A Fireside Chat with Dick Garwin -- IBMer Since 1952 -- and Mark Ritter

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Fireside Chat (via Zoom)
IBM Distinguished Speakers Series

September 22, 2021, at noon EDT

Thanks, Will. Before we begin our chat, Dick, perhaps you can introduce the audience to your Federation of American Scientists' web archive where they can read more about the utopics you will discuss, as well as the other work you have done. <Wait for Dick to discuss the title page>

AT THE UNIVERSITY OF CHICAGO:

When you started as a graduate student at the University of Chicago, you wanted to start research work as soon as possible. How did you chose and approach the professor you wanted to work with, and what was some of the first research you did?

Enrico Fermi was clearly a gifted experimenter and clearest lecturer. I wanted to help him if I could. So, I just asked.

A Differential Analyzer for the Schrödinger Equation, Review of Scientific Instruments **21**, 411 (1950); https://doi.org/10.1063/1.1745603

1950 Differential Analyzer.pdf

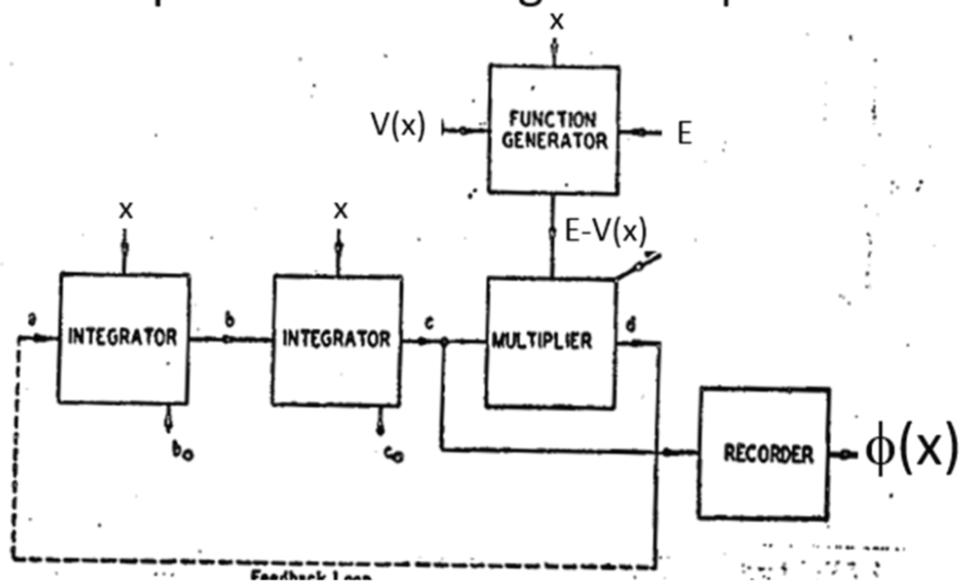
05_1950_Differential Analyzer for Schroed k

A Differential Analyzer for the Schrödinger Equation

1-Dimensional Schrödinger equation:

$$(\partial^2\varphi/\partial x^2)+[E-V(x)]\alpha\varphi=0$$

How to solve this equation before digital computers could?



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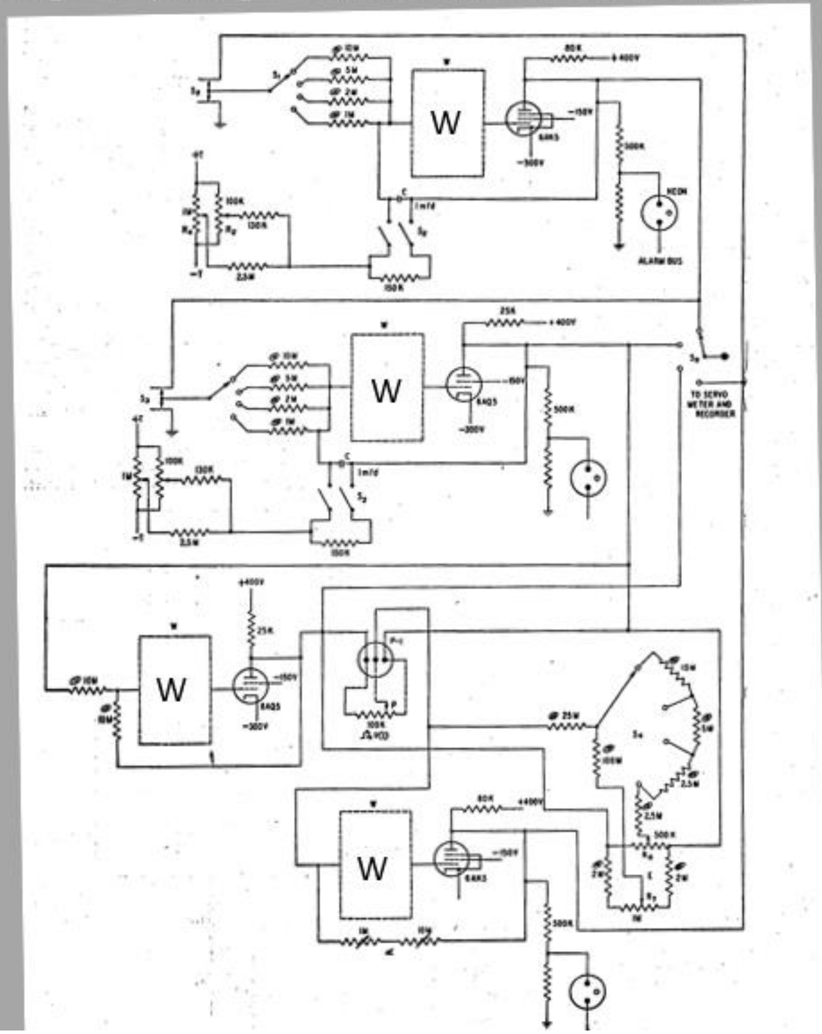
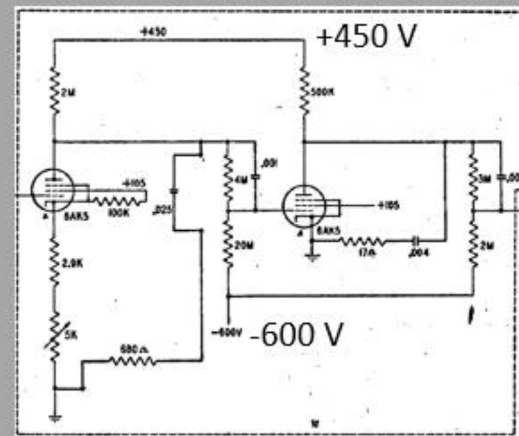
Analog Computing Circuit to Solve Differential Equations



Integrator block "W"



Solution Accuracy ~ 0.5%

Same approach could solve higher-order and nonlinear D.E.s

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With Enrico Fermi as your advisor and with your appointment in December, 1949 to the Physics faculty of the University of Chicago you must have had some interesting scientific discussions. Can you relate some of those?

Most such discussions were at the W.F. Libby weekly afternoon seminar, typically with one speaker each week – called upon if not scheduled. Or at lunch, where Fermi would ask his notorious questions, e.g., “How many piano tuners are there in Chicago?” or “How much faster does a wristwatch run in Denver than in New York City? “(1950, you recall, so it had a balance wheel and a spiral hair spring).

Libby Seminar included Libby’s own on C-14 dating, and Fermi’s intergalactic magnetized plasma clouds for stochastic acceleration of cosmic rays.

AT IBM RESEARCH:

What led you to seek a position at IBM Research?

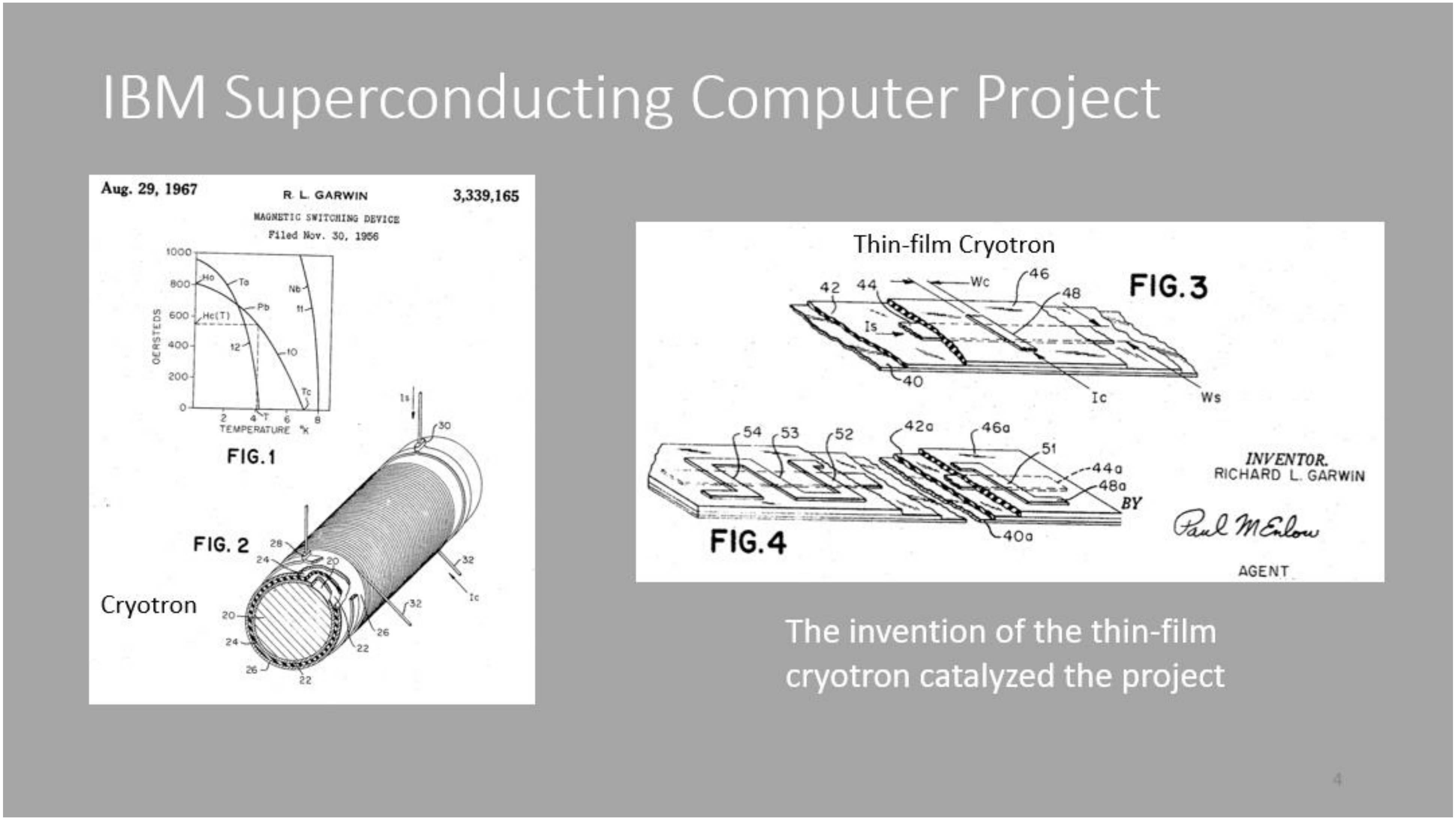
A favorably reported visit by Emilio Segré of Berkeley. He had been offered the Directorship of the newly created IBM Watson Scientific Laboratory at 612 W. 115th St., probably in early Fall 1952 and had visited and spoken with the Director of the IBM Watson Scientific Computing Laboratory, Wallace J. Eckert, and his Advisory Committee from Columbia -- I.I. Rabi, Polykarp Kusch, and was favorably impressed and probably so reported to Enrico Fermi. Segré was on sabbatical from Berkeley at the University of Illinois at Champaign-Urban, and I visited him there to get his personal views. I then was invited to New York and offered a position as a staff member of the Watson Lab, which I accepted.

But here is a key aspect of my entire career at IBM. I requested that my employment contract specifically include a provision that I could spend one-third of my IBM time working with the U.S. government on matters of technology, in view of my three summers of consulting at Los Alamos, where I discovered that I was good at this. IBM would continue to pay my salary during these times, and my travel expenses, and I would reimburse IBM with whatever I could collect from the government in compensation. IBM would have no insight into what I was doing. IBM fulfilled these provisions until I retired in June 1993.

You led work to create a superconducting computer. Can you tell us about that project?

Based on the wire-wound Cryotron invented by MIT engineering graduate student Dudley Buck. *"Dudley Buck's Forgotten Cryotron Computer - IEEE Spectrum"* of 03/19/2014 misses my IBM invention of the thin-film cryotron and puts the start of the IBM thin-film cryotron computer effort in 1957, when it really began in the late summer of 1956.

By about September 1956, I had some 80 people on this program at three labs, including the Watson Lab, where it was mostly me, graduate student Myriam Sarachik and part of the support staff such as a draftswoman, machinist, etc. With my technician, Pat Maloney, we built a helium liquefier / cryostat to support such a computer at 4.2°K, with my patents on the use of the Stirling engine as refrigerator, as well as other patents on superconducting planar, thin-film technology. The technical staff at Poughkeepsie who learned and developed IBM photolithography for this program became the foundation for IBM's work on integrated circuits.



You resigned your position leading the superconducting computing effort to pursue some exciting particle physics work. Can you describe what interested you and what your research discovered?

I had devised the planar thin-film cryotron technology and a lot of the other enabling, flat transformer coupling, and the like, all with superconductors, that persuaded me one could make what would then have been the highest performance computer, and this was apparently endorsed by Wallace Eckert and by the IBM Director of Pure Science, John McPherson and maybe even by the IBM Vice President and Director of Engineering, W.W. McDowell, so somehow I was put in charge of this new program of 80 people with only a bit of corporate direction-- that I needed to work with Arthur D. Little who had been contracted by IBM to consider supplying liquid helium to distributed cryotron computers, and the like. That was a small part of my effort, but I decided that that was not the way to go, anyhow, so Pat Maloney and I built and operated a three-stage Stirling-engine refrigerator/liquefier -- to which Arthur D. Little had rights under the existing IBM- A.D. Little contract. They commercialized some of this invention and marketed the product, as I recall.

I had left the field of particle physics in December 1952 when I joined IBM, but I had created the instrumentation that was used in that field –- a pulser for the "millimicrosecond range" (now “nanosecond”), what were known as the "Garwin coincidence circuits" in two publications in the Review of Scientific Instruments -– RSI, and two publications in RSI on large scintillation counters, all of which became standard in the field of electronic counting in nuclear and particle physics.

When I left Chicago to come to IBM, I commissioned the Chicago electronic shop to build a six-input coincidence/anti-coincidence analyzer and had IBM pay for it. It sat on my shelf at the Watson Lab until about 1955, when I lent it to my friend and colleague, Leon Lederman, at the Columbia Nevis cyclotron in Irvington, NY.

He and his graduate student, Marcel Weinrich, used it and my standard scintillation counters to set up an apparatus for measuring the lifetime of the muon –- positive and negative, from external beams from the Columbia synchrocyclotron, as the muons were stopped in various solid materials. The mu+ should all have exhibited the two-microsecond lifetime for isolated muons at rest, and the mu- significantly shorter lifetime; in their K-shell orbits 207 times smaller than that of the electron; the mu- had substantial presence inside the positively charged nucleus.

011657.NYT H_M_Schmeck.pdf   011557CUPD.pdf   021557 Garwin-Lederman-W

So here is the front page of The New York Times from 01/16/1957 with the headline about our experiment and a pointer to Page 24 of the Times which was an information sheet on parity provided by the Columbia University Physics Department and, you can bet, largely done by T.D. Lee himself. I open that sheet because the parity description is clearly written and you won't do any better or get any closer to the horse's mouth than this. It explains the two experiments that had been done in the previous two weeks, including ours at the Nevis Cyclotron Laboratory that had been performed (and the paper written) between the night of January 4 and the morning of January 8. Here in our paper published in Physical Review Letters 02/15/1957 is the actual arrangement of the muon beams from the cyclotron stopping in a graphite block and having their spins precessed by current in a coil wound on the graphite block.

However, a spin one-half particle was certain up to that time not to have any asymmetry in its electron decay because this would have been a correlation between an axial vector and a real vector, forbidden by the conservation of parity. That's all I'm going to say about it. The discovery that the muon did have an easily detectable and very large asymmetry in the emission of its high-energy electron within the two-microsecond half-life of the muon revolutionized not only particle physics but our understanding of the universe and gave us new tools to do physics and chemistry.

THE NEW YORK TIMES, WEDNESDAY, JANUARY 16, 1957.

Text of Columbia Report on Physics Experiments

The Meaning of Parity

(Mirror Symmetry)

The parity law of physics states that for any atomic or nuclear system no new physical consequence or law should result from the construction of a new system differing

vatron. The data consisted of the study of the properties of the unstable Kmesons, particles which were only recently discovered (1952-53). One aspect of Kmeson disintegra-

well as the violation of "charge-conjugation-invariance." It was discovered that when the familiar pi meson (well known since 1947 and now known to be principally responsible for

Observations of the Failure of Conservation of Parity and Charge Conjugation in Meson Decays: the Magnetic Moment of the Free Muon*

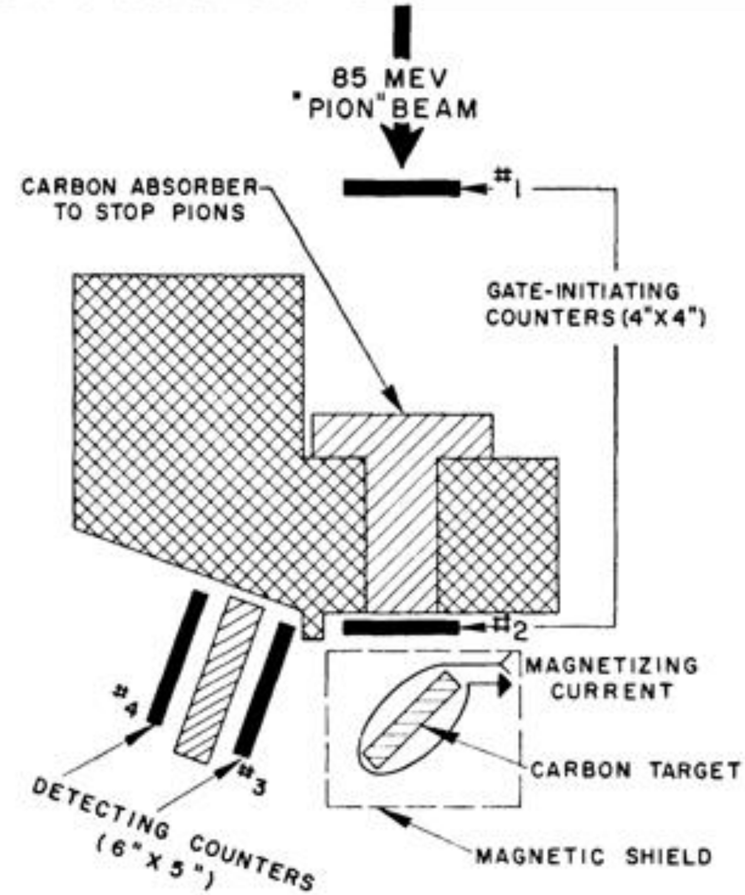
Richard L. Garwin,† Leon M. Lederman, and Marcel Weinrich

Physics Department, Nevis Cyclotron Laboratories, Columbia University, Irvington-on-Hudson, New York, New York

(Received January 15, 1957)

$$\pi^+ \rightarrow \mu^+ + \nu,$$

$$\mu^+ \rightarrow e^+ + 2\nu.$$



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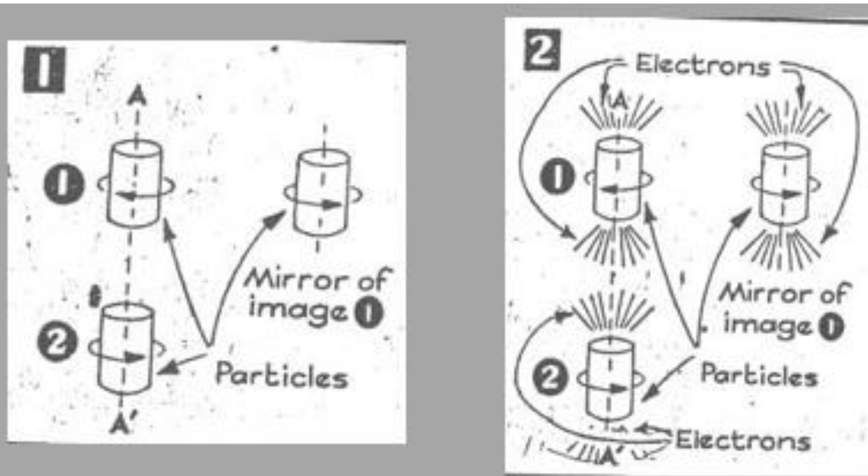

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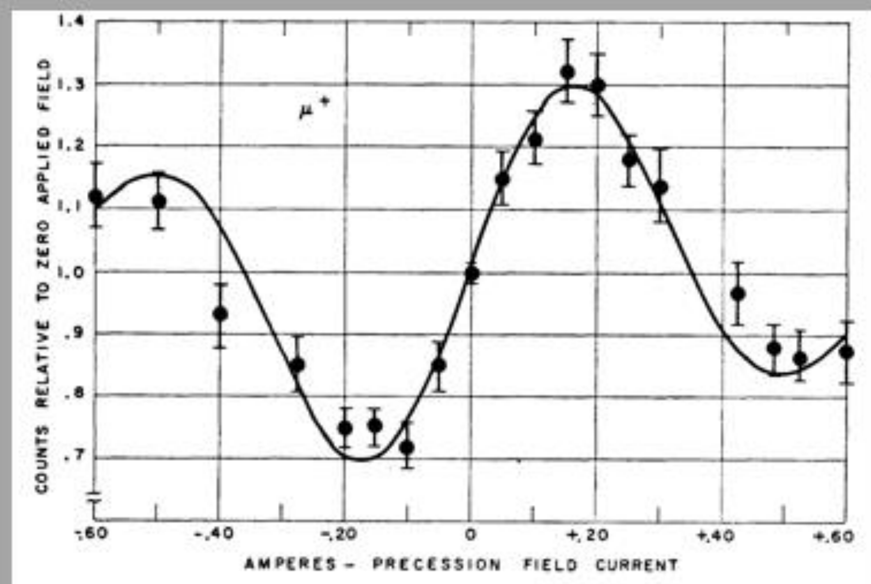


Can you explain how the position of IBM Fellow came to be?

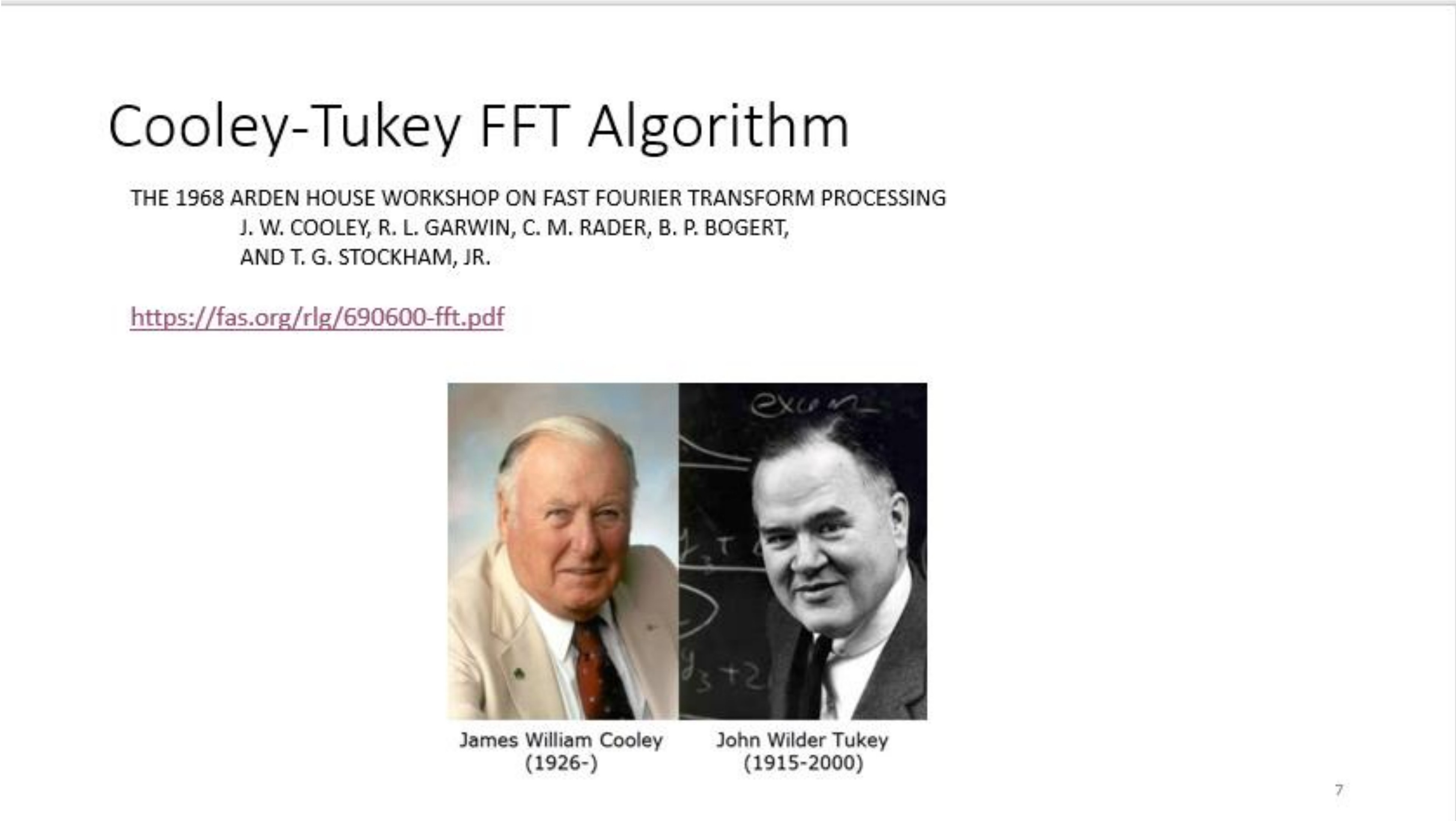
I recall having written Mannie Piore, by then IBM Chief Scientist, that there ought to be a rank at IBM of "Leading Engineer" and this could be given additional status by appointing perhaps four or six per year but also finding a similar number of outside people who would receive an award for being so select. I don't think I ever got a response from Mannie, but in 1963, while on a skiing vacation in Vermont with my family, I received a telephone call asking me to be at a New York hotel the following evening to be made an IBM Fellow—a new rank. I regret saying that this was the first vacation I had with the family in many years and I really didn't want to do this and couldn't they do it next year. I know now, of course, how much preparation had gone into this, without my knowledge, and it took four years for it to happen in 1967.

You have also had an eye for encouraging important collaborations among colleagues. Can you describe how you encouraged how the Cooley-Tukey Algorithm came to be?

I have really told the full story of the FFT that happened in 1963, in my post for the IEEE Transactions on Audio and Electroacoustics

> "The Fast Fourier Transform as an Example of Difficulty in Gaining Wide Use for a New Technique" (The 1968 Arden House Workshop on Fast Fourier Transform Processing), IEEE Transactions on Audio and Electroacoustics AU-17, No. 2, pp. 68-72, 06/00/69. https://fas.org/rlg/690600-fft.pdf

and Jim Cooley also has his say there. Jim didn't know much about my background and why I was interested and what I was doing, so please rely on what I said and not what he said in that regard. Jim Cooley did a superb job on this, taking no more storage in the computer than was used for the initial array, at a time when computer storage was a dollar a bit.



You also explored some early non-silicon memory technology; can you describe that work?

When I first came to IBM, I sat down and wrote up several possibilities for computer storage, among them ions moving in vacuum at different speeds and being reversed in position that would compensate their differing speeds. Others were, ultrasound in crystals, in analogy to mercury delay lines, and I even did some little experiments in that regard, exhibiting the complications of dealing with thin solid ultrasonic delay lines rather than bulk liquid mercury.

Another approach was simply a superconducting coaxial delay line. I set up in my little lab means for insulating and jacketing fine niobium wire in a lead coating, so I could spool miles of that superconducting coaxial cable and store five or eight microseconds per mile at gigabit rates.

There was a spin-echo storage project under Bob Walker when I joined the Lab, involving also Watson Lab physicist Erwin Hahn, and I helped out with that informally, showing that one didn't need a recollection pulse but could, in principle, reverse the field gradient that provided the distributed storage. We were able to demonstrate the capability of thousands of bits of proton storage in Wild Root Hair Oil (an emulsion of water in oil --important that it is not the other way around) in order to avoid rapid loss of coherence due to unbounded diffusion. This "reversal of gradient field" was patented by IBM in my name and is used in every medical MRI (magnetic resonance imaging) system unfortunately long after the 17-year expiration of the patent.

The potential for paramagnetic spin-echo storage was many megabits, and that was worth talking about.

You were director of Applied Research at IBM. Can you give a sense of the challenges of curating early computational resources in the lab?

As Director of Applied Research in IBM Research, I had charge of all integrated circuits, all types of memory, and a lot of other things.

It was kind of an accident that I was Director of Applied Research. The Director of Research at the time, Gardiner L. Tucker, had twisted my arm and appointed me Director of Pure Science, and Rolf Landauer Director of Applied Research. We shared an office suite, each of us having a large office connected to the central room, in which each of our secretaries worked. When I showed up for the job, driving from Scarsdale to Yorktown Heights instead of to Manhattan, I faced the question of an agenda and realized that I was much better at directing applied research then I was at pure science, so I asked Rolf on the spot whether he would switch, and we did.

One of my first actions as Director of Applied Research was to make three half-million-dollar expenditures – one to buy a 500-kilobit core memory for use in a time-shared system that would be available to all researchers – CP/CMS, offering also APL, an elegant language created by Ken Iverson and colleagues in IBM Research. Another was to buy an electron microscope, and the third an ion probe or deposition system.

The CP/CMS was an excellent system and quite efficient. I made it a condition of continued employment at the Watson Lab, that all the professional staff take the course in APL on this time-shared system.

Directing Applied Research fit me quite well, and I did it for about a year, being very much involved (more than half-time, really) with many matters in Washington, including the space intelligence systems of imaging satellites, ELINT satellites, chairing the President's Science Advisory Committee's Military Aircraft Panel and its Naval Warfare Panel, as well as being a member of the Defense Science Board, advisory to the Secretary of Defense.

You had a parallel career advising the government on science and engineering solutions to pressing issues. Can you give some examples of solutions you pioneered?

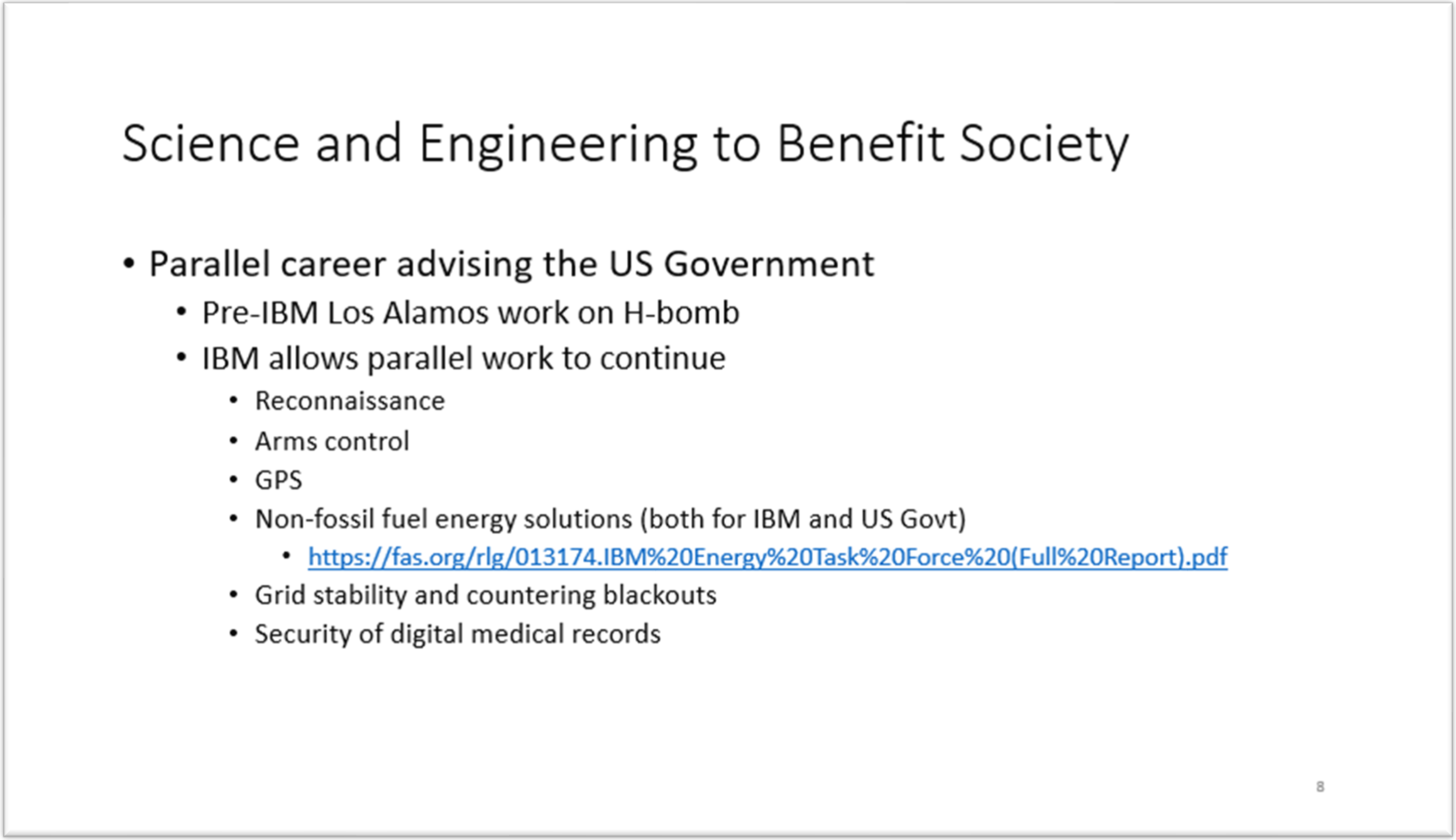
In my government service, I really didn't work on science policy, except rarely, but mostly on providing solutions to real problems or taking concrete steps to capitalize on real opportunities.

- (his name not publicly associated with the H Bomb until 2001)

One of these was the hydrogen bomb, where in going to Los Alamos for my second summer of consulting in ~May, 1951, I asked Edward Teller what was new, being unable to talk with him about classified matters on the faculty at the University of Chicago, but I could when I saw him again at Los Alamos. He told me of his publication at Los Alamos of a Secret document March 9, 1951 with mathematician Stan Ulam, *"On Heterocatalytic Detonations I: Hydrodynamic Lenses and Radiation Mirrors."*

Edward felt that this was finally the way to make a hydrogen bomb and he implored me to devise an experiment that would be persuasive to even the most skeptical that this was the way to go.

I tried to devise a “small” experiment—e.g., one with the explosive yield of the Alamagordo/Nagasaki nuclear weapon -- but decided that the surest thing and the way to make most progress was to use the enormous resources at Los Alamos to build a full-scale test device, which was exploded as the MIKE shot of the Ivy series in the Pacific on November 1, 1952 with the yield of almost 11 Mt – 500 times the yield of the Hiroshima or Nagasaki bomb.



Similarly, my work in the later period after I had moved to IBM, and, for instance, when I was Director of Applied Research, 1965-6, I was working on the particular implementation of electro-optical imaging for satellites to replace the film-return systems – CORONA, operating from 1960 to 1972 and now fully declassified both as to the technology and the images; HEXAGON and GAMBIT film return systems following CORONA, whose cameras and spacecraft are in the Air and Space

Museum in Washington, but which have not been fully declassified, all of which have been replaced by systems that transmit the images by radio to the ground, and it is those on which I had very substantial specific impact.

I was working also on the details of what became the GPS system and on aspects of military aircraft and ships, as befits my chairing of the Military Aircraft Panel and the Naval Warfare Panel of the President's Science Advisory Committee which until 1973 met two days every month in the Old Executive Office Building, as did each of my Panels. I was very busy – spending half-time on this government work. This was possible because my initial IBM employment contract permitted me to spend one-third of my IBM-paid time (plus weekends and vacation) helping the government on technology for national security. I did work a little on science policy as a member of PSAC itself – during those meetings, and also in chairing two specific committees for the National Academy of Sciences – the design and function of the Solar Energy Research Institute (SERI) [now National Renewable Energy Laboratory – NREL] and then the 1977 review of the agreement between the United States and the Soviet Union for cooperation in the fields of science and technology – a parent agreement for many in individual fields, which latter automatically renewed, but the parent agreement did not. I had to devise a scheme in which in just a few weeks we could get a sound basis for evaluating the effectiveness of this 1972 agreement, which we did.

How did you contribute to the understanding of energy production for the nation?

I did work on various aspects of energy, both in the 1977 book "Nuclear Power Issues and Choices" (the Ford/MITRE study) and then one for resources for the future, "Energy: The Next Twenty Years." And then I had chaired in 1974 the Energy Task Force for the Research Division, the report of which is available on the Garwin Archive.

> "Report to the Director of Research by the Energy Task Force," R.L. Garwin (Chairman), et al, January 31, 1974. Formerly IBM CONFIDENTIAL-- declassified 01/09/90 by RLG.
> https://fas.org/rlg/013174.IBM%20Energy%20Task%20Force%20(Full%20Report).pdf

I worked also in countering blackouts, particularly beginning with the 1965 Northeast US blackout for which Governor Rockefeller employed Edward Teller to help prevent future such events. I was caught in it myself, and became a fan of "islanding" the ability to cut power lines and have immediate fallback on whatever local generation exists, making use of special contracts with industry instantly to cut a large fraction of power being delivered. If there is excess generation, then one could switch in cast-iron or water resistors to absorb the excess generation until it could be turned down, and if there was excess load, one would selectively and immediately shed load. As in so many real problems "The best is the enemy of the good enough." and islanding has always been reserved for the great by-and-by.

You also wrote some early recommendations about the security of medical records and computerizing these records. Can you tell us about the origins and outcome of that research?

I served for six months on the New Technologies Panel of the National Commission on Health Manpower, that Panel being chaired by Albert D. (Bud) Wheelon, who was at Hughes commercial communication satellites, having retired from his position as the initial Deputy Director of CIA for Science and Technology – DDS&T. I had worked with Wheelon in 1958-9 in Geneva as members of the US delegation to the UN-sponsored Conference on Prevention of Surprise Attack.

The New Technologies Panel of the National Commission on Health Manpower visited six or so research institutes and hospitals including a couple of Kaiser Healthcare facilities in California, the Karolinska Hospital in Stockholm, and the like. I decided that the problem was not the absence of new technology so much as the inability of applying best practices, such as were available at the Karolinska, and just as in my design of the hydrogen bomb, I designed a healthcare system that could be implemented then and did use best practices; but it would require people to change what they were doing, which was not very popular.

I can't emphasize strongly enough that my work in all these fields was in actual invention, prototyping, and development of real things – including touchscreens, laser printers, and the like – some of them useful immediately both for government and IBM purposes.

What about fossil fuels and energy generation capacity now?

I worked a lot on nuclear power, but nuclear power is just too expensive for us as practiced in the United States. And we have continuously deferred the creation of a mined geologic repository for the highly radioactive "spent" nuclear fuel, which should be done on an international commercial basis, but is mired in political problems.

I have worked some on wind power and solar power. Rooftop solar and distributed distribution including charging of batteries for electric cars is now a booming industry, and we need the evolution of more battery capacity but there are things that can be done for instance with offshore wind, in generating hydrogen by electrolysis and storing the hydrogen essentially in open bottom tanks or cups in ocean water, which replaces the need to build high-pressure storage vessels for the hydrogen. I did not invent this idea – just admire it.

I really don't have time to say how the grid infrastructure should be updated. I've spent years and a lot of effort with OSTP and elsewhere in trying to implement such a simple thing as neutral-current blocking devices -- https://fas.org/rlg/grid-emp.pdf -- for long-distance primary power transmission, for which Y-connected transformers are very vulnerable to geomagnetic storms or also to space nuclear explosions. But I can protect the transformers perfectly against such hazard – turning off the power only for a minute or so after a space nuclear explosion, and making a very modest investment to handle all but the very biggest geomagnetic storms (caused by coronal mass ejections from the Sun engulfing the Earth's magnetosphere.)

TURN TO THESE QUESTIONS IN THE LAST 5 MINUTES OR SO:

What areas of research are most interesting to you today?

I really do believe we need to work on preservation of human civilization against some natural but also largely human activities, and I've been doing this now for at least 40 years with the Erice (Sicily) Ettore Majorana Center for Science and Culture, beginning with three years, 1981-3, of summer meetings on preventing a nuclear war. After 9/11 I chaired for many years a Permanent Monitoring Panel – PMP – on Mitigating Risk of Terrorist Acts. This evolved not only to handle terrorist-induced epidemics, but also natural pandemics. This should not be taken as a judgment that this is the most important thing for everybody study; we just need to pay attention and be honest about it.

What is your advice to our young colleagues for successfully navigating a research career?

I am so far distant from the modern scene that I can't be of much help. Not that I don't work with young colleagues, mostly as part of the JASON Summer Study. About 15 years ago I worked with a professional from the Institute for Defense Analyses – IDA – who was part of the JASON Summer Study on simulating pulsed x-ray systems for monitoring the details of the chemical implosion of nuclear weapon primaries. We built an unclassified code that would obtain from the pulsed radiographic images densities of the uranium, plutonium, and the boundary layers among them, in order to guide the creation of the DARHT system at Los Alamos – the Dual Axis Radiographic Hydro Test facility.

And just this summer, I worked with another Fellow from IDA, on an idea I had for automatically producing terrain maps from the historical CORONA stereo imagery, which turns out to be quite a hot topic. In fact, with the use of the consumer imaging drone quadcopters such as those made by www.dji.com, one can use multiple images along the programmed GPS-navigated straight flight path in order to do this for not only terrain maps but for managing stockpiles of ore, grading of terrain, and the like. But as for dealing with employers, I am at a total loss.

I just comment that reading carefully composed documents on a smart phone rather than on a computer leads to false impressions and misunderstandings.

National Medal of Science

In recognition of his research and discoveries in physics and related fields, and of his longstanding service to the Nation by providing valuable scientific advice on important questions of national security over a half a century. Also affiliated with IBM Corp.

Presidential Medal of Freedom

"Dick's not only an architect of the atomic age. Ever since he was a Cleveland kid tinkering with his father's movie projectors, he's never met a problem he didn't want to solve. Reconnaissance satellites, the MRI, GPS technology, the touchscreen all bear his fingerprints. He even patented a mussel washer for shellfish—that I haven't used, the other stuff I have. Dick has advised nearly every president since Eisenhower, often rather bluntly. Enrico Fermi, also a pretty smart guy, is said to have called Dick the only true genius he ever met." President Obama

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