The Farce of Physics

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by Bryan G. Wallace

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Introduction

A 1986 Harris poll found that about 70 percent of the responding adult Americans described themselves as interested in science and technology, and they said their understanding of the subject was very good or adequate. [153] The word *scientist* entered the English language in 1840, and few individuals earned a living doing research, with most of the investigations carried out by gentlemen of wealth and leisure. At that time, a handful of American scientist were taking steps to transform their status and image and separate themselves as professionals from those they considered amateurs. [154] The major tactic used to create this artificial separation has been the elaborate use of technical jargon and complex mathematics. This erection of higher and higher barriers to the comprehension of scientific affairs is a threat to an essential characteristic of science, its openness to outside examination and appraisal. [155]

Because of this, modern theoretical physics has become to a large degree, little more than an elaborate farce. I will attempt to explore and document this argument, and this book is meant for anyone who is interested in this subject. I have tried to reduce the technical jargon and mathematics to a minimum in order to reach the widest possible audience. If the reader finds parts that are hard to understand, just skip them, and perhaps come back to them later if you decide to explore that part in greater detail. You should realize that in general only about 90% of professional physicists are able to make sense of less than 10% of what other physicists say. [156]

For the past 50 years most of the scientific research has been funded by the federal government, and the number of Ph.D. scientists working in the U.S. has far outstripped the growth of the population as a whole. President Eisenhower stated that "in holding scientific research and discovery in respect, as we should, we must also be alert to the equal and opposite danger that public policy could itself become the captive of a scientific-technological elite." [150]

You the taxpayer fund this research, and you also enjoy the benefits that legitimate research can bring. That is why it is important to understand what you are getting for your money, and for you to inform your elected representatives when you think your precious tax dollars are being wasted. This book is a journey through my career as a physicist, giving the interesting details of the many events, arguments, and evidence encountered along the way. I suspect that the reader will discover that the truth can be stranger than fiction.

The term physics was derived from the Greek word "physis" for nature, and the roots of physics lies in the first period of Greek philosophy in the sixth century B.C., where science, philosophy and religion were not separated. The aim of physics is to discover the essential nature of all things, and it lies at the base of all of natural science.

The father of modern physics and astronomy, Galileo Galilei, was outspoken, forceful, sometimes tactless, and he enjoyed debate. He made many powerful enemies, and was eventually tried by the Inquisition and convicted of heresy. In Galileo's time it was heresy to claim there was evidence that the Earth went around the Sun, and in our time it is heresy to argue that there is evidence that the speed of light in space is not constant for all observers, no matter how fast they are moving, as predicted by Prof. Albert Einstein's sacred 1905 Special Relativity Theory. The heresies change, but as you will find from reading this book, human nature remains the same!

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Sacred Science

The title of this book was inspired by Dr. Fritjof Capra's book The Tao of Physics. Capra, a theoretical physicist states:

> The purpose or this book is to explore this relationship between the concepts of modern physics and the basic ideas in the philosophical and religious traditions of the Far East. We shall see how the two foundations of twentieth-century physics *quantum theory and relativity theory* both force us to see the world very much in the way a Hindu, Buddhist, or Taoist sees it, and how this similarity strengthens when we look at the recent attempts to combine these two theories in order to describe the phenomena of the submicroscopic world: the properties and interactions of the subatomic particles of which all matter is made. Here the parallels between modern physics and Eastern mysticism are most striking, and we shall often encounter statements where it is almost impossible to say whether they have been made by physicists or Eastern mystics. [1 p.4]

This presents an interesting question, what is the difference between modern physics and Eastern mysticism? There was a fascinating debate concerning creation-science published in the letters section of the journal Physics Today that directly relates to this question. The journal is sent free of charge to all members of the American Physical Society. The Society is the largest physics society in the world, and has world-wide membership. The letters section is popular, and is probably the most important communicative link between the world's physicists. The following quote is from a letter by Prof. Harry W. Ellis, a Professor of Physics at Eckerd College:

> On the other hand, the scientist (or anyone) who dismisses religion because the idea of an omnipotent God is logically inconsistent is guilty of intellectual hypocrisy. Does he or she think that science is free from inconsistencies? Perhaps he or she is not aware of the existence of Russell's paradox or Goedel's Theorem. Actually, aside from obvious methodological differences, science and theology have much in common. Each is an attempt to model reality, founded on unprovable articles of faith. If the existence of a benign supreme being is the fundamental assumption at the heart of religion, certainly the practice of science is founded on the unprovable hypothesis that the universe is rational that its behavior is subject to human understanding. Through science we construct highly useful models which permit us to understand the universe, in the sense of predicting its behavior. Let us not commit the elementary epistemological mistake of confusing the model with reality. Surely scientists, as well as religious leaders, should possess sufficient maturity to realize that whatever ultimate reality there may be is not directly accessible to mortal humans. [2]

Dr. Rodney B. Hall of the University of Iowa writes:

> Perhaps faith or the lack of it is simply a matter of indoctrination. You have been indoctrinated by the priests or the professors or both. [3]

Dr. John C. Bortz of the University of Rochester argues:

> Faith is not a valid cognitive procedure. When it is accepted as such, the process of rational argumentation degenerates into a contest of whims, and any idea, no matter how absurd or evil, may be successfully defended by claiming that those who advocate it feel, somehow, that it is right. In such a philosophical environment ideas are accepted not on the basis of how logical they are but rather on the basis of how much "feeling" their advocates seem to have. Unfortunately, the acceptance of ideas on this basis has been and continues to be the dominant epistemological trend in the world. [4]

Dr. Anthony L. Peratt of Los Alamos states:

> It is almost amusing to see the proponents of Big Bang cosmology, who have themselves been accused of fostering a religious intolerance toward those who question whether the foundations of the Big Bang hypothesis are scientifically justifiable, now getting a dose of their own medicine from biblical creationists. [5]

Dr. Carl A. Zapffe presents the view that:

> Science deserves every whack it gets from the so-called creationists, for a charge of puritanical posture belongs as much to one side as to the other. [6]

The governing body of the American Physical Society has released the following official statement on the matter:

> The Council of The American Physical Society opposes proposals to require "equal time" for presentation in public school science classes of the biblical story of creation and the scientific theory of evolution. The issues raised by such proposals, while mainly focused on evolution, have important implications for the entire spectrum of scientific inquiry, including geology, physics, and astronomy. In contrast to "Creationism," the systematic application of scientific principles has led to a current picture of life, of the nature of our planet, and of the universe which, while incomplete, is constantly being tested and refined by observation and analysis. This ability to construct critical experiments, whose results can require rejection of a theory, is fundamental to the scientific method. While our society must constantly guard against oversimplified or dogmatic descriptions of science in the education process, we must also resist attempts to interfere with the presentation of properly developed scientific principles in establishing guidelines for classroom instruction or in the development of scientific textbooks. We therefore strongly oppose any requirement for parallel treatment of scientific and non-scientific discussions in science classes. Scientific

> inquiry and religious beliefs are two distinct elements of the human experience. Attempts to present them in the same context can only lead to misunderstandings of both. [7]

I expect that the average scientist would agree with the following argument presented by Dr. Michael A. Seeds:

> ...A pseudoscience is something that pretends to be a science but does not obey the rules of good conduct common to all sciences. Thus such subjects are false sciences.
>
> True science is a method of studying nature. It is a set of rules that prevents scientists from lying to each other or to themselves. Hypotheses must be open to testing and must be revised in the face of contradictory evidence. All evidence must be considered and all alternative hypotheses must be explored. The rules of good science are nothing more than the rules of good thinking that is, the rules of intellectual honesty. [8 p.A5]

This brings up an interesting question; Do scientists actually practice what they preach? The evidence clearly shows that the average scientist tends not to use the rules of good science. In fact, it appears that Protestant ministers are inclined to have more intellectual honesty than Ph.D. scientists. To document this fact, I will quote from an article titled "Researchers Found Reluctant to Test Theories" by Dr. David Dickson:

> Despite the emphasis placed by philosophers of science on the importance of "falsification" *the idea that one of a scientist's main concerns should be to try to find evidence that disproves rather than supports a particular hypothesis* experiments reported at the AAAS annual meeting suggest that research workers are in practice reluctant to put their pet theories to such a test.
>
> In a paper on self-deception in science, Michael J. Mahoney of the University of California at Santa Barbara described the results of a field trial in which a group of 30 Ph.D. scientists were given 10 minutes to find the rule used to construct a sequence of three numbers, 2,4,6, by making up new sequences, inquiring whether they obeyed the same rule, and then announcing (or "publishing") what they concluded the rule to be when they felt sufficiently confident.
>
> The results obtained by the scientists were compared to those achieved by a control group of 15 Protestant ministers. Analysis showed that the ministers conducted two to three times more experiments for every hypothesis that they put forward, were more than three times slower in "publishing" their first hypothesis, and were only about half as likely as the scientists to return to a hypothesis that had already been disconfirmed. [9]

There is an interesting article by Dr. T. Theocharis and Dr. M. Psimopoulos of the Department of Physics of the Imperial College of Science and Technology in London titled "Where science has gone wrong," that explores the arguments put forth by prominent scientists and philosophers with regard to the nature of modern science. [10] The following is several quotes from that article:

> On 17 and 22 February 1986 BBC television broadcast, in the highly regarded *Horizon* series, a film entitled "Science ... Fiction?", and in the issue of 20 February 1986 *The Listener* published an article entitled "The Fallacy of Scientific Objectivity". As is evident from their titles, these were attacks against objectivity, truth and science...
>
> This state of affairs is bad enough. But things are even worse: perversely, many individual scientists and philosophers seem bent on questioning and rejecting the true theses, and supporting the antitheses. For example, most of the participants in the "Science ... Fiction?" film were academic scientists...
>
> Popper also thought that observations are theory-laden. He phrased it thus: "Sense-data, untheoretical items of observation, simply do not exit... [11]
>
> But if observations are theory-laden, this means that observations are simply theories, and then how can one theory falsify (never mind verify) another theory?... [12]
>
> So back to square one: if verifiability and falsifiability are not the criteria, then what makes a proposition scientific? It is hard to discern the answer to this question in Lakatos's writings. But if any answer is discerned at all, it is one that contradicts flagrantly the motto of the Royal Society: "I am not bound to swear as any master dictates". [13] This answer is more obvious in Thomas Kuhn's[14] writings: a proposition is scientific if it is sanctioned by the scientific establishment. (Example: if the scientific establishment decrees that "fairies exist", then this would be scientific indeed.)
>
> According to Kuhn, science is not the steady, cumulative acquisition of knowledge that was portrayed in old-fashioned textbooks. Rather, it is an endless succession of long peaceful periods which are violently interrupted by brief intellectual revolutions. During the peaceful period, which Kuhn calls "normal science", scientists are guided by a set of theories, standards and methods, which Kuhn collectively designates as a "paradigm". (Others call it a "world-view".) During a revolution, the old paradigm is violently overthrown and replaced by a new one...
>
> Kuhn's view, that a proposition is scientific if it is sanctioned by the scientific establishment, gives rise to the problematic question: what exactly makes an establishment "scientific"? This particular Gordian knot was cut by Paul Feyerabend: *any* proposition is scientific "There is only one principle that can be defended under all circumstances and in all stages of human development. It is the principle: Anything goes"... [15]
>
> In 1979 Science published a four-page complimentary feature[16] about Feyerabend, the Salvador Dali of academic philosophy, and currently the worst enemy of science. In this article Feyerabend was quoted as stating that "normal science is a fairy tale" and

> that "equal time should be given to competing avenues of knowledge such as astrology, acupuncture, and witchcraft." Oddly, religion was omitted. For according to Feyerabend (and the "Science ... Fiction?" film too), religion *and everything else* is an equally valid avenue of knowledge. In fact on one occasion Feyerabend characteristically put science on a par with "religion, prostitution and so on."[15]

The above mentioned Prof. Thomas S. Kuhn, was a man who wrote a controversial book on science. In an interview of Kuhn by John Horgan on page 40 of the May 1991 issue of the prestigious US journal SCIENTIFIC AMERICAN, we find the following:

> ... "The book" *The Structure of Scientific Revolutions*, commonly called the most influential treatise ever written on how science does (or does not) proceed. Since its publication in 1962, it has sold nearly a million copies in 16 languages, and it is still fundamental reading in courses on the history and philosophy of science.
>
> The book is notable for having spawned that trendy term "paradigm." It also fomented the now trite idea that personalities and politics play a large role in science. Perhaps the book's most profound argument is less obvious: scientists can never fully understand the "real world" or even *to a crucial degree* one another...
>
> Denying the view of science as a continual building process, Kuhn asserts that a revolution is a destructive as well as a creative event. The proposer of a new paradigm stands on the shoulders of giants and then bashes them over the head. He or she is often young or new to the field, that is, not fully indoctrinated...

Dr. Spencer Weart directs the Center for History of Physics at the American Institute of Physics in New York. In his interesting article THE PHYSICIST AS MAD SCIENTIST published in Physics Today, he writes:

> The public image of the scientist partly evolved out of ideas about wizards. Here was an impressive figure, known to all from early childhood, reaching back through ancient sorcery legends to prehistoric shamans. [17 p.28]

Prof. Albert Einstein states the following on the general lack of scientific integrity in the temple of science:

> In the temple of science are many mansions, and various indeed are they that dwell therein and the motives that have led them thither. Many take to science out of a joyful sense of superior intellectual power; science is their own special sport to which they look for vivid experience and the satisfaction of ambition; many others are to be found in the temple who have offered the products of their brains on this altar for purely utilitarian purposes. Were an angel of the Lord to come and drive all the people belonging to these two categories out of the temple, the assemblage would be seriously depleted, but there would still be some men, of both present and past times, left inside. [39 p.224]

In Ronald W. Clark's definitive biography of Einstein, we find what Einstein means when he makes the above statement pertaining to the Lord, or some of his other famous statements such as "God is subtle, but he is not malicious" or "God does not play dice with the world.":

> However Einstein's God was not the God of most other men. When he wrote of religion, as he often did in middle and later life, he tended to adopt the belief of Alice's Red Queen that "words mean what you want them to mean," and to clothe with different names what to more ordinary mortals and to most Jews looked like a variant of simple agnosticism. Replying in 1929 to a cabled inquiry from Rabbi Goldstein of New York, he said that he believed "in Spinoza's God who reveals himself in the harmony of all that exists, not in a God who concerns himself with the fate and actions of men." And it is claimed that years later, asked by Ben-Gurion whether he believed in God, "even he, with his great formula about energy and mass, agreed that there must be something behind the energy." No doubt. But much of Einstein's writing gives the impression of belief in a God even more intangible and impersonal than a celestial machine minder, running the universe with undisputable authority and expert touch. Instead, Einstein's God appears as the physical world itself, with its infinitely marvelous structure operating at atomic level with the beauty of a craftsman's wristwatch, and at stellar level with the majesty of a massive cyclotron. This was belief enough. It grew early and rooted deep. Only later was it dignified by the title of cosmic religion, a phrase which gave plausible respectability to the views of a man who did not believe in a life after death and who felt that if virtue paid off in the earthly one, then this was the result of cause and effect rather than celestial reward. Einstein's God thus stood for an orderly system obeying rules which could be discovered by those who had the courage, the imagination, and the persistence to go on searching for them. And it was to this task which he began to turn his mind soon after the age of twelve. For the rest of his life everything else was to seem almost trivial by comparison. [38 p.38]

In an expansion of Einstein's views with regard to a scientific cosmic religion, Clark states:

> Maybe. To some extent the differences between Einstein and more conventional believers were semantic, a point brought out in his "Religion and Science" which, on Sunday, November 9, occupied the entire first page of the *New York Times Magazine*. "Everything that men do or think," it began, "concerns the satisfaction of the needs they feel or the escape from pain." Einstein then went on to outline three states of religious development, starting with the religion of fear that moved primitive people, and which in due course became the moral religion whose driving force was social feelings. This in turn could become the "cosmic religious sense ... which recognizes neither dogmas nor God made in man's image." And he then put the key to his ideas in two sentences. "I assert that the cosmic religious experience is the strongest and noblest driving force behind scientific research." And, as a corollary, "the only deeply religious people of our largely materialistic age are the earnest men of research."[38 p.516]

With reference to the general view of most scientists with regard to science and religion, there is a very interesting FOCAL POINT article in the journal Sky & Telescope by Dr. Paul Davies, a professor of mathematical physics at the University of Adelaide Australia. [139] The title of the article is What Hath COBE Wrought?, and the following statements are from the article:

> THE BLAZE of publicity that accompanied the recent discovery of ripples in the heat radiation from the Big Bang focused attention once again on the subject of God and creation. Commentators disagree on the theological significance of what NASA's Cosmic Background Explorer, or COBE, found. Some referred to the ripples as the "fingerprint of God," while others lashed out at what they saw as the scientists' attempt to demystify God's last refuge.
>
> When the Big Bang theory became popular in the 1950s, many people used it to support the belief that the universe was created by God at some specific moment in the past. And some still regard the Big Bang as "the creation" a divine act to be left beyond the scope of science... Cosmologist regard the Big Bang as marking the origin of space and time, as well as of matter and energy... This more sophisticated, but abstract, idea of God adapts well to the scientific picture of a universe subject to timeless eternal laws... If time itself began with the Big Bang, then the question "What caused the Big Bang?" is rendered meaningless... New and exciting theories of quantum cosmology seek to explain the origin of the universe within the framework of scientific law. Their central feature is Heisenberg's uncertainty principle, which permits genuine spontaneity in nature. As a result, the tight linkage between cause and effect so characteristic of classical physics is loosened. Quantum events do not need well-defined prior causes; they can be regarded as spontaneous fluctuations. It is then possible to imagine the universe coming into being from nothing entirely spontaneously, without violating any laws.

Sir Isaac Newton, in his reasoning in support of the particle (corpuscular) model of light in space, as opposed to the wave in ether model, presented the argument:

> Against filling the Heavens with fluid mediums, unless they be exceeding rare, a great Objection arises from the regular and very lasting motions of the Planets and Comets. For thence it is manifest, that the Heavens are void of all sensible resistance, and by consequence of all sensible matter. [140]

In 1846 Michael Faraday wrote in his diary:

> All I can say is, that I do not perceive in any part of space, whether (to use the common phrase) vacant or filled with matter, anything but forces and the lines in which they exerted. [141]

This was the beginning of the dominant modern physics theories, where it is the geometric and physical conditions of space itself that is fundamental. Prof. Eyvind H. Wichmann, in the Berkeley Physics Course, Volume 4, quantum physics, presents the following argument:

> 35 Today the *mechanical* ether has been banished from the world of physics, and the

> word "ether" itself, because of its "bad" connotations, no longer occurs in textbooks on physics. We talk ostentatiously about the "vacuum" instead, thereby indicating our lack of interest in the *medium* in which waves propagate. We no longer ask what it is that "really oscillates" when we study electromagnetic waves or de Broglie waves. All we wish to do is to formulate *wave equations* for these waves, through which we can predict experimentally observable phenomena... [122]

There is a popular argument that the world's oldest profession is sexual prostitution. I think that it is far more likely that the oldest profession is scientific prostitution, and that it is still alive and well, and thriving in the 20th century. I suspect that long before sex had any commercial value, the prehistoric shamans used their primitive knowledge to acquire status, wealth, and political power, in much the same way as the dominant scientific and religious politicians of our time do. So in a sense, I tend to agree with Weart's argument that the earliest scientists were the prehistoric shamans, and the argument of Feyerabend that puts science on a par with religion and prostitution. I also tend to agree with the argument of Ellis that states that both science and theology have much in common, and both attempt to model reality on arguments based on unprovable articles of faith. Using the logic that if it looks like a duck, quacks like a duck, and waddles like a duck, it must be a duck: I support the argument that since there is no significant difference between science and religion, science should be considered a religion! I would also agree with Ellis' argument of the obvious methodological differences between science and the other religions. The other dominant religions are static because their arguments are based on rigid doctrines set forth by their founders, such as Buddha, Jesus, and Muhammad, who have died long ago. Science on the other hand, is a dynamic religion that was developed by many men over a long period of time, and it has a flexible doctrine, the scientific method, that demands that the arguments change to conform to the evolving observational and experimental evidence.

The word science was derived from the Latin word scientia, which means knowledge, so we see that the word, in essence, is just another word for knowledge. An associate of mine, Prof. Richard Rhodes II, a Professor of Physics at Eckerd College, once told me that students in his graduate school used to joke that Ph.D. stood for Piled higher and Deeper. If one considers the vast array of abstract theoretical garbage that dominates modern physics and astronomy, this appears to be an accurate description of the degree. Considering the results from Mahoney's field trial that showed Protestant ministers were two to three times more likely to use scientific methodology than Ph.D. scientists, it seems reasonable to consider that they have two to three times more right to be called scientists then the so-called Ph.D. scientists. I would agree with Popper's argument that observations are theory-laden, and there is no way to prove an argument beyond a reasonable shadow of a doubt, but at the very least, the scientist should do more than pay lip service to the scientific method. The true scientist must have faith and believe in the scientific method of testing theories, and not in the theories themselves. I agree with Seeds argument that "A pseudoscience is something that pretends to be a science but does not obey the rules of good conduct common to all sciences." Because many of the dominant theories of our time do not follow the rules of science, they should more properly be labeled pseudoscience. The people who tend to believe more in theories than in the scientific method of testing theories, and who ignore the evidence against the theories they believe in, should be considered pseudoscientists and not true scientists. To the extent that the professed beliefs are based on the desire for status, wealth, or political reasons, these people are scientific prostitutes.

I agree with Newton's argument that if light was a wave in the ether, the ether would have to be nonsensible matter. Calling the ether space or vacuum does not solve the problem. Its existence is based on blind faith and not experimental evidence. As I will show in the following Chapters, there is an overwhelming body of evidence that light is a particle, as Newton predicted. The fact that most modern physicists have refused to objectively consider this evidence, has made a farce of physics. This empty space of modern physics is a supernatural solid[123] that can have infinite temperature and density. [105] A spot of this material that is smaller than an atom is supposed to have created the entire universe. [8 p.325] This physical material has become the God of most modern physicists!

Pathological Physics

There is a very interesting article published in the October 1989 issue of Physics Today. [86] The article is titled "PATHOLOGICAL SCIENCE" and the abstract reads:

> Certain symptoms seen in studies of 'N rays' and other elusive phenomena characterize 'the science of things that aren't so.'

The introduction to the article starts:

> *Irving Langmuir spent many productive years pursuing Nobel- caliber research (see the photo on the opposite page). Over the years, he also explored the subject of what he called "pathological science." Although he never published his investigations in this area, on 18 December 1953 at General Electric's Knolls Atomic Power Laboratory, he gave a colloquium on the subject that will long be remembered by those in his audience. This talk was a colorful account of a particular kind of pitfall into which scientists may stumble.*

Langmuir begins his presentation with:

> The thing started in this way. On 23 April 1929, Professor Bergen Davis from Columbia University came up and gave a colloquium in this Laboratory, in the old building, and it was very interesting...

Langmuir then gives the details of the Davis and Barnes controversial experiment that produced a beam of alpha rays from polonium in a vacuum tube with a hot cathode electron emitter and a microscope for counting alpha induced scintillations on a zinc sulfide screen. Then Langmuir described the results of a visit he and a colleague, C. W. Hewlett, made to Davis's laboratory at Columbia University. With regard to the experiment Langmuir states:

> And then I played a dirty trick. I wrote out on a card of paper ten different sequences of V and 0. I meant to put on a certain voltage and then take it off again. Later I realized that [trick wouldn't quite work] because when Hull took off the voltage, he sat back in his chair there was nothing to regulate at zero so he didn't. Well, of course, Barnes saw him whenever he sat back in his chair. Although the light wasn't very bright, he could see whether [Hull] was sitting back in his chair or not, so he knew the voltage wasn't on, and the result was that he got a corresponding result. So later I whispered, "Don't let him know that you're not reading," and I asked him to change the voltage from 325 down to 320 so he'd have something to regulate. I said, "Regulate it just as carefully as if you were sitting on a peak." So he played the part from that time on, and from that

> time on Barnes's readings had nothing whatever to do with the voltages that were applied. Whether the voltage was at one value or another didn't make the slightest difference. After that he took 12 readings, of which about half were right and the other half were wrong, which was about what you would expect out of two sets of values.
>
> I said: "You're through. You're not measuring anything at all. You never *have* measured anything at all."
>
> "Well," he said, "the tube was gassy. The temperature has changed and therefore the nickel plates must have deformed themselves so that the electrodes are no longer lined up properly."
>
> "Well," I said, "isn't this the tube in which Davis said he got the same results when the filament was turned off completely?"
>
> "Oh, yes," he said, "but we always made blanks to check ourselves, with and without the voltage on."
>
> He immediately *without giving any thought to it* he immediately had an excuse. He had a reason for not paying any attention to any wrong results. It just was built into him. He just had worked that way all along and always would. There is no question but [that] he is honest: He *believed* these things, absolutely...

At the end of that section, Langmuir states:

> To me, [its] extremely interesting that men, perfectly honest, enthusiastic over their work, can so completely fool themselves. Now what was it about that work that made it so easy for them to do that? Well, I began thinking of other things. I had seen R. W. Wood and told him about this phenomenon because he's a good experimenter and doesn't make such mistakes himself very often if at all. [*Wood was a physicist from Johns Hopkins University*.] And he told me about the N rays that he had an experience with back in 1904. So I looked up the data on N rays. [87]

Then Langmuir gave a detailed account of N rays, and how they were discovered in 1903 by a respected French physicist, René- Prosper Blondlot, at the University of Nancy. The N-rays were supposed to be generated by a hot wire inside an iron tube that has an 1/8 inch aluminum window in it, and the rays are detected by a calcium sulfide screen which gave out a very faint glow in a dark room. One of the experiments involved a large prism of aluminum with a 60 degree angle. Wood visited Blondlot's lab and Langmuir recounts the following trick Wood played on Blondlot:

> Well, Wood asked him to repeat some of these measurements, which he was only too glad to do. But in the meantime, the room, being very dark, R. W. Wood put the prism in his pocket and the results checked perfectly with what [Blondlot] had before. Well, Wood rather cruelly published that. [88] And that was the end of Blondlot.

Langmuir next deals with the 1923 mitrogenetic ray experiments of Prof. Alexander Gurwitsch at the First State University of Moscow. [89] After the mitrogenetic ray section, Langmuir presents the following section, which is the heart of his article:

> Symptoms of sick science
>
> The Davis-Barnes experiment and the N rays and the mitogenetic rays all have things in common. These are cases where there is no dishonesty involved but where people are tricked into false results by a lack of understanding about what human beings can do to themselves in the way of being led astray by subjective effects, wishful thinking or threshold interactions. These are examples of pathological science. These are things that attracted a great deal of attention. Usually hundreds of papers have been published on them. Sometimes they have lasted for 15 or 20 years and then gradually have died away. Now here are the characteristic rules [*see the box above*]:
>
> > The maximum effect that is observed is produced by a causative agent of barely detectable intensity. For example, you might think that if one onion root would affect another due to ultraviolet light then by putting on an ultraviolet source of light you could get it to work better. Oh no! *Oh no!* It had to be just the amount of intensity that's given off by an onion root. Ten onion roots wouldn't do any better than one and it didn't make any difference about the distance of the source. It didn't follow any inverse square law or anything as simple as that. And so on. In other words, the effect is independent of the intensity of the cause. That was true in the mitogenetic rays and it was true in the N rays. Ten bricks didn't have any more effect than one. It had to be of low intensity. We know why it had to be of low intensity: so that you could fool yourself so easily. Otherwise, it wouldn't work. Davis-Barnes worked just as well when the filament was turned off. They counted scintillations.
>
> > Another characteristic thing about them all is that these observations are near the threshold of visibility of the eyes. Any other sense, I suppose, would work as well. Or many measurements are necessary many measurements because of the very low statistical significance of the results. With the mitogenetic rays particularly, [people] started out by seeing something that was bent. Later on, they would take a hundred onion roots and expose them to something, and they would get the average position of all of them to see whether the average had been affected a little bit... Statistical measurements of a very small...were thought to be significant if you took large numbers. Now the trouble with that is this. [Most people have a habit, when taking] measurements of low significance, [of finding] a means of rejecting data. They are right at the threshold value and there are many reasons why [they] can discard data. Davis and Barnes were doing that right along. If things were doubtful at all, why, they would discard them or not discard them depending on whether or not they fit the theory. They didn't know that, but that's the way it worked out.
>
> > There are claims of great accuracy. Barnes was going to get the Rydberg constant more accurately than the spectroscopists could. Great sensitivity or great specificity

we'll come across that particularly in the Allison effect.

> Fantastic theories contrary to experience. In the Bohr theory, the whole idea of an electron being captured by an alpha particle when the alpha particles aren't there, just because the waves are there, [isn't] a very sensible theory.

> Criticisms are met by ad hoc excuses thought up on the spur of the moment. They always had an answer always.

> The ratio of the supporters to the critics rises up somewhere near 50% and then falls gradually to oblivion. The critics couldn't reproduce the effects. Only the supporters could do that. In the end, nothing was salvaged. Why should there be? There isn't anything there. There never was. That's characteristic of the effect.

In an evaluation of modern physics based on Langmuir's arguments, we find that many of the dominant theories should be classed as pathological science. For example, starting with his first characteristic rule "The maximum effect that is observed is produced by a causative agent of barely detectable intensity."; we find that Einstein's special relativity theory which is generally acknowledged as the foundation of the rest of the dominant theories of 20th century physics, is based on the fact that the Michelson-Morley experiment could not detect the motion of the earth through the ether! As I have shown in Chapter 3 "Mathematical Magic", Einstein believed that the ether sea exists but that it is invisible and can't be detected by experiments.

As a second example of the spectrum of modern theories that should be classed as pathological, we have the particle physicists that argue that invisible quarks exist inside of the detectable protons and neutrons. [64] Actually, their arguments have expanded over the years to include a whole zoo of invisible particles that come in different colors and flavors, the zoo contains, quarks, gluons, gravitrons, Higgs bosons, etc. All of these particles are detectable only by using very elaborate "Mathematical Magic" to analysis the particles that are detected. On this question, Werner Heisenberg, one of the most prominent physicists of this century, makes the following remarks in his article [90] titled "The nature of elementary particles":

...Before this time it was assumed that there were two fundamental kinds of particles, electrons and protons, which, unlike most other particles, were immutable. Therefore their number was fixed and they were referred to as "elementary" particles. Matter was seen as being ultimately constructed of electrons and protons. The experiments of Anderson and Blackett provided definite proof that this hypothesis was wrong. Electrons can be created and annihilated; their number is not constant; they are not "elementary" in the original meaning of the word... A proton could be obtained from a neutron and a pion, or a hyperon and a kaon, or from two nucleons and one antinucleon, and so on. Could we therefore simply say a proton consists of continuous matter?... This development convincingly suggests the following analogy: Let us compare the so-called "elementary" particles with the stationary states of an atom or a molecule. We may think of these as various states of one single molecule or as the many different molecules of chemistry. One may therefore speak simply of the

> "spectrum of matter."...
>
> My intention, however, is not to deal with philosophy but with physics. Therefore I will now discuss that development of theoretical particle physics that, I believe, begins with the wrong questions. First of all there is the thesis that the observed particles such as the proton, the pion, the hyperon consist of smaller particles: quarks, partons, gluons, charmed particles or whatever else, none of which have been observed. Apparently here the question was asked: "What does a proton consist of?" But the questioners appear to have forgotten the phrase "consist of" has a tolerably clear meaning only if the particle can be divided into pieces with a small amount of energy, much smaller than the rest mass of the particle itself. ...In the same way I am afraid that the quark hypothesis is not really taken seriously today by its proponents. Questions dealing with the statistics of quarks, the forces that keep them together, the reason why the quarks are never seen as free particles, the creation of pairs of quarks inside an elementary particle, are all left more or less undefined. If the quark hypothesis is really to be taken seriously it is necessary to formulate precise mathematical assumptions for the quarks and for the forces that keep them together and to show, at least qualitatively, that all these assumptions reproduce the known features of particle physics...
>
> Therefore this article can be concluded with a more optimistic view of those developments in particle physics that promise success. New experimental results are always valuable, even if they only enlarge the data table; but they are especially interesting if they answer critical questions of the theory. In the theory one should try to make precise assumptions concerning the dynamics of matter, without any philosophical prejudices. The dynamics must be taken seriously, and we should not be content with vaguely defined hypotheses that leave essential points open. Everything outside of the dynamics is just a verbal description of the table of data, and even then the data table probably yields more information than the verbal description can. The particle spectrum can be understood only if the underlying dynamics of matter is known; dynamics is the central problem.

In 1977, in collaboration with Prof. Wilbur Block and Prof. Richard Rhodes II, I submitted a research proposal through Eckerd College to the National Science Foundation. The proposal was for $159,512, of which $99,655 was to go for a high-performance Harris computer. We intended to use computer methods to attack the difficult mathematics of the underlying dynamics of matter as outlined in Heisenberg's article. The February 1978 rejection letter from Dr. Barry R. Holstein, Program Officer for Theoretical Physics, stated the proposal was declined because their reviewers had an overwhelming feeling that there is no reason to abandon the conventional and remarkably successful theories of electron and quark interactions in favor of our model. The letter supplied the motivation for my campaign to discredit the quark theorists. The campaign involved for the most part, attacking prominent quark theorists at the American Physical Society meetings, and to add insult to injury, I published the following letter [91] in Physics Today:

> Heisenberg and QCD

> I would like to comment on Gerald E. Brown's and Mannque Rho's recent paper "The structure of the nucleon" (February, page 24). At the APS 1982 Spring Meeting in Washington, D.C., Brown gave an invited paper entitled "Structure of the Nucleons." [92] After he delivered his paper, I challenged Brown to defend his QCD arguments. I stated that Werner Heisenberg had argued [90] that he was afraid that the quark hypothesis was not really taken seriously by its proponents. He pointed out that they do not deal with the mass dynamics of the transformation of mass from energy to the particle spectrum, and that it was irrational to speculate on the division of quarks into subparticles because it would take many times the rest energy of the particles to produce them. I asked him how he would challenge Heisenberg's arguments. He stated that he could not, and that it would be best to ask this of others since he was a nuclear physicist.
>
> In answer to Brown's comment, I have asked other QCD theorists and their supporters how they would challenge Heisenberg's arguments. One prominent particle theorist who presented an invited paper at the same Spring Meeting shouted "*No Way!*" before I could even finish pronouncing Heisenberg's name. In general, this question has had the same sort of devastating effect on all the physicists I've asked it of. Considering Heisenberg's status, it's no wonder that few physicists are willing to challenge his arguments...

In the April 1982 issue of Physics Today, [93] there appeared an article titled "Instant fame and small fortune" which states:

> At the San Francisco APS meeting in January, Arthur Schawlow announced the results of a contest he initiated last year (PHYSICS TODAY,March 1981, page 75). In his retiring presidential address he said, "This year, I have sponsored a contest for APS members to propose the best way to publicize their own contributed papers. The contest has been judged by a distinguished panel of graduate students and secretaries, who will remain anonymous for their own safety.
>
> "First prize of ten dollars goes to...
>
> "Second prize of, five dollars, goes to...
>
> "Third prize, a copy of my latest paper, goes to...
>
> "Fourth prize, a copy of my two latest papers, goes to Bryan G. Wallace of Eckerd College, who pointed out that the abstracts are reproduced photographically, and so he had been able to use tricks like italics and extra heavy type to make his abstracts stand out...

Actually, the full text of my entry concerned more than dark italic type, and goes as follows:

> Dear Art:

With reference to your open letter that accompanied the 1982 renewal invoice, I would like to enter your "Instant Fame and (small) Fortune contest. We have had a major problem with QCD theorists acting as referees in trying to obtain funding and publication for our mass dynamics research. As an example, one of our NSF proposals was declined because "There was an overwhelming feeling that there is no reason to abandon the conventional and remarkably successful theories of electron and quark interactions in favor of your model which is beset with a number of fatal conceptual difficulties." In order to compensate for this problem we have adopted a policy of presenting current research results in the form of a contributed paper annually, with abstracts published in the Bulletin making an archival record. Since the APS Spring Meeting is traditionally held at or near Washington D.C. we felt we could get the most bang per buck from it.

I have devised a number of methods of publicizing the contributed papers. To begin with, I use my trusty old Sears typewriter that has large Italic type, use a new ribbon, and set it for maximum impact to type the published abstract. Enclosed you will find a copy of the abstracts published to date. They stand out like a sore thumb from the other abstracts, and are real eye grabbers. The next tactic is to attend the Spring Meeting symposiums where the QCD super stars are giving their invited papers. The idea is to present short, high impact commercials, our brand (Mass Dynamics) versus the other brand (QCD). Where a TV commercial might use a well known movie or television star to help sell their product, I use statements made by Werner Heisenberg, who of course is a physics super duper star. The statements come from Heisenberg's article "The nature of elementary particles" in the March 1976 issue of "Physics Today." Heisenberg had some nice things to say about mass dynamics and some very nasty things to say about QCD type theories. His statements have made effective stones for the sling of this modern day David. As two examples of what I consider to be the best shots fired to date:

> At the 1979 HA Special Session To Celebrate The Hundredth Anniversary Of Dr. Albert Einstein's Birth before a packed room of perhaps 1000 physicists, Steven Weinberg presented a talk entitled "Unification of the Forces of Nature." Peter Bergmann who was presiding the session gave me Weinberg's throat mike, we were all standing by the overhead projector. I stated that Heisenberg published a paper on the nature of elementary particles a few years ago in Physics Today, and that in the paper he made the contention that Quark theories are little more than a verbal description of the data table and that we will not understand the nature of the particle spectrum until we invent a theory of the dynamics of matter, then I asked him to comment on this. He was flustered and stated that of course this was a legitimate point of view and there are many problems with trying to develop the mass dynamics of quark theories, then he threw up his hands and in an emotional voice shouted that he just believed in them!

> At the 1981 JA Symposium "High-Energy Facilities of the Future," Leon Lederman gave a talk on "Future Facilities at Fermilab." I was the first to comment and said that Heisenberg has argued that QCD theories are nothing more than a verbal description of the data and that we would not understand the nature of the particle spectrum until we developed theories of the mass dynamics, and here he was basing his arguments for more funding on theories that were mere verbal descriptions of the data. Perhaps the large accelerators are the SSTs of modern physics and we should let the Europeans waste their money on them and we could spend ours on more important things like physicist's salaries and computers. He answered that of course he would not want to argue with Heisenberg, and that I had a good point and he would like to get with me later and talk about it. After the session, he came over and asked "Why me? Why me? Why didn't you pick on any of the others?" I said he was the first to use QCD to support his argument for more funding. He stated that he felt that if Heisenberg were still alive, he probably would support QCD, look at the Nobel prize, the large number of theorists that support it. I asked him if he had read Heisenberg's article, he said no, but now he was going to make a point to read it. At the end of our conversation, I gave him 2 cents and said it was my share of the money he needed and that I had nothing against accelerators, only quarks.

In an 1985 Physics Today article [94] titles "The SSC: A machine for the nineties," Dr. Sheldon L. Glashow and Dr. Leon M. Lederman present the following argument:

> True, the Standard Model does explain a very great deal. Nevertheless it is not yet a proper theory, principally because it does not satisfy the physicists naive faith in elegance and simplicity. It involves some 17 allegedly fundamental particles and the same number of arbitrary and tunable parameters, such as the fine-structure constants, the muon- electron mass ratio and the various mysterious mixing angles (Cabibbo, Weinberg, Kobayashi-Maskawa). Surely the Creator did not twiddle 17 dials on his black box before initiating the Big Bang, and its glorious sequela, mankind. Our present theory is incomplete, insufficient and inelegant, though it may be long remembered as a significant turning point. It remains for history to record whether, on the threshold of a major synthesis, we chose to turn our backs or to thrust onward. The choice is upon us with the still-hypothetical SSC.

In effect, Glashow and Lederman are arguing that after spending billions of dollars on particle accelerators, all we have to show for it is a bunch of worthless mathematics, or what Heisenberg calls using the language of mathematics to produce "a verbal description of the table of data." They want us to spend many more billions of dollars to build the SSC, a machine that is up to 112 miles in circumference and that can accelerate protons to 40 trillion electron volts of energy. They offer the slim hope that if we explore the short-lived trash at the high end of the particle spectrum at energies far beyond that of the stable particles of the everyday world, we might have some additional insight into a unified theory! The 1985 APS retirement address of the particle physicist Dr. Robert R. Wilson that I quoted in Chapter 4, and the above reply to my NSF proposal tends to indicate that the average particle physicist is opposed to a unified theory along the lines presented by Einstein and Heisenberg, and that funding of the SSC could very likely hamper the development of a realistic unified theory that would bring enormous benefits for mankind. At the 1985 APS Spring Meeting, the Nobel prize winning particle physicist Dr. Carlo Rubbia gave a talk in which he indicated a major problem in separating the data from the artifacts of machine operation. The only way to be certain of the results, was when different accelerators gave consistent data at the same energies. During the comment and question session following his talk, I asked him if the current accelerators had reached the point of diminishing returns, and he answered "Yes." So we face the prospect of spending many billions of dollars for a machine that will produce uncertain results, of marginal value, a real "white elephant." The following excerpts from the letter published in the July 1988 issue of Physics Today, [95] by Dr. John F. Waymouth of GTE that is titled "WHAT PRICE FUNDING THE SUPER COLLIDER?" brings to bear some interesting arguments on this question:

> I am an R&D director in industry whose own work is almost entirely company funded. I nevertheless believe that government funding of long-range research in the physical sciences is essential to the future health of the US economy.
>
> I am, however, extremely distressed by the direction that recent proposals for such funding are taking toward hundreds of millions, ultimately billions of dollars for a gigantic particle accelerator to explore physical phenomena in the tera- electron-volt range. At the same time, I see from my perspective as an eventual "customer" of university-based low- energy plasma, atomic, molecular, electron and optical physics research, and as a former member of the NSF Advisory Committee for Physics, that these areas are being severely constrained by inadequate funding. I believe that this allocation of priorities in funding of the physical sciences would be in error, for the reasons outlined in the following...
>
> This line of reasoning leads me to the conclusion that the only satisfactory argument justifying society's support of physics research over the long term is the fourth one: that physics research in the past has led to a cornucopia of new products, industries and jobs and thereby to the wealth and quality of life that we now enjoy; failure on our part to provide the same kind of support will deprive our children, and our children's children, of similar benefits in the future...
>
> As I reflect back on what physics research has provided to society in the past, I am struck by the fact that not all physics research is uniformly productive of economic benefits. In my own mind, I have divided physics into three basic areas: electron-volt physics, in which energy exchanges on an atomic, molecular or electronic scale are less than 100 000 volts; MeV- GeV physics, which primarily involves nuclear and subnuclear particles; and high-energy physics, covering GeV to TeV and up, involving the structure of subnuclear matter.
>
> Out of Ev physics have come electricity and magnetism, telegraphy, telephony, the electric light and power industry, stationary and propulsion electric motors, radio, television, lasers, radar and microwave ovens, to name just a few. In short, it is the core

> science of the modern world.
>
> X rays and the resulting medical physics industry were the high-energy physics of their day, but fall within my definition of Ev physics. Digital computers arose from the computational needs of MeV physics, but the technology for satisfying those needs came entirely out of Ev physics; microminiaturization of those computers for space exploration was accomplished also by Ev physics, resulting in the capability to put computing power undreamed of by John von Neumann in the hands of an elementary school child.
>
> Moreover, Ev physics has been the core science in the training of generations of engineers who have invented, developed and improved products in all of the above areas. It is, in addition, the core science in the extremely exciting development of understanding of the detailed processes involved in chemical reactions, and the ultimate understanding of biological reactions and the life process itself. Every single member of our society has been touched in very substantial ways by the accomplishments of Ev physics, and many of them are fully aware of it.
>
> MeV-GeV physics has given us radioisotope analysis, a substantial portion of medical physics, and nuclear energy (which a significant, vocal minority of our society regards as an unmitigated curse instead of a blessing). High-energy physics has to date given us nothing...
>
> In my opinion, there is another interpretation. Electron- volt physics is the science of things that happen on Earth; MeV-GeV physics is the science of things that happen in the Sun, the stars and the Galaxy; TeV physics has not happened anywhere in the universe since the first few milliseconds of the Big Bang (except possibly inside black holes, which are by definition unknowable).
>
> Consequently, it should come as no surprise that items useful on Earth will come primarily from the branch of physics that deals with what happens here on Earth, with lesser contributions from the science of what happens in the nearby Sun and the intervening space. I firmly believe that this situation is quite fundamental, and that despite the best efforts of many dedicated TeV physicists, the probability that economic benefit to society in the future will result from their activities is very remote: in the phraseology of the research director justifying his budget, "a high-risk, longshot gamble."

Waymouth's above article presented the currently popular argument for the justification of funding the SSC, that it will shed light on the phenomena that happened in the first few milliseconds of the Big Bang creation of the entire universe. In examination of this argument we should consider the fact that there is ample evidence that Big Bang creation theories are pathological science at its very worst. Some interesting insight into the development of the Big Bang type of theories is contained in the following excerpts from a recent Physics Today article [96] titled "EDWIN P. HUBBLE AND THE TRANSFORMATION OF COSMOLOGY":

> ...It is now usual to trace the idea of an expanding universe, at least in the mathematical sense, to two papers [97] published by the Russian mathematician and meteorologist Alexander Friedmann in 1922 and 1924. Friedmann's starting point was the field equations of general relativity that Einstein had developed in 1917,... Rather, the first person to join theory and observation in a way that would come to be widely seen as physically meaningful within the general framework of the expanding universe was, as Helge Kragh has argued convincingly, [98] a 33-year-old Belgian abbé and professor at the University of Louvain, Georges Lemaître.
>
> In 1927 Lemaître published what would later be recognized as the seminal paper on the expanding universe. [99] But for a brief time, Lemaître's work drew no interest. Even Einstein told Lemaître, at the fifth Solvay conference in 1927, that he did not accept the notion of the expanding universe or the physics underpinning the paper...
>
> Hubble was always careful in print to avoid definitely interpreting the redshifts as Doppler shifts. But the writings of Eddington and others soon meshed the calculations of Lemaître and various theorists with Hubble's observational research on the redshift-distance relation. The notion of the expanding universe was swiftly accepted by many, and the linear relationship between redshift and distance was later widely accepted as Hubble's law.
>
> ...But Eddington explicitly rejected the notion of a creation of the universe, as seemed to be implied by a universe with more mass than the Einstein universe, because "it seems to require a sudden and peculiar beginning of things."...
>
> During the early 1930s several people, including a sometime collaborator of Hubble's, the Caltech mathematical physicist Richard C. Tolman, examined possible physical mechanisms to explain the expansion. Of course an alternative explanation of the expansion was that it really did start with the beginning of the entire universe, and it was Lemaître who introduced this concept into the cosmological practice of the 1930s. In 1931 he suggested the first detailed example of what later became known as Big Bang cosmology. But unlike the universe of modern Big Bang theories, Lemaître's universe did not evolve from a true singularity but from a material pre-universe, what Lemaître referred to as the "primeval atom". [98]

Additional insight into Hubble's views of this matter comes from the following material taken from a 1986 article [100] by Dr. Barry Parker of the Idaho State University, titled "Discovery of the Expanding Universe":

> It was evident by now, however, that Hubble's attitude had changed. He no longer referred to his graph as a velocity- distance relation, though still confident that his distance scale was reasonably accurate. The interpretation of redshifts as velocities bothered him, and he now referred to "apparent velocity displacements." This wording implied there were other possibilities, and indeed there were...

> Lemaitre's theory also predicted an expanding universe, so in itself it probably did not bother Hubble. However, a paper published the same year by his Mount Wilson colleague Fritz Zwicky apparently did. Zwicky was convinced that the redshift did not necessarily indicate motion; he was sure that the extremely large speeds recently obtained by Humason were impossible.
>
> As an alternative, Zwicky introduced the idea that the redshifts were due to an interaction between light and matter in space. The light gradually lost energy, which shifted it, and the spectral lines, to redder wavelengths. The farther away an object, the more its light would "tire" during the trip to Earth... He was now very close to the limit of the 100- inch telescope, but there was a new one on the horizon, the 200-inch. He was confident that this instrument would enable astronomers to resolve, once and for all, most of the major cosmological problems...

With regard to Hubble's expectation that the 200-inch would resolve the problem, the following information taken from a recent article [101] published in THE ASTROPHYSICAL JOURNAL by Dr. Paul A. LaViolette, and titled "IS THE UNIVERSE REALLY EXPANDING", shows that the current evidence supports the Zwicky tired-light model. The abstract of the article reads:

> The no-evolution, tired-light model and the no-evolution, *q*o = 0, expanding universe cosmology are compared against observational data on four kinds of cosmological tests. On all four tests the tired-light model is found to make the better fit to the data without requiring the ad hoc introduction of assumptions about rapid galaxy evolution. The data may be interpreted in the simplest fashion if space is assumed to be Euclidean, galaxies cosmologically static, evolutionary effects relatively insignificant, and photon energy nonconserved, with photons losing about 5%-7% of their energy for every 109 light years of distance traveled through intergalactic space. The observation that redshifts are quantized may be accommodated by a version of the tired-light model in which photon energy decreases occur incrementally in a stepwise fashion.

The introduction of the article starts with:

> The notion that the cosmological redshift is a non-Doppler phenomenon in which photons continuously undergo an energy depletion or "aging" effect is not new. This idea was first suggested by Zwicky (1929). Later, Hubble and Tolman (1935) discussed this alternative, postulating that photon energy was depleted in a linear fashion with increasing photon travel distance. Hubble (1936) claimed that his galaxy number count results strongly supported the linear energy depletion hypothesis...

On the 2nd page of the article LaViolette writes:

> The performance of the tired-light and expanding universe comologies are evaluated on four cosmological tests: the angular size-redshift test, the Hubble diagram test, the galaxy number-count-magnitude test, and the number-count-flux density test (log *dN/dS*-log *S* test). It is determined that on all four tests the tired-light model exhibits

> superior performance. That is, it makes the best fit to the data with the fewest number of assumptions. Finally, the redshift quantization phenomenon is briefly discussed. Although not a cosmological test per se, this phenomenon is something that any candidate cosmology must somehow address. It is shown that redshift quantization is quite compatible with the tired-light model. On the other hand, when the expanding universe hypothesis is adhered to, ad hoc assumptions must be introduced about the possible existence of macroscopic dynamical quantization in the universe's expanding motion.

In the CONCLUSION LaViolette states:

> ...It is concluded that the tired-light model makes a better fit on all four data sets. The expanding universe hypothesis may be considered plausible only if it is modified to include specific assumptions regarding the evolution of galaxy cluster size, galaxy radio lobe size, galaxy luminosity, and galaxy number density. In addition, if the redshift quantization effect is also to be accounted for, special assumptions must be introduced regarding the operation of dynamical quantization on a cosmological scale. But the required assumptions are numerous. Consequently, the tired-light model is preferred on the basis of simplicity. Presently available observational data, therefore, appear to favor a cosmology in which the universe is conceived of as being stationary, Euclidean, and slowly evolving, and which photons lose a small fraction of their total energy for every distance increment they cover on their journey through space.

In a recent review [102] of a book [103] titled "QUASARS, REDSHIFTS, AND CONTROVERSIES" published by Dr. Halton Arp, the world-renowned astrophysicist Dr. Geoffrey Burbidge, writes:

> Chip Arp started with impeccable credentials. Educated at Harvard and Caltech, after a short spell at Indiana he was appointed to a staff position at the Mount Wilson and Palomar Observatories, where he remained for 29 years. A little more than 20 years ago Arp began to devote all his time to extragalactic astronomy. At first he compiled the marvelous Atlas of Peculiar Galaxies. Then he started to find what he believed were physical associations between some of these galaxies and previously identified powerful radio sources. Soon he found many cases of apparent associations between galaxies and quasi-stellar objects, or quasars.
>
> All of this would have been completely acceptable if the associated objects had the same redshifts, but they did not. Yet Arp believed in the reality of the associations, and, after struggles with referees, his papers were published. Others were finding similar results, and soon the terms "nonvelocity redshifts" (those not associated with the expansion of the universe) and "local" (as distinct from distant, or "cosmological") quasars entered the literature. Arp's ranking in the "Association of Astronomy Professionals" plunged from within the first 20 to below 200. As he continued to claim that not all galaxy redshifts were due to the expansion of the universe, his ranking dropped further.

> About four years ago came the final blow: his whole field of research was deemed unacceptable by the telescope-allocation committee in Pasadena. Both directors (of Mount Wilson and Las Campanas, and Palomar, observatories) endorsed the censure. Since Arp refused to work in a more conventional field, he was given no more telescope time. After abortive appeals all the way up to the trustees of the Carnegie Institution, he took early retirement and moved to West Germany. Earlier, Fritz Zwicky had also been frequently criticized by his colleagues in Pasadena (by coincidence?). Zwicky remained a staff member at Mount Wilson and Palomar until he retired, but much of his work continued to be ignored or derided until some years after his death.
>
> Quasars, Redshifts, and Controversies contains Arp's account of his own work and that of others leading, in his mind, to the conclusion that redshifts are not always correlated with distances. It also contains his personal view of the way he has been treated. When he is critical of others, he omits their names. Zwicky was more blunt in his Morphological Astronomy...
>
> The other part of this learning process has been unpleasant, probably because I have a strong instinct for fair play. It may be argued that this is no substitute for good judgement. But neither are the tactics that have been used by those who want to maintain the status quo. These include interminable refereeing, blackballing of speakers at meetings, distortion and misquotation of the written word, rewriting of history, and worst of all, the denial of telescope time to those who are investigating what some believe are the wrong things. Thus, for both scientific and sociological reasons, I am sympathetic to Arp...
>
> In my view the best evidence for the existence of noncosmological redshifts is the following: the three quasars within 2 arc minutes of the center of NGC 1073, each have a redshift at a peak in the distribution found earlier; the low- redshift quasar Markarian 205 joined to NGC 4319; the pair of galaxies NGC 7603 and its companion, which are connected by a luminous bridge but have very different redshifts; and the statistical evidence relating many quasars to bright not faint galaxies...
>
> One of the most fascinating chapters describes the idea that the alignments of objects with different redshifts are not accidental, but real, implying that galaxies can eject objects, up to and including other galaxies...

Dr. I. E. Segal of M.I.T. has published an article [104] that examines the claim that the cosmic background radiation is evidence in support of the Big Bang theories. In the last sentence of the article, he states:

> ...Unless it can be shown that a temporally homogeneous universe is not physically sustainable, and this has not been possible even in the specific, nonparametric case of the chronometric cosmology, a claim for the big bang theory that it is the natural or logical explanation for the CBR and its apparently Planck law spectrum would appear untenable.

With regard to the current evidence on the radiation, a recent article [134] titled "Background radiation deepens the confusion for big bang theorists" states:

> THE LATEST results from NASA's Cosmic Background Explorer (COBE) satellite are continuing to mystify astronomers. They show that the matter of the early Universe was spread so smoothly that it is difficult to understand how galaxies and clusters of galaxies could have formed (New Scientist,Science, 19 December).
>
> Astronomers presented the results last week at a meeting of the American Physical Society in Washington DC. Although the results confirm those released earlier, they are from observations of the whole sky rather than from just a small portion (This Week,20 January).
>
> COBE was launched earlier this year to observe the cosmic background radiation, the remnant radiation of the big bang in which the Universe was born 15 billion years ago. The radiation was created a mere 300 000 years after the big bang. By determining how smoothly that radiation is distributed across the sky we can learn how smoothly matter was distributed at that epoch.
>
> "These measurements are more and more puzzling," says Michael Hauser of the NASA-Goddard Space Flight Center. The COBE data show that 300 000 years after the big bang, the matter of the Universe had a density uniform to one part in 10,000.
>
> Many of the scientists at the meeting expressed concern that many accepted theories of galaxy formation will have to go if the data build up and continue to show there is no variation in the background radiation. Galaxies could only have condensed from the stuff of the big bang if it was lumpy.
>
> "We will be surprised if we don't start seeing wiggles at the level of one part in 100 000 of accuracy," said David Wilkinson of Princeton University. "If COBE gets to [one part in a million] and still sees things smooth big bang theories will be in a lot of trouble."
>
> According to George Smoot of the University of California, Berkeley, the data from COBE are really more accurate than one part in 10,000, but the scientists are not revealing these data until they have a chance to correct for any systematic errors. They hinted, however, that they have found nothing even at this level of detail.

There was a 1/3/91 article in my local St. Petersburg Times newspaper that was reprinted from The New York Times. The title of the article was Big Bang theory turning out to be big bust and the abstract states:

> Satellite research casts doubt on a key part of the widely held theory of how the universe was formed.

Two paragraphs in the middle of the article state:

> In a report published today in the journal *Nature*, they said the theory in its present form must be abandoned.
>
> The journal noted that the report by Dr. Will Saunders of Oxford University and colleagues "is all the more remarkable for coming from a group of authors that includes some of the theory's long time supporters."

The Big Bang theories fit all of Langmuir's rules for pathological science, but in particular, they fit his 4th one of "Fantastic theories contrary to experience." For example, the following is the sort of fantastic arguments one finds in most modern text books on this matter:

> ...These new theories are call Grand Unified Theories or GUTs.
>
> Studies of GUTs suggest that the universe expanded and cooled until about 10-35 seconds after the big bang, at which time it became so cool that the forces of nature began to separate from each other. This released tremendous amounts of energy, which suddenly inflated the universe by a factor between 1020 and 1030. At that time the part of the universe that we can see now, the entire observable universe, was no larger than the volume of an atom, but it suddenly inflated to the volume of a cherry pit and then continued its slower expansion to its present extent... [8 p.325]

As another example of the fantastic type of arguments one finds in scientific journals, the following was taken from a article [105] titled "The Inflationary Universe" that was published in the prestigious journal Scientific American:

> From a historical point of view probably the most revolutionary aspect of the inflationary model is the notion that all matter and energy in the observable universe may have emerged from almost nothing. This claim stands in marked contrast to centuries of scientific tradition in which it was believed that something cannot come from nothing.

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Mathematical Magic

There is a tradition of brown-bag lunch in the foyer of the Science auditorium at Eckerd College. Most of the Natural Science Collegium faculty tend to observe this tradition, and it is not unusual to have faculty from the other Collegiums or even the President or Dean of the College to attend the lunch as well. The well upholstered easy chairs and sofas are dragged over the carpet to form a circle, and the lunch becomes an informal discussion group, with wide ranging topics from sports to philosophy. Many of the arguments presented in this book have evolved from the discussions and debates at this lunch, and even the book itself has become a topic of discussion, as I've passed out copies of the material as it has developed to interested faculty members, in an effort to obtain input from the group. One of the topics that was discussed was the question of the nature of mathematics. It was interesting to find that the Math faculty had no simple well defined definition of Mathematics! My Grolier Encyclopedia states that the word was derived from the Greek word for learning *mathema*, and that Mathematical scholars disagree upon a definition of mathematics. The article goes on to state under HISTORY:

> As a recognizable discipline, mathematics is found first among the ancient Egyptians and the Sumerians. In fact, the Egyptians probably had considerable mathematical knowledge as early as 2900 B.C., when the Great Pyramid of Gizeh was built. A handbook upon mathematics, known as the Ahmes Papyrus, written about 1550 B.C., shows that the early Egyptians could solve many difficult arithmetical problems. Some modern scholars believe that the Sumerians, who were the predecessors of the Babylonians, may have had a system of arithmetic as early as 3500 B.C. The Sumerians and Babylonians applied arithmetic and elementary geometry to the study of astronomical problems and to the construction of great irrigation and other engineering projects.
>
> The Greek philosopher-mathematician Thales is usually regarded as the first to realize the importance of organizing mathematics upon a logical basis. Such a tradition was carried on and further developed in early times by Pythagoras, Plato, Aristotle, and especially by the mathematicians of the Alexandrian School. The famous University of Alexandria, between 300 B.C. and 500 A.D., had upon its staff such distinguished mathematicians as Euclid, Archimedes, Apollonius, Eratosthenes, Ptolemy, Heron, Menelaus, Pappus, and Diophantus.
>
> For nearly a thousand years before the 15th century little original work was done in the field of mathematics except that produced by the Hindus and the Arabs. In the 16th century Tartaglia, Cardan, and Ferrari in Italy and Vieta in France laid the foundations of modern algebra. The 17th century produced many outstanding mathematicians including Descartes, Newton, Leibnitz, Fermat, Pascal, Desargues, Napier, and Kepler. During the 17th century mathematics was extended in many directions, and modern

> analysis was born with the invention of the calculus. The 18th, 19th, and the first half of the 20th centuries have seen a tremendous growth in the development of mathematical theory, and mathematical techniques have been introduced into virtually all branches of pure and applied science.

I presented the argument that mathematics was a language. My view on this matter was based on the following statement by Dr. Robert B. Fischer, in his book "Science Man and Society":

> The language of mathematics, which consists of its symbols and their relationships, is very much at the heart of the practice of virtually all fields of science. [40]

My view was also shaped by various statements made by Prof. Albert Einstein such as the following sentence:

> It demands the highest possible standard of rigorous precision in the description of relations, such as only the use of mathematical language can give. [39 p.225]

Prof. Richard Rhodes II, a member of the Physics faculty, and a graduate of Yale University, told a story in support of my argument. The story concerned a statement made by Prof. Josiah Willard Gibbs, Yale's first professor of mathematical physics. With regard to Gibbs, the following was taken from an article on him entitled "A loner's legacy":

> Gibb's work was so advanced that one of his great admirers, Albert Einstein, complained about one of his papers that "it is hard to read and the main points have to be read between the lines." However, Einstein also termed it "a masterpiece." Scientists have been reading between the lines since Gibbs first laid out the fundamental equations of thermodynamics and reshaped the study of relations between energy and the composition of matter into a modern field with implications still being found. [41]

The story came from a biography on Gibbs by Dr. Muriel Rukeyser, and goes as follows:

> A story is told of him, the one story that anyone remembers of Willard Gibbs at a faculty meeting. He would come to meetings - these faculty gatherings so full of campus politics, scarcely veiled manoeuvres, and academic obstacle races - and leave without a word, staying politely enough, but never speaking.
>
> Just this once, he spoke. It was during a long and tiring debate on elective courses, on whether there should be more or less English, more or less classics, more or less mathematics. And suddenly everything he had been doing stood up and the past behind him, his father's life, and behind that, the long effort and voyage that had been made in many lifetimes and he stood up, looking down at the upturned faces, astonished to see the silent man talk at last. And he said, with emphasis, once and for all:
>
> "Mathematics *is* a language." [42]

Following Rhodes' story about Gibbs, everyone seemed to agree, that yes, mathematics is a language.

The major problem with mathematics, is that for the average person, it is a foreign language. To illustrate this point, I will cite several paragraphs taken from a very interesting article published in Physics Today, entitled "Math anxiety and physics: Some thoughts on learning 'difficult' subjects":

> However, students bring more than Aristotelianisms to class. They consider science in general and physics in particular "hard" subjects to learn. As Robert Fuller of the University of Nebraska points out, professors intentionally and unintentionally contribute to this reputation. In a proposal, since funded by Exxon, for AAPT workshops to help teachers develop student confidence in physics, Fuller notes that "Opening lectures often describe the *high* standards maintained by the department, the *firm* math prerequisites, the poor grade records of previous classes." Even when they do not make such explicit statements, teachers convey the message that physics is a particularly difficult subject, says Fuller, and this damages student confidence.
>
> How significant, then, is apprehension in discouraging nonscience undergraduates from attempting physics? Might the anxiety-reduction techniques that proved useful in treating fear of mathematics work for the physics student? While it remains to be seen whether the sources of physics anxiety and math anxiety are the same, one thing is clear to someone who has dealt with fear of mathematics in college-age students: The two have similar manifestations. Hence, even though the discussion in the first half of this article focuses on obstacles to learning mathematics, I think readers will find that it rings true for physics as well. ...
>
> Instead, what appears to link students of very diverse mathematical "ability" is a collection of what might be called ideological beliefs or prejudices about the subject. Students' early experiences with mathematics typically give them false impressions not only of the nature of the subject, but also, and more perniciously, of the kinds of skills required to master it. They think, for example, that speed is more important than persistence. Even more humbling, most come away from their exposure to mathematics believing they do not have the *sine qua non* of mathematics success, namely, a "mathematical mind."
>
> When the students that I interviewed particularly the woman students decided to stop taking mathematics, they explained this in terms of their feelings: They felt helpless and out of control in confronting mathematics; they were easily bewildered and found themselves humiliated in class; they were uneasy solving or analyzing problems under time pressure, and they had become distrustful of intuitive ideas that had not been formally introduced in the text. Because of all this, the students felt compelled to memorize solutions to individual problems. [43]

Mathematics forms the foundation of the technical jargon that the average physicist uses to confuse the issues and enhance his status by over publishing his work. The same basic equations, or algebraic variations of them, are repeated over and over in the literature. If the unneeded equations were eliminated, the articles would be easier to understand, and the inflated volume of the physics journals would be reduced by at least 90%. To illustrate the problem, I will make several quotes from an article by Prof. N. David Mermin entitled "WHAT'S WRONG WITH THESE EQUATIONS?":

> A major impediment to writing physics gracefully comes from the need to embed in the prose many large pieces of raw mathematics. Nothing in freshman composition courses prepares us for the literary problems raised by the use of displayed equations. Our knowledge is acquired implicitly by reading textbooks and articles, most of whose authors have also given the problem no thought...
>
> Admittedly sometimes an equation is buried so deep in the guts of an argument, so contingent on context, so ungainly in form that no brief phrase can convey to a reader even a glimmer of what it is about, and anybody wanting to know why it was invoked a dozen pages further on cannot do better than wander back along the trail and gaze at the equation itself, all glowering and menacing in its lair... Indeed, is the equation itself essential? Or is it the kind of nasty and fundamentally uninteresting intermediate step that readers would either skip over or, if seriously interested, work out for themselves, in neither case needing to have it appear in your text?...
>
> We punctuate equations because they are a form of prose (they can, after all, be read aloud as a sequence of words) and are therefore subject to the same rules as any other prose... Most journals punctuate their equations, even if the author of the manuscript did not, but a sorry few don't, removing all vestiges of the punctuation carefully supplied by the author. This unavoidably weakens the coupling between the math and the prose, and often introduces ambiguity and confusion. [44]

Dr. Oliver C. Wells is a research scientist at the IBM Thomas J. Watson Research Center, and concerning the difficulty in understanding the mathematics and technical jargon in physics, writes:

> On the subject of writing style, I am frequently horrified to discover that I quite simply cannot understand even the first paragraph of a technical article on a subject quite close to my own major area of interest. [45]

The Executive Director of the scientific research society Sigma Xi, has published a booklet on scientific ethics. [50] On page 11 of Chapter 3 which is titled "Trimming, Cooking, and Forging" Dr. Jackson starts with:

> Charles Babbage (1792-1871) is generally remembered as the prophet of the electronic computer, because of his "difference engine" and the uncompleted "analytical engine." But he had a much more extensive influence on scientific development. As professor of mathematics at Cambridge University, he published a book entitled *Reflections on the Decline of Science in England.* Since the year was 1830, the same year that Charles Lyell began to publish his *Principles of Geology* and shortly before Charles Darwin set sail on the "Beagle," the title may seem as premature as his calculating devices. Babbage's book, however, is generally given credit as a catalyst in the creation of the

British Association for the Advancement of Science, and indirectly of similar associations in the U.S.A., Australia and elsewhere.

Babbage, the "irascible genius," was also concerned with how science should be done, and the same book describes the forms of scientific dishonesty that give this chapter its title. The definitions used here are phrased in contemporary English; otherwise not much seems to have changed in 150 years.

Trimming:
: the smoothing of irregularities to make the data look extremely accurate and precise.

Cooking:
: retaining only those results that fit the theory and discarding others.

Forging:
: inventing some or all of the research data that are reported, and even reporting experiments to obtain those data that were never performed.

Dishonest deceptions are not unusual in the history of physics. They began with Galileo Galilei, the man who laid the foundations of modern physics. My insight into this matter came from a book titled "The Birth of a New Physics" by Dr. I. Bernard Cohen. [51] On page 66 we find:

> ...Galileo was born in Pisa, Italy, in 1564, almost on the day of Michelangelo's death and within a year of Shakespeare's birth. His father sent him to the university at Pisa, where his sardonic combativeness quickly won him the nickname "wrangler."

And then on page 111:

> Galileo's originality was therefore different from what he boastfully declared. No longer need we believe anything so absurd as that there had been no progress in understanding motion between the time of Aristotle and Galileo. And we may ignore the many accounts that make it appear that Galileo invented modern dynamics with no debt to any medieval or ancient predecessor.
>
> This was a point of view encouraged by Galileo himself but it is one that could be more justifiably held fifty years ago than today. One of the most fruitful areas of research in the history of science in the last half century began chiefly by the French scholar and scientist Pierre Duhem has been the "exact sciences" of the Middle Ages. These investigations have uncovered a tradition of criticism of Aristotle which paved the way for Galileo's own contributions. By making precise exactly what Galileo owed to his predecessors, we may delineate more accurately his own heroic proportions. In this way, furthermore, we may make the life story of Galileo more real, because we are aware that in the advance of the sciences each man builds on the work of his predecessors...

More than any other man, Sir Isaac Newton set the tone for scientific dishonesty in modern physics by his skilled use of "Mathematical Magic." My insight into this came from a very interesting article titled "Newton and the Fudge Factor" by Dr. Richard S. Westfall. [52] To advance my argument I start with the following paragraph from the article:

> And having proposed exact correlation as the criterion of truth, it took care to see that exact correlation was presented, whether or not it was properly achieved. Not the least part of the *Principia's* persuasiveness was its deliberate pretense to a degree of precision quite beyond its legitimate claim. If the *Principia* established the quantitative pattern of modern science, it equally suggested a less sublime truth that no one can manipulate the fudge factor quite so effectively as the master mathematician himself.

In explaining Newton's motives in fudging his work, I present the following paragraph from Westfall's article:

> The second edition of the *Principia* was at once an amended version of the first edition and a justification of Newtonian science. The battle with the continental mechanical philosophers who refused to have truck with the occult notion of action at a distance still raged. The second edition made its appearance framed, as it were, by its two most important additions, Cotes' "Preface" at the beginning and Newton's "General Scholium" at the end, both of them devoted to the defense of Newtonian philosophy, of exact quantitative science as opposed to speculative hypotheses of causal mechanisms. By 1713, moreover, Newton's perpetual neurosis had reached its passionate climax in the crusade to destroy the arch-villain Leibniz. Only a year earlier the Royal Society had published its *Commercium epistolicum*, a condemnation of Leibniz for plagiary and a vindication of Newton, which Newton himself composed privately and thrust upon the society's committee of avowed impartial judges. In Newton's mind, the two battles merged into one, undoubtedly gaining emotional intensity in the process. Not only did Leibniz try to explain the planetary system by means of a vortex and inveigh against the concept of attraction, but he also encouraged others to attack Newton's philosophy. His arrogance in claiming the calculus was only a special instance of his arrogant presumption to trim nature to the mold of his philosophical hypotheses. In contrast, the true philosophy modestly and patiently followed nature instead of seeking to compel her. The increased show of precision in the second edition was the reverse side of the coin stamped *hypotheses non fingo*. It played a central role in the polemic supporting Newtonian science.

The term "fudge factor" is of course, just a polite way of describing Newton's dishonest ways of Trimming, Cooking, and Forging the data. The following is taken from one of the examples of Newton's fudging in the article:

> In examining the alterations, let us start with the velocity of sound since the deception in this case was patent enough that no one beyond Newton's most devoted followers was taken in. Any number of things were wrong with the demonstration. It calculated a velocity of sound in exact agreement with Derham's figure, whereas Derham himself

> had presented the conclusion merely as the average of a large number of measurements. Newton's assumptions that air contains vapor in the quantity of 10 parts to 1 and that vapor does not participate in the sound vibrations were wholly arbitrary, resting on no empirical foundation whatever. And his use of the "crassitude" of the air particles to raise the calculated velocity by more than 10 percent was nothing short of deliberate fraud.

Interesting additional information with regard to Newton's lack of scientific integrity can be found in an article published by Dr. I. Bernard Cohen in the journal Scientific American. [53] The article is titled "Newton's Discovery of Gravity" and contains the following paragraph:

> A decisive step on the path to universal gravity came in late 1679 and early 1680, when Robert Hooke introduced Newton to a new way of analyzing motion along a curved trajectory. Hooke had cleverly seen that the motion of an orbiting body has two components, an inertial component and a centripetal, or center-seeking, one. The inertial component tends to propel the body in a straight line tangent to the curved path, whereas the centripetal component continuously draws the body away from the inertial straight-line trajectory. In a stable orbit such as that of the moon the two components are matched, so that the moon neither veers away on a tangential path nor spirals toward the earth.

Later in the article Cohen writes this paragraph:

> In his letter Hooke ventured the suggestion that the centripetal force drawing a planet toward the sun varies inversely as the square of the separation. At this point Hooke was stuck. He could not see the dynamical consequences of his own deep insight and therefore could not make the leap from intuitive hunch and guesswork to exact science. He could go no further because he lacked both the mathematical genius of Newton and an appreciation of Kepler's law of areas, which figured prominently in Newton's subsequent approach to celestial dynamics. The law of areas states that the radius vector from the sun to a planet sweeps out equal areas in equal times.

With regard to Newton's philosophy as to the cause of the gravitational force, we find the following paragraph:

> Although Newton at times thought universal gravity might be caused by the impulses of a stream of ether particles bombarding an object or by variations in an all-pervading ether, he did not advance either of these notions in the *Principia* because, as he said, he would "not feign hypotheses" as physical explanations. The Newtonian style had led him to a mathematical concept of universal force, and that style led him to apply his mathematical result to the physical world even though it was not the kind of force in which he could believe.

With regard to Newton's dishonest attempt to claim full credit we find:

> In 1717 Newton wanted to ensure his own priority in discovering the inverse-square law of gravitation, and so he invented a scenario in which he made the famous moon test not while writing the *Principia* but two decades earlier in the 1660's...

And in this same regard, Cohen states this paragraph:

> Newton never published his invented scenario of the early moon test. He included it in the manuscript draft of a letter to the French writer Pierre Des Maizeaux but then crossed it out. Newton also circulated the familiar story that a falling apple set him on a chain of reflections that led to the discovery of universal gravitation. Presumably this invention was also part of his campaign to push back the discovery of gravity, or at least the roots of the discovery, to a time 20 years before the *Principia*.

With Newton as a role model, it's no wonder that modern physics is riddled with an almost complete lack of scientific objectivity and integrity! Additional insight into this matter comes from a very interesting book by Dr. Rudolf Thiel. [54] The insight starts on page 183 with the following paragraph:

> René Descartes dominated the first half of the seventeenth century in his dual capacity of mathematician and philosopher. He had developed mathematical analysis, which wiped out the boundary between geometry and algebra, in which curves became functions. By comparison, Euclidean thinking seemed pedantic and limited. Then he attempted to explain the entire mechanism of the world by ether eddies. These supposedly transmitted light, and at the same time set the celestial bodies in motion. He succeeded in reducing all the phenomena of nature known at the time to this single cause, which transmitted its effect tangible from one thing to another; thus everything was connected in a chain with everything else. Descartes's contemporaries hailed this triumph of reasoning which seemed to explain every detail of the entire Creation.
>
> Then Newton came along with his mathematical proofs of gravitation, which could not be explained by ether eddies. Gravitation was a mystery working over great distances in some inconceivable manner. Such a thing was repugnant to Europeans, who wanted to see the interlocking cause and effect with their own eyes. Newton's version of nature therefore seemed to be a descent from the heights attained by Descartes, retrogression to an outmoded stage of philosophy.
>
> Worse still, in Newton's mighty system there was no room left for the ether. This also undermined the wave theory of light, which Huygens had recently presented to the world. Newton himself regretted this, for the wave theory was essential to his theory of color. There still remained the problem of explaining the spectrum: why were the rays of primary light arranged in the particular order of red, yellow, green, and violet? Why did light consist of many colors; what were colors? According to Huygens they were simply waves of differing lengths, differing frequencies, just like different pitches. The spectrum represented a scale, a gamut of light.

> This explanation seemed to emerge again from another of Newton's experiments. If light is passed through a lens pressed upon a plate of glass, a wreath of colored rings is produced. When monochromatic light is sent through such an apparatus, the rings of each color appear at different distances from one another. Newton measured the distances and was in effect measuring the wave lengths of light. But he would not accept this explanation; light waves could not exist because there was no medium, no ether, to transmit them. So impossible, nonsensical a concept as that of the ether had no right to existence. Anything that did not follow from observations was a hypothesis, he maintained, and hypotheses had no place in experimental science.
>
> Newton therefore concluded that light consisted of corpuscles passing through empty space. The differing distances of the colored rings proved only that the corpuscles were affected by their passage through the lens and the glass, that their character was affected in some way, to what degree depending on their color.
>
> Only Newton with his incredibly sane and all-embracing system, could have succeeded in putting across so absurd a conception. He won the battle completely. The wave theory vanished, and with it Descartes's ether eddies. The whole triumphant world-view of the Baroque Age had been shattered. In its place Newton offered the inexplicable, remote force of gravitation which was, admittedly, a mystery to himself. When he was asked what accounted for it, he flatly refused to venture any opinion: "I do not invent hypotheses."
>
> This attitude of his became a model for future natural philosophers. Henceforth scientists considered it more important to recognize where the limits of science lay than to satisfy the urge for knowledge by unproved speculations, no matter how pretty they might be.
>
> The incomprehensibility of gravitation Newton considered a divine dispensation. The Almighty had denied man ultimate insight into the mystery of His Creation. A Christian must be able to reconcile himself to this fact and Newton was a devout Christian...

With regard to Newton as the role model for the corrupt politics of modern physics, we find on page 185:

> In his mid-fifties there came a radical change in Newton's way of life. He was appointed master of the Royal Mint, an office equivalent to what would now be governor of the Bank of England. He exchanged his modest lodgings at Cambridge for a palace in London, entered society, kept horses, carriages, and servants. His income shot abruptly from sixty to five hundred pounds a year, besides various perquisites; he was able to indulge his taste for philanthropy. He was knighted, and became an influential personage at court. Most important of all, he became president of the Royal Society.

This celebrated association of scientists was about the same age as Newton himself. At the time he was given his professorship, the society became "royal," and was provided with special privileges, robes of state, a mace, and a seal bearing the motto: "Let no one's word be law." But the motto went by the board once Newton was elected with absolute regularity to the presidency. His word was sacred. An excellent model for a cannon was unanimously rejected because Newton declared: "This diabolic instrument will only multiply mass killing." In London the Royal Society was generally known as Sir Isaac's Parliament.

This parliament became the platform for Newton's world fame. But it also embittered the closing days of his life by its frenetic partisanship, in connection with his fourth great contribution, the calculus of fluxions, which has become the core of modern mathematics. This time, however, Newton was not the sole discover of the method. It was simultaneously developed, under the name of the differential calculus, by the German philosopher Leibnitz...

Most of the technical terminology of modern mathematics derives from Leibnitz. All of Europe learned the differential calculus from his textbook. He described the new art of reckoning in such lucid terms that a veritable race began among mathematicians, each trying to outdo the other in elegant solutions of hitherto unsolved problems. Mathematicians posed each other riddles, and sent each other the results in code to be sure that no one copied. The period immediately after Leibnitz was an exciting and glorious era in the history of mathematics. And all the newest discoveries were made by means of Leibnitzian differential quotients. No one had ever heard of Newton's counterpart, his fluxions. Newton had created the method for his own private use, and hesitated to publish it because it was so difficult to grasp. For his *Principia* he therefore invented a less difficult, more geometrical method of proof...

The most remarkable aspect of the whole barren struggle was this: no participant doubted for a moment that Newton had already developed his method of fluxions when Leibnitz began work on the differential calculus. Yet there was no proof, only Newton's word. He had published nothing but a calculation of a tangent, and the note: "This is only a special case of a general method whereby I can calculate curves and determine maxima, minima, and centers of gravity." How this was done he explained to a pupil a full twenty years later, when Leibnitz's textbooks were widely circulated. His own manuscripts came to light only after his death, and then they could no longer be dated.

Though Newton's priority was not provable, it was taken for granted, while Leibnitz was always asked to prove that he had not plagiarized a charge as humiliating as it was absurd. This grotesque situation demonstrates most vividly the authority Newton enjoyed everywhere. He was truly the monarch of all he surveyed, a unique phenomenon. To Western science he occupied the same place that had been held in classical antiquity by Pythagoras whose disciples were wont to crush all opponents with the words: "Pythagoras himself has said so."

In our time, Einstein has replaced Newton as the monarch of physics. Einstein's disciples tend to crush all opponents of his relativity theories by citing chapter and verse of articles he has published. The main problem with this is the fact that Einstein tends to be a moving target, and his arguments are not consistent from paper to paper, and often within the same paper. Louis Essen has published a booklet titled "The Special Theory of Relativity A Critical Analysis" in which he examines this question in great detail. [55] Essen is a prominant English physicist who built the first caesium atomic clock in 1955 and determined the most accurate value for the velocity of light by using a cavity resonator. Skipping around the math, I present the following excerpts from the booklet:

> Perhaps the strangest feature of all, and the most unfortunate to the development of science, is the use of the thought-experiment. The expression itself is a contradiction in terms, since an experiment is a search for new knowledge that cannot be confirmed, although it might be predicted, by a process of logical thought. A thought-experiment on the other hand cannot provide new knowledge; if it gives a result that is contrary to the theoretical knowledge and assumptions on which it is based then a mistake must have been made. Some of the results of the theory were obtained in this way and differ from the original assumptions...
>
> A common reaction of experimental physicists to the theory is that although they do not understand it themselves it is so widely accepted that it must be correct. I must confess that until recent years this was my own attitude. I was, however, rather more than usually interested in the subject from a practical point of view, having repeated, with microwaves instead of optical waves(Essen 1955), the celebrated Michelson-Morley experiment, which was the starting point of the theory. Then with the introduction of atomic clocks, and the enormous increase in the accuracy of time measurements that they made possible, the relativity effects became of practical significance...
>
> Many of the thought-experiments described by Einstein and others involve the comparison of distant clocks. Such comparisons are now made every day at many laboratories throughout the world. The techniques are well known. It seems reasonable, therefore, to consider the thought-experiments in terms of these techniques. When this is done, the errors in the thought-experiments become more obvious. The fact that errors in the theory arise in the course of the thought- experiments may explain why they were not detected for so long. Theoretical physicists might not have considered them critically from an experimental point of view. But if one has been actually performing such experiments for many years, one is in a more favorable position to detect any departure from the correct procedure. In the existing climate of opinion, one needed to be very confident to speak of definite errors in the theory. Was there not perhaps some subtle interpretation that was being overlooked? A study of the literature did not reveal any, but even so it was familiarity with the experiments that gave one the necessary confidence to maintain a critical attitude.
>
> The literature sometimes reveals a remarkable vagueness of expression, a lack of a clear statement of the assumptions of the theory, and even a failure to appreciate the

basic ideas of physical measurement. Ambiguities are not absent from Einstein's own papers, and various writers, even when advancing different interpretations of the theory, are correct in as much as these interpretations can all be attributed to Einstein...

The contraction of length and the dilation of time can now be understood as representing the changes that have to be made to make the results of measurement consistent. There is no question here of a physical theory but simply of a new system of units in which c is constant, and length and time do not have constant units but have units that vary with v2/c2. Thus they are no longer independent, and space and time are intermixed by definition and not as a result of some peculiar property of nature... If the theory of relativity is regarded simply as a new system of units it can be made consistent but it serves no useful purpose... The argument about the clock paradox has continued interminably, although the way the paradox arose and its explanation follow quite clearly from a careful reading of Einstein's paper... The experiment is often expressed in the dramatized form of two twins, one of whom returns from a round trip younger than his brother; and in this form it has received wide publicity... It is illogical to suggest that a result obtained on the basis of the special theory is correct but is a consequence of a completely different theory developed some years later. It is also illogical to assume that accelerations have no effect as he does in A's picture of the events and then to assume that gravitation, which in the general theory is assumed to be equivalent to acceleration, does have an effect... It may be surprising, therefore, to find that a more critical examination of the experiments and the experimental conditions suggests that there is no experimental support for the theory... The experiments of the Michelson-Morley type cannot be taken as supporting the theory, because the theory was developed in order to explain the null result that was obtained... The increase of mass with velocity was predicted for the case of charged particles directly from electromagnetic theory before the advent of relativity theory and was confirmed experimentally by Kaufmann...

18. Conclusions

A critical examination of Einstein's papers reveals that in the course of thought-experiments he makes implicit assumptions that are additional and contrary to his two initial principles. The initial postulates of relativity and the constancy of the velocity of light lead directly to length contraction and time dilation simply as new units of measurements, and in several places Einstein gives support to this view by making his observers adjust their clocks. More usually, and this constitutes the second set of assumptions, he regards the changes as being observed effects, even when the units are not deliberately changed. This implies that there is some physical effect even if it is not understood or described. The results are symmetrical to observers in relative motion; and such can only be an effect in the process of the transmission of the signals. The third assumption is that the clocks and lengths actually change. In this case the relativity postulate can no longer hold.

The first approach, in which the units of measurement are changed, is not a physical theory, and the question of experimental evidence does not arise. There is no evidence

> for the second approach because no symmetrical experiment has ever been made. There is no direct experimental evidence of the third statement of the theory because no experiments have been made in an inertial system. There are experimental results that support the idea of an observed time dilation, but accelerations are always involved, and there is some indication that they are responsible for the observed effects.

My main insight into Einstein and his work came from a book by Dr. Abraham Pais titled 'Subtle is the Lord...' The Science and the Life of Albert Einstein. [37] Pais is an award-winning physicist who knew Einstein personally during the last nine years of his life. On page 13 we find that in Einstein's own words he had been an "unscrupulous opportunist." On page 44 we learn that Einstein did not attend lectures or study, but instead used Marcel Grossman's lecture notes to pass his college examinations. With regard to the mathematics of relativity, page 152 states:

> Initially, Einstein was not impressed and regarded the transcriptions of his theory into tensor form as 'uberglussige Gelehrsamkeit,' (superfluous learnedness). However, in 1912 he adopted tensor methods and in 1916 acknowledged his indebtedness to Minkowski for having greatly facilitated the transition from special to general relativity.

Since most scientists do not use or are conversant in tensor mathematics, its use has tended to obscure the intimate meaning behind the relativity theoretical arguments. On page 164 Pais asks:

> Why, on the whole, was Einstein so reticent to acknowledge the influence of the Michelson-Morley experiment on his thinking?

On page 168 we find the answer to this question in the second volume of Sir Edmund Whittaker's masterpiece book entitled "History of the Theories of Aether and Electricity", where:

> Whittaker's opinion on this point is best conveyed by the title of his chapter on this subject: 'The Relativity Theory of Poincaré and Lorentz.'

In effect Whittaker showed that Einstein's special relativity theory was not original work, but just a clever restatement of the theoretical work of Poincaré and Lorentz. The translation of Lorentz's 1904 relativity paper [57 p.12] states:

> ...Poincaré has objected to the existing theory of electric and optical phenomena in moving bodies that, in order to explain Michelson's negative result, the introduction of a new hypothesis has been required, and that the same necessity may occur each time new facts will be brought to light. Surely this course of inventing special hypotheses for each new experimental result is somewhat artificial. It would be more satisfactory if it were possible to show by means of certain fundamental assumptions and without neglecting terms of one order of magnitude or another, that many electromagnetic actions are entirely independent of the motion of the system.

The translation of Einstein's 1905 special relativity paper [57 p.37] presented the argument that one could explain many electromagnetic actions by fundamental assumptions based on two postulates and that the "introduction of a "luminiferous ether" will prove to be superfluous", and his paper made no direct reference to the Michelson-Morley experiment or the work of Poincaré and Lorentz. On page 313 of Pais' book we learn that in 1920, after Einstein had become famous, he made an inaugural address on aether and relativity theory for his special chair in Leiden. In the address he states:

> The aether of the general theory of relativity is a medium without mechanical and kinematic properties, but which codetermines mechanical and electromagnetic events.

So we finally find that relativity is an ether theory after all, and that this ether has arbitrary abstract contradictory physical characteristics! This illustrates the arbitrary nature of relativity, most physicists, and for that matter, most physics text books, present the argument that relativity is not an ether theory. On page 467 we find that near the end of his life, Einstein wrote to his dear friend M. Besso in 1954:

> I consider it quite possible that physics cannot be based on the field concept,i.e., on continuous structures. In that case, *nothing* remains of my entire castle in the air, gravitation theory included, [and of] the rest of modern physics.

With regard to the problem of the average physicist not understanding relativity theory, Dr. S. Chandrasekhar, a Nobel laureate physicist, writes in an article [46] titled "Einstein and general relativity: Historical perspectives":

> The meeting of November 6, 1919 of the Royal Society also originated a myth that persists even today (though in a very much diluted version):"Only three persons in the world understand relativity." Eddington explained the origin of this myth during the Christmas-recess conversation with which I began this account.
>
> Thomson, as President of the Royal Society at that time, concluded the meeting with the statement:"I have to confess that no one has yet succeeded in stating in clear language what the theory of Einstein really is." And Eddington recalled that as the meeting was dispersing, Ludwig Silberstein (the author of one of the early books on relativity) came up to him and said: "Professor Eddington, you must be one of three persons in the world who understands general relativity." On Eddington demurring to this statement, Silberstein responded, "Don't be modest Eddington." And the Eddington's reply was, "On the contrary, I am trying to think who the third person is!"

This lack of comprehension of Relativity theory, is not uncommon among physicists and astronomers. Over the years, in many intimate conversations and correspondence with them, I've found few scientists willing to admit to an indepth understanding of the theory, yet most of them will argue of their belief in it. I have also discovered that even the scientists that are willing to admit to full comprehension of the theory, have serious gaps in their knowledge of it. For example, Prof. William H. McCrea of England wrote the counter argument to Prof. Herbert Dingle's controversial attack on the inconsistent logic in the theory, which was published in the prestigious journal NATURE. [47] Dingle was an interesting fellow, at one time he was a leading proponent of the relativity theory, and even was a member of several British solar eclipse expeditions. He was a professor at University College in London, and the author of many books and papers on astrophysics, relativity, and the history of science. I was introduced to McCrea by Prof. Thornton Page, at the 1968 Fourth Texas Symposium on Relativistic Astrophysics. McCrea who is considered to be an authority on relativity theory, was surprised to find that Einstein considered relativity to be an ether theory. With regard to the argument that I showed McCrea that represented relativity as an ether theory, Einstein and Infeld state:

> ...On the other hand, the problem of devising the mechanical model of ether seemed to become less and less interesting and the result, in view of the forced and artificial character of the assumptions, more and more discouraging.
>
> Our only way out seems to be to take for granted the fact that space has the physical property of transmitting electromagnetic waves, and not to bother too much about the meaning of this statement. We may still use the word ether, but only to express some physical property of space. This word ether has changed its meaning many times in the development of science. At the moment it no longer stands for a medium built up of particles. Its story, by no means finished, is continued by the relativity theory. [20 p.153]

There is a very interesting article on this question published in the August 1982 issue of Physics Today by Prof. Yoshimasa A. Ono. The article begins:

> It is known that when Albert Einstein was awarded the Nobel Prize for Physics in 1922, he was unable to attend the ceremonies in Stockholm in December of that year because of an earlier commitment to visit Japan at the same time. In Japan, Einstein gave a speech entitled "How I Created the Theory of Relativity" at Kyoto University on 14 December 1922. This was an impromptu speech to students and faculty members, made in response to a request by K. Nishida, professor of philosophy at Kyoto University. Einstein himself made no written notes. The talk was delivered in German and a running translation was given to the audience on the spot by J. Isiwara, who had studied under Arnold Sommerfeld and Einstein from 1912 to 1914 and was a professor of physics at Tohoku University. Isiwara kept careful notes of the lecture, and published his detailed notes (in Japanese) in the monthly Japanese periodical *Kaizo* in 1923; Ishiwara's notes are the only existing notes of Einstein's talk...

Ono ends his introduction to his translation with the statement:

> It is clear that this account of Einstein's throws some light on the current controversy as to whether or not he was aware of the Michelson-Morley experiment when he proposed the special theory of relativity in 1905; the account also offers insight into many other aspects of Einstein's work on relativity.

With regard to the ether, Einstein states:

> Light propagates through the sea of ether, in which the Earth is moving. In other words, the ether is moving with respect to the Earth...

With regard to the experiment he argues:

> Soon I came to the conclusion that our idea about the motion of the Earth with respect to the ether is incorrect, if we admit Michelson's null result as a fact. This was the first path which led me to the special theory of relativity. Since then I have come to believe that the motion of the Earth cannot be detected by any optical experiment, though the Earth is revolving around the Sun. [48]

The above information gives us insight into the nature of Einstein's relativity theory. He believes that the sea of ether exists, but he also believes that it cannot be detected by experiments, in other words, he believes it is invisible. The situation in modern physics is very much like the Hans Christian Andersen tale of "The Emperor's New Clothes", with Einstein playing the part of the Emperor. The tale goes that the Emperor, who was obsessed with fine clothing to the point that he cared about nothing else, let two swindlers sell him a suit of cloth that would be invisible to anyone who was "unfit for his office or unforgivably stupid." It turned out that no one could see the suit not the emperor, not his courtiers, not the citizens of the town who lined the street to see him show off his new finery. Yet no one dared admit it until a little child cried out, "But he doesn't have anything on!"

In regard to Einstein's reluctance to acknowledge the influence of the Michelson-Morley experiment on his thinking, and Whittaker's argument that his special relativity theory was a clever restatement of the work of Poincaré and Lorentz, I report the following published [56] statements which Einstein made to Prof. R. S. Shankland on this matter:

> The several statements which Einstein made to me in Princeton concerning the Michelson-Morley experiment are not entirely consistent, as mentioned above and in my earlier publication. His statements and attitudes towards the Michelson-Morley experiment underwent a progressive change during the course of our several conversations. I wrote down within a few minutes after each meeting exactly what I recalled that he had said. On 4 February 1950 he said,"...that he had become aware of it through the writings of H. A. Lorentz, but only after 1905 had it come to his attention." But at a later meeting on 24 October, 1952 he said, "I am not sure when I first heard of the Michelson experiment. I was not conscious that it had influenced me directly during the seven years that relativity had been my life. I guess I just took it for granted that it was true." However, in the years 1905-1909 (he told me) he thought a great deal about Michelson's result in his discussions with Lorentz and others, and *then he realized* (so he told me) that he "had been conscious of Michelson's result *before 1905* partly through his reading of the papers of Lorentz and more because he had simply assumed this result of Michelson to be true."...

With regard to the politics that led to Einstein's fame Dr. S. Chandrasekhar's article [46] states:

> In 1917, after more than two years of war, England enacted conscription for all able-

> bodied men. Eddington, who was 34, was eligible for draft. But as a devout Quaker, he was a conscientious objector; and it was generally known and expected that he would claim deferment from military service on that ground. Now the climate of opinion in England during the war was very adverse with respect to conscientious objectors: it was, in fact, a social disgrace to be even associated with one. And the stalwarts of Cambridge of those days Larmor (of the Larmor precession), Newall, and others felt that Cambridge University would be disgraced by having one of its distinguished members a declared conscientious objector. They therefore tried through the Home Office to have Eddington deferred on the grounds that he was a most distinguished scientist and that it was not in the long-range interests of Britain to have him serve in the army... In any event, at Dyson's intervention as the Astronomer Royal, he had close connections with the Admiralty Eddington was deferred with the express stipulation that if the war should have ended by 1919, he should lead one of two expeditions that were being planned for the express purpose of verifying Einstein's prediction with regard to the gravitational deflection of light... The Times of London for November 7, 1919, carried two headlines: "The Glorious Dead, Armistice Observance. All Trains in the Country to Stop," and "Revolution in Science. Newtonian Ideas Overthrown."

Dr. F. Schmeidler of the Munich University Observatory has published a paper [49] titled "The Einstein Shift An Unsettled Problem," and a plot of shifts for 92 stars for the 1922 eclipse shows shifts going in all directions, many of them going the wrong way by as large a deflection as those shifted in the predicted direction! Further examination of the 1919 and 1922 data originally interpreted as confirming relativity, tended to favor a larger shift, the results depended very strongly on the manner for reducing the measurements and the effect of omitting individual stars.

So now we find that the legend of Albert Einstein as the world's greatest scientist was based on the *Mathematical Magic* of Trimming and Cooking of the eclipse data to present the illusion that Einstein's general relativity theory was correct in order to prevent Cambridge University from being disgraced because one of its distinguished members was close to being declared a "conscientious objector"!

Publication Politics

Marilyn vos Savant is listed in the "Guinness Book of World Records" under highest IQ and publishes an "Ask Marilyn" column in the Sunday Newspaper Magazine PARADE. In the May 22, 1988 issue, Jennifer W. Webster of Slidell, La. asks:

> What one discovery or event would prove all or most of modern scientific theory wrong?

Marilyn replies:

> Here's one of each. If the speed of light were discovered not to be a constant, modern scientific theory would be devastated. And if a divine creation could be proved to have occurred, modern scientists would be devastated.

I suspect that Marilyn has hit the nail on the head. Einstein's special relativity theory with his second postulate that the speed of light in space is constant is the linchpin that holds the whole range of modern physics theories together. Shatter this postulate, and modern physics becomes an elaborate farce! Along with the creation-science debate being published in the letters section of *Physics Today*, there is also a continuing debate on Einstein's relativity theories. My first entry [21] into this debate was as follows:

> Relativity debate continues
>
> I would like to challenge two statements made by Allen D. Allen (November, page 90) in his reply to Wallace Kantor on the question of experimental relativity. Allen states "But Kantor is incorrect in claiming that there is a reliable experiment that refutes special relativity." With regard to this statement the 1961 interplanetary radar contact with Venus presented the first opportunity to overcome technological limitations and perform direct experiments of Einstein's second postulate of a constant light speed of c in space. When the radar calculations were based on the postulate, the observed-computed residuals ranged to over 3 milliseconds of the expected error of 10 microseconds from the best fit the Lincoln Lab could generate, a variation range of over 30,000%. An analysis of the data showed a component that was relativistic in a $c+v$ Galilean sense. [18,19] With regards to Allen's statement "Einstein's original contribution here was to assume that there just is no ether, that is, no frame R such that one's speed with respect to R affects the speed of light," Einstein and Infeld state "This word ether has changed its meaning many times in the development of science. At the moment it no longer stands for a medium built up of particles. Its story, by no means finished, is continued by the relativity theory." [20 p.153,21]

Part of my second letter [22] on this matter, goes as follows:

> ...Concerning Dehmer's comment "In choosing appropriate persons to review the numerous manuscripts, the journal editors use various methods that reflect their own style and areas of expertise," I would like to present the following example of how this has worked for me. On 3 June 1969, I submitted a paper, "An Analysis of Inconsistencies in Published Interplanetary Radar Data," to PRL. The last paragraph of the referee report sent back August 15 states "It is suitable for *Physical Review Letters*, if revised, and deserves immediate publication *if* the radar data can be compared directly to geocentric distances derived from optical directions and celestial mechanics." I revised the paper as the referee recommended and resubmitted it 21 August. The editor, S. A. Goudsmit, sent me a reply 11 September, in which he stated that the paper had been sent to another referee and rejected. I sent a letter 13 September, complaining about the use of the second referee. I received a reply from Goudsmit on 23 September, in which he then stated that he had made a mistake in saying the paper had been sent to a second referee and that it had actually been sent back to the first one. He did this, in spite of the fact that there was absolutely no correspondence between the two reports. They were obviously typed on different typewriters, the first was completely positive, while the second was strongly negative and made no mention of the first report! I eventually published a revised version "Radar Testing of the Relative Velocity of Light in Space" in a less prestigious journal. [18] At the December 1974 AAS Dynamical Astronomy Meeting, E. M. Standish Jr of JPL reported that significant unexplained systematic variations existed in all the interplanetary data, and that they are forced to use empirical correction factors that have no theoretical foundation. In Galileo's time it was heresy to claim there was evidence that the Earth went around the Sun, in our time it is heresy to claim there is evidence that the speed of light in space is not constant...

The above unfair treatment I received in trying to publish a paper challenging Einstein's relativity theories, is not an isolated incident. As an example, as I mentioned in Chapter 6, in a June 1988 letter I received from Dr. Svetlana Tolchelnikova from the USSR, she wrote that thanks to PERESTROIKA she was writing me openly, but that her Pulkovo Observatory is one of the outposts of orthodox relativity. Two scientists were dismissed because they discovered some facts which contradicted Einstein. It is not only dangerous to speak against Einstein, but which is worse it is impossible to publish anything which might be considered as contradiction to his theory. It seems the same situation is true for her Academy. Lest one thinks that this sort of repressive behavior with regard to relativity theory happens only in the USSR, I have heard or read many horror stories of this happening to scientists throughout the world. To document the nature of the problem within the US, I would like to make several quotes from a book on this problem by Ruggero M. Santilli who is the director of The Institute for Basic Research:

> This book is, in essence, a report on the rather extreme hostility I have encountered in U.S. academic circles in the conduction, organization and promotion of quantitative, theoretical, mathematical, and experimental studies on the apparent insufficiencies of Einstein's ideas in face of an ever growing scientific knowledge. [23 p.7]

> In 1977, I was visiting the Department of Physics at Harvard University for the purpose of studying precisely non- Galilean systems. My task was to attempt the generalization of the analytic, algebraic and geometric methods of the Galilean systems into forms suitable for the non-Galilean ones.
>
> The studies began under the best possible auspices. In fact, I had a (signed) contract with one of the world's leading editorial houses in physics, Springer-Verlag of Heidelberg West Germany, to write a series of monographs in the field that were later published in ref.s [24] and [25]. Furthermore, I was the recipient of a research contract with the U.S. Department of Energy, contract number ER-78-S-02- 4720.A000, for the conduction of these studies.
>
> Sidney Coleman, Shelly Glashow, Steven Weinberg, and other senior physicists at Harvard opposed my studies to such a point of preventing my drawing a salary from my own grant for almost one academic year.
>
> This prohibition to draw my salary from my grant was perpetrated with full awareness of the fact that it would have created hardship on my children and on my family. In fact, I had communicated to them (in writing) that I had no other income, and that I had two children in tender age and my wife (then a graduate student in social work) to feed and shelter. After almost one academic year of delaying my salary authorization, when the case was just about to explode in law suits, I finally received authorization to draw my salary from my own grant as a member of the Department of Mathematics of Harvard University.
>
> But, Sidney Coleman, Shelly Glashow and Steven Weinberg and possibly others had declared to the Department of Mathematics that my studies "had no physical value." This created predictable problems in the mathematics department which lead to the subsequent, apparently intended, impossibility of continuing my research at Harvard.
>
> Even after my leaving Harvard, their claim of "no physical value" of my studies persisted, affected a number of other scientists, and finally rendered unavoidable the writing of IL GRANDE GRIDO.*
>
> * S. Glashow and S. Weinberg obtained the Nobel Prize in physics in 1979 on theories, the so-called unified gauge theories, that are crucially dependent on Einstein's special relativity; subsequently, S. Weinberg left Harvard for The University of Texas at Austin, while S. Coleman and S. Glashow are still members of Harvard University to this writing. [23 p.29]

Even Albert Einstein was not immune from pressure from the established politicians in the physics community with regard to the sacred nature of the original special relativity theory, especially with respect to the postulate of the constant speed of light. For example the following quote is from a letter by Dr. E. J. Post in a continuation of the relativity debate:

> At the end of section 2 of his article on the foundations of the general theory, Einstein writes: "The principle of the constancy of the vacuum speed of light requires a modification." [26] At the time, Max Abraham took Einstein to task (in a rather unfriendly manner) about this deviation from his earlier stance. [27]

With regard to the scientist's image of himself, Dr. Spencer Weart writes:

> A number of young scientists and science journalists, mostly on the political left, declared that the proper way to reshape society was to give a greater role to scientifically trained people that is, people like themselves. [17 p.31]

An excellent example of a physicist politician in action was given by President Reagan's former national security adviser Dr. John M. Poindexter who has a doctorate in nuclear physics from the California Institute of Technology, in the 1987 US Senate Iran-Contra hearings. Asked about his destruction of the presidential order, known as a finding, that authorized the November 1985 shipment of missiles to Iran and described it as an arms-for-hostage swap, Poindexter denied that he did it to give the President "deniability." "I simply did not want this document to see the light of day," Poindexter said, puffing on his pipe. Sen. Warren B. Rudman, the vice chairman of the Senate panel, said Poindexter's stress on secrecy and deception was "chilling." As a second example of the open arrogance and lack of objectivity and integrity of the modern physicist politician, I would like to quote from the published retirement address of the particle physicist Dr. Robert R. Wilson, the 1985 president of the American Physical Society:

> Just suppose, even though it is probably a logical impossibility, some smart aleck came up with a simple, self- evident, closed theory of everything. I and so many others have had a perfectly wonderful life pursuing the will-o'-the-wisp of unification. I have dreamed of my children, their children and their children's children all having this same beautiful experience.
>
> All that would end.
>
> APS membership would drop precipitously. Fellow members, could we afford this catastrophe? We must prepare a crisis- management plan for this eventuality, however remote. First we must voice a hearty denial. Then we should ostracize the culprit and hold up for years any publication by the use of our well-practiced referees. [28 p.30]

It might appear that Wilson was just trying to be funny, and that his arguments do not have a remote possibility of being true. I have learned over the years that many of the more prominent politicians in physics love to clothe serious arguments with humor. Wilson is well aware of the fact that APS editors go out of their way to censor controversial material that could damage the status and careers of the established politicians, such as himself. To demonstrate Wilson's awareness and hypocrisy on this question, I would like to quote from a letter I published in the journal SCIENTIFIC ETHICS entitled SCIENTIFIC FREEDOM:

> I attended the American Physical Society Council meeting at the 1985 Spring APS

meeting in Washington,D.C. The only real debate that took place during the meeting was over the motion to set up a million dollar contingency fund from the profits derived from library subscriptions to the Physical Review Journals. The point was that there was no real problem raising large amounts of money. Toward the end of the meeting, the President, Robert R. Wilson, expressed concern over the problem of government censorship of publication and presentation of papers at meetings. [29] The current increase in censorship dealt mainly with various aspects of lasers, [30] which apply to "Star Wars" research. [31] Wilson proposed the idea that he could write letters to the concerned government officials stating the APS Council's resolution that "Affirms its support of unfettered communication at the Society's sponsored meetings or in its sponsored journals of all scientific ideas and knowledge that are not classified."

I stated that it would be hypocrisy for him to send such a letter since the Council does not practice what it preaches. The Society's PR journals openly censor publication of papers based on the philosophical prejudice of editors and anonymous referees. Wilson dryly remarked that, "You have made your point!" [32]

The point being that I had used the same argument in the following letter published in Physics Today:

Scientific freedom

I would like to comment on Robert Marshak's editorial "The peril of curbing scientific freedom" (January, page 192). At an APS symposium in Washington, D.C., in 1982, our Executive Secretary William Havens gave an invited paper whose arguments were similar to those presented in Marshak's editorial. In answer to my comments, which concerned the inconsistency of his arguments in view of the fact that the Physical Review journals used a policy of censorship similar to that proposed by the government, Havens agreed with the argument that there is no such thing as an objective physicist, but defended the Physical Review policy on the grounds that it saves paper and people are free to start their own physics journal. I suspect that the government officials concerned with creating the new censorship policy who attended the symposium probably felt that national security is a better reason for censorship than saving paper, and, after all, anyone is free to move to a different country.

The APS Council has approved a POPA resolution on open communication (January, page 99). The resolution states that the Council "Affirms its support of the unfettered communication at the Society's sponsored meetings or in its sponsored journals of all scientific ideas and knowledge that are not classified." The policy of unfettered communication at APS-sponsored meetings is an established practice, but it has not been the policy of the APS Physical Review journals. A Physical Review Letters editor has arbitrarily rejected a current paper I submitted without sending it to a referee. I suspect the true reason for the rejection was the fact that I had the audacity to publish a letter in PHYSICS TODAY that was critical of the journal's editorial policy (January 1983, page 11). If the Council follows up on its resolution by adopting a policy of

> allowing APS members the right to publish in the Physical Review journals, the concerned government officials will see that the resolution is more than hypocritical rhetoric, and may see the wisdom of adopting a similar policy! [33]

Despite the hypocrisy, Wilson published an editorial titled "A threat to scientific communication" in the July 1985 issue of Physics Today that includes the following:

> Membership in The American Physical Society is open to scientists of all nations, and the benefits of Society membership are available equally to all members. The position of The American Physical Society is clear. Submission of any material to APS for presentation or publication makes it available for general dissemination. So that there could be no doubt as to where our Society stands on the question of open scientific communication, the Council adopted a resolution on 20 November 1983 that concludes:
>
> > Be it therefore resolved that The American Physical Society through its elected Council affirms its support of the unfettered communication at the Society's sponsored meetings or in its sponsored journals of all scientific ideas and knowledge that are not classified. [34]

A few months after the publication of my above "Scientific freedom" letter that tended to show the APS Executive Secretary in a bad light, the editor resigned! He was well known for his editorials on just about every subject of interest to modern physics, yet he wrote nothing about his intention to resign or his long tenure as editor. The only mention of his resignation was the following short notice:

> Search committee established for Physics Today editor
>
> At the end of 1984, the tenure of Harold L. Davis as editor of PHYSICS TODAY came to an end. He has left the American Institute of Physics to pursue other interests. AIP director H. William Koch noted that during Davis's 15-year stint as editor, PHYSICS TODAY became an important vehicle for communication among physicists and astronomers and reached a larger public as well. The magazine, he said, has earned its reputation as authoritative, accurate and responsive to the needs of the science community it serves. [35]

Since then, I've been unable to publish any further letters in Physics Today, no matter how important the subject. For example, I made the startling discovery that the NASA Jet Propulsion Laboratory was basing their analysis of signal transit time in the solar system on Newtonian Galilean $c+v$, and not c as predicted by Einstein's relativity theory. There is a short mention of the major term in the equation as the "Newtonian light time" but no emphasis on the enormous implications of this fact! I tried to force this issue out into the open by submitting a letter to Physics Today 9 July 1984, with the cover letter to the editor indicating that I had sent a carbon copy to Moyer at JPL for his comment on the matter. The following is the text of the letter I submitted:

The speed of light is c+v

During a current literature search, I requested and received a reprint of a paper [36] published by Theodore D. Moyer of the Jet Propulsion Laboratory. The paper reports the methods used to obtain accurate values of range observables for radio and radar signals in the solar system. The paper's (A6) equation and the accompanying information that calls for evaluating the position vectors at the signal reception time is nearly equivalent to the Galilean *c+v* equation (2) in my paper RADAR TESTING OF THE RELATIVE VELOCITY OF LIGHT IN SPACE. [18] The additional terms in the (A6) equation correct for the effects of the troposphere and charged particles, as well as the general relativity effects of gravity and velocity time dilation. The fact that the radio astronomers have been reluctant to acknowledge the full theoretical implications of their work is probably related to the unfortunate things that tend to happen to physicists that are rash enough to challenge Einstein's sacred second postulate. [22] Over twenty-three years have gone by since the original Venus radar experiments clearly showed that the speed of light in space was not constant, and still the average scientist is not aware of this fact! This demonstrates why it is important for the APS to bring true scientific freedom to the PR journal's editorial policy. [33]

I received a reply 4 January 1985, from Gloria B. Lubkin, the Acting Editor following the Davis resignation, in which she said they reviewed my letter to the editor and have decided against publication. Since that time I've had two more rejections. On 14 January 1988 I submitted the following letter that contained important published confirmation of my *c+v* analysis from a Russian using analysis of double stars:

Relativity debate continues

In a letter in the August 1981 issue (page 11) I presented the argument that my analysis of the published 1961 radar contact with Venus data showed that the speed of light in space was relativistic in the *c+v* Galilean sense. On 17 October 1987 I received a registered letter from Vladimir I. Sekerin of the USSR. The translation of the letter by Drs. William & Vivian Parsons of Eckerd College states:

"To me are known several of your works, including the work on the radar location of Venus. Just as you do, I also compute that the speed of light in a vacuum from a moving source is equal to *c+v*.

I am sending you my article "Gnosiological Peculiarities in the Interpretation of Observations (For example the Observation of Double Stars)", in which is cited still one more demonstration of this proposition. It is possible that this work will be of interest to certain astrophysicists in your country."

On 13 January 1988 I received a final translation of the paper which was published in the Number IV 1987 issue of CONTEMPORARY SCIENCE AND REGULARITY ITS DEVELOPMENT from Robert S. Fritzius. The ABSTRACT states:

> "de-Sitter failed disprove Ritz's C+V ballistic hypothesis regarding the speed of light. C +V effects may explain certain periodic intensity variations associated with visual and spectroscopic double stars."

Since I realized that there was little chance that Physics Today would publish the letter, after the passage of about 3 months, I submitted a similar letter to the journal Sky & Telescope. Within 2 days of mailing the letter, I received a reply from the Associate Editor Dr. Richard Tresch Fienberg, in which he stated that if a research result as unusual as this is being confirmed by Soviet scientists, then the appropriate department of SKY & TELESCOPE for the announcement is News Notes, not Letters. Accordingly, he wanted me to send him copies of my original paper and the English translation of the new Soviet work. I sent the requested material, and within several weeks received a letter from him saying that they have decided not to review my papers on the relative velocity of light in their News Notes department at this time. Dr. Fienberg was a co-author of a recent paper published in the journal that states that their Big Bang arguments are based on Einstein's general theory of relativity! [146]

Since Einstein's theories and his status as a scientist are at the core of the problem of modern physics being an elaborate farce, I will quote from various statements he has made with regard to the issues that have been raised. In a June 1912 letter to Zangger he asked the question:

> What do the colleagues say about giving up the principle of the constancy of the velocity of light? [37 p.211]

With reference to the question of double stars presenting evidence against his relativity theory, he wrote the Berlin University Observatory astronomer Erwin Finlay-Freundlich the following:

> "I am very curious about the results of your research...," he wrote to Freundlich in 1913. "If the speed of light is the least bit affected by the speed of the light source, then my whole theory of relativity and theory of gravity is false." [38 p.207]

In a 1921 letter concerning a complex repetition of the Michelson-Morley experiment by Dayton Miller of the Mount Wilson Observatory, he wrote:

> "I believe that I have really found the relationship between gravitation and electricity, assuming that the Miller experiments are based on a fundamental error," he said. "Otherwise the whole relativity theory collapses like a house of cards." Other scientists, to whom Miller announced his results at a special meeting, lacked Einstein's qualifications. "Not one of them thought for a moment of abandoning relativity," Michael Polanyi has commented. "Instead as Sir Charles Darwin once described it they sent Miller home to get his results right." [38 p.400]

With regard to the question of scientific objectivity he states:

> The belief in an external world independent of the perceiving subject is the basis of all

> natural science. Since, however, sense perception only gives information of this external world or of "physical reality" indirectly, we can only grasp the latter by speculative means. It follows from this that our notions of physical reality can never be final. We must always be ready to change these notions that is to say, the axiomatic basis of physics in order to do justice to perceived facts in the most perfect way logically. Actually a glance at the development of physics shows that it has undergone far-reaching changes in the course of time. [39 p.266]

With respect to his own status he argues:

> The cult of individuals is always, in my view, unjustified. To be sure, nature distributes her gifts unevenly among her children. But there are plenty of the well-endowed, thank God, and I am firmly convinced that most of them live quiet, unobtrusive lives. It strikes me as unfair, and even in bad taste, to select a few of them for boundless admiration, attributing superhuman powers of mind and character to them. This has been my fate, and the contrast between the popular estimate of my powers and achievements and the reality is simply grotesque. [39 p.4]

In an expansion of this argument, he states:

> My political ideal is democracy. Let every man be respected as an individual and no man idolized. It is an irony of fate that I myself have been the recipient of excessive admiration and reverence from my fellow-beings, through no fault, and no merit, of my own. The cause of this may well be the desire, unattainable for many, to understand the few ideas to which I have with my feeble powers attained through ceaseless struggle. I am quite aware that it is necessary for the achievement of the objective of an organization that one man should do the thinking and directing and generally bear the responsibility. But the led must not be coerced, they must be able to choose their leader. An autocratic system of coercion, in my opinion, soon degenerates. For force always attracts men of low morality, and I believe it to be an invariable rule that tyrants of genius are succeeded by scoundrels. [39 p.9]

On the question of scientific communication, he states:

> For scientific endeavor is a natural whole, the parts of which mutually support one another in a way which, to be sure, no one can anticipate. However, the progress of science presupposes the possibility of unrestricted communication of all results and judgments freedom of expression and instruction in all realms of intellectual endeavor. By freedom I understand social conditions of such a kind that the expression of opinions and assertions about general and particular matters of knowledge will not involve dangers or serious disadvantages for him who expresses them. This freedom of communication is indispensable for the development and extension of scientific knowledge, a consideration of much practical import. [39 p.31]

With regard to Einstein's opinion on peer review of scientific papers:

> In the course of working on this last problem, Einstein believed for some time that he had shown that the rigorous relativistic field equations do not allow for the existence of gravitational waves. After he found the mistake in the argument, the final manuscript was prepared and sent to the *Physical Review*. It was returned to him accompanied by a lengthy referee report in which clarifications were requested. Einstein was enraged and wrote to the editor that he objected to his paper being shown to colleagues prior to publication. The editor courteously replied that refereeing was a procedure generally applied to all papers submitted to his journal, adding that he regretted that Einstein may not have been aware of this custom. Einstein sent the paper to the *Journal of the Franklin Institute* and, apart from one brief note of rebuttal, never published in the *Physical Review* again. [37 p.494]

On the question of peer review, I would like to make some comments with regard to the article APS ESTABLISHES GUIDELINES FOR PROFESSIONAL CONDUCT that was published in the journal PHYSICS TODAY. [137] My first comment on the American Physical Society guidelines concerns the fact that the C. Peer Review section tends to contradict the intent of the guidelines on ethics. In the second paragraph of the section we find the sentence:

> Peer review can serve its intended function only if the members of the scientific community are prepared to provide thorough, fair, and objective evaluations based on requisite expertise.

With reference to this point, as shown by my quotation of my published letter, [33] the former APS Executive Secretary William Havens agreed with the argument that there is no such thing as an objective physicist, but defended the Physical Review policy on the grounds that it saves paper and people are free to start their own physics journal. I would like to point out the obvious fact that if there is no such thing as an objective physicist, it follows that there is no such thing as an objective peer review of a physics paper! While it may be true that the APS *Physical Review* policy saves paper for the journal, people are free to start their own physics journals, and many of them have done so. The result has created a crisis situation, not only for physics, but for the rest of science as well. An illustration of this problem is an article published in the New York Times newspaper by William J. Broad titled Science publishers have created a monster, the article was reprinted on page 1D of the February 20, 1988 edition of my local St. Petersburg Times newspaper. The article starts:

> The number of scientific articles and journals being published around the world has grown so large that it is starting to confuse researchers, overwhelm the quality-control systems of science, encourage fraud and distort the dissemination of important findings.
>
> At least 40,000 scientific journals are estimated to roll off presses around the world, flooding libraries and laboratories with more than a million new articles each year.

An abstract of some statements taken from the rather large article are as follows:

> ..."The modern scientist sometimes feels overwhelmed by the size and growth rate of

> the technical literature," said Michael J. Mahoney, a professor of education at the University of California at Santa Barbara who has written about the journal glut... Belver C. Griffith, a professor of library and information science at Drexel University in Philadelphia, said: "People had expected the exponential growth to slow down. The rather startling thing is that it seems to keep rising..."But experts say at least part of it is symptomatic of fundamental ills, including the emergence of a publish-or-perish ethic among researchers that encourages shoddy, repetitive, useless or even fraudulent work... Surveys have shown that the majority of scientific articles go virtually unread...It said useless journals stocked by university libraries were adding to the sky-rocketing cost of college education and proposed that "periodicals go first" in a bout of "book burning."... An added factor is that new technology is lowering age-old barriers to science publication, said Katherine S. Chiang, chairman of the science and technology section of the American Library Association and a librarian at Cornell University... Researchers know that having many articles on a bibliography helps them win employment, promotions and federal grants. But the publish-or-perish imperative gives rise to such practices as undertaking trivial studies because they yield rapid results, and needlessly reporting the same study in installments, magnifying the apparent scientific output...In some cases, authors pad their academic bibliographies by submitting the same paper simultaneously to two or more journals, getting multiple credit for the same work...A final factor is the growth of research "factories," where large teams of researchers churn out paper after paper...

An article titled Peer Review Under Fire states the following:

> ...Despite its crucial role in the era of "publish or perish," scientific peer review today limps along with its own disabling wounds, asserts Domenic V. Cicchetti a psychologist with the Veterans Administration Medical Center in West Haven, Conn. In his comparative review of peer-review studies conducted over the past 20 years by various researchers, Cicchetti finds consistently low agreement among referees about the quality of manuscript submissions and grant proposals in psychology, sociology, medicine and physics...The belief that basic research deserves generous funding because new understanding springs from unexpected, serendipitous sources a cherished argument in scientific circles implies that no one can accurately forecast which work most needs financing and publication, points out J. Barnard Gilmore, a psychologist at the University of Toronto in Ontario...Gilmore envisions a future in which journal and grant submissions reach a far-flung jury of scientific peers through computerized electronic mail. Rather than jostling for space in prestigious journals, authors would vie for the attention of prestigious reviewers and other readers who subscribe to the electronic peer network. Reviewer's computerized suggestions and ratings would determine a submission's funding or publication destiny...[138]

I believe that Gilmore's idea holds the key to the resolution of the problem of scientific communication, except it would be far more effective to have a hard copy paper journal that would be a permanent archival record of the democratic debate of the far- flung scientific peers. The computer far from being the cure, is actually the major source of the problem. A word processing program on a computer is a creative writing tool that makes it possible to create a vast array of different very involved abstract hard to understand articles using the same data base. This business of acquiring status by publishing in a prestigious journal after a peer-review is the core element of the problem. If one acquired status by obtaining a large positive vote from one's peers, one would try to write easy to understand comprehensive articles with significant results and arguments, thereby diminishing the size and cost of the scientific literature.

My second comment is based on the following paragraph that starts the D. Conflict of Interest section of the APS article:

> There are many professional activities of physicists that have the potential for a conflict of interest. Any professional relationship or action that may result in a conflict of interest must be fully disclosed. When objectivity and effectiveness cannot be maintained, the activity should be avoided or discontinued.

On page 1337 of a December 19, 1980 news article published in SCIENCE you will find the following statements:

> It was quite an admission, but there it was in a December 1979 editorial in the *Physical Review Letters* (PRL), the favorite publishing place of American physicists: "...if two-thirds of the papers we accept were replaced by two-thirds of the papers we reject, the quality of the journal would not be changed."...The fact that only 45 percent of the papers submitted to PRL were accepted for publication helped the journal gain an unintended measure of prestige. In the end, the prestige associated with being published in PRL outweighed the original criteria of timeliness and being of broad interest...

Peer review in like communism, it sounds good in theory, but because of human nature, does not work very well in actual practice. If the APS Council is serious about scientific ethics, they would eliminate the section of on peer review, and do their best to wean physicists away from this destructive practice in the PR journals. Perhaps they could publish versions of the journal where the authors would be completely responsible for the content of their papers. The journal could reduce costs and response time by having the authors submit camera ready manuscripts that could be reduced to 1/4 size, and there would be no reprints, but anyone, including the author, would have the right to make as many copies as they wanted. I suspect that such a journal would flourish, and even replace many of the so-called prestigious journals. I would not be surprised to find its format copied by many of the remaining journals, and that this new trend would help resolve the current scientific communication and ethics problems.

There seems to be a growing willingness of US newspapers to print articles critical of relativity theory. For example, I came across an article in the 3/10/91 edition of my local newspaper that was reprinted from The New York Times. The title of the article was Einstein's theory flawed? and the article starts with:

> A supercomputer at Cornell University, simulating a tremendous gravitational collapse in the universe, has startled and confounded astrophysicists by producing results that

> should not be possible according to Einstein's general theory of relativity...

In the body of the article Prof. Wheeler was mentioned as follows:

> Dr. John A. Wheeler, an emeritus professor of physics at Princeton University and an originator of the concept of black holes, said: "To me, the formation of a naked singularity is equivalent to jumping across the Gulf of Mexico. I would be willing to bet a million dollars that it can't be done. But I can't prove that it can't be done."

In a 5/22/91 telephone call from Robert Fritzius, the man I mentioned in Chapter 6, who accompanied me to the 1st Leningrad Conference, he said that he had sent a reprint of his recently published paper [142] to Prof. Wheeler, and that Wheeler had sent back a very nice reply. The title of the paper was The Ritz- Einstein Agreement to Disagree and mainly concerned the 1908 to 1909 battle between Ritz and Einstein that ended with a joint paper. [143] In the 5. CONCLUSIONS Robert states:

> ...The current paradigm says that Einstein prevailed, but many of us never heard of the battle, nor of Ritz's electrodynamics. So if an earlier court gave the decision to Einstein, it did so by default. Ritz, at age 31, died 7 July 1909, two months after the joint paper was published.

An extremely interesting part of the paper was the 4. SECOND THOUGHTS? section where Robert writes:

> Einstein, in later years, may have had second thoughts about irreversibility, but because of his revered position with respect to the geometrodynamic paradigm was probably prevented from expressing them publicly. We do have three glimpses into his private leanings on the subject. In 1941 he called Wheeler and Feyman's attention to Ritz's (1908) and Tetrode's (1921) time asymmetric electrodynamic theories. [This was while Wheeler and Feynman were laying the groundwork for their less than successful (1945) time-symmetric *absorber theory*, [144] which was really *emission/absorber* theory, with a lot of help from the future. They could not embrace time asymmetry, but Gill [145] now proposes to revitalize absorber theory by creating a generalized version *without* advanced interactions.] Two pieces of Einstein's private correspondence touch indirectly on the subject of time asymmetry. [37 p.467] In these letters Einstein expresses his growing doubts about the validity of the field theory space continuum hypothesis and all that goes with it.

To understand the nature of the problem you need to understand 20th century science as it really is, and not what it pretends to be. An excellent article on this was published in Science by Prof. Alan Lightman and Dr. Owen Gingerich. In the Discussion section of the paper we find the following paragraph:

> Science is a conservative activity, and scientist are reluctant to change their explanatory frameworks. As discussed by sociologist Bernard Barber, there are a

> variety of social and cultural factors that lead to conservatism in science, including commitment to particular physical concepts, commitment to particular methodological conceptions, professional standing, and investment in particular scientific organizations. [147]

Dr. Chet Raymo, a physics professor at Stonehill College in Massachusetts, and the author of a weekly science column in the newspaper the *Boston Globe*, in a FOCAL POINT article published in Sky & Telescope, expands on the above paper with the following arguments:

> Science has evolved an elaborate system of social organization, communication, and peer review to ensure a high degree of conformity with existing orthodoxy...
>
> In a recent article titled "When Do Anomalies Begin?" (*Science*, February 7th), Alan Lightman of MIT and Owen Gingerich of the Harvard-Smithsonian Center for Astrophysics describe the conservation of science. They acknowledge that scientist may be reluctant to face change for the purely psychological reason that the familiar is more comfortable than the unfamiliar...
>
> Usually, say Lightman and Gingerich, such anomalies are recognized only in retrospect. Only when a new theory gives a compelling explanation of previously unexplained facts does it become "safe" to recognize anomalies for what they are. In the meantime scientist often simply ignore what doesn't fit...
>
> For some people outside mainstream science, the path toward truth seems frustratingly strewn with obstacles. Like everyone else, scientist can be arrogant and closed-minded... [148]

The editor of the American Physical Society journal PHYSICS AND SOCIETY, Prof. Art Hobson, wrote an editorial titled Redefining Physics, and it starts as follows:

> My friend Greg burst into my office the other day shaking his head and asking "What are physicist good for, Hobson? Why would anybody want to hire one? What is special about physics?" He complained that PhD programs prepare graduates who do things that only physicists care about, graduates who settle into other departments where they prepare other students to do the same thing. How can we change the barely self-perpetuating system? Even relatively small reforms, such as the Introductory University Physics Project's recommendations for bringing introductory physics into the twentieth century (let alone the twenty-first), are difficult. The system has great inertia.
>
> Greg is a successful quantum optics experimentalist. He loves physics. He is one of our department's best teachers. Despite having every reason to feel good about the future of physics, he doesn't. He is not an isolated case. Judging from recent surveys conducted by Leon Lederman and others, evidence of low morale in the entire scientific community has been building lately.

Within the body of the editorial, Prof. Hobson writes:

> Congressman George Brown, Chair of the House science and technology committee and one of science's best friends in Congress, has recently written on these matters. Excerpts from one of his articles are reprinted above. His strong words are worthy of our attention. [149]

Some of the more interesting excerpts from one of Congressman Brown's articles are as follows:

> For the past 50 years, U.S. government support for basic research has reflected a widespread but weakly held sentiment that the pursuit of knowledge is a cultural activity intrinsically worthy of public support... ...Lobbyists for the scientific community have been perhaps excessively willing to bolster this rhetoric by claiming for basic research an exaggerated role in economic growth... ...In fact, there are many tangible and intangible indicators of a decline in the standard of living in the United States today, despite 50 years of increasing government support for research...
>
> ...In the absence of pluralistic democratic institutions, science and technology can promote concentration of power and wealth and even autocratic and dictatorial conditions of many kinds. An excessive cultural reverence for the objective lessons of science has the effect of stifling political discourse, which is necessarily subjective and value-laden. President Eisenhower recognized this danger when he stated that "In holding scientific research and discovery in respect, as we should, we must also be alert to the equal and opposite danger that public policy could itself become the captive of a scientific-technological elite."...
>
> The fundamental challenge for all of us is not to increase funding for research, it is to enhance the societal conditions that permit research to thrive: educational and economic opportunity, freedom of intellectual discourse, and an increased capacity for all human beings to achieve their individual potential within a just and humane global society. [150]

BACK CONTENTS NEXT

Light Lunacy

At first I did not realize the military implications of realistic knowledge of the nature of the relative velocity of light in space. The article that opened my eyes on this matter was titled "The Search for a Nuclear Sanctuary (II)", and it was published in the journal Science in 1983. [58] The following quotations are from that article:

> Buried inside the Defense Department's bureaucracy is a small, well-run program of enormous significance in the ongoing debate over whether or not the United States should construct a large-scale antiballistic missile system, as President Reagan proposed in his widely publicized "Star Wars" speech last March. It is known as the Advanced Strategic Missile System (ASMS) program, and almost everything that falls under its jurisdiction is considered secret...
>
> For roughly two decades, the technical managers of ASMS and its bureaucratic antecedents have analyzed potential Soviet strategic defenses and devised the means to defeat them...
>
> ASMS, along with several newer Pentagon programs aimed specifically at countering potential Soviet space-based laser systems, will have a significant impact on the strategic balance in the event that the United States proceeds with Reagan's plan to "counter the awesome Soviet missile threat with measures that are defensive."... The Air Force, which directs the ASMS program, does not like to crow about the program's technological successes, preferring that the Soviets, and perhaps the general public, be kept in the dark about what is obviously one of its most sensitive scientific endeavors...
>
> The active decoy is a product of substantial wizardry in microelectronics and computing, engineered by MIT's Lincoln Laboratories and by the General Electric Company...

In 1968, Dr. Thornton Page, a prominent astrophysicist, reviewed my original $c+v$ analysis paper titled INCONSISTENCIES IN RADAR DISTANCES TO VENUS. At that time, Page was Director of the Van Vleck Observatory, Chairman of the Astronomy Department of Wesleyan University, Associate Director of the Smithsonian Astrophysical Observatory, Vice President for Astronomy of the American Association for the Advancement of Science, and an Associate Scientist with NASA. He chopped the paper down to at least half its original size, making many significant changes. Page also helped me to present arguments with regard to the work to many prominent scientists he introduced me to at the Fourth Texas Symposium on Relativistic Astrophysics that was held in Dallas Texas in 1968. He concentrated mainly on radio astronomers and had advised me not to answer questions in the conclusive sense but always in the possible sense. A fair number of the scientist asked for preprints of the paper.

On the fourth day of the Symposium, Dr. Irwin I. Shapiro presented a talk titled OBSERVATIONAL TESTS OF RELATIVITY. Shapiro was the principal investigator for the above mentioned Massachusetts Institute of Technology Lincoln Laboratory's analysis of the interplanetary radar data that came from radar stations scattered throughout the world, and his research was funded by the Air Force. In the talk, Shapiro presented the illusion that the radar data was consistent with Einstein's general relativity theory. The talk was essentially the same as the paper titled "Radar Observations of the Planets" which he had published in the prestigious journal Scientific American. [59] In my debate with Shapiro, in the comment session that followed his talk, he admitted that all his calculations were based on a constant speed of light *c* (the wave in ether model), and he had not tested *c+v* (the particle model). He did this, in spite of the fact, that the major problem in modern physics, is the wave-particle paradox. That is, in some experiments light seems to behave like a wave, and in other experiments it seems to behave as a particle. He admitted the fact that the published radar analysis showed very large impossible variations in the calculated value of the astronomical unit (the mean distance between the earth and the sun), that were far larger than their maximum estimate of all possible errors. The graphed calculated values of the astronomical unit contained a daily component that was proportional to the relative velocity due to the Earth's rotation, a 30-day component, related to the Earth-Moon rotation, and a component related to the relative solar orbital velocities of the Earth and Venus. [60] The variations in the calculated value of the astronomical unit are what one would expect to find if the speed of light was *c+v*, and the calculations were based on *c*. The astronomical unit is the basic unit of measurement used by astronomers for the solar system. The telescopic methods used to determine its value, had an uncertainty of as much as 170,000 miles (273,589 kilometers), due to the fact that until the interplanetary radar observations became technologically possible, the only way to measure distances was by the indirect method of analysis of the angular positions of bodies in the solar system. [61] The radar observations were estimated to be capable of measuring the distance to Venus with an accuracy of within 1.5 kilometers, the only important variable being the relative velocity of light in space. The Earth's rotation could cause a maximum difference in calculated distance between the two theories of 260 kilometers when two radar stations, one on either side of the Earth, observe Venus at the same time when the planet is at its closest point to the Earth. This difference would increase as the distance between the Earth and Venus increased. An analysis of the data based on the incorrect theory would show the center of Venus to be at different distances from the center of the Earth at the same time. The analysis of the data published by Shapiro's research group also presents evidence against the *c* theory from observations made at the same time from different points on the Earth. The Lincoln Laboratory made a complete *c* analysis of all the radar data up to 1966. The Einstein general relativity time delay goodness-of-fit for the US Massachusetts radar station was 1.57, the value for the Puerto Rico station was .97, the value for the USSR Crimean station was 7.10. The article [62] states:

> Although not apparent from inspection of Fig. 4, the residuals of the U.S.S.R. time-delay are systematically negative relative to the Arecibo and Lincoln Laboratory residuals during the time period (June 1964) when all three groups were observing Venus. This incompatibility cannot be removed by assuming simply that different units of time were used by the different observatories.

In his chapter of the book [63] "Radar Astronomy", Shapiro states:

> If the theory is wrong, the values of the parameters will selected from the data in a manner that tends to cover up the inadequacies of the theory (for example, if least-mean-square fitting is employed).

I told Shapiro that my analysis of the published 1961 Venus radar data [18] showed a much better fit to the Newtonian particle $c+v$ model for light than for the Einstein wave c model. I stated that my analysis would have been far more impressive if I had more than the sparse set of data that was published. Shapiro made no effort to challenge any of my arguments, and promised to send me any data I would require to make a more in depth analysis of the relative velocity of light in space. Thornton Page was furious over the corrosive nature of my arguments and the tone of voice that I had used, and let me know about it in no uncertain terms! From that point on, Page has not given me any further assistance in my efforts to bring scientific objectivity and integrity to the question of the relative velocity of light in space. The loss of Page's support has proven to be a devastating blow to this cause. For example, Walter Sullivan, the science editor of The New York Times, was at the Symposium and had shown an interest in publishing an article on my results. In a short letter sent 13 March 1969, he thanked me for sending him copies of my exchanges with Shapiro. He stated he was far from being qualified to assess the merit of my case and would have to depend on old friends who are including Thornton Page. Needless to say, Sullivan never wrote the article. To show the impact that this article could have had, I would like to quote from Michael Riordan's recent book [64 p.180] "The Hunting of the Quark":

> One might question all this concern over a mere newspaper article, but The New York Times, as the nation's foremost daily, informs scientists in other fields and especially Washington policymakers about new discoveries. In a science so dependent upon government money for its continued progress, Sullivan's front-page article was a valuable trump card in the annual budget scramble.

With regard to the correspondence with Shapiro mentioned by Sullivan, my first letter of 26 December 1968 states:

> Enclosed you will find a copy of my paper "Inconsistencies In Radar Distances To Venus" that I promised to send you. Dr. Heinrich K. Eichhorn checked the calculations and Dr. Thornton L. Page suggested how to write it and reviewed and edited it. I have sent copies to most of the research centers and observatories in the U.S. as well as a few other countries. Enclosed you will also find a small sample of the answers I have received.

In Shapiro's answer of 13 January 1969, he thanked me for sending him a preprint of my paper, and said he found himself in agreement with the comments of Prof. Dingle. The Prof. Dingle Shapiro spoke of, was the Herbert Dingle I had mentioned earlier, who had published the article in Nature concerning the inconsistent logic in Einstein's Special Relativity theory. [47] With regard to the problem of Dingle's understanding the interplanetary radar paper, Dingle wrote in his letter of 16 August 1968 that he agreed that Dr. Page (whom he knew) has condensed the account too much at any rate for the understanding of those who are not primarily dynamical astronomers but are concerned with that subject in relation to their own interests. In Shapiro's letter of 17 January, he states that the radar data are not consistent with the "ballistic theory" of light, but are consistent with general relativity (cf. their article on the verification of the second-order Doppler shift in Phys. Rev. Letters circa. October 1966).

An interesting side note is the fact that Dr. Svetlana A. Tolchelnikova-Murri, a professional Russian astronomer and mathematician working at the Pulkovo Observatory, has published a paper titled The Doppler Observations of Venus Contradict the STR in the US journal GALILEAN ELECTRODYNAMICS. [151] Dr. Tolchelnikova delivered a Russian version of that paper at a 1991 Conference that I talk of in the next chapter. In my answer to Shapiro of 23 January, I wrote:

> With reference to your letter of January 17, I read the article you referred me to. You should know by now you can't bluff me. The article does not support your argument and you know it.
>
> You admitted at the symposium in front of 500 scientists that all your calculations were based on c. How can you state that the radar data is not consistent with the "ballistic theory"? Prove it, and then publish it. Considering the capabilities of the Lab. and the importance of the question, this is the most responsible thing you can do.
>
> You state the radar data is consistent with general relativity, yet when the observing time is varied you get variations that are far larger then the maximum possible and the variations are proportional to the change in the observing time. The variations disappear when the observing time is held constant but variations between radar stations that are proportional to the distance exist. These facts are documented by articles published by your group in "The Astronomical Journal."
>
> You lost the fight when you did not challenge me at the symposium. This fact has made a impression on a large number of scientists. Your only hope is to finish a half-finished job, and make a complete and fair analysis of the radar data based on the ballistic model before someone else does.
>
> I brought the matter out in the open, now you must decide to sink or swim. Good luck!

In my 13 February letter to Shapiro, I wrote:

> Dr. Wilbur Block is a radio astronomer who is doing research on radio radiations from Jupiter. He has collaborated with others in publishing a number of papers on this subject. He was the one who invited me to give a lecture at his college on radar testing of Special Relativity.
>
> I told Block that you had promised to send me all the data I needed. He wants to do research on this himself. If he limits his analysis to a test of relativity and does not get involved in a deep analysis of Venus' orbit, he will probably be the first one to publish verification of my work.

> He would like the data from about five consecutive days during inferior conjunction. He needs transit time and Doppler shift from three observations each day, one at the earliest time, one at 12:00 UT and one at the latest time, all from your station. The larger the difference between observing times, the more dramatic will be the results. He would also like the geographic location and data for 12:00 UT for the same five days from the U.S.S.R. station.
>
> Please send it as soon as possible. From the way the other radio men were talking at the symposium he may not have much time.
>
> Here is his address:...

In Shapiro's letter of 27 March, he wrote that unfortunately the data did not exist in the form in which I wanted them and, hence, he could not honor my request. In my reply of 3 April, I wrote:

> You promised to send me all the data I would need, yet when I requested a limited amount, you ignored the request in two letters and offer an excuse for not doing so when I make an issue of it.
>
> I have quoted your remarks as I remembered them. The main reason that your newer results appear to look better, is that your group found it could eliminate the large daily variations by changing to a constant observing time (12:00 UT), even when the planet was not observed or in some cases was not even visible.(J. V. Evans, etal., Astron. J. 70, 486 - 1965) Of course there is a second-order difference in the Doppler formulas between c and $c+v$, but it is obviously irresponsible to state that a solution based on c that is valid only for a constant observing time and a single radar station, proves that the velocity is c.
>
> I will tell you what more one could ask. One could ask for a complete and honest evaluation of the data based on c and $c+v$. Then one would have sufficient information to make a valid and intelligent comparison of c and $c+v$.

Since the Department of Defense had funded the research, I wrote a letter to Dr. John Foster, Jr. the Director of Defense Research and Engineering, requesting the data. In reply to the request, I received a letter dated 29 September from Dr. Lowell M. Hollingsworth, Technical Advisor for Electronics, Department of the Air Force, Headquarters Air Force Cambridge Research Laboratories. Hollingsworth wrote that he spoke with Dr. Shapiro regarding the data requested by me on radar observations of Venus. The data in the form I requested did not exist. However, if the data in the form in which it did exist would be of value to me a deck of IBM cards could be prepared from which I could by analysis obtain the data I desired. This deck would be a stack of IBM cards totalling an inch or so in thickness. For machine computation the data resides in the holes punched in the cards but pertinent data would appear typed on the cards. Thus I would be able to read the cards visually for the purpose of my analysis. I wrote Hollingsworth 1 October, stating:

> With reference to your letter of September 29, please send the IBM cards containing the radar data. They will make possible a far more conclusive test of the relative velocity of light in space. I have read Fox's 1965 article and have a correspondence with him. He is enthused about the results from my radar analysis.

And I closed my letter with the sentence:

> I plan to attend the AAAS meeting in Boston in December. I would be interested in talking to you on this subject.

I wrote Shapiro 4 October stating:

> Wilbur Block has suggested to me that I offer to collaborate with you and the Lincoln Lab., in a full investigation of the relative velocity of light in space and celestial mechanics. I am willing, if you are. It is obvious that continued opposition will be mutually destructive to both of us. On the other hand, collaboration is bound to be mutually beneficial.

Shapiro's reply of 13 October asked if I would be more explicit as to exactly what form I would wish the collaboration to take? In my letter to Shapiro of 18 October, I start out with:

> Your letter of October 13 has caused me to make a fast rewrite of the lecture I am going to give October 30, at Florida Presbyterian College. I had planned to be rather hard on both you and the Lincoln Lab, but it would not be wise to try and hurt the credibility of a potential ally. The lecture will be publicized and open to the public. I am hoping for enough publicity to bring this thing out into the open.

further into the letter, I wrote:

> L. M. Hollingsworth sent the radar data I wanted and he asked me to call him when I get to Boston in December so that we can get together. I think there is a good possibility that the data will make a more impressive test of c. I will send you the results of the analysis. Perhaps you would like a joint paper on this? Both our names on the same paper would be mutually beneficial; it would tend to repair any damage I may have done to your reputation and help me by making it easier to overcome the remaining psychological barrier that exists on this question.

Shapiro's answering letter of 6 November stated that he was pleased to hear that I had received the radar data that they had sent me, and he hoped that I would find them useful. In my reply of 12 November, I stated:

> I am sorry to say that the data you sent me can't be used for a test of the relative velocity of light in space. There was no significant difference in the location of the stations or the observing time, so there would have been no significant difference between a c and *c+v* analysis.

> How about us having a private get together when I am in Boston this December?

Shapiro's answer of 28 November states that he could not understand why I concluded that the Venus data sent to me was inappropriate for my purpose. In my letter to Hollingsworth of 25 October, I wrote:

> I am afraid that Shapiro has pulled a fast one on us, the data you sent me can't be used for a test of the relative velocity of light in space. There is very little difference in the distance between the radar stations so I can't show that the false theory shows the planet Venus in different places at the same time while the true theory shows it in the same place. There is almost no difference in the observing time so I can't show that the false theory shows Venus doing a jig while the true theory shows it moving in a rational manner.
>
> Analysis of Shapiro's article in The Astronomical Journal (72, 338 - 1967) shows that the Lincoln Lab has the data that I would need. Page 343 shows that they had data from both their station and the U.S.S.R. station for June 1964 and Figure 4 on this page shows considerable daily variations for 1964 indicating data at different observing times. The only possible way they could have eliminated the synodic variation from the General Relativity Fit part of Figure 4 was by using empirical corrections similar to Duncombe's corrections. They eliminated the daily variations for later years by observing for only a short time at the same time each day. The early articles published by the Lincoln Lab group are open and above board, but the later articles are little more than misleading fabrications and I am sure that Shapiro knows this. They started out by believing that c was a proven fact so they made no attempt to treat the velocity of light as a variable. After several years of not being able to make sense out of the data, they were probably under considerable pressure. So they used empirical methods to overcome the inconsistencies they did not understand. I do not think that one should blame them. For all practical purposes Einstein's Theory is based on empirical ad hoc equations that were designed to save the ether theory from the Michelson-Morley experiment. The ballistic theory explains the results of that experiment in a simple manner without any ad hoc assumptions. They had a precedent in the fact that the Duncombe empirical corrections were already used to correct similar variations in the optical data when all the calculations were based on c. The only real difference is the higher accuracy of the radar measurements made the inconsistencies more obvious.
>
> Shapiro has already shown an interest in collaborating in a full analysis of the relative velocity of light in space. Considering the resources and capability of the MIT Lincoln Lab group, they would be the ideal ones to conduct this investigation. It would be great if it were possible for you to persuade them to do this.

In Hollingsworth's reply of 7 November, he wrote that there were a number of reasons why it is impossible for him to persuade Dr. Shapiro or other Lincoln Laboratory people to prove that the velocity of light in space can be measured as anything else than a constant value c, and that he I looked forward to talking with me when I would be in Boston that December. In my letter to Shapiro of 5 December, I wrote:

> I received another letter from Hollingsworth. He is looking forward to talking to me on this during the week I will be in Boston. I am going to try and get him to recommend to the Air Force that they finance a full *c+v* investigation. Would you be interested in heading this investigation? This sort of thing is just not my cup of tea. Of course, I would be available to help in case there were any difficulties.

Shapiro's reply of 9 December 1969 states that he was at present too much occupied with university matters to direct any large- scale investigation. When I attended the AAAS meeting in Boston that December, Hollingsworth drove through a snow storm to meet with me in my hotel room. It was a long and interesting meeting that lasted for about four hours. I found him to be far more reasonable in person than he had been in his correspondence. I had brought with me copies of all the referenced articles, as well as a copies of correspondence with scientists on this matter from around the world. Hollingsworth admitted that while the Lab's published center value for the astronomical unit had stayed virtually unchanged, the graphed individual values ranged over thousands of kilometers, and that the variations were related to the relative velocities. He also admitted that the data I wanted existed, but he refused to release it without Shapiro's permission. I now suspect that he was just giving me the run around and the real reason he would not release the data was military secrecy.

I now think that it is most probable that the Soviet military is not involved in the speed of light coverup, and that the main force behind the coverup is the US military "Star Wars" adventure. The many conversations and the evidence of text books that were little more than translations of US text books, that I saw during my visit to the USSR in 1989, seemed to show that the dominant trends of Soviet physics and astronomy, are little more than copies of their US counterparts. Then there was my conversation with the young man with the long nose and fancy suit, that came to sit beside me during the Pulkovo Observatory conference. He asked for information with regard to the articles I had published in regard to this matter. When I told him I would be glad to send him reprints, he stated that his institution had a very extensive library that contained all the western journals, and he only needed the journal names, dates, etc. He seemed genuinely concerned about the fact that he had not heard these arguments before. Svetlana told me that he worked at an small elite institution in Moscow, and that the people working there were high paid, and she did not know what work was done at the institution. I suspect that the work involves the Soviet military, and they are about to find that they had been duped with regard to this matter.

Actually it is easy to see how this was done, the (2) equation of my 1969 paper shows the radar evaluated *c+v* Newtonian distance to the planet to be $D = t((c+v)/2 - tv/2 = tc/2$ for the time the beam returns to the transmitter. The fact that the $+v$ in the first term which is related to the motion of the photon relative to the transmitter, can be canceled by the $-v$ of the second term which is related to the motion of the target relative to the transmitter, presents the illusion that the combined term of $tc/2$ is relativistic in the Einstein relativity c sense. But the true Einstein c equation for the distance to the target at the time the beam returns to the transmitter is $D = tc/2 - tv/2$, and the two equations differ in magnitude by the second term of $tv/2$. Dr. T. D. Moyer of the JPL, in his 1981 Celestial Mechanics paper [36] evaluates the distance at the time the signal returns to the transmitter, does not include the $-tv/2$ term that would make the evaluation relativistic in the Einstein c sense, renames the terms and rearranges the equations, adds the smaller corrections due to time dilation, gravity, and the troposphere and charged particles in the beam path, and correctly identifies his major term as the Newtonian light time. What Moyer does not do is clearly explain the enormous implications of his mathematics, or explain how the transit time of light signals in the solar system is the ultimate test of the Einstein wave in ether *c* model and the Newtonian particle *c+v* model of light. The fact that he does not present an analysis that compares the results of the c and *c+v* models tends to maintain the illusion that there is nothing wrong with the Einstein general relativity model! I have sent Moyer reprints of the articles I've published that present the argument that his mathematics is relativistic in the *c+v* sense, and he has not chosen to rebut this argument either by correspondence or publication. Moyer's sin is the sin of omission, he has not lied, but has simply refused to present the full truth. Of course, a half truth that presents the illusion of a lie, is for all intents and purposes, a lie.

In my 1969 paper [18] I quote Shapiro as stating "If the theory is wrong, the values of the parameters will usually be selected from the data in a manner that tends to cover up the inadequacies of the theory", so you see, even Shapiro does not state outright lies, if you carefully read everything he has published on this matter, he only presents the illusion of a lie. But the funding for all this research comes from the US Department of Defense, and they have strict control over all information that results from research that they fund. So one of the questions one could ask is it wrong for a scientist to publish the illusion of a lie to preserve a military secret? Years ago I worked on top secret defense work, and this sounds like a classic case of how the system works. All top secret information is handled on a need to know basis, it does not matter how high a position you hold, if you do not have a need to know the information in order to help you in your work, you cannot obtain access to that information. This would also tend to explain Shapiro's refusal to challenge my arguments published in journals or presented at meetings, it is a federal crime to confirm or deny top secret information, even if it is published in journals or newspapers. In an expansion of the military secret argument, a recent article [75] titled THE BIRTH OF THE LASER states on page 27:

> In July 1958 Townes applied to the Air Force Office of Scientific Research for funds to initiate work on a potassium laser at the Columbia Radiation Laboratory.

and on page 28:

> One agency TRG approached was the Advanced Projects Research Agency, which had been set up after Sputnik in the secretariat of the Department of Defense and oriented in 1959 toward exploration of innovative weapons technologies. ARPA, which had more money than it could easily spend, proved a good choice: TRG made a request for $300 000 but ARPA, which was interested, inter alia, in the possibility of beam weapons, awarded it a $999 000 contract for a secret program leading to operating lasers.

and on page 32 we find:

> The high energy density in a laser beam interested ARPA, which was then investigating every plausible scheme for anti- missile defense.

In a current article [76] titled HOW THE MILITARY RESPONDED TO THE LASER, on page 36 we find:

> "I feel as do others here that the LASER may be the biggest breakthrough in the weapons area since the atomic bomb." This statement, made in 1962 in a letter by Major General August Schomburg, head of the Army Ordnance Missile Command, reflected an attitude that was pervasive in the military in the first years after the birth of the laser.

and further on page 36 we find:

> By forcing a change from small to big science, from academic to in-house and contract laboratories and from open research to classified development, military interest in the laser transformed the nature of laser research and development.

on page 37 we find the paragraph:

> The laser offered a coherent, directed, concentrated beam of light that promised to realize an ancient dream, epitomized in Archimedes's idea to attack the Roman fleet at Syracuse by using mirrors and lenses to focus burning solar rays on ships at sea. Science fiction's preoccupation with burning "death rays" added modern sanction to the ancient dream. The Soviet Union's large boosters, which lofted Sputnik and the first cosmonauts into space and might equally well launch warheads, provided suitable targets for the rays. The promise of beam weapons enhanced the services' interest in lasers and launched a number of industry and service research programs that transcended the interest in laser ranging, communication and detection.

on page 38 we find the statement:

> "Defense at the speed of light!" became a rallying cry for the military-industrial complex.

As an example of military secrecy with regard to lasers, or anything connected to them such as the relative velocity of light in space, I present the following taken from the front of an article [135] titled "Incident over SPIE papers muddies scientific secrecy issue":

> Just when it seemed the furor over Defense Department restrictions on certain scientific papers had been quelled, the situation flared up again. The fracas this time involved an international symposium of the Society of Photo-Optical Instrumentation Engineering (SPIE) meeting in Arlington, Virginia, on 8-12 April. Two weeks before the meeting, DOD informed SPIE officials that 20% of the 219 scheduled papers could not be presented, even in a "controlled" session. Until then, SPIE organizers were so confident the reports of work done under military contracts had been cleared by the DOD that titles, and in some cases abstracts, were printed in the program. But it turned out that some authors had failed to follow all the Pentagon procedures for clearing their papers

> and others were unaware of the new authority that Congress had provided in the 1984 Defense Authorization Act to deny public access to technical data under DOD control that are judged to possess military or space applications of use to the Soviet Union and its Warsaw Pact neighbors.
>
> About 100 papers were submitted to DOD for clearance. Of these, 45 raised security problems. On closer reading, DOD reviewers decided that 16 papers, most by defense scientists, contained classified information and another 6, by scientists at the Air Force Weapons Laboratory, required special releases for foreign disclosure. What's more, presentation of the other papers at an open session, DOD officials argued, would violate US export controls.
>
> `Star Wars' connection. Most of the papers had originally been scheduled for a classified session on synthetic aperture optical systems and laser beams to be held at the Naval Research Laboratory, across the Potomac River from the main SPIE meeting. Another SPIE session on adaptive optics was set for the Corps of Engineers offices in nearby Alexandria, Virginia. It came off without a hitch. Although these fields are important to astronomy, most applications are military, dealing with high-energy laser optics and space surveillance that could benefit the Strategic Defense Initiative, more commonly known as the "Star Wars" program.

Current documentation of the nature of this coverup was published in a news article [77] titled Reactors in Space Threaten High-Energy Astronomy. In 1976 satellites started to record very-high energy radiation. Then in 1979 a NASA scientist received a call from the military saying the information was classified and not to be published. Amid growing discontent at NASA, the classification was lifted last August. It turns out that the radiation that had hampered US high-energy astronomy programs and even damaged some detectors was from the nuclear reactors powering Soviet spy satellites, and the Soviets obviously know of the problem because their current detectors will orbit far above the interference.

With regard to USSR military laser research, there is an interesting paragraph in a recent article [78] titled US PHYSICISTS PAY FIRST VISIT TO CHERNOGILOVKA SOLID STATE INSTITUTE on page 74 as follows:

> The invitation to visit the Institute for Solid State Physics came after the conference had begun in Moscow, and on 2 June the ten US participants piled into a bus and were driven by a somewhat circuitous route to a spot about 50 km east- northeast of Moscow. The standard explanation for why no previous visit by Westerners had been allowed was that Chernogolovka is situated in the first ring of ballistic- missile defenses that surround Moscow. Another explanation was that an institute doing work on high-power chemical lasers may be situated at Chernogolovka.

on page 76 is the paragraph:

> The group also was impressed, more generally, by the freedom with which their Soviet

> counterparts talked about politics and expressed critical opinions in public. "Most striking...is the new freedom of people to know and speak the truth. For us, this removes several layers of barriers in our relationship with Soviet scientists," Worlock observed.

There is a possibility that the Soviet military is aware of the exact nature of the relative velocity of light in space. The American Institute of Physics publishes translation journals of the major Soviet journals, and some of these journals are carried by the University of South Florida Library in nearby Tampa. One finds many hints to the fact that Einstein's general relativity does not give a proper explanation to the transit of light signals in the solar system. For example, in the abstract of an article [79] titled "Measurements of delay time and Doppler correction in radar observations of Venus in 1975", we find:

> It is shown that the discrepancies between the actual position of Venus and the position calculated on the basis of the existing theory of motion of the planets at different inferior conjunctions have different characters.

The concluding sentences of the article state:

> An analysis of the data presented shows that the differences between the measured and calculated delay times have different dependence on the time in the different conjunctions and reach 3500 microseconds, which when converted to the distance from the earth to Venus comprises 500 km. The presence of such errors in the prediction of the position of Venus relative to the earth on the basis of the existing theory of motion of the planets in the absence of radar measurements could hinder considerably the successful performance of the terminal stages of flights of automatic interplanetary stations to Venus, landing on its surface, and the insertion of artificial satellites of Venus.

Along with my campaign to discredit QCD that I mentioned in Chapter 2, I've also been involved in a campaign to discredit "Star Wars." As part of this effort I published the following letter [31] titled "Directed-energy weapons":

> At the 1981 APS Spring Meeting, we had a Symposium of the Forum on Physics and Society on directed-energy weapons that was filmed by the BBC for a documentary on the arms race. The first speaker, Douglas T. Tanimoto of the Defense Advanced Research Projects Agency, showed a film of a propeller-driven dark red drone plane that was destroyed by an infrared laser that focused on it for a period of several seconds as it slowly circled at a relatively close distance. I asked Tanimoto if it was not cheating to use a close, slow-moving, dark red drone to simulate a large number of fast-moving, distant, polished metal targets, and he admitted that it was cheating "a little." The last speaker, Kosta Tsipis of MIT, presented conclusive evidence that the technology needed to develop effective ABM Directed Energy Weapons did not now, and probably never would exist!

> At the 1982 Spring meeting, I attended the APS Council Meeting and learned that they intended to hold off on taking a stand on the nuclear freeze issue until after hearing the results from the Forum Symposiums on this question. One of the Forum speakers, Hans Bethe, gave a talk that showed beyond a reasonable doubt that the US was the aggressor in the nuclear arms race and that the Russians have been desperately trying to catch up! At the final Forum meeting I called for a vote on the question of a nuclear freeze, and there was almost a 100% show of hands!
>
> At the 1983 Spring meeting, George A. Keyworth II, President Reagan's science adviser and the director of the Office of Science and Technology Policy, gave a talk in which he asked for help from the physics community to develop the technology for future antiballistic missile systems. I was the first to comment on his talk, and I argued that development of these types of weapons would expand the arms race and bankrupt the economy! The loud applause in support of my argument left little question that a large majority of the physicists have had enough of the arms race and would not support the development of Reagan's "Star Wars" weapons.

In my local newspaper there was also a 2/3/91 article titled NASA scientist to speak at SPJC, and the content of the article was as follows:

> The man who headed NASA's planetary astronomy program for almost 20 years will speak on future Venus and Mars missions Wednesday at the St. Petersburg Junior College's St. Petersburg campus.
>
> Dr. William E. Brunk directed the programs from 1964 through 1982. He was also program scientist for the Voyager mission to the outer planets. He retired from NASA in 1985.
>
> Brunk will appear as a Harlow Shapely Lecturer of the American Astronomical Society. The speech, open to the public, begins at 7:30 p.m. in the Lynch Auditorium, 6605 Fifth Ave. N.

I went to Brunk's speech, and during the questions and answer period, asked Brunk the following question:

> Considering the importance of the wave-particle paradox of light in modern physics, I am surprised by the lack of scientific objectivity and integrity of the concerned NASA scientists on the question of the relative velocity of light in the solar system. My 1969 published analysis of the first published Venus radar contact data showed that the best fit to the data was for the Newtonian $c+v$ particle model, and not the Einstein general relativity c wave model. The equations in T. D. Moyer's JPL NASA 1981 Celestial Mechanics journal article were based on the $c+v$ particle model, and Moyer called the main term the Newtonian light time, yet the lack of emphasis of the importance of this fact, means that the average person does not know of the overwhelming evidence against the Einstein special and general relativity theories. What is your comment on

this matter?

Brunk's answer was that the analysis of the data and publication of the results, was the responsibility of the individual involved scientists, and that NASA was only the bus driver. I suspect that when the final history of science in the 20th century will be written, NASA's greatest blunder will be considered to be the lack of objectivity and integrity on the question of the relative velocity of light in space, and not the 1986 Challenger space shuttle explosion, or the flawed mirror on the $1.5-billion Hubble Space Telescope!

BACK CONTENTS NEXT

Relativity Revolution

There is an interesting article [80] titled SOVIET SCIENTISTS TELL IT LIKE IT IS, URGING REFORMS OF RESEARCH INSTITUTES, that starts as follows:

> A specter is haunting the Soviet Union the modernization of virtually every part of Soviet society. The Russian catchword for this is perestroika, which translates as "restructuring" or "reform." The concept has been described in recent books and statements by General Secretary Mikhail S. Gorbachev and by his favorite economist, Abel G. Aganbegyan, who is director of the economics section of the Soviet Academy of Sciences. Its implications for science and technology in the Soviet Union, observes Loren Graham, a longtime MIT history of science professor, "are as sweeping as anything undertaken by Peter the Great or Lenin. Like those historic figures, Gorbachev hungers to improve the country's science and technology. All of them realized that if significant advances weren't made, the country would be left permanently behind."
>
> Gorbachev's program, which combines perestroika with glasnost, or "openness," already has gone further than the revisions another Soviet leader, Nikita Khrushchev, tried to introduce 30 years ago.

on page 98 we find the statement:

> Accordingly, say US observers of Soviet science such as MIT's Graham, the obvious aim of perestroika is to remove the heavy hand of the bureaucratic old guard, to "democratize" the scientific establishment and to restructure basic research by strengthening a diversity of disciplines and making these more relevant to industry. This is also the message delivered in Sagdeev's essay in the current Issues in Science and Technology, a quarterly journal published by the US National Academies of Sciences and of Engineering. In it, Sagdeev calls for breaking up many of the research institutes that he labels "bureaucratic dinosaurs" into smaller, more flexible and more responsive operations, declassifying much of the research that the Kremlin still considers militarily significant and relaxing restrictions on international scientific cooperation.

and on page 99 the article closes with the following paragraph:

> Indeed, says another Carnegie Endowment senior analyst, Andrew Nagorski, Soviet science is compartmentalized, "so that military applications get first call and the civilian economy is left to rot. The Soviet Union is a military superpower, but not an

> economic superpower." It is somewhat ironic, he observes, that in order to save his science establishment, Gorbachev must first buck it.

In October of 1987, I received a registered letter from a Dr. Vladimir Ilich Sekerin of the Russian science city of Novosibirsk. The translation of the letter by Drs. William & Vivian Parsons of Eckerd College states that he knew several of my works, including the work on the radar location of Venus. Just as I did, he also computed that the speed of light in a vacuum from a moving source is equal to c +v, and he included a copy of his article "Gnosiological Peculiarities in the Interpretation of Observations (For example the Observation of Double Stars)", in which is cited still one more demonstration of this proposition.

In July of 1988, I received a letter, written in English, from a Dr. Svetlana Tolchelnikova-Murri of Pulkovo Observatory. In the letter she said that she got a copy of my paper "Radar Testing of Relative Velocity of Light in Space" from Dr. Vladimir Sekerin in Novosibirsk. It was very interesting to her. She was working with Pulkovo Observatory, and her field was astrometry. She felt that the intrusion of relativistic theories into fundamental astrometry was quite a failure, that was not yet comprehended by the majority. Thanks to PERESTROIKA she was writing me openly, but their (Pulkovo) Observatory is one of the outposts of orthodox relativity. Two scientists were dismissed because they discovered some facts which contradicted Einstein. It is not only dangerous to speak against Einstein, but which is worse it is impossible to publish anything which might be considered as contradiction to his theory. It seems the same situation is true for their Academy. In February 1989 in Leningrad, they planned to organize a conference (during two days) "The Problem of Space and Time in Modern Science." Its real goal was hidden under the philosophical covering. Their only desire was to publish the results. There were only 6 reports in a schedule, but the lectors were of a middle (or low) scientific grade (rank) and now two official participants philosophers were added by the directors of their institutes. It was out of her power to invite me, but she could send me afterwards the copies of the reports in Russian if I was interested. She asked if I had ever been to Leningrad? If not she thought I should come. Her friends and her were very interested in my work after 1969. Under a separate cover she was sending me a book with several papers which might be interesting to me. In my reply to Svetlana, I sent her reprints of all the material I had published over the years. Since her original letter, I've had an extensive correspondence with Svetlana, and in a November 1988 letter she wrote that on the 13th of March 1989 during three days there would be a conference in Leningrad "The Problem of Space and Time in Natural Science" with participants from other cities of the USSR, and it would be alright for me or any of my friends from the USA to come to this conference. They hoped to invite TV and a journalist in order to raise the question of scientific ethics in their scientific community. The best guarantee that their scientific papers will be published not in ten or thirty years, but now, will be the presence of some objective observers or participants from my country at the conference, and it would be easier for them not to use Aesopian language.

In an effort to comply with Svetlana's request to bring western scientists and journalists to the conference, I used my personal copy machine, computer, and daisy wheel printer to send a 4 page personal letter to 23 journalists and 43 scientists, along with a copy of her letter that contained the conference invitation and information. The following is a sampling of some of the replies: Paul C. Tash, the Metropolitan Editor of the local newspaper, the St. Petersburg Times wrote that unfortunately, circumstances did not permit them to accept our offer. However, if there should be developments at the conference that I considered newsworthy, please contact their reporter David

Ballingrud, who covers science and aerospace; Walter Sullivan of The New York Times, whom I mentioned earlier in Chapter 4, wrote that he had not been to Pulkovo for many years and would love to return, but is retired now and could not justify the trip; Dr. David Lazarus, the Editor-in-Chief of The American Physical Society wrote he was sorry not to be able to accept our invitation to attend your upcoming meeting in the USSR. It might be enjoyable as well as enlightening. In his role as Editor-in-Chief, however, he must constrain himself to a totally hands-off or arm's-length posture regarding any field of research; Dr. Jean Pierre Vigier of the Institut Henri Poincaré in France, wrote that in his present situation it is absolutely impossible for him to attend the Pulkovo Conference unless he received an official invitation, which is also necessary to obtain a Soviet visa and raise the travel expenses. He has always had his doubts on Prof. Shapiro's observations and would appreciate a discussion on the radar experiments. The Sekerin results were unknown to the experts in Paris and he hoped I can inform them after my trip to Leningrad. If I or some soviet observer has new significant results on our problem he would be happy to consider them for publication in Physics Letters A of which he was an Editor; Dr. Louis Essen of England, whom I mentioned in Chapter 2, wrote that it would have been interesting to attend the meeting at Pulkova Observatory - which he visited a long time ago, but health problems prevent him from travelling - quite apart from the expense. He hoped that Svetlana and I did not expect too much from the meeting. Many criticisms of relativity theory have been published without having any effect on the Establishment, showing that publication is not enough. Indeed the more the theory is criticized the more strident the support is maintained - a common feature of all irrational beliefs. He had heard a former Director from there give a paper in which he showed that a careful analyses of the 1915 eclipse results did not support Eddington's claim, on the Relativity Theory.

In her letter of 2/12/89 Svetlana wrote that if 1/5 of the people I had invited will come it would cost her head. During my visit I learned that the Observatory had received a large number of letters from western scientists, that expressed dismay over the fact that such a conference was being held. I now know that the ease of which I obtained my Visa was the exception and not the rule. It seems that my visit was sponsored by the Academy of Sciences of the USSR, and since it was an unusual Visa, no one at the Observatory knew how to handle it, in particular, Svetlana could not determine where or if I needed to register my arrival at my destination. The only one I found to accompany me to the conference was Robert Fritzius of the Magnolia Scientific Research Group at Starkville Mississippi. Even though Svetlana sent him a personal invitation to the conference, and told him he could stay at the guest quarters at the Observatory, he was forced to get his Visa thru Intourist which cost him an extra $200, and he had to stay at the Moscow Hotel in Leningrad which cost an additional $1000.

Robert kept an extensive log that contained intimate details of the trip, and he sent me a copy of it. He intended to use the log to help him translate the papers and other information that came from the conference. He translated the original c+v double star paper Vladimir Sekerin sent me in 1987, and has now also finished the translation of Vladimir's book. I found from his log, that with regard to his Visa, he was registered automatically when he arrived at the hotel, and that the Intourist person kept his passport and Visa for the first night. He had to obtain his room key from a lady in charge of his floor to enter his room, and he had to return the key to her when he left his room. In contrast to Robert, I stayed in Svetlana's daughter's room in her flat at the Observatory. Her daughter was 14 years old and her name was Katja, and she had pictures of Western and Eastern rock music stars on the walls.

I brought my 35mm SLR Pentax camera with me along with 3 rolls of 36 exposure Kodak Ektachrome 400 slide film, so I now have 108 very nice slides of the tours thru the Hermitage museum, St.Isaac's Cathedral, a famous cemetery, all the speakers at the conference, radio and optical telescopes and related equipment at the Observatory, meals and meetings at different homes, etc. During my visit I had many intimate conversations with regard to just about any subject of interest from politics to science, and in several conversations with people who seemed to have intimate knowledge of what was happening behind the scenes, I learned that my correspondence with Svetlana was being monitored and that Gorbachev had read Vladimir's book STUDIES INTO THE THEORY OF RELATIVITY, and it was his references to my published papers that had lead to my unusual Visa. I told Svetlana that I believed that we were pawns in a larger game that concerned Gorbachev's interest in stopping "Star Wars" and the nuclear arms race, and that revealing intimate knowledge of the c+v relative velocity of light in space and military secrecy with this regard, were all elements of this game. Svetlana was unhappy at the prospect of being a pawn, and would have preferred to have the illusion of freedom, but I on the other hand, am happy to be a pawn, just as long as the game moves in the direction I wish it to go.

The fact that our correspondence had been monitored was obvious. I have observed the wrinkled appearance of the sealed parts of the envelopes, the gloss of the resealing glue, as well as the erratic arrival times of our air mail letters. The surveillance of our mail had caused a major communication problem, for instance Robert called the Director Dr. Victor Abalakin, and was told there was no March conference at the Observatory. Since I had not heard from Svetlana for some time, I called Abalakin and he said the same thing, but added the suggestion that perhaps it was being sponsored by some other scientific society in Leningrad. Abalakin said he would have someone meet me at the airport, and asked if I would contact Dr. J. Lieske at the Jet Propulsion Lab for him. I managed to contact Lieske at a different number than the one Abalakin had given me, and he was surprised to find it was a simple matter to call the USSR, he thought that you had to obtain permission from the KGB. The main problem with calling the USSR is the fact that the lines tend to be very busy, the best time I had found to place a call turned out to be around 5:30 AM, and I suggested to Lieske, that because of the larger time difference in California, the best time for him would probably be around 1:00 AM.

On 2/1/89 I received 3 letters from Svetlana dated from 11/17/88 to 1/15/89, which tends to illustrate the erratic nature of our airmail correspondence. One letter contained a New Years card that had about 20 signatures of people from around 5 different scientific organizations in Leningrad, another letter contained 3 postcards, with one card showing the building where the conference was to take place, the Leningrad Academy of Sciences which was built in 1873, on the card Svetlana said that they hoped to organize two lectures for me to deliver, one at the Academy of Civil Aviation, and the other at the Institute of the History of Science. On 2/14/89 I wrote Svetlana of my change in travel plans, I was to arrive on an Air France flight from Paris at 3:45 PM instead of the Aeroflot flight from Moscow at 4:20 PM. She did not received that letter by the time I arrived on March 10, and she was waiting at the wrong building. Prof. Pavel F. Parshin, the Chief of the Department of Physics at the Aeroflot Academy of Civil Aviation, showed up looking for Robert, and found me instead. Both Robert and I had arrived on the same plane, and Intourist had already taken him to his hotel. Svetlana turned out to be a pleasant looking 52 year old woman with light red hair, and she greeted me as her soulmate from the USA. We drove to her flat at the Observatory, and had a very elaborate dinner, that was prepared by two of Svetlana's male associates, and included champagne to celebrate my arrival.

One of the men fixing the dinner was Dr. Konstantin Manuilov, and he gave a talk at the conference that was based on his solution to the n-body problem based on Newtonian mechanics. We were quickly on a first name basis, and because of my poor memory, many of the names, conversations, and events that took place during the visit were a blur.

The next day, which was a Saturday, Robert and myself along with Vladimir Sekerin and his wife Lydia went to visit Pavel and his wife and son in their apartment in Leningrad. Robert had a number of questions concerning his effort to translate Vladimir's book, then we had in depth discussions about Pavel's work concerning a modern variation of the Ive's experiment, it seems that while he was able to publish the details of the experiment and the resultant data in a prominent USSR scientific journal, he was not able to publish his theoretical analysis because it was not consistent with Einstein's special relativity theory. We had a very elaborate dinner followed by cognac and more discussions.

The following day, which was a Sunday, Svetlana and Katja, took Robert, Vladimir, Lydia, and myself on a tour of Leningrad. We visited a cemetery across the street from the hotel where many prominent people were buried, than a Russian Orthodox church service where Svetlana, Katja, and I lit candles, then we went to the Hermitage Museum. The Hermitage was a fabulous place with over a thousand rooms, it would have taken days to visit all of them, one of the pictures taken was of me standing next to a portrait of my ancestor Oliver Cromwell. After that we had coffee and filled pastry at a Russian version of a fast food restaurant, and then paid a visit to the Victory Square War Memorial and Museum dedicated to the World War II 900 day siege of Leningrad. That night Svetlana, Katja, Robert, and myself had dinner at an apartment built during the Khrushchev era which was the home of one of Svetlana's younger friends, a woman whose husband was a geophysicist working with marine gravity measurements, and who had a daughter the same age as Katja.

The next day was a Monday, and Svetlana took me to pay an official visit to the Observatory's front office. The only problem was that all the top officials had flown to Moscow, and there was no one in charge of the Observatory??? Svetlana arranged to have an Observatory van and driver for us to use, and then we made a trip to Leningrad to pickup a young woman physicist named Olga who was to serve as an interpreter for my lecture that afternoon. She was given a copy of my famous 1969 "RADAR TESTING OF THE RELATIVE VELOCITY OF LIGHT IN SPACE" paper to familiarize her with the terms I would use in the lecture, and as we drove along, she was reading the paper and said "this is madness I can't be reading this, I must be going mad." That morning we visited St.Isaac's Cathedral, an unbelievable place, then we paid a visit to a respected elder scientist, Dr. S. A. Bazilevsky, who had been unable to publish anti-relativity papers during his career. He knew of my 1969 paper and wanted to meet me, and during our meeting he handed me a carbon copy of one of his unpublished papers. One of Svetlana's friends read a paper of his during the conference, and my 1969 paper was referenced and it caused quite a stir. After the visit, we went to the House of Scientists, a large palace that was still in the process of restoration, and I delivered my lecture which dealt for the most part, with the philosophy and history behind Einstein's relativity theories. The lecture was well received, with many comments and questions, and afterwards we went into another room and attended a banquet in which Robert and I were the guests of honor. The banquet ended with two bottles of cognac, and many toasts, it was a wonderful experience.

The next day was Tuesday March 14, the 110th. anniversary of Einstein's birth, and the new starting date of the conference, which was now being held in a lecture hall at Pulkovo Observatory. At the entrance to the hall, Svetlana had set up a poster display containing a drawing of a dragon and the earth, and a number of humorous satirical poems and arguments, and beneath the poster she put a copy of the article "RELATIVITY - joke or swindle?" which was published by the prominent English physicist Dr. Louis Essen. [70] The Director of the Observatory flew in from Moscow to deliver the opening address, and then flew back to Moscow. Svetlana introduced me to the Director before the conference started, and he laughed when I told him what Lieske had said about the KGB. Abalakin said that shortly after my call to Lieske, Lieske had called him. Svetlana was surprised by the objective tone of Abalakin's speech. Before he had become the Director, he had been an anti-relativist, then after he obtained the position, he switched camps and became a relativist, and even won a state prize for introducing relativity into celestial mechanics. Also under his administration, two of the other woman anti-relativists had been forced into early retirement, and now it seems that position has been reversed, and they are back at work.

A humorous twist to the conference was the fact that some of the relativists at the conference complained that they were being persecuted. A number of relativists withdrew their papers, and that changed the length of the conference from 3 to 2 days. Because of the many changes that had taken place, the printed program was no longer valid, and due to the lack of a copy machine at the Observatory, I was unable to match speakers and papers with my slides, but Svetlana later sent me an updated program so I could do so. During the afternoon session, Svetlana delivered her talk concerning her anti-relativistic views with regard to positional astronomy. Svetlana's talk was followed by Vladimir's talk presenting the binary star evidence showing the speed of light in space was c+v. That night there was a meeting of the conference committee. It was decided that Robert and myself would become members of the committee and that there would be another conference to be held in Leningrad two years later. I suggested, and it was accepted that Dr. H. Aspden of England, Dr. J. P. Vigier of France, and Dr. J. P. Wesley of West Germany be invited to become members of the committee. I told the committee that Vigier was a member of the Institute Henri Poincaré in Paris and an editor of Physics Letters A. I also said that in his reply to my letter in regard to the conference, he had expressed an interest in the results from the conference, and suggested that participants submit papers to him for possible publication in his journal. Svetlana announced that V. N. Bezwerchy had contacted her and offered to publish the proceedings of the conference. I had talked with Bezwerchy a number of times during my visit, he was an interesting fellow and he seemed to have a great deal of inside information with regard to political and scientific matters in the USSR. I suggested that we consider publishing the proceedings in English as well as Russian, and it was agreed to investigate that possibility.

The following day was Wednesday, and my talk was the last one of the morning session and Svetlana served as the translator. I used overhead projector slides for illustration and to help prompt me, since I did not have a prepared text, and the title of the talk was "The Problem of Space and Time in Modern Physics." Robert's log with his notes on the lecture allowed me to create a written version which was to be published in the conference proceedings. The talk was based on the arguments and information in my paper [71] "THE GREAT SPEED OF LIGHT IN SPACE COVERUP" and the followup paper [32] "SCIENTIFIC FREEDOM" which was in part a reply to I.Shapiro's reply [72] to the first paper. One of the many interesting comments and questions that followed the talk, was where a participant asked me to summarize my opinions with regard to relativity theory. I stated that the special relativity first postulate with regard to detection of translatory motion, was obviously false, and referenced Einstein's former research associate's argument in this regard. [73] I went on to state that any reasonably objective physicist should realize that the ultimate test of the second postulate that the speed of light in space is constant, is to analyze the modern data on the transit times of light signals in the solar system, and this evidence shows beyond a shadow of a reasonable doubt that the postulate is also obviously false, and I cited my above arguments in this regard. I also mentioned that the prominent British astronomer Dr. G. C. McVittie in both publication and correspondence has indicated that he has had the same sort of problem in trying to obtain meaningful information from Shapiro, and in a 1970 letter writes that the secrecy with which Shapiro surrounds his methods and his observational results makes him wonder whether there is something to be concealed. In McVittie's paper, he points out the fact that:

> in the Einsteinian theory of gravitation, an exact solution for the gravitational field of a set of discrete bodies is possible only when one of the bodies is of finite mass whereas the rest are of infinitesimally small mass. This is in contrast to the Newtonian theory of gravitation in which an exact solution for the field of two massive bodies is possible, complications arising only when three or more bodies are in question... [81]

The fact that Moyer's equation (3) is the "Newtonian" approximation to the n-body metric, should be considered as evidence against Einstein's general relativity equivalence principle. [36] On the other hand, Dr. J. C. Hafele and Dr. R. E. Keating have used commercial jet flights and atomic clocks to present convincing empirical evidence that tends to resolve the relativistic clock "paradox", and they found that the relativistic dilation of time was a function of the clock's speed relative to an absolute coordinate system at rest relative to the distant galaxies. [74] I certainly have no problem with E=mc2 since the atomic bomb is ample evidence that it is true. So in general, much of relativity theory is true, but many of the original arguments are not. The real problem with modern science is the lack of scientific objectivity and integrity on the part of many prominent scientists, they are little more than politicians, and are far more concerned with the advancement of their careers and status, then the advancement of science. What is needed are true scientific journals that publish all arguments and evidence in a reasonable period of time and at a modest cost. The peer review should take place after publication, and should involve all scientists, and not just a privileged few. The key to the more rapid advancement of scientific knowledge, is a more efficient and democratic forum for communication.

On Thursday, the last day of my visit, I had a tape recorded interview by a newspaper correspondent from the Soviet science city of Tomsk. He was surprised to find that I considered the most important man alive today to be Gorbachev. I explained that his efforts to end the arms race would result in a much safer world, and would bring vast economic benefits to both the US and the USSR. Science would also benefit from the end of the arms race, since military secrets invariably involve scientific knowledge. A major element in the c+v speed of light in space coverup may have been the military interest in using the laser as a "Star Wars" weapon.

After the interview I asked Svetlana if I could take a picture of the large 26" refractor telescope. She explained that this was hard to do since the administration did not like to show off their equipment to visitors. She asked her friend Dr. Alexandra Schpitalnaya, one of the reinstated anti-relativitists, to try and show me the telescope while she fixed our final lunch. Alexandra took me to the building where the keys are kept, and the woman in charge of the keys refused to give them to her, then

Alexandra made a phone call, and a short while latter a man ran down a large hallway, signed out the keys, and gave them to Alexandra. After a fast tour of the radio and optical telescopes, museum, and library, we returned to Svetlana's, and found that Katja and her friend Anna had baked me a cake with "For Your" written on top. At the airport Svetlana said we would say goodbye in the Russian way, so we hugged and kissed each other's cheeks. As the plane flew into the low lying clouds, my last view was of the large dome of the 26" telescope at the Pulkova Observatory, a fitting end to my wonderful visit to the USSR.

On my return to the US, the first thing I did was to use the copy of Roberts notes which he sent me, to generate a paper concerning my invited talk at the conference. Then I sent the paper to Svetlana, so that it could be published in the conference proceedings. The next thing I did was to use a slide copier to make prints from 36 of the slides, which I then sent to Svetlana and Robert. Then I wrote a long letter to Walter Sullivan concerning my visit to the USSR, and also sent copies of the letter to other people that I thought would find the trip to be of interest. One of the more interesting replies was from Dr. J. P. Wesley of West Germany, who wrote thanking me for the copy of my exciting saga to Leningrad letter that I wrote for Sullivan, he also enclosed a list of individuals who would be interested in space-time physics. With regard to Wesley's list, I received a 4 December 1989 letter from Prof. Jorge C. Cure' of Miami Florida inviting me to an informal gathering in St. Petersburg Florida on the 27th of December 1989, to put in practice the old Greek art of exchanging ideas in friendly dialogues. He wrote that due to a strange circumstance the state of Florida had attracted seven free thinkers, that dared to walk the lonely path of fundamental inquiries. It seems that Jorge had gotten the seven names from Paul's list! The Florida anti- relativist conference was held at Lewis House on the Eckerd College campus, and was sponsored by the Academy of Senior Professionals, of which one of the participants, Earl C. Sherry, was a member. The one day conference was video taped by one of the participants, Francisco Muller. My talk was the first one, and was illustrated with 80 35mm color slides. The first 40 slides were related to my work concerning radar testing of the relative velocity of light in space, [18] and research done at Eckerd College in collaboration with Prof. Wilbur F. Block and Prof. Richard A. Rhodes II on H- ions, [82] crossed beam electron-electron scattering at low energy, [83] and computer simulation of mass dynamics in electrons. [66] And the last 40 slides were from my USSR visit and the Pulkovo conference. The talk was well received, with many interesting questions and comments from the participants.

In a letter dated 6/4/89 Prof. Pavel Parshin informed me that a Dr. Fedor A. Morochov intended to publish a paper about my talk at the Pulkovo conference, and in a letter dated 12/14/89, he informed me of the increasingly large number of anti-relativistic works being published in the USSR, including a booklet titled "Miracles of the Relativistic theory" written by a Supreme Soviet deputy, Dr. A. A. Denisov from the Leningrad Politechnical Institute. Pavel also informed me of the special program "Mirror" of the Leningrad TV that had a show devoted to an "Is Einstein right?" discussion, and he suggested that I submit an entry to the program. I submitted a six page single spaced letter on this subject, and included copies of my Scientific Ethics articles on this question. [32,71] In a letter dated 3/25/90 Svetlana said that Parshin went to Minsk in early February 1990, where about 40-50 physicists had a five day anti- relativistic conference. In my reply to her of 4/9/90, I wrote:

> I have not heard from Parshin about the February conference, but you know how unreliable the mail is, he may have written but I have yet to receive it. The anti-relativistic conference sounded exciting, 40-50 physicists, and lasting 5 days! Very

> good. It confirms my suspicion that the anti-relativity Renaissance will come from the USSR. I expect that the next 20 years will see a varietal explosion of science and technology coming from Russia. It would be wonderful if it would lead to PERESTROIKA and GLASNOST in science in the US and the rest of the world as well. As you say about the results from the conference, there are many different variations of anti- relativity theory in the US and other Western countries as well. The important thing, is not that there be a consensus of opinions, but that there be a free and democratic right to voice and publish opinions, the consensus will come in time. And it will be a realistic consensus, and not the unrealistic authoritarian consensus we now have with regard to Einstein's relativity theories. Since you now have N.2 of GALILEAN ELECTRODYNAMICS you have that conference report. As I was writing this letter, the current copy of Physics Essays arrived, and the back of this letter contains a copy of that report, and I also enclose a spare copy for you to share.

The reports I referred to in the letter were ones that I had published on the 1989 Pulkovo conference. [83,84] I have received a large correspondence that includes many reprint request from the GALILEAN ELECTRODYNAMICS article. [84] One of the letters came from the Editor of the journal APEIRON, who asked if I thought any of my contacts in Leningrad would like to see his journal and that contributions were welcome. In my letter to him I wrote that Svetlana is the ring leader of the Leningrad anti- relativists, and writes and speaks English fairly well, and that he should write her directly to find out if any of them would like to subscribe or publish in his journal. One of the more interesting replies came from Prof. Howard Hayden of the Department of Physics of the University of Connecticut. He started out by requesting a reprint of my 1969 Venus radar paper, and then wrote that he did not wish to count himself among the defenders of relativity theory, but he doubted whether the discovery that the speed of light isn't constant will revolutionize much physics. It may "devastate" a few people, but not the knowledgeable ones, on the other hand, getting a physicist to *say* that the speed of light isn't constant is like trying to exsanguinate a turnip. It is somewhat futile to argue with special relativity theory, primarily because it is *inherently* irrefutable. That is, it is supposed to work only in *inertial* frames, which are non-existent. With regard to General Relativity he closed with the hope that it will die a slow death at the hand of Ockham's razor. I received a 3/15/90 letter from the editor Prof. Petr Beckmann, who wrote that a Palo Alto physicist, Dr. Eugene Salamin, had sent a long letter arguing with the papers published so far in Galilean Electrodynamics. Concerning my report, Salamin's letter contains the following paragraph:

> "The report on the Soviet Conference claims there is evidence from binary stars that the speed of light in space is $c+v$. This is totally absurd: after thousands of years travelling to earth, the light from the different members of the binary systems would get out of phase. If the $c+v$ theory were true, some binary systems would exhibit simultaneous red shifts from both members, instead of one member red shifted and the other blue shifted."

I sent Beckmann the following 3/19/90 reply:

> Eugene Salamin is correct in arguing "If the $c+v$ theory were true, some binary systems would exhibit simultaneous red shift from both members, instead of one member red shifted and the other blue shifted." In a classic astronomy textbook136 we find

following ad hoc *c* argument to explain this observed phenomena:

> Struve concludes that the gas whirlpools cause the seeming discrepancy in the behavior of a few eclipsing binaries which long puzzled the investigators. Where the velocity curve of the binary implies an orbit of considerable eccentricity, the light curve may require a circular orbit.

Fox has done an extensive investigation of the supposed evidence against the Ritz *c+v* emission theory68 and with regard to binary stars argues:

> There are also some difficulties for Struve's hypothesis. The model would seem to have consequences similar to those of the Ritz theory.

The analysis of the transit times of light signals in the solar system does not suffer from the same ambiguity as that of the binary star data. With this regard I have recently published67 the following argument:

> Theodore D. Moyer of the Jet Propulsion Laboratory has published a paper that reports the methods used to obtain accurate values of range observables for radio and radar signals in the solar system. Moyer's (A6) equation and the accompanying information that calls for evaluating the position vectors at the signal reception time is nearly equivalent to the Galilean *c+v* equation (2) in my paper 'Radar testing of the relative velocity of light in space.' With regard to his equation, Moyer states "The first term on the right-hand side is the Newtonian light time" but he does not go on to explain the enormous implications of this statement. I sent Moyer a reprint of this paper, and to date, he has not seen fit to comment on my argument.

I received a 3/27/90 telephone call from Beckmann, and he asked many questions with regard to my views on this matter, then he stated that he may not have sufficient room in his journal for my reply. In his publication of Salamin's comments in the May/June 1990 issue, he dropped the above paragraph and did not publish my reply. I am not surprised that Beckmann did not accept my answer for publication since he now realizes that the modern solar system data presents evidence against his theory that light is a wave in the gravitational field.

I found my participation in the 1991 II International Conference on Space and Time Problems in Natural Sciences to be an exhilarating experience. The Conference was convened and organized by the Leningradian Branch of The Academy of Sciences of the Russian Soviet Federative Socialist Republic which was created in April of 1991 by the Russian parliament. The Conference was held at the 15 story 746 room Leningrad Hotel on the bank of the Neva river, and the participants stayed and had meals there as well. From my hotel room window I could see the cruiser Aurora that fired the shot to signal workers and sailors to begin their February 1917 victorious assault on the Winter Palace. The Palace has become the Hermitage Museum with more than 3 million works of art, and it was also visible from my window, and was only a short walk from the hotel. The food, lodging, and

Cultural programs were all covered by the $270 registration fee, with the only additional expense being the optional $50 cost of the Friday Conference Banquet. The Conference had a total of 114 participants, with 14 of them from countries outside the USSR. The Local Organizing Committee limited the number of Soviet participants to 100 in order to maintain a more desirable size for the Conference. The foreign participants received an English version of the program that contained abstracts of the papers to be presented, and the Soviets received a Russian version. The program underwent extensive revision, mainly due to the fact that around 26 of the expected foreigners did not show up, probably because of fear of what to expect from the recent Soviet coup attempt and the normal USSR communications problems. The participants received a radio receiver and ear phone that allowed them to receive both Russian and English simultaneous translations, and a staff of translators were available for translation of conversations between individuals and small groups and meetings. Most of the more important talks were held in the large Grand Hall, and the talks that the Local Organizing Committee decided were of lesser importance were held in the afternoon sessions in two smaller Halls. Much of the proceedings were televised, and some of the participants, including myself, received televised interviews.

The Conference opened at 9:00 AM on Monday September 16th, with a welcoming address by Prof. Leonid Maiboroda, the chairman of the Leningradian Branch of the Academy. There was a Reception at 6:00 PM that night, with plenty of fancy food and drink.

Lee Coe of California, delivered an excellent presentation of his paper GALILEAN-NEWTONIAN RELATIVITY VERSUS EINSTEINIAN RELATIVITY at 10:40 AM on the Tuesday Plenary Session in the Grand Hall. I was one of the chairmen for the session, and I could see from the podium that his talk had been televised. I told Lee, and he was able to obtain a VHS cassette copy for $50. I received a September 24th phone call from Lee, and he said that the tape did not play back at the proper speed on his daughter's VCR but that he would be able to have it transcribed to the proper US speed for a reasonable cost. The Tuesday Cultural program was an afternoon tour of the Hermitage Museum, but I did not go since I had been there on my last trip, and I had to spend most of my spare time meeting with groups and individuals. I kept the staff of translators busy, and gave most of them small print copies of a preprint of this book, to express my gratitude for their difficult task. I received a large collection of booklets, reprints, etc. from various individuals, and also gave them copies of my book in return, for a grand total of 52 book copies that I gave out during the trip.

Prof. Petr Beckmann, who I mentioned earlier, was the US co- chairman of the Conference Scientific Organizing Committee and one of the chairmen for the Wednesday Plenary Session, and he announced that unlike the previous sessions, he intended to rigidly follow the schedule. He said that each speaker would have 20 minutes to talk, even though the program listed 30 minute sections, and the previous talks had been 25 minutes to talk, and 5 minutes for questions and comments? Svetlana Tolchelnikova's paper titled VERIFICATION OF EINSTEIN'S SECOND POSTULATE BY MEANS OF ASTRONOMICAL OBSERVATIONS was the second one for the session, and she had been left a full blackboard from the previous speaker. As she was cleaning the board Beckmann announced that the time would be deducted from her talk. I mounted the platform and told Beckmann that I was donating the time for my talk to Svetlana. He announced this, and Svetlana and many other participants protested my decision, since they wished to hear what I had to say. I went to the floor microphone and stated that my talk was merely an abstract of my book THE FARCE OF PHYSICS, that I had plenty of condensed preprints for anyone that wanted them, and that Svetlana had shown me the material she would present and that in my opinion her talk would eventually be considered to be the most important event in science in the 20th century. Svetlana then had plenty of time to give her presentation that was essentially an indepth confirmation by a professional astronomer and mathematician of my 1969 paper on this question. Her evaluation of the published mathematics used by the professionals who had analyzed the modern solar system signal data, was that the classical theory was confirmed since the equations with the second order terms empirically found by investigators coincide with the classical formulae, and not the relativistic ones! Prof. Beckmann later made a translation of her talk from Russian to English, and published it in his journal GALILEAN ELECTRODYNAMICS. [151] The Cultural program that night was in the Grand Hall and consisted of singing, music, and Russian folk dancing.

I delivered Prof. John E. Chappell, Jr.'s paper THE PROBLEM OF INTOLERANCE IN AMERICAN UNIVERSITIES, AND THE PATH TO A NEW NATURAL PHILOSOPHY as the first paper to be delivered during the Thursday afternoon session in the Grand Hall. After the talk there was an extended applause, and when it stopped I said that since it was not my paper, I would not answer any questions, but that he had given me copies of the talk and other material to hand out to anyone who was interested. As soon as I reached my seat, I was surrounded by people that wanted his material, and I did not have enough for all of them. During the session there was a very impressive well illustrated talk by a high tech research type individual Dr. V. O. Beklyamishev, and the title of his paper was ON GNOSIOLOGICAL SIGNIFICANCE OF WALLES'S INVESTIGATION OF THE DATA OF VENUS RADIOLOCATION. This version of the spelling of my name appeared a number of times during the Conference, and seems to be how it is translated from Russian back into English. From the way Beklyamishev spoke, I got the impression he was a member of a research group, and that he was talking about a continuing research project. In a photograph taken at the Conference, he was sitting with a group of people that included a man wearing a military uniform. Svetlana had told me that the space data was controlled by the military in the USSR. I have a sneaking suspicion that Beklyamishev's paper is the opening round in a dramatic Russian research project that will bring an end to the Einstein Relativity era of modern physics. That evening the Cultural program was a tour of churches and palaces and so forth, but I did not go because of a business meeting. The meeting was with a man from Moscow, Dr. Sergei Goncharov who was the General Director of "Intertechnopark" a Economic Scientific Research Institute. The meeting included a number of the foreign participants and involved tentative plans on the foundation of an international school of advanced physics, the organization of groups to run seminars of Soviet and foreign scholars, work on modern textbooks, and international commercialization of advanced technologies.

The Friday afternoon session in the Grand Hall was titled Problems of Scientific Ethics, and Dr. A. A. Denisov (President Gorbachev's advisor, and the head of Commission on Ethics problems of the Supreme Soviet) was the principle chairman. I was the first speaker and the essence of my talk was that the main problem was the lack of scientific ethics in modern scientific journalism. I proposed that the Russian Academy would start a new archival international scientific journal that would be devoted to democratic journalism free from arbitrary prejudicial and political censorship. The talk was well received and I was handed a number of notes from participants that wanted to help establish such a journal. At the Conference Banquet that night, I was introduced to a woman who was the producer of the TV show called "Is Albert Einstein Right?" and she said that most of the large volume of mail that had been received had concerned me and my radar evidence against Einstein's theories. At the Banquet I met a very interesting business man who had spent 4 years in prison because he had made too much profit! He was a fascinating person to talk to, his name was Mikchail Ivanov, he spoke perfect English, and I learned much about current Russian economic problems, and how he was working to help solve them. At a business meeting the next day, he was the advisor for the Russian Academy, and we made plans for the new journal which will be published in simultaneous Russian and English versions. At present we have plans to hold the III Conference in March 1994 in St. Petersburg (Leningrad) Russia, and I am a member of the organizing and editorial committee.

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Ultimate Unification

I now suspect that the original foundation for my 1964 intuitive insight on how to create the ultimate unified theory of physics came from a 1959 Scientific American article [106] titled "Descartes." The statements that probably had the most impact on my thinking, are as follows:

> "I should consider that I know nothing about physics if I were able to explain only how things *might* be, and were unable to demonstrate that they *could not be otherwise*. For, having reduced physics to mathematics, the demonstration is now possible, and I think that I can do it within the small compass of my knowledge."
>
> With these words René Descartes declared the viewpoint that placed him among the principal revolutionaries in the 17th- century scientific revolution. Against the "forms" and "qualities" of Aristotelian physics, which had proved to be a blind alley, he asserted the "clear and fundamental idea" that the physical world was sheer mechanism and nothing else. Because the ultimate laws of nature were the laws of mechanics, everything in nature could ultimately be reduced to the rearrangement of particles moving according to these laws. In analytical geometry, perhaps Descartes' most enduring achievement, he created a technique for expressing these laws in algebraic equations. He thus put forward the ideal program of all theoretical science: to construct from the smallest number of principles a system to cover all the known facts and to lead to the discovery of new facts.
>
> All subsequent theoretical physics has been aimed at the realization of this ideal of a single theoretical system in which the last details of observable regularities should be shown to be deducible from a minimum number of fundamental equations, written perhaps on a single page. Blaise Pascal and Isaac Newton may certainly be said to have carried on in the 17th century the Cartesian program of looking for the explanation of the physical world in terms of its mechanism. In this century we have witnessed attempts at universal theories by Albert Einstein and Werner Heisenberg, among others. In the vision of Descartes, however, his indisputable first principles "nearly all so evident that it is only necessary to understand them in order to assent to them" were not the end but the beginning of the search...
>
> Descartes himself came to recognize that his purely deductive, mathematical ideal for science had failed in the face of the complexities of nature and the enigmas of matter...
>
> In order to explain how the planets were kept in their orbits, Descartes put forward his famous vortex theory, according to which the fine matter of the "ether" forms great whirlpools or vortexes round the stars and the sun. The planets are carried about in the

> sun's vortex, rather like a set of children's boats in the celestial bathwater, and the moon is carried round the earth in the same way. The astonishing thing is that Descartes did not bother to check whether or not this very important part of his physical system agreed with the facts as expressed by Kepler's laws of planetary motion. It was Newton who destroyed Descartes' famous vortex theory. In fact, he may have chosen the title *Principia Mathematica* to give point to his polemic against Descartes' *Principia Philosophiae*. Newton treated the vortex theory as a serious problem of fluid dynamics and utterly demolished it...

My first standard radar paper was dated 12/9/67 and titled "AN INTERPLANETARY RADAR TEST OF RELATIVITY," and it went through a number of titles and revisions as it was submitted to, and rejected by a large selection of journals. I received a letter dated October 13, 1969 from the Editor of the journal SPECTROSCOPY LETTERS, Prof. J. W. Robinson of the Department of Chemistry of Louisiana State University, who wrote that it had been brought to his attention that I was interested in the special case of relativity and that I had evidence that the speed of light may not be *c*. I submitted the paper to his journal and it resulted in my first published paper [18] titled "RADAR TESTING OF THE RELATIVE VELOCITY OF LIGHT IN SPACE," and the abstract read:

> Published interplanetary radar data presents evidence that the relative velocity of light in space is *c+v* and not *c*.

I next published a series of three more papers in that journal, the second paper [107] was titled "COSMOLOGICAL IMPLICATIONS OF A c+v RELATIVE VELOCITY OF LIGHT" and the abstract goes:

> The c+v relative velocity of light explains the observational data from spectroscopic binaries and presents evidence that the Universe is not expanding. Inconsistencies between previous laboratory experiments that present evidence of *c*, and the interplanetary radar evidence of *c+v*, can be explained in terms of a dynamic ether.

The third paper [108] was titled "RADAR EVIDENCE THAT THE VELOCITY OF LIGHT IN SPACE IS NOT *c*" and the abstract states:

> Observed-computed residuals of Earth-Venus radar time-delay measurements from 1961 to 1966 show variations that range to over 30,000% the expected error from the best possible general relativity fit the Lincoln Lab could generate. The variations are not random but are related to relative radial velocity and intervening plasma. These variations are evidence that the relative velocity of light in space is some form of *c+v* and not *c* as predicted by Einstein's general relativity theory.

The forth paper [109] was titled "EXPANSION OF A DYNAMIC ETHER HYPOTHESIS OF PHYSICAL REALITY" and revised the models of atomic structures presented in the second paper [107] by replacing fused electrons with neutrons. In a 4/4/79 letter from Dr. Robinson, he informed me that because the very negative reader reaction to these type of arguments he could no longer publish my papers on mass dynamics and relativity. In a 7/23/90 letter he expanded on his first answer by saying that he had received completely unsavory and unobjective anonymous letters and phone calls.

The fifth paper [19] I've published on this was in the prestigious journal FOUNDATIONS OF PHYSICS, a journal that many prominent scientists have published papers in over the years. The paper presents the current foundation and the fundamental equations of my work on a unified theory based on mass dynamics. The title of the paper is "The Unified Quantum Electrodynamic Ether" and the abstract reads:

> *The basic evidence and doctrines of physics and astronomy are examined and found to contain a simple, consistent unitary nature. It is proposed that all physical phenomena may be better explained in terms of a single physical entity if one accepts a conceptual advancement of presently accepted doctrine. The modification postulates that the inertial mass of matter is the same entity as the virtual mass of a photon and that a circular motion of speed c is transformed into a linear motion of speed c when mass is transformed into energy. The logical expansions of the modification seem to give simpler explanations for basic phenomena and the infinite and eternal nature of the universe.*

In part of section, 2. THE UNIFIED QUANTUM ELECTRODYNAMIC ETHER, of the paper, I wrote:

> I think that Dirac's idea of reintroducing the ether in a modified form [65] has a great deal of merit. A viable theory must operate within the limits of man's psychological limitations. The word "ether" seems to have a more desirable descriptive potential than Einstein's use of the words field, unified field, or energy in describing a unitary physical entity. I think the best name for the entity would be "unified quantum electrodynamic ether" or "dynamic ether" for short.
>
> The dual wave-particle nature of radiation and matter forms the basis of quantum mechanics. The conceptual difficulty of understanding quantum mechanics resides in Born's probability interpretation of the wave nature in terms of the distribution of particles. The wave-particle paradox occurs only if one insists on describing the physical entity as a wave or as a particle. If, on the other hand, one describes the entity as a quantity of a compressible fluidlike ether moving through space, the paradox disappears. [107,109]
>
> A photon's momentum is normally stated as E/c, which is equivalent to mc since $E = mc2$, the average physicist considering the m of the photon as virtual mass which is somehow different from the inertial mass of matter. When a thermal positron and a thermal electron are transformed into two photons moving in opposite directions, the virtual mass of the photons is equal to the inertial mass of the particles, the difference being that the particles had almost no linear motion, while the photons have a linear motion of velocity c. The fact that the center of mass of a particle is at rest does not automatically mean the mass does not have an internal motion. This in essence is the

> flaw in the conceptual basis of the average modern-day physicist; he ignores the obvious, the possibility that a circular motion of speed c of the mass of matter is changed into a linear motion of speed c of the mass of a photon when matter is transformed into energy. The penalties he must pay for ignoring the above possibility are substantial; he must invent inconsistent additional hypotheses such as: (1) The virtual mass of a photon is somehow different from the inertial mass of matter. (2) When matter is transformed into energy, somehow motion is created. (3) Momentum is *conserved* if it is *created* or destroyed in equal and opposite amounts, etc. In order to rectify this situation, I would like to advance current doctrine with the following basic postulate: "An internal circular motion of speed *c* of the mass of particles is changed into a linear motion of speed *c* of the mass of photons when matter is transformed into energy." The following is an attempt to determine some of the possible consequences of this basic postulate:
>
> I.
> The conservation of mass; dynamics ether can neither be created nor destroyed.
>
> II.
> The conservation of momentum; the momentum of dynamic ether can neither be created nor destroyed.
>
> III.
> The equality of action; when two quantities of dynamic ether meet, they both experience an attraction that changes the direction of their motion by an amount proportional to their masses.
>
> If the above three properties are correct, they should describe all physical phenomena in a consistent manner...

In sections 2.1. Photons, and 2.2. Electrons and Positrons, I define the basic equations that form the foundation of Mass Dynamics. In section 3. THE FIRST POSTULATE OF RELATIVITY, I presented Einstein's former research associate's argument [73]:

> In the foregoing, I have pinned the breakdown of the principle of relativity to the background radiation: but this is only by way of emphasis. One can construct local frames of rest also by averaging over the observed proper motions of the surrounding galaxies; the field of direction obtained by this procedure will not deviate grossly from the one gained from observing the background radiation. Either way, permitting large-scale samplings to enter, one is led inexorably to the breakdown of the principle of relativity.

Then in the next section 4. THE SECOND POSTULATE OF RELATIVITY, I presented a short review of the interplanetary radar evidence that the speed of light in space was not a constant of speed *c*. Then in section 5. RELATIVISTIC DILATION OF TIME, I wrote:

> Hafele and Keating [74] have used commercial jet flights and atomic clocks to present convincing empirical evidence that tends to resolve the relativistic clock "paradox." They found that the relativistic dilation of time was a function of the clock's speed relative to an absolute coordinate system at rest relative to the distant galaxies. The clocks that circumnavigated the earth in the eastward direction ran slower than the clocks at rest on the earth's surface by an average of 59 billionths of a second, while the clocks that traveled westward ran faster than the clocks at rest on the earth's surface by an average of 273 billionths of a second.

In the next section 6. THE INFINITE, ETERNAL UNIVERSE, I argued:

> Arp [110] has discovered observational evidence of galaxies joined by luminous bridges that have completely different red shifts, thereby casting doubt on the assumption that the red shift is a Doppler effect. Pecker et al. [111] have presented a photon-photon interaction theory that explains the red shift as an energy loss in which the lost energy goes into a soft photon pair. The transformation characteristics of matter and energy imply the potential of explaining the eternal nature of reality in terms of recycling photons back into matter. The attractive nature of the dynamic ether operating over vast time and distances could transform the energy lost in the red shift into huge columns of dynamic ether. Where these columns collide, energy would be transformed into matter. A likely candidate for such a collision event would be the nearby irregular galaxy *M*-82. A hydrogen-alpha photograph of M-82 taken by the 200-in. on Mount Palomar shows a spectacular array of hydrogen filaments that extend more than 14,000 light-years above and below the galactic disk. Photographs reveal that the galaxy cannot be resolved into individual stars, although at its distance, normal stars should be visible. The light from the filaments is highly polarized, indicating a regular, large- scale magnetic field aligned predominantly along the axis of rotation. It is obvious that conventional thermonuclear reactions are not adequate to explain the phenomenon. [112]
>
> Since the heavier atoms are considered to have evolved from hydrogen fusion, it seems obvious that the age of a galaxy would be proportional to its interstellar hydrogen. Radio astronomers have found that some irregular galaxies have as much as 30% of their mass as interstellar hydrogen. In *Sc* spiral galaxies, the hydrogen content runs as high as 14%, while in *Sb* spiral galaxies, the content is about 1%. In galaxies with little flattening or spiral structure, they have been unable to detect any interstellar hydrogen. [113] Recent evidence shows large amounts of extragalactic hydrogen falling into the spiral arms of our galaxy. [114] The quantity of infalling hydrogen is sufficient to explain the formation of new stars and the spiral nature of the arms. It seems obvious that the hydrogen expelled from an irregular galaxy such as *M*-82 would eventually fall back to the galaxy, forming the spiral arms. The evolution of galaxies would be from irregulars to *Sc, Sb, Sa,* and *E,* finally ending their lives as quasars. The compact starlike nucleus of a Seyfert galaxy is similar to a quasar, indicating the possibility that the quasar is a huge super-massive star that forms from the dense nuclear material of a galaxy, Quasars release far more energy than can be accounted for by known physical processes. From the beginning, theorists have postulated that some form of matter

> annihilation must be involved. [115] The planet Jupiter radiates 2½ times more energy than it receives from the sun and it is impossible to explain the energy generation in terms of conventional theories. The energy generation of stars seems to be proportional to their density. This all seems to indicate the possibility that the dynamic ether orbital structure could be disrupted by sufficient pressure, causing matter annihilation, this being the principal energy source of massive celestial bodies. The quasar would be expected to be an efficient mechanism for transforming the matter in a galaxy back into electromagnetic radiation. The red shift would degrade the radiation and eventually it would be recycled back into matter in an infinite and eternal universe.

I now think that the quasars are globular clusters that form in the dense nuclear regions of a galaxy, rather than single massive stars. The n-body dynamics would suck up the dense material and the pressure mass annihilation mechanism culminates with massive stars exploding as supernovae. [152] The clusters could be expelled from the nucleus by uneven massive gas pressure, and then orbit the galaxies as normal globular clusters. The last two sentences of paper's 7. CONCLUSION, read:

> ...I think the ultimate task of physicists should be to invent the simplest possible consistent unified theory that would fit all known empirical information. The theory would rise in status as it became possible to program advanced computers with the basic equations and the fit between computer readout and empirical information improved.

The sixth paper [82] I've published was in collaboration with Prof. Wilbur Block and Prof. Richard Rhodes II at Eckerd College, and marked the experimental phase of my career as a scientist. The paper also reflected my interest in the electron as the possible fundamental building block of the heavier particles. The paper was published in the prominent journal REVIEW OF SCIENTIFIC INSTRUMENTS, and the title of the article was "Glow discharge source of H- ions."

The seventh paper [83] was also in collaboration with Block and Rhodes, as well as a senior student at Eckerd, Carey Floyd, and the paper was published in the prestigious journal The Journal of Chemical Physics. The title was "Crossed beam electron-electron scattering at 90° and 300 Ev" and the abstract read:

> An extensive search of the literature has revealed no evidence that a primary isolation type experiment such as crossed beam electron-electron scattering has ever been performed at low energies. High energy scattering was first performed by a colliding beam technique at a total energy of 600 MeV in 1966. In the usual cathode ray tubes the density of residual gas molecules far exceeds the density of electrons. An analysis of crossed beam scattering equations revealed that if the electron beams intersected each other at an angle of 90° the energy *E* of electrons scattered in the direction of the c. m. velocity vector could range to as high as twice the primary beam energy *E*. Since electrons scattered from the residual gases would be expected to have energies *E*, it seemed possible to separate the electron scattered electrons from the gas scattered electrons with an energy analyzer. We performed an extensive series of experiments using a parallel plate energy analyzer that revealed no significant results above the

> rather large background count. The experiments showed how difficult it is to detect the scattering with conventional apparatus. We next constructed an apparatus designed to detect almost all the electron scattered electrons that had energies greater than the retarding potential of a grid. The experiments were performed with beam energies of 300 Ev and currents 1.2 and 1.3 µA. The experimental results were compared to predictions based on Mþller's quantum mechanical model for electron-electron scattering. A computer was programmed to numerically integrate Mþller's nonrelativistic c.m. differential cross section equation and the crossed beam equations due to Morse and Bernstein. We found the experimental results to agree well with theory.

My eighth published paper, [66] and the third and last one done in collaboration with Block and Rhodes, was published in The Journal of Classical Physics and was titled "Computer Simulation of Mass Dynamics in Electrons." The abstract of the paper read as follows:

> *Werner Heisinberg contends that modern particle theory is little more than a "super review of particle properties" and that we will not understand the nature of matter until we devise a theory of natural law and boundary conditions defining the dynamics of matter. In order to address this question we have devised an initial computer model of possible natural law that is based on two simple first principles and the equation for mass dynamics. Simulated experiments based on the model give high resolution explanations of the experimental evidence of photon emission at speed c and the 1/r mass distribution of rest and moving electrons. The model also tends to give low resolution first principle explanations of the nature of photon-electron interactions, electron-electron interactions, electron spin forces, gravitational forces, and nuclear forces.*

My ninth, and last research paper [67] to date, was published in the journal Speculations in Science and Technology, and the abstract reads:

> Einstein's dream of a causal unified theory of physics is coming true. The dynamic ether has the potential of explaining all microscopic and macroscopic physical phenomena in terms of simple first principles.

A sampling of some of the highlights of the paper, goes as follows:

> Much of Albert Einstein's life was devoted to searching for a theory that incorporates gravity and other fields into a generalized geometrical structure derived from the general theory of relativity. Peter G. Bergmann collaborated with Einstein on research on this problem and in his paper `Unitary field theories', [116] he gives a brief review of the fragmentary nature and the difficulties inherent in this type of approach... Banesh Hoffmann's paper, `Einstein the catalyst', [117] shows how Einstein's bold and iconoclastic style and his pioneering endorsement of other people's revolutionary ideas influenced many important 20th century physicists. `What of Einstein's refusal to accept as final the indeterminacy probabilistic nature of the quantum theory that he had

done so much to bring into existence? There was a time when it was almost professional suicide for a physicist to raise doubts about the so-called Copenhagen interpretation.' It now appears that the tide has changed in Einstein's favor on this question. In 1951, David Bohm's causal pilot wave theory caused Louis de Broglie to abandon the Copenhagen interpretation and return to his original deterministic philosophy of quantum mechanics. [118] In 1953, Erwin Schrodinger, in his paper, `What is matter?', [119] writes: `Physics stands at a grave crisis of ideas. In the face of this crisis, many maintain that no objective picture of reality is possible. However, the optimists among us (of whom I consider myself one) look upon this view as a philosophical extravagance born of despair.' In 1957, the Soviet physicist V. A. Fock `went to Copenhagen and presented Niels Bohr with a paper in which complementarity was criticized in four different ways: (1) one should insist on the fact that the psi function of quantum mechanics represents *something real*; (2) the presence of precise mathematical laws is equivalent to a certain type of causality; (3) limitations in understanding come only from the use of a classical language; (4) no "uncontrollable interaction" between apparatus and system takes place during measurements. After reading the paper, it is known that Bohr agreed on these four points.' [120] In 1963, P. A. M. Dirac, in his paper, `The evolution of the physicist's picture of nature', [65] writes: `one can make a safe guess that uncertainty relations in their present form will not survive in the physics of the future'. André Mercier reports [121] a conversation with Werner Heisenberg, in which Heisenberg argued `that even major modifications of present physical theories would not transform them into the desired new theory, as quite different and novel ideas are required. Secondly, the impact of quantum theory and relativity theory on the minds of those scholars who helped found them during the first half of our century is conceivably such that they are imprisoned by these theories and thus cannot help but reason conformably, that is, in terms of traditional concepts; whereas the need is for a whole revolution of thought, which can only be carried through by nonconformists.'... There is a popular myth in modern physics that argues that relativity and quantum mechanics are not ether theories. The current publication of the translation of a 1922 lecture by Einstein shows that he developed relativity as an ether theory. [48] He reconfirms this fact in his 1938 book, *The Evolution of Physics*, [20 p.153] and argues that because of the `forced and artificial character of the assumption' he gave up on trying to devise a mechanical model of ether. There are a few enlightened physicists who admit that the `vacuum' of quantum mechanics is really the ether. [122] The problem with the static ether is the fact that it is a solid which if it had the shear modulus of elasticity no less than steel, must have a density less than that of our best vacuum in order to transmit transverse waves with the speed of light. [123] On the other hand, the compressible- fluid-like mass of my c model of mass dynamics [19] is equivalent to a dynamic ether that moves with the physical phenomena, and it is a simple matter to make mechanical models where the elasticity and density are proportional to the phenomena. The concept of a dynamic ether is hardly new. Lord Kelvin developed this type of theory in the middle of the 19th century. It was far ahead of its time, and Maxwell gave it a glowing review. [124]... Our paper, `Computer simulation of mass dynamics in electrons', [66] attacks the mathematics problem of the *c* model by developing a mass-in-cell technique that is similar to the 3D gridless charge cloud-in-cell computer numerical integration method used in plasma simulations. [125]

Figure 1 plots the results from current simulation experiments where each particle is divided into 12 independent cells of radius 2.8 X 10-15 m and the differential mass of the particle is simulated by a computer algorithm that determines the c.m. of the particle and substitutes a centre cell of radius 2X10þ14 m. All cells move at speed *c* and the position of each cell was plotted at 2 X 10-24 sec intervals with 1/2 step integration and calculations at 10-25 sec intervals. The cell surfaces are plotted at their initial starting positions and the elapse time for all but the *(c)* and *(d)* experiments was 1.2 X 10-22 sec which gave slightly more than one rotation of a rest particle. The *(a)* experiment shows the wave pattern that results from a two-cell photon, *(b)* shows a captured one-cell photon moving with the mass flow of a rest electron, *(c)* gives the path of a photon moving through the electron with the mass flow, and *(d)* shows the path of a photon moving against the electron mass flow. All the photon cells had one-tenth the mass of the electron cells. The *(e)* experiment shows the repulsion of two electrons with opposing mass flows in the same plane, *(f)* shows electron-positron annihilation that results from the mass flows coming together from the same direction, and *(g)* shows two-electron repulsion from a head-on collision and the wave patterns of moving electrons. The *(h)* experiment shows positron-electron bonding with mass flows moving in the same direction... The use of independent mass cells can be expensive in terms of computer time. Higher resolution using far more mass cells would be desirable, but calculation time tends to be proportional to *n*2, and it may take massive parallel processing computers to obtain resolution that would result in reasonably good quantitative results... Figure 2 lists a computer program called UNIFIED that introduces the gravitational force as due to a mass cell surface tension that is very small when any mass is immersed within the fluid-like mass of the body of the electron, but tends to approach the magnitude of the Lorentz-type mass flow force when the cell starts to separate from the surface of the electron. The model postulates that the inner radius that determines the rest mass of the electron is similar to the inner surface of a bubble that is held together by the surface tension... the FG values gives the predicted gravitational force in (10-43 N), and the FGCM values give the equivalent force derived from the surface tension characteristics of the mass cells... Both the FLCM and FGCM results are good to within 3 s.f. of the predicted values out to 100,000 (10-16 m) using a PRIME 750 running BASICV at 13 s.f... Figure 3 shows plotted curves of the Lorentz force FL between two electrons moving in the same direction along parallel paths at the same speed that ranged from 0 to 0.9 *c*... The points plot the FLCM *c* model values obtained from the UNIFIED program. Note that at the 10-13 m interelectron distance there is no observable difference between the Lorentz and *c* model predictions, while at the 10-14 m distances one can observe a deviation that occurs for both the Lorentz and gravitational forces when the interelectron distance is within the 1.1 X 10-14 m point where the 50% electron mass distribution distances touch. Analysis of weak decay of hadrons and simulation experiments of test cells through stacked arrays of electrons and positrons lead to the proposal of a neutral pion content of 104 electrons and positrons with mass flow binding energy that could carry spin might tend to explain the ~ 100X strong to electromagnetic interaction ratio... In John S. Bell's paper `On the Einstein Podolsky Rosen paradox', [126] Bell states: `It is the requirement of locality, or more precisely that the result of a measurement on one system be unaffected by operations on a distant system with which it has interacted in the past, that creates the essential difficulty' (for causality). If one follows Dirac's suggestion to introduce non-local hidden variables inside the particles themselves, i.e. drop the point-particle picture, then one opens the possibility of such an action at a distance propagating as phase motion. [127] This is consistent with Louis de Broglie's argument [128] that a particle `could be compared to a small clock', and it is also compatible with the Figure 1 photon *(a)* and electron *(g)* wave patterns. Modern laser interference experiments [129,130] clearly show that the old probabilistic argument that a photon interferes with itself, is untenable. The experiments can be explained, however, by the argument that clock-like photons synchronize clock-like electrons in the interference area, and future photons then interact with the electrons.

The evidence of energy transfer between photons in intense laser beams, [131] the large body of evidence of anomalous red- shifts in galaxies and quasars, [132] and the large-scale filamentary structure of the galaxies in the universe, [133] all tend to support the steady-state model presented in my earlier paper. [19] The *c* model of mass dynamics is probably the simplest possible first principle unified theory that can be devised. It is, I suspect, little more than a first-order approximation to an ultimate model because of the evidence that the speed of light in space is not constant. A $c+v$ model will have to be developed, but because of flexibility of the dynamic ether concept, I do not anticipate any major problems. I feel that this type of approach will lead mankind toward an intimate understanding of the simple microscopic and macroscopic nature of our infinite eternal universe. This is the dawning of the golden age of physics.

My concept of a dynamic ether was not completely original; Few ideas are, most knowledge being built from the work of those who have gone before. A number of prominent scientist have advanced this type of argument in the past, to mention a few that come to mind, René Descartes, Lord Kelvin, and P. A. M. Dirac. I am sure that if I had never existed, others would eventually return to the concept, since it is so simple and self evident. I expect that the scientists of the future will consider the dominant abstract physics theories of our time in much the same light as we now consider the Medieval theories of how many angels can dance on the head of a pin or that the Earth stands still and the Universe moves around it.

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