

OPEN LETTER TO THE SECRETARY OF THE ARMY, HONORABLE ROBERT
FROEHLKE ; A REPORT ON SERIOUS SCIENTIFIC INCOMPETENCY IN
OUR NATIONAL DEFENCE, AS UNVEILED BY THE "SLIDING PHASE"
INCIDENT.

by Dr. Harry E. Stockman, Harvard 1946

This Report , prepared as reading material for Congressmen, and constituting a serious complaint, directed to the Government, suggests that the American National Defence System, as represented by the ARMY and USAF, be checked for ethics in its handling of scientific matters associated with Bid Proposal Evaluations. The ARMY's failure to withdraw , upon my repeated requests, its scientifically faulty Bid Proposal Evaluation of 4-15-70, and to openly admit that its scientific "experts" were grossly in error , seems to violate even minimum requests of ethics. Similarly, the failure of USAF to admit that it was scientifically in error in its Bid Proposal Evaluation of 3-10-70 falls in the same category. Further, the failure of USAF to admit to its deliberate switching of my two introductory technical statements in my "Sliding Phase" Proposal of 2-1-70, thus rendering my Proposal a meaningless technical mess , contradictory beyond imagination, BUT NEVERTHELESS OFFICIALLY "EVALUATED", IS UNQUESTIONABLE LACK OF ETHICS IN ANYBODY'S BOOK. I AM TALKING ABOUT LACK OF ETHICS IN OUR NATIONAL DEFENCE.

I am finding the same grossly erroneous scientific thinking in the original MITRE Evaluation of my "SP" invention, and in the later reviews of my submitted paper by the AMERICAN INSTITUTE OF PHYSICS, AIP, and by the INSTITUTE OF ELECTRICAL AND ELECTRONICS ENGINEERS, IEEE. In spite of the many scientific errors made by these three Institutions, "SP" works and is 100% scientifically correct, as is today verified by Harvard and MIT. "SP" was always scientifically correct, as I always said, however overwhelming the odds were against me.

If the fate of "SP" indicates a degradation of scientific values and ethics in our National Defence, this should be a matter of immediate and great concern for the new Secretary of Defence, Honorable Elliot Richardson. Note that my criticism of the National Defence does not include the NAVY, whose ethics has always been above reproach.

PART A : GENERAL.SCOPE OF REPORT.

This Report gives detailed evidence of gross scientific incompetency, not only in our National Defence, but also in America's two leading scientific bodies, AIP and IEEE. The incompetency pertains to the field of my "SLIDING PHASE" invention, which belongs in the realms of Wave Propagation and Electromagnetic Theory. The question appears whether or not the reported incompetency reveals a more widespread degradation and dilution of scientific values and scientific knowhow in America. If so, the subsequent question appears what can be done about it. One shudders at the very thought that the amateurish scientific mistakes the Government "evaluators" of "SP" are guilty of, extend to the Government's judgement of far more important projects, involving perhaps billions of the taxpayer's money, and pertaining perhaps to projects shielded and protected by high security classification, for which there may not be any justification. Long before I brought up the idea, other people alarmed by the Government's often lavish, and sometimes unwise, and sometimes unjustified, spending of research money, derived from taxation, on war projects, came up with the idea of CLOSE SUPERVISION OF THE MILITARY BY CIVILIANS IN ALL SCIENTIFIC MATTERS CONCERNING OUR NATIONAL DEFENCE. More specifically, when I worked for USAF, 1946-1948, as Chief Communications Laboratory, AMC, Cambridge Field Station, one bright engineer came up with the idea of a CENTRAL SCIENTIFIC SUPERVISION BOARD, founded on the well-known CPA*idea, and staffed COMPLETELY by civilians. The idea was most appropriate already at Cambridge Field Station because of the Air Force use of research money there; a use sharply criticized by two of the five Laboratory Chiefs, and I was one of the two. This "BOARD" must be initiated and officiated by Congress WITH UNLIMITED RIGHT TO LOOK INTO ANY WAR PROJECT IT WISHES TO EXAMINE, INDEPENDENT OF CLASSIFICATION. The "BOARD" scientists must be selected from the cream of the crops of the body of American scientists. There is no doubt in my mind, having seen at close range the Government's wasteful use of research funding for some 25 years, that the very first week of operation of such a "BOARD" would save the taxpayers millions of dollars; just the removal of unnecessary duplication, and unjustified, unnecessary, and harmful classification would save billions. Had such a "BOARD" been in operation at the time I worked at MITRE, 1967-1970, as staff scientist, none of the unbelievable things that happened to the Holography Research Project, and to "SP", could ever have materialized.

To prove that a "BOARD" with very highly qualified scientists can outdo institutions with lesser qualified scientists, or poor scientists, I only have to point to the case at hand, in which the world elite of scientists at Harvard and MIT just shattered the "scientific findings" at MITRE, US ARMY, USAF, AIP, and IEEE for the four winds. More specifically, the surefire statements by incompetent scientists that "SP" did not work, and that my "range nulls" did not obtain, etc., melted like butter in sunshine once Harvard and MIT said what goes. They separated the men from the boys.

In view of what I have always said, and what now Harvard and MIT say, one can begin to measure the depth of the displayed scientific incompetency at MITRE, U S ARMY, USAF, AIP, and IEEE, revealing here and there most unethical actions. It is not a pretty picture, and perhaps we have real reason to worry about the future quality of our National Defence, governing the safety of our country. However, only one infinitesimally small case, that of "SP", has been examined, and it would not be fair to draw general conclusions from this isolated case. Only a thorough investigation by Congress, and one may be started by anyone of the many Congressmen reading this report, can verify

* CPA: Certified Public Accountant

whether or not the black picture from the "SP" incident is an isolated case. Note that MITRE has some of the world's leading scientists in its employ, many of whom are my friends, people even on Nobel-Prize winners level. MITRE has hundreds of highly competent scientists, anyone of whom could have correctly evaluated "SP" in five minutes, but the men chosen for the evaluation lacked the theoretical skill necessary for the evaluation of "SP", although the subject matter of "SP" is not highly advanced; it is on BS level. The "evaluators" failed to grasp the basic principle of "SP", which to no extent inhibited them from issuing a "thumbs down" verdict. U S ARMY and USAF similarly have outstanding American scientists on their payrolls, but I am sure none of them had any control of the "evaluation" results, or these results would have been scientifically correct! By the contrary, I have reasons to believe that both the ARMY and USAF were influenced in their evaluation thinking by the happenings at MITRE; for details see Part C. There is no question in my mind that the ARMY was the innocent bystander, hurt by a locally executed, sloppy, scientifically erroneous, but nevertheless officially issued U S ARMY BID PROPOSAL EVALUATION OF "SP". It was done by two doctors, thoroughly incompetent in the field of my invention. To prove that my principle was wrong, and that my "range-nulls", today verified by Harvard and MIT, did not obtain with the method I described, these two doctors made a mathematical analysis, so thoroughly in error that it was not worth the paper it was written on. They just could not grasp the "SP" principle, which to no extent hindered them from issuing an official Bid Proposal Evaluation. Much worse, this amateurish mathematical derivation was almost line by line similar to the one previously executed at MITRE, and if this was a coincidence, it was the coincidence of the century! Nevertheless, the ARMY leaders are just as guilty as the two doctors and the INSTITUTE FOR EXPLORATORY RESEARCH in Fort Monmouth, which officially issued the "evaluation". The ARMY is 100% guilty until such a day comes when it officially discards and withdraws this faulty Bid Proposal Evaluation, admitting openly that the reason for the withdrawal is that the Evaluation is scientifically in error. This, again, is a request to the ARMY to take such action, clearing the ARMY, and putting the blame where it belongs, on the two doctors who failed so bitterly in knowledge of elementary physics. THE SCIENTIFIC REPUTATION OF U S ARMY AND USAF IS AT STAKE! I have asked a number of Congressmen to follow this development very closely, and you may hear from some of them.

As far as AIP and IEEE goes, the joke in the matter is that some of the scientists who always said "SP" was correct may be members of both AIP and IEEE. Then, how can these two Institutions, with a world-wide reputation in science, be in error? It is very simple. It is because of the unwise action by the AIP and IEEE leaders, speaking for the entire body of members, few of whom ever heard of "SP", and "proving" by means of official "reviewers", badly in need of a refresher course in High-School physics, that the "SP" principle was "wrong" and that the "range nulls" I claim do not obtain. It is by the use of amateurs instead of professionals as reviewers in the field of my "SP" invention that AIP and IEEE arrived at grossly erroneous results, PROVEN TOTALLY ERRONEOUS BY THE HARVARD AND MIT EVALUATIONS! I prepared and submitted dozens upon dozens of pages, showing in detail the errors made by the "reviewers", but neither Institution was willing to admit to these errors, or acknowledge the fact that the "reviewers" were incompetent in the quoted field of physics. They both refused to act on my constant demand for a competent reviewer. It is by the unwise refusal by the top leaders of AIP and IEEE to admit the erroneous evaluation of "SP", and to submit my paper to a competent reviewer, who in the light of the Harvard and MIT findings would have found "SP" scientifically correct, that the two Institutions prove themselves scientifically incompetent in the field of my invention.

THE ISSUE.

The year 1969 saw the initiation of one of the most debated scientific matters in recent time; that of "SP", today the subject matter of Technical Monograph No.26, issued by the SERCOLAB COMPANY, P.O. Box 78, Arlington, Mass., 02174, and available for 50 cent. "SP" led to heated arguments in many quarters, with celebrated scientists with a good background in physics saying that indeed I obtained the "range nulls" I claimed, and equally celebrated scientists who were somewhat shaky on their physics fundamentals saying just the opposite. A typical case is MITRE's Theoretical Physicist, Dr. Saul Bergmann, who claimed that my "SP" principle was right as long as I worked at MITRE, but upon my retirement in 1970 claimed precisely the opposite; that my "SP" principle was wrong and that the "range nulls" I claimed with my method did not obtain. As "proof" he quoted this and that basic law and theorem in physics. Such incompetency in physics is not a recommendation for MITRE in as much as Dr. Bergmann stated he was the only Theoretical Physicist at MITRE, THE MITRE Theoretical Physicist. In view of the evaluations by HARVARD and MIT, Dr. Bergmann is totally and completely in error.

During my employment as Staff Scientist at MITRE 1967-1970, I constantly introduced my "SP" invention FOR THE NATIONAL DEFENCE. I never asked for any compensation, I just tried to get research work started on "SP", well knowing that once such research work was started, one application after another would be found. In the process of trying to create an interest for my invention, I found alarming incompetency at MITRE in the field of Wave Propagation and Electromagnetic Theory, in spite of the fact that MITRE under its roof probably has as many, or more, real experts on electromagnetic theory as any similar institution in the world. These real experts, however, were not linked into the chain of events leading to the "evaluation fiasco" in the Office of the Director for the Communications Division. It all begun in 1969, when I took advantage of the yearly presentation of scientist's ideas to the famous MITRE RESEARCH COMMITTEE, which included many outstanding experts, including experts in the Wave Propagation and Electromagnetic Theory field. I issued a most official Memorandum No. D91-M-361, dated 9-30-1969 of official subject: SUBMISSION OF FUNDING REQUEST ON "SLIDING PHASE" TO THE RESEARCH COMMITTEE. It could not have been more direct and to the point than that. It was directed to my Department Head, Dr. Herman Blasbalg, and referred to an earlier Memo D91-M-341 of 9-19-69. The Department Head bluntly refused to introduce my "SP" proposition in the Research Committee on basis that the principle was in error; this was the same principle that later Harvard and MIT found correct. He claimed that my method did not produce the "range nulls" I claimed, and he stated that he could prove mathematically that this was the case. He gave me an appointment in his office, and started to write out line after line of a mathematical analysis, which, however, was totally in error! It turned out that he had just not grasped the basic idea of "SP". For every equation he wrote down, I told him exactly what mathematical and other errors he made, but he was so unfamiliar with the subject matter that he did not understand my corrections. After having finished the "analysis", proving his incompetency in this field, he ignored my technical objections, pointed to his erroneous equations, and without blinking an eye concluded that here was the "proof" upon which he based his decision not to forward my invention to the MITRE Research Committee.

I thought this would be a very easy matter to have corrected, and approached the Department Head's superior, the Communications Division Director Mr. Lewis Billig. He scolded me for criticizing a Department Head, sided with him technically, and bluntly refused to give "SP" any further consideration with reference to the pending meeting in the MITRE Research Committee. It became evident during our conversation that the Division Head was even more

incompetent in this field than his Department Head. His refusal should be seen against the fact that there were some hundred scientists right in the Communications Division, entirely competent to evaluate "SP" correctly, so it would have been a very simple matter for the Director to obtain a correct review, but then, of course, the embarrassing scientific incompetency of the Department Head would be verified, and stand out in big letters for everyone to see. So, the Director choose not to correct the scientific error! Right in the very corridor at MITRE where I worked, there was a dozen of scientists who could have evaluated "SP" correctly in a matter of minutes.

At this time I had issued a large number of official Memos on "SP", many through the MITRE PATENT COMMITTEE, proposing that MITRE make an official scientific evaluation of "SP"; one of these Memos being sent to the Division Director, asking for such an evaluation. Ultimately, the date for an OFFICIAL DIVISION EVALUATION was set, but when I appeared at this Evaluation, held 10-3-69, I found to my very great surprise that the Director had not chosen the two Evaluators from the ranks of MITRE scientists, distinguished scientists working in the field of physics covering my invention, and publishing in this field, but instead from the ranks of MITRE Department Heads, with extensive administrative functions. More specifically, Department Heads working under the direct supervision of the Director, and therefore not very likely to go against him in a scientific matter where the Director had already stated that the scientific principle to be evaluated was wrong. Neither evaluator had the necessary background in physics to evaluate "SP", something which became extremely clear once the session got under way. Also present was Dr. Sauk Bergmann, not belonging to the Communications Division, but the Director told me that he did not carry any vote. The Evaluators were Department Head "L" (Donald Levine) and Department Head "D" (David Dettinger). Both had earlier failed bitterly in the investigation or evaluation of another research project; MITRE RESEARCH PROJECT No. 9903, my previous assignment, a HOLOGRAPHY PROJECT. Although both had been appointed for the investigation, it turned out that neither one had sufficient background in Holography to judge this rather special and theoretical field of science. This did not hinder them in any way from formulating devastating and scientifically fully erroneous conclusions about the obtained research results, none of which they understood. As an example, the "Wild West", highly unethical investigation or evaluation of the Holography Project by Department Head "D" reflects most unfavorably on MITRE research, and resulted in the official MITRE Memo D90-M-688 of 9-19-69, verifying that Department Head "D" had been given this assignment most officially by the Communications Division Director, Mr. Lewis Billig. My objections to MITRE to authorize an incompetent man to evaluate years of research results on this project, some of them contributed by Dr. Zarwyn, and some published in Proc IEEE and in other journals, were ignored. This black page in MITRE's research history, an outright "rape" of research and research values, occurred just two weeks before the very same Department Head "D" became appointed as "evaluator" in still another field of research, "SP", about which he knew very little. Note: there was a witness present during the Holography Project "evaluation". This unfortunate Holography Project suffered for a long time under the incompetency in the field by the preceding Director for the Communications Division, and I once made an estimate that much time and about 60% of the running project expencies could be saved if Dr. Zarwyn (who headed up the project before I took over) and I would be allowed to do the research right instead of being forced to follow MITRE's High-School level physics research "directives". When Mr. Billig, who knew still less about Holography research, started to add his totally unacceptable research "directives", Dr Zarwyn left the MITRE installation, and I can't say that I blamed him. I HEREWITH CALL DR. BERTHOLD ZARWYN AS A WITNESS TO EVERYTHING SAID ABOVE! I quote MITRE MATTERS No. 15, 8-5-70, where Dep. Head "D" was written up: "he wasn't cut out to be a theoretical physicist". But at MITRE he was competent enough to judge advanced physics. See Part C.

PART B: PROOF OF THE SCIENTIFIC CORRECTNESSOF THE STOCKMAN"SLIDING PHASE"PRINCIPLE.

To save space I am only including the most important documents and I am only quoting the most essential part of each document.

B1. DR. F.H. REDER, U.S. ARMY.

dated 11 Aug. 1971

This is an official letter, from "Department of the ARMY, Headquarters United States ARMY Electronics Command, Fort Monmouth, New Jersey, 07703", signed by Dr F.H. Reder, Actg Chief, Antennas and Geophysical Eff Rsch Tech Area U.S. ARMY Electronics Tech and Devices Lab. Dr. Reder is one of Americas most outstanding scientists in the field of my "SP" invention, which is Wave Propagation and Electromagnetic Theory. Dr. Reder has a world reputation as a scientist, and is equally well known as a truly outstanding scientist in Europe. His contributions to science through many years of R/D work are truly outstanding. Every word above is true and can be verified.

Quote:" There is, of course, no doubt that your sliding phase method produces range nulls for the equipment set up you propose and if the propagation medium is dispersive. -----The employment of a frequency translator at the receiver converts the situation of 2 waves of about equal amplitude, different frequency and slightly different phase velocities (due to dispersion) to the desired combination (for producing range nulls) of 2 signals of equal amplitude, equal frequency and slightly different phase velocities."

Note that Dr. Reder received the same paper that earlier constituted my official Bid Proposal, dated 3-19-1970, and sent to U.S. ARMY, who trusted the ARMY INSTITUTE FOR EXPLORATORY RESEARCH, Ft Monmouth, with the official evaluation. The Institute bitterly failed the ARMY's confidence in its professional knowledge in this field, and produced a thoroughly erroneous mathematical "analysis", signed by Dr. Ballard and Dr. Goubau, as discussed else where in this report.

B2. DR. DAVID DEWITT, IEEE, IBM.

Dr. DeWitt is an outstanding and well known scientist, and the only introduction he needs is the version presented here of his strikingly brief and direct analysis of "SP", and the sharpness of his mind allowed him to "hit the nail on its head" and correctly describe my "Range Nulls" WITHOUT making the same embarrassing pitfall as Dr. Ballard and Dr. Goubau made. Dr. DeWitt is a former editor of the IEEE SPECTRUM. His letter is dated 1-24-1972.

Quote : " $E(r, t) = A(r, \omega) \sin \left[\omega \left(t - \frac{r}{v(\omega)} \right) + \phi \right]$

If it were possible to have available at r two signals both at the same frequency but transmitted at v_1 and v_2 They would differ in phase by $\omega r(1/v_1 - 1/v_2)$, and what I believe you mean by range nulls

would occur in increments of r equal to

$$\left| \frac{2\pi}{\omega \left(\frac{1}{v_1} - \frac{1}{v_2} \right)} \right|$$

..... the E_1 would have been effectively transmitted at v_1 even though its frequency is ω ."

Dr. DeWitt uses a single trigonometric form, while Dr:s Ballard and Goubau use a double term trigonometric form, which is immaterial. Similarly the two doctors talk about "intervals of x ", but here ends the similarity, with Dr. Ballard and Dr. Goubau taking off on an erroneous path. While Dr. DeWitt absolutely correctly acknowledges the receiver frequency conversion, and accordingly comes out with ONLY ONE RESULTING ANGULAR VELOCITY ω , precisely as Dr. Reder above reasoned, Dr Ballard and Dr. Goubau blindly stumble on with indices "1" and "2", even in their last equation, totally ignoring the heart of my "SP" invention, which is the receiver frequency conversion. Misled by their faulty mathematics, the doctor's draw the false conclusion that my nodes, or "Range Nulls" are "not stationary", when in reality they stand still, within the accuracy of the used equipment, and the constancy of the index of refraction. The result they arrived at can never be interpreted any other way than being totally in error, since they add another paragraph, still more scientifically erroneous, saying that "Stationary nodes are only obtained if the two waves have the same frequency but are propagating in different directions". Well, my waves do not propagate in different directions, but I surely obtain stationary nodes in spite of the fact that the two doctors claim that I cannot.

B3. LINCOLN LABORATORY.

In a letter of 2-1-1972 this IEEE editor, M.I.T. Lincoln Laboratory, states: "..... your letters are not only candid but in many ways accurate. After a thorough review of your paper, I find that it is technically correct"

B4. HARVARD UNIVERSITY: My letter to the President. Dr. D.C. Bok, asking for an evaluation, was dated 11-30-1971. The reply from the President, dated 2-14-72, includes the statement: "My impression from colleagues is that your method is basically correct" I was then authorized by Harvard University to see the official evaluation, *made by a selected Harvard scientist. He is an expert in this field of such high status, and with such enormous contributions to science, that his findings cannot be questioned by anyone, anywhere. I shall only quote a couple of statements from the two page document: "I should state clearly that the effect described by Dr. Stockman does exist addition of the two signals will yield a signal whose amplitude varies cyclically with distance from the transmitter, having Stockman's "range nulls" at those distances where the two multiples are in opposing phases My conclusion, then, is that the Stockman proposal is technically correct"

* dated 1-14-72

B5. M.I.T. My letter to the President Dr. Jerome Wiesner, asking for an evaluation, was dated 11-30-71. It is well known that the President himself is an outstanding scientist. The actual evaluation was done by Dr. L.D. Smullin, Head of the E.E. Department; an outstanding scientist, too well known to require any introduction. In his reply document, Dr. Smullin first acknowledged the assignment by Dr. Wiesner, and all I need to quote from Dr. Smullin's technical statements is the following: "..... If the main point is the production of range nulls, we must agree with you. letter to you on 11 August 1971 from F.H. Reder says the same thing." There is no need for anyone to say any more than this. * dated 1-27-1972,

Anyone who is trying to uphold earlier destructive, amateurish, grossly erroneous statements that my "SP" method does not work, and that my "Range Nulls" do not obtain, should forever hold his tongue. This advice is directed to: USAF, MITRE, U.S. ARMY, American Institute of Physics, and IEEE.

PART C: ADDITIONAL INFORMATION.

HOW U S ARMY GOT INTO TROUBLE.

I have described in Part A how Dr. Blasbalg at MITRE wrote out a mathematical analysis to disprove "SP"; an analysis with many serious errors. He never issued it as an official Memo, although I challenged him to do so. Although Dr. Zarwyn had left the installation, I maintained contact with him, and as an example we had a publication on Holography in Proc. IEEE (April 1968). When Dr. Zarwyn learned about the troubles that I submit my "SP" principle acknowledged as correct at MITRE, he suggested that I submit my Bid Proposal to U.S. ARMY, more specifically to the INSTITUTE FOR EXPLORATORY RESEARCH in Fort Monmouth. At this time, Dr. Zarwyn apparently believed that this Institute was highly competent in the field of physics in which "SP" belonged, since he assured me that this was the case; he said my Proposal would be judged by real experts. Dr. Zarwyn, then in the ARMY, must have been very much surprised to learn later that these "experts" were incompetent in the field of my invention, because the official Bid Proposal Evaluation issued by the Institute was all in error. The salient point right here is, however, that the mathematical analysis constituting the Institutes's "evaluation" is a dead ringer for the above mentioned MITRE "analysis", made by Dr. Blasbalg. The "evaluators", Dr. Ballard and Dr. Goubau, wrote equation after equation, line after line, similar to those Dr. Blasbalg had written at MITRE, and equally erroneous. Now the two doctors claim that they came up with the analysis themselves (which makes their case still worse) and did not follow any Blasbalg analysis. If so, this is the coincidence of the century. If the faulty Blasbalg analysis was transmitted to Fort Monmouth, there were numerous ways in which this could have been done, because this was half a year later, and many people travelled all the time between MITRE and Fort Monmouth; one of those a fellow by name Harry E. Stockman. I spent considerable time on synchronization and timing problems, and many people knew about my "SP" principle, and many of those, but not all of them, had been given the illusion that it was wrong. Among those who wanted to know more about my system was the OMEGA people, and I have an inquiry from them dated 6-3-69, addressed to Dr. Blasbalg. From my discussions with these people I learned that they knew everything there was to know about transmission in dispersive

media, and, indeed, recently something has come out of all this. In the future OMEGA Global Rescue Alarm System, the solution may be contributed by DISPERSION; the very tool I am using in "SP". Another communication that proved how widely known "SP" had already become came from, you guessed it, Fort Monmouth, dated 9-24-69, signed by Dr. F.H. Reder. He referred to standard MITRE business, but also expressed interest in "your new system", and set the date of 10-14-69 for a meeting with him in Fort Monmouth. When I arrived, Dr. Reder had been called away, but was represented by Dr. Hafner, another ARMY scientist of great scientific reputation. In the second-day session, 10-15-69, after the formal MITRE business had been concluded, I gave a briefing of "SP", and from questions asked by Dr. Hafner, it was evident to me that he grasped the basic principle of "SP" almost instantly. In this briefing I specifically pointed out the pitfalls some people encountered in "SP" (without reference to MITRE). So in view of this briefing alone, the correct principle of "SP" was quite well known to the top scientists at Fort Monmouth, but nevertheless just a few months later the official "evaluators", Dr. Ballard and Dr. Goubau totally failed to understand the simple principle of "SP", thus producing a mathematical "analysis" not worth the paper it was written on.

THE FAULTY ARMY "EVALUATION".

Let me describe in more detail the faulty ARMY Bid Proposal Evaluation document, issued as a bona fide document by the U S ARMY INSTITUTE FOR EXPLORATORY RESEARCH, Fort Monmouth, under Dr. Ballard. This entire transaction was initiated by and handled by Dr. Berthold Zarwyn, with a view of aiding me in obtaining the qualified scientific judgement I was seeking. There fore, it is only natural that the first of the two forwarding letters accompanying the Bid Proposal Evaluation was issued by Dr. Zarwyn. Dated 5-13-1970, this forwarding letter reads: HEADQUARTERS UNITED STATES ARMY MATERIEL COMMAND, WASHINGTON DC, 20315, AMCRD-TF, addressed to me, and transmitting the second forwarding letter, signed by the DIRECTOR FOR THE INSTITUTE FOR EXPLORATORY RESEARCH, Dr. G.E.H. Ballard, followed by the one page BID PROPOSAL EVALUATION proper, signed by Dr. Georg Goubau. In his first forwarding letter, Dr. Zarwyn does not at all state that I am wrong and Ballard/Goubau right, by the contrary, he asks for my comments. Dr. Ballard's letter is dated 4-15-70, and starts with the specification "AMSEL-XL-D (19 Mar 70) 1st Ind". Then comes a very confusing line referring to the "COMMERCIAL PATTERN DISCLOSURE". This is where the sloppiness characteristic for this unfortunate Bid Proposal Evaluation begins. To conclude the description of this three-page "evaluation" package, the first page is signed: BERTHOLD ZARWYN, Ph.D. CHIEF, TECHNOLOGICAL FORECASTING BRANCH, RESEARCH DIVISION, RESEARCH, DEVELOPMENT AND ENGINEERING DIRECTORATE. The last entry on the third page is: "GEORG GOUBAU, CHIEF, EM TRANSMISSION RESEARCH AREA, IER."

To begin with, I never submitted a Bid Proposal that even mentioned "Commercial" and "Pattern". These are inventions by the Institute, and apparently, the Bid Proposal Evaluation was never proof read! This is evident from the first line in Dr. Goubau's "analysis" where "commercial pattern" mysteriously becomes "commercial patent", although my Bid Proposal had nothing to do with any "patent". Perhaps if the Institute had proofread its "Evaluation", "commercial" would have become "communications", which is the term I used. Since Dr. Goubau repeats "patent" at the bottom of his page, it is one good guess that "commercial pattern" in a proofread edition would have been "communication patent". Dr. Ballard states: "DR. GOUBAU, AS A RECOGNIZED EXPERT IN THE FIELD, IS EMINENTLY QUALIFIED FOR SUCH A JUDGEMENT" There are two ways of interpreting this, in view of the serious mathematical errors Dr. Goubau makes on his "analysis" page, (1) either it is not true that Dr. Goubau is "eminently qualified for such judgement", since the Harvard/MIT evaluation proves that he is dead wrong, or (2) the Institutes knowledge in this field is so poor that anyone who can write a bunch of equations, even if all of them are wrong, is "eminent".

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Ballard proves one thing beyond any doubt; that neither one of the two doctors was able to grasp the simple principle of my "SP" invention. The fact that Dr. Goubau's mediocre "analysis" was heralded in the Director's forwarding letter as something exclusive, a peak performance in science, is a slap in the face of every competent ARMY scientist; gross scientific incompetency in the field of my invention covered up by "carnival barking" in a forwarding letter. Another reason why the ARMY should disqualify this entire document. Let us take a closer look at Dr. Goubau's totally erroneous "analysis"; the heart of the "evaluation". Dr. Goubau makes the mistake of referring to ω as a "frequency". It is not a frequency, it is an angular velocity. Perhaps this becomes more clear to Dr. Goubau if he considers a phasor which does not quite describe a first revolution; then there is no frequency concept at all. Technical slang should not be used in a mathematical analysis intended to disprove a new principle in physics. On the second line it says DISPERSIVE MEDIA in other words, the author was fully aware from the very beginning that "SP" occurred in a dispersive medium. This is very important since attempts have been made to whitewash Dr. Goubau, stating that his analysis is correct because it applies to non-dispersive media. It does not! The analysis is line for line the same as Dr. Blasbalg's, and equally erroneous! As an example, the author of the analysis fails to introduce the vital relation between ω_1 and ω_2 , except that $\omega_1 > \omega_2$, which is immaterial since one can also make the opposite assumption. ALREADY IN HIS VERY FIRST EQUATION, DR GOUBAU PROVES THAT HE NEVER GRASPED THE "SP" PRINCIPLE BY FAILING TO INTRODUCE A FIXED RELATIONSHIP BETWEEN ω_1 AND ω_2 , SUCH AS $\omega_1 = k\omega_2$, SUCH THAT IN THE RECEIVER ω_1 VIA THE MULTIPLIER $(1/k)^2$ CAN BE REDUCED TO ω_2 , SO THAT THE HEART OF MY INVENTION, THE RECEIVER INTERFERENCE PATTERN, CAN BE OBTAINED. There is not even the most remote hint of an interference pattern in Dr. Goubau's grossly misdirected "analysis". Instead, Dr. Goubau stumbles on to the grossly erroneous findings that my "nodes" are not stationary, WHILE HARVARD AND MIT SAY THAT THEY ARE: PRECISELY THE OPPOSITE SCIENTIFIC FINDING TO THAT OF DR. GOUBAU whose mathematical "analysis" is a disgrace to the ARMY, and should be disqualified. An additional reason why this should be done is that Dr. Goubau claims on line 3 that my principle is based on a "misconception" and then states "as shown by the following analysis". The fact that the Goubau "analysis is totally in error, as I have shown above, proves that if there was any "misconception", this "misconception" consists of the Goubau "analysis".

Due to my violent complaints to the ARMY about the Institute's most official but also most erroneous Bid Proposal Evaluation, I was granted a hearing in Washington at the HARRY DIAMOND LABORATORY, with all my expencies paid. The hearing was most officially held 2-2-71. It was quite formal and included a dozen of people, top ARMY executives as well as top ARMY scientists. With one exception, I have no complaints with reference to this meeting, which was run in a most excellent manner, and lasted for hours. I was particularly pleased that as soon as the meeting got under way, the participants agreed with me 100% that my "SP" principle was correct, and that the "range-nulls" I claimed with my system indeed obtained, although there were many questions. My single complaint is that the meeting did not extend to discuss the Fort Monmouth "evaluation"; I was told that the meeting agenda did not include the question whether Dr. Ballard and Dr. Goubau were right or wrong in their submitted "evaluation". Naturally, the ARMY has the full right to make up the kind of agenda it approves of for a meeting, and then stick to it, but to me the entire meeting was a waste of time, since the proof of the scientific incompetency of the two doctors was not made official; it was my scientific standing against that of the two doctors, and apparently, the official ARMY viewpoint was still that they were right, in spite of this meeting, which said precisely the opposite to what the two doctors said in their "analysis", but did not mention the doctors' names. This meeting, therefore, was a total

failure AS FAR AS THE DISQUALIFICATION OF THE FAULTY ARMY BID PROPOSAL EVALUATION WAS CONCERNED. It is hoped the ARMY will do better this time, since this time the ARMY's scientific reputation is really at stake. If the ARMY continues to offer protection to the two doctors, who do not deserve it, the general conclusion amongst Congressmen and others will be that the ARMY was wrong THANKS TO THE EXISTENCE OF THE HARVARD/MIT EVALUATION. I did receive a confirmation of the meeting and the meeting results, but it might just as well have remained unissued since it carefully avoided the acid question about the faulty Bid Proposal Evaluation.

WHITE-WASHING ATTEMPTS.

The Ballard/Goubau "evaluation" did a lot of harm to my case, since non-technical people, such as Congressmen, could not possibly believe that the glorious ARMY INSTITUTE OF EXPLORATORY RESEARCH could be wrong in a mathematical analysis on Freshman physics level. The authority of the ARMY's INSTITUTE was so great that I was told more than once to "admit" that I was wrong, because "this Institute is too advanced in science to make petty mistakes of the nature claimed". In everybody's book, except for a handful of scientists, I had lost the case, and this has been the situation up to the day when the HARVARD/MIT EVALUATION appeared. The U S ARMY ought to have a bad conscious, and should today try hard to make up for the harm it once did. Apparently, it became one of America's favored indoor sports to get the two doctors off the hook SOMEHOW, and I mentioned some attempts in the earlier text. The most serious attempt to turn black into white goes as follows. Thoroughly justified from an educational point of view, I used "standing waves" in quotation marks, to describe BY ANALOGY, the "nodes" I obtained. Nowhere did I say that "SP" IS a standing wave phenomenon, or even associated with standing waves. The analogy is well justified, because in both systems, "nodes" indeed obtain, although of different nature. Now, the white-washing claims (and there have been several) state that, mysteriously, if it was "standing waves" that the two doctors evaluated, instead of the "SP" principle then their "analysis" is right. This is completely in error. NOTHING ON EARTH CAN MAKE THE INSTITUTE'S FAULTY "ANALYSIS" RIGHT. First of all, if the two doctors analyzed "standing waves" instead of the assigned subject matter of "SP", they did not follow the given assignment. If they copied a textbook page about standing waves instead of evaluating "SP", they deliberately deviated from the given assignment, which was to evaluate "SP". It is further clear that Dr. Goubau by no means was analyzing "standing waves" from Dr. Ballard's forwarding letter of 4-15-70, which says on line 8 : " THE COMMERCIAL PATTERN DISCLOSURE WITH YOUR LETTER OF 19 MARCH 1970 HAS BEEN REVIEWED BY DR. GEORG GOUBAU, CHIEF, ELECTROMAGNETIC TRANSMISSION AREA, INSTITUTE FOR EXPLORATORY RESEARCH." So, this proves that indeed Dr. Goubau was analyzing "SP" and nothing else. In their concluding remarks, the two doctors mention the textbook concept of standing waves PROPAGATING IN DIFFERENT DIRECTIONS, so suppose for a moment that I would go along with the idea that the two doctors were analyzing "standing waves" instead of "SP", then I find that they jumped from the frying pan into the fire. This is so, because on line 6, over Dr. Goubau's signature, they burn this remaining ship by stating that THE TWO WAVES PROPAGATE IN THE SAME DIRECTION. You can play semantics to the hilt, but you can never hide the fact that the two doctors NEVER UNDERSTOOD THE PRINCIPLE OF "SP", AND THAT THEIR "ANALYSIS" IS SCIENTIFICALLY IN ERROR TO SUCH AN EXTENT THAT IT IS NOT WORTH THE PAPER IT IS WRITTEN ON. This is why it now has become a matter of scientific prestige for U S ARMY to withdraw this unfortunate Bid Proposal Evaluation on basis that it is wrong. I have asked a large number of Congressmen to watch how this situation develops. There has been accepted standard of ethics in America, since the days of Benjamin Franklin, that when you are scientifically in error, and the error is pointed out to you, you admit to it! Not so the two doctors. In a letter

dated 5-18-70 to Dr. Ballard, I described on three pages in the greatest detail the scientific errors made by Dr. Goubau. In a subsequent letter of 12-12-70, sent through AMC, U S ARMY, I openly challenged the Director of the Institute, Dr. Ballard, to disprove my charges of a scientifically faulty Bid Proposal Evaluation in a document submitted DIRECTLY TO THE ARMY LEADERS IN WASHINGTON, with copies to Dr Zarwyn and I, or, as the only possible alternative, ADMIT IN WRITING THAT HE AND DR. GOUBAU WERE GUILTY OF THE SCIENTIFIC ERRORS I HAD POINTED OUT. There was no answer; only silence. Is there any further need of proofs? Of course, at that time Harvard and MIT were not yet in the picture, but today, thanks to Harvard and MIT. THE TWO DOCTORS HAVE BEEN PROVEN WRONG, and it makes little difference whether they pretend to be right. The thing that has not been done yet is the official ARMY withdrawal of the faulty Bid Proposal Evaluation, with the officially given explanation for the withdrawal that the Bid Proposal Evaluation is scientifically in error.

THE USAF FAILURE.

After having encountered a number of MITRE's "Experts" who for their lives could not grasp the basic "SP" principle, I submitted an Official Bid Proposal document, dated 2-1-70, to USAF. It was sent to the nearby Hanscom Field, with which MITRE had extensive exchange of information. Having worked at Cambridge Field Station, now moved to the Field, and having visited the Field many times for MITRE, I knew many people at AFCRL and at other units. I even gave a briefing on HOLOGRAPHY there. Again I stressed that my Bid Proposal was submitted for our National Defence. While AFCRL has some of the world's best scientists in its employ, right in the center of my "SP" invention, it soon became evident that none of those had had any control over the issued Bid Proposal Evaluation, dated 3-10-70, or this Evaluation would have been correct! This was the date of the forwarding letter, signed by Colonel D.J. Flinders. The Evaluation was signed by Mr. C.J. Sletten, and dated 3-5-70. The Evaluation was totally in error and represents a disgrace to USAF. From the very first page the aim was clearly to denounce and disprove "SP" as an invention, at any cost, including nonethical means. Both Colonel Flinders and Mr Sletten were MITRE visitors, and as an example, during an earlier visit by Mr. Sletten, I gave him an extensive tour of the Holography Laboratory. The names of the real authors of the Evaluation were, however, withheld, and USAF steadfastly refused to give their names. In the Evaluation they were referred to as "we", "us", and "ours". Specifically, I tried via Congressmen to find out if anyone of "we", "us", and "ours" worked outside of USAF, and if so where. At least two different typewriters were used in the typing of the Evaluation, and there were three different kinds of English used. Many of the objections stated against "SP" had been stated earlier at MITRE, perhaps in different wordings. None of this proves any participation by MITRE in this Bid Proposal Evaluation, although it is quite a coincident that certain objections against "SP" came up here in about the same way as they earlier entered in at MITRE. The Evaluation has many technical errors, and is highly contradictory between different paragraphs, probably the result of the use of several "Reviewers"; one in making his point not bothering to read what another "Reviewer" had already stated. It is a poorly composed document. For details, see my extensive Rebuttal of 3-20-70.

Had my submitted version of my Bid Proposal of 2-1-70 been the one evaluated, it would have been very difficult even for a highly biased "evaluator" to deny the later Harvard/MIT finding that my "SP" principle was right. BUT MY SUBMITTED BID PROPOSAL WAS NOT THE ONE EVALUATED! To make sure that no one could make technical sense out of my Proposal, MY TWO INTRODUCTORY TECHNICAL STATEMENTS WERE SWITCHED, DELIBERATELY INTERCHANGED, SO THAT A NUMBER OF THE FOLLOWING TECHNICAL STATEMENTS I MADE BECAME TOTALLY ERRONEOUS. USAF has constantly ignored the many attempts I have made through Congressmen like Edward M.

Kennedy to bring this matter out in the open, with the USAF, (1), admitting to the deliberate switching, and (2), explaining why the switching was done, giving the name of the person who did it. There is not the most remote possibility that the switching was done by accident. Admittedly, a group of "Reviewers" were at work ("we", "us", "ours"). In the very act of "reviewing", each individual must now and then look back upon my initial technical statements, or he would not know what the technical statement he is deliberating on. So, for the reason of an argument, let us say that four "Reviewers" were at work. Assume that each one looked back ten times on my initial technical claims. That makes 40 look backs! SO, 40 CONSECUTIVE TIMES THE "REVIEWERS" READ MY TWO INITIAL TECHNICAL STATEMENTS UPSIDE DOWN; NOT ONCE DID A "REVIEWER" READ THE PLAIN ENGLISH IN THE ORDER IT WAS TYPED! The probability for this is about 1 to infinity. I don't think any court on earth can become convinced that the switching was an accident. SOMEONE INSIDE OR OUTSIDE OF USAF DID THE SWITCHING DELIBERATELY FOR THE PURPOSE OF DEFINITELY RUINING MY PROPOSAL, SINCE ALL REQUIRED, OTHERWISE, WOULD BE A COMPETENT REVIEWER, AND, BINGO, HERE IS THE HARVARD/MIT EVALUATION, OBTAINED ALREADY IN 1970. Without accusing MITRE, I wish to draw attention to the fact that MITRE would have lost scientific prestige had USAF found, just a matter of months later, that the MITRE "finding" that "SP" was wrong, was wrong. Indeed, everything about "SP" was right; MITRE just never understood how "SP" worked. One must not draw the conclusion that someone from MITRE did the switching. Some friend of MITRE may have done it. But it surely is a very queer thing. Anyhow, with the switching done, affecting several pages of my Bid Proposal, there was enough technical contradictions for "SP" to be thrown out, and it was thrown out. This unethical handling of an officially submitted National Defence Bid Proposal is a scandal of no small proportions in our National Defence. It might well have shaken up the Air Force if appearing at some politically inopportune time. So far USAF has dodged this scandal BY THE VERY SIMPLE TACTIC OF TOTALLY DODGING THE SUBJECT MATTER, EVEN WHEN THE INQUIRY CAME FROM A CONGRESSMAN, WITH SOME CONGRESSMEN TRYING MOST SINCERELY TO FIND OUT THE TRUTH IN THIS SERIOUS GOVERNMENT MATTER. I have many replies from the top USAF generals and executives in Washington, totally dodging the switching issue, but telling me how THOROUGHLY the matter has been investigated, proving there is NO FOUNDATION for the made allegations, etc. I conclude that the "switching" must have been investigated at least five times by different authorities in USAF (including the legal ones), but mysteriously, everything else is discussed, except the "switching". This indeed is a sore point with the USAF; an "Achilles' heel". One does not need any super intelligence and to be a Philadelphia lawyer to figure out that USAF is square GUILTY. But it will not admit its guilt! In my Rebuttal of 3-20-70 I spent 5 (five) pages on the switching issue alone, showing in the greatest detail JUST HOW THE SWITCHING WAS DONE. Senator Kennedy was very helpful; he sincerely tried to help me, writing many letters. From this and other experiences I learned, however, that the individual Congressman is powerless against the USAF. He just gets one or more of these smooth whitewashing letters from the USAF, whereupon he forwards them to me, and that is the end of the line. In my humble opinion, the only way a Congressman can get anywhere against the USAF is to get the subject matter officially entered in one of the Congressional Committees, with a strong charge of unethical handling of a Bid Proposal by USAF. But unless Congress itself is involved, I would not be surprised if USAF would just chose to ignore the charge, just as it ignored, in the same meaning, the inquiry from Congressmen.

Whether or not USAF admits to the "switching", it lost the scientific battle, because the net result of the "evaluation", however it was done, was that my "SP" principle was wrong, BUT HARVARD AND MIT SAY THAT IT IS RIGHT, SO, USAF HAS BEEN PROVEN SCIENTIFICALLY INCOMPETENT IN THE FIELD OF MY "SP" INVENTION. This means loss of scientific prestige for USAF; its scientific reputation is at stake. The single person responsible for putting USAF in this jeopardy is COLONEL FLINDERS, HANSCOM FIELD, who made the grave mistake in line of duty of issuing OFFICIALLY, as an OFFICIAL Bid Proposal Evaluation, an unethical and scientifically erroneous "evaluation". Note that USAF did not have to issue this "evaluation"; it could simply have excused itself. I am under the impression that Colonel Flinders' was most anxious to issue this "evaluation", going all the way back to Cambridge Field Station days in his official forwarding letter of 3-10-70, being very negative towards "SP". Colonel Flinders was a frequent MITRE visitor; in fact I was in the same meetings he was in more than once.

MITRE'S "KANGAROO COURT".

The famous MITRE RESEARCH COMMITTEE was still under Dr. Robert Naka when the "SP" issue started. (Dr. Naka is now Deputy of the Air Force in Washington.) Every year this Committee receives scores of scientific ideas from brilliant MITRE people, and no doubt some of these ideas result in valuable contributions to our National Defence. Therefore, when a new principle of physics, submitted directly for our National Defence, is prevented from reaching the Committee, it is most serious even in the case when the preventive action is merely caused by scientific incompetency at MITRE. The Evaluation Meeting of 10-3-69 was held in response to my many complaints via different MITRE channels to the amateurish "analysis" made by Dr. Herman Blasbalg, ruling out "SP" from consideration by the Research Committee. One would therefore think that the Director of the Communications Division, Mr. Lewis Billig, would lean over backwards to make up for the black spot on one of his Departments (D91) by chosing as Evaluator some real top MITRE scientist in the field, and I have already mentioned that MITRE has scientists on Nobel-Prize winners level. INSTEAD MR. BILLIG PICKED THE VERY TWO DEPARTMENT HEADS WITH WHOM I WAS ALREADY INVOLVED IN SERIOUS TECHNICAL, AND IN ONE CASE ALSO ETHICAL, DISAGREEMENTS, HAVING REPORTED BOTH FOR SCIENTIFIC INCOMPETENCY IN THE FIELD OF HOLOGRAPHY, AND ONE FOR UNETHICAL ACTION UNWORTHY OF A MITRE DEPARTMENT HEAD. On the other hand, I do not think I had any other enemies at MITRE than these two. Accordingly, this was not a scientific Evaluation Meeting, IT WAS A DELIBERATELY ARRANGED "KANGAROO COURT" WITH ONLY ONE AIM; TO GIVE THE DEATH BLOW TO "SP", with total disregard of scientific values. The Meeting "proved" that "SP" does not work; also the opinion of Mr. Billig as expressed in his later grossly erroneous Memo D9-M2164 of 10-7-1969; BUT HARVARD AND MIT SAY THAT IT DOES WORK! More specifically, I entered the Meeting with six points on my agenda, covering all aspects of my "SP" including a medical application in which my "Approved For Release 2002/06/24 : CIA-RDP75B00514R000100190004-2" laser beam, but I never got beyond the first point

because of the fierce attacks on "SP" the two in this field incompetent "evaluators" delivered. In the words of Dr. Bergmann, one of them was "angry". THIS "KANGAROO COURT" REPRESENTS A BLACK SPOT ON MITRE'S RESEARCH IMAGE; ONE THAT CAN NEVER BE ERASED. At the end of this biased "evaluation" the Director stated that he could not forward my "SP" invention to the Research Committee on basis that I had been unable to convince the two Evaluators that my principle was right. HOW CAN YOU CONVINC ANYBODY ABOUT ANYTHING THAT HE DOES NOT UNDERSTAND? The entire evaluation was a big farce; all I had been able to do was to merely begin to explain how the two waves were generated; my additional five points had not been touched upon. This "evaluation" was not a recommendation for MITRE as a Government "THINK TANK", because no thinking went into this "evaluation" on the part of the "evaluators", at least no scientific thinking. Further, this "evaluation" was not a recommendation for MITRE as a link in our National Defence, and this was a project submitted for our National Defence. The "evaluation" was a disgrace to MITRE!

On 10-7-69 the Director called me in for a continuation meeting, saying that he wanted to "emphasize" on certain aspects of "SP" which had not been given full consideration in the previous meeting. There was nobody else present. The Director embarked upon a mathematical analysis on the black board, dealing with the generation of the two waves; the subject matter I had started out with in the preceding meeting. HIS ANALYSIS WAS TOTALLY IN ERROR AND REVEALED VERY POOR KNOWLEDGE ABOUT THE FUNDAMENTALS OF COMMUNICATIONS, particularly with reference to the subject matter of frequency and bandwidth. Instead of disproving "SP", he actually proved it, but he did not know enough of the physics fundamentals to understand this, although I immediately pointed out his errors to him, getting nowhere. He said he would write up the analysis in a Memo and send to me. I asked if I could reply to it, and he actually requested me to reply to it. This was fortunate, because when I later replied to it, I was instantly refused a secretary, and I had to refer to the Director's office to get one. The administrative machinery under Mr. Billig was in many respects third rate, with an administratively poorly trained Department Head (D91; Dr. Herman Blasbalg), and a still more poorly trained Sub-Department Head (D91; Mr. Otto Cardinale). Denial of a secretary was the standard procedure I encountered when my Memos included true statements not appreciated by the administration, and to slow down my work for USAF on urgent Defence Matters, so that I could be charged with falling behind in my work, not delivering in scheduled time the goods ordered from me. My complaints to the Division Director only made my situation worse. I had numerous friends at MITRE who would type anything for me, and some of them helped me out on several occasions, but then D91 used another, much more powerful tool to stop a scientist who would not "fall in line"; REFUSAL OF AN OFFICIAL MEMO NUMBER! This indeed was a powerful tool. You could work for a long time on a scientific problem, the urgency of which was indicated by the fact that several scientists in your own corridor worked on the same or a similar subject matter, and you could get a rough draft typed, and even a final typed, but being refused a Memo number, you had produced nothing. It was the ultimate weapon, that could make you look as a total failure as a MITRE scientist. A still more powerful weapon to slow down a scientist in D91 was to exclude him from the Project Meetings discussing the very subject matter he was assigned to work on, leaving him to guess how his work for USAF tied in with the requirements, rather than to go by fact. Fortunately, I had many friends, and I always had a pretty good idea of the progress in my assigned field. NOT ONCE IN D91 WAS I ALLOWED TO ATTEND A PROJECT MEETING DISCUSSING THE SUBJECT MATTER OF MY USAF ASSIGNMENT, and complaints up the line failed to remedy the situation. I have a ringbook full of evidence to prove what I have said above. And do not let me forget the major D91 weapon against any scientist; the firing list. I do not recall how many times I was threatened to be fired just because I disagreed with the amateurish, semitechnical instruction I received just how I should do my work, and ultimately I ended up on the firing list. I had to go all the way up to the MITRE President to get out of that one. Had I been fired, I would have issued the content of my ringbook to every member of Congress. I mention all this to give a true background to the D09 "Kangaroo Court"; to prove that it was a lot of "sand" in the Communications Division administrative "machinery"! There was quite widespread dissatisfaction with both the executives mentioned; apparently, I was not the only scientist who suffered because of their lack of knowledge about administration.

Indeed Mr. Billig issued a Memo over his signature; a memento over his incompetency in this communications field. It was Memo D9-M2164 of 10-7-69, having most of the errors of his verbal presentation that day. My reply was a polite four-page Memo D91-M-408 of 10-20-69, in which I pointed out his gross errors. I corrected his erroneous assumptions underlying his "proof", in which he wrote impossible mathematical relations. He did not even understand the well-known Wheeler's formula about the proper frequency-bandwidth relation. He used this formula incorrectly, as I pointed out. I showed by means of a Fourier analysis that when he tried to prove that "SP" was no good because of enormous bandwidth requirements, he really showed instead mathematically that "SP" does not need any bandwidth at all; the utopia of $1/f$ relationships. Mr. Billig was incompetent in the communications field to the extent that he did not even understand his own formulas. I also suffered from Mr. Billig's lack of technical knowledge in the Holography field; something I can prove with a reliable witness; Dr. Berthold Zarwyn.

MORE MITRE SCIENTIFIC INCOMPETENCY.

The scientific incompetency revealed with reference to my "SP" invention was not limited to the MITRE Communications Division. Early in 1969 I took my "SP" invention to the top MITRE scientist, Dr. Robert Naka, who seemed very interested, and agreed that my principle was correct and that "SP" worked. Later, however, when my disagreement with the Communications Division Director developed, and when Dr. Naka had become Deputy of the Air Force in Washington, he denied via the MITRE Patent Committee that he had ever said "SP" was right. At the time I first approached him, however, he was most helpful, and sincerely tried to help me make headway with "SP" at MITRE. Accordingly, he authorized a meeting with the Director for the Radar Division, Mr. Ed Key, D08. Dur

damentals, and during our conversation he made several worthwhile suggestions, proving to me that he understood the "SP" principle almost instantly. What I was looking for was Microwave Experimental Facilities, and he sent me to an administrator, who was very helpful, and again sent me to the radar experts; the ones supposed to look into the technical matters. Here, however, I run into severe scientific incompetency. While some of these people were very quick in writing down electromagnetic theory equations, it was apparent that some of them, at least, had not grasped the true meaning of Maxwell's equations; the interpretation they gave of these equations were sometimes in error. Of course, for each one of these people, MITRE has ten outstanding radar experts, thoroughly familiar with Maxwell's equations, but those were not the people I had been steered to. My first contact ultimately arrived at the grossly erroneous conclusion that, dispersive space or not, you cannot obtain "range nulls" with two waves sent in the same direction. Due to my objections, however, he offered to take my proposal to two other radar experts, I do not know their names, and a week later, he presented me with the verdict that indeed my "SP" principle was wrong; the three had considered this scientific matter in very great detail, and agreed that I could not obtain the "range nulls" I claimed with the method I described, he said. BUT HARVARD AND MIT TODAY SAY I CAN, proving that these three "radar experts" were very poorly trained in electromagnetic theory, and did not know what they were talking about! They just could not grasp the simple "SP" principle in spite of my detailed explanations to their spokesman. I proceeded to the next radar expert, who also said that the "SP" principle was wrong. When I got to the fifth and last "expert" and found him saying the same thing, I gave up, and never returned to the MITRE E-Building. The last "expert", however, made the very serious mistake of transmitting his erroneous findings to me in the form of a signed letter, dated 10-2-69, from which I quote: "----- range nulls cannot be achieved by forward waves only". BUT HARVARD AND MIT SAY THEY CAN : Note that this scientist most definitely was dealing with dispersive space; in fact the dispersion formula he used appears on the same page I quoted from. There is no way on earth in which this scientist can get around the fact that HIS ANALYSIS WAS TOTALLY IN ERROR; the written evidence will stand up in court.

LOSS OF SCIENTIFIC PRESTIGE FOR THE AMERICAN INSTITUTE OF PHYSICS.

On 10-2-70 I submitted my "SP" manuscript to the AMERICAN JOURNAL OF PHYSICS, in which I had published several scientific contributions through the years. With reference to the submitted paper, however, my dealings soon extended to the American Institute of Physics, AIP, which is fully responsible for the third-rate editorial policy displayed by the Journal. The amateurish handling of my paper is perhaps to some extent evident from the abnormally drawn-out editorial proceedings, with received letters from the Journal dated: 10-5-70, 1-7-71, 2-22-71, 4-6-71, 5-7-71, 7-16-71. Slowing down the editorial progress to almost a standstill, with the Institute stretching its editorial decisions over almost a year, the responsible party tried desperately to get itself out of the hole it had fallen in by officially submitting A FULLY OFFICIAL AMERICAN INSTITUTE OF PHYSICS OFFICIAL REVIEW, thoroughly and completely in error, not worth the paper it was written on. One way of dodging the issue was by not answering letters, or not answering them in time, and when answering my letters, TOTALLY DODGING MY REQUEST FOR A COMPETENT REVIEWER REPLACING THE INCOMPETENT ONE, WITH THE ISSUED OFFICIAL "REVIEW" OFFICIALLY WITHDRAWN FOR THE REASON THAT IT WAS SCIENTIFICALLY IN ERROR. The scientific incompetency of the First Reviewer was beyond the wildest imagination. He was so totally lacking knowledge in fundamental physics pertaining to waves that he would not even get beyond an "F" as a High-School senior. How the American Institute of Physics could lower itself to use such an amateur scientist in the field of my "SP" invention is impossible to understand. I described his frightful scientific error IN AN 8,000 words DOCUMENT, dated, 6-1-71, and sent to the AIP Chairman and other AIP officials. THERE WAS NOT THE SLIGHTEST ATTEMPT BY THE AMERICAN INSTITUTE OF PHYSICS TO ADMIT TO THE GROSS SCIENTIFIC ERROR OF ITS OFFICIAL FIRST REVIEWER, OR EVEN TO DISCUSS HIS ERROR, AND TO WITHDRAW THE FAULTY "REVIEW", AND TO APPOINT A COMPETENT REVIEWER AS REPLACEMENT FOR THIS IN THE FIELD OF MY "SP" INVENTION POORLY TRAINED "EXPERT". In this 8,000 words Document I showed in the greatest detail how this incompetent "Reviewer" had totally failed to grasp the basic principle of "SP", and could not even understand the concept of a composite wave; clearly a High-School level subject matter. To illustrate the degree of incompetency of this First Reviewer, let me go back for a moment to a statement made by MITRE's Mr. Billig. He said that "SP" simply could not work BECAUSE ONE CANNOT PRODUCE TWO WAVES WHICH ARE ALWAYS OUT OF PHASE AT THE TRANSMITTER. My reply to Mr. Billig and to others coming up with the same statement constantly was: "WHO IS TRYING TO PRODUCE TWO WAVES ALWAYS OUT OF PHASE AT THE TRANSMITTER: I AM NOT". I claimed "range nulls" with waves vexing in and out of phase. Now this "expert" officially chosen by AIP as its First Reviewer out of the clear sky comes out with the same statement! Specifically he says that MY PAPER CAN BE PUBLISHED ONLY AFTER I HAVE PROVEN EXPERIMENTALLY (or all things) THAT I CAN PRODUCE FREQUENCIES WHICH ARE ALWAYS OUT OF PHASE AT THE TRANSMITTER. Where did he get this worn-out MITRE slogan from? Was there any tie-in between this "Reviewer" and MITRE, or rather, with anyone working at MITRE? The AIP "Reviewer" was not even in the same ball park as my submitted paper. He committed a deadly sin as a reviewer of submitted papers namely TO CLAIM A SCIENTIFIC STATEMENT, WHICH NEVER APPEARED IN THE SUBMITTED PAPER, AND THEN BASE HIS "REVIEW" ON THIS STATEMENT, WHICH HE HIMSELF HAD INVENTED, TREATING IT AS A BONA FIDE PART OF MY PAPER. The truth in the matter is that I obtain my "range nulls" with the two waves doing JUST THE OPPOSITE TO BEING "ALWAYS OUT OF PHASE", NAMELY VEXING IN AND OUT OF PHASE ALL THE TIME. This amateur scientist in the "SP" field, serving as an official AIP "Reviewer", put words in my mouth that I had never uttered, IMPLANTING UPON MY METHOD A WAVE-STARTING CONDITION WHICH DID NOT EXIST, THUS FAILING TO REVIEW MY SUBMITTED PAPER, AND INSTEAD REVIEWING A PAPER CONTAINING HIS WILFULLY INTRODUCED CHANGES. Note that in his submitted "review" this AIP "Reviewer" clearly states that he is reviewing my basic method of creating "range nulls", although he refers to them as "standing waves", which is perfectly acceptable. If he was reviewing something else, he should have stated what this "something else" was, for example, laser beam "SP" requires quite a special generation technique, in Ultrasonics "SP" the wave

generation is completely different, and if interference reduction is the aim, the simple "SP" scheme for the creation of "range nulls" is not used at all, and is replaced by a rather complicated system. But, this "expert Reviewer" by his own admission is "reviewing" just one thing, and that is my "range null" system, and here he makes the monumental error of the specific assumption that I must have waves "always out of phase" or I cant do it, WHILE HARVARD AND MIT SAY THAT INDEED I CAN DO IT WITH WAVES WHICH CONSTANTLY VEX IN AND OUT OF PHASE. This "Reviewer" is a disgrace to the American Institute of Physics. A still greater disgrace are the two AIP editors I were dealing with, who refused to admit to the gross scientific error made by this amateur scientists in the field of "SP". Nor did AIP admit to this gross scientific error when I made it extremely clear to the AIP leaders via my extensive rebuttal, the outstanding message of which was MY REQUEST FOR A COMPETENT REVIEWER REPLACING THE INCOMPETENT ONE. The AIP:s reaction to this request is unbelievable. One would think that after its major fiasco in the reviewing of a scientific paper, that AIP would look all over America for a real expert on Wave Propagation and Electromagnetic Theory, to make up for its failure. It looked for months, but not for the kind of Second Reviewer I had expected. THEY CAME UP WITH ONE WHO KNEW STILL LESS ABOUT THE "SP" FUNDAMENTALS OF PHYSICS THAN THE FIRST REVIEWER, and of course, they did not tell me the name of either "reviewer". After I had pointed out the frightful scientific errors of the "Second Revier", AIP followed the same policy it had applied to the "First Reviewer", namely to refuse to admit to his errors. Like the "First Reviewer", the "Second Reviewer" totally failed to understand the "SP" principle. He attempted to prove that "SP" was using part of a previously published technique, employed in a well-known navigational system, described in a paper discussing the famous OMEGA and other systems. The basic principle for the OMEGA system, invented by the famous Harvard scientist Jack Pierce, and my "SP" system, have practically nothing in common, but the poorly trained "Second Reviewer" never discovered that. He did not even discover that in the OMEGA system, the two waves have the SAME frequency, but DIFFERENT directions, while in my "SP" system, the two waves have DIFFERENT frequency, but go in the SAME direction. But this "expert" stumbled on to a much worse error. He failed to understand that OMEGA and "SP" are as different as day and night for the special additional reason that OMEGA employs NON-DISPERSIVE SPACE while "SP" employs DISPERSIVE SPACE. Further, this OFFICIAL AIP REVIEWER failed to grasp the concept of the INTERFERENCE PATTERN I employ in my receiver. His comparison between interference in the "SP" system and interference in the system he uses for comparison is shattered into smithereins for the simple reason that THE OTHER SYSTEM DOES NOT HAVE ANY INTERFERENCE AT ALL ; IT IS A "TIME-SHARING" SYSTEM; ONE WAVE IS OFF WHEN THE OTHER ONE IS ON. This kind of amateurish mistakes did not stop the "Second Reviewer" from throwing out my paper, stating that it "contributes nothing new", and, "does not merit publication". The use of two consecutive official AIP "Reviewers" with amateurish knowledge in the field of the review subject, does not speak well for AIP. The Institutes refusal to admit to the gross errors of the two "Reviewers" does not speak well for the AIP. The Institutes use of two Editors, none of whom discovered the "Reviewers'" serious errors but bluntly mailed out this scientific trash as bona fide SUBMITTED PAPER REVIEWS does not speak well for AIP. THE HARVARD-MIT FINDINGS PROVE THAT AIP IS SCIENTIFICALLY INCOMPETENT IN THE FIELD OF MY "SP" INVENTION. THE SCIENTIFIC REPUTATION OF THE AMERICAN INSTITUTE OF PHYSICS IS AT STAKE IN VIEW OF THE FINDINGS OF HARVARD AND MIT. Still, the AIP members never heard of "SP", they constute the elite of the world's experts on Wave Propagation and Electromagnetic Theory. IT IS THE AIP LEADERS WHO FAILED, NOT THE MEMBERSHIP BODY, BUT THE AIP LEADERS SPEAK OFFICIALLY FOR THE TOTAL MEMBERSHIP.

NOTE: My "SP" paper was submitted to the AIP office at Dartmouth College. Dr. D.V. Ragone at Dartmouth College was at this time also a TRUSTEE of MITRE. I do not know if Dr. Ragone and MITRE had anything to do with all this. I would say that an Editor or Reviewer seeking information from outside experts about the subject matter of a submitted paper should be complimented; he is doing the right thing. But suppose he receives erroneous technical information, and is not sufficiently well qualified to discover this, then he is in trouble.

LOSS OF SCIENTIFIC PRESTIGE FOR IEEE.

The AIP "review" of my submitted "SP" paper marks a black page in America's history of editorial policy with reference to technical editing. When I subsequently submitted the same paper to IEEE, where (in Proc.IRE,IEEE) I have published many scientific papers, I found the page contributed by IEEE to the history of the reviewing of scientific papers in America to be "blacker than black". To the extent possible, the IEEE "review" contained still worse technical errors than the AIP "review", produced by an amateur scientist in the field of my "SP" invention; a fellow who did not even master High-School trigonometry, as I shall prove below. The "review" came in three installments, dated: 8-17--71, 9-30-71-, and 11-24-71. With the many letters I wrote to numerous IEEE officials, for example demanding a competent reviewer, this submitted paper "review" extended over many months, and fills a ringbook. I got absolutely nowhere: IEEE REFUSED A COMPETENT REVIEWER AND IT REFUSED TO ADMIT TO THE GROSS SCIENTIFIC ERRORS MADE BY THE OFFICIAL, BUT IN THIS FIELD TOTALLY INCOMPETENT, "REVIEWER". Ultimately, IEEE used additional "Reviewers", contributing "reviews" WHICH I WAS NOT ALLOWED TO SEE, and then on basis of these additional "reviews" ruled out my paper from publication. One additional "review" got into my hands; as I had expected, it was totally in error. I can only conclude that all originators of SECRET ADDITIONAL IEEE OFFICIAL "REVIEWS" were equally scientifically incompetent in the quoted field. IN VIEW OF THE HARVARD AND MIT EVALUATIONS, IEEE IS SCIENTIFICALLY INCOMPETENT IN THE FIELD OF MY "SP" INVENTION. IEEE'S SCIENTIFIC REPUTATION IS AT STAKE.

Space only allows me to give a few examples of the unbelievable scientific incompetency of this official IEEE "Reviewer"; my Rebuttal of 11-27-71 ALONE is 28 pages long, spelling out every scientific error in greatest detail, and sent to THE IEEE DIRECTORS, THE IEEE EXECUTIVE COMMITTEE, SEVERAL PAST PRESIDENTS, SCORES OF IEEE EDITORS, AND TOP IEEE OFFICIALS, etc. NOTHING WAS DONE TO REMEDY THE SITUATION. IEEE would not even discuss the scientific errors, not to mention admitting to them. Of course, the IEEE members, some of them Nobel-Prize Winners in physics, I'm sure, knew nothing about all this. Just ONE review by ONE competent IEEE Reviewer would have proven "SP" scientifically correct.

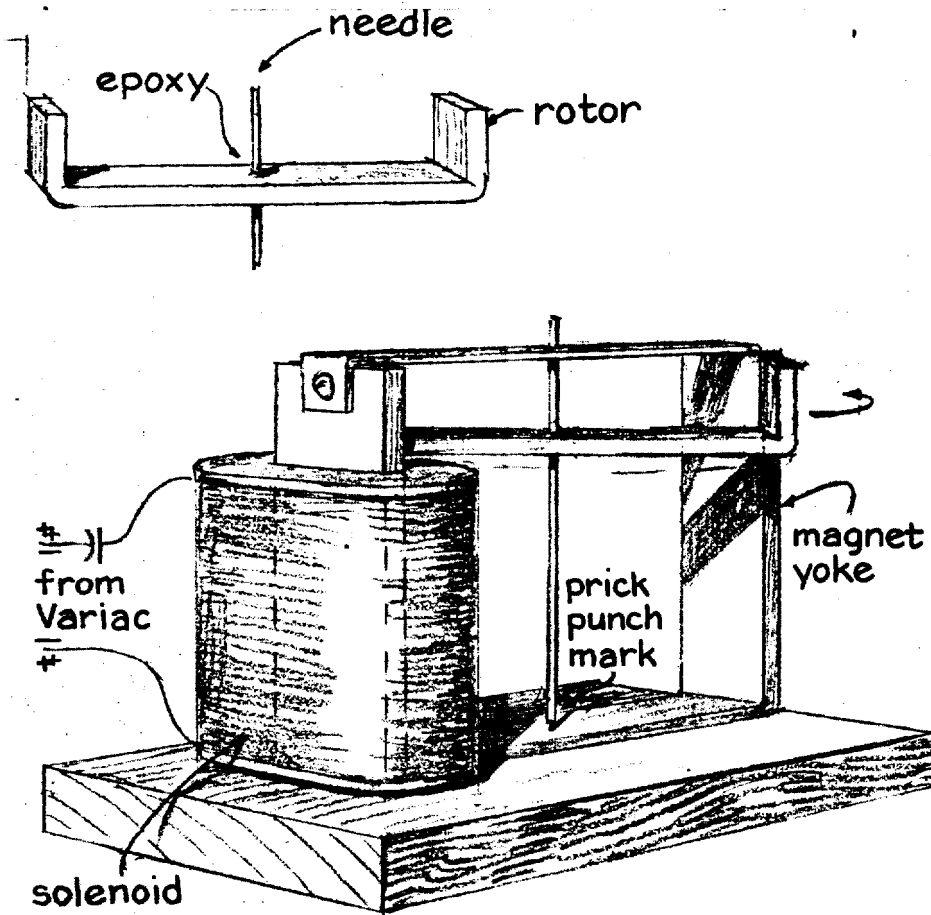
Here are some unbelievable "tid bits", all verified by official IEEE "Review" documents. Already in his first "review" this poorly trained "Reviewer" proved his incompetency in the field of Wave Propagation by stating that two waves propagate with the DIFFERENT velocities v_1 and v_2 , but, mysteriously, AFTER SOME TIME BOTH HAVE GONE THE SAME DISTANCE x . Then he writes a formula which can only be designated as idiotic (page 9 in my Rebuttal). Now he has forgotten his original assumption, and surprisingly proves that $v_1 = v_2$. No one caught this error. As if this was not enough, the official IEEE "Reviewer" uses his High-School level physics knowledge in the field to prove that " x and t are linearly related". They are not; this is another totally erroneous conclusion on the part of this "expert" (See page 10 of my Rebuttal). In direct response to this "First Review" I pointed out his errors to IEEE, AND THIS WAS THE POINT WHERE IEEE SHOULD HAVE DISQUALIFIED THIS INCOMPETENT "REVIEWER" AND REPLACED HIM WITH A COMPETENT ONE. Nothing doing, apparently the IEEE editors at work were just as incompetent in this field as the "Reviewer", because they failed to find the errors AFTER I HAD POINTED THEM OUT! The "Reviewer" apparently got somewhat worried, however, and in his "Second Review" he set out to PROVE that he was right, ALL WITH THE FULL BLESSING OF IEEE (which could have refused to officially issue this scientific trash in the form of a most official IEEE review; the second one). To my great surprise, this "expert" in his "Second Review" produced a trigonometric formula which indeed proved that my "nodes" never stand still in space. What decides whether my "nodes" stand still or not is the sign between the two last terms in the equation (See page 11 of my Rebuttal). The "Reviewer" had a + sign here, BUT HE HAD MADE A BAD SIGN ERROR; IN A CORRECT DERIVATION THE SIGN COMES OUT -, AND NOW, JUST AS I ALWAYS CLAIMED, MY "NODES" STAND STILL, IN FULL AGREEMENT WITH THE LATER HARVARD AND MIT FINDINGS. THIS IS HIGH-SCHOOL LEVEL TRIGONOMETRY THAT THE OFFICIAL IEEE "REVIEWER", AND THE IEEE EDITORS AT WORK, ARE FAILING IN! How IEEE can issue such scientific trash in the form of a SECOND "review" after I had proven the scientific incompetency of this official IEEE "Reviewer", is very hard to understand. I can only conclude that the aim of the entire editorial "reviewing" was to kill off "SP" AT ANY COST! Then IEEE would "sing" the same "song" previously "sung" by MITRE, USAF, U S ARMY, and AIP. After I had corrected this unbelievable "hoppla" officially issued by IEEE as the official "SECOND REVIEW", I felt confident that IEEE would replace this amateur in the field of Wave Physics by a competent Reviewer. To my surprise, IEEE KEPT THIS "EXPERT" ON THE JOB AS THE OFFICIAL IEEE REVIEWER, AND GAVE ITS FULL CONSENT TO THIS THREE-RING CIRCUS OF ERRONEOUS "REVIEWING" BY ALLOWING HIM TO ISSUE A THIRD "REVIEW", this time a "Review" with so frightful errors in mathematics and physics that the previous ones seemed very slight in comparison. Apparently, this time the "reviewer" was mad because I had reported his equations as being wrong to IEEE, so he starts out with the bold statement ".... THIS REVIEWER DID NOT MAKE ANY MISTAKE". He surely did; he made numerous mistakes. HOW CAN IEEE ALLOW A "THIRD REVIEW" TO GO OUT WITH THE "REVIEWER'S" STATEMENT THAT HE WAS RIGHT, AFTER I HAD PROVEN HIS OBVIOUS MATHEMATICAL AND OTHER ERRORS? Where the IEEE editors so poorly trained in mathematics that they could not even understand simple corrections of High-School level trigonometric equations? Don't tell me they sent out three consecutive official "Reviews", KNOWING that the official "Reviewer" was all wrong all the time? The fact in the matter is that the "reviewer" deliberately made an untrue statement, and that THE IEEE EDITORS TRANSMITTED AN UNTRUE STATEMENT IN LINE OF DUTY BY ISSUING A "THIRD REVIEW", WITH WORSE MATHEMATICAL ERRORS THAN THE TWO PREVIOUS "REVIEWS" PUT TOGETHER. Now the "Reviewer" claims that he is going to repeat "THE DETAILED STEPS", but here he makes a second untrue statement, BECAUSE HE DOES NOT REPEAT "THE DETAILED STEPS". WITHOUT SAYING A WORD HE SECRETLY CHANGES THE FAULTY SIGN SO THAT HIS EQUATION CANNOT BE FURTHER CRITICIZED SIGN-WISE, but in doing so he introduces another error in the same equation, and then proceeds to disprove "SP" by a mathematical relation derived from the equation under discussion, "PROVING" THAT INDEED MY "NODES" FLY ALL OVER THE MAP AND THAT THEREFORE MY "SP" PRINCIPLE IS WRONG. Now just how did this glorious OFFICIAL IEEE "REVIEWER" get from an equation which sign-wise at least proves that my "nodes" stand still, to an equation on the next line that proves just the opposite? Such an achievement really requires the skill of a Nobel-Prize Winner in mathematics. This one really required ingenuity by this "expert Reviewer", but he measured up to the task, by INVENTING A NEW TRIGONOMETRIC FORMULA, PREVIOUSLY UNHEARD OF BY MANKIND. It goes like this:

$$\cos(A-B) - \cos(A+B) = 2\sin A \sin B.$$

And with this handy-dandy home-made super-practical formula, this "expert" and official IEEE "Reviewer" without blinking an eye "PROVES" that my "nodes" do not stand still. THIS WITH THE FULL BLESSING OF THE RESPONSIBLE IEEE EDITORS, SELLING THE SCIENTIFIC REPUTATION OF IEEE DOWN THE RIVER! Perhaps a change of IEEE editors is indicated? However, there seems to have been some doubt in the mind of this Junior High-School level "Reviewer", because he goes on to "prove" his result by deriving it via a different route, adding another page of erroneous High-School level mathematics. I had always indicated with reference to "SP" that there are two prime starting conditions for the two waves; either you consider the moment when they are in phase, or the moment they are out of phase. In the first case, after half a period of "sliding", a real "range null" appears. In the second case, a maximum appears; the sum of unity amplitude becomes 2. Now this hopelessly confused amateur in the field of Wave Propagation treats the opposite case, without π , but not making a sign error in this parallel derivation, God help him, he finds my "nodes" standing still. Well, how to get out of this one? He quickly invented another handy-dandy trigonometric formula which goes like this:

$$\cos(A-B) + \cos(A+B) = 2\cos A \cos B.$$

Like before, he now derives a final formula, and Horay, the IEEE honor is saved, this derivation result backs the previous derivation result 100%. Now the "nodes" fly all over the map again. IEEE proved the "SP" principle wrong, just as MITRE, USAF, US ARMY, and AIP had done previously. But this incompetent "Reviewer", and the IEEE responsible editors forgot one thing! When you slide the waves π , the equation yielding the sum, should not come out 0, but 2. So, after all this mathematical fraud, IEEE proved me right without knowing it; as Harvard and MIT say, my "nodes" or "range nulls" stand still.



Harry E. Stockman's parametric motor

120

A physicist investigating phenomena mathematically occasionally writes an equation that suggests a novel device of no obvious use. An example of such a device is an electric motor invented recently by Harry E. Stockman of Arlington, Mass. The machine consists essentially of a rotor in the form of a soft bar of iron supported at its middle by a vertical shaft that is free to turn between the poles of an electromagnet [see bottom illustration at left]. The magnetic field is provided by a solenoid that operates on alternating current. When the solenoid is energized by a 60-hertz current, the polarity of the magnet reverses 3,600 times per minute.

If the rotor consisted of a thin strip of hard steel instead of the soft iron bar, and if the strip were given an initial spin at the rate of 3,600 revolutions per minute, the steel rotor would become permanently magnetized, fall into lockstep with the alternating magnetic field and continue to rotate synchronously with the field. The synchronous motors of

When the soft iron rotor is given a start, it continues to turn at a speed that varies roughly with the applied voltage. The tips of the rotor experience a strong force of attraction as they approach the poles of the magnet. The force decreases as the rotor moves away from the poles.

The variation of the force is explained by a similar variation in the inductance of the solenoid, reflecting the property of solenoids, which is to oppose electric current. The inductance of the coil is maximum when the poles of the magnet are bridged by the soft iron bar. The current and the magnetic force of attraction decrease.

Conversely, when the iron bar turns away from the poles, the inductance approaches the minimum and the exciting current and the force of attraction increase. In actuality the force of attraction does not become minimum at the instant the bar reaches its middle position between the poles of the magnet. The field cannot change instantly. Accordingly the bar is strongly attracted as its ends swing toward the poles of the magnet and less strongly attracted as they coast away.

Inductance is known as a parameter of electric circuits: a quantity that is normally constant but that can be varied by circumstance. In the case of this device the determining circumstance is the position of the rotating bar of iron. For that reason Stockman refers to his invention as a "parametric" motor.

The core of the electromagnet can be bent from a strip of flat iron 1/8 inch thick and one inch wide, making a flat-bottomed U with upright legs about 2 1/2 inches long. The rotor can be bent from 1/2-inch stock. The ends of the rotor should be bent upward to form legs 1/2 inch long that leave an air gap of about 1/32 inch at each end when the rotor is placed between the poles of the magnet. The dimensions are not crucial.

The solenoid can be made of approximately 2,700 turns of No. 26 enameled copper wire. The assembled electromagnet will have an inductance of about .3 henry. Stockman connects a capacitor of eight microfarads in series with the solenoid and energizes the unit through a variable-voltage transformer. The capacitor is not strictly necessary, but the operation is greatly facilitated by taking advantage of resonance. The motor will operate on about five volts. The speed is nonsynchronous and roughly proportional to the applied voltage up to about 10 volts. At higher voltage the machine with the frequency of the power source and to operate as a

Sercolab

P. O. BOX 78

1-20-73

ARLINGTON, MASSACHUSETTS 02174

AN APPROPRIATE LETTER ON PK TO THE PROGRESSIVE AMERICAN SCIENTIST.

SERCOLAB recently celebrated its 20:th year of supplying American universities with ingenious models for the physics classroom. Apparently our models have fulfilled highest scientific requirements, because they have been written up in the world's leading scientific and technical journals, both in America and in Europe, for example in American Journal of Physics, Proc. IRE, and Proc. IEEE. The latest write-up of one of our models appears in this month's issue of SCIENTIFIC AMERICAN; America's most distinguished scientific journal. You cannot buy this kind of publicity for any money in the world. We have been honored with this kind of write-ups because our models reflect original research work, aiming at highest educational value in the field of physics.

We tackle the unconventional in science, and try to come up with new things that nobody else has, and naturally this has led to some formidable clashes with reference to patents, and also with reference to science in general and scientific thinking. A shining example here is the "SLIDING PHASE" controversy, in which we won out against overwhelming odds, proving that opposing statements by MITRE, U.S.A.F., U.S. ARMY, AMER. INST. OF PHYSICS, and INSTITUTE OF ELECTRICAL AND ELECTRONICS ENGINEERS, IEEE, were all in error. * Some of these people went as far in scientific incompetency as to issue mathematical "analyses", supposed to prove that the "range nulls" characteristic for my invention, obtained with the physical apparatus I described, do not obtain. These "analyses" are not worth the paper they are written on. What makes it particularly bad for the opposition is that one of these high-school level physics "analyses" was issued by the Government as a most official part of a most official Bid Proposal Evaluation. My requests to the Government that this evaluation be withdrawn on basis that it is totally in error have been met by silence. Well, you can't win them all.

Today "SLIDING PHASE" is acknowledged by the real authorities as completely scientifically correct. Of cause it is correct, precisely as I stated from the very beginning. But SERCOLAB is today entering into a scientific argument of far greater importance than that pertaining to "SP". The task ahead is TO PROVE THE TRUE EXISTENCE OF THE PSYCHOKINESIS, PK, VECTOR FIELD AND TO OUTLINE THE AVENUE OF APPROACH FOR UTILIZATION OF THE PK FORCE IN PHYSICS AND ELECTRICAL ENGINEERING; ELECTRONICS. The odds against proving this is probably 1,000 to 1; not at all because of technical difficulties, but because of the resistance against establishing of the truth. The proof has been fully established for years, to the PSYCHIC, or SENSITIVE, who performs the experiment, and to the onlookers. What SERCOLAB is talking about is something very different; an unquestionable, fully official proof to the scientific society of America, leaving NO ONE who can say that PK is fraudulent, without being corrected by everybody around. We do not mind when we write Psychology Departments and electronics journals about PK and fail to receive answers, but we do mind when people claim PK to be trickery, and at the same time refuse to look at the acid tests now available, proving that certain people can move table-top items by "thought power"! In the PK force field, the PK force combine

* SERCOLAB TECHNICAL MONOGRAPH No. 26,

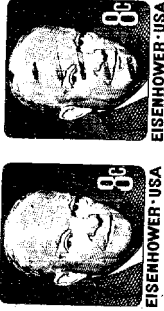
with conventional and better known forces in full agreement with Newton's laws (amply demonstrated by our Model SD11B). The PK force is a concept in Freshman physics; it has nothing to do with mystics and the occult. While different from the physiological e-m field that surrounds man and animal, the PK field, controlled by the mind, has certain associations with the e-m field. In this field, the strongest component under certain conditions is a variational one, and is due to the Helmholtz'"Streaming Potential", associated with the Zeta Potential, well-known to bio-physics scientists. (As a popular illustration, think of the friction electricity due to the blood rush.) At night time, when the lights are on, and your dog moves from one room to the next, your lights will brighten up a little, or dim a little, due to the current change originated by the dog. You cannot see this effect, because it is deeply imbedded in noise, but it is nevertheless there.

Non-direct PK (Placement Tests) were proven years ago. The reason why non-direct PK is not called fraud today is that the test results were published years ago by America's top psychologists in America's top medical journals. By analogy, direct PK (the moving of table-top items by "thought power") ought to be equally well acknowledged once our top psychologists describe the successful tests in our top medical journals. The men in process of doing this deserve our admiration. Sercolab is following a somewhat different line; proposing the utilization of PK for the creation of new electronics devices, and attempting to get the American electronics industry interested in developing such devices. The breakthrough in electronics and communications was established by one of America's most outstanding scientists in the field of parapsychology, Professor Evelyn M. Monahan, a well-known PSYCHIC and SENSITIVE. During 1972 she succeeded to operate two of Sercolab's PK models, essentially proving the following: rotation of a magnetic needle, inducing an emf, demonstrating the principle of STIMULANCE injection in an electric circuit, and proving ENERGY CONVERSION OF MIND ENERGY INTO MECHANICAL, ELECTRICAL ENERGY, AND HEAT. Through her, the university in which she teaches, the Georgia State University, has made a major contribution to the scientific field of parapsychology. The above merely marks the beginning of what this talented professor can do. If you can get together a group of scientists, and go to Atlanta, and obtain an appointment with this outstanding scientist, she might also show you other things, such as rotating a wheel, and "command" a paper clip lying on the table to move. Do not forget to bring a movie camera, because, otherwise, most people will tell you that you were hypnotized, and that it all was trickery.

Our Model ME65B allows the Sensitive to put in rotation a large magnetic needle in front of a large inductor. If you bring your own Voltohmyst, a peak-to-peak reading of roughly 0.1 volt is obtained, with the power in a matching resistor approaching 1 μ W. Ask the Sensitive to produce more violent rotation on even minutes, and less rotation on odd minutes, and the output power will vary accordingly. You now have an example of PK AMPLIFICATION. While such demonstrations with mechanical devices are interesting, it is the DIRECT CONTROL OF CARRIERS WITH PK FORCE which is of interest for the development of new and revolutionizing electronics devices. One of our models for direct carrier control is presently submitted for testing. It is not surprising that we soon will be able to build new kinds of amplifiers and oscillators this way in view of the fact that PK particle control was already proven years ago by Placement Tests.

Unfortunately, PK and ESP have the potentiality to provide powerful war weapons, and the Government scientists who work on associated problems in the service laboratories are highly advanced. There is only one way in which you can stop PK from becoming a war weapon; encourage the research centers in the private industry to beat the rate of progress in the service laboratories. You cannot classify the electronics journals.

Dr. Harry E. Stockman Harvard, 1946



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Mr. James R. Schlesinger
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Washington D.C.