid fever. It was followed by more advanced designs, and the first Soviet liquid-fueled rocket was launched on Aug. 17, 1933.

Eight days later, Korolev wrote an article for the newspaper *Vechernaya Moskva,* with the title, "Towards the Rocketplane." Misled by some of the fantastic versions of international rocket developments then prevalent in the press, Korolev reportedly warned that the Germans had "allegedly built a rocket of such size that it will be able to fly a man." This was not the first time the Russian rocket enthusiasts were misled about their counterparts abroad. In 1924, the world's first amateur rocket society, established by Tsander in Moscow, had fallen into disrepute by debating for days the "fact" that Goddard had launched a rocket to the Moon!

In his 1933 article, Korolev warned that although rocket technology held great promise, "practical resolution of this huge problem requires many years and persistent work."



RSC Energia, Korolov, Russia

The first Soviet "first"—Sputnik.

As Harford reports, in August 1932, the amateur rocket organization in Moscow began receiving funding from the military, thanks to the interest of Marshal Mikhail Tukhachevsky. In October 1933, the Soviet Council of Labor and Defense created the Reaction Propulsion Institute—a recommendation by the Revolutionary Military Council of the Soviet Union—and Korolev was the deputy chief engineer of the new institute.

Between 1933 and 1938, families of rocket engines and their guidance, control, and other requirements were under full-scale development in these new institutes in Russia.

Research in the Gulag

At the age of 31, Korolev's work on liquid propellant rockets had become well recognized. Without warning, in the early morning hours of June 27, 1938, two men from the Soviet secret police and two "witnesses" entered Korolev's apartment, and he was taken away.

The charges against Korolev and other rocket researchers, who had also been arrested, was "subversion in a new field of technology." They were accused of collaborating with an anti-Soviet organization (of rocket enthusiasts) in Germany. Soon after, Wernher von Braun and his German colleagues would be arrested by the Nazi Gestapo, with similar charges levelled. Most likely, it was Korolev's work with Tukhachevsky, who was executed by Stalin, that led to Korolev's arrest and exile.

Korolev was sentenced to 10 years in prison, and, by October 1939, was "in one of the most dreaded of all prisons, a camp in the Kolyma area of far eastern Siberia," Harford reports. The privations of his time in the camp, under unspeakable conditions, led to a permanent deterioration of his health, and, in all likelihood, contributed to his premature death at the age of 59.

But Korolev was called back to Moscow and was moved, in September 1940, to a *sharaga,* or prison factory—most likely through the intercession of the great aircraft designer Andrei Tupolev, who himself had been

RSC Energia, Korolov, Russia

Sergei Korolev (1906–1966)

arrested on Oct. 21, 1937. Earlier, Tupolev had been Korolev's teacher at the Bauman Institute. During World War II, Tupolev and Ilyushin attack aircraft, which were made famous during the war, were produced in Gulag "design bureaus." In the summer of 1944, the entire experimental design bureau where Korolev was working, was released from custody, and Korolev's "prior convictions expunged."

In 1945, Korolev was commissioned a colonel in the Red Army. As Harford reports, Korolev flew to Germany in September to "join other Soviet colleagues gathering information" on the V-2 rocket, which had been developed by the von Braun team during the war. Von Braun himself was already in the United States. When German technical specialists, snatched from the East German Soviet zone, were brought to the Soviet Union to work on missile technology, one group worked under Korolev. But by 1950, the Germans were being sent home, and Korolev was leading the effort to design, build, and test the world's first ICBM, the R-7.

According to Harford, Korolev believed that the World War II V-2 liquid propellant technology could not be significantly extended, and he preferred to work on his own designs.

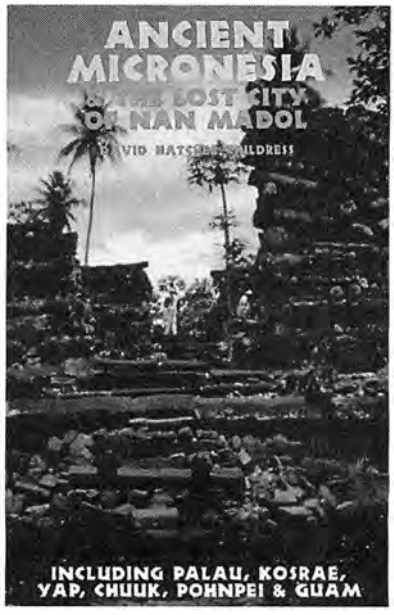
A Leap into the Space Age

On Oct. 4, 1957, the world was awed by the orbiting of the first artificial satel-

(Continued on page 79)

Micronesia's Lost Cities

by Charles Hughes



Ancient Micronesia & the Lost City of Nan Madol
David H Childress
Kempton, Ill.: Adventures Unlimited Press, 1998
Paperback, 200 pp., $16.95

In this book on Pacific exploration, David Hatcher Childress describes in great detail, with many illustrations, maps, and color plates, the archaeological wonders found in the islands of Micronesia. His belief is that human civilizations have existed for 50,000 or so years, and that what the archaeological establishment would have us believe is disinformation. The evidence for those civilizations is what his book attempts to portray, albeit with a focus on the "mysteries" of the place.

Micronesia means "Little Islands." The total area of these tiny spots of land northeast of New Guinea amounts to about the area of Rhode Island, but spread over an expanse of the Pacific Ocean that is as large as the United States, about 4,000 miles. Micronesia consists of four great archipelagos, or island groups: the Marshalls, the Gilberts, the Carolines, and the Marianas.

Ruins which were once cities, built from giant stone slabs, are to be found on many of these islands. The largest and most extensive of these is on Pohnpei Island, and there is a similar but smaller ruin on Kosrae Island. Pohnpei is found in the Caroline group of Micronesia, and its large, ruined city of Nan Madol covers about 11 square miles. Nan Madol has been called the eighth wonder of the world, or the Venice of the Pacific, because the city is divided into artificial built-up islands separated by canals.

The ruin of Nan Madol is estimated to contain more than 250 million tons of basalt rock, with the average stone weighing between 10 and 20 tons, and and some more than 50 tons. The thousands of details Childress gives, including construction particulars, point to a city which was surely an important capital of a lost civilization of sea people, perhaps 5,000 to 10,000 years ago.

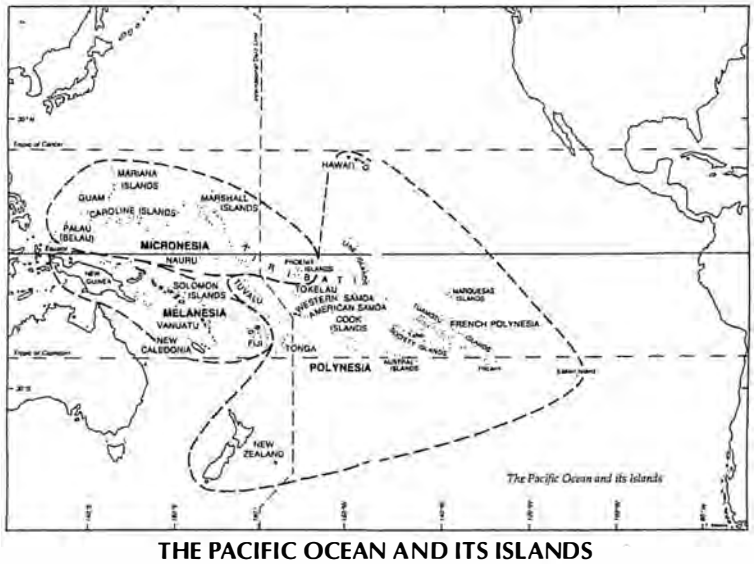
Now, one reaches the city by boat, or by wading across a coral reef at low tide. The largest building in the "downtown" of the city has walls 30 feet high, constructed of basalt logs 5 meters or so long, of hexagonal shape. To obtain the required 250 million tons of stone, Childress estimates that an entire mountain must have been dismantled.

The author conducted many underwater explorations in the ocean near Nan Madol with a team of eight divers, in order to investigate rumors of another city submerged near Nan Madol.

Childress also reports on some of the investigations of the late Barry Fell, and other investigators, such as John McMillian Brown of the University of New Zealand, concerning Egyptian, Libyan, and Dravidian contacts in Micronesia. One of the unconventional ideas he presents is that agriculture may have started in the Pacific and not the Near East.

For example, Childress discusses the findings of archaeologists M. Spriggs and S. Wickler of Yale University, who excavated Kilu Cave on Buka Island in the Solomons and found tools of stone with starch residues from the taro root. Radiocarbon dating on the residues placed them at 28,000 B.C. Taro is still a major crop on this island, as it is throughout Polynesia.

Childress also reports on many ancient world artifacts found on the beaches in Micronesia and inland in nearby Australia: for example, a three-foot carved statue of a baboon holding a papyrus scroll was found in Queensland, near a mound in the town of Gympie. In Geraldton, Australia, in



THE PACIFIC OCEAN AND ITS ISLANDS

Nan Madol, the lost city explored in this book, is on the eastern Caroline Island of Pohnpei.

Source: *Ancient Micronesia*, frontispiece.

1963, a workman found an Egyptian bronze plate. The Sidney suburb of Fivedock yielded gold coins and Egyptian jewelry. Numerous wrecks have been excavated on Australian beaches, Many of them very large ships, which were built without nails or screws.

Childress describes the excavation of some 400 gravel hills at the Isle of Pines on New Caledonia, which contained cement columns. Yale scientists found that the columns which included organic matter were dated to between 5,000 and 10,000 years ago.

Childress has written almost a dozen books describing his explorations over the last 30 years, and is the founder of Adventures Unlimited Press. A self-described maverick archaeologist, who focusses on the "mysteries" of the places he visits, he has combed the globe to gather data on lost civilizations, often walking or hiking for weeks at a time to see ruins at some very remote sites where no tourist would attempt to travel.

Despite the emphasis on "mysteries," this, and the other books by Childress in his ancient lost civilization series, have much to provoke readers.

Wernher von Braun

(Continued from page 77)

lite, Sputnik. Less than two months before that, the R-7, which launched Sputnik, made its first successful test for its intended purpose, carrying a dummy warhead. The leap into the Space Age for the Soviet leadership was useful merely as a publicity stunt, to prove the superiority of the Soviet system to capitalism.

As Harford richly documents, the Soviet regime was constantly an obstacle to Korolev's plan for the orderly progression of new space capabilities. The Soviet government funded design bureaus that were developing competing designs to Korolev's, in everything from second-generation ICBMs to Moon rockets—a practice that created confusion and left inadequate funds for everyone. The regime, and Khrushchev personally, made outrageous demands on the Chief Designer to meet deadlines for space "firsts," based on political considerations, such as speeches before the United Nations, or concerns about when the United States would do it.

The Anonymous Chief Designer

Unlike Wernher von Braun, who became a widely recognized proponent of space exploration in the United States through magazines, newspapers, and television in the early 1950s, Sergei Korolev's name was not known outside the circles of the Soviet government, the military, and his colleagues who were involved with the space program, until after his death. Although intelligence services in the West knew there was a "Chief Designer," his name was never made public. He was not seen in photographs with the cosmonauts, nor at state ceremonies where awards and medals were bestowed upon those recognized for their contributions. Yet, he was the man most responsible for their successes.

Anonymity was undoubtedly vexing to Korolev, although it was not his most serious problem. But the successful effort by the paranoid Soviet government to keep the identity of the Chief Designer a virtual state secret, produces a most frustrating aspect of Harford's book.

Although the author spent many years and made an herculean effort to interview hundreds of people who knew and worked with Korolev, the Chief Designer's thoughts, his vision of the future, and his long-range plans for space exploration, are missing. There is no explanation given as to how how this man, who had been unjustly imprisoned, and stymied at every turn by a vicious and opportunistic regime, could have driven himself and his colleagues to accomplish what they did during his lifetime. Ones assumes that this was possible because he had a goal that allowed him to see everything else around him as merely ephemeral.

It seems inconceivable that Korolev left no notes, diaries, letters, or other personal material which would give us an insight into what ideas sustained him. It *is* conceivable that such material has not yet been made accessible to researchers by the Russian government, which is a great disservice to the memory of the Chief Designer.

It can be hoped that such material will soon be made available, and that Jim Harford will be able to write a sequel to this informative and moving biography.

Ancient Civilizations in Australia's Kimberleys

by Marjorie Mazel Hecht

Courtesy of *The New Citizen*

Rock painting of an ancient watercraft, shown in graphic outline for clarity .

The Bradshaw rock art of the Kimberleys is a stunning record of early civilization in Australia, dating back to at least 20,000 years before present (YBP) and perhaps even as early as 40,000 YBP. The rock art is named after explorer Joseph Bradshaw, who first observed it in 1895. At the time, he wrote that the paintings looked distinctively Egyptian. "Indeed, looking at some of the groups, one might think himself viewing the painted walls of an Egyptian pyramid."

The next reporting of this rock art was done by an expedition of the Frobenius Institute, based in Frankfurt, Germany, which studied the paintings for several months in 1937-1938, made copies of them, and named them "Bradshaw" figures, after their discoverer.

This very brief report reviews some of the work of contemporary explorer Grahame Walsh, who over the past 21 years has discovered and recorded tens of thousands of Bradshaw rock art sites, travelling 100,000 kilometers over some of the world's roughest terrain. A more detailed report, on which this article is based, appears in the Aug.-Oct. 1999 issue of *The New Citizen,* published by the Citizens Electoral Council of Australia.[1]

Walsh, whose data base of rock art contains 1.2 million images,[2] is convinced that the Bradshaw rock art is not simply an art form, but an early type of hieroglyphics, using a stylized human form, and is similar to that of the Egyptians, or the Mayans in Central America. He has demonstrated that the people who painted the Bradshaw figures had a maritime culture and arrived in Australia with their high culture fully formed.

Walsh's work, therefore, contradicts the assumptions of accepted archaeology and anthropology, which maintain that modern man descended from apes only very recently (in the scale of prehistory), that proto-humans had no capability of speech until about 40,000 years before present, and that such proto-humans could not possibly have had a maritime culture, or even hunted for food. Particularly in Australia, where Aboriginal land priorities are a political issue, Walsh's work is explosive.

Three Epochs of Rock Art

According to Walsh, there are at least three epochs of rock art in the Kimberleys. The oldest is what he calls the "Pecked Cupule Period," in which small pits are pecked out of rock, sometimes as many as 4,000 at one site, sometimes in patterns. These are found in very hard sandstone surfaces, which, Walsh says, "would have required a considerable amount of time and energy" to peck out. The cupules are typically found in caves that were used for human habitation.

The next form of Kimberley art, Walsh terms the Irregular Infill Animal Period, named for its characteristic rock paintings of animals, which have clear outlines, but are irregularly filled in. Some of these figures are of animals or fish known from the fossil record but now extinct, thus providing contemporary scientists with an accurate record of what the actual animal or fish looked like.

The rock paintings shown here are from what Walsh calls the Erudite Epoch. These paintings are typically found in rock shelters, which are not used as living places, and often do not have even room to lie down or sit. This art is very well preserved, Walsh says, because of a combination of rock type ("white to pale brown coarse-grained thick-bedded quartz sandstone") and the paleoclimate. "Sometime after the paintings were applied to the rock, the rock developed a thin skin, probably composed of silica and oxalates, which bonded the pigments right into the rock. "

The most interesting paintings, Walsh says, therefore may be *discovered* underwater, off the Kimberley coast, where the maritime culture that produced the Bradshaw paintings probably would have lived. Because of the rock's bonding process, Walsh thinks that the art would survive well under water.

One of the ways the paintings can be placed in time is by using state-of-the-art dating techniques for wasps' nests found on top of a Bradshaw figure. The nests are built by the wasps with grains of sand. Using optical stimulated thermoluminescence, scientists can take a single grain of sand from one of these nests, and precisely date when it was last exposed to light; that is, when the wasp brought it into the rock shelter and out of the sunlight. this is one of the methods by which Walsh has dated the paintings to at least 20,000 YBP.

Tassels and Sashes

Walsh has divided the Erudite Epoch into phases, the first phase being the Tassel Bradshaws, and the largest group what he calls the Sash Bradshaws, which followed the Tassels. The names refer to the particular headdresses depicted—tassels or three-cornered sashes. There are subgroups of these, which Walsh has named "Elegant Action" Bradshaws and "Dynamic" Bradshaws, referring to the motions that are beautifully depicted. In some cases, figures of the later period are painted over earlier Bradshaws, indicating which came first.

An even later period is termed "Clothes-Peg," named for the characteristic clothes-peg look of the legs. In this period, there are the first portrayals of aggression, with barbed spears loaded to throw, and boomerangs ready to launch. Walsh has hypothesized that this coincided with the onset of arid cli-

Courtesy of *The New Citizen*

A typical Bradshaw painting.

Courtesy of *The New Citizen*

Rock painting of an ancient watercraft.

climate and a struggle over dwindling resources.

Walsh has argued that the clothes-peg figures are not a degeneration from the more elegant paintings, however, but that they represent "transfer of information." In other words, he believes that the clothes-peg figures are deliberately stylized in order to convey concepts, and that the Bradshaw paintings evolved over time in the same way that hieroglyphics developed in ancient Egypt. As Walsh says, "Rock art is a record of the cognitive development of mankind."

To further his argument that the Bradshaws had a high level of culture, Walsh notes that the Bradshaw figures are almost all male, and that none of them, male or female, has any of the crude sexual imagery present in other such art. For Walsh, this means that the rock art is not simply an accurate portrayal of what the artist sees, but a way of conveying a concept.

Other indications of the level of Bradshaw culture, Walsh notes, are the technical and artistic ability evidenced by the paintings and the high quality of the paint materials and the brushes used—all of which would have required a society in which there was leisure time, beyond that possible in the "hunting and gathering" mode, which conventional anthropology holds is the early human condition.

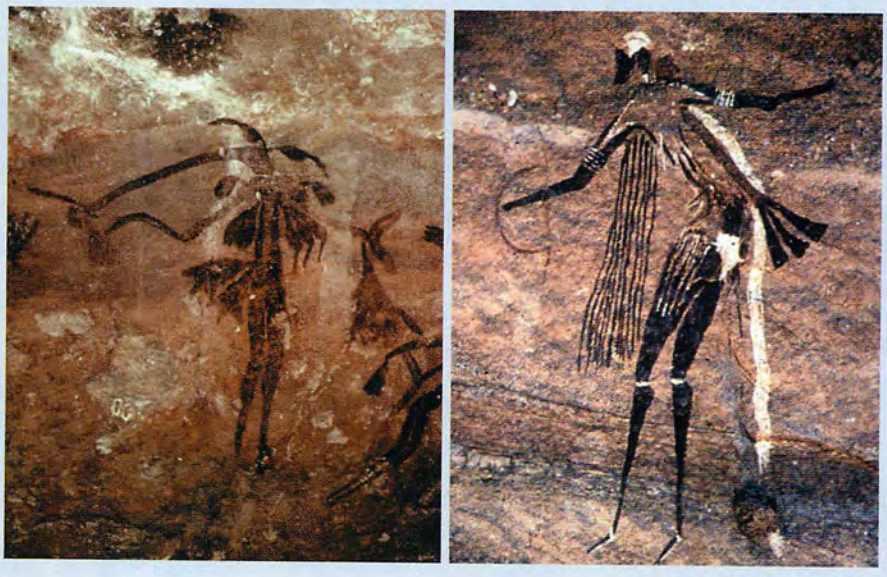
Where Did They Come from?

There is much evidence that the Bradshaw culture existed for a very long time in Australia, but there is none indicating that it was born there. In other words, it suddenly appears, fully formed, which for Walsh means that it had to have come via boats from overseas. Although there are similarities with both Egyptian and African art, Walsh thinks that the Bradshaw culture probably originated in Asia, and then sent out colonists to both Africa and Australia. There are Bradshaw rock paintings of several kinds of ancient watercraft, including ocean-going types of vessels, with a characteristically high bow and stern, and with what is either a keel or a rudder.

Like other archaeological work that contravenes conventional academic assumptions, exploration of the Bradshaw rock art, the world's largest repository of ancient rock art, is not funded—aside from Walsh's personal efforts—and is even disparaged. To quote *The New Citizen:* "The Kimberley region is famous for its mineral wealth, and firms such as BHP Minerals, the Queen's own Rio Tinto, Shell Development and others have extensive mining operations there. However, as significant as those riches are, they pale in comparison to the riches of the human spirit, whose history lies painted on the rock walls of the region, waiting to be fully discovered. . . ."

Notes

1. "The Bradshaw Rock Art of the Kimberleys: The Cognitive History of Man," by Allen Douglas and Kevin Heslop, *The New Citizen*, Aug.-Oct. 1999, pp. 9-23. Copies of this full-color issue, plus a 110-minute video lecture by Grahame Walsh on his rock art discoveries, are available at U.S. $30 postpaid, from *The New Citizen*, Citizens Media Group, P.O. Box 376 Coburg, Victoria, Australia 3058.
2. Walsh's published work includes *Bradshaws: Ancient Rock Paintings of North-West Australia*, (Geneva, Switzerland: Edition Limitee, 1994), and *Australia's Greatest Rock Art* (Bathurst, New South Wales: E.J. Brill-Robert Brown & Associates, 1988). The web page of Walsh's Takarakka Art Centre (http://www.takarakka.com/tinfo.html) gives an account of what is involved in reaching the remote Kimberley areas.



Courtesy of *The New Citizen*

A Sash Bradshaw painting (above left) compared with a Tassili rock art figure from Chad, in Africa. Both have the same style of feet and calves, a three-point appendage hanging from their backs, decorative apparel hung from the armpit, and nicely shaped arms. The Bradshaw figure holds a boomerang, while the Tassili figure holds a bow.

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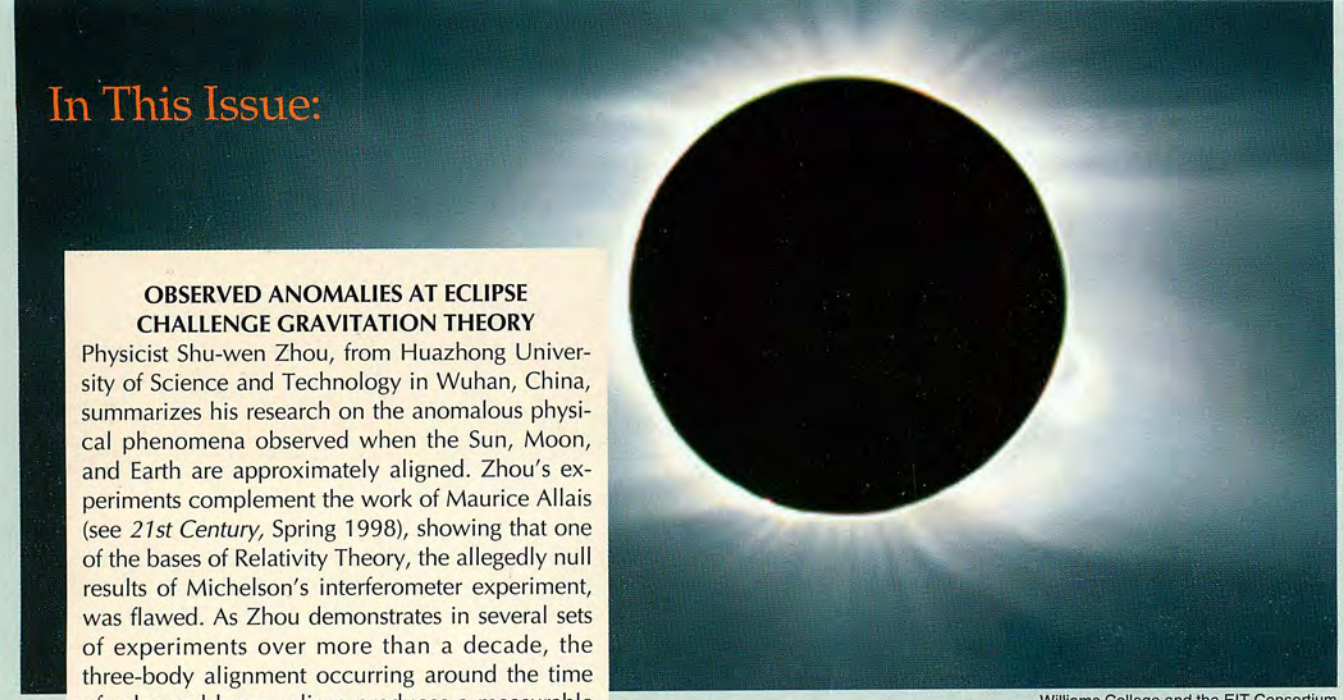
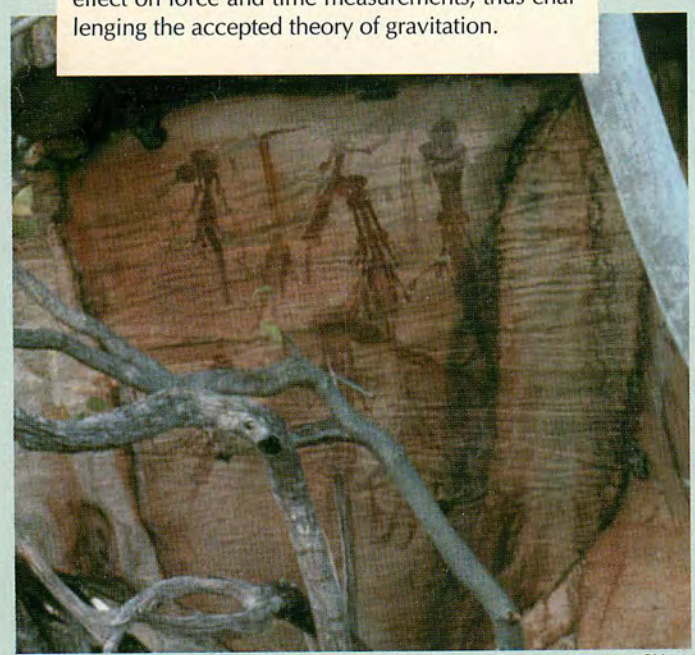


Williams College and the EIT Consortium

Total solar eclipse in February 1998, photographed from Aruba and composited from several exposures in order to show the wide dynamic range of intensity of the corona.

OBSERVED ANOMALIES AT ECLIPSE CHALLENGE GRAVITATION THEORY

Physicist Shu-wen Zhou, from Huazhong University of Science and Technology in Wuhan, China, summarizes his research on the anomalous physical phenomena observed when the Sun, Moon, and Earth are approximately aligned. Zhou's experiments complement the work of Maurice Allais (see *21st Century,* Spring 1998), showing that one of the bases of Relativity Theory, the allegedly null results of Michelson's interferometer experiment, was flawed. As Zhou demonstrates in several sets of experiments over more than a decade, the three-body alignment occurring around the time of solar and lunar eclipse produces a measurable effect on force and time measurements, thus challenging the accepted theory of gravitation.



Courtesy of the New Citizen

A typical Bradshaw rock art site.

THE BRADSHAW ROCK ART OF THE KIMBERLEYS

An advanced culture existed in the Kimberleys, the northwest mountains of Australia, at least 20,000 years ago, and possibly as long ago as 40,000 years. This issue's Ancient Discovery section reports on the work of Grahame Walsh, who for 21 years has researched and documented the rock art in the remote, inhospitable terrain of the Kimberleys. Walsh also shows, via rock painting evidence, that there were at least two distinct cultures that flourished there before the Bradshaw period.

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