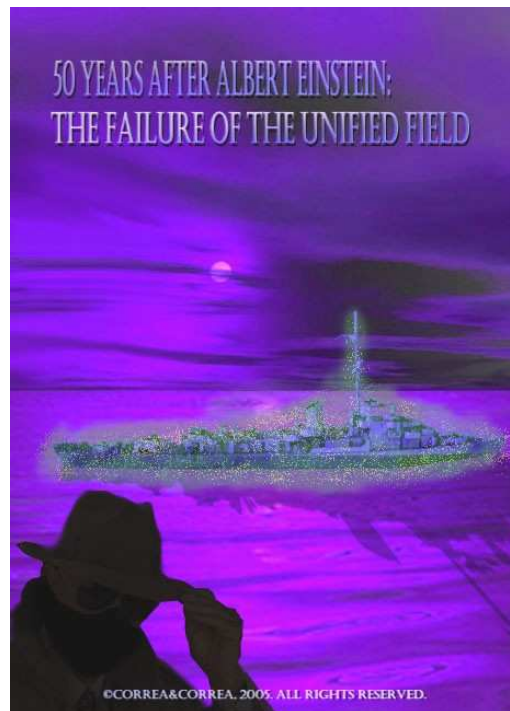


“Gone Dark”

50 Years after Albert Einstein: The Failure of the Unified Field



Disclaimer and Notes by Akronos Publishing

The text from Gone Dark is being made available by Akronos Publishing. Its author (or assumed author) “Mr. W.B. Smyth” has agreed to our conditions including this disclaimer as well as an introduction and a closing commentary written by the Correias.

No one at Akronos Publishing can vouch for the veracity of Smyth's text nor for its author's being who he says he is. All that we can say is that its content appears to be rather plausible even if our in-house consensus is that it is fictionalized.

Smyth offered to us more of his text than we -- in the end -- chose to publish. It is our understanding that Smyth wants to publish the entire book from which the two chapters he sent us were taken. Yet he explicitly asked us to place a copyright disclaimer on the excerpted text. We have agreed to that. When queried, Smyth also asked us to retain the observed discrepancy in the initials of the apparent interviewer, who at first goes by the letter 'S' and shortly thereafter appears with the letter 'J'. We have no idea why Smyth did this, and it only appears in the first chapter of the text he sent us.

Smyth's text is written in the form of an interview that he apparently conducted (at least that's what one gathers, since Smyth has declined to answer that question directly) with an anonymous scientist -- a 'Dr. W.' who poses as having worked for the U.S. intelligence community, possibly the NRL (Naval Research Laboratory) or the ONR (Office of Naval Research). The style of the text -- although quite relaxed -- is also rather polished, which suggests that it is most likely not a literal transcript of a live interview but has, if nothing else, undergone some editorial smoothing and rounding.

We do not know if Smyth's story is real or not. But from what we could corroborate of it, its references are frequently factual which led us to conclude that it was worth publishing -- all the more as it seemed to demystify much of the speculation concerning what happened with **Project Rainbow** (aka project Invisibility). Maybe the truth lies in these lines and maybe it will come out some day. Maybe in Smyth's book, who knows?

We have marked in bold all of our additions, corrections, and editorial remarks.

Feedback from our readers is appreciated. We can only hope to have made the right decision in publishing this material.

Dr. M. Askanas
Editor-In-Chief, per Editorial Board
Akronos Publishing

Introductions:

- A. [Disclaimer and Notes by Akronos Publishing](#)
- B. [Introduction by the Correas](#)
- C. [Comments by Stanton Friedman](#)
- D. [Smyth's introductory letter](#)

From Gone Dark by W.B.Smyth

- E. [Aftermath of the meetings between A. Einstein and W. Reich](#)
- F. [Einstein and Project Rainbow](#)
- G. [Einstein's unified field theories and Project Rainbow](#)
- H. [Faraday and the magnetic nature of space](#)
- I. [Degaussing and Project Rainbow](#)
- J. [Project Rainbow, nuclear magnetic resonance, and E. Purcell](#)
- K. [The convergence of optical, magnetic and radar countermeasures at the NRL](#)
- L. [Why the Carlos Allende tale?](#)
- M. [The Project Rainbow experiments](#)
- N. [What was wrong with Einstein's UFT?](#)

Closing commentary:

- O. [Commentary by the Correas](#)

Select references:

- P. [Editor Footnotes](#)
- Q. [W.Reich, The Riddle](#)
- R. [Einstein in the New York Times, January 1950](#)
- S. [Infeld in the New York Times, March 1950](#)
- T. [Infeld obit and short bio, 1968](#)
- U. [Infeld in the U of Toronto news, 1995](#)
- V. [Infeld in the Ottawa Citizen, 1999](#)
- W. [Lorenzen bio](#)
- X. [Abelson's memoir on Ross Gunn](#)
- Y. [Ross Gunn and nuclear propulsion](#)

B. Introduction by the Correias

A word on the context of the subject of Smyth's text is in order:

The text was provocatively sent by a Mr./Ms. Smyth who purports to have credible information about the real story of the so-called '**Philadelphia Experiment**' -- a notorious caper that has been much exploited by ufologists and was initiated in 1956 by a Carl Meredith Allen under the pen name of Carlos Allende. The 'Philadelphia Experiment' -- or the 1941 War Office Project Rainbow -- has been the subject of much sensationalistic myth-making and disinformation and the theme of one of the worst movies ever produced in the history of Hollywood. It is true that the quality of Allende's letters wasn't any better. The topic is quite popular, and over the years an entire folklore of the imaginary has assembled around it.

Despite

- Allen's retractions in 1969,
- Ret. USAF Capt. K. Randle's interviews with Capt. S. Shelby of the ONR (U.S. Office of Naval Research) in 1970
- the expose made by a family-acquaintance of Allen's (R. Goerman) in *Fate* magazine in 1980
- the labored retraction in 1997 by Jacques Vallée (in B. Haisch's *Journal of Scientific Exploration*) of his admittedly opportunistic 1991 version of the 'Philadelphia Experiment'
- as well as the consistent denials by the Naval Research Laboratory of any such experiment or project ever having taken place,

the story has survived to this day in various forms, most of them tediously impoverished. This is largely the result of the fact that after more than 25 years, the only attempt to investigate the story was the work of William L. Moore. Published in 1979 with Charles Berlitz as co-author, The Philadelphia Experiment: Project Invisibility claimed to have unearthed "facts" which no researcher to date has been able to follow up on. Smyth's text may be the exception.

Unfortunately, Moore and Berlitz did not hesitate to mix in the fantastic with the facts. And what was good about the journalistic work of Moore was simply lost in the mystifying veil of secrecy and ridiculous tall tales. Moore's later discreditation on other matters did not improve the situation. The entire Philadelphia Experiment was just another one of his canards. Or was it?

Perhaps the worst outcome of the blurring of facts characteristic of the 'Philadelphia Experiment' stories, was the implicit suggestion that **Einstein's Unified Field Theory (UFT)** had actually succeeded. This suggestion was 2-pronged, claiming (1) that Einstein had managed at some point to formulate a successful UFT, but somehow the world has never found out what it was or might have been; and (2) that an experiment -- the so-called 'Philadelphia Experiment' -- was actually the proof (no matter how horrible) of its success.

Neither the scientific nor the biographic history of Einstein warrants the first suggestion. For from at least 1922 onwards, Einstein was on-and-off repeatedly preoccupied with the problems of uncovering a viable unified field theory and unsuccessfully continued to work on it until the end of his life. His last attempt clearly indicates that he knew he had failed. It is apparent that Project Rainbow existed. It is possible that it tested one-or-more of Einstein's UFT hypotheses. But there has never been any proof that it succeeded, let alone that it confirmed any of Einstein's UFTs. Yet that is the essential suggestion of the Allen hoax.

In our view, all the tall tales start from these two unproven premises -- that Einstein succeeded and that Project Rainbow confirmed it. Once that is swallowed, everything else becomes possible and can be believed:

- instant teleportation of the ship between Norfolk and Philadelphia
- invisible men parts of bodies that change dimensions
- spacetime distortions
- human-eating extraterrestrials
- watching angels
- the voices of Allen as representative of other galactic species keenly interested in reading Jessup(!), controlling the US government

etc. etc. -- a whole delirium that came in with the stark black-and-white of the emerging television programs of the '50s but is now in full X-file digitized color.

Moore could have put a stop to all this nonsense just by doing his science-journalist job well. He did not need to become a physicist! Unfortunately, Moore and Berlitz greatly indulged in the recycled fantasies of a foregone era (the last chapter of their book in particular doing the greatest disservice to the facts). Condemned to discredit, the story has been relegated to still more mediocre pens and sensationalist claims.

Through all its contortions, however, the story retained its one central myth -- the myth of an Einstein who had succeeded and was confirmed in his success by a Navy experiment but the truth of which, however, was too horrible to tell. It is curious that Smyth, to our knowledge, is the first writer to present a story where in fact, Einstein's UFT was tested but the result was negative and the horrors no greater than those associated with other tests in times of war. In this, he already breaks significantly with the tradition.

More recently, a highly speculative suggestion has been made by **Nick Cook**, Editor of *Jane's Defense Weekly*. Supposedly, the so-called 'Bell project' of the German SS under Gen. H. Kammler was a Nazi counterpart to the 'Philadelphia Experiment' undertaken to determine whether gravity could be controlled electromagnetically. Cook was told by J. Dering of California-based SAR, Inc. that Walther Gerlach commissioned the project as a test of Einstein's UFT.

But such a claim boggles the mind of anyone who knows both the history and the training of the German SS. For the last German wartime organization that would dream of testing anything from 'Jewish Physics' would be the SS State! Gerlach himself never had any interest in Relativity, nor was he a member of the SS body of so-called scientists. Much has been gratuitously made of Gerlach's sabotaging of the Aryan Physics movement (Lennard, Stark, Dingler, etc). Gerlach, in fact, was concerned about the failure of German physics -- in particular the failure of theoretical physics -- and what ill this boded for a German victory in the war but in particular, for his own ambitions to give the Nazis a nuclear bomb!

Gerlach's support of Finkelburg against Dingler in 1941 illustrates precisely how Gerlach entirely agreed with the ideology of the Aryan Physics movement, though not with the abandonment of physics that had become in his eyes its irrational emblem. It is with Gerlach's support that Finkelburg exposes Dingler's pre-1933 praise of Einstein and General Relativity. And Rosenberg's office (the organizer of the Aryan Physics movement) supports this exposé.

In 1943 when Prof. Abraham Esau falls into disgrace with Albert Speer and Hermann Göring, Gerlach replaces Esau as the "Reich Plenipotentiary for Nuclear Physics" responsible to Göring's Reich

Research Council. While his immediate objective is to develop a “nuclear explosive”, he promptly makes sure that Heisenberg (whom he mistrusted) comes under his control. But by then, German research efforts had begun to taper off. Despite Gerlach's efforts, his sleepless nights of work and his neglect of his health, and despite a focused German effort to develop an atom bomb, the backwardness of German physics remained an insurmountable obstacle throughout the war.

To the end, Gerlach and his group led by K. Diebner constituted the most advanced German nuclear research group committed to developing a nuclear bomb. Though Gerlach had been a leader in creating degaussing facilities for the German Navy, one is hard pressed to see him suggest a test of the much reviled theory of General Relativity (GR) let alone one based upon any of the Unified Field hypotheses. Moreover, the suggestion that the SS itself was responsible for such a test of the UFT/GR is even more dubious for not only was the SS “line” scornful of Einstein's theories, but the SS would also certainly not have needed Gerlach's support or knowledge, let alone his authority, to go about its business.

So, Cook's story just adds more embroidery to the myth of the success of Einstein's UFT and goes as far as suggesting that Gerlach himself collaborated with Einstein during the 1920s in joint efforts to test Einstein's UFT! Mind-boggling how one could try to sell that one! Cook surely does justice to his own name.

Tim Ventura's latest addition to this idiocy does not improve its standing or credibility. His suggestion that Project Rainbow could be explained(!) with the so-called '**Hutchinson effect**' -- when this so-called effect is haphazard, unclearly defined at best, and not even remotely understood by its “inventor” -- the terminally-confused John Hutchinson (so dear to the New Energy Movement) -- only adds more misinformation to the swelling aura of the fantastic, unexplained, and mythical. Nor does Ventura fare any better when he vents Dering's deliria. In fact, Dering's description of the SS Bell experiment makes it sound, at best, like a typical brain-dead SS experiment designed to test biological responses to high-frequency fields (possibly those associated with electromagnetic weapons).

Could modern researchers in electromagnetism, gravity, and the problems of the so-called Unified Field succeed where Einstein failed? There's always hope, but... we doubt that the problem can be so neatly packaged. The problem in a nutshell is that the framework of Einstein's Relativity is flawed. And the flaws are only aggravated as one progresses from the Special to the General and the Unified theories. For as long as one tries to keep to the relativist framework, there will be no approach that can resolve the real physical questions which that framework was trying to address.

This brings us squarely to Smyth's text and why we thought it should be published. If it is in error, it could not do worse than any of the texts that Allen, Barker, Crabb, Moore and Berlitz, Vallée, Dering, Cook, etc. have contributed. If it is fiction, it seems more factual than the “facts” these popularizers sold en masse. If it is not, it will have served to clear the field in a way that is very much to our liking -- and we suppose that's the reason why Smyth sent his text to us and did not send it to Cook, Ventura, etc. -- by demystifying Einstein's efforts at finding a UFT.

It seems to set these events back on their historical tracks by making clear that there was not one “Einstein's UFT” but several. And not one of them was successful in theoretical terms. It argues that Rainbow was a scientific and military series of tests of one-or-more of them. And that the tests were in all cases a failure. Plenty went wrong with them. But one of the main causes of failure was that nuclear magnetic resonance was not yet understood. And though not one of the UFT models tested was experimentally successful, the myth arose in the '50s that Einstein had found the correct UFT. This myth -- though directly contradicted by Einstein himself in his very last writing -- led nevertheless to the Allende letters.

This, in summary, is what Smyth's text says. And its suggestion is precisely that the discredit brought to bear on Project Rainbow -- ever since the 'Philadelphia Experiment' caper began -- is a way of hiding Einstein's failure while promoting the shared "secret" that he really succeeded.

It is now 50 years after the death of Albert Einstein and 100 years since the first paper on Special Relativity was published. The problems of a unified field are nearly 90 years old and -- if Smyth's text can be relied upon -- the secret experimental failure of the UFT efforts is now 59 years old. In all this time and despite innumerable attempts, no researcher, no lab, no physicist has been able to put together what Einstein left apart. And though Einstein's theories rest upon the most tenuous of experimental bases, few have seen fit to systematically criticize them [1-4]. Today, both Special and General Relativity are established theories -- part of the canon of Official Science -- and unified physical theories have been relegated to the status of "Theories of Everything".

Some still criticize Einstein for having ignored Quantum Mechanics. But it is hard to see what if anything quantum theories -- such as they stand today and have developed since 1920 -- would have been able to contribute to Einstein's dilemmas regarding a UFT. If this is true today, it could *a fortiori* only have been more true in Einstein's time and thus fully justifies his disregard.

To this day, no link -- theoretical or experimental -- has been discovered by relativists or quantum (car-)mechanics between gravity and electromagnetism that has not been just another fable:

1. Gravitational waves do not travel at speed c (contrary to Einstein).
2. And one does not need to assume that the Universe has a finite radius in order to understand or calculate G (contrary to Einstein).
3. The "spread-out" energy minimum that prevents Absolute Zero temperature from being reached anywhere is not electromagnetic (contrary to Einstein, Stein, Boyer, etc).
4. And the electromagnetic microwave background is not able to account for G (contrary to Haisch, Puthoff).
5. The electron pairs do not annihilate to produce either photons or gravitons (contrary to Wheeler, Ivanenko), and the graviton is not massive or supermassive.
6. The gravitons, gravitinos, and goldstinos of supersymmetry are no better than the gravitons, graviphotons, and graviscalars of metric theories.
7. And Sakharov's equation for G does not even provide for an intrinsic gravitational relation between 2 particles.

It is, therefore, little wonder that so much mystification surrounds Einstein, his UFTs, and Project Rainbow or whatever it was that the NRL was up to with Einstein and the Navy Bureau of Ordnance (NBO) back in 1942-43. Physicists -- Einstein included -- have sacrificed any real understanding of physical nature to arbitrary number games, rigging the solutions of real physical problems with abstract topologies endowed with time dilations and length contractions and false metric theories that entirely lack consideration of the structure of energy whether mass-bound or mass-free.

They have indulged in pure fantasy-production -- like the notions of spacetime with 4, 5, and even 'n' dimensions, the Higgs particle of Dark Energy, or the formalisms of QCD, the Hafnium Bomb, etc. etc. -- so they can hardly now expect to be respected when they deny any authority to the tall tales of UFOs being powered by the same knowledge of the Unified Field that "Einstein took to his grave" or of the 'Philadelphia Experiment' as a confirmation of Einstein's "final" UFT, etc. In point of fact, UFO abductees make about as much sense as physicists today do. It's become one global democratic asylum.

The myth of Einstein's success exists only as a political expedient: it appears to confirm the complex paths which scientific fantasy has taken over this past half-century for purposes of sensationalistic mass-consumption.

March 30, 2005
Paulo Correa, MSc, PhD
Alexandra Correa, HBA

References:

- [1] H. Aspden has the distinction of being the first physicist to present a theory of a dynamic Aether compatible with the null-result of the Michelson-Morley experiment while providing a systematic criticism of Einstein's Special and General Relativity theories -- see Aspden, H (1969), Physics without Einstein, Sabberton Publications, Southampton, England. Previous criticisms of Einstein like those of H. Ives always took refuge in the notion of the validity of the "residuals" to illegitimately conclude to a "fixed" or "stationary" Aether. Others like D. Miller concluded to an Aether drag in various forms (for a criticism of Miller, see Correa, P & Correa, A (2001) A note on Dayton Miller's supposed discovery of an Aether drift). All pre-Aspden criticisms of Einstein return one way or the other with more-or-less entrainment to the pre-Einstein concepts of a stationary or dragged Aether.
- [2] L. Essen -- the inventor of the cesium clock -- also expressed basic doubts about Special Relativity. See Essen, L (1978) "Relativity and time signals", *Wireless World*, 84:44; and Essen, L (1988) "Relativity: Joke or Swindle?", *Electronics & Wireless World*, 94:126.
- [3] Correa PN & Correa AN (2001) "Consequences of the null result of the Michelson-Morley experiment": the demise of the stationary aether, the rise of Special Relativity, and the heuristic concept of the photon", *Infinite Energy*, 38:47.
- [4] Correa PN & Correa AN (2001) "The Sagnac and Michelson-Gale-Pearson experiments": the tribulations of General Relativity with respect to rotation", *Infinite Energy*, 39:32.

C. Introductory Comments by Stanton Friedman

Smyth's text is both fascinating and somewhat confusing for people who weren't there or aren't familiar with the scientific, military, and political landscape of the first post-WWII decade.

In the 1940s and 1950s, many important scientists and engineers were involved in a variety of war-related shenanigans ranging from theoretical physics projects focused on Einstein's work to studying UFOs. These activities were pervaded by government secrecy and have been the subject of many bizarre legends and outright fabrications. The Internet -- with its potential for publishing widely differing viewpoints, documents, and uncensored accounts -- did not exist in the '40s and '50s. And it seems unlikely that we will get any closer to the truth now.

In Smyth's text, his interlocutor -- the mysterious "**W**" -- sheds some light on several episodes of scientific history and rivalry that took place within this framework. In addition to a number of prominent mainstream scientists and engineers of the time, a figure that plays a prominent role in "W's" recounting is **Wilhelm Reich** -- a medical doctor, psychiatrist and biophysicist who may have come up with some very exciting physics discoveries but whose work has been distorted and groundlessly dismissed.

He was certainly victimized by the U.S. Government. His books were burned; many of his papers were destroyed; and he died prematurely in a federal penitentiary. Many people have never heard of him or have only heard he was some kind of nut. And yet he may have made some startling discoveries that were much ahead of their times. Smyth's text discusses at some length his ill-fated relationship with Einstein who is the central figure of the story.

2005 is the 100th Anniversary of Einstein's publication of 3 incredible physics papers -- an appropriate time to publish this tantalizing material.

Stan Friedman
to [Stanton Friedman's home page](#)

D. Smyth's Introductory Letter

January 25, 2005

To Akronos Publishing -

Dear Doctors:

I took the liberty of sending you 2 chapters from my book Gone Dark because I felt that I could choose no better forum to tell about my meetings with often very weird characters in my field of investigation, or about my lifelong effort to grasp what I have been doing here all these years on this Earth. I do not expect to hang around for much longer, but I hope I will make it long enough to eventually see this book published.

The 2 chapters are interviews with a prominent [sic] man who was once a very active scientist – ‘Dr. W’. Please, do not bother to attempt to find out who he is. Just look at the facts he presents and decide for yourselves whether what he says is deserving of merit.

The Correias have put Reich back on his feet, so to speak, and I'd like to give a little push for that to continue to happen. I am not a student of Aetherometry and I'm afraid that it's just too late for me to learn new physics. But all of you have my express support.

I would be pleased if your publishing house would find it appropriate to distribute these texts for me for free. I waver all my copyrights and would only ask you to print a similar waver if you choose to publish this material in whatever form. I took the liberty of suggesting its wording below. You may excerpt the material, but I do request that you do not otherwise change the text.

Might you be interested in publishing the entire book? Let me know if you are and I will consider it.

Respectfully,

“W.B. Smyth”

Albert Einstein, Wilhelm Reich and the 'Philadelphia Experiment': Is there a spin to spacetime?

an excerpt from Gone Dark (Thoroughly Unremarkable Meetings) by W. B. Smyth

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NOTE. Editor's comments are in square brackets in **bold**.

E. Aftermath of the meetings between A. Einstein and W. Reich

S - And you heard about this fall-out [**between A. Einstein and W. Reich**] at that time in late 1943?

W - Yes, it circulated, you know. From Einstein's coterie or Reich's, unpleasant things were whispered and leaked. Reich's threat to publish their correspondence was not taken lightly in many circles. We were still at war and who-knew-what manner of secrets would come out?

S - Yes, but in a letter from March '44 that appears to never have been forwarded, Reich informs Einstein that he has instructed Dr. **[T.] Wolf[e]** not to publish their exchanges for the time-being in deference to Einstein's request [\[1\]](#). There is also a private memo to his lawyer Pete**[r]** Mills [\[2\]](#) where he **[Reich]** explains why he had complied with Einstein's request. He explains this was because Einstein had assured him that he hadn't put any untoward rumors about Reich into circulation.

W - What does that tell you? Get this: the entire process surrounding their communications was notarized by Mills who kept the full archive. Judging by how he later turned on his own client, I'd wager that he was one such source of rumors. I believe that it's in that same letter **[to Einstein]** that Reich repeatedly asserts how he's puzzled by the fact that Einstein had found no time to respond to Reich's rebuttal of the control results [\[3\]](#).

S - Yes, Reich is convinced that the responsibility for the “unpleasant situation” that has arisen between them is Einstein's.

W - He says to Wolf**[e]**, I think, that he will wait until he's able to understand the motives behind Einstein's “strange behavior” before making the matter public. Back to my point which is ever since that time, many people have come to believe this “affair” of Einstein with Reich was all about the orgone accumulator business. It wasn't.

S - ...but it started that way, no?

W - I'm not really convinced that it did. In another letter to Wolf**[e]** [\[4\]](#), Reich explains that he sought Einstein out because he had discovered a basic form of cosmic energy that was responsible for the gravitational field that Einstein's unified field theory claims must exist as a local distortion of the curvature of spacetime. This energy -- what Reich called the orgone -- was mass-free and not electromagnetic. In his *Ether and Relativity* lecture [\[5\]](#), Einstein had plainly stated that space is endowed with physical properties and that -- if I can remember verbatim -- “consequently there exists an ether”. He put it in simple terms that this new ether is a *necessary consequence* of the General Theory

of Relativity. He says that the ether of General Relativity is not the ether of Newton, Fresnel, or even Lorentz -- that it is ***not an electromagnetic ether***.

S - That's where the break with the fixed ether models occurs ...?

W – Yes. He calls it plainly enough a "gravitational ether" and describes its properties. It sets the metric qualities of any spacetime continuum. It sets the structure of gravitational fields and the molecular configurations of matter. He suspects that it might be involved in the structure of elementary charges and particles of matter, but confesses **[this]** is something that remains unknown. Do you see now the importance of that meeting between Reich and Einstein? Reich thought that he could provide Einstein's Gedanken **[thought-]**model of a gravitational ether with concrete physical evidence, with his own experimental discovery of orgone energy. Orgone and the “gravitational ether” would be one-and-the same thing. The ether that Einstein was talking about would be composed of mass-free energy.

J - I see, yes. Reich thought that he was providing the physical basis for Einstein's “gravitational ether”.

W - Little wonder that he was upset that something of so much importance and with so many implications would be neglected...

J - ...and of all people, by Einstein!

W - There you go. There were all sorts of implications, not just scientific ones.

J - So he couldn't understand Einstein's silence and was rather fearful of the consequences of rumors?

W - We'll get to that in a minute. Talk about consequences! It was the equivalent of an earthquake in Reich's life! He oscillated in his feelings for years after the events. It went from being one of the most fruitful and important meetings of his scientific life ... to a relationship that was hurting his own credibility and making him feel incredibly alone ... to a realization that he did not need Einstein nor could rely on him and had to do his own hard work in basic physics. It is a complex relationship! Somewhere, Reich comments that Einstein failed to understand the possibility of free energy in atmosphere.

J - ... and in '44, where was he at?

W - He still wanted Einstein to give him serious consideration, but he was coming to realize that this would never happen. To placate Einstein's indignation, Reich dotted the i's. He wrote that he hadn't accused Einstein of spreading rumors, and only wanted to draw his attention to the fact that others were spreading rumors, and how this was hurting his own work, and even Einstein's anti-fascist cause. As a precaution and to try to preserve a record of what happened between them, Reich deposited in New York and in Palestine sealed copies of all the notarized documents of the affair. Reich later referred to Einstein's behavior as “***the riddle***”.

Einstein had built his Special Theory of Relativity on the basis that a fixed ether does not exist. His work led him to search for the unifying principle between gravitation on the one hand, and electricity and magnetism on the other. But this was a doomed effort. Aside from the new topological descriptions of the gravitational field, there was little else that he could say about the gravitational ether or how it related to the internal structure of matter. The effort appeared to have been self-defeating.

J - What could Reich have contributed here? Anything?

W - He claimed to have discovered how orgone energy at once generated and maintained gravitational fields and the structure of matter.

J - And did he?

W - If he did, he took it to the grave. At any rate, he thought that he did. So he was mystified by Einstein's reaction. Within Reich's circle of collaborators, the debate was whether Einstein was aware of the implications of Reich's experimental discovery of the gravitational ether or whether Einstein had been so embarrassed by Infeld's criticisms that he disregarded the entire discovery -- that he didn't really understand it.

But Reich wouldn't admit that Einstein had failed to understand what he'd told him or its implications in light of all the perceptive remarks that Einstein had made during their face-to-face meetings. This suggested to him that Einstein harbored some ill intent against him. This perception was intensified because some of the "leaks" could be traced back to Infeld. Rumors had traveled all the way to the Soviet Union and back because Infeld, as you know, was a Stalinist informer. And he never paid for it.

J - ...and Einstein didn't know that at the time?

W - So it seems. What Einstein couldn't avoid was finding out that against his own wishes, Infeld had made the whole affair known to people from the Navy, Army, and the scientific community. In low whispers, the "Professor" -- and not just Reich -- had become the butt of ridicule. And that's really how the American stigma that Reich would bear to the end of his life was born, you know. There was considerable pressure soon after their meetings **[in 1941]** for Einstein to dissociate himself from Reich.

J - What kind of pressure?

W - Look, before Reich was issued a passport in 1940, he was investigated by the FBI for his past communist affiliations. That's what the FOIA files show.

J - Uh-huh ...

W - But then a year later -- a few weeks after Reich and Einstein's first meeting -- a planted denunciation is made to the State Department by the American Consul in Oslo, Norway. This leads none other than J. Edgar Hoover himself to take a personal interest in the matter in March of '41. The first internal response of the Bureau is that it's old hat and it had already been investigated. But Hoover himself comes back and orders the investigation re-opened at the end of April.

So over the exact span of time that involves the meetings and main correspondence between Einstein and Reich, Reich is being re-investigated under Hoover's direct supervision. An informer **[#D066]** conveniently pops up with the bogus allegation that Reich is a member of the American Communist Party. By May, the FBI concludes that he poses a dangerous potential threat to American national security! Custodial detention is prepared but for some reason, the FBI doesn't appear to want to act on it.

Then in July, Hoover decides that Reich should be considered for detention in case of a national emergency as war is already being anticipated. During this period, Reich wrote 3 more letters to Einstein -- the last as late as September -- all of which go curiously unanswered. On the very day following Pearl Harbor, Hoover asks the Special Defense Unit **[SDU]** to look into the case. And on December 12 at 2:00 in the morning, Reich is picked up and transferred directly to Ellis Island where he

stayed until his eventual release on January 27, 1942 -- some 5 weeks later [7]. There were, however, a few twists -- all of which seem studiously contrived. One twist is that Reich had filed a patent application in early '41 in between his 2 meetings with Einstein and which he thought was important in case Einstein sanctioned his work, as he expected.

J - What was the patent for?

W - I never saw it stated what it was for. Bbut it seems certain that it was for the orgonoscope and likely also for the orgone accumulator and the orgone field meter. The Patent Office requested a demonstration of the devices by December 15. And a meeting was scheduled, I believe, for that same Friday, December 12 -- the very day of Reich's arrest. Another twist is that Reich was suspected not just of being a communist but also a fascist! Hoover himself suspected Reich of fascist tendencies!

J - Wild!

W - Hoover says this when he orders yet another investigation after he's informed that Reich had been released from Ellis Island! The last twist is the matter of a 'William Robert Reich' who does belong to the American Communist Party and is repeatedly -- and deliberately it would seem from reading the files -- confused with Wilhelm Reich. This keeps happening all during the re-opened FBI investigations in '42 and again in '43.

J - Despite the national war emergency, it seems a bit peculiar. It would have been enough for the appropriate channel to inform Einstein that Reich was under FBI investigation for Einstein to want to keep well away from him ...

W - ... and if the warning suggested that he could be a Nazi agent, this would have been sufficient to deter Einstein from pursuing any further contact with Reich or from answering his letters.

J - Did Reich overlook this possibility?

W - He suspected the rumors had been planted by Stalinists and he knew that during their meetings, Einstein had shown genuine comprehension and considerable interest in his work. He might have suspected that Infeld had embarrassed Einstein. I don't know. When he found out during his interrogation at Ellis that he was also suspected of being a fascist, he was livid. Yes, from the Ellis Island nightmare he might have wondered what else was being sold to Einstein -- that he was a charlatan working for the Nazis and trying to gain access to Einstein's papers on military secrets?

J - Atomic secrets?

W - Who knows?

J - So, one would have to say that there would have been something severely wrong with Reich if he *didn't* show any symptoms of paranoia after such malicious persecutions! But what you're also saying is that interfering with his attempts to remain in contact with Einstein was one of the reasons why he was placed on that custodial list for possible detention prior to Pearl Harbor. It seems, though, that you're suggesting that his work and his patent application were also significant factors in timing exactly when he was to be rounded up!? Shouldn't the intelligence powers have waited for his demonstration before arresting him in that Gestapo fashion?

W - We'll never know what would have happened to his patent application had Pearl Harbor not occurred. But it is likely that when his case was reviewed by the SDU at the express request of Hoover,

a G-2 flag would have popped up. Especially after Pearl! So, one way of explaining that 2:00am arrest is precisely because of G-2 involvement if not after word got out that Reich had met with Einstein -- or after he filed a patent application -- then for sure on the heels of Pearl and Hoover's request the morning after.

J - Why G-2 -- that's Army intelligence?

W - What you might not know is that Army General George Strong was not just head of G2 but also at the time the head of the Patent Office. There is a way in which all these matters are intimately connected: the contacts with Einstein, the patent application for the orgone accumulator, the field meter and the orgonoscope -- including the one that Reich loaned to Einstein and which Einstein didn't, by the way, want to return -- and the evidence for a mass-free gravitational ether. Just as there is a link between Reich's contacts with Einstein, the FBI's insidious allegations that Reich was either a communist or a fascist, the G-2, and the Patent Office.

When Reich first contacted Einstein, America was not yet at war. The Commissioner of Patents was Strong -- a personal friend of Vannevar Bush and Nelson Rockefeller -- and the chief of G-2 intelligence in charge of projects ULTRA and MAGIC. **[Vannevar]** Bush was then the Director of the Office of Scientific Research and Development **[OSRD]** -- the most powerful office in the land. As Bush described in his book Pieces of the Action, there was a crucial military need to control scientific information and patent applications. Patents were considered essential assets because they could potentially tip off enemy intelligence on sensitive technology or on technologies prematurely or inadvertently ignored by U.S. interests.

The OSRD had a special committee dedicated to reviewing all submitted patents that related to atomic energy and to any other technology suspected of being sensitive. By order of the President, all government patent rights arising from any invention and potentially relevant to war developments were to be placed in the custody of the Director of the OSRD. The current patent office **[USPTO, United States Patent and Trademark Office]** policy of not publishing patent applications unless patents are granted was introduced back then, you know, as a protective measure to ensure the secrecy of sensitive applications or of patents denied for sensitive political, economic, or military reasons.

The contents of Reich's patent applications will therefore never be known. What one knows for sure is that had his patent been granted during Strong's mandate, Reich could never have been later prosecuted by the FDA under the fabricated contention that his orgone accumulators did not function the way he claimed they did. And he would not have ended up in the FDA Museum of Quacks.

J - Likewise, had Einstein confirmed the temperature difference and not succumbed to the trivial explanation supplied by Infeld, the Patent Office would have felt constrained to accept Reich's application and issue a patent, no?

W -- Yes. Now you can imagine the pressure that was brought to bear on Einstein when he was still wondering about the positive thermal difference, still reviewing Reich's long rebuttal letter [\[8\]](#), still interested in the observations he was making with the orgonoscope. There was a convergence of interests. Before Einstein could make the "mistake" of helping Reich by behaving as an honorable scientist, Bush, Strong, and Hoover all got to him with their respective crazed suspicions. Infeld -- with his own Stalinist agenda -- was only too happy to help for it was he who first drew everybody else's attention to the meetings.

J - And as you said, Reich repeatedly referred to Einstein's strange behavior -- the silence in response to his letters, the multiple delays, and misunderstandings in returning the orgonoscope, Einstein's written

denial that he had initiated rumors that were prejudicial to Reich, etc -- as '*the riddle*'. He seemed to think that once he approached Einstein -- and once Einstein understood that the discovery of the orgone would substantiate the notion of a unified field responsible for both gravitation and electromagnetism -- he would undoubtedly have his undivided attention.

W - Yes, but his later explanation suggested that Einstein did not see matters in this light. One way of interpreting Einstein's expression that Reich's discovery of the orgone was "a bombshell in physics" is to follow Reich's reading of it at the time -- that this discovery might provide the solution to the unified field which Einstein had predicted that it existed but had failed to provide [\[#D099\]](#). Another way of interpreting it is that Einstein had sensed something that threatened his attempts -- and Infeld's! -- to provide that unified field solution.

J - But Einstein was already afraid for the future of relativity and the loss of his efforts to discover a unified theory, well before Reich showed up at his door!

W - Yes. But maybe during that crucial meeting with Reich, he sensed that Reich's work was pointing towards an entirely new cosmogony. This is what Reich himself intimates in that short text entitled precisely "*The Riddle*". But then in his own handwriting, he added a note sometime later that concludes that he had been wrong in thinking that his discovery of orgone energy was compatible with Relativity after all -- let alone that it could have been construed as a verification of Einstein's field theory.

J - Hmmmm...

W - Read the man -- that's what he says!

J - So, he concluded that he had been wrong in having approached Einstein to begin with -- not because Einstein wouldn't have understood what he told him, but because Einstein would have understood enough to sense that it was a possible threat to his own and Infeld's efforts?

W - Yes. That's how Reich eventually solved his own riddle, I think. He came to the conclusion that his theory really wasn't compatible with Einstein's and that Einstein must have felt threatened.

J - I never fully understood how Reich could have thought it was compatible with Relativity to begin with.

W - Well, the Special Theory assumed there was no fixed luminiferous ether -- an assumption that was compatible with the negative result of the Michelson-Morley experiment but compatible also with Reich's view of a non-electromagnetic mass-free ether that he found to be in permanent motion and capable of driving the precise movement of celestial bodies. Likewise, Einstein's equivalence of mass to energy was compatible with Reich's framework and so was -- back then, that is -- the notion that photons were likely mass-less particles. Einstein's topology still appeared as the geometry of light. His notion of a gravitational ether could be made to coincide with the gravitational properties that Reich claimed for orgone energy ...

J - ...and, I guess, the notion of a unified field. The unified field wasn't electromagnetic but surely had to be gravitational and involve some fundamental form of spin.

W - Yes, uh, not quite... It needed to be able to give rise to gravitational fields but somehow interact with electromagnetic fields, so the more pressing problems facing the unified field hypothesis concerned the *high-frequency interaction* of electromagnetic fields with matter. Reich claimed this interaction

was not really electromagnetic but an orgone- mediated interaction -- which is the old problem of *Tesla waves revisited*.

F. Einstein and Project Rainbow

J - How did Reich eventually come to the conclusion that his own theory wasn't compatible with Einstein's?

W - Through his own experimental research in the post-war years. Reich came to the realization that Einstein's Unified and General theories had literally banned any systematic analysis of physical nature in exchange for a theory of topology -- not even geometry or metrics. But before I try to answer that question, let me get on with my story and then maybe at the end, you'll understand better why Reich became convinced that his own theory wasn't compatible with Einstein's field theory after all.

Let me get back to their meetings and correspondence because there is a very important aspect to this matter that no one has noticed. In one of his letters to Einstein [8], Reich reiterates the surprising claims that:

- magnetism and magnetic fields were poorly understood properties of orgone energy
- Earth magnetism is not ferromagnetic
- he has magnetized dielectrics in an orgone accumulator
- orgone fields act transversely to electric fields and in the direction of magnetic fields.

J - That would seem to suggest that there is such a thing as “**magnetic energy**”. And that orgone would be the same as magnetic energy -- just another name for it.

W - Yes, in a way. It's a curious argument and one that suggests that the magnetic field is an ambient reaction of the orgone to the flux of electric charges.

J - But then magnetic and electric fields would belong to different energy manifestations, no? The energy of the medium versus the energy of the currents?

W - Yes, and that could be just the kind of parallel relationship required to provide a satisfactory solution to the Unified Field problem if, in fact, magnetic fields could be simply a reaction from a medium. Reich told this to Einstein -- including the fact that he was working on a theory of the magnetism of dielectrics -- and, at the same time, claimed some surprising results like their ability to make magnetic compasses spin.

J - Would Infeld and Einstein have dismissed that too in light of their own work on magnetism?

W - Yes. That's a very curious question, isn't it? Infeld had been helping Einstein since the early 30's in his quest for the Unified Field solution suggested by the General Theory. More than a decade of effort, but little came out of it. Moreover, the Einstein of 1941 was a very different Einstein from the one of 1920 with his gravitational ether ideas. Or even from the Einstein of 1930 pursuing the elusive Unified Field. In 1938, he and Infeld stated something to the effect that all models of the ether had led nowhere -- that it was time to forget its name and never mention it again.

J - Anathema! But they were just referring to models of an electromagnetic ether, weren't they?

W - In context, yes, I believe you're right. But the way it was phrased, it had a definitive tone to it. Einstein and Infeld had cast their damnation and had officially given up on the problem of any ether. Even a gravitational ether.

J - But why? What made Einstein change his mind?

W - He couldn't for the life of him find a way to tie the gravitational field to an energy system or to successfully unify gravitational and electromagnetic fields. He wanted a topology that reflected energy content and generated both a metric and a geometry. But all he had to work with was the mathematical thought of an abstract topology that pretty much lacked any energy considerations.

His formalism was choking him. And he couldn't -- no matter which way he turned it -- squeeze out of it any tangible relationship relevant to the internal structure of matter. The divide between the quantum-mechanical world of matter and electromagnetism and the world of the gravitational ether seemed impassable. On considerations of geometry alone and employing Riemannian curves with 5 components, he had been led to conclude [10] that the geometry of space-time "caused" gravitational fields. And these bent light rays.

J - Is this how the U.S. Navy later became interested in his work?

W - In essence. But you cannot yet see the connection. I think that it was in July of '42 when Einstein approached [Dr. Vannevar] Bush because he wanted to participate directly in the war effort. Formally, it wasn't until early '43 that Bush appointed him to be a member of his committee [the OSRD] -- the same committee that was also in charge of the scientific-civilian part of a special project that has remained classified to this day. This project originated from work done at the Naval Research Laboratory [NRL], located on the Virginia side of the Potomac, just south of Washington, DC and across from Alexandria.

At the end of '41, the Special Developments Section of the Radio Division in charge of developing countermeasures had learned that Royal Navy researchers had found a method to bend the German control beams first used in guided bombers, and later on gliding bombs and the V1-flying bombs. At the time, the Radio Division was under Ross Gunn's direction. But 'Doc Taylor' [Albert Hoyt-Taylor] was the Superintendent "in perpetuity".

J - Who was responsible for the Section?

W - A very creative fellow by the name of **Howard Lorenzen**. But between him and 'Doc Taylor', there was Taylor's assistant Lou[is A.] Gebhard[t], previously from Carnegie and Marconi Wireless Company. Until '42, the Section had mainly focused on jamming high-frequency radar; the development of electronic chaff countermeasures; and sensitive, high-gain receivers for detection. But with Gebhard's support, Lorenzen progressively steered the Section to focus on radio countermeasures. The idea was to bend the radar bounces and replace them, successively, with displaced ones so that the receiver got the wrong location of the target. By mid '42, Taylor wanted to know the field intensities that would be needed to bend the beams sufficiently to generate such a false target image.

But it was a bad time for the NRL, for its Director Admiral [H.G.] Bowen, and for Technical Director Gunn. They'd just lost a major fight with some of the most powerful figures of that period -- Carnegie President [V.] Bush, MIT President [K.] Compton, Harvard President [J.] Conant, [F.] Jewett, the President of AT&T Bell Laboratories, and the wealthy [A.L.] Loomis of Tuxedo Park. They lost. And contrary to the predominant views, this was not simply because the Manhattan Project went to General [L.] Groves, to the Army Corps of Engineers and not to the Navy. No, they lost in a big way

because MIT's Rad Lab also took over the radar research, which was something that Jewett himself was not too happy with. Bowen and Gunn came out the losers. Gunn's expression was "we were hosed down". He described it as a trauma to their psyche that they never forgot. And it cost Bowen his position as NRL Director. However, the new Director -- **[Rear]** Admiral **[A. H. van]** Keuren -- covertly kept Bowen's directives and kept Bowen himself informed. I guess the Admirals had their own agenda.

J - Was Bowen removed just to pacify Bush?

W -- Yes. Some held that Bowen remained the covert Director. I don't know. The administrative fights didn't seem to concern us much. But Bowen's departure did. But work kept on going under great pressure. I think it was Taylor who came up with the idea to send Lieutenant Commander **[F.L.]** Douthit to liaise with Einstein. It might have been von Neumann who gave it to Taylor.

Anyway, by June '43, Einstein had become a consultant for the Navy's Bureau of Ordnance in a hush-hush NRL project whose precise purpose was to test whether light could be bent by a man-made artificial gravitational field. The project was called **RAINBOW**. And the proposal was written by Einstein, von Neumann and Taylor. Gunn, **[E.O.]** Hulburt, Gebhard**[t]**, and **[O.]** Veblen also contributed to it. It permitted Einstein to go once more over the problems that he'd faced with his Unified Field Theory but having a practical goal in mind this time: to see whether **it would be possible to distort the electromagnetic perception of a ship's location -- or even render it entirely invisible --** by manipulating the gravitational field of the ship or around the ship.

J - How was this to be achieved?

W - Nobody knew. That's why Einstein had to write the darn thing and come up with a way to implement it. At first, Einstein told von Neumann and Taylor that he had to think about it. Meanwhile, Taylor discussed the task with the Radio and Radar Divisions which is when someone suggested that intense electromagnetic fields could interfere with nuclear spin. Someone else added that this could be the basis for space-time distortion and for creating magnetic and optical camouflage.

J - This was at a meeting?

W - Yes, a meeting of the responsible Section chiefs at the NRL. It might have been Hulburt -- the chief of the Heat and Light Division -- and the head of the Special Developments, Lorenzen. Hulburt was brought in because the project concerned the optical image of a target as much as the magnetic and radar images.

G. Einstein's unified field theories and Project Rainbow

J - My impression was that Rainbow had older roots than those relating to the problems of a Unified Field Theory?

W - Yes, there are several precursors. Rainbow was a convergence of efforts addressing electromagnetic countermeasures for guided missiles, magnetic and electric countermeasures for magnetic fuses, and optical countermeasures for ship and airplane recognition. Einstein's Unified Field predictions could potentially impact all of these. That was the idea.

J - Tell me about the magnetic countermeasures program.

W - This was a joint Anglo-American Navy project that goes back to 1939 when the Germans began laying magnetic mines with aircraft. At the time, Captain **[Hollis M.]** Cooley was still director of the NRL and he answered to Bowen who was in charge of the Navy's Bureau of Engineering. Gunn was already the Technical Director and chief of several Divisions (one of them was Electricity and Magnetism) that took over that problem.

With the shift of the NRL from under the Bureau of Engineering back to the Secretary of the Navy -- if I recall, hmmm, under the auspices of the Bureau of Ships -- Bowen became NRL Director and a major effort was initiated to develop countermeasures and understand the basic science behind them. The Naval Ordnance Laboratory **[NOL]** also got involved through Commander **[J.B.]** Glennon, Officer-in-Charge of the NOL, with Dr.s **[R.C.]** Duncan (in charge of scientific matters) and **[R.D.]** Bennett and **[F.]** Bitter in charge of degaussing. Duncan had asked **[Vannevar]** Bush for help. And Bush had recommended Bitter from MIT to serve as scientific liaison between the Navy Bureau of Ordnance **[NBO]** and the Royal Navy. Bitter had the rank of Navy Commander during the war.

J - Did **[Lt.] Townsend Brown** have a role in this project?

W - He was the junior officer in charge of magnetic mine sweeping. In 1940, **[R.W.]** Ladenburg had suggested that sufficiently strong electromagnetic fields could be used to counter torpedoes and mines. If powerful electromagnetic fields could be employed to distort space-time and to interact with the Earth's gravitational field, then it might also be possible to bend light rays; produce optical, magnetic, and radar illusions; or even to achieve total electromagnetic invisibility.

J - You mean optical, magnetic, and radar invisibility all at once?

W - Yes, if strong magnetic fields could distort space-time this would alter the propagation and reflection of all electromagnetic signals. So, the idea arose whether one could employ the "degaussing" methods that remove the stray magnetic field generated by the magnetized iron of ships to create a controllable gravitational field distortion.

J - I don't understand. I thought that Einstein's General Theory permitted electromagnetic fields to interact with gravitation -- to be bent by the curvature of space-time -- but not to cause it.?

W - Yes. It was more of a geometric constraint on light than an actual interaction in the physical meaning of the word. But that is so. Einstein's insertion of Maxwell's theory of the electromagnetic field into his own theory was pretty forced. And he was quite aware that his treatment as it stood wouldn't really permit what has been called a "unified field theory" -- a UFT. All field equations -- gravitational or electromagnetic -- should be derived solely from the internal logic of the theory. What he called "a unitary and logical theory of the total field".

The departure point for all this was the topological notion that there are 2 families of curves in space: (a) those defining the structure of gravitational fields and (b) those defining electric fields. It might be possible to find a dynamic topology that could generate both types of curves from a single set of equations. But he admitted that to succeed at this, one needed a much better understanding of the physical nature of matter.

That's where the problems of magnetism and quantum mechanics come in. But he basically ignored them. His 1916 General Theory proposed a model for the bending of light rays operated by the tensors describing the curvature of space-time. And he argued that the energetic action of the gravitational field acting on matter transmitted its impulses to matter through the space-time curvature. But Einstein, you have to see, was very careful to limit the use of the General Theory. He often repeated that it can't teach

anything about the structure of matter. And he pinned his hopes on a joint theory of electromagnetism and gravitation that was yet to be discovered.

J - Is that why we get to the UFT? To solve the problems left hanging by the General Theory?

W - Not directly, no. At first, the General Theory appeared to be independent of the Unified Field problems. And that's even how it's still sold. But the fact is that the real challenge of the General Theory was whether-or-not it could lead to the Unified Field.

So in the mid-20's, Einstein goes through repeated drafts of a UFT. Because of quantum mechanics, he knows full well that Maxwell's equations can't apply to very intense electromagnetic fields. But he's trying to bypass quantum mechanics altogether. Others doubted it could be done and explicitly suggested that the field approach was inappropriate. But Einstein believed at various moments that he'd found a definitive or smooth solution.

J - When was this breakthrough?

W - You mean by "breakthroughs" what Einstein thought were breakthroughs at different times. Between 1927 and 1931? He produced several attempts with slightly different formalisms. And he published several papers on the subject, beginning in 1928. 2 main versions resulted -- one published in 1929 and the other in 1931. Both were presented to the Academy of Sciences in Berlin. And neither was well received. I think it was Max Born who referred to them as a great tragedy -- that Einstein had been wasting his time.

J - What were the differences between the 2 versions?

W - In 1929, Einstein thought he'd succeeded in introducing a tensor for the electromagnetic potential. But by 1930, he changed his mind. He'd also introduced a topological torsion tensor that reflected the helicity of magnetic fields. Within the Riemannian geometry employed by the General Theory, the torsion tensor was simply assumed to be zero. There was no spin of space-time, and thus no asymmetries of distance in geometric terms.

J - What do you mean?

W - Simply put, that a given path across a region of space will not necessarily be equal to the return path. If space-time has a torsion, the metric tensor will have antisymmetric properties. But if the torsion is zero, the helicity can be disregarded.

Back in 1922, **[E.]** Cartan had proposed a theory of spaces with torsion to follow up on his own 1913 theory of spinors. And **[J.]** Schouten in 1923 had proposed a topological representation of the electromagnetic field based on the torsion or twisting of a four- dimensional continuum. These are problems that geometrically belong to the distortion of a metric, and topologically belong to the 'teleparallel' dislocation or transport of vectors in spacetime. What's important for you to retain, though, is that, if the torsion is not zero when the electric field vanishes - as is the case for a perfect plasma - then plasma motion along magnetic field lines could generate a co-linear electric field.

J - Like a dynamo effect?

W - Exactly. For a spinning body like a planet, this co-linear electric field would be somewhat like the vortex of stacked eddy currents generated on a non-laminated iron core by magnetic induction. The first attempt at a unified field theory was made by **[Th.]** Kaluza. He employed Einstein's 10 gravitational

potentials and the 4 components describing the electromagnetic potential, but in a 5-dimensional continuum so that the paths of the motion of charges coincided with the geodesic lines. [O.] Klein and Einstein worked on this in 1926 and '27.

In his 1931 variant of the UFT, Einstein refined his formalism by adapting Kaluza's theory of the total field. Instead of Kaluza's 5-dimensional continuum, he followed Veblen and stuck to a 4-dimensional continuum correlated in parallel with a 5-dimensional "linear vector space". He thought his approach succeeded where Kaluza's had failed in establishing a constant relation between the electrical mass and the "weighty" mass of a "material point". He believed that he had successfully joined Maxwell's first system of equations with the equations of gravitation, connecting them through the curvature of space-time. He left open the question of the anti-symmetric tensor and didn't even touch the possibility of a torque to space-time.

But he was satisfied that his approach appeared to work for gravitational and electromagnetic fields in space devoid of matter. When matter is introduced into the equation however, he admits that his only recourse is to resort to a fiction -- the term "density of matter" and the tenuous assumptions regarding its distribution.

J - Is this where the famous *cosmological constant* makes its appearance?

W - Yes. That was one of the gimmicks that he used to adjust the overall energy density and fit it in with the dogma of the accelerated expansion rate of the Universe. Later he was very ambivalent about this procedure. Moreover, the unified system of equations only applied to space containing matter IF the equally tenuous assumption of no magnetic mass was also made. So he admits that the nature of these points as material particles is still not understood. That their corpuscular structure or graininess remains a mystery. They are still only topological singularities, even if one calls them "material points".

J - So the solution couldn't be so definitive after all!

W - No, it couldn't. And it wasn't. And the solution that he -- along with Infeld and [B.] Hoffman -- presented later in 1938 for the total field sustaining the motion of many bodies only considered isotropic distributions. If there were torsions in space-time, they were not considered.

J - But they certainly would have to be taken into account by Project Rainbow, wouldn't they?

W - Yes, yes. All the possibilities had to be taken into account. In particular those that involved nonzero torsion tensors or skew tensors. Space-time could be deformed not only by the rubber-band analogy but by a spherical distortion -- a spin -- if a full integration of the electromagnetic field was to succeed.

That's also one of the reasons why in 1941, **Reich** thought that his own discoveries about orgone-induced magnetism was pertinent to this problem of a Unitary Field description. Do you see my argument?

J - Yes, I'm beginning to ... hum-hum...

W - It's also why, in parallel, demagnetization experiments with very intense electromagnetic fields became significant in '42 to '43 because of the development of magnetic fuses for mines and torpedos. And all these lines were frantically converging at the NRL in the very desperate context of the war effort.

J - And do these 2 lines have a direct connection between them? I mean, space-time twisting and demagnetization experiments?

W - One connection is that a deformation of space-time which is transverse to an axis of spin is every bit analogous to Reich's notion of cosmic superimposition between 2-or-more orgone energy streams that create spiraling or spinning orgone envelopes. Because of their energy density, these structures would more likely be discoidal than spherical and would create flux tubes around their axes.

This is an extremely important clue, you see, because of the analogy between these cylindrical flux tubes and the 5-dimensional cylindrical treatment of the continuum that Kaluza proposed in his shot at a unified field theory. Reich's orgone envelope seemed to have all the conditions that were required to produce 2 different families of curves for the 2 resultant fields 2- gravitational and electromagnetic. Reich, as you know, was after the physics of energy ...

J – Mass-free ...

W – Yes. **The physics of a mass-free energy that would be responsible for creating what topologically appeared as a torsion to space-time.**

J - But Reich never explicitly addressed the descriptive problems of metric and topology, did he?

W - He worked extensively on the problems of the co-ordinatization of the solar and galactic systems. But his thrust was that geometry and condensation of matter were created by the superimposition of mass-free energy within the same space occupied by matter so that **it was space, in fact, that could be engineered.** Do you see?

J - Not entirely. But what's the connection to demagnetization?

W - Well, you see, demagnetization involves placing the permanently magnetized object that one wants to demagnetize in very strong electromagnetic fields generated by the pulsation of high-frequency currents. The object -- for instance, even something as large as a ship -- is placed in one direction and then is either placed in the opposite direction or is completely rotated through successive angles until it arrives at the opposite direction while the electromagnetic field is being applied

The effects of the induced alternating field is very much like the effect of imposing an oscillating diamagnetic field. This disorders the ferromagnetic structure of the magnetized body. And the disorder increases as the applied currents are gradually reduced. Reich had discovered how “orgone-charged and magnetized dielectrics” disturbed electromagnetic instruments and iron- magnetic needles, even though they had magnetic properties quite distinct from those of iron-magnetism or paramagnetism.

H. Faraday and the magnetic nature of space

J - I'm afraid I'm still not clear on all the important differences and connections...

W - Think of it this way. Paramagnetic substances tend to orient their long axis parallel to the magnetic force vector and are attracted to one of the poles of the field either in parallel or in an anti-parallel orientation, the parallel orientation being the most frequent. This is also called the lower energy state. Their permeability to magnetic fields is slightly greater than unity so they act like a magnetic lens that makes the lines of force converge. Aluminum, platinum, manganese, and chromium are examples of paramagnetic substances.

J - But I thought that iron was also paramagnetic?

W - Yes, but truly so only when it's heated to 786 degrees Centigrade! Ferromagnetism is a special instance of paramagnetism for high permeability substances -- one that involves a "cooperative alignment" of molecular magnetic domains. There are only 3 elemental ferromagnetic substances: iron, nickel, and cobalt. And they all cluster together between atomic numbers 26 to 28. Ferromagnetic substances are magnetized by the geomagnetic field -- by magnetic induction. The pole of the compass needle that points North is actually a South pole.

J - Yes, that much I remember. But so, the effect of orgone energy was neither paramagnetic nor ferromagnetic?

W - Quite. The effect appeared to be diamagnetic. And diamagnetic substances have low magnetic permeability. Less than unity. They act as divergent magnetic lenses. They avoid the magnetic field line, as though they're being repelled by the applied magnetic field. Some metals are diamagnetic like copper, zinc, silver, gold, antimony, bismuth and mercury. Dielectrics employed in friction machines to store electrostatic charge -- like glass, sulphur, rubber -- are also diamagnetic. The hydrogen atom or free radical is paramagnetic. But hydrogen gas is a diamagnetic substance because, normally, the magnetization of one atom cancels out that of the other. An air flame is diamagnetic and is repelled by either of the poles of a strong magnet.

J - Do diamagnetic substances align their long axis predominantly in antiparallel orientation?

W - No, not quite. They align their long axis perpendicular to either the parallel or antiparallel orientations of paramagnetism. It's true that a rod of iron suspended in a strong magnetic field will line itself up along the lines of the field because, as Faraday first put it, it tends to move from the weaker to the stronger parts of the field. In contrast, in the same arrangement, a rod of bismuth or glass will orient their longitudinal axes perpendicular to the magnetic force vector because it tends to move from the stronger to the weaker parts of the field. This was discovered by Faraday in 1845.

Others had observed the phenomenon before but had discarded it because they didn't understand what it meant. Faraday's studies showed that most substances or materials are diamagnetic -- not paramagnetic. In the absence of an applied permanent magnetic field, induced diamagnetic effects can be observed in diamagnetic substances that are subject to a changing magnetic field, like the action of a transformer or an induction coil. The effect only lasts for as long as the changing magnetic field is applied, and the induced diamagnetization is directed transversely to the inducing field.

The idea is that in the absence of an applied magnetic field, there is no spinning motion of the atoms of diamagnetic substances. In other words, diamagnetic atoms don't behave like small magnets -- not the way that paramagnetic substances do. And, you know, Faraday's discovery of diamagnetism had cosmic implications.

J - No, aside from his name and the laws of electrolysis and the charge unit that carries his name and a little more, I'm actually rather ignorant of his life and work.

W - So are most people nowadays. But what I'm going to tell you next -- if you have any patience left, um -- is the very beginning of the classical thought of a field theory in physics. You see, in that same year 1845, Faraday concluded that space -- mere space as he used to call it -- had to have magnetic properties. In fact, properties intermediate between those of paramagnetics and diamagnetics. This is

what Faraday called the magnetic 'zero- point' of the vacuum. Only these physical properties permitted the reality of magnetic lines of force and their persistence in a vacuum, in empty space.

Becquerel later suggested by analogy with Archimedes' principle of buoyancy -- and to preserve Ampère's Law, it's true -- that diamagnetic repulsion could be understood as a differential form of magnetism with attraction and repulsion being seen as a matter of the relations between a test body and the medium. Of course, the fundamental problem with this approach is that it failed to explain why in a vacuum, diamagnetic repulsion persists just as strongly. For as long as one assumed a luminiferous ether medium that had magnetic properties, one could get out of this problem by pointing out that the medium itself drove the repulsion of diamagnetic substances. But this solution brought in turn a whole new batch of problems. If the ether was magnetic, why did one need to invoke the action of matter that was more magnetic than diamagnetic substances?

Much later, Maxwell returned to Becquerel's hypothesis. He saw it as the basis for the existence of circular currents in the electromagnetic ether -- something that Faraday had anticipated, but said it stretched his own credulity too much. Before Maxwell, however, Oersted and especially Weber vindicated Faraday instead - diamagnetism had to be a new force of Nature because the repulsion was irrespective of magnetic polarity and a consequence of same-pole induction. Same-pole induction generated in diamagnetics molecular currents, otherwise absent, that were opposed to the molecular currents in the inducing magnet. Paramagnetics were subject to opposite-pole induction, diamagnetics to same-pole induction.

Weber showed this with an induction apparatus coupled to a falling rod of iron or bismuth, and also showed that the induced currents had opposite polarity. Even so, Faraday later abandoned Weber's notion of diamagnetic polarity because he concluded instead that diamagnetic substances didn't have magnetic polarity, nor the closed currents required by Ampère's theory.

J - So, how did Faraday resolve the problem of magnetic polarity?

W - Actually, he didn't. He transposed the problem to the physical reality of the magnetic lines of force. In a series of experiments, he refuted Weber's findings and concluded that polarity is a directional property of the lines of force.

J - As if these lines were part of an infinitely large closed circuit?

W - That's just the problem that Faraday wanted to avoid and that Maxwell jumped into head first. With his description of paramagnetics as converging lenses and diamagnetics as diverging lenses, Faraday came to the conclusion that magnetic polarity didn't really exist -- neither for paramagnetics nor for diamagnetics. He argued that paramagnetics simply intensified the applied field and diamagnetics simply weakened it. In media more paramagnetic than themselves, paramagnetics behaved as diamagnetics. And in media more diamagnetic than themselves, diamagnetics behaved as paramagnetics.

It all came down to the problem of transmission of the magnetic force. Unlike electricity, magnetism had no poles. Hence, no detachable magnetic monopoles could exist unlike charges of one polarity that can exist on their own. Magnetic lines of force couldn't be transmitted by "magnetic particles" -- not the way electrostatic lines were transmitted by contiguous electric charges. If you cut a magnet in half, you will always get a new set of 2 poles -- not 2 isolated monopoles -- one North and one South.

J - All right. But I still don't see how Faraday resolved it.

W - For a while, he considered whether the fluid ether hypothesis could explain the transmission of magnetic force along the magnetic field lines. He speculated whether this ether might produce vibrations transverse to the direction of electric currents or whether it would instead have longitudinal vibrations. He wondered whether light could be a longitudinal vibration and, if not, what would constitute such a vibration?

Confronted with having to assume a magnetic polarization of the luminiferous ether -- an ether tension -- he came to a very strange conclusion. That magnetic lines of force were stresses in space. They were a physical property of mere space that was only revealed when space was disturbed by matter.

J - I see ... this is the core vision of field theory, then?

W - Yes. The core axiom. Space has physical properties. One of these properties is to be strained by matter. And when strained by matter, it reveals the magnetic lines of force caused by that strain.

J - Yes, but don't you have to also add that this matter must be magnetic or at least paramagnetic with respect to that space?

W - That was just the problem that Faraday thought he had found an answer to. Previously for him, if diamagnetics had no polarity and did not respond to magnetic fields, it was difficult to see how matter, by itself, provoked that strain. But once diamagnetics became only divergent lenses for the lines of force, matter -- whether magnetic or diamagnetic -- would always produce a stress in space.

Stop and think about this for a minute and you'll see that it's very close to the guiding notion of a unified field. Space has magnetic properties. And so does matter. And matter can only make these strains bend one way or another. Like a filter.

J - Yes. And like Einstein much later, you could conclude there's no luminiferous ether, no need for it - yes, I see.

W - In a magnetic medium, you can only demagnetize substances that are more paramagnetic than the medium is. Diamagnetic substances -- less paramagnetic than the medium -- would not be susceptible to permanent magnetization. You would have to pick a more diamagnetic medium to see that happen to those substances because now they would behave as paramagnetic ones.

J - So, there was no molecular transmission of the magnetic lines of force? They could exist just as well in a vacuum because the transmission wasn't a "*mysterious action-at-a-distance*" but a physical stress in the neighboring space itself. Is that it?

W - Yes. For Faraday space had physical properties. It transmitted the gravitating, electric, and luminiferous forces as well as the magnetic force. The idea that the magnetic lines-of-force were physical stresses in the fabric of "pure space" was further reinforced by the fact that magnetic propagation along the field lines seemingly took no time. So he sought to differentiate space from matter, how the former acts differently from the latter, how the medium of pure space acts differently from material media.

J - How did he do that?

W - To answer you, I need to bring up an important series of facts seldom mentioned today. Our current explanations for diamagnetic behavior still resort to Oersted's and Weber's same-pole induction. Yet, the behavior of diamagnetics is far from uniform. Another of Faraday's discoveries sheds some light on this and also relates to Hulburt's specific interests.

It concerned the atmospheric variation in optical propagation and light frequency, and its dependence upon atmospheric heat, magnetism and electricity, in particular, upon the roles of paramagnetic oxygen and so-called diamagnetic nitrogen. I'm going a little quickly here, but I think you'll see where I'm headed. The first crucial observation made by Faraday is that a ray of plane-polarized light that's transmitted through a diamagnetic medium with a high refraction index -- like glass -- can be made to rotate when acted upon by a magnetic force.

J - So a magnetic field can affect light...

W - Yes. The effect of magnetism upon light depended upon the geometry of the applied magnetic field, the nature of the diamagnetic and the distance traveled by the light ray through it, and the intensity of the magnetic lines-of-force. At first, it appeared that the direction of rotation of the plane of polarization was positive or right-handed. That is, clockwise when seen by an observer placed at the end of the diamagnetic where the light exited. And that for ferromagnetic substances, the direction was reverse -- negative or counterclockwise.

Maxwell later used this observation to return to the argument of same and opposite pole inductions that Faraday had abandoned and concluded that ferromagnetic and diamagnetic substances cannot be simply explained by the lens-argument of "magnetic permeability" but must, in fact, have real opposite physical properties. But this re-establishment of Faraday's original polar argument had to be framed properly, because some diamagnetics like neutral potassium chromate produced negative rotations; other diamagnetics like quartz could cause rotation in either direction irrespective of the presence of an external magnetic field and dependent only upon the direction of light entry; and still others like turpentine only produced clockwise rotations irrespective of the direction of the light's entry.

It's easy to see that if the rotation with respect to the direction of the light ray entry was constant -- say counterclockwise -- an observer looking at a light pencil reflected back to its point of entry would see no rotation, as the two rotational effects would cancel out. But that's not what happens with turpentine or sugar solutions and so on when a magnet is applied. An observer at the point of entry and looking at the reflected ray will see a rotation which is double that observed at the point of reflection opposite the point of entry, and in the same direction. No matter whether it is the emitted or reflected ray, an observer at the point of entry will always see an accelerated counterclockwise rotation. And an observer at the point of exit or reflection -- an accelerated clockwise rotation. The ray returns to the point of entry with its rotation increased -- not cancelled -- as should be the case if it was due to a molecular action or a molecular transport.

According to Faraday, this action of turpentine depended on the diamagnetic nature of the matter but was not itself a molecular action. It was rather an indication that the magnet had induced stacks of electrical currents on planes transverse to the light ray and running counterclockwise when seen from the point of entry of the light ray.

J - Like a **vortex**?

W - Yes. That is exactly what Maxwell later concluded. Maxwell imagined that all materials present 2 uniform circular vibrations with opposing direction. **2 opposing vortices**. When they are equal in all respects -- periodic time, amplitude, acting on the same plane or the same longitudinal series of

juxtaposed planes -- they balance out to produce what he called a “rectilinear vibration” on any plane, anywhere where they meet.

J - I think I follow ... In ferromagnetic materials, one of the vortices carries over the other. And there's a net negative twist...

W - Yes, that's it! In diamagnetic substances if a magnet is applied, the other carries over and there's a net positive twist. It suffices to accelerate the phase of one of the vortices. And the plane position of that rectilinear vibration will rotate in the direction of the circular vibration that was accelerated. That's what happens in turpentine.

J - So, some diamagnetic substances only show positive rotation in the presence of a magnetic field while others show it independently, as if the substance had a natural diamagnetic order or “polarization”. Is that it?

W - Yes, pretty much. You see, Faraday was after the distinction between space and matter -- the magnetic field being a property of space that was disturbed by matter. In the same way that there were ferromagnetic substances such as permanent, saturable paramagnetics, there could also be diamagnetics which retained their structure just as permanently. Faraday wouldn't exclude the possibility that certain matter or material media might have properties identical to those of space. And he thought he'd found the model of space in nitrogen.

At first, he thought that nitrogen was diamagnetic. But when he used the torsion balance principle for detecting motion induced by a magnetic field and mounted on the torsion bar identical volumes of nitrogen at different pressures, they failed to show differential attraction and repulsion in the presence of a magnetic field. So, he concluded that nitrogen was like space itself -- neither diamagnetic or paramagnetic.

J - But according to Maxwell, wouldn't that just mean that the vacuum -- or pure space -- had to be formed by some sort of balanced vortices that magnetics or diamagnetics merely threw out of balance?

W - That's just the problem. Pure space cannot be empty. And when a magnetic interacts with it, wouldn't space have to behave like a diamagnetic substance and screw up all the magnetic lines-of-force? You see the problem - space never appears as a paramagnetic medium for diamagnetics, outside of them, just as it never appears as a diamagnetic medium for magnetics.

J - Somehow, Faraday's progressive scale between magnetics and diamagnetics fails because space is not really at the center?

W - Yes. It is and it isn't. Rather, space complies with the twist that is intrinsic either to permanent paramagnetics or permanent diamagnetics in the presence of one or of the other. Maxwell went on to think of molecular vortices aligned along the same axis and rotating in the same direction in analogy to a stack of coins. But he wanted to remove the longitudinal component of the interaction -- the very component necessary for establishing direction and time of propagation and for constructing the mental image of a helix.

These molecular fluxes could only embody the net result of those counter-coupled vortices and not be confused with one or the other vortex. That's why the molecular explanation does not work for empty space. What are the molecules forming the magnetic or gravitational lines-of-force if matter is absent? It all comes down to the physical nature of those lines-of-force. What is it that spins and what is it that forms counterbalancing spins?

That's where Reich comes in. Faraday's lines-of-force had no other physical reality that one could point to. And neither did Maxwell's counterbalancing vortices. All one was left with were phenomenalistic or mathematical descriptions that would take recourse to these types of abstractions only when necessary, and wouldn't need to suppose any physical reality for those vortices. Reich, on the opposite side of the spectrum, was convinced that **orgone energy could explain magnetism**. That it acted on the same plane as magnetism but in the opposite direction and perpendicularly to the electric field.

J - All right, let's see if I get this... Orgone energy would have diamagnetic properties. Yet in the presence of magnetic fields, it would develop a countervortex whose excess over the natural diamagnetic vortex would explain the magnetic lines of force.

W - Very good, very good. But you see what this implies?

J - Well, it means that space is not like nitrogen. That it has to have either diamagnetic properties or some excess of diamagnetism over magnetism.

W - That is one of the thoughts. And perhaps the best one. More profoundly though, it means that space is made up of mass-free energy. And that everywhere this energy is in states of superimposition or spin and counterspin ...

J - But when Reich gives spiral galaxies as Macroscopic examples of what you say, the 2 streams of mass-free energy minimally required for the process spin in the same direction...

W - That indeed happened much later. But I don't think Reich kept much to these ideas on magnetism past 1944.

J - Would the layering of ferromagnetic and dielectric substances in the orgone accumulator create magnetic force vectors that are alternately pointing at right angles to each other as one goes from layer to layer?

W - If the layers were polarized with respect to their stacking axis then, yes, they could present alternating oppositely-dominant directions of rotation. And what you say isn't entirely foreign to what Reich contended -- that the ferromagnetic layer continuously attracted and repelled the orgone flow, concentrating it, focusing it, while orienting its field along local geomagnetic lines. And that the dielectric or insulator only attracted the orgone and even "soaked" it in. But I think that these were still very primitive ways of describing the effect of magnetized dielectrics or what it all means.

Here's where the real problem emerges. How is it that certain diamagnetics -- even certain plastics! -- can be induced to acquire permanent magnetism without changing the medium from a more paramagnetic one to one that is more diamagnetic? You can see how important it is to determine exactly what physical characteristics of space permit the transmission of magnetic lines of force.

Reich's answer was that **all fields are orgone energy phenomena**. For Faraday terrestrial magnetism was largely a surface phenomenon caused by the paramagnetic properties of oxygen. For Reich at the time of Project Rainbow, geomagnetism was due neither to oxygen nor to an iron-nickel core in the planet. He thought it was the flux of mass-free orgone charges coursing through the Earth that generated geomagnetism.

And this was the alternative explanation he gave to Einstein. The Earth's magnetic field was not due to iron-magnetism, he said, but due to a magnetic reaction brought about by the interaction between the rotating mass and the diamagnetic properties of primary mass-free charges traversing the earth, concentrating at its core and feeding that rotation. This would equally explain why a magnetic compass in the Northern hemisphere does not point horizontally to magnetic North but points with a dip that reaches the vertical or 90 degrees at the pole.

J - ... and the same geomagnetism would be responsible for the magnetization of ships during their construction?

W - So there's your direct connection.

I. Degaussing and Project Rainbow

J - OK, now I see the connection between Reich, magnetism, and Einstein as well as the origins of field theory and the problems raised by the responses of the vacuum or the properties of space. Tell me about the connection to the military problem concerning the detection of magnetic fields in WWII.

W - All right. To develop effective countermeasures for magnetic proximity fuses, we had to understand not only how to remove the permanent magnetism of a ship once it was built but also how to remove its induced magnetization at sea -- the magnetization induced by its motion and its heading in the magnetic field of the Earth's dynamo.

For Reich, these effects upon the ferromagnetic structure of ships were caused by the streams of spinning mass-free charges that coursed through matter. By manipulating these fluxes, he thought one could either intensify a magnetic or electromagnetic field or, alternatively, neutralize them. In other words, from the point-of-view of remote detection, **make them go "dark"**.

J - Sort of like producing dark Faraday spaces in a glow discharge?

W - Sort of ...

J - Is it just a matter of destructive interference involving magnetic spins or beam cancellation?

W - No, no ... Well, in a way, perhaps. But the kind of "destructive interference" that I'm talking about would be far more fundamental. Not a "plane event" but a "volume event", if you will. It would be something that would have to be related to those counter-vortices of Maxwell or describe similar results. If the interaction was a property of space, maybe topology could get at its abstract logic. But IF it were a property of energy -- of an unknown form of energy -- then all bets would be off.

Would the interaction of space with high intensity magnetic fields still be the same? It was one thing to assume that a magnetic field interacted with light directly (but this, of course, could only be phenomenalistic!). But it was another thing altogether to realize that this interaction was mediated by **something**, by a property of space or a property of mass-free energy composing that space - a property which light **only indirectly** translated for us.

Now, a similar problem occurred for unified field approaches. There, it was assumed that light was bent -- not rotated -- because it interacted with space with the deformation or surface strain of space-time. If we consider torsion a lateral strain in pseudo-Euclidean space, then in a spherical system of coordinates we need a torsion tensor for space-time. This would imply some form of electromagnetic

feedback on the gravitational fields themselves. A mass-free energy spinor could be such a system. And it might be capable of deforming or distorting any electromagnetic signals employed to detect it.

J - Are you saying that the observations on magnetism that Reich certainly must have related to Einstein were not relevant to Einstein in 1941, but became relevant when he began working for the Navy in 1943?

W - No. They were relevant all along. Reich's notion of a superimposition of mass-free energy giving rise to a spinning mass-free energy field that generates self-enclosed envelopes -- or multiple spherical surfaces -- or to spinning and counter-spinning magnetic reactions was relevant all along. But Einstein had never considered energy that was free from mass. His famous equivalence between energy and matter $E = mc^2$ was symmetric. There could be no energy without matter.

But Reich's model was not symmetric. Mass-free energy existed “before” matter, he claimed. It was mass-free energy that created matter by superimposition and was responsible for its gravitational displacements. And this energy was distinct from the energy of the electromagnetic field which, for Reich, was energy “after” matter.

These quasi-ontological and cosmological notions -- the “before” and “after matter” qualifications -- were placed there to remind us that all matter is not just equivalent to energy but is energy. Whereas, not all energy is matter. Topological space-times -- and the field geometries they called for -- were, according to Reich, nothing more than reductionistic descriptions of superimposed mass-free energy. Spaces without matter would only exist by the constant superimposition of mass-free energy. The question of the metric could not be resolved unless one discovered the intrinsic metrics of mass-free energy. Einstein would have to accept that his most fundamental equation had this asymmetry -- that **all matter is energy but not all energy is matter**.

J - Which he never did, of course! You know, I'd studied some of Reich's work. But I had no idea this is what he had to contribute. I guess like millions of others, I never read him seriously. Still... it looks like despite Einstein having been read to death ...

W - ... supposedly!

J - ... he wasn't taken very seriously either. Or was he? Either way -- and from what you're telling me -- what an extraordinary series of blunders he made! So, how do we get from the demagnetization of a ship to an experiment that implicates predictions by Unified Field theories that introduce space-time torsion?

W - Einstein's connection to the Navy problem came from the questions provoked by the use of the very powerful electromagnetic fields involved in degaussing, from the seemingly unsolvable UFT problems raised by the lack of consideration of the magnetic mass of a “material point” and from the possibility that there could be a spin to space-time.

J - All right, first the degaussing story...

W - Yes... Under the direction of [**W.**] Gerlach [11], German naval research into torpedo and mine fuses successfully developed a murderous magnetic proximity fuse. They had also begun a comprehensive program of countermeasures which focused on ship degaussing. By early 1940, the [**U.S.**] Navy's Bureau of Ordnance -- with Neumann and Veblen in charge of solving this problem -- was working on its own countermeasures, prompted by British reports of the new magnetic German mines. So Francis [Bitter] was sent over from MIT. Remember that America was not yet at war with Germany.

As I recall, it was in November of '39 that the first magnetic mine was captured -- right in the Thames Estuary -- and defused, just as Bowen took charge of the NRL. A secret NBO salvage operation led by [T.T.] Brown brought back the mechanism of another captured magnetic mine and Francis [Bitter] discovered that when the residual magnetism of a ship distorted the local geomagnetic field of the sensor, it activated the magnetic needle of the trigger. Francis and his colleagues started to wonder how much of the ship's residual ferromagnetism was left over from the permanent magnetization induced by the local magnetic field during the ship's construction, and how much was magnetization induced by the motion and heading of the ship once at sea.

The Royal Navy was already developing empirical methods to cancel this residual magnetism. So, a joint NBO/NRL mission was formally sent to England where Bitter, Brown, and the other members of the team studied the work being done by the British with boats they called "electric tail sweepers". Electric cables were run on deck and around the outside of the ship to cancel the residual ferromagnetic state. A large floating cable was trailed behind the ship, connecting 2 electrodes immersed in sea water.



Sailors laying down degaussing coils on the deck of a warship (*contr. by Akronos Publishing*)

The electrode furthest from the ship was cathodic and the one closest to the ship anodic. With large homopolar generators, a powerful current was made to flow between the 2 electrodes and across the water in a wide, circling path. These currents produced powerful magnetic fields behind the ship, effectively informing the magnetic mines that the ship was at a location other than it actually was and thus causing them to detonate at a safe distance with the right maneuvering of the ship.

J - It was a combination of degaussing the sweeper at sea...

W - ...hmm-hmm, by cancelling its induced magnetism...

J - ...and creating a **ghost magnetic image** of a ship where there was no ship.

W - Yes, the degaussing coils eliminated the magnetic image of the ship and the hydro-ionic currents gave a magnetic image of a ship, and thus a false location. Bitter first saw this method in Liverpool harbor where the British had developed it. It immediately struck him that one could use much lower currents and much stronger fields if one employed coils instead of water as the medium for current conduction. But placing coils behind a ship introduced significant resistance to its speed and imposed a tremendous load on it.

To solve the problem, the NRL and the NOL experimented with coils wound around bar magnets weighing tens of tons and over 1 meter in diameter, going from the bow to the stern and protruding or projecting at the front of the ship for many tens of feet. The purpose was to project the intense magnetic field forward to the bow -- not astern -- and permit more efficient magnetic mine sweepers. Several variants were built. Bitter also started to think about employing water to cool the coils. With special enclosures, combinations of different winding gauges and the right coil insulation, turbulent cooling permitted very powerful fields.

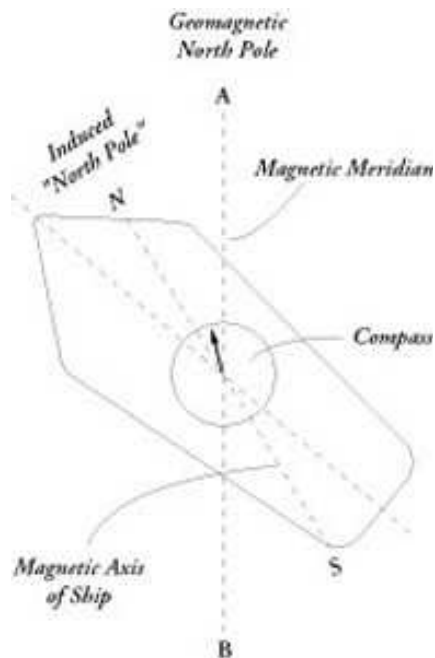
J - Were the Germans also working along similar lines?

W - Well, part of Gerlach's tenure until 1942 as Director of German naval research was focused on designing and building degaussing installations for construction yards. The Germans didn't focus on countermeasures at sea. I believe that the Anglo-American project was far more comprehensive.

Francis [**Bitter**] was after a systematic understanding. He wanted to know what caused residual ship magnetism and how magnetic signatures of ships might vary with size, building methods and motion. Permanent magnetization of ships was the result of the assembly process, particularly the riveting steps. The magnetized parts aligning themselves in the direction of the local geomagnetic field with the magnetic axis along the magnetic meridian.

To prevent this magnetization of the ship during construction, the French had developed the **deperming** technique before the war. During construction, the ship was entirely wrapped with coils. At different building stages, these coils were pulsed at "resonant" frequencies and high current. If the ship was in the water, the process was carried out with the ship being rotated through a complete circle. The frequencies and currents were largely determined by trial and error, and the process was poorly understood. A depermed ship would still have a residual magnetic field. Its strength depended on several factors. Francis wanted to understand all of them: how deperming always produced a variable, residual signature. Studies of magnetic ship signatures utilizing detector coils placed on the bottom of harbors indicated that the intensity of the stray magnetic field varied from ship to ship, even within the same class.

J - And the orientation of a ship's motion would contribute to this --?



Induced ship magnetization and its effect on a magnetic compass (*contr. by Akronos Publishing*)

W – Exactly. Induced magnetization is the result of the interaction of the ship with the geomagnetic field. That's why getting rid of the residual had to be such an intensive task. A ship moving north will have a north pole induced in its bow and a south pole induced in its stern. And if heading south, the opposite results. The extent of the induced magnetization depends not only on the strength of the local magnetic field and the heading of the ship but also the age of the ship, its composition, and its speed.

The objective of completely degaussing the ship was to eliminate its induced, residual, or stray magnetism -- to go beyond deperming. This process of cancellation could be quite sophisticated. But complete elimination was a complex, difficult, and costly affair, not to say a full-time job. Mine sweeping could deal with the magnetic mines. But a more radical solution was needed to deal with torpedos armed with magnetic detonators. The 1940 German version of these torpedos was very imperfect as U-47 found out at Scapa Flow. But by '42, the Germans had developed sophisticated magnetic fuses for torpedos. Neutralization of residual and stray magnetism became imperative.

J - So, from what you're telling me, the problem of magnetic countermeasures converged with the problems of controlling enemy guidance beams and electromagnetic camouflages.

W - All in mid '42 when the Germans appeared to be winning the war.

J - ...because of Döenitz's U boats in the Atlantic?

W – Yes. And with [V.] Bush's administrative and hierarchical victory over Bowen, let me tell you, the Navy did not look too sharp. No, Siree!

J - OK. But back to the magnetic countermeasures. There were also all sorts of problems with the tail sweeper method. The problem of poor dielectrics for insulating the high-current electricity from the water; the heating of the water, ...

W - More like vaporizing, yes. Bitter's turbulent system mostly took care of that. And the NRL Chemical & Light and Heat Divisions worked very hard on the dielectric problems as did [T.T.] Brown himself.

J - And there was also the problem of the current magnitude in producing strong magnetic fields, right?

W – Yes. A problem that the French had resolved using a solution that Kapitza developed before he left Cambridge **[England]** to return to the Soviet Union. Bitter was well aware of this and had his own ideas about it.

The problem was the magnitude of the currents required to produce those “ghost” signatures. Kapitza had found that if the high-frequency coil currents were pulsed within certain parameters, very intense currents could be created and fast enough that it didn't heat up the coils. The coils still had to be very thick. But using this method, Kapitza had succeeded in producing -- with air-core secondaries -- magnetic fields 10 times greater than was possible with iron cores.

J - Do you mean that Kapitza was employing something like a **Tesla coil**?

W - He'd made the primary an exact function because he had to control the heating. But Francis **[Bitter]** wanted to go a step further. He wanted to employ active cooling to permit still higher pulsed currents. He thought that one could take advantage of the large water reservoir (the ocean) to cool the emitting coil and direct the turbulence caused by the ship's movement to do that cooling.

J. Project RAINBOW, nuclear magnetic resonance and E. Purcell

J - Some time ago in one of our first meetings, you told me that the so-called 'Philadelphia Experiment' was some sort of a precursor of Purcell's discovery of nuclear magnetic resonance in late '44. I think you said then that Bitter thought that residual magnetism might be a nuclear phenomenon?

W - That's right. That's how this whole story really started for Bitter and for the MIT Rad Lab. It's also where Bitter falsified what he actually did for the Government. Anyway, he'd been sworn to secrecy and never received any public recognition for his contribution to the discovery of nuclear magnetic resonance. He suggested that there might be resonant magnetic energy levels -- literally unoccupied quantum levels -- that had to be reached before the residual could be completely eliminated.

He also claimed -- following **[I.]** Rabi's studies -- that there was no doubt that nuclei were magnetic dipoles. And so we had to consider the possibility that residual magnetism was a gross external magnetic effect resulting from the magnetic properties of ferromagnetic nuclei. This part of the work refers to the obliteration of the ship's magnetic image -- not to what we discussed before, the creation of a false ship image, a “ghost”.

J - Twin ghosts? Really! One made into a ghost ship. And now the other -- a ship ghost...

W - Hah-hah, yes... Bitter's idea was that one needed to **combine a permanent magnetic field with an alternating one**. Achieving this with permanent magnets was impossible. Even today, it still is. So instead, one had to drive permanent electromagnets with homopolar generators to create the permanent field, and then superpose an alternating electromagnetic field that constantly changed the orientation of the atomic dipoles. The idea was a bit more complex because the permanent field was also periodic. It was also pulsed but at a much slower rate, and changed its magnetic polarity 180° with each impulse.

J - Fundamentally, this is the method employed by **[E.]** Purcell at Harvard to discover proton magnetic resonance in December of '45.

W - ...and don't forget [F.] Bloch at Stanford. Yes, after the studies of Rabi at Columbia in the 1930s. The problem was that back in '42, Bitter lacked most of the equipment needed to produce or detect these nuclear magnetic resonances. No one knew where they were or at what energy levels. As you know, nuclear magnetic resonance is ...

J - Please explain.

W - Yes ... in a permanent magnetic field, atoms and nuclei do not so much oscillate like small permanent magnets would -- along an axis parallel to the magnetic field or the force vector -- as they rotate like precessionary gyroscopes around the direction of the constant applied magnetic field. From discussions with colleagues at the Rad Lab, Bitter got the idea that it might be possible to superpose over the constant magnetic field a resonant oscillating radio-frequency field that would selectively flip the direction of some nuclei. He was encouraged by both Purcell and Bloch in this.

J - I don't see the rationale.

W - In a permanent magnetic field, atoms and their nuclei align themselves in predictable ways. In a magnetized piece of iron, all the molecular dipoles [12] will tend to orient close to -- or align with -- an axis parallel to the magnetic lines of force. Some, as I said, align parallel and others anti-parallel. This is a little more complicated than I am making it because these alignments are precessionary.

In iron, the lowest energy state is the parallel alignment. But there are substances whose dipoles lock in parallel and anti-parallel orientations within the same magnetic domain. They are called antiferromagnetics like permanganate. If they are heated above a certain temperature, they become paramagnetic. Atomic hydrogen is like an iron dipole -- it preferentially settles in the parallel orientation.

The problem then became how to flip sufficient parallel, lower energy states into anti-parallel, higher energy states so that residual magnetism is cancelled (much as it happens in antiferromagnetic substances). From Boltzmann's thermodynamics, Purcell did not expect the distribution of the 2 orientations to be homogenous. Their quantum energy levels or excitation states would not be symmetrically distributed between the two main alignments. The presumption, as I said, was that the lower energy states predominated to generate the permanent magnetic field. Maybe residual magnetism was due to this predominance, no matter how small.

With a particular radio frequency signal at the right frequency, it might be possible to shift more atoms or nuclei from the lower to the higher excitation levels and achieve a balanced distribution. There was no equipment designed to detect these energy absorptions in water, let alone in ferromagnetic materials. But such a balanced distribution could result in the elimination of residual magnetism and be used to prevent or cancel out induced magnetism.

J - So Rainbow was a precursor of NMR, is that it?

W - You might say so. In an NMR machine, the "samples" are placed in a permanent, static magnetic field, and a transverse "radio-frequency" field is continuously applied at the Larmor frequency to cause *zeemanizing* (or the splitting of the excitation states). If the permanent field is increased, any nucleus precessing in parallel orientation becomes more resistant to being flipped into the anti-parallel orientation and so higher frequency radiation is required to flip it.

When the particular combination of an external magnetic field and the applied RF field causes atomic nuclei to flip, the nucleus is said to be in resonance -- in a state of nuclear magnetic resonance.

Part of the original Rainbow protocol was similar to this except that the permanent field was periodically switched 180 degrees. And the RF field was also pulsed.

J - OK. I think I 'm beginning to understand this at last ...

W - A constant magnetic field applied to paramagnetic substances always induces the creation of a molecular magnetic field. Because the induced magnetic field has 2 orientations for a precessing paramagnetic substance -- parallel or anti-parallel -- when one forces the parallel into anti-parallel flip at electromagnetic resonance, the induced molecular field ceases to aid the applied magnetic field and opposes it instead. As the magnetic field strength is increased, the parallel nuclei become more resistant to flip and higher energy RF must be injected at a higher resonant frequency for the flipping to occur.

When there is permanent magnetism -- even residual -- in a substance and the applied permanent magnetic field is parallel to it, the 2 fields add and the lower energy molecular dipoles are said to be shielded because more RF energy is required to flip them into the anti-parallel orientation. If the applied permanent magnetic field opposes the permanent magnetism of the target, then the lower energy dipoles are said to be **de-shielded** because less RF energy is required to flip the dipoles.

J - I see. That's why Bitter wanted to pulse the permanent magnetic field and reverse its orientation. He would periodically de-shield the parallel dipoles, making it easier to have them flip.

W - Uh-huh.

J - And if the permanent applied field was constantly opposing the residual magnetization, would one reach a balanced state like that of anti-ferromagnetism?

W - That's roughly the idea. But these are quantum processes, and one never even gets close to it. As Purcell found out in late '44, stimulated emission compensated for the absorption ...

J - But the idea was to dissipate residual magnetism by balancing the parallel states with more anti-parallel states?

W - Yes. A very tough problem indeed. No one knew where these resonances were. And by **[J.]** Ewing's theory of molecular magnetization, magnetic alignment of the molecular domains or the molecular dipoles is a step process that takes time and never reaches saturation. There were lags in demagnetization and relaxation and much heat could be expected from partial gyrations and counter-gyrations of the magnetic domains.

J - What would happen if one could, say, align all domains, make them all either parallel or anti-parallel?

W - That's the problem of superconducting magnets. Making most of the dipoles parallel is the problem of magnetization or induced magnetization of a sample. **Tesla** had already encountered this problem when he designed his electromagnet rectifiers with iron cores. Fields much greater than 3,000 gauss were needed to bring the cores to saturation. Beyond that limit, Tesla claimed that one had to employ low-frequency disruptive discharges which means **pulsing** the coil.

J - So Tesla preceded Kapitza, the French, and Bitter in this technique?

W – Yes. In the case of Tesla, you're trying to get as many domains as possible into a parallel orientation. But as for turning half of all domains into anti-parallel orientation. That is well nigh impossible -- at least with ferromagnetic or ferrimagnetic cores.

By 1938 at Bitter's Magnet Laboratory at MIT, fields of 100,000 gauss were attained. Your question is important, though, because Einstein's General Theory proposed that gravitational fields have 2 main components. One static and present in space devoid of matter. And the other dynamic and caused by the gravitational coupling of 2-or-more bodies in relative motion. 2 spinning bodies would exert a mutual force of gravitation. If the nuclear gyroscopes of a rotating body could be aligned in a preferred direction -- say, parallel or anti-parallel to the body's axis of rotation - they would generate a **force field** normal to that axis. The alignment condition is called spin polarization. If the force field varied periodically, then it might be possible to generate a secondary gravitational field.

J - Would this then superpose another curvature upon the local space-time continuum?

W - If we disregard magnetic torsion, one can only think of it as either intensifying the existing curvature or relaxing it. This is equivalent to saying that it would either increase the density of the gravitational field flux lines or decrease it.

J - How could we disregard magnetic torsion? Isn't there a relation, for example, between the Earth's rotation and its axis and the orientation of the magnetic field and its axis? Couldn't the apparent magnetic axis offset be the mean of the precessionary motion? After all, you jumped from magnetism to gravity. But you were assuming, implicitly, that the 2 fields were coincident or nearly so, no??

W - I understand ...

J - ... even the idea of varying the line density goes back to your discussion of Faraday's notion of magnetic lenses.

W - Well, yes ... hmm, hold those magnetic field notions in your mind and suppose that it is possible to give a single description of them which could be made identical to a description that would apply to any gravitational field. Now, in the General Theory where there is only one metric tensor g to express local geometry, 'g' is locally determined by the gravitational field by its local flux line density. The metric tensor simply expresses the acceleration of the frame of reference.

Therefore, the flux line density of a spin-polarized, rotating body must decrease with respect to the flux line density of surrounding space if the gravitational attraction of that body towards any other revolving one is to decrease. The space-time occupied by the spin-polarized rotating body would have to have less of a curvature than the surrounding space-time.

J - But I don't understand. That would only allow one to decrease the acceleration of the local gravitational field. How would weightlessness or anti-gravity be possible?

W - It wouldn't. Not from any of Einstein's theories 'Unified' or 'General', by the way. But some interpretations permit one to think in terms of a shield of the space-time occupied by the spin-polarized rotating body. The weight of 2 bodies with respect to each other would only exist if the secondary gravitational field within that shield had a line density greater than that of the space surrounding either body.

In contrast, weightlessness would be a condition of **degravitation** where the density of gravitational flux lines within the shield of either of the spin-polarized rotating bodies would be equal to that of

surrounding space. And anti-gravity would be a negative weight characteristic reached when the line density within a shield was less than that of surrounding space. If this condition occurred, then spin polarized nuclei would align themselves anti-parallel to the weight vectors.

J - Something like a curvature that can be straightened or even inverted to form a geometric negative?

W - That's the idea. Yet this interpretation [13] cannot be entirely correct. Geometry results from gravitational fields, from states of acceleration. It is, of course, hardly possible to understand how space devoid of matter is subject to a state of acceleration when it is equally supposed to be devoid of energy when time itself is taken to be one-dimensional and simultaneity is seen as a relative state.

Somehow, one has to imagine space as being subject to an acceleration without being able to treat that same space as a physical property of energy.

J - If I understand you correctly, Reich wanted Einstein to do just the reverse -- to treat that space empty of matter as a property of mass-free energy?

W - Yes. One might think of energy in flux as subject to an acceleration, even if treating the energy as mass-less presents conceptual difficulties. But to say that space is in motion -- or is subject to acceleration -- is physically meaningless. It is at least as mysterious as Faraday's lines-of-force and Maxwell's superposed, counter-rotating vortices.

But suppose nuclear spin polarization could permit us to alter or even invert the curvature of space-time. Then, the main technical difficulty would be to come up with a process that flips most of those gyroscopes from parallel to anti-parallel orientations with respect to the mutual weight vectors.

That's where magnetism -- or some form of it -- comes back into focus. After all, outside of a critical distance, 2 mutually gravitating bodies do not fall towards each other. Some force already keeps them apart. And unless one takes recourse to the gravitational shield interpretation, one is only left with the space-time torsion.

J - And neither is satisfactory? What about nuclear magnetic resonance? Could it not be used to cause that torsion at very high field intensities?

W - Well, the solution can only come from asking whether a body which had a majority of its magnetic dipoles oriented in anti-parallel magnetic direction could also function as a body which was nuclei spin polarized in anti-parallel orientation with respect to its main weight vector. Without introducing the question of the magnetic properties of space or of some form of mass-free energy, it's difficult to see how the gravitational relationship could be modified or inverted. You may regard the shield as a finite space-time region. One can treat the curvature as a matter of flux density. But there is no physical mechanism to create relative differences in this density that would generate opposing curvatures, "convex" curvatures.

J - So Rainbow was going to test for the complete removal of stray magnetism with methods analogous to NMR. But it was also going to test for General Relativity and the Unified Field to see if a high-intensity rotating magnetic field generated a secondary gravitational field? And if either its curvature or torsion -- or both -- would change?

W - Yes. Engaging magnetic resonance with very intense fields might change the local curvature of space-time that "contained" the ship. That was the idea. This could bend all light rays further than they are already bent on the surface of the Earth and ...

J - ... give the wrong optical or electromagnetic image of the ship's location.

W – Yes. The electromagnetic image of the ship would be red-shifted and the ship would appear to be further away than it was. The radar bounce would take longer to return. The bounce would be stretched or bent longer and would not return therefore on the expected frequency for which the receiver was tuned.

You see, this is the reason why Rainbow was such an important project and its outcome would bear on many different fields. It would address a number of very different questions -- all of them of crucial importance.

K. The convergence of optical, magnetic, and radar countermeasures at the NRL

J - Let's see if I can catalogue the main series of problems that converge in Project Rainbow. I'll count them and you can correct me. (1) to make a ship magnetically invisible to magnetic fuses on mines or torpedoes by canceling its residual magnetism with some form of nuclear magnetic resonance developed from degaussing. (2) to create a false magnetic image of a ship -- a “double” -- that would permit harmless detonation of these mines and fuses. (3) to bend the radio-controlled beams of gliding bombs and flying bombs, I presume with strong magnetic and electric fields?

W - Uh-huh .

J – (4) to replace guidance beam echoes -- and radar beam bounce -- with false ones that would give off a false electromagnetic image of the target. (5) to make a ship -- or airplane, you said -- optically invisible on gun sights etc. by using very strong electromagnetic fields to bend the space-time curvature.

W – Yes. But you see, if intense fields **altered local gravity**, you also got a bonus. They would also alter magnetic and electromagnetic signal detection. The attraction of the UFT was that one could have it all -- **all the countermeasures in one** -- and that was the optimistic rationale of Rainbow.

J - At which point did the various “roots” meld? Where did the impetus come from for such a comprehensive undertaking?

W - I recall that it was on word from Einstein that Neumann went to Navy Commander [L.] Strauss -- personal assistant to the Navy Under-Secretary [J. Forrestal] -- and told him about all the military possibilities of an experiment combining degaussing with induced nuclear magnetic resonance of a target ship. Understanding gravity, magnetism, optics, and radar -- and proving the General Theory -- were bonuses of a possible new military arsenal of electromagnetic countermeasures. One might be able to eradicate the residual magnetization of a ship and make the ship appear to be where it wasn't. One would fool magnetic mines and torpedoes, and might even fool optical sighting in gun mechanisms or do one better and foil radar. One might be able to make the ship invisible with sufficient curvature near the line of horizon. And so on.

J - The eyes of the brass must have rolled out of their sockets!

W – Yes. The idea of bending light or radar beams really struck their imagination.

J - But if it weren't for von Neumann himself and his Einstein reference, they would never have bothered!

W - You're forgetting Taylor and Gunn. Plus just how bad it was in '42-'43. One thing was losing Bowen to Bush. But when it came to doing the "right thing" when the War appeared to be lost, you would be hard put to find a more determined bunch of Navy civilians!

J - So, all the pressures combined made the Navy more prone to accepting off-the-wall propositions?

W - Yes, if backed up by established authorities. Von Neumann was at the time employed by the NBO. And he had tremendous pull and charisma. He was a real organizer and figured that Van **[V. Bush]** owed him a couple. The NRL had let radar and the nuclear fission projects go to the OSDR, so the OSDR now had to let the NRL take this one over. With Einstein and von Neumann on their side, it was the turn of Harvard and MIT -- along with Rad Lab -- to make a few contributions in return.

J - Hence Purcell and Bitter?

W - Yes. Gunn was already a member of Bush's OSDR and its superstructure -- the National Defense and Research Committee [NDRC], where he battled for the independence of the NRL.

J - Was Professor Allen part of it?

W - Yes. **[C.M.]** Allen of the Worcester Polytechnic and **[Dr. R.H.]** Kent...

J - In The Philadelphia Experiment book, William Moore alludes to most of these people.

W - Yes. He and co-author **[C.]** Berlitz ... they don't mention Purcell's involvement, though, nor ...

J - ... and they come up with 2 main characters -- a "W.W. Albrecht" and a "Dr. Franklin Reno". No one has ever been able to determine who these characters were or if they ever existed. Was Moore telling the truth about their existence -- even to Berlitz or to **[S.]** Friedman?

W - Well, Moore bent the truth a little.

J - Everybody does.

W - Uh-huh. But what I mean is that all the Relativity computations and so on -- particularly for the models of the Unified Field -- were mostly done by Gunn and Hulburt. Gunn published seminal papers on astrophysics, on the anomalous rotation of the Sun, on the rotating Earth as source of terrestrial electricity and magnetism -- the Earth as dynamo ...

J - But Gunn was the technical boss?

W - Yes, the effective director during Keuren's tenure.

J - So 'Albrecht' is Ross Gunn?

W - No, no, no! 'Albrecht' is 'Doc Taylor' -- the Superintendent of the Radio and Radar Divisions!

J - Huh?

W – Yes. Albert Hoyt-Taylor -- the wireless warrior of WWI in charge of Marconi's Delmar Station or Camp Evans when the Navy took it over during WWI -- the man who initiated the quest for radar with his 1922 experiments.

J - He ... he's the "W.W. Albrecht"? Why the 'WW' and the German name?

W - Haven't you figured it out yet? They're *inside jokes*! Look, some historians called WWI the “wireless war”. At Camp Evans -- the Marconi station -- they called themselves the “wireless warriors” and sometimes called Taylor the “Wireless Washington”.

J - Like the General?

W - Yes, and on the same stomping grounds. Other times, to distinguish him from Einstein, Navy people would also call him “our Albert” or -- because of his strictness -- 'Albrecht'.

J - ... which Einstein was.

W – Yes. So Taylor became, quite naturally, W.W. Albrecht or W.W.-you-know- who. When he'd meet up with Einstein, there'd be unending jokes about the two Alberts.

J - Well then, that would make “**Dr. Franklin Reno**” **Lou Gebhard**, then!?

W - Right you are!

J - So [**G.E.**] Simpson and [**N.R.**] Burger in their novel Thin Air were barking up the right tree when they first named him “Rinehart”?

W – Yes. That was a superb clue from a very poor and sensationalized account. You'll find very little, if anything, about Taylor's assistant and co-worker, and one of the inventors of radar. But “Rinehart” was an excellent choice because, you see, his real name is not Gebhard but Gebhardt. Louis Alfred Gebhardt with the ‘t’ at the end like “Rinehart” or like the real German name for Rinehart which is 'Rheinhardt', which has the same ending 'dt' as 'Gebhardt'. And he didn't die in 1977 or '78 but 10 years earlier in '68.

J - I see...hmm ... But why did Moore and Berlitz never reveal this -- his true name and that of “Albrecht”? It would have lent substantial credence to their book, no?

W - I believe they went as far as they thought it was safe to go.

J - Do you mean someone got to them?

W - I know nothing about that. I mean they didn't think -- back in '78 -- that they should reveal more than they did about the supersecret doings of the NRL.

J - The uncertainty certainly tainted their reputations!

W - But it must also have been secretly amusing to see newsmakers and aficionados running around making wild claims about Reno, Rinehart, and Albrecht.

J - [A.] Bielek swore that Reno-Rinehart was von Neumann.

W - That's all part of the silly lore that has surrounded this project. The more buried the thing is and the more categorical are the denials by the Navy, the more fantastic are the tales by the radical ufological and fringe communities. Remember, it was the ONR [**Office of Naval Research**] that got this whole ball rolling in the first place.

L. Why the Carlos Allende tale?

J - Was it not the 2 letters sent by Carl Allen -- or Carlos Allende -- to [**M.**] Jessup that got this thing started?

W - Yes. They were the leak that concentrated all the legends. But who knows what prompted them? They perplexed Jessup, that's for sure. But he was nearly ready to drop the matter when Commander [**G.W.**] Hooper and Capt. [**S.**] Sherby called him over [**in 1957**] to the ONR in Washington. Had they not printed the Varo edition with Carl's *extraterrestrial* voices annotated in color...

J - So what are you telling me? That there are only 2 possibilities? That either Carl Allen was disturbed and he sent the letters to Jessup and then the annotated copy of Jessup's book to the ONR. Or that he was some kind of a disinformation agent.

W - I imagine it's far more complicated than that. Allen was definitely disturbed -- by megalomaniac fears. This fact alone predisposes any sane person to immediately view the entire story as a hoax that he perpetrated. That would be the case if his neurosis or psychosis simply latched on to rumors that he'd run across -- you know, sailors' stories.

You see, following the failure of Rainbow and then 9 months later the explosion of [**P.**] Abelson's uranium purification plant in the Philadelphia Yard [**14**], the veils of secrecy over the NRL were thicker than ever before. Tales of great dangers, fantastic events, and dark powers were frequently employed to dissuade the incautious or attract potential spies. They were planted by plainclothesmen in bars and meeting halls along every important waterfront. If you know the marine milieu -- merchant and naval -- you know how fast tales travel and how they're spun.

J - Hmm, I can imagine.

W - But Allen could have been disturbed because of events connected to Rainbow either because he'd lived through some of them -- and this caused his disturbance -- or because he was able to somehow find out about these occurrences. In the latter case, we still have several possibilities. He could have found out about these events because of proximity, or through a third party, or he was given access to knowledge of those events while being "handled".

J - O.K., I see there's a lot of possibilities. But what's your hunch?

W - I don't need one. 'Franklin Reno' was real enough. And he clearly was in touch with Carl Allen long after Rainbow was buried. So, I have little doubt that Allen was being "handled".

J - By Gebhardt?

W - Precisely. The question is why. He might have been handled because he was, in fact, some kind of a human guinea pig in one of Rainbow's experiments and so he was a risk and had to be shut up and made to appear delusional. Or he might have been handled because of his potential to bring discredit to the emerging field of ufology and counteract the near-panic being caused by the sighting flaps. Or to

sow confusion and disinformation around a military black ops project that did involve Einstein. Or as part of some rogue action. It's no coincidence that the Allende Letters are contemporary with an intense UFO flap and the emerging policy of denial.

J - Yes, Allen's first letter to Jessup dates from January 1956. The policy of denial comes in full force after the debunking carried out by the CIA [**H.P.**] Robertson-[**D.**]Menzies panel [**in 1953**] and just as [**T.T.**] Brown and [**USMC Maj. D.**] Keyhoe were preparing to create the Flying Saucer Discussion Group which later -- in October of that year -- led to the National Investigative Committee on Aerial Phenomena (NICAP) supported by Rear Admiral [**D.S.**] Fahrney and Admiral [**R.H.**] Hillenkoetter, who 10 years earlier had been the first Director of the CIA. I don't think that the USAF was happy with what all these Navy people had done in creating NICAP.

W - Yes. Don't forget that other equally important events were afoot. Einstein died in early 1955 [**15**] and -- though he left no Unified Field solution -- he was hailed in the media as having had one since 1950! This notion of Einstein's success impacted the problem of the UFOs when it was at its most intense, on the brink of turning into a mass-panic nearly like that of [O.] Welles' ill-fated "War of the Worlds" radio experiment [**in 1938**].

[**USAF Capt. E.**] Ruppelt -- who was in charge of the USAF Project Blue Book until 1953 -- had previously placed the Air Force on a collision course with the CIA's policy of denial. In fact, those years of '53-to-'56 are crucial ones. Remember that it's in '54 that Eisenhower creates a top-secret group to oversee the "**invisible government**" (as some have called it). It was known as the "54/12 Special Group" and was supposed to centralize all intelligence -- civil and military -- and all major propaganda and disinformation efforts.

J - Is that the real Majestic-12?

W - That's a different story for another occasion. But don't forget the Special Report debunking UFOs produced by Battelle in October of '55, which was severely criticized by Ruppelt and Keyhoe.

In fact, the year of '56 saw a crescendo of important books on the subject of UFOs. One was The Truth About Flying Saucers by the French mathematician Aimé Michel which drew attention to the 'cosmic-ray' gravitational force-field hypothesis put forth a year earlier by French Air Force Lt. [**J.**] Plantier [**16**]. The other was Keyhoe's The Flying Saucer Conspiracy which concluded that UFOs flew in accordance with [**H.**] Oberth's hypothesis of a 'g-field' and then had a chapter entitled '*Redell explains a Riddle*' in which the mysterious Redell tells Keyhoe that both Oberth and Plantier are only proving that Einstein's UFT is correct! The third was Ruppelt's honest account -- The Report on Unidentified Flying Objects -- which, though critical of Keyhoe, also concluded that UFO propulsion could only be explained by Einstein's UFT [**17**].

J - By all accounts, Einstein's UFT was seen as a stunning success.

W - At least amongst those seriously concerned with investigating UFO phenomena. So, this matter was very much in the air at the height of McCarthyism and the Cold War. Which is to say also at the height of American paranoia and disinformation. A firm **policy of denial** was now in place. All governmental and military research into unknown aerial or maritime phenomena went deeply underground. And so did any research into esoteric technology that might be linked to it -- even if just by mere similarity.

J - Like what?

W - There were too many to count. All the advanced research into flying platforms, coleopters, flying wings, stealth bombers and fighters, turbine suction aircraft, perforated skin suction, skin polarization, not to mention rocketry, satellites, and so on. It was a technological avalanche of experimentation. Many different things were tried and many things went wrong. Rockets, by then, were beginning to be mastered. But circular suction aircraft were every bit as bad in their crash rates. The saving grace was that they rarely exploded.

The point is this -- and it was made by **[R.]** Vesco in his Intercept UFO -- circular-turbine craft and perforated-skin craft were the focus of a great effort of development by the U.S. and by the Russians, the British, and the French right after the end of the War and until the early '90s. There were more crashes with these devices than with just about anything else. And all these projects were highly classified.

J - You forgot the Canadians.

W - Those were really joint Anglo-American projects including Wilbert Smith's 'Project Magnet' in Ottawa (supposedly under the Ministry of Transportation) and the AVRO car.

J - And out of all these efforts, nothing ever materialized?

W - No, no ... many things came from these highly classified projects. Including the caper of "crashed UFOs" to cover up the frequent crashes of experimental craft outside of their test ranges. But none of them involved anything remotely like a technology that permitted gravity control. None of them tested Einstein's Unified Field theories. None of them were follow-ups of Project Rainbow or further elaborations of it.

J - So, what could Allen's objective have been in writing his letters to Jessup (or the objective of his handler or handlers)?

W - Allen's apparent fear was that Jessup's book The Case for the UFO would stimulate the U.S. government to reopen the book on investigating Unified Field technologies. That's delusional since the U.S. never stopped investigating the possibility that such a field exist (even if it has never succeeded in finding one). Nor would the U.S. government bother to listen to Jessup.

It appears, at first, that Jessup's book must have been the problem. It wasn't a particularly good book, well written or researched. In fact, it was lousy. But it was written by an astronomer and read like a catalogue of horrors through the ages. Jessup appears as someone who is at least as deluded as Allen with his race of higher men flying metal machines some 300,000 years back; or space intelligences that hide in "big clouds"; or falling "live things" that are "the inhabitants of celestial hydroponic tanks; or a master culture of Atlanteansl and so on. He was an ideal target for disinformation.

J - You mean someone gullible enough to be made to swallow a fabulous tale?

W - In the Special Section, we had "spotters" that picked people like this. In particular, "talkers" like Allen and "targets" like Jessup. Jessup believed that some space-beings were material and others massless or ethereal. So as you can imagine, this left many degrees-of-freedom to play him with ... - ha ha!

J - I can only imagine. But if anything, this should have suggested the ONR stay away from Jessup.

W - Yes, except maybe for all the baits. And that's my point. And these were simply too many:

- They had been sent a book written by someone who could be handled and annotated by someone who was handled.
- The annotations explicitly refer to 1943-1944 experiments of the Navy with electromagnetic force-shields and the horrors that resulted from them.
- They alluded to Einstein's UFT efforts, stated that they succeeded but that the sudden and uncontrollable invisibility of those subjected to such fields had been so disturbing that Einstein had been forced to retract his solution.
- And the esoteric references in Jessup's interpretation of the history of religions are matched by even more exotic notes made by Allen that purport to explain ship and plane disappearances happening up to 1955.

J - They had to look into the matter, especially if it was a hoax designed to smoke them out?

W - At the NRL and ONR ... yes, to make them tell the truth about Rainbow or to deny it and suddenly be put on the spot. But ...

J - Hold on ... So Jessup's book was really immaterial?

W - Yes, fundamentally. Allen could have picked up any of a variety of recent books about UFOs and made similar annotations. Perhaps not one that was so far off the wall, but still...

Nor was Jessup the first to claim ship and plane disappearances, time freezes, or human abductions. Jessup also argued that these space travel machines are not rockets and are neither propelled by magnetic fields nor atomically powered. He talks of controlling “gravitational field reactance” or propulsion by controlling gravitational fields and is convinced that the Russians must have some form of exotic technology that they've been hiding. He calls repeatedly for the Government to carry out research on gravitation, to create saucer patrols, and so on.

But, as I told you, so had Plantier, Michel, Keyhoe, and Ruppelt before him. And in much more cogent fashions. So, ONR's interest could only be due to Allen's annotations because he alone had claimed that Einstein's UFT had been tested. And that this had been done by the Navy in '43 to '44. That was the new item. And that's the first giveaway.

J - This must have immediately prompted the question: how did Allen know about these experiments, no?

W - Certainly. But you see, Allen -- who seems to identify with “Mr. A”, one of the annotated voices -- makes a reference to the magnetic levitation of paramagnetic substances [\[18\]](#) -- like aluminum -- and this was Gebhardt's work.

J - 'Franklin Reno' left a mark.

W - In more ways than one. Allen explicitly suggests that Jessup is wrong. That Jessup doesn't know what he's talking about and refuses to admit that electromagnetic fields simply can't be employed to do what he wants them to do -- i.e., to alter gravitational fields.

J - So it was Allen's contentions that tweaked the interest of the ONR?

W - In part. And Allen and Gebhardt had to know that this would be the case. Sending Jessup's annotated book to the ONR would trigger something. The ONR's concern was that details about the real

Rainbow might be released -- potentially causing both discredit for Einstein and public embarrassment for the NRL.

But there is another twist to this. That's the second giveaway, and it's been well tucked away all these years. You see, back in '56, Project Rainbow was still going on. Ha ha ha!!

J - I don't get you. What are you talking about?

W - I'm laughing because I think that this is the main reason why intelligence people got alarmed by the Allende tale. Rainbow had in the meantime become a joint Navy, CIA, and Air Force effort at radar camouflage. Remember the CIA/USAF flights of the Lockheed U-2s over the Soviet Union -- SINGINT and ELINT missions they were called -- beginning August 1955?

J - Yes ...

W - That was part of 'Operation Soft Touch'. There was a problem with it, however. It could not be implemented unless U-2s could be made invisible to Soviet radar. It seemed to be working in the beginning but not too well because, as you may recall, Powers got shot down in November of 1956!

So, in great alarm, more resources were thrown into Rainbow when the 54/12 group met in early 1957 [19] with President Eisenhower, Chief of Staff USAF [**Major**] General [**N.**] Twining, CIA Director [**A.F.**] Dulles and his 2 sidekicks -- his Deputy Director of Operations [USAF] Brigadier General [**C.P.**] Cabell who had earlier created the national UFO-tracking radar network, and his Deputy Director of Plans [**R.**] Bissell.

Bissell was also placed directly in charge of managing the U-2 program. Anything that would have had to do with radar invisibility or Rainbow would have highest priority and greatest sensitivity. It's been said that when Bissell despaired of getting more sophisticated electromagnetic camouflage for his U-2s, he came up with the notion that the right canvas -- he called it "the right wallpaper" -- would do the trick.

So the Allende tale was sensitive also because of its timing with reference to the ongoing Rainbow efforts to make the U-2 plane invisible. Officers at the ONR would be quite concerned with the possibility that the book (with its annotations) contained some sort of cipher that could pass highly classified information under the wire. They wouldn't want to be blamed for anything that could hinder what had by then become Bissell's pet project.

J - So you don't believe Allen was an ONR disinformation agent or that the ONR was targeting Jessup?

W - No, not at all. They certainly must have weighed the pros-and-cons of calling Jessup in. But that's understandable, because they needed information on who had written the comments, and they had no other obvious lead but Jessup.

J - So the Allende letters cannot be an intelligence caper?

W - I didn't say that. There is obviously intelligence behind them. Ad they are a caper of some sort. Just what sort of caper is the problem, wouldn't you say?

J - I'm not seeing it ...

W - If it was part of an organized caper, then it's pretty obvious that the caper has its analogies with the UFO capers themselves. In both cases, a subject deserving of scientific attention is released to the public under circumstances that stretch its credibility beyond any reasonable limit and thereby bring it squarely into disrepute.

The public is confronted with hush-hush information that appears to be simultaneously very deep and totally imbecilic. And the public -- including scientists -- has no means to ascertain which is which, which regularly divides them into willing believers and militant skeptics. So everything is distorted by this dualistic lens.

A test vehicle unexpectedly crashes -- and no small numbers of U-2s crashed at landing! -- that's bad. But if on demand it can be camouflaged as an ET saucer crash, then -- with a public denial policy in place -- no serious questions will ever be asked about what was really going on. On the other hand, if people come to believe that what crashed was an ET machine, they can rest assured that the U.S. has the same technology and will protect them from any unforeseen ET horrors.

J - What if a real ET saucer or an enemy craft eventually crash lands?

W - No problem. One can always say it was a test-vehicle. It is a superb circular caper whereby **truth was afforded real invisibility**. Likewise with Rainbow. It would come out one day or another. And what better way than through someone who was handled and as disturbed as Allen? Except for those few in the know, no one would take him seriously. And those that would, would be stigmatized by his madness.

And that itself would rub off on the opinions, all now in agreement with each other, expressed by Plantier, Michel, Keyhoe, and Ruppelt about the UFO and Einstein's UFT. At worst, it went to Einstein's credit. At best, to their discredit.

J - Yes, but there's one snag with what you're saying. The horrors that went with it in the Allen(de) annotations and letters. Michel, Keyhoe, and Ruppelt -- even though they admitted that some strange events might be going on -- didn't have the same opinion as Allen(de) or Jessup had about at least some of "their" ETs or the horrors that Allen(de) said were caused by Einstein's UFT in the context of the 'Philadelphia Experiment'.

W - That's the last possibility. That the caper wasn't organized by the ONR or by some supersecret intelligence group that the ONR itself was not aware of, but that it was a rogue caper -- a "renegade come-on" from inside the NRL or from among its ex-members. Some sort of dangerous provocation, given the times and the context.

If it was an official caper that handled Allen, then clearly its objective can't be understood outside of the more general policy of denial - a policy that ran something like:

"It's best to deny the validity of sightings and ridicule the reports -- which permits suppression of all those unexpected events involving classified projects. If a minority of UFO sightings may be genuine unknowns, the majority certainly are our own experimental military craft. In either case, we don't want to panic the public, nor find ourselves in the awkward position of having to reveal advanced projects that have turned out badly. But while denying the validity of the sightings, it doesn't hurt to have a little mystique, an aura of astonishing achievement leak out. Because if we're ever really confronted by an aerial enemy -- terrestrial or otherwise -- we can always turn around and say that we've had it all along which will keep people from panicking".

So, Allen could have been handled by the ONR to release disinformation that would ridicule the UFO field. And at the same time glorify the Navy and Einstein who had supposedly discovered what space intelligence has known for millions of years. But just as likely, Gebhardt and others -- working as a rogue group -- could have created a caper to draw the Navy and the NRL out into the open about work that they had done during the War, work that was still going on and for which they were never recognized, and which would shed -- in their minds -- some light on the problems of a Unified Field.

J - Which is the right answer?

W - Look, it is obvious that in the following decades -- until the early '90s -- capers like these continued to be staged fairly frequently. So-called radicals in ufology are the greatest consumers of this stuff. And there are entire groups of imitators that follow. With the New Age movement, there is no end to belief -- conspiratorial, gargantuan, paranormal ... a permanent "Paranoia Inc".

J - These events in the '50s sound like a road map for what was to come.

W - Yes. The Lazar caper and countless others, crashed saucers, abductions, crossbreeding, come to mind. These capers took on lives of their own, that's for sure. Now think for a moment ... There's no doubt that Gebhardt handled Allen. But someone handled Moore, too!

J - I follow, yes. And unless Moore lied about the year Gebhardt died, he couldn't have been handled by Gebhardt himself but by **someone impersonating him**. Is that it?

W - There you are! So, the caper had continuity beyond Gebhardt. And that's pretty unusual for a rogue caper, wouldn't you say? Someone who knew all about Gebhardt and Rainbow continued what had been started by Gebhardt and Allen, well past Gebhardt's death.

J - Unless Gebhardt went dark for the last 10 years of his life and didn't die in '68 but much later - some 10 years later to be exact.

W - Quite. What caught my attention back then -- when I first heard about the ONR/Varo incident -- was that so many of Allen's facts were right about Einstein's involvement, the ships that were used, the dates, etc. It made me think that this was exactly like the camouflage strategies drummed up by the Special Developments Section.

J - That's how you knew Gebhardt was feeding this material to Carl(os) Allen(de)?

W - Yes. And if Allen was aboard one of the ships [\[20\]](#), he kept that contact with Gebhardt for at least 13 years.

M. The Project Rainbow experiments

J - Alright, this has cleared up quite a bit for me. Can we go back to Rainbow itself? By mid-'42, Rainbow seems to be well under way at the NRL. And by early '43 with Einstein formally engaged, it becomes a test of his UFT. Taylor was in charge of the project, and Gebhardt ...

W - Yes, Gebhardt supervised several sections for Taylor. And that's where Lorenzen and **[Lt. Cmdr. Dr. Lloyd]** Berkner came in. Berkner was the radio engineer aboard the first 1928 expedition of Admiral **[R.E.]** Byrd to Antarctica. Taylor and Hulburt had designed a special high-frequency radio

system that was built by the NRL Radio Division for Byrd's 1,500 kilometer flight over the South Pole. Berkner was the chief operator.

From '33 to '41, Berkner worked at the Carnegie Institute on both terrestrial magnetism and ionospheric studies, following up on the high-frequency studies of [M.A.] Tuve with the crystal-stabilized transmitters invented by [L.C.] Young and Gebhardt at the Carnegie Institute and the studies of Taylor and Hulburt.

J - Sounds like a Carnegie-club operation.

W – Yes. [V.] Bush's home-ground. Berkner had joined the Navy Reserve in '26 and was called to active duty in '41. When his rank was revealed after the War, surprisingly he'd become a Rear Admiral. As of 1940, Berkner became a consultant to Bush's NDRC -- the National Defense and Research Committee where Gunn had a seat.

J - What was Berkner's role in Rainbow?

W - He was the chief engineer in charge of overseeing the technical part of the project for the NDRC, directly in charge of the high-frequency component and radar instrumentation. At the NRL, he interfaced with Gunn, Taylor, and with Hulburt in particular. Between '43 and '45, Berkner was the Director of the Electronics Materials Branch of the Navy's Bureau of Aeronautics [NBA].

J - So RAI Rainbow NBOW was not exclusively an NRL project! It really was a joint NDRC/NBO/NRL and NBA project.

W – Right. That's how it started. Hulburt's Division of Light and Heat was renamed -- I think in '41 -- the Physical Optics Division. Hulburt was a man of great character who joined the NRL in 1924. He was the first to propose a mathematical treatment for the propagation of low and high frequencies that took into account the role of the ionosphere. This was in a paper co-authored by the "Wireless Washington". His later work on rocketry at the NRL led to the discovery of the role of the ozone layer in absorbing ultraviolet below 3,000 angstroms. For these contributions, he was awarded the John Adam Fleming Medal from the American Geophysical Society in '64.

Berkner got one in '62. Aside from his many other specialties, Hulburt was also the supreme expert in optical camouflage and mirage. He was the author of the Navy's handbook of ship camouflage patterns and colors. After the 1935 reorganization of the NRL, the Bureau of Aeronautics asked him to look into the possibility of optical camouflages that might make a plane seem closer than it was or make it invisible until it reached a very short range. The idea of the brass was to employ varied illuminations for these purposes.

But Hulburt thought this was totally impractical. And he was proved right by experiments carried out at the Anacostia Naval Air Station. He immediately began tinkering with the possibility of bending light. If light could be artificially red-shifted, a plane would appear to be further away than it was. Or it could even be made invisible. And that's how he, Gunn, and Gebhardt began thinking about the implications of a Unified Field for the General Theory.

J - So this problem must have joined the other countermeasures problems that Lorenzen's Section was working on?

W – Yes. Hulburt was the chief scientist in charge of the Rainbow experiments, data gathering and analysis. He operated largely through Lorenzen's hands-on approach. Ship procurement and project

logistics were left to Commander [William S.] Parsons. [T.T.] Brown -- now made a Lieutenant Commander -- was placed in charge of the electric and magnetic equipment used to generate the fields. Duncan, Bennett, and Bitter from the NBO/NOL were in charge of degaussing and the electromagnetic transmitters.

The long- term target of the experiment was to produce **magnetic, optical, and radar invisibility**. That's how it was sold in the end. Einstein and Infeld did the preliminary study regarding expected local distortions of space curvature. But afterward, the calculations for the gravitational and nuclear-magnetic effects were made by Einstein, Taylor, and Hulburt; correlated by Gebhardt, Bitter, and Berkner, and checked by von Neumann and Veblen. The required strength of the total field -- if it was to bend light and produce an electromagnetic mirage -- was anticipated by some of the models to be incredibly high.

J - I'm not clear on this. On one hand, the idea of completely degaussing at sea was to use methods similar to NMR to achieve a more balanced distribution of atomic dipoles. But this wouldn't interfere with light. How could it, since you would be countering any expression of a magnetic field? Then you raised the question of spin polarization of most nuclei either in parallel or anti-parallel direction with respect to both the gravitational and geomagnetic fields. But this polarization requires an effect opposite to balancing the dipole orientations. It calls, instead, for a preferred orientation?

W – Yes. These were 2 different experiments. (A) How to create what you could call an anti-ferromagnetic state and erase all residual magnetism. (B) How to bend light by inducing a greater curvature of space-time with high power magnetic or “anti-magnetic” spin polarizations.

J - You mean with parallel or ...

W - ... preferably, anti-parallel spin orientations with respect to the applied magnetic field. Correct. If I remember, Hulburt had questioned the notion that a steady optical light displacement would be possible if one succeeded in inducing a substantial anti-parallel spin polarization. Expected resonance states would suggest sudden shifts. The result could also be a fuzzy pattern of light -- a **colorless fog** of electromagnetic waves caused by random destructive and constructive interference.

J - Why so?

W - Keep in mind that nuclear magnetic resonance was known to exist. But **resonance levels** were unknown. Many models had to be considered. And the values were widely different for such guiding parameters as magnetic field frequency, Larmor frequency, field strength and so on, not to mention optical shifts. Hulburt, Gunn, and Abelson were all of the opinion that the field would interact with the protons in the surrounding air and water and produce all manner of possible mischief.

J - Yes, no one knew what the NMR thresholds would be for air or water.

W - Nor were the calculations accurate for the amount of heating that the ship would have to endure. Or the amount of ozone and hydrogen gas that would be released from air and water. Worse still, for heterogeneous materials where magnetic domains have different sizes, it's more difficult to orient large magnetic domains than smaller ones in a non-uniform field. And an alternating magnetic field will involve all manner of hysteresis lags in magnetization and relaxation, as I've said. If the timing of the superposed fields was not appropriate, the result could be quite disorderly. Like dissociation of molecular structures and magnetic domains.

J - How were they going to try to generate a secondary gravitational field -- one seated on the target ship, that is? Would it be done by increasing the strength of the permanent magnetic field when the

ship's dipoles were deshielded and then varying the RF field in frequency and intensity? Surely the ship was not going to be set spinning?

W - The target of the experiment was not anti-gravity or even weightlessness. Demagnetization of residuals was the objective of the first experimental runs. Redshift of electromagnetic radiation was the next step. But the equipment required for the second step was going to be tested from the start. All the equipment was to be installed under the cover of degaussing the ship at the end of its construction period. When the ship was launched, it was pretty much ready to go.

J - So, the work really began in '42.

W - Yes. The construction of the gigantic permanent electromagnets; the homopolar generators; the coils to be wrapped around the magnets and the ship; the transformers; the motorized current interruptors; radar transmitters and receivers in various radio and microwave bands; the optical detectors; the magnetic resonance detectors; the gaussmeters ... It was a major ordeal.

Most of the effort took place at the NRL and at the Philadelphia Naval Yard. By June '43, the DE-173 *USS Eldridge* was fully fitted with the gargantuan coils, transformers, generators and magnets and the *USS Furuseth* with all the monitoring and test equipment. Several short experiments were conducted throughout the early Summer of '43 in the hope of hitting the most likely resonances. There were some observations of possible resonant spikes, but they turned out to be nothing more than instrumentation glitches.

The final experiment of this first phase -- on-or-about August 12, 1943 -- appears to have been performed at some resonant condition. But the result was catastrophic. A cloud of **green fog** enveloped the ship and it appeared opaque to radiation. Water and air boiled off all around its periphery, surfaces became charged -- it was a hell of a freakish thing and plenty of equipment broke down and ignited. A terrific electrostatic field developed alright. But it bathed the entire ship. Outside the foggy envelope, the ship became surrounded by an envelope of shimmering light due to 'zeemanizing' and constructive interference. The men had been issued rubber shoes, suits, and gloves as well as gas masks. Even still, virtually the entire crew ended up at the Bethesda Naval Hospital with severe psychosomatic ailments, toxicity, and burns. Brown himself -- who had been aboard the *USS Eldridge* -- suffered a nervous collapse.

Confronted with the dismal and unexpected result, the experiment in its original form was discontinued. Over the years, physicists like Einstein, Infeld, Gunn, Hulburt, Gebhardt, Bitter, etc. poured over the data to figure out what had happened, where it went wrong. For Einstein, the problem was tremendous. There was little chance now that he would ever succeed in completing his Unified Field Theory since the properties of the total field clearly were not understood.

J - So the ship did not disappear and leave an indentation symmetric to its hull on the surface of the sea?

W - No, that's sailors' lore. The ship "disappeared" in the sense that it became enshrouded in fog. It was described as grayish green fog traversed by constant shimmering as though it was surrounded by a swirl of thick, silvery heat waves that appeared to spin.

J - And no gravitational redshift was observed?

W - When it came to the radar bounce, it was more like a jamming effect that dispersed the reflected beam in every direction. At first, they thought this was good news -- some form of invisibility had been

reached with a new kind of jamming to boot. But then to their horror, they realized it was sheer havoc down there.

J - Was there an optical mirage?

W - No, not like that. The optical ranging of the fog was still in the same location, so no bending of light had occurred.

J - So what caused the fog?

W - It took me many years to understand the answer to that question. The observation that put me on to it was that **Reich had seen a similar green fog during some of his Oranur experiments**. He attributed it to a very high concentration of orgone energy that was exposed to ionizing radiation -- particularly to neutron radiation.

J - I thought that Reich claimed this green fog was caused by DOR – “Deadly ORgone” he called it --

W – Yes. Orgone concentrated by an inrush would turn into deadly orgone, that was his interpretation. That could well be the case. Without going there, my point was that the green fog could also be due to the secondary release of protons.

J - I still don't understand... Was the fog due to DOR radiation or protons? Or like **[J.] Corum** claimed in '94 [\[21\]](#), due to chlorine gas released from the water? You know, the yellow-green color of chlorine gas.

W – No, Corum is mistaken. The green fog that was observed was the green line characteristic of the formation of hydrogen gas from atomic hydrogen, from the free-radical state. Something that was totally unknown back in '43 but not today. Whenever you have free protons – say, because of ionization or electric polarization -- and they are subjected to some cycle of recombination with an electron plasma, you will generate hydrogen free radicals. The hydrogen radical may convert to hydrogen gas and release that green light or instead absorb more energy and re-ionize.

Reich, in fact, was generating protons and hydrogen radicals with nuclear sources inside his orgone room. And Rainbow with its tremendous magnetic and RF fields was doing exactly the same thing in a much grander scale and at much greater intensities. These combined fields ionized the water and released protons, polarized and recombined them, generating hydrogen gas and ozone, and produced an electron plasma that was pushed out all around the ship.

J - Was this the cause of the shimmering?

W - Right you are! The energies imparted to the electrons were split by a permanent magnetic field. So the photon frequencies radiated by all possible orbital transitions were greatly multiplied by the Zeeman effect. Some interpreted what they had seen as proof that some distortion of space-time had occurred due to employing electromagnetic fields resonant with the nuclear structure of matter. So it appeared for a time to have validated the general hunch of Unified Field theorists ...

... except for the fact that it's all bunk. In my view, you know, to think that any of Einstein's Unified Field theories were on the right track or met with success is a bit like imagining that a Roman charioteer with all his precise knowledge of chariots might have been able to engineer a modern automobile if he happened to stumble across one. What would his description be like? "It's not drawn by horses, but seems to have a legless metal horse permanently imprisoned inside its bowels. Wood is absent, and the

horse is not fed with grass but rather with a slightly colored alcohol. The wheels have soft shoes made of solid tar ..." and so on.

You see my drift. The charioteer's description may actually be accurate in terms of what he knows and the language he has at his disposal. But he would never be able to reverse-engineer let alone design or build a modern automobile from his own description, nor understand how an automobile works, or how the internal combustion engine functions as "a horse".

Likewise with Einstein's theories of the Unified Field. They simply don't give the tools one needs to be able to understand gravity let alone anti-gravity. Everything that happened in Rainbow can be understood today with tools that require no invocation of a Unified Field. The events can be entirely explained by what we now know about nuclear magnetic resonance, the chemistry of water, free radicals, and so on. All contributions made by quantum physics -- not Relativity or the Unified Field.

This is not to say, of course, that there are no gravitational anomalies including anti-gravitational ones associated with truly incomprehensible observations. But our physicists and our physical knowledge are just as impotent to understand them today as they were back in '43. We grasp them. But only like the Roman charioteer would have grasped a modern automobile -- totally inadequately with respect to understanding how gravitational fields form and how they can be counteracted.

N. What was wrong with Einstein's UFT?

J - What do you think the main problem was with all the predictive theoretical work behind the early stages of Rainbow?

W - For my part, I believe that one of the main problems Einstein was faced with was integrating Maxwell's equations. It's not simply that they fail when powerful fields are employed. I think they have intrinsic errors -- even in their application to ordinary fields. So, Einstein was, in fact, lacking an appropriate electrodynamic model and so his treatment of the electromagnetic field cannot be correct.

You see, Maxwell's equations can be fitted to what Einstein called "a skew-symmetrical tensor" that permits their treatment in Euclidean space. But only if we stick to Lorentz's interpretation of those equations for closed circuits. Magnetic intensity never enters into the tensor treatment -- only the electromagnetic field and the electric current density do. But as the magnetic field turns out to be a property mediated by the energy structure of the charges -- or what's worse, by the relative mass of charge carriers -- the same intensity could have very different field velocities or the same field velocities could exist for very different energy magnitudes. This connects to another problem -- the complete disregard that all of Einstein's attempts at a Unified Field Theory had for the quantum structure of matter.

J - But he did willingly admit this.

W -- Yes. Little was known about the electromagnetic field of "concentrated charges". If inserting a scalar for the density of matter appeared fictitious, replacing it with a tensor of energy per unit volume that was capable of expressing the roles of matter and electromagnetic energy was even more dubious.

Moreover, today, I don't believe his formalism can be correct. If the General Theory is a matter of physical geometry where it is important to ascertain whether its axioms are true-or-false by empirical determinations, then its axioms regarding space and time cannot be said to be correct. Metric relations are seen only as the geometric properties of bodies -- not as energy relations.

Then, there is an assumed priority of topology over metric as if space-time had merely a formal order for its points and as if these points had a real existence that constituted space-time. Einstein is aware that this simple numbering of space-time points had no metric significance whatsoever. Space and time remain distinct, he says -- so that space is three-dimensional and time one-dimensional -- but they are treated as a continuum by the mathematical artifact of a 4-dimensional space-time. Time-like line elements are treated as if they were space-like line elements, as imaginary distances.

In the Special Theory, the coordinates have both metric and topological functions. But in the General Theory, they only perform a topological function and do not permit any calculation -- by means of coordinates -- of the interval or the distance between any 2 points. The metric functions are performed instead by the metric tensor g , made up of 16 components (4 components per imaginary dimension). For a theory that wanted to derive the metric properties of space-time from the content of this space-time, from the energy of the continuum, this formalism would seem quite arbitrary!

J - It sounds to me like the failure of mathematical formalism divorced from empirical data. But wasn't it also -- and more profoundly -- a failure of field theory? Above all, I wanted you to answer this question.

W - You mean failure of Faraday, Maxwell, Einstein ...? Which? All of them?

Perhaps ... hmmm, perhaps they all failed if by “field” you mean these topological lines of force that to this day lack any physical reality. But, if you mean “field” as a discrete zone of activity formed by some energy flux -- following the intuition that energy generates forms, that energies act in concert, and there is continuity of energy and media -- no, the failure cannot be taken as anything other than the failure of specific field theories.

I'd much rather say that the scientific objective should be understanding the morphology of energy systems, not replacing them with *ad hoc* topologies. Einstein pushed a formal concept as far as it could go. But “field” without energy -- and without considering energy properties -- is not a viable approach, if you see what I mean.

J - And he failed.

W - Because he had no direct way to connect metric systems with the structure of energy. When he thought “structure of energy”, he always thought the influence exerted by the average density of matter upon the metric structure of space. His theory of the total field could never go beyond topology ... could never extract the metric relationships that are inherent to the **structure** of energy ... could never get at **energy morphology**.

Space without matter might well be subject to acceleration, but that can only be because it cannot be void of energy. If space-time is determined by its energy content, then the metric has to be intrinsic to that content. It cannot be arbitrary. To know the structure of the energy **would be to know its metric**. Einstein was likely correct in assuming that gravitational fields still exist in space without matter. But he could only introduce an energy density tensor by reducing energy to electromagnetic energy and its main field component (matter).

So he proceeded to develop his Unified Theory with a provisional “energy tensor of matter” that was strictly an electromagnetic field function. Despite all these shortcomings, a perception was created after the War -- in the public, the media, and in the scientific community -- that, with his unification of the gravitational and electromagnetic fields, Einstein had succeeded in finding the key to the physical

understanding of the Universe. Announcements of this "profound discovery" were carried by major newspapers such as *The New York Times* in [January of 1950](#). And that is the perception that remained throughout the '50s.

J - Yes, it's the second time you bring this up. But you've never really explained why you think it happened besides the obvious "feel good" effects for our sense of national pride?

W - Maybe it was also a signal of sorts. I like to think that it was the aftermath of the original Rainbow.

J - How so?

W - Shortly after the War, Infeld left Princeton to go to the Department of Mathematics at the University of Toronto [\[22\]](#). There he concentrated on the problems raised by a unified treatment of magnetism and gravitation in light of the emerging field of nuclear magnetic resonance. Now, earlier in that same year of 1950 [\[23\]](#) at the height of the Cold War, Infeld defects to the Soviet block (Poland) with all the work that he'd done in that field. Which is why Reich printed these news pieces in his "Einstein Affair" in '53.

J - The implication being that, all along, Infeld was a Stalinist spy?

W - Quite so. There can be no doubt about it. So the signal to the Soviets appears to have been *"You've got Infeld, but we've got Einstein. And meanwhile, our Einstein has solved the problem, thank you very much."* Which he hadn't, of course.

J - Face-saving. Yet later, as nowadays, it became commonplace to think that his UFT was ill-fated. So was this just posturing? Einstein hadn't resolved anything after all?

W - Well, it's claimed that all three of the acid tests that Einstein suggested would provide proof for the General Theory have now confirmed it. But the theory of the total field has never been proven. And the ill fate of the early Rainbow is there to remind us that its predictions were deeply flawed.

Are you beginning to see now why it was obvious to me that Reich's orgone theory wasn't at all compatible with Einstein's approach to the Unified Field?

J - Yes, but not yet fully.

W - Look, if the continuum is one of mass-free energy, it cannot be treated by a mere topological representation in 4 dimensions. Einstein considers gravitational fields as exclusively questions of topology. And the only energy he recognizes is electromagnetic and has inertial effects. To speak of curving light in a gravitational field is just another way of saying that light carries weight and inertia.

Reich discovers a mass-free energy that has no inertia and will permit us to engineer space. The notion of a space-time continuum is foreign to Reich. Einstein's Relativity abolished the notion of simultaneity. Reich's work with pendulums and periodic motion restores simultaneity and brings it back to questions of energy. Einstein's Special Theory treats photons like fibers of light that propagate across space. And later his General Theory treats them as geodesics. What use would Einstein have for a theory like Reich's that claimed that light or photons propagate by "orgonotic excitation" or that they are the "lamination" property of orgone?

Electromagnetic fields may well be consequences of a unified field. But this would not even begin to tell Einstein how such consequences arise. And now that we're on this one, what did Reich ever think

he wanted with Relativity -- a theory that made metric subordinate to light, Time subordinate to Space, and invoked time-dilation and length-contraction?

J - I see now. So the riddle...

W - [The Riddle](#), yes. The riddle is a double one that Einstein -- despite the negative results of Rainbow -- stubbornly refused to realize that the electromagnetic field is secondary to the primary organotonic properties of mass-free energy fields. He couldn't really understand Reich for the same reasons that he couldn't understand the early Rainbow results. There too, if I'm right, what that underlying mass-free field did was never understood.

The other side of The Riddle is that Reich himself didn't know enough about his own work at the time to realize that it really wasn't at all compatible with Einstein's approach to a UFT or to really be of any help to Einstein in his quest for a UFT. That realization only came later -- some time before '53.

J - Maybe the whole story teaches us something about seeking approval for new ideas from established authorities ...

W - Or about the openness of scientists or science in general. Now imagine that you're a car mechanic and by way of some sort of time-machine, you drop in -- equipped with toga, manners, Latin and all -- on an ancient school of charioteers. Suppose further that they're perplexed by this automobile that was dropped on some earlier occasion on some patrician's estate. If you want to explain to them what this car is and how it works, you must first get them to listen to you.

But you are not a charioteer. They've never seen your mechanical or driving prowess. They simply won't listen to you. You must first become a charioteer, and then they might listen. Now suppose you set out to become a charioteer, you go through the motions year-after-year until they will listen to you. And if you're lucky, one day they do. And you compress for them the knowledge that led to the internal combustion engine across 20 centuries of history. They might even be disposed to listen at first. But soon enough they'll throw you out of there. Or kill you if you're unlucky. They'll be absolutely certain that you are speaking nonsense. Yet, you could be telling them a profound truth!

Major discoveries in Science often suffer this fate. The peers ignore them, there is intention to suppress, the discoverer is scorned, poor replications are carried out. Why? you might ask. Because the reaction of most people when confronted with having to learn something new that is complex is to feel threatened and insulted while doubting its veracity. People don't like to change their ways nor to be told that they're wrong or -- worse still -- be proven wrong! That is the real answer to the riddle.

J - That's almost what Reich himself said over and over...

W - Uh-huh. But it gets worse for I believe that in the end, people admired Einstein for the very same reasons that they ridiculed Reich. They didn't understand either of them. When the physicist Harold Urey from the OSRD asked Infeld why one couldn't use the "master theory" to resolve problems such as those posed by Rainbow, Infeld answered condescendingly: "Like Chinese, you must first learn the alphabet". And you know what? Urey replied, "Had I bothered to read it, I would still not have understood it!" So clearly, he hadn't!

And no one - neither Infeld, nor Einstein, nor any of the FDA physicists -- ever presented evidence to contradict Reich's experimental claims. Not even in 1953 when Reich felt the FDA tightening its noose around his neck and finally published almost all of his correspondence with Einstein in a booklet

called "The Einstein Affair". A thousand copies were printed by the Orgone Institute Press without asking any authorization from Einstein himself. But from Einstein's side came not a whisper.

J - That's where [The Riddle](#) text comes from, right?

W - Quite so. But Einstein appears to have betrayed Reich's confidence well before then. A German physician, Dr. [E.] Jacobsohn claimed in '47 that Einstein had told her that he'd been unable to reproduce Reich's findings. Which would have been exactly the same lie that Infeld had been spreading and that the FDA was clinging onto. Don't forget the other significant event in this story -- Reich's final arrest and imprisonment in 1956.

J - No, I won't. But Einstein appeared to have broken Reich's confidence in '47?

W - That is what the record shows. And you know, that's how one knows about these little conspiratorial chapels. They always leak insidious rumors while suppressing overt publication of what was or was not said. With the beginning of the Cold War, Einstein lost many friendships.

But you are right. Yes, Reich's real mistake was to have sought approval from the authority of "great men" -- people like Freud and Einstein. But if he hadn't, we might never have known the truth about Einstein, heh?

J - I don't get you. It's apparent that his Unified Field Theory failed. But Special and General Relativity are well established. You said so yourself. Reich was unable to do anything that would shake this. It stands to this day.

W - I mean the truth beyond the pretensions of the physics establishment. And the proof is visible. They no longer seek to understand gravity or anti-gravity through Unified Field Theory but through all manner of "quantum" hypotheses about gravitons, conversion of electron-positron annihilation, infinite energy of the Zero-Point, and so on.

The basic understanding of the structure of matter that Einstein lacked is still missing. And we are still stuck on the dogmas of the topology of space-time and have gleaned little else about gravitational fields.

J - So, there were no positive, direct outcomes from Rainbow aside from the U-2 business and such like?

W - No, there were -- as you see -- even direct ones. Hmm ... but not that way, not with respect to the Unified Field hypothesis. For one, you have NMR and all the benefits -- including medical ones -- that have come from it.

J - Yes, you're right ...

W - ... And for two, you've got a major triumph for Lorenzen's Section in '44. When radio-controlled bombs were launched against Allied ships in the Mediterranean in late '43, the Navy captured a Henschel-293 glide bomb like the one that destroyed the Rohna in November. This was an integral part of Rainbow, with similar ship support protocols but entirely different setups. Lorenzen's Section employed a 2-destroyer system to record magnetically on steel wire the German steering signals and to analyze the frequency shifts. His renamed Special Projects Section was able to jam the German radio signals, and even successfully usurp the frequencies so that the Luftwaffe was deluded into thinking that the guidance system was faulty and unfriendly.

The War was made and won by these small victories. And they often came from comprehensive investigations in basic science -- even incomplete or unsatisfactory ones. In fact, when it comes to this, we should take our hats off to people like Bowen, Gunn, Hulburt, Gebhardt, and so on for the simple reason that before and during the War, they always pleaded the case of basic science and never entirely succumbed to the logic of research only for direct military use. But they were all heavily penalized for it, too.

J – Extraordinary! I do very much want to thank you for shedding so much light on these troubling matters. I'm sure it took a great deal of courage to come forward. I know, I know you feel you're close to the end of your life. You've told me. And you believe that the truth must be told, that too many lies and injustices have been committed to prevent it from getting out.

W - The Navy's denial has always bothered me. It seemed to be protecting Einstein's reputation and us from his failure. But then Allen's letters and their timing -- and the unnecessary death of Jessup and what was done to Reich -- they, too, bothered me greatly. Maybe one day you'll release this information?

J - Yes, though I wonder who will listen to me.

W - Yes, who?

J - I have one last question, if you don't mind. Einstein's last texts on the Unified Field problem -- I believe in '53 -- do they address his failures?

W - Yes, they do. There is some serenity that can be read in them. And there is a parting line where he states unequivocally that all of his efforts did not succeed in coming up with a model of the real continuum and that maybe, somewhere, there lurks a more fitting theory -- one that will succeed in describing reality with a purely algebraic approach [\[24\]](#). Do you know what is the greatest irony of all in the riddle of the meeting between Reich and Einstein?

J - No, tell me ...

W - When they met, Reich thought that he had something to contribute to the Unified Field. But he hadn't -- as he later realized -- because his discovery was not compatible with the Unified Field nor did it need it. But by the time Einstein concluded that his entire life's work might have to be superseded by a purely algebraic theory, Reich was in fact laying down the basis for one.

He never systematized it or completed it. But supposedly while in jail, he was working on a book entitled Creation in which he was providing an integral mathematical presentation. Apparently, that book was burned in '57 by the prison authorities after Reich died from a very suspect heart attack. Why these authorities burned Reich's manuscript instead of giving it back to his family has never been answered. Certainly I, for one, don't know of anybody who has succeeded in creating such a purely algebraic theory. What's more, I believe that nobody is even looking for one.

O. Commentary by the Correias

"Many of the theorists like Hal Puthoff (...), seem to be taking quantum mechanics and General Relativity at face value. They are trying to wring from theories of 'Zero-Point Energy' or

distortions of space-time advanced methods of space-propulsion and anti-gravity. Others speak of exotic 'torsion fields'.

“From my perspective, this will eventually be seen to parallel the bird-imitating mechanical wing-flapping devices of some of the failed aeronautical pioneers. The answer to anti-gravity is probably much closer to home. It may be present in relatively straightforward laboratory experiments that demonstrate the measurable properties of an energetic aether that manifests omnipresent anti-gravitational components -- even in such mundane devices as elevated gold-foil electroscope leaves.

“Great truths -- like lost car keys -- are often right under our noses. We can't find them because we are looking in all the wrong places.”

-- Eugene Mallove, 2004

The “Philadelphia Experiment” is a well-known caper as the Smyth interview explicitly acknowledges. So we can only wonder who is attempting to handle us by sending Akronos this information for free, when a best-seller could be made of it, irrespective of whether it is fact or fiction.

However, just as Dr. W saw a backdrop of truth in Allen's caper, we see a great deal of truth in this interview. Yet, we cannot corroborate the sources, nor even the identities of the interviewer or the interviewed. “Unremarkable” men indeed!

Speaking of the unremarkable, we should like to draw the reader's attention to a curious, unremarked detail of the official biography of Einstein. Somehow in the critical years between 1941 and 1944, he appears to have done nothing -- not just nothing remarkable. In 1941, the year of his meetings with Reich, he is kept out of the Army's Manhattan Project that he indirectly helped initiate with his August 2, 1939 letter to President F.D. Roosevelt because, most ironically, he is considered a security risk! He appears to have done nothing for that entire year and positively the same for 1942. The same would seem to apply to 1943 and 1944, except that we're told that in 1943 he was employed by the Navy as an adviser on high explosives!

That is all the curiously as Einstein knew nothing of explosives. And the only 'high explosives' to which his advice could have been of any pertinence were the nuclear ones -- the very same ones whose development at the time had been exclusively taken over by the Army to the exclusion of Einstein and Einstein's presumed employer (i.e., the Navy).

Einstein's inactivity is the more remarkable since these are the key war years during which Einstein had Infeld working as his personal assistant. In light of this, one can only wonder whether Smyth's text does not relate precisely what Einstein was up to during those critical years of 1942 to 1944.

So, we are left with what the text says. Which is exactly what Smyth wants and says he wants. Dr. W's ideas in the text are curiously close to our own work and analytical model (i.e., Aetherometry). Yet Dr. W does not seem to be familiar with it. He has not even realized that Aetherometry is precisely that purely algebraic theory that continued in Reich's last footsteps and has made Relativity obsolete. He's not a Reichian, but seems to employ some of the language and concepts that Reich used to describe phenomena which the analytical and mathematical language of Aetherometry has long ago surpassed. This suggests to us that Dr. W has been retired from an active interest in the field of Aether Physics for some time.

Even though we have agreed to publish Smyth's text because we recognize its multiple virtues, we are compelled to disagree with Dr. W. As we have shown in our report on the replication of the Reich-

Einstein Experiment, the problem of anomalous evolution of heat was really central to the disagreement between Einstein and Reich -- not peripheral to it. On Reich's own admission in a 1943 letter to A. O'Neill, the exclamation "a bombshell in Physics" referred directly to the consequences of Reich's discovery **for thermodynamics**, **not** to the implications of Reich's work with magnetism. It might have referred to the totality of the implications of his work and have included Reich's views on magnetism, as he would have orally conveyed them to Einstein. But this is not -- at the very least -- how Reich chose to report it. Since Dr. W did not provide his sources, one can only be left to wonder about how he got his facts.

In what concerns magnetism, we should note that Dr. W also does not make any reference to how Reich changed his views on magnetism between 1941 and 1947. In the Cancer Biopathy, Reich will no longer regard magnetism -- ordinary magnetism or ferromagnetism -- as a manifestation of orgone energy. Curiously, he arrives at this conclusion by experimentally observing that the North-pointing end of sensitive magnetic needles placed inside of 1 cubic foot Faraday cages -- regardless of their orientation -- point upwards towards the center of the 4 upper edges of the box and never downwards or towards any vertex.

A perfectly balanced needle placed horizontally on the Earth's magnetic field does not orient itself horizontally. Rather, in the northern hemisphere, its N pole will dip downwards. And in the southern hemisphere, its S pole will do the same. The Earth's magnetic field is not horizontal. And neither do the magnetic meridians coincide with the geographic meridians. An iron rod is more easily magnetized by the local field when it is set not horizontally but at the angle of the dip. The resultant magnetic field of the Earth points therefore to the inside of the Earth. The angle between the field's direction and the horizontal plane is called the magnetic dip. And the angle between magnetic and geographic meridians is called the magnetic declination (in naval language, the variation of the compass).

Reich's argument is therefore that inside the Faraday cage and in the northern hemisphere, there is "an inverse geomagnetic relationship" whereby -- independent of the orientation and position of the cage -- the N pole of the needle will point up to the center of the top 4 corners, not down along the direction of the dip. That's the anomaly which he claims and which made him state in 1947 that orgone was different from magnetism. In the Feb. 1941 letter to Einstein, he reports this finding but states that sometimes the needle points up, sometimes down. This is the same letter in which Reich proposes that we should regard geomagnetism as being due not to ferromagnetism, but to the flux of orgone energy. He never explained how this was to be construed.

If geomagnetism and gravitation are interlinked, magnetic dip -- with its complex variations as a function of solar radiation, atmospheric structure and thickness, air temperature, atmospheric electrification, oxygen mass of the columns of air, etc. etc. -- would have to be construed as a function of an Aether flux that was nearly vertical to the Earth's surface. If we put this together with our aetherometric theory where gravitation is caused by horizon-incident, downward-curving spinning lattice flows of mass-free energy (see [A Note on Dayton Miller](#) and [Mysteries of Inertia](#)) responsible for the Earth's rotation and the null result of the Michelson-Morley experiment, it becomes easy to see a possible link between geomagnetism and gravitation and the cause of the magnetic dip.

Assuming that Reich would have done proper controls with different cages placed in different orientations, that the needles were properly centered and not simply pointing towards the opposite poles of a permanent magnetization that was geomagnetically induced in the cages, the observed reversal of the magnetic dip would then suggest that a local reversal of the field had taken place.

If we make one more correlation -- with the observation of anomalous sensible and latent heats evolving, respectively, atop and beneath the top of these cages (see [Vol. I of Experimental](#)

[Aetherometry](#) and our [last communication on the HYBORAC technology](#)) -- it is tempting to conclude that this dip reversal confirms the antigravitational properties of the trapped latent heat in the kinetoregenerative phenomena of electroscopes. One could easily take one more step and argue that dip-reversal is a genuine diamagnetic response of the mass-free Aether and thus connect pole-independent magnetic repulsion with anti-gravitation.

Why have we not done so? Certainly not because we believe in field theory so much as to argue that at the South Pole, the geomagnetic field has exiting “ex-cident” lines -- not incident ones. If magnetic polarity is not real, lines enter-or-exit only in a relative way and in a manner of speaking. But this relative way is what specifying relative helicity or direction of vortex rotation is all about. So it cannot be entirely discarded. For the Earth to present a near coincidence of the magnetic and rotary axes, they must be connected in some fashion. And the magnetic field can only be the product of longitude-oriented electric currents, back-to-back northern and southern vortices which are pretty much reflected in atmospheric flows. The geomagnetic field can only originate where those electrical currents also originate. They regeneratively feed on each other - geomagnetism working as an energy storage system that feeds back the dynamo currents which, in turn, sustain the magnetic field. But their energy must come from somewhere.

Had Smyth or Dr. W been familiar with Aetherometry, they would have realized that magnetic fields of particles of matter in a “vacuum” or the “medium of pure space” or, still, in a plenum of mass-free energy, correspond to very different densities of magnetic lines than are found for the magnetic fields of the same mass-bound particles traveling through a material medium be it a gas or a solid, a diamagnetic or a ferromagnetic medium.

And they would have realized that the same cyclotron frequency will produce different magnetic line densities, according to whether the medium is a material one (and what its magnetic nature is) or is, instead, composed solely of mass-free energy; or, still, according to whether the field itself is created by mass-bound charges or mass-free charges. However, there may well be deep truth to the idea that if anti-gravitation has magnetic effects or is to result from an interaction with magnetic fields, only a diamagnetic system which secured same-pole induction, irrespective indeed of the polarity of the local hemisphere, could make sense. The typical 'free-energy' (para)magnetic approach, whether ferromagnetic or anti-ferromagnetic, could never make any sense. These thoughts tie directly into the failure of ElectroMagnetic Field theory.

Perhaps the greatest failure common to Maxwell, Lorentz, Einstein, and Reich is that not one of them succeeded in correctly understanding what electric and magnetic fields are. The functions and the concepts have to this day remained imprecise -- so imprecise that even the physical dimensions of magnetic fields have remained erroneous.

From first principles, we have provided a radically new approach to the problem of magnetism and its practical applications [[AS2-15](#), [AS2-16](#)]. One that distinguishes between the magnetic field, the magnetic reaction of a medium (composed of massfree charges), the magnetic wave functions of kinetic energy, and the corresponding magnetic functions intrinsic to the structure of matter (electrons, protons, neutrons). We compared -- for the first time in the history of Physics -- the magnetic field effects of mass-free and mass-bound charges and provided the correct value of the gauss.

And we went further. We demonstrated how only the impedance of the vacuum to electromagnetic waves is a **constant** (conventionally set at 376.728 ohms and aetherometrically at 303.87 ohms) how -- with respect to mass-free and mass-bound charges -- the vacuum impedance is a **variable** that directly takes into account the inertial or noninertial status of those charges including the precise nature of the charge carrier. Yet...

Turning to Einstein and the Unified Field Theories: our own understanding of the ambivalent relation which Reich entertained towards Relativity is very close indeed to the one that Dr. W. presents in Smyth's text. However, Reich never provided a systematic criticism of Special and General Relativity. Nor of the Unified Field Theory, for that matter.

We have tried to do just that, respectively in our publicly-available communications: [Consequences of the Null Result of the Michelson-Morley Experiment](#), [The Sagnac and Michelson-Gale-Pearson Experiment](#), and [The cosmic background microwave radiation as evidence for cosmological creation of electrons with minimum kinetic energy and for a minimum of cosmic ambipolar massfree energy](#). Dr. E. Mallove -- then Editor-In-Chief of *Infinite Energy* -- thought these systematic deconstructions to be of such critical importance that in 2001, he placed the first two as the centerpieces of Issues #38 and 39 dedicated to the Special and General Relativity theories.

Yet the second latest issue ("100 years: Einstein's legacy", #59) of the new *IE* -- now under the technical direction of W. Cantrell -- manages to entirely omit the contributions of both Aetherometry and Dr. Harold Aspden to this very subject! To our grim amusement, Cantrell wants to resurrect the Aether but in its near-original condition as a "luminiferous Aether" once again.

Mallove repeatedly spoke against the viability of such an outdated notion. Even in its incarnation of the partially entrained or dragged Aether of Miller which the belated DeMeo has recently put on his banner. This is precisely where Cantrell leads the incautious with his one-sided editorial: back to the tired notion that the only alternative to Special Relativity is to assume that the residuals of the Michelson-Morley have the physical meaning of a non-rotating, partially-dragged luminiferous Aether.

The same "political line" continues on in the latest issue (#60) with the R. Cahill reprint where, once again, the same Fitzgerald-Lorentz contraction is taken to heart to illustrate how sense can be made of selected residuals from the Miller experiment. Mathematically, it is always possible to find residual data that permits one to keep the faith in absolute space (an absolute quantum foam) and a nearly-stationary Aether. But as Einstein saw it, it would be a queer neo-classical Aether -- one that occupied an absolute space with variable lengths. All of Mallove's patient and insistent pedagogical efforts are thus obliterated at once with the new editorial line. Just as Massfree Energy is wished away as if consigned in the new *IE*, to retrograde oblivion.

The entire work carried out by Mallove to enlighten the world about the non-electromagnetic and mass-free nature of the Aether and the powerful new tools of Aetherometry is thus erased in favor of Cantrell's old-style college-boy bias. Long live the old luminiferous static Aether renamed as foam! Long live unsystematic residuals! For good measure, Silvertooth is thrown back into the pile and the argument about discrepant directions -- which we made back in 2001 -- is repeated but without, of course, any credit being given to us! Brave new *IE*! But it gets better. By way of a fudged reference in his #59 editorial, Cantrell has Einstein publishing his Special Theory without knowledge of the Michelson-Morley experiment!!!

That was a good one that made us laugh. Especially because in the very same *IE* issue (#59), a reprint of one of Cantrell's "pet articles" -- authored by L. Essen -- states precisely the contrary: Einstein's Special Relativity "was essentially the electromagnetic theory of Maxwell and Lorentz modified to incorporate the Michelson-Morley result"!!! Does Cantrell bother to read his references? Apparently not.

But better still ... the role of weight distribution and inertia in real, imperfect clocks employed for physical measurement (an issue that should be of no greater concern than electric, magnetic, or thermal

influences and which arises because of the interaction between the uneven mass-distribution in the fabricated parts and the accelerated states of the mechanism) is taken by Cantrell to mean that "there is absolutely no argument that time-keeping mechanisms do slow down when moving at high speed and that they obey the time dilation formula of Lorentz and Poincaré."

Stunning idiocy!

At the end of the day, one is never too sure of what this truly retrograde resurrection of the stationary Aether -- and its fixation on the infamous residuals -- wants to blame Einstein for. After all, like Special Relativity, they swallow not just the Lorentz length transformation but just as well -- for good measure -- time-dilation.

Like Kafka's Oedipus, these are so-called Aether Theories that serve only for laughter. Faith in the residuals, careful selection of these, combined with a few tried and true mathematical tools and presto! space is absolute because it is static or nearly so. As if the Lorentz formula were any different from that of Special Relativity, as if invoking Lorentz would somehow save the luminiferous Aether, as if one could have time-dilation without length contraction, and as if the latter were not a shortcut concocted by Lorenz to save the dying theory of an electromagnetic Aether!

Step by step, Cantrell manages to rewrite our papers in absentia, upside down, for the benefit of a last-ditch re-interpretation of the old stationary, electromagnetic Aether (no real student of Reich would be found here; only DeMeo who, we expect, will soon jump on the bandwagon of this revamped *IE* that now makes a point of avoiding even a mention of Aetherometry).

Again, it's curious to note that in that same above-mentioned reprint, Essen himself concludes the obvious -- that the 300 nanoseconds of time delay reported indicates only that the clocks were not made accurate to that resolution!

No, Mr. Cantrell, no! The question of how to avoid confusing the artifacts of the density and distribution of mass with corroboration of time-dilation is not metaphysical (as if one could not readily ascertain where the error lies...). And neither can the confusion be indulged without importing everything else beginning with the length-contraction. Well, that shows us the quality of the new Technical Editors of *IE*.

Since 2003 and with the active support of Mallove, we have been publishing the foundations for an [aetherometric theory of gravity](#) -- directly integrated with the behavior of electric and magnetic fields -- and the electric properties of massfree energy. But we have stayed away from commenting directly on Einstein's repeated failures to derive a Unified Field Theory from General Relativity. A good way to begin would be to realize that there is an [alternative theory](#) as to why light-rays or photon-distributions appear to bend in gravitational fields. And that it has everything to do with how photons are emitted from decelerating charges; the globular and not fascicular nature of light (yes, yet another part of Reich's work that Reichians like DeMeo simply and arbitrarily discard); and the fact that these gravitational fields can be physically and mathematically explained by the differential (electric and nonelectric) structure of mass-free energy lattices.

Until aetherometric thought came along, even the nature of the graviton was elusive and mysterious. Mallove was well aware of the breakthroughs of Aetherometry in this critical chapter of Physics. And [he would certainly not have missed](#) the 100th anniversary of Einstein's 3 seminal papers (on Special Relativity; on the relation $E=mc^2$ which was first formulated by Jeans a year earlier; and on the photoelectric effect) as an opportunity to disseminate the entirely new, non-relativistic, and non-electromagnetic theory of a dynamic Aether. The last thing he would have done would be to engage --

as Cantrell has -- in a re-hash of the siren sing-a-song of the entrained static luminiferous Aether of yesteryear.

This text of Smyth's, by contrast, has some rare virtues. It takes Einstein seriously and feels no need to judge or deride him. The UFT failure appears only as the straightforward theoretical and experimental failure that it is. Such "failures" are the lifeblood of basic science. And it's quite tragic that only military budgets and only in times of emergency support this lifeblood. That society has become so dysfunctional as to no longer value basic science. And if Smyth does not suggest that Aetherometry is the answer, his/her text has at least the courage of not appearing to proselytize a return to half-assed theories even more useless than Relativity has been. Or to still greater miscomprehensions of Wilhelm Reich.

Perhaps the main reason why we think that most of Smyth's text is factual is that the author(s) seem to have gotten the main facts of the Science and History right. Those, at least, that we can check because indeed a curtain of secrecy seems to have descended upon the lives of most of the prominent characters of the story -- scientists like Gebhardt, Hulbert, Berkner, Lorenzen, etc. even if they were once made famous by their achievements.

The lesson of the story -- it also seems to us -- is indeed that matters of Science and natural Philosophy should never be left to authorities. Neither the authority of wise men, nor that of Generals and Admirals, nor of Editors and Peers. As Reich was fond of saying, there are no judges in matters of natural science. If scientists are condemned to making errors -- and especially those of interpretation -- then we had better learn from those errors IF we are to become real scientists. If there is one greatness to Einstein, it is that he had the courage to kill all of that useless and confused talk of an electromagnetic or luminiferous Aether. And if there is one great failure of Einstein's -- and a failure of all General and Unified theories -- it is that he did not succeed in understanding what the gravitational Aether was, nor how it was produced from [Mass-free Dark Energy](#) -- the energy source of all unitarian fields.

Even the Pre-socratic Anaxagoras appears to have been closer to natural reality when he first taught that the Aithr was a substance and its principle was nous, the principle of Levity.

Somehow, modern science is too massified, too heavy, too weighty, too cumbersome, too allied with social powers to grasp the inner workings of Nature -- in all likelihood -- because of its over-reliance on operational formalism and a form of empirical democratism where *ad hoc* models are judged by academic peer-systems entrenched in professional interest societies. The character of modern science lacks lightness and speed. And without Levity or Celerity, the thought and practice of Science will find neither the principle nor the substance of Nature. Only the crassest of phenomenologies.

P. Editor Footnotes

1. Later in 1953, Reich published this material as [The Einstein Affair](#).
2. Peter Mills -- prototype of the treacherous little man -- would later become one of the prosecutors and judges(!) of Reich.
3. These are the so-called "control results" adduced by Leopold Infeld to suggest that the anomalous evolution of heat atop Faraday cages was due to thermal convection.
4. The reference here is likely February 18, 1944.
5. Delivered in 1920.
6. Presumably Elsa Stai, though at least 2 other informers are mentioned in the FOIA files.

7. Actually there is an interesting discrepancy between the FOIA files and the facts related at various times by Reich himself and his wife I. Ollendorff. Release was only ordered in the timeframe given by Smyth's text. But Reich and Ollendorff always maintained that he was released on Jan. 5 following Reich's threat of a hunger-strike. This discrepancy leads one to believe that 'Dr. W.' was only familiar with Reich's files at the FBI or at State or possibly G-2 and not with the facts themselves.
8. Presumably, the February 20, 1941 letter.
9. Dr. W is demonstrably wrong since Reich himself made it perfectly clear on several occasions that the "bombshell in physics" expression referred to the thermal anomaly atop a Faraday cage.
10. In 1908 and 1916.
11. Later in 1943, he would become H. Göring's Reich Plenipotentiary in charge of the development of a German atom bomb.
12. This applies only to within a magnetic domain.
13. Apparently, this refers to a possible interpretation of Einstein's General Relativity Theory.
14. At the NRL, Abelson, later editor of Science for 23 years, was in charge of U235 purification by the diffusion process that he invented for the Manhattan project. The explosion took place on September 4, 1944.
15. Exact date: April 18, 1955.
16. This is incorrect. Plantier's postulate was published in the French Airforce magazine *FAF* in Sept 1953 - ie three years earlier.
17. See for instance p. 244 of The Report on Unidentified Flying Objects.
18. See p. 113 of the Varo/ONR edition of Jessup's The Case for the UFO.
19. In May 6, 1957.
20. The SS *Andrew Furuseth*.
21. Corum KL, Corum JF & Daum JX (1994) Tesla's Egg of Columbus, Radar Stealth, the Torsion Tensor, and the 'Philadelphia Experiment'.
22. It is curious that the [official biography of L. Infeld](#) has precisely the opposite information, claiming that Infeld worked at the University of Toronto during 1938-1940, ie before Pearl Harbor, and that he worked at the Institute for Advanced Studies (Princeton, N. J. USA), from 1936 to 1950, when he defected. This would make Infeld's stay at Princeton partially concurrent with his stay at the University of Toronto, masking Infeld's 1946 entry into the latter institution.
23. *NY Times*, March 17, 1950.
24. This is precisely what the Correas claim to have done through their Aetherometric Theory.

Q. The Riddle

from The Einstein Affair, Orgone Institute Press, Rangeley, ME, 1953 (HMP transl., 2005)

... Einstein's behavior remains, to this day, a riddle. Why had he not answered? Why did he break the promise he had given? There were many opinions about this in our circle. Some believed it possible that he considered the whole Orgone matter to be a hoax. This was contradicted by the understanding he had shown during our encounters. Others thought that he was influenced by certain industrial interests that did not wish the discovery of the Orgone to be confirmed by Einstein. This was contradicted by the fact that an Einstein does not have any need to bow to such interests.

To me, 2 other possibilities seemed more probable. Einstein -- as he himself indicated in his letter -- did not understand the Orgone. It contradicted firmly established fundamental notions of physics. He did not want to risk exposure and considered it better to wait. The other explanation seemed more brutal but closer to the truth. It was that Einstein had understood perfectly well that the Aether had finally been discovered in a practically tangible manner. However, Einstein had built his whole theory of Relativity on the assumption that no Aether whatsoever exists [*] and that its existence is not even necessary for the solution of cosmic problems. For they could be solved purely by mathematics -- a notion supported by the confirmation of his theories through [scientific] observation.

It was humanly and scientifically understandable that Einstein would not want to take part in the overthrow of his own life's work even though strict scientific objectivity would have demanded it. The existence of an actual Aether did not, by itself, have to disturb Einstein's model of Nature. Only if a new cosmogony was successfully derived from the properties of the newly discovered Aether would Einstein's theory be unseated since it would have become superfluous.

[Margin note in German, in WR's handwriting:

"Consequently, I had to take back the assertion I had made in the letter to Neill according to which my discovery supported Einstein's Field Theory."]

I do not know whether these ideas are correct or erroneous. Einstein himself is to blame for the confusion in that he withdrew from the affair in such an ugly way. And yet this "affair" was compelling and clear. It is possible that Einstein underestimated the scope of my discovery and its consequences. In brief, I can only advance conjectures but not assert anything with certainty.

In subsequent years, I increasingly leaned towards the view that this [first] encounter on January 8, 1941 was a meeting between 2 strictly inimical worlds: Mechanistic and Functional Astrophysics. The former a giant beast with unlimited means for combat and control. The latter an immature baby which had barely crawled out of the mother's womb. The newborn baby clutched in one fist the facts of "Cosmic Energy" and in the other the facts of "Sentient matter". This would be enough to frighten even the most courageous of men.

Wilhelm Reich, MD

* Editor's note: Notice the subtlety of Reich's meaning ("Nun hatte aber Einstein seine gesamte Theorie der Relativität auf der Annahme aufgebaut, dass der Äther überhaupt nicht existiere"): he does not say that Einstein had built his theory "on the assumption that the Aether does not exist", which would still allow for the possibility that the word "Aether" referred only to the luminiferous Aether. Instead, he writes: "Einstein had built his whole theory of Relativity on the assumption that no Aether whatsoever exists", which means that "Aether" includes not just yesteryear's models of a stationary Aether (luminiferous, etc) but also any notion of a gravitational Aether, including Einstein's own.

R. Einstein in the New York Times, January 1960

Editor's Note - Our investigation has suggested 2 possible *N.Y. Times* references to Einstein and his success at construing a UFT during January 1950, listed and excerpted below:

New York Times, January 1, 1950, p. 81

DR. EINSTEIN'S LATEST GREAT THEORY
Gravitation and Light Different Kinds of Physics

"(...) At the end of the year came Dr. Albert Einstein with the important announcement that he had developed a generalized field theory which in 4 equations unified gravitation and electromagnetism (light, magnetism, electric phenomena in general)"

New York Times, January 9 (13?), 1950

EINSTEIN ANNOUNCES PROFOUND DISCOVERY

"(...) Now Dr. Einstein has gone one step further. He has a series of equations which, he says, expresses all the relationships of the physical Universe. Particularly, they tell the relationship between gravitation and the electromagnetic force that is all around us."

Editor's Note - This second reference and excerpt was taken from F. Scully's The Truth Behind Flying Saucers. We should view its accuracy (including the stated time of publication of the NYT article) and relevance with great skepticism.

Commentary by the Correias - Both of these references appear to be simply follow-ups on Einstein's enigmatic explanation of his quest for a "theory of the total field" put forward in a *Scientific American* article in January 1950 entitled "On the Generalized Theory of Gravitation". There, Einstein described the problem posed by the very different or opposing solutions (E1 and E2) to the unified or total field that he and others had found. And how he had then come up with a third solution (E3) such that "every solution of E3 is a solution of E1 as well as of E2." As far as we know, Einstein has never presented that solution E3 and, in fact, none was mentioned in his last writings on General Relativity (also quoted by Smyth's text).

It might be good to keep these enigmatic words of Einstein's in historical context in relation precisely to the events that led Infeld to defect . Here is the temporal sequence: Klaus Fuchs' first interrogation occurs on December 21, 1949 (followed by interrogations on December 30, 1949 and January 13, 1950); Einstein's *Scientific American* article - where he reports that he has solution E3 to the UFT but "cannot yet postulate it" as such because "it is not justified without further analysis" - appears in the first week of January; on January 23, 1950 under questioning by the MI-5, Fuchs confesses and on February 3rd he is arrested by the Scotland Yard; 6 weeks later, Infeld [defects](#) to Poland from Canada, following a refusal by the University of Toronto to grant him a leave of absence.

Infeld's defection was quietly regarded as being due to the security risk he posed because of his knowledge of nuclear physics. But his actual field of investigation was General Relativity, which raises the question of the extent to which his knowledge of the Unified Field played an additional role in his defection. More recently (since 1995), Infeld's defection has been rehabilitated as an "exile" and an "[injustice](#)" caused by his "[peace-activism](#)".

Most curiously and ironically (given what happened to Reich), in accordance with new details emerging from the FBI files on Einstein which were declassified in 2002 (The Einstein File: J. Edgar Hoover's Secret War Against the World's Most Famous Scientist by Fred Jerome, St. Martin's Press,

2003), on February 13, 1950 -- the day after Einstein had appeared on the first of Eleanor Roosevelt's television shows to discuss his views on nuclear proliferation -- J. Edgar Hoover personally demanded a complete investigation of Einstein. The same declassified files show that Army intelligence was aware that Einstein's office in Berlin -- prior to his 1933 emigration to the U.S. -- was a "cable-drop" for Stalinist agents. Whether Einstein was aware of this or not remains an open question (also ironically, one of the main objections -- drawn up by the Woman Patriot Corporation -- to allowing his entrance into the U.S., cited the large number of anti-Stalinist anarcho-communist organizations that Einstein was affiliated with!).

S. Infeld in the *New York Times*, March 1950

New York Times, March 18, 1950

SCIENTIST'S LEAVE STUDIED

Einstein's Ex-Associate Seeks to Teach Again in Poland

Special to *The New York Times*

OTTAWA, March 18 - Opposition leader George Drew in the House of Commons this afternoon released the question of the propriety of permitting Dr. Leopold Infeld -- a former associate of Albert Einstein and at present a teacher of mathematics at the University of Toronto -- to return to Warsaw to organize certain educational programs in cooperation with the Communist Government of Poland.

Mr. Drew said that Mr. Infeld -- who had been given hospitality as a refugee in Canada and the United States -- had gained considerable knowledge of the latest discoveries in the atomic field.

He had several times stated that he would return to Europe if-and-when a "progressive government was established in Poland". And he returned there last summer and taught in the Universities of Warsaw and Cracow, Mr. Drew said. Mr. Drew added, he [Infeld] had applied for a sabbatical year during which he would receive half pay from [the] Toronto University in order to permit him to work at educational organization[s] under the Polish Government.

Editor's Note - In connection with this news item, see also the [Commentary on the NYT new items of January 1950](#).

T. Infeld obit and short bio, 1968

Time, January 26, 1968

OBITUARY

Died. Leopold Infeld, 69, Polish theoretical physicist; of a heart ailment; in Warsaw. At Princeton during the 1930s, Infeld helped his friend Albert Einstein develop the General Theory of Relativity. With Einstein, he also shared the work of writing *The Evolution of Physics*, a 1938 text so fascinating to laymen that it hit the bestseller lists. At the University of Toronto, Infeld did pioneer work on the

unified-field theory of magnetism and gravitation. Then in 1950, he suddenly returned home to teach and proved something of a problem to the Communists after criticizing Warsaw's scientific censorship.

American Institute of Physics

BIOGRAPHY

Biography/History: Physicist (theoretical physics; general theory of relativity; quantum mechanics; studies in Cracow (W. Natanson) and Berlin (M. von Laue); professor in Lwow; Institute for Advanced Studies (Princeton, N. J. USA), 1936-1950; Toronto University, 1938-1940; Warsaw University, 1950-1968. Publications: Foundations of the New Field Theory (Born, Infeld), 1934; The Gravitational Equations and the Problem of Motion (with A. Einstein and B. Hoffmann), 1938-1940; On the Motion of Bodies in General Relativity Theory, 1954.

U. Infeld in the University of Toronto News, 1998

University of Toronto News, May 29, 1995

Closing the Circle

Posthumous designation "a blow against parochialism"
by Susan Bloch-Nevitte

The posthumous designation of professor emeritus for Leopold Infeld was a healing gesture. But it also became a reaffirmation of the University's international commitment. Infeld -- a U of T faculty member in the late 1940s -- had resigned following the Second World War when the University would not grant him a leave of absence to return to Poland.

This month in the wake of VE day celebrations, the University rectified the situation. Members of Infeld's family were invited to U of T May 17 to accept his professor emeritus designation.

At a Faculty Club luncheon, Professor Michael Marrus of the Department of History said the occasion marked a different and perhaps less honourable time just after the War. The climate throughout North America was one of intense anti-communism, characterized by "an utter lack of sympathy for the new regimes in eastern Europe and their challenge to rebuild their societies."

In the midst of that milieu, Infeld had planned to take his leave. It was his personal commitment to a society and an educational system in need after the War's devastation.

But fear and distrust held sway in Canada of 1949-50, prompting government and media speculation that Infeld's plans could pose a security risk due to his alleged knowledge of nuclear bombs. (In fact, his field was Relativity theory.) Unrelated departmental staffing issues conspired to delay his request. Infeld -- who was already in Poland -- resigned from U of T and began his work with the Polish academic community.

A little more than a year ago Chancellor Rose Wolfe brought the matter to the attention of Governing Council secretary Jack Dimond. He and Marrus spent considerable time scouring the University archives to find out more about U of T's role in the issue. Their findings suggested that while the University missed a critical opportunity 45 years ago to speak out on the principles of academic

freedom on behalf of Infeld, it had an opportunity now to acknowledge that omission through the posthumous designation.

44 years after Infeld left Canada and 26 years after his death, President Robert Prichard sent a letter to Infeld's son Eric, a professor at the Soltan Institute in Warsaw. He requested permission to designate Infeld professor emeritus -- a distinction that would permanently recognize Infeld as an "honourable member of the academic and Canadian community."

Up to that point, the Infeld family believed there was an important injustice waiting to be rectified. The letter in Eric's view crowned a 45-year "history of revision of official Canada's attitude toward my father." Marrus noted Infeld "was a person who felt justifiably aggrieved and there was a chapter that was unfinished. Ironically his international outlook was what we now acknowledge as central to our institutional mission. Our international agenda speaks of an institution not bound by parochial preoccupations. The story of Leopold Infeld and his legacy at U of T is a blow against parochialism."

V. Infeld in *The Ottawa Citizen*, 1999

The Ottawa Citizen Sunday, January 24, 1999

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HEADLINE: Shadowing Infeld: Secret documents show the lengths to which Canadian spies went to try to prove exiled physicist was a communist

BYLINE: LEONARD STERN; *THE OTTAWA CITIZEN*

On the evening of April 16, 1948, a member of the RCMP's "special branch" attended a public lecture at McGill University in Montreal. The special branch served as a domestic spy agency, forerunner to the Canadian Security Intelligence Service. On this spring night, the operative mingled in the audience, probably posing as a scholar or student, most of whom were associated with McGill's theology department. The agent had been assigned to report on guest speaker Dr. Leopold Infeld, then Canada's greatest physicist. A professor at the University of Toronto, Infeld was well known as a close collaborator of Albert Einstein. It was a great prize for Canada's scientific community when 10 years earlier, Dr. Infeld left the United States to come north.

Like Einstein, Infeld was a peace activist. "It has been clearly proven that no problem has been solved by war. And if we think any problem can be solved by war, we are preparing a chain of many wars," declared Infeld, according to the RCMP report filed after the Montreal lecture. ...

Yet 2 years later, Parliament denounced Infeld as a traitor who planned -- if he hadn't done so already -- to provide Russia with atomic secrets. As the *Citizen's Weekly* reported last July, the allegations against Infeld were based on rumour. Even so, his photograph was splashed across the nation's newspapers, his family was harassed, and in 1950 he moved to Poland, his country of birth. His Canadian citizenship and that of his young Canadian-born children were revoked. Infeld died an unhappy man in Warsaw in 1968. His tombstone says simply, "Leopold Infeld, physicist."

Last year, Warsaw University held a symposium in honour of Infeld's memory and organizers invited the Canadian government to send a representative -- someone, it was hoped -- who would clear the great scientist's name and acknowledge he had been unjustly hounded out of Canada. ...

The Canadian government probably began monitoring Infeld's activities not long after he left Princeton University in 1938 and took up his position in Toronto. By 1945 he was the subject of at least three investigations.

In the early Cold War period, one did not need to be a confessed Communist to be a security threat. Infeld once made a complimentary remark about a youth labour federation and that alone earned an entry in RCMP files. Years later, after he was exiled to Poland, he reflected on this climate of mistrust in an essay published by the journal of the Polish Writers Union. ...

W. Lorenzen bio

NRL Press Release
32-00r
5/22/2000

A Tribute to the Father of Electronic Warfare

Howard Otto Lorenzen
born - June 24,1912, Atlantic, Iowa
died - Feb. 23,2000, Redmond, Washington

In the early 1940s -- 2 decades before electronic warfare emerged as an ethereal form of combat with no obvious subordination to land, naval, or air warfare -- a military science that mattered equally to soldiers, sailors, and airmen -- Howard O. Lorenzen thought of it as radio countermeasures. After World War II during his years of investigating captured German and Japanese electronic equipment, he began to think of it as electronic countermeasures -- a discipline that detected and either interfered with or exploited for intelligence purposes any electromagnetic energy that an enemy might transmit for military purposes.

During the Korean War when some of his colleagues started thinking of their art as electronics intelligence (ELINT), he deemed their view too narrow and refused to adopt the term to describe his activities. He even stuck with the concept of ECM when he led the way in the late 1950s to its first successful application in outer space - an ECM satellite. In 1965 when the Cold War got hot in Southeast Asia and U.S. aviators were first brought down over North Vietnam by Guideline missiles that had to be thwarted, he fired up his project engineers by requiring them to think from now on in terms of electronic warfare. EW remained his central focus thereafter even when the Vietnam War wound down and he was called upon to lead space engineering for the Navy.

In his retirement after 33 years of public service to the Navy and the Nation, Howard Lorenzen relived his career by reading between the lines of histories of WW II, Korea, Vietnam, and the Cold War. Articles and books he enjoyed most were ones written by his former colleagues -- electronic warfarers on both sides of the Atlantic Ocean. He never wrote a book himself. He had a mania about security, and he was so renowned among those who lived the early history of EW that he felt no need to stake any claims. He satisfied his natural urge to communicate with his peers by means of letters and amateur radio -- a lifelong hobby and one he shared with most EW pioneers (who got their start in radio in their teens by building their own antennas, receivers, and transmitters).

The news is out now -- by means of obituary notices, letters, HAM stations, e-mail, and word of mouth -- that Howard Lorenzen is no longer with us. He died of pneumonia in a hospital in Redmond, Washington on Feb. 23, 2000 at age 87. As the word spread in today's EW community, some of its graybeards (like William E. W. Howe, James H. Trexler, Reid D. Mayo, Charles T. Christman, Lynwood A. Cosby) reflected on Howard's contributions and thought that the next generation might profit from hearing about them. Hence, this tribute to the father of U.S. EW.

War Years

In July 1940 after 5 years of designing commercial radios and components for Colonial Radio and Zenith Radio, 28-year-old Howard Otto Lorenzen started his career at the Naval Research Laboratory in Southwest Washington, working under the radar pioneers A. Hoyt Taylor, Robert M. Page, Louis A. Gebhard, Robert C. Guthrie, John P. Hagen, and Edwin A. Speakman. They had already partnered with RCA to get air search radar (200 MHz, 15 KW) installed on selected battleships, aircraft carriers, heavy and light cruisers, and one seaplane tender. Lorenzen built a high-gain receiver including an anti-jam capability for a lightweight version, successfully demonstrated on the destroyer *USS Semmes* in July 1941, then manufactured by RCA and General Electric for Navy destroyers and smaller ships. Radio Superintendent "Doc" Taylor and Assistant Lou Gebhard looked the other way when Howard got a lathe and welding equipment and set up his own machine shop to manufacture engineering models. They gave him new tasks and more 2- and 3-man groups of engineers and technicians to supervise.

Before the U.S. entered World War II, Lorenzen participated in NRL technical interchange with British scientists fighting the air war over Great Britain. After the Japanese struck the Pacific Fleet at Pearl Harbor, his radar work continued in receivers, cathode ray tube displays, transmitters, and motors for naval shore installations, ships, and aircraft. He consulted to MIT Radiation Laboratory engineers who developed microwave radar sets for the U.S. Army Air Forces under Division 14, National Defense Research Committee (NDRC). As the war progressed, Lorenzen gradually expanded to radio countermeasures against enemy system -- most notably, the air-launched guided bomb. He developed a system installed on 2 destroyer escorts to intercept, record magnetically on steel wire, and analyze German aircraft radio signals that controlled the glide bombs built to sink allied warships in the Mediterranean Sea. The knowledge helped NRL's Special Projects Section develop intercept-jammers that defeated the Henschel 293 system. The unwitting Luftwaffe engineers concluded that RF energy was too fickle or pilots too inept to make the intricate control mechanism work as designed. Howard's friendship with Louis W. Tordella (future Deputy Director, NSA) started when the Navy lieutenant came to his laboratory to learn how the successful intercept system had worked.

To thwart enemy jamming and eavesdropping, Lorenzen engineered a selectable 880-channel UHF voice receiving and transmitting system. He coordinated U.S./British UHF efforts and became well acquainted with the men who focused on radio and radar in the British Admiralty's Signal and Radar Establishment in Portsmouth, England and their counterparts in the Royal Air Force and Royal Army. (His UHF transceiver transitioned to Raytheon, AN/URC-3, and was later mass-produced as the AN/ARC-27 for use during the Korean War by U.S. and allied forces.)

By war's end, Howard Lorenzen supervised 11 small groups working in different areas of radio engineering. He was a key member of a countermeasures partnership between NRL and the Office of Naval Intelligence (ONI) in the Pentagon, which interfaced with NDRC Division 14 (Countermeasures) and industry. Sponsorship for NRL's intercept, direction-finding, radar-jamming, and decoy systems came from the Navy Bureaus of Ships and Yards & Docks which also shepherded transition to industry for systems produced in quantity.

Post War

After the war when many Navy scientists and engineers returned to their civilian occupations, Howard Lorenzen and some key members of his groups remained government employees and concentrated on captured electronic equipment sent to the United States from Europe and Asia. He and Jim Trexler arranged to permanently borrow certain other gems that fell into British hands such as the Athos crystal video receiver and the Wullenweber goniometer. The WWII NDRC evolved to the Office of Scientific Research and Development (later Joint Research and Development Board), and Lorenzen maintained his military and industrial contacts. He represented the Navy to joint and allied committees and working groups with interests in countermeasures -- particularly in the U.S., Canada, and Great Britain. He underwent nuclear orientation at facilities in New Mexico in 1948 and became an ECCM consultant to NATO's Mutual Weapons Development Program.

When the Soviet Union emerged as a new threat, Ed Speakman and Howard Lorenzen organized NRL's Countermeasures Branch and initially focused on Russian adaptations of western lend-lease equipment. Lorenzen's prior investigations of German equipment and documents soon paid off for Soviet adaptations of German technology and techniques began to appear. The Branch developed intercept and DF systems for deployment to Navy ships, shore stations, and aircraft. A collaboration with Airborne Instruments Laboratory (Dr. Hector R. Skifter) yielded the first tunable airborne microwave intercept receiver (later AN/APR-9), which was integrated at NRL with James E. Gall's ECM signal display and analysis equipment, including multi-gun CRTs. The new capabilities enabled the outfitting of naval land-based ferret probes along the periphery of the Sino-Soviet Bloc. Their first airborne systems were installed on P4M-1Q Mercators, PB4Y-2 Privateers, and P2V Neptunes. Other versions were tailored to surface ships and submarines. To get operational feedback on the UHF signal environment and the systems' usefulness and shortcomings, Lorenzen undertook an unofficial program to analyze intercept operators' logs at NRL. Initially, most of this analytical work was performed by himself and Robert D. Misner. By 1949, in cooperation with the Stromberg-Carlson Company, they had created the first U.S. magnetic tape recorder for intercept work -- the Radio-Countermeasures Sound Recorder-Reproducer IC/VRT-7.

National ELINT Program

To broaden participation in NRL's log analysis effort, Lorenzen promoted in ONI and helped organize a Countermeasures Intercept Analysis Group with sponsorship from the Joint Communication and Electronics Committee (JCEC) of the JRDB. Participating organizations were ONI, NRL, Naval Security Group (NSG), Army Signal Corps, and Air Technical Intelligence Center (ATIC). He chaired the JCEC analysis working group during the Korean War until it evolved in 1953 to the Army-Navy Electronic Evaluation Group (ANEEG), collocated with NSG Headquarters at 3801 Nebraska Avenue in Northwest Washington. By then his branch had added drum recorders to the arsenal of intercept equipment, and second-echelon analysis of raw data tapes had become a practical objective.

During the 1950s, ECM technology advanced by Lorenzen's engineers included electronic signals intercept, direction finding, jamming, and deception techniques. Howard Lorenzen and John C. Link supported BUAER and the Air Force Tactical ECM Wing in Biloxi, Mississippi with new forward-launch dispensers and lightweight chaff that worked. Lorenzen's team provided equipment (antennas, receivers, recorders, analysis devices), technical support, and technology transfer for various surveillance and reconnaissance platforms - via the Navy Bureaus of Ships and Aeronautics, the Army Signal Corps (ELINT vans), the CIA Office of ELINT (U-2 aircraft, crash boats, and agent devices), and ONI (covert installations and equipment loan to friendly foreign navies). Equipments were regularly upgraded to exploit new technology and keep pace with the threat signal environment as it spread into higher regions of the RF spectrum.

A National ELINT Program was established in 1955 under Air Force Secretary Donald A. Quarles, who had previously chaired the JCEC. ANEEG was reorganized as the National Technical Processing Center (NTPC). Soldiers, sailors, and civilian technicians (including some from CIA) were joined by an influx of airmen and civilians from ATIC at Wright Patterson AFB, Dayton, Ohio. NTPC's primary mission was to inform the Strategic Air Command about Soviet air defense radar that SAC's B-47 and B-52 bombers would encounter in the event of nuclear war. Lorenzen's Countermeasures Branch participated in and supported the National ELINT Program by serving on technical committees; developing intercept equipment; collaborating with Army Signal Corps Signals Research and Development Laboratory in Fort Monmouth, New Jersey and Air Development Centers in Rome, New York and Dayton, Ohio; and evaluating data acquired from ECM configurations. NTPC was headed by an Air Force lieutenant colonel, and continuity was provided by senior civilian engineers like John E. Libbert and Henry F. (Hank) DeCourt. Lorenzen continued to chair the steering committee of the Navy's Technical ELINT Panel which supported NTPC.

Jim Trexler was Lorenzen's project engineer for PAMOR (PASSive MOon Relay, a.k.a. "Moon Bounce"), which collected interior Soviet electronics and communication signals reflected from the Moon. It was Trexler who first started calling Howard "Father", and he also led the way to Communications Moon Relay which established operational communication circuits between Washington and Hawaii in the mid-1950s and set the stage for communication satellites in the 1960s. Charles W. Price -- the Branch's chief mechanical engineer -- designed the mechanical structures employed in several generations of Trexler's massive Moon Bounce antennas.

When ELINT was assimilated under the charter of the National Security Agency in March 1959, Lorenzen provided technical and engineering support to the Advanced Signals Analysis Division of NSA's Office of Collection and Signals Analysis and successor offices (C-1 and K-4), headed by John Libbert and, in the latter 1960s, by Raymond B. Potts. GCHQ mathematician Sylvester Stanley Strong worked there, too. Stan was a Lorenzen friend from the post WWII years.

Communications and radar intercept systems on aircraft, ships, submarines, and shore stations captured signals near the periphery of the Soviet Union and Communist China. Project Boresight created a global Navy network of HFDF stations to intercept and fix the source of radio transmissions from Soviet surface ships and submarines. Boresight successor Bullseye's huge circularly disposed antenna arrays included up to 2 rings of dipole antennas and 2 reflector screens and grew to an effective diameter of 800 feet. The first of the CDAA arrays -- installed at the Hybla Valley Coast Guard Station, Alexandria, Virginia in 1957 -- was used to track the Soviet Sputnik's 20 MHz signal and determine its orbit. Mack J. Sheets was Lorenzen's antenna engineer for Boresight and Bullseye; Bob Misner, signal processing.

Serving Operational Needs

Typical of the way Howard Lorenzen operated was the overnight revolution in the way his branch supported the silent service. Starting with the *USS Pike* in July 1944, all of NRL's submarine intercept and DF systems had located the intercept antennas on the conning tower, necessitating exposure and attendant risk when collecting signals. In December 1957, Rear Admiral Elton W. Grenfell -- a submariner and mechanical engineer -- came in from the Pacific Fleet where he commanded U.S. submarine forces and complained to Howard Lorenzen that he had written his last memorandum to BUSHIPS. He wanted hardware - now!

Lorenzen summoned several engineers and technicians to a brainstorming session in his office. Submariner Grenfell accepted Ralph A. Carpenter's proposed communications intercept configuration (15 KHz-265 MHz) and Reid Mayo's ELINT amplifier and crystal video detector (2.5-12 GHz), but

rejected all of his ELINT antenna offerings as too large and grotesque -- which stimulated William Edgar Withrow's design of a double-armed spiral antenna, not much bigger than a silver dollar, to fit inside a periscope.

A month later following integration and testing at Kolmorgen Optical, Inc. in North Hampton, Massachusetts, Kolmorgen's modified periscope (type 8A) and NRL's intercept equipment were installed on the *USS Dogfish* in New London, Connecticut to support its mid-January deployment to the Barents Sea. Reid Mayo and Ed Withrow observed the installation, tested the ECM system, participated in sea trials, and trained operators. They did the same in February for a second system on the *USS Wahoo* in Yokusaka, Japan. Wahoo would operate in the western Pacific. The full-production system (AN/BLR-6) transitioned to industry in June, just 6 months after Rear Admiral Grenfell pounded the table.

GRAB

When Russian Sputniks and Lunas, Army Explorers, and Navy Vanguard's began orbiting the Earth a dozen-or-so times daily, Lorenzen was already so renowned in ECM that the Director of Naval Intelligence -- Rear Admiral Laurence H. Frost -- forbade him from attending the launch of GRAB-1 or follow-on missions for fear his presence would give away their ELINT mission.

Lorenzen's vision for a low-cost ELINT satellite was first published late in 1957, a section of the NRL's secret proposal for a U.S. satellite and space vehicle program beyond Vanguard. The space agenda proposed by Navy to the Armed Forces Policy Council defined military and scientific objectives that would later be parceled out, respectively, to DoD's Advanced Research Projects Agency (ARPA), when it was formed February 1958, and to the NASA which became operational October 1958 and assimilated NRL's Vanguard team.

Lorenzen hoped for -- but could not count on -- DoD to promptly approve and fund his proposal. So he persuaded BUAER to task NRL for an intercept system -- subminiaturized and lightweight -- to be installed on "supersonic vehicles, manned or unmanned" and to automatically retransmit intercepted data to existing naval receiving stations on the periphery of the Communist Bloc. The task was on NRL's books before ARPA's new bureaucrats could find their way around the Pentagon, and Lorenzen's first quarterly engineering progress report was submitted to BUAER in July 1958. If anyone objected, then the supersonic vehicle was a high-performance jet aircraft; otherwise, a satellite.

Enough Vanguard veterans stayed with the NRL to build the GRAB satellite and later form a new Satellite Techniques Branch under Martin J. Votaw. Lorenzen's own Countermeasures Branch designed the ELINT payload, ground electronics, and transportable equipment shelters for ELINT stations overseas. Beyond imparting the vision and addressing technical problems raised to his level, he entrusted design and engineering to a loosely coupled team of engineers and technicians supporting project engineer Reid Mayo. "Don't worry about money," he told Mayo. "I'll get you the money. Just don't let me down."

While their work progressed, Lorenzen led a parallel campaign to get the project approved and fully funded. Starting with ONI in July 1958 using large briefing boards mounted on a pedestal, Project Tattletale was sold at the Pentagon, Main Navy on Constitution Avenue, CIA Headquarters, NASA, and Capitol Hill. Congressmen were delighted to be briefed on a project "not costing tens of millions." In the spring of 1959, DNI Laurence Frost, ARPA Director Roy W. Johnson, and OSD's Graves B. Erskine (Special Operations) agreed that Tattletale was too visible, though. ARPA officially killed it and established Project Canes as a top-secret security control system. Oaths were signed by those authorized

for indoctrination -- under 200 people altogether -- with President Dwight D. Eisenhower heading the list.

The President approved Project Canes on August 24, 1959. On May 5, 1960 -- just 4 days after a CIA high-altitude U-2 reconnaissance aircraft was brought down over the Soviet Union (by an SA-2 Guideline missile, according to Soviet claims) -- he approved launch of the first Canes-controlled satellite. Known operationally as GRAB (Galactic RAdiation and Background), the ELINT satellite was launched successfully from Cape Canaveral, Florida on June 22, 1960 and tested by NRL in Hawaii July 5-8. Howard Lorenzen, Reid Mayo, Ed Withrow, Edgar L. Dix, and Vincent S. Rose were the first to hear the medley of radar signals detectable by a wide-open receiver in outer space. On the recommendation of State, Defense, and CIA, President Eisenhower authorized NRL to "trigger Project Canes on 12-15 passes over the Soviet Union during the course of a two- to four-weeks period of time," subject to a final phone-check with State, CIA, and the White House chief of staff before each turn-on. The Canes/GRAB ELINT tapes soon saturated U.S. analytical capabilities. On October 18, 1960, President Eisenhower approved the request from State, Defense, and CIA for more Navy ELINT satellites.

Bob Misner led the way to machine processing of the GRAB take. Howard Lorenzen collaborated with Lou Tordella on a joint NRL/NSA effort to automate ELINT data processing. Intelligence derived from GRAB satellites -- processed by the NSA and SAC -- marked a turning point in U.S. strategic doctrine. The Soviet air defense system was too robust for penetration by SAC's high and medium altitude bombers which were succeeded by low altitude bombers and ballistic missiles, sea and land versions. (An air-launched version, Skybolt, was scrapped in the mid '60s.) GRAB also yielded the first intercept of a signal associated with the developmental Soviet Galosh anti-ballistic missile system.

While the National Reconnaissance Office (NRO) was being formed, Bill Howe looked out for Navy interests on behalf of ONI. He had high level help from the DIRNSA, Vice Admiral Laurence Frost, who wanted to continue the Navy ELINT satellites in the National Reconnaissance Program. (Lorenzen was then busy helping to save the Navy aircraft carrier by deception techniques that foiled U.S. Air Force simulated air attacks while President Kennedy monitored the exercise.) Project GRAB was assimilated in the NRO in July 1962.

EW Division

Lorenzen became NRL's first Superintendent of Electronic Warfare when his branch was upgraded to division status in September 1966. He typically managed upwards of a hundred scientists, engineers, and technicians who were kept busy pushing the state of the art, sharing their technology and ideas with cleared firms, and harvesting any useful components produced by industry. Their achievements under his leadership spanned the entire breadth of EW. During the late 1960s, his highest priority was the development of equipment for naval aircraft, particularly for defense against guided missiles which he considered the Laboratory's most vital support to U.S. Navy aviators at war in Vietnam. When the battleship *USS New Jersey* was refitted in 1968 to shell enemy supply routes inland, Lorenzen's EW Division equipped her with every device in their arsenal that could foil attack from North Vietnamese MIGs, missiles, or fast attack boats. He so overweighted his defensive EW branch that it emerged as a separate division under Lyn Cosby within a few years.

Lorenzen had the status of a presidential appointee (under Public Law 313) and he was as comfortable in acquisition management and operations circles as in science, engineering, and manufacture. When need be, he explained his projects on Capitol Hill, the Pentagon, the United States Intelligence Board, the President's Scientific Advisory Committee, the Bureau of the Budget, the General Services Administration, and the intelligence agencies (CIA, DIA, NSA, and NRO).

Throughout the 1960s, he helped represent the United States at NATO and SEATO EW conferences and served as an advisor to the Joint Chiefs of Staff and the Secretary of Defense. When he was written up for an award, someone went through his personnel record and figured that he had spent nearly 10 percent of his career since WW II abroad -- 78 trips to 17 different foreign countries.

On February 11, 1970, the interagency ELINT RDT&E Coordinating Group met at NRL. NSA's David Wolfand chaired the ERG, but Howard Lorenzen was recognized as leader by most of the members. These captains of ELINT included Army Security Agency Chief Scientist Ed Speakman, Army ACSI Senior Technical Adviser Bill Howe, CNO Development Technical Director Stirling Thrift, NSA R&D Directorate's Robert J. Hermann, Air Force Colonel John B. Marks, NSA's Art Thom, Army Missile Intelligence Directorate's Mel Bachman, and representatives from CIA and DIA. Lorenzen's scientists and engineers displayed and discussed their latest technologies for every sort of platform: surface ships, submarines, early warning (VQ) and long range patrol (VP) aircraft, transportable equipment vans (land and sea), SIGINT stations, satellites, and even human beings (e.g., crystal video receivers appearing to be eyeglasses and hearing aids). It was a most productive session and reunion of old friends and pioneers in ELINT. No one guessed that Howard Lorenzen would soon embark on a new mission.

Space Systems Division

In September 1970, Deputy Defense Secretary David Packard aligned space systems acquisition responsibilities with those for weapon systems acquisition and authorized the military departments to pursue departmental need for space systems, including "unique surveillance (i.e., ocean or battlefield) needs" (DoDD 5160.32, Development of Space Systems). The Navy established a Navy Space Project Office (PM-16) in the Naval Material Command as a successor to NAVAIR's Astronautics Division (Air 538). NRL had been Air 538's prime space engineering asset. SOLRAD supported NASA's Apollo Program by monitoring solar radiation and predicting Sun activity that could interfere with communications during Moon missions. TIMATION (forerunner of GPS) provided time transfer and navigation data via satellite to mobile platforms. SURCAL calibrated the Navy Space Surveillance (NAVSPASUR) CW fence across and above the southern United States. CALSPHERE calibrated the Navy's Bullseye HFDF system. A classified program supported national capabilities. Wanted were Navy space systems for communications, ocean surveillance, and global positioning. At NRL, the satellite platform now seemed to eclipse the electronic warfare mission and would become the basis for a new, first-echelon Space Science and Technology area.

NRL turned to Howard Lorenzen to repeat in space what he had accomplished in EW: design total systems for military operational support. In February 1971, Lorenzen was appointed Superintendent of Space Systems and organized a new division that consolidated moon relay SIGINT and most of NRL's on-going space projects, including fabrication of lightweight satellite platforms by Peter G. Wilhelm's Satellite Techniques Branch and development of payload electronics and ground readout systems for communications, ELINT, time & navigation, ocean surveillance, and scientific experiments. Some of the former EW branches became a new Tactical EW Division under Lyn Cosby, specializing in systems for Navy ships and aircraft. Those specializing in HFDF systems at naval shore stations joined an expanded Communications Sciences Division, soon assigned to Bruce Wald.

Presiding over the southeast end of NRL's main mall near the front gate, Howard stayed in touch with his EW and SIGINT colleagues in buildings across the street even as he concentrated on tactical applications of space technology. His advocacy of space systems was motivated not only by cost-effectiveness, but also by Navy losses, since 1950, of 78 killed or missing and eight wounded crewmen by Sino-Soviet destruction of 8 naval reconnaissance aircraft, most of which had been outfitted by his team. Within 2 years, NRL's major space R&D projects were destined for operational systems

development and management by Navy or Air Force systems commands as joint or national programs. (The Space Systems Division underwent several transformations and expansions in the next 3 decades and is now the Naval Center for Space Technology, directed by Pete Wilhelm.)

His missions accomplished, Lorenzen retired in June 1973 at the peak of his intellectual power and capabilities but suffering increasingly from an adulthood affliction of Meniere's disease -- particularly vertigo. He and wife Etta Mae moved to Bellevue, Washington in 1976 to be near their daughter Susan A. Black's family.

There, he enjoyed family and new friends; built the amateur radio station of his dreams; welcomed visits from old friends from back East; and gardened some. Physical infirmities prevented Howard from participating in NRL's Diamond Jubilee in June 1998 and the initial public disclosure of Project GRAB by DNRO Keith R. Hall. But he enjoyed video tapes of those proceedings attended by GRAB alumni. He is survived by Etta Mae, Susan, grandsons Timothy and Dale, three great grandchildren, and dozens of electronic warfare and space systems he pushed into operational use (many of them still classified) that have evolved and will continue to support the Nation in the year 2000 and well beyond.

X. Abelson's memoir on Ross Gunn



Ross Gunn

May 12, 1897–October 15, 1966

by Philip H. Abelson

Ross Gunn was one of the most versatile physicists of the early and mid-20th century. He made significant contributions to knowledge in many fields of science and technology. He created novel instrumentation, much of which was designed to facilitate studies of natural phenomena such as thunderstorms. In the course of his career, he obtained more than 40 patents.

From 1927 to 1947, Gunn was a research physicist on the staff of the U.S. Naval Research Laboratory. In 1934, he was appointed technical adviser for the entire laboratory. In that role, he interacted with important naval personnel. In March 1939, he wrote a memorandum to Admiral H.G. Bowen, chief of the Navy's Bureau of Ships, outlining the tremendous advantages that could be expected from the use of atomic energy in submarine propulsion.

In the latter years of World War II, Gunn was simultaneously superintendent of the Mechanics and Electricity Division, superintendent of the Aircraft Electrical Division, and technical director of the Army-Navy Precipitation Static Project as well as technical adviser to the naval administration. He also fostered development of the liquid thermal diffusion method for separation of Uranium isotopes. This led to large-scale use of the process by the U.S. Army's Manhattan District at Oak Ridge, Tennessee.

In February 1947, Gunn became director of the Weather Bureau's Physical Research Division where for 10 years he conducted and supervised important research related to severe weather phenomena. Until his death in 1966, he remained active in research and consultation while a professor of physics at American University.

Ross Gunn was born in Cleveland, Ohio on May 12, 1897. His forebears were of Scotch and English descent. Three of his ancestors were soldiers in the American Revolution; two served as officers directly under George Washington. His father Ross D. A. Gunn was a graduate of Western Reserve Medical College and a practicing physician. As undergraduates, both his father and mother (Lora A. Conner) attended Waynesburg College in Pennsylvania.

In 1923, Ross married Gladys J. Rowley, an alumna of Oberlin College. Over the next 15 years, 4 sons were born -- Ross, Jr., Andrew Leigh, Charles, and Robert Burns. All have had successful

professional careers. Ross Gunn, Jr. -- who holds a degree in electrical engineering and an MBA -- is in business in California. Rev. Andrew Leigh Gunn attended Yale Divinity School and is a minister. Charles Gunn is an aeronautical engineer and at one time was director of the NASA shuttle program. He is now engaged in private enterprise. Robert Burns Gunn is currently professor of physiology and chairman of the Department of Physiology at the Emory University School of Medicine.

While in high school in Oberlin, Ohio, Gunn became interested in amateur radio (then called "wireless telegraphy"). Without help, he built a successful wireless receiving apparatus and qualified for a commercial wireless operator's license. He also built one of the first long-range amateur wireless stations in northern Ohio and carried on conversations with amateur stations in most regions of the United States. These early activities in radio are reminiscent of the youthful interests of other physicists including Ernest O. Lawrence and Merle A. Tuve.

Ross entered Oberlin College in 1915. But after 2 years he transferred to the University of Michigan. With the entry of the United States into World War I, he enlisted as a private in the Signal Corps and was later called to active duty. He received his B.S. degree in electrical engineering in 1920 and an M.S. degree in physics in 1921 from the University of Michigan.

In the interval from 1921 to 1923, Gunn spent 1-1/2 years with the U.S. Air Service as a radio research engineer. As part of his duties, he did pioneer work in developing a radio range aircraft navigation system. In the course of this work, he made a number of the first cross-country instrument flights. While employed in the U.S. Air Service, he also developed devices for radio control of pilotless airplanes (drones). The Navy later used this technology as the master control mechanism for 50 of its first pilotless aircraft.

The years from 1923 to 1927 were spent at Yale University where Gunn held an appointment as instructor in engineering physics and where he received a Ph.D. degree in physics in 1926. One of his mentors at Yale was Professor Leigh Page, a theoretical physicist. A consequence was a good grounding in classical physics.

In his later career, Gunn combined excellent capabilities in identification of important problems with skill in developing innovative instrumentation, a zest for experimental work, and aptitude for theoretical analysis of practical problems.

In 1927 Gunn accepted an offer from the Naval Research Laboratory to become a research physicist in the Radio Division. He intended to spend only a few years at the laboratory, but he remained there until 1947. In the pre-War years, the civilian staff was small and the naval officer management was willing to encourage pioneering basic research related to radios, the new electronics, and instrumentation employing vacuum tubes. Gunn was skilled in these areas and he interacted well with naval personnel. Within a year he was promoted to assistant superintendent of the Heat and Light Division. He was allowed to choose his own agenda.

During the period 1929-33, Gunn published 28 articles in the open literature. Most of the items were theoretical treatments of natural phenomena such as terrestrial and solar magnetism, cosmic rays, and other astrophysical phenomena. 13 of the articles were published in *Physical Review*. The remainder appeared in other standard journals. During this highly productive period, Gunn invented and was subsequently granted 17 patents on useful instrumentation. One device was an induction-type electrometer that could produce an induced alternating voltage from a small free charge. The basic principle was incorporated in a large number of instruments including the vibrating reed electrometer. In addition to these activities, Gunn conducted classified research relevant to naval problems.

In 1934 Gunn was appointed technical adviser for the entire Naval Research Laboratory. He became responsible for the quality of the technical program and its coordination with the needs of the naval service. He took this top administrative and scientific job with the understanding that he would be given skilled assistance and that he would be allowed to continue his own research on problems of interest to the Navy.

In 1938 Gunn invented and subsequently patented another instrument that was widely used -- a portable device that amplified thermocouple electromotive forces. The instrument was useful in detecting infrared radiation emitted by enemy ships and aircraft.

During the World War II years, Gunn was assigned many administrative duties in addition to his role as technical adviser to the naval administration. One of them was to act as technical director of the Army-Navy Precipitation Static Project. This was a successful effort to identify and alleviate interference produced on aircraft flying through ice-crystal clouds or snow. A group headquartered in Minneapolis conducted the major part of the investigation.

Immediately after the announcement of the discovery of uranium fission in early 1939, Ross Gunn became a keen observer of and participant in developments relevant to nuclear power. He was particularly interested in its possible application to propulsion of submarines. Conventional submarines were propelled by batteries, which in turn were charged by electricity supplied by generators coupled to diesel engines. These required air. While near the surface of the ocean, the submarines were vulnerable to detection and attack.

By mid-1940 it had become evident that the rare ^{235}U was fissionable and that a chain reaction creating nuclear power was likely to be achieved. Gunn learned that I was conducting experiments on uranium isotope separation and arranged to provide me with financial support. I was then an employee of the Carnegie Institution of Washington. I obtained my first tiny isotope separation using equipment manufactured by me but housed at the National Bureau of Standards.

The method involved liquid thermal diffusion of uranium hexafluoride (UF_6). The simple apparatus consisted mainly of 3 concentric tubes 12 feet long. The inner tube was heated by steam. A second tube was maintained at 65°C . The 3rd tube served to contain the 65°C cooling water. The UF_6 occupied the space between the walls of the inner and middle tubes. Runs on this column were made in April 1941 when a measurable isotope separation was obtained.

When Gunn learned that I had achieved a small separation of uranium isotopes, he invited me to join the staff of the Naval Research Laboratory where enhanced supplies of high-pressure steam could be made available. In June 1941, the move was made. A series of experiments was conducted to determine the optimum spacing between the hot and cool walls. In June 1942, a column 36 feet long heated by 100 psi of steam produced an isotope separation factor of 1.11. This success led to an expanded effort that included authorization to build and operate 14 columns 48 feet long. It also led to the procurement of a propane-fired boiler capable of delivering $1,000\text{ lb/in}^2$ of steam. For a time, the facility at the Naval Research Laboratory was the World's most successful separator of uranium isotopes.

Ross Gunn -- who was a member of the federal government's S-1 uranium committee -- communicated results of the isotope experiments to committee chairman Lyman J. Briggs in August 1942. This led in October 1942 to a visit to the Naval Research Laboratory by General Leslie R. Groves and Admiral W. R. Purnell. Later in January 1943, a special committee assembled by the Manhattan District inspected the installation. The committee was impressed by the simplicity of the equipment and commented favorably.

A Naval Research Laboratory report submitted to the Bureau of Ships by Gunn in January 1943 pointed to the advantages of using enriched uranium in nuclear reactors. It would be a necessary step in creating a nuclear-powered submarine. The report also stated, "A liquid thermal diffusion plant costing 1-to-2 million dollars could provide the necessary separated isotopes."

During the next 6 months, improvements were made in the construction of the separation columns. At the same time, the pilot plant produced 236 pounds of UF₆ possessing isotope separation. The quantity and the separation were greater than had been obtained by the gaseous diffusion method at that time.

Gunn decided that an expansion of production capabilities of the liquid thermal diffusion method was warranted. Doing so would provide an alternative if the Manhattan District's magnetic and gaseous diffusion methods failed. A survey of naval establishments showed that large-scale sources of high-pressure steam could be made available at the Naval Boiler and Turbine Laboratory at the Philadelphia naval base. Authorization to build a 306-unit plant at Philadelphia was obtained on November 27, 1943. Rear Admiral Earle Mills -- assistant chief of the Bureau of Ships -- signed the project order.

In June 1944, the Philadelphia plant was approaching completion. J. Robert Oppenheimer learned of the progress and recognized that a supply of partially-separated uranium would increase the production of an electromagnetic plant at Oak Ridge. He communicated with General Groves who sent a reviewing committee to the Philadelphia plant. Its report was favorable and led to the decision to build a 2,142-column plant at Oak Ridge. Construction there was rapid. The \$20-million facility achieved production that shortened the duration of World War II by 8 days.

Secretary of the Navy James Forrestal presented the Navy Distinguished Civilian Service Award to Ross Gunn on September 4, 1945. The citation included:

For exceptionally distinguished service to the United States Navy in the field of scientific research and in particular by reason of his outstanding contribution to the development of the atomic bomb ... For his untiring devotion to this most urgent project, Dr. Gunn has distinguished himself in a manner richly deserving of the Navy's highest civilian award.

Immediately after the end of the War, Gunn returned to the concept of the nuclear submarine. Methods of detecting diesel-powered submarines had advanced greatly. In the latter part of World War II, large numbers of German submarines had been destroyed. I was tasked with becoming familiar with the current state of nuclear reactors -- particularly those using enriched uranium. I was provided with access to experimental programs at Oak Ridge and Argonne, and I participated in criticality experiments of enriched uranium assemblages.

Gunn also took part in obtaining blueprints of the most advanced German submarine. Analysis showed that the energy system of the submarine could be replaced by a shielded nuclear reactor. In September 1946, a report on the feasibility of a nuclear submarine was submitted to the Bureau of Ships. Later, Admiral Hyman Rickover directed a highly-successful development and construction of nuclear submarines. However, some part of the credit for nuclear submarines belongs to Ross Gunn.

In the late autumn of 1946, Gunn decided he would not accept additional naval administration duties. Rather, he would return to more science-oriented activities. In February 1947 he transferred to the Weather Bureau to organize and direct a fundamental study of the basic physics of weather. A core objective was to investigate the processes responsible for precipitation under various physical conditions.

His first task as director of the Weather Bureau's Physical Research Division was to organize a program to study the practicality of producing rain by cloud-seeding. Results showed that while sometimes rain was produced, it was insufficient to be of much economic value. Subsequent events showed this early conclusion to be correct.

Physicists who are inclined to observe and study natural phenomena have been presented with great opportunities and puzzles. Among these are solar-terrestrial climatic effects, possible human-induced global warming, and violent weather phenomena. As early as 1935, Gunn became sufficiently interested in thunderstorms to begin studies on them. A paper titled "The Electricity of Rain and Thunderstorms" was published in *Terrestrial Magnetism and Atmospheric Electricity*.

In 1944 after being named technical director of the Army-Navy Precipitation Static Project, Gunn participated actively in choosing instrumentation for it and in devising research and instrumentation aspects. Later he analyzed many of the experimental results. In the course of the program, airplanes flew through 25 thunderstorms collecting valuable data. These measurements provided what was then and later the best available cross section of thunderstorm electrification data. The airplanes were equipped with induction-type electric field meters placed on both the top and bottom of the main cabin. An apparatus capable of measuring the electric charge on snow and raindrops was installed under one wing. Simultaneous measurements could be made on the electric fields and on the charges on drops.

Repeated flights through active thunderstorms showed that the electric fields at levels close to ground were of the order of 1-to-10 volts/cm. These fields generally increased to a maximum in the vicinity of the freezing level. At this point, the electric fields frequently exceeded 1,000 volts/cm. The aircraft encountered both negative and positive fields of 2,000 volts/cm and more. The charges on snow and raindrops were largest when the electric fields were high. The potential differences between the top and bottom of a thundercloud were frequently greater than 108 volts.

In 1947 soon after Gunn joined the U.S. Weather Service, he began to devise experiments and new equipment aimed at obtaining better knowledge of the basic physics of weather phenomena. His flair for the development of new equipment was repeatedly evident. An early example was an unprecedentedly huge cloud chamber. A mining shaft in Arizona 700 feet long and 7 feet square was sealed and provided with means to humidify and compress the air within it. When the pressure was suddenly released a dense cloud formed throughout the chamber. If water drops of known size were released at the top of the shaft, their growth as they passed through the cloud could be measured.

In another set of experiments performed with different equipment, the terminal velocity of various sizes of water drops was determined. The extensive data obtained from these studies were a unique contribution. The data continue to be widely used and quoted.

From the instrument development efforts emerged electric field meters capable of operating continuously in very heavy rain whether on the ground or on aircraft. Instruments were also created to determine the sign and magnitude of free charges carried on falling rain. About these and other activities aimed at developing instrumentation, Gunn could state, "As a direct result of efforts to develop new and better instruments, we have the largest store of coherent measurements yet made in the field of atmospheric electricity."

As a fellow of the Institute of Radio Engineers, Gunn was invited to write an article that appeared in 1957 describing recent developments in an important field of his choosing. He chose to present some of the knowledge that had been created about thunderstorms. I have selected a few items from the article to paraphrase and summarize:

- Cosmic rays and radioactivity produce at heights just above ground level about 10 positive and 10 negative ions/sec. At an altitude of 15 km, the rate of production is about 45 pairs/sec.
- In a cloud that is not yielding rain the net overall charge is zero. However, droplets in the cloud become charged. About half are positive and about half are negative.
- Charges on raindrops may become enhanced when small drops join together to make large drops. When there is turbulent motion in the cloud, the relative mobility of plus and minus ions results in differential charging and in separation of charged droplets. The electrification observed in thunderstorms implies a gross separation of free electrical charges with a consequent expenditure of large amounts of energy.
- An important index of thunderstorm activity is the electric field measured both at ground level and inside an active cloud high above. In fair weather, the surface field intensity is negative and of the order of -1.5 volts/cm. In a typical thunderstorm the electric field at the ground may increase to +/- 100 volts/cm and more. The field may be negative part of the time, but mostly it is positive. The field changes instantaneously during and immediately after a lightning strike and then recovers. Often the direction of the field overshoots during the strike.
- The typical summer thunderstorm is about 20 km in diameter. The cloud mass itself may be somewhat larger. It commonly extends vertically about 12 km, but occasionally can extend to 15 km.
- Normal lightning activity is not observed in clear air except in the vicinity of falling precipitation. The principal electrical effects accompanying a thunderstorm are closely related to the production and fall of precipitation, but the connection of lightning and precipitation is not a direct one.

From 1947 until his death in 1966, Gunn devoted most of his efforts to the study of atmospheric phenomena. He created improved measuring equipment on which he was granted about 30 patents. He directed research while also conducting measurements. A substantial part of his effort was devoted to theoretical analysis of results obtained by him and others. He also published about 40 articles in the scientific literature. He was the sole author on most of them.

After he left the Weather Bureau in 1957, Gunn's rate of publishing diminished. Much of his time was spent as a consultant. However, in the last 4 years of his life, he returned to active experimental work and the development of instrumentation. The core of his efforts related to aspects of the physical phenomena occurring in thunderstorms.

His professional office was located at American University where he was a research professor of physics. One of his last publications was an extensive invited article in *Science* which described a new instrument for studying effects of collisions between simulated raindrops.

In his approach to research, Gunn followed a procedure that many of the most successful scientists follow. He identified an important phenomenon that involved potentially measurable reproducible effects. He devised or procured instrumentation that would measure the effects more reliably than they had been previously. He analyzed the data using tools of a classical theoretical physicist but with the attitude of a practicing engineer. Others have followed and will follow in his footsteps. Some of his

data and analyses will be improved on, but in many instances it will be noted that he was the first explorer to view the new frontier.

After his death, an issue of the *Monthly Weather Review* -- published by the U.S. Department of Commerce -- was assembled as a memorial to Ross Gunn. It contained 23 articles, most of which dealt with violent weather phenomena. An exception was a biographical sketch portraying some of Gunn's character traits. Prepared by F. W. Reichelderfer, a member of the National Academy of Sciences, the article was titled, "Ross Gunn, the Scientist and the Individual." Reichelderfer chose to quote a portion of a talk Gunn had given in 1938 in which he described the ideal research physicist. Reichelderfer states that the words quoted reflected the standards set by Gunn as a scientist:

The scientist should be distinguished by intelligence and firm grounding in the fundamentals of physics, chemistry, and engineering. He should be especially keen in estimating situations and reaching sound decisions. His judgment and perspective should be such that he can give his talents systematic direction. He should be an original thinker ... exceptional in his ability to plan, think, and do things without being told. He should have the courage of his convictions, yet not be blinded by them. He should constantly seek the truth. He should be especially successful in working harmoniously with others toward a common end.

In my dealings with Ross Gunn, I noted that in a situation where he was certain of the facts, he did not avoid conflict, and he was resourceful when in a fight. Reichelderfer perceived a different side of Gunn's character. He stated:

Any man whose work comes to public attention and who holds to his beliefs when the facts support them encounters opponents as well as supporters, especially when his work may incidentally affect the ambitions of others. So Gunn had his critics -- this is rather well known. But he has strong support from associates who believe that most of the criticism directed at him was a result of misunderstanding, sometimes misrepresentation or ignorance of what he actually thought and did. Gunn's nature did not make him inclined to waste time in "explaining" to critics. He hoped the facts would speak for themselves and in such matters he preferred to remain silent.

During his life, in whatever role he found himself, Ross Gunn gave the best he could. As a result, his existence made the kind of difference to this world that only a few achieve.

Selected Bibliography

1930

On the anomalous rotation of the Sun. *Phys. Rev.* 35:635-42. A new frequency stabilized oscillator system. *Proc. Inst. Radio Eng.* 18:1560-74. 1932

A mechanically resonant transformer. *Proc. Inst. Radio Eng.* 20:516-619.

Principles of a new portable electrometer. *Phys. Rev.* 40:307-12.

On the evolutionary origin of the solar system. *J. Franklin Inst.* 213:639-59.

1935

The electricity of rain and thunderstorms. *Terrestr. Magnet. Atmos. Electr.* 40:79-106.

1938

Some experiments on the amplification of thermocouple electromotive forces. *Rev. Sci. Instrum.* 9:267-69.

1946

With R. G. Stimmel, E. H. Rogers, and F. E. Waterfall. Electrification of aircraft flying in precipitation areas. *Proc. Inst. Radio Eng.* 34:167-77.

With J. P. Parker. The high-voltage characteristics of aircraft in flight. *Proc. Inst. Radio Eng.* 34:241-47.

1947

The electrical charge on precipitation at various altitudes and its relation to thunderstorms. *Phys. Rev.* 71:181-86.

1948

Electric field intensity inside of natural clouds. *J. Appl. Phys.* 19:481-84.

1949

The free electrical charge on thunderstorm rain and its relation to droplet size. *J. Geophys. Res.* 54:57-63.

With G. D. Kinzer. Terminal velocity of fall of water droplets in stagnant air. *J. Meteorol.* 6:243-48.

1950

The free electrical charge on precipitation inside an active thunderstorm. *J. Geophys. Res.* 55:171-78.

1952

A vertical shaft for the production of thick artificial clouds and the study of precipitation mechanics. *J. Appl. Phys.* 23:1-5.

1954

Electric field meters. *Rev. Sci. Instrum.* 25:432-37.

Diffusion charging of atmospheric droplets by ions and the resulting combination coefficients. *J. Meteorol.* 11:339-47.

1955

The statistical electrification of aerosols by ionic diffusion. *J. Colloid Sci.* 10:107-19.

Droplet electrification processes and coagulation in stable and unstable clouds. *J. Meteorol.* 12:511.

Raindrop electrification by the association of randomly charged cloud droplets. *J. Meteorol.* 12:562-68.

1957

The electrification of precipitation and thunderstorms. *Proc. Inst. Radio Eng.* 45:1331-58.

1964

The secular increase in the world-wide fine particle pollution. *J. Atmos. Sci.* 21: 168-81.

1965

Collision characteristics of freely falling water drops. *Science* 150:695-701.

Thunderstorm electrification and raindrop collisions and disjunction in an electric field. *Science* 150:888-89.

1966

Thunderstorm electrification of hail and graupel by polar drizzle. *Science* 151:686-87.

Y. Ross Gunn and Nuclear Propulsion

A Global Forum for Naval Historical Scholarship
International Journal of Naval History
Volume 2 Number 1 April 2003.

“We had the hose turned on us!”

Ross Gunn and The Naval Research Laboratory’s Early Research into Nuclear Propulsion, 1939 – 1946.
Joseph – James Ahern,
American Philosophical Society Library

The United States Navy’s development of a nuclear-powered submarine is generally associated with Admiral Hyman Rickover’s post-World War II initiative. What many are unaware of is that the Navy’s research into the use of nuclear power **predates** Rickover’s work by almost 10 years and the creation of the Manhattan Engineering District by 3 years. Between 1939 and 1946, the Naval Research Laboratory (NRL) conducted research to determine the feasibility of using nuclear energy for submarine propulsion. During this time, Navy scientists developed methods for the production of Uranium hexafluoride and for isotope separation using liquid thermal diffusion. Both of these methods were vital to the production of Uranium-235 and were used in the creation of the atomic bomb.

However, the Navy’s research was carried out in an environment of isolation from and in competition with the Manhattan District. Ross Gunn -- with the support of the Naval Research Laboratory -- struggled with Manhattan to get the supplies the program needed and to show the potential of the research to the overall program. This paper argues that the **Navy** -- not the Army -- deserves credit for laying the groundwork for nuclear energy in the United States. Although the atomic bomb was built by the Manhattan Engineering District under General Leslie Groves, the little-known and nearly-suppressed story of the Navy’s prior work in this field gives credence to Dr. Ross Gunn’s claim that the Navy “got hosed”. How and why the Navy was cut out of nuclear research and how the story was ignored illuminates another side of the first military applications of nuclear energy.

The U.S. Navy’s interest in developing a nuclear-powered submarine originated in the separate quests to find an ideal means of submarine propulsion and a new power source for naval vessels in general. For fleet submarines, the important issue became finding the best means of propulsion to meet their mission requirements. The Navy adopted diesel-electric engines for submarine use in 1912 with the inherent limitation that the submarine had to carry both fuel and oxygen to operate when submerged, restricting its range and speed. Inside the Navy, Gunn was alarmed at the nation’s disappearing coal and oil reserves. To him, the Navy had an obvious interest in new forms of power given its position as one of the world’s largest consumers of petroleum [\[1\]](#).

During the early 1930s, NRL’s Mechanics and Electricity Division -- headed by Gunn -- was looking into new power plants for submarine and torpedo propulsion. The central limitation in all of the methods under consideration was providing an adequate oxygen source for propulsion that the submarine could carry with it, and a means of regeneration when running on the surface. The 1938 announcement and confirmation of German scientists Otto Hahn and Fritz Strassmann’s experiment to deliberately split Uranium atoms by bombarding them with neutrons accelerated scientific interest in atomic energy.

Gunn felt this was an answer to the submarine propulsion problem by simultaneously removing the oxygen problem and providing the submarine with a long cruising range. Gunn's division had numerous discussions about the application of the nuclear energy to naval problems and creating a tentative research program. However, Gunn's group decided not to present such a theoretical program to the no-nonsense Navy bureau chiefs until they had significant data to back it up [2].

While scientists at NRL had theorized about the use of nuclear energy, it was not until Enrico Fermi met with Navy representatives that nuclear energy research got underway. The meeting with Fermi took place on March 17 in the Navy Department building and was attended by representatives from the Navy's Bureaus of Engineering, Ordnance, and Construction and Repair, NRL, and the Army's Ordnance Department.

In a little over an hour, Fermi gave a briefing on the success of Hahn and Strassman, focused on the potential of an atomic bomb, and briefly discussed the possibility of using it as a power source. While Fermi came away feeling the meeting had yielded little, it in fact had an impact on the NRL representative (Gunn) by providing the evidence that he needed for his division to take their idea before the Bureau of Engineering. 3 days after the meeting, Gunn and Captain Hollis M. Cooley (director of the NRL) approached Admiral Harold G. Bowen (director of the Bureau of Engineering) with a request for \$1,500 to start Uranium research. When Gunn and Cooley left Bowen, they had their funding and within a week were conducting research. NRL's work began almost 7 months before President Franklin D. Roosevelt received Albert Einstein's famous letter about the potential for an atomic bomb [3].

As work began at NRL, there were a number of problems to solve. Physicist Neils Bohr had theorized that Uranium-235 would be an ideal source for a chain reaction. NRL needed to determine a method to separate that isotope from Uranium and develop a method for producing the chemicals needed for separation. At this early stage, the Navy was not focused on using fission as a weapon. In addition, he felt that the United States would not have a cause to use such a horrible weapon [4].

Before separation research could begin, NRL needed to find a method to supply adequate amounts of Uranium hexafluoride (UF_6 or "hex"). UF_6 was considered the principle material for use in isotope separation methods since its ability to exist in either a gaseous or liquid state allowed its use in the various methods under consideration. R.R. Miller of NRL's Chemistry Division and T.D. O'Brien of the University of Maryland began working in April 1939 on the production of UF_6 . The method they developed used a reaction of fluorine gas and a powdered Uranium-nickel alloy that proved to be expensive and time consuming [5].

While the Miller and O'Brien method allowed NRL to supply UF_6 for research, it was not able to produce adequate quantities to meet all research and production requirements because of the rarity of Uranium-nickel alloy. Physicist Philip Abelson at the Carnegie Institution of Washington required more than a kilogram --or 10 times this amount -- of UF_6 for his experiments. Abelson set out independently to find a method of UF_6 production that did not require the metal. He was able to devise a rather straightforward method using a common salt of Uranium that could inexpensively produce nearly a kilogram of Uranium hexafluoride per day by July 1941 [6]. Eventually production was moved from NRL to the Harshaw Chemical Company of Cleveland, OH.

The Navy next turned to the problem of isotope separation. A method was needed that could effectively separate U^{235} from Uranium on a manufacturing scale. NRL contracted research out to laboratories at some of the nation's top universities and research institutions including Columbia University, the University of Virginia, and the Carnegie Institution of Washington. Of the dozen methods initially proposed and researched, 4 methods (gaseous diffusion, ultra centrifuge, mass spectrograph, and liquid thermal diffusion) were developed to the point where they could be included

into a manufacturing plant. The program was financed by both the Navy's Bureau of Ordnance and Bureau of Ships and with Army Ordnance with the work coordinated by NRL [7].

Lyman J. Briggs -- director of the National Bureau of Standards and chair of the Uranium Committee -- recommended to Bowen that NRL enter into a contract with the Carnegie Institution to support Abelson's research. The basis of the method is that lighter isotopes have the tendency to diffuse to a hotter area, where as heavier isotopes diffuse towards cooler areas. As such, the Uranium-235 enriched material would move to the top of a column where it could be collected. Abelson began by building and testing a few basic columns at the Carnegie Institution, which proved successful and encouraged further research. He formally suggested using liquid thermal diffusion in September 1940. Eventually an arrangement was reached where Carnegie Institution was paying Abelson's salary, NRL was furnishing the equipment, and the Bureau of Standards was providing laboratory space and a chemist.

This lasted until June 1, 1941 when Abelson became a NRL employee and all of Abelson's work was officially transferred to NRL's Anacostia Station. Abelson felt that the main advantage of the process was its simplicity and low startup cost. This simplicity was shown by the speed with which the first plant was constructed after authorization. The main disadvantage was the large steam requirement [8].

In June 1941, the decision was made to proceed by constructing a small pilot plant with 36-foot columns next to the Boiler House at NRL. Construction of the equipment was completed in November. Over the next 6 months, NRL staff experimented with the spacing for the interior of the columns and their continuous operation, learning the optimum spacing and ease of operation. Encouraged by these findings, NRL decided to expand their research by constructing a pilot plant with 14 48-ft. long columns. The plant was authorized in July 1942 and was substantially completed by November [9].

Since the Navy was focused on submarine propulsion, they chose to use an enrichment method that would provide quantity over quality and decided to pursue liquid thermal diffusion. It was acknowledged that liquid thermal diffusion was not the best method for ultimate performance because of its high consumption of power. At the same time, it was seen as a feasible method for producing large amounts of material [10].

The Laboratory had 10-15 columns up and running by November 15, 1942 and producing accurate, usable data by December. On December 10, General Leslie R. Groves and representatives of the Army's Manhattan Engineering District visited the NRL plant to inspect the setup. They were given a briefing on Abelson's separation method and a complete report of the Naval Research Laboratory's work.

NRL provided all of the information they had and recommended the potential importance of their program on Uranium production. It was at this time that NRL was informed that MED had been placed in charge of isotope production by order of the President. Gunn's comments following the visit indicate he was not happy with the situation. What most disagreed with Gunn was the Navy's lack of representation since the navy was not directly represented on any of the research committees. An advisory committee from MED followed up Groves' visit in early 1943 and had a favorable view of NRL's work.

However, the use of liquid thermal diffusion by MED was vetoed. Groves felt that liquid thermal diffusion was unsuitable as an independent process due to its large requirement of steam. According to Groves, the size of the Navy project and its lack of urgency did not impress him. Finally, it was also felt that to transfer the Navy program to MED would have major administrative and security problems [11].

Despite Groves' views, an order by President Roosevelt was what kept the Navy outside the nuclear research program. When Vannevar Bush -- director of the Office of Scientific Research and Development (OSRD) -- heard that Groves intended to visit NRL, he felt this was "a mistake". Bush had insisted to Roosevelt that the Navy should be excluded from nuclear research and that the work should be given to the Army. It is quite likely that Bush wanted the work given to the Army since he had influence over the War Department leadership as well as a respect for Secretary of War Henry Stimson. In contrast, Bush had run into conflict with the Navy.

Bowen had criticized Bush and OSRD's work as supplanting those of the service laboratories and taking needed funding from NRL. Bush in turn had no qualms about making an example of Bowen and NRL. Roosevelt -- who trusted Bush's advice -- was made aware of the potential for using atomic energy for military purposes at the end of 1939 through Albert Einstein's letter and Briggs' Uranium Committee report.

Only a few naval officers and civilian engineers were sent from the Navy to MED. When the Uranium Committee became the S-1 Committee of OSRD, all Navy personnel were removed from membership. This further isolated the Navy's work. Finally, the fact that Abelson's findings were not available until February 1943 contributed to Roosevelt's decision to have the Army pursue nuclear research -- leading to the creation of MED in September 1942 [\[12\]](#).

To determine if NRL research would be of any use to MED, it was recommended that NRL begin a series of experiments to determine if thermal diffusion can provide consistent results. There was concern that NRL research could have a negative effect on MED. As such, it was recommended that NRL continue its work as a small-scale problem [\[13\]](#).

While Manhattan was not interested in liquid thermal diffusion, it remained the Navy's primary means of isotope separation. By 1943, MED had expended over 2 million dollars on their project where as the Navy's work had only cost \$60,000.

The general feeling was that NRL should be included in further research since the Laboratory had significantly contributed in the beginning. Since NRL had been involved up to that time, Gunn felt that it was not "in the best interest of progress" to be excluded from further work. As he saw it, NRL was "a military laboratory entitled to have access to any information in the country available on this subject" [\[14\]](#). Gunn was not alone in his views. Admiral Alexander H. Van Keuren -- who became director of NRL in 1942 -- was equally outraged over the Army's expenditure of "astronomical sums" while the Navy had "independently carried forward a fruitful research program" at considerably less cost [\[15\]](#).

By January 1943, research had proven that the set-up was dependable and capable of continuous operation. This indicated that use of the thermal diffusion method of isotope separation on a large scale was achievable. However, additional research was needed before a production plant could be designed. Between February and July 1943, NRL constructed 18 columns which were operated for 1,000 days. During this period, NRL realized its steam facilities were inadequate to expand research using larger columns, resulting in the search for a new steam source. The Naval Research Laboratory made a review of several naval facilities and came across the Naval Boiler and Turbine Laboratory at the Philadelphia Navy Yard [\[16\]](#).

NRL proposed the construction of a pilot plant at the Philadelphia Navy Yard in June 1943. On July 24, Van Keuren, Gunn, and Abelson visited NBTL to determine if steam production and available facilities would meet their research needs. 11 days later at a meeting between representatives of both laboratories, it was agreed that NRL's research would move to the Philadelphia facility. One stipulation

of the set up was that the NRL research could not interfere with turbine testing. NRL stressed the need for team work, requesting that both the Public Works Office at the Philadelphia Navy Yard and NBTCL be instructed that this project have “priority” and to “collaborate” with the NRL in the plant’s construction and operation. These requests were most likely a result of the NRL’s growing difficulty in getting assistance from the Army and to insure against problems within the Navy.

On November 17, 1943, the order was signed that authorized NRL to construct a 300-column pilot plant in Philadelphia with the stipulation that they not use technical personnel possibly needed by MED. Construction on the Philadelphia plant began on January 1, 1944 and by February, work was progressing well [17].

In addition to putting NRL on the back burner, MED hindered their access to information and materials. By the beginning of 1942, NRL had stopped receiving information from the S-1 Committee. Despite Abelson’s reports being sent to the Committee through Gunn, he was unable to garner the interest of the Committee and eventually was no longer in liaison with them. While the Navy did not place limitations on the development of their work, they were unable to proceed further without information from the Army. Gunn was unwilling to spend money on research that was potentially being conducted by other government scientists. To do otherwise held the potential for senseless expenditures.

Van Keuren supported the idea of the NRL having access to nuclear research information that would benefit both the Navy’s interest in submarine propulsion and weapon’s development. Despite the best efforts of the Navy, NRL and MED were completely out of contact with each other between September 1942 and April 1943 [18].

Once MED took over nuclear research, NRL began to have difficulty in acquiring material. As early as January 1943, NRL was informed that in order to obtain supplies of UF_6 it would have to go through the Army. When the S-1 Committee reviewed the Navy’s work before Labor Day 1943, it decided that NRL would be cut off. As a result, NRL was not to receive new supplies of Uranium hexafluoride to conduct experiments. When NRL requested additional supplies of UF_6 in October, Groves refused. MED informed NRL on October 11 that it would not be able to supply them with the material “for an indefinite period.”

NRL pointed out that it was Abelson who had developed the current method of producing Uranium hexafluoride and that NRL had freely shared this information. As a result, the Army reluctantly supplied the material. At this point, all information exchange between the 2 projects stopped again. In November, MED ordered the War Production Board to withhold UF_6 supplies from NRL that were necessary for the Philadelphia plant.

At that point NRL sought to restart its own production of UF_6 until it realized that the Army controlled the nation’s entire raw Uranium supply. Furthermore, Abelson learned from Richard Lund at the Rare Minerals Division of the War Productions Board that Army officers had previously informed him not to give NRL additional Uranium. Gunn saw such actions as “unwarranted, unjustified and manifestly an attempt to override the best interests of the Navy in this work.”

Gunn and the other scientists did not see how their request for a mere 2,000 pounds could effect or jeopardize the Army’s project. Van Keuren directly contacted Groves to remind him that the S-1 Committee had decided that NRL should continue its research “on a small scale” which was “being undertaken as an insurance against the failure of the isotope separation project.” In pointing out the Navy’s need for the Uranium hexafluoride, Van Keuren stated that “[t]his material is essential for the completion of the present phase of the Navy’s work on isotope separation” and that the Army’s attitude was “not understood” [19].

After excluding the Navy from the main program, the Army decided to use the electromagnetic and gaseous diffusion processes for isotope separation and constructed 2 plants in Oak Ridge, TN. As the Philadelphia plant neared completion in spring 1944, MED only had the electromagnetic plant in operation with the gaseous diffusion plant still months away from completion. Looking at other separation methods they had discarded earlier, J. Robert Oppenheimer (MED's scientific director) started to take renewed interest in liquid thermal diffusion after reviewing two 1-year old reports on Abelson's work and getting oral reports from Captain William S. Parsons. The estimates of the Philadelphia plant led Oppenheimer to consider using slightly-enriched Uranium as a feed material for the other processing plants to speed up production.

Oppenheimer appraised Groves of this possibility, to which Groves responded that he was not sure if the Army would use the Navy's process. A review committee went to Philadelphia in mid-June 1944 and recommended the construction of a liquid thermal diffusion plant at Oak Ridge.

On June 26, Groves arrived at NRL to obtain the blueprints for the Philadelphia plant. The Army broke ground on July 6 for the plant(labeled S-50) and had the first columns ready by September 15 [\[20\]](#).

As construction of S-50 progressed, the Army sent personnel to the Philadelphia for training. On September 4 -- a week after arriving -- an explosion occurred at the Philadelphia Plant when a cylinder of UF_6 overheated and exploded, fracturing nearby steam pipes. The mixture of UF_6 and steam created hydrogen fluoride (a very caustic acid) which injured 13 men, of which two died. The accident halted the training in Philadelphia and sent all of the Army trainees and 15 men from the Naval Research Laboratory under Abelson to Oak Ridge.

A thorough investigation was conducted to rule out faulty designs at the Philadelphia plant. Instead of poor construction work on the part of the Navy, it was found that the cause of the accident was the result of the tanks and the lack of cooperation on the part of Manhattan. The Army's control of nickel production had prevented the Navy from constructing seamless nickel tubes for UF_6 storage. Instead, the Navy had to build tanks with a thin nickel liner.

When the meeting turned to discussing the safety procedures that the Army had developed, Gunn asked how they had arrived at their calculations only to have an Army representative state he was ordered not to disclose that information. Gunn's anger at the Army must have been greatly increased by this time. Not only had the Army excluded the Navy from nuclear research in terms of material and information, but now it was also unwilling to share safety information following 2 deaths [\[21\]](#).

Repairs were quickly made to the Philadelphia plant and production of enriched Uranium continued. In excess of 5,000 pounds were turned over to MED to feed the electromagnetic isotope separator, which contributed to the construction of the first nuclear bomb [\[22\]](#). The Philadelphia plant was used even after the S-50 plant was shut down. Plans were made for the plant to be shut down by January 1, 1946. It was not until September 1946 that the decision was made to dispose of the Philadelphia plant [\[23\]](#).

With the end of World War II, NRL scientists were eager to continue with their research into nuclear propulsion. However, as a result of the security restrictions placed on nuclear work, NRL was still blocked from getting information on Manhattan research. Bowen felt that if the Navy was going to pursue the creation of nuclear propulsion, it needed to control all of the related activities. The Navy would need to create its own capabilities in both basic nuclear science and propulsion. In his plea for the

Navy's re-inclusion into nuclear research, Gunn noted that submarine propulsion was at the top of the list for the Navy's prime interest.

Gunn felt an obligation to make the Navy aware of the potential of nuclear energy. Despite the security blackout, he was able to organize a symposium at NRL on November 19, 1945 for submarine leaders to discuss the facts of nuclear propulsion.

The interest generated by this symposium eventually led to a report prepared by Abelson, and other Navy scientists that was issued on March 28, 1946 [24]. Abelson did not hesitate to point out that the Navy's work on submarine propulsion had been deferred first to conduct the preliminary work on isotope separation, and then to assist in completing the atomic bomb. Furthermore, the Navy clearly saw the lack of cooperation between NRL and MED as an obstacle. The report stated that NRL needed adequate support from the Navy, the President, and the Manhattan District to continue its research [25].

It is undeniable that Gunn was proud of his efforts -- especially in terms of cost. For Gunn, it was the Army's dog-in-the-manger control of the nuclear research program that prevented NRL from actually producing a nuclear submarine sooner. He saw the flow of information between the NRL and MED as one-way. In 1945, he noted that even though the Navy was represented in the beginning of the atomic energy research it did not have "access to the technical developments of the Army since the middle of 1941."

Gunn felt the close relationship between the Army and the Uranium Committee had "jeopardized the Navy's interest in the work" and put NRL "years behind in knowledge and details of operation of atomic power plants." Gunn felt that, "[t]he Manhattan District missed no opportunity to scuttle the NRL program and no useful assistance was ever obtained from them." As such, Gunn goes on to state, "[i]t is my view that this action prolonged the War by many months" [26].

NRL being cut off from Uranium between November 1943 and June 1944 was another sign that the Army wanted to sidetrack the NRL's work until it became apparent it would be of use to them. It was Gunn's opinion that MED only renewed interest in the NRL's work when confronted by possible failure [27].

Gunn's feelings towards the treatment of NRL were expressed in a 1954 letter to Bowen. In it, he states that he was "puzzled" as to why the NRL method was not adopted earlier than 1944 since "it certainly fitted in very well indeed with available facilities during the War." Gunn expressed the opinion that Groves and Oppenheimer had ignored the Navy's work in order to promote their own programs. Overall, Gunn believed that the separation between the work of the Army and Navy "had its roots in partisan Presidential politics."

Gunn wrote that, "Roosevelt had no business appointing an independent political group to be responsible for atomic energy when there was already established -- under forward-looking Navy management -- a team and program designed not only to produce a bomb but who also were dedicated to its long range utilization as a military tool and implement of public welfare."

Obviously Gunn felt the rug had been pulled out from under him as he was the one who had initiated the first research into atomic energy, only to have control placed in the hands of the S-1 Committee with the limelight being given to the Army. Gunn's overall opinion was summed up in the statement, "I think we had the hose turned on us!" [28]

It is undeniable that Ross Gunn and the Naval Research Laboratory made significant contributions to nuclear research in the United States. NRL contracts initiated the first practical research into isotope

separation, and Navy funding was behind Abelson's method for Uranium hexafluoride production (a process still used today). Therefore, Gunn's view that NRL's work was sidelined and redirected by the Manhattan District is understandable. However, it is more likely that the real reason NRL's work was sidetracked by the Army was its goal -- nuclear propulsion.

From the beginning of Gunn's work, a nuclear-powered submarine was the primary goal with a nuclear weapon as a far second. Those in the Navy did not begin to view their work as contributing to a weapon until 1943. The Army, on the other hand, believed they were in a race to produce an atomic bomb before the Germans and did not want the NRL taking personnel and material they needed.

Unfortunately, rather than seeing NRL as contributing to nuclear research, the MED saw them as competing for resources. After the war, the Navy was further blocked by Grove's unwillingness to release information without authority from either the president or the passage of the Atomic Energy Act. This further delayed the Navy's nuclear reactor program until 1947.

Once the Navy did begin work on a nuclear-powered submarine, Rickover was able to build a support base that allowed him to control the Navy's nuclear program for over 30 years. In that time, Rickover's ability to get the *Nautilus* and other boats in the water overshadowed the early efforts of Gunn and NRL.

Ross Gunn and the NRL got caught in the wake of the 2 major military history events of the nuclear age – the Atomic Bomb and the *Nautilus*.

References

- [1] Ernest Andrade, Jr., "Submarine Policy in the United States Navy, 1919 – 1941," (*Military Affairs*: Vol. 35, No. 2) 50; Norman Polmar, Rickover, (New York: Simon and Shuster, 1981), 117; U.S. Senate Special Committee on Atomic Energy, Atomic Energy: Hearings Pursuant to S. Res. 179, 79th Cong., 1st sess., 13, 14, 19, and 20 December, 1945, 364-365, 367 (hereafter Senate COA); Ross Gunn, "Notes prepared for the Senate, 13 December 1945", 1, Navy's Role in Development of Atomic Energy, Operational Archives, Naval Historical Center, Washington, D.C. (hereafter Atomic Energy, Operational Archives, NHC).
- [2] Senate COA, 364 – 365; Ross Gunn, "The Early History of the Atomic Powered Submarine at the U.S. Naval Research Laboratory March 1939 to March 1946." Revised 1959. Unpublished manuscript, 7 Gunn, Ross Biographical File. Niels Bohr Library, American Institute of Physics, College Park, MD. 1 – 2 (hereafter Gunn, "The Early History ...," AIP).
- [3] Ross Gunn had to request funds from Admiral Harold Bowen since the Bureau of Engineering controlled funding for NRL. Philip Abelson, "Early History of Uranium Power for Submarines," (Washington, D.C.: Naval Research Laboratory, 1 May 1946), 1; Lewis L. Strauss, Men and Decisions, (New York: Doubleday and Co., 1962), 236; Memorandum for File, 17 March 1939 [Box 1 / Folder 1] S-1 Files, Naval Research Laboratory Correspondence, 1939-1946, Records of the Office of Scientific Research and Development, RG 227, National Archives and Records Administration at College Park, College Park, MD (hereafter S-1 Files, RG 227, NACP); Laura Fermi, Atoms in the Family: My Life with Enrico Fermi (Chicago: University of Chicago Press, 1954), 163; Richard G. Hewlett and Oscar E. Anderson, Jr, *The New World, 1939/1946: Volume 1 A History of the United States Atomic Energy Commission*, (University Park, PA: Pennsylvania State University, 1962), 15; Richard G. Hewlett and Francis Duncan, Nuclear Navy, 1946-1962, (Chicago: University of Chicago Press, 1974), 16 – 17; Gunn, "The Early History ...," AIP, 2; Ivan Amato, *Pushing the Horizon: 75 Years of High Stakes Science and Technology at the Naval*

Research Laboratory, (Washington, D.C.: Naval Research Laboratory, 1998), 140 – 142; “World War II Nuclear Research at NRL,” 1958, p.1, General Correspondence, Papers of Harold Gardiner Bowen, Manuscript Division, Library of Congress, Washington, D.C. (hereafter Bowen Papers, LoC).

- [4] Harold G. Bowen, Ships Machinery and Mossbacks: the Autobiography of a Naval Engineer, (Princeton, N.J.: Princeton University Press, 1954), 182 – 183; Abelson, “Early History ...,” 2; Ross Gunn to Director, *NRL*; 1 June 1939; Subject: Submarine Submerged Propulsion – Uranium Power Source – Status of as of this date, Gunn, Ross Biographical File. Niels Bohr Library, American Institute of Physics, College Park, MD.; Senate COA, 366, 371; Gary E. Weir, *Forged in War: The Naval-Industrial Complex and American Submarine Construction, 1940 – 1961*, (Washington, DC: Naval Historical Center, 1993), 155.
- [5] Gunn, “The Early History ...,” *AIP*, 4; Abelson, “Early History ...,” 1; J.B. Cochran, “History of Uranium Project at the Naval Research Laboratory,” (Washington, D.C.: Naval Research Laboratory, 9 August 1945), 1; Director NRL, “Uranium Project, Memorandum of Progress 1940 – 1941.” 8 April 1942, Bureau of Ships, RG 19, National Archives and Records Administration at College Park, College Park, MD (hereafter RG 19, NACP); Bowen, 183 – 184; Ross Gunn to George Pegram 27 November 1939, Gunn to Jessie Beams 27 November 1939, Pegram to Gunn 9 December 1939, Gunn to Pegram 19 December 1939, Gunn to Pegram 10 January 1940 [Box 1 / Folder 1], S-1 Files, RG 227, NACP.
- [6] Philip H. Abelson, “An Exciting Era in Nuclear Physics” (talk presented at the Washington Academy of Sciences, 15 March 1951), 7 – 8, Archives of the Department of Terrestrial Magnetism, Carnegie Institution of Washington, Washington, D.C.; R.Briscoe to Lyman J. Briggs 9 July 1941 [Box 1 / Folder 2] S-1 Files, RG 227, NACP.
- [7] Gunn, “The Early History ...,” *AIP*, 3; Hewlett, *The New World*, 22 – 23; Philip Abelson, “Progress Report on Liquid Thermal Diffusion Research,” (Washington, D.C.: Naval Research Laboratory, 3 January 1943), 1; Bowen, 184; Senate COA, 367; Director NRL, “Uranium Project, Memorandum of Progress 1940 – 41,” RG 19, NACP.
- [8] Briggs to Bowen 10 September 1940 [Box 7 / Folder NRL] Briggs Alphabetical Files, S-1 Files, Records of the Office of Scientific Research and Development, RG 227, National Archives and Records Administration at College Park, College Park, MD (hereafter Briggs Files, RG 227, NACP); Philip H. Abelson, “An Exciting Era in Nuclear Physics” (talk presented at the Washington Academy of Sciences, 15 March 1951), Archives of the Department of Terrestrial Magnetism, Carnegie Institution of Washington, Washington, D.C. 4, 6 – 7, 8; Abelson, “Progress Report ...,” 2, 3; Richard Rhodes, The Making of the Atomic Bomb, (New York: Simon & Schuster, 1986), 550; R.E. Ruskin, “Separation of Isotopes,” September 1947, 2; Henry DeWolf Smyth, Atomic Energy for Military Purposes: The Official Report on the Development of the Atomic Bomb Under the Auspices of the United States Government, 1940 – 1945, (Princeton: Princeton University Press, 1945), 68, 161 – 162; Senate COA, 367, 371; Amato, 144; Bowen, 184 – 185; Gunn, “The Early History ...,” *AIP*, 2-3; Weir, 156.
- [9] Abelson, “Progress Report ...,” 4 – 5; Philip Abelson, *Liquid Thermal Diffusion*, (Washington, D.C.: Naval Research Laboratory, 1946), 23; Notes on Statements by Abelson and Gunn, Naval Research Laboratory, November 9, 1944, Series I, Henry DeWolf Smyth Papers, American Philosophical Society; Jones, 173.

- [10] Philip J. Abelson Memorandum for Director and Files: “Present Status of Uranium Problem – Centrifugal Separation of Isotopes” 27 March 1942, Abelson Memorandum for Director and Files: “Present Status of Uranium Problem – Centrifugal Separation of Isotopes” 14 July 1942 [Box 1 / Folder 4], Harold G. Bowen to Briggs 2 September 1942 [Box 2 / Folder 5] S-1 Files, RG 227, NACP; Rhodes, 551; Jones, 172 – 173; Abelson, “Progress Report ...,” 20 – 21.
- [11] Abelson, “Early History of Uranium ...” 3; Senate COA, 368; Gunn, Memorandum for File “Production of Separated Isotope 235” 10 December 1942 [Box 2 / Folder 5], S-1 Files, RG 227, NACP; The recommendation for the Lewis Group to visit NRL was made by Briggs on 9 December, Hewlett, *The New World*, 169 – 170; Gunn to Bowen 9 April 1952, Harold G. Bowen Papers, MC #033, Public Policy Papers/University Archives, Department of Rare Books and Special Collections, Princeton University Library (hereafter Bowen Papers, Mudd); Leslie R. Groves, Now It Can Be Told: the Story of the Manhattan Project, (New York: Harper and Brothers, 1962), 119; Jones, 172, 174; The follow up committee in January 1943 consisted of Briggs, Urey, E.V. Murphree, Karl Cohen, and W.I. Thompson, Abelson, Liquid Thermal Diffusion, 23.
- [12] Robert William Love, History of the U.S. Navy, Vol. 1, (Harrisburg, PA: Stackpole Books, 1992), 271; Groves, 22 – 23; Statement of the Secretary of War, [Box 80, “Nuclear Physics” folder], Records of the Office of Naval Research, RG 298, National Archives and Records Administration at College Park, College Park, MD; G. Pascal Zachary, Endless Frontier: Vannevar Bush, Engineer of the American Century, (New York: The Free Press, 1997), p 119-128; Hewlett, *The New World*, 20; Hewlett, *Nuclear Navy*, 15, 18, 20; Cochran, p 2; Weir, 156.
- [13] Leslie R. Groves to W. R. Purnell 27 February 1943 [Box 2 / Folder 5] S-1 Files, RG 227, NACP; Stephane Groueff, Manhattan Project: The Untold Story of the Making of the Atomic Bomb, (London: Collins, 1967), 338, 340; Briggs, Urey, Murphree & Lewis to James B. Conant 8 September 1943, James Conant to. Purnell 15 September 1943 [Box 2 / Folder 8] S-1 Files, RG 227, NACP; Jones, 175.
- [14] Amato, 146; Gunn, Memorandum for Director, NRL “The Uranium Problem and the Utilization of Uranium Fission by the Navy” [Box 2 / Folder 5], S-1 Files, RG 227, NACP.
- [15] Director of NRL to Chief of the Bureau of Ships, “The Uranium Problem and the Utilization of Uranium Fission by the Navy. Review of Present Status,” 15 June 1943 [Box 2 / Folder 6], S-1 Files, RG 227, NACP.
- [16] Abelson, “Progress Report on ...,” Abstract, 21; Abelson, “Memorandum for the Director, NRL” 4 January 1943 [Box 2 / Folder 5], Director NRL to Chief of Bureau of Ships, “The Uranium Problem and the Utilization of Uranium Fission by the Navy. Review of Present Status,” 15 June 1943 [Box 2 / Folder 6], S-1 Files, RG 227, NACP; Philip Abelson, “Fourth Partial Report on Liquid Thermal Diffusion Research,” (Washington, D.C.: Naval Research Laboratory, 30 July 1943), 1, 3.
- [17] The design work for the Philadelphia plant was conducted between June and October 1943 based on the 14-column plant at NRL. The main goal was to construct a unit that could be expanded to a production plant. Capt. C.A. Bonavillian of NBTL was concerned about the limited amount of time for the completion of the NRL facility. In addition, he was not sure that the boiler design would permit continuous operation. After a conference in Philadelphia on 20 October 1943, it was tentatively decided that a 100-column pilot plant would be installed and operated utilizing steam from the boilers originally intended for Annapolis. As for installation time, NBTL felt that they

could have the facility up and running by 1 April 1944. Abelson, *Liquid Thermal Diffusion*, 60, 62; C.A. Bonavillian to Director, Naval Research Laboratory, "Proposed Chemical Reflux Installation at the Naval Boiler and Turbine Laboratory," 7 August 1943, S-1 Files, RG 227, NACP; Memorandum for File: "Reflux Heat Exchanger Test," 21 October 1943 [Box 2 / Folder 9], Gunn, Memorandum for the Files, "Uranium Problem – Steam and Facilities at Naval Boiler and Turbine Testing Laboratory, Navy Yard, Philadelphia" 24 July 1943, "Minutes of Meeting between Representatives of the NRL and NBT," 4 August 1943 [Box 2 / Folder 7], S-1 Files, RG 227, NACP; Abelson, "Early History of ...," 3-4; Jeffery M. Dorwart, The Philadelphia Navy Yard: From the Birth of the U.S. Navy to the Nuclear Age, (Philadelphia: University of Pennsylvania Press, 2001), p.188; Director NRL to the Chief of the Bureau of Ships, "Uranium Project, Transfer of, To Naval Boiler Laboratory, Navy Yard, Philadelphia, Pa." 10 August 1943 [Box 2 / Folder 7], Gunn to Director NRL, "Erection of Isotope Separation Pilot Plant at the Naval Boiler and Turbine Laboratory, Philadelphia, Pa." 3 November 1943 [Box 2 / Folder 9], S-1 Files, RG 227, NACP; Abelson, *Liquid Thermal Diffusion*, 24 – 25; Weir, 156.

- [18] Hewlett, *The New World*, 169; Senate COA, 374; Abelson, "Progress Report ...," 20 – 21; Van Keuren to Purnell 11 February 1943 [Box 2 / Folder 5], S-1 Files, RG 227, NACP; Jones, 175; Weir, 156 – 157.
- [19] H.W. Elley to R.W. Dole 14 January 1943 [Box 2 / Folder 5], Gunn to Mills "Raw Materials for Isotope Separation Plant" 3 November 1943, Van Keuren to Groves, 10 November 1943 [Box 2 / Folder 9], S-1 Files, RG 227, NACP; Hewlett, *The New World*, 171; Gunn, "The Early History ...," *AIP*, 4.
- [20] Groves, 94, 120 - 121; Abelson, *Liquid Thermal Diffusion*, 25; Hewlett, *The New World*, 168, 172; Groves had selected the H.K. Ferguson Co. to be the prime contractor for the Manhattan Project's liquid thermal diffusion plant. It was decided against using the Philadelphia Plant as it would not be under Army control and that the Manhattan Project plant could be built quicker if not distracted by operation at the Navy's plant. Jones, 175 – 179; Smyth, 202 – 204.
- [21] Amato, 149; Dorwart, 187 – 188; The log book listed the injured as E. Achutt, A.T. Young, N. Piscano, R Kendig, A. Wouch, Albert Pirolli, John Snyder, George Levefre (PFC - U.S. Army), Merlin Hanson (PFC - U.S. Army), John E. Tompkinson (PFC - U.S. Army), Stuart B. Bloom (PFC - U.S. Army), Arnold Kramish (PFC - U.S. Army), Douglas Meigs (employee of H.K. Ferguson & Co.), and Peter Bragg. (employee of Naval Research Laboratory), 2 September 1944, October 1, 1941 – December 31, 1944, Log Book, Philadelphia Navy Yard, Records of Naval Districts and Shore Establishments, RG 181, National Archives and Records Administration – Mid-Atlantic Region (Philadelphia, PA); Jones, 179 – 180; Manhattan District History: Book VI - Liquid Thermal Diffusion (S-50) Project (National Archives Microfilm Publication A1218, Roll 10), Records of the Office of the Chief of Engineers, RG 77, National Archives and Records Administration at College Park, College Park, MD S12; Groves, 122; Gunn, "Meeting with the Army group interested in Uranium at the office of Major General L. R. Groves," 12 September 1944 [Box 3 / Folder 11], S-1 Files, RG 227, NACP.
- [22] Notes on Statements by Abelson and Gunn, Naval Research Laboratory, November 9, 1944, Smyth Papers, APS; Gunn, "The Early History ...," *AIP*, 5; Abraham, "Navy Yard Has its own Atom Secret," Atomic Activity File, Box 173A, Philadelphia Navy Yard Mounted Clippings, Urban Archives; Gunn to Chief, Office of Research and Inventions, "Review of the Navy's part in the utilization of atomic energy and necessity for reevaluation of its bearing on Naval Problems" 21 August 1945 Draft, Bowen Papers, Mudd; Senate COA, 368.

- [23] Memo to Director, NRL, Subj.: Naval Research Laboratory Project at U.S. Naval Boiler and Turbine Laboratory, Philadelphia - Future Work on. 12/13/45, RG 19, NACP; Philip Abelson, et al, "Atomic Energy Submarine," (Washington, D.C.: Naval Research Laboratory, 28 March 1946), cover memo; Groves, 385; Mills to Commander PNSY, Reflux Plant - Disposition of. 5 Nov 1946. (Box 74, Folder 600.12), Records of Office of the Commanding General, Manhattan Project, General Administration Files, General Correspondence, Manhattan Engineering District, Records of the Office of the Chief of Engineers, RG 77, National Archives and Records Administration at College Park, College Park, MD (hereafter RG 77, NACP).
- [24] Carol O. Holmquist and Russell S. Greenbaum, "The Development of Nuclear Propulsion in the Navy" (United States Naval Institute Proceedings, Vol. 86, No. 9), 67; Hewlett, Nuclear Navy, 25; Gunn to Chief, Office of Research and Inventions; "review of the Navy's part in the utilization of atomic energy and necessity for re-evaluation of its bearing on Naval Problems" 21 August 1945 Draft, Bowen Papers, Mudd; Gunn, "The Early History ...," AIP, 6.
- [25] Abelson, et. al., "Atomic Energy Submarine," 1, 5.
- [26] Gunn to Chief, Office of Research and Inventions, "Review of the Navy's part in," 21 August 1945 Draft, Bowen Papers, Mudd; Senate COA, 371; Gunn, "The Early History ...," AIP, 4-5.
- [27] Gunn to Bowen, 9 April 1952, Bowen Papers, Mudd; Gunn, "The Early History ...," AIP, 5; Bowen, 189
- [28] Gunn to Bowen 29 September 1954, General Correspondence, Bowen Papers, LoC. The Editors International Journal of Naval History editors@ijnhonline.org

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