Serendipities from Long Ago

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Keynote Address for the Harvard Physics Department
Post-Doc/Research-Scholar Retreat
Lion Hall, Cohasset, MA

September 11, 2019

I am very pleased to have had the opportunity to spend the day absorbing information from the work of those present at the cutting edge of research and understanding. And maybe gathering some tips on more mundane aspects of technology and life.

It is also a pleasure for me to relate to you some of what I found interesting in my past contributions, of which I have chosen four for a brief exposition, together with a few slides.

My thesis for a Ph.D. with Enrico Fermi in December 1949 was to do the first experiment looking for angular correlation between the electron in beta decay and the ensuing gamma ray from the excited nucleus. Gamma- γ correlations following beta decay had been measured for several years by then, but there were experimental difficulties in measuring β-γ angular correlation.

In my graduate work at Chicago from 1947-1949 I had volunteered to work with Prof. Fermi because I missed experimental work, so I helped him in his lab and was delighted to find that he was a hands-on experimenter, as was I, with a lathe in his lab and a power hacksaw. He enjoyed making his own equipment. I did my part in observing that coincidence circuits of microsecond resolving time were inadequate for forthcoming work with the cyclotron or other pulsed accelerators; so I devised and publishing coincidence circuits and coincidence-anticoincidence analyzers with few-nanosecond resolving times. All I needed to do in the vacuum-tube Rossi circuits was to add a semiconductor diode so that the 20-mA of common anode current in the coincidence pair of tubes would cause a voltage change of only one volt across the diode, whereas cutting the current in both tubes would create a voltage rise that would exceed another volt in a few nanoseconds. This provided highly reliable multiple-coincidence-anticoincidence analyzers with no tuning or adjustment necessary.

In his lab, Fermi and his young colleague, Leona Woods Marshall, were doing experiments on the lifetime of positronium, with Geiger tubes, that they sealed and filled themselves, each with a thread containing a bit of Na-22 solution—a positron emitter. But they were scooped by Martin Deutsch of MIT, who had access to two-inch end-window photomultiplier tubes, under development at RCA, viewing organic-crystal scintillator. Fermi got some of the RCA tubes for his work, and I immediately put them to use in my thesis. Along with the fast coincidence circuits, the end-window photomultipliers increased the rate at which I could take data by a factor 100, which I and my wife welcomed.
      So that's how a simple semiconductor diode advanced the fields of particle and nuclear physics.

That was fun. I used these technologies and some innovations in scintillation counters in my work on the external pion beams of the 450-MeV Chicago synchrocyclotron. But I didn't like the sociology of particle physics with the necessity to tell the scheduling committee six weeks in advance the experiment you wanted to do with the cyclotron, and to work with perhaps six other people on the experiment, so I decided to change my field to cryogenics—superconductivity and liquid and solid He^4 and He^3, and chose to leave Chicago for what I thought was a better city environment in New York, to work at the newly created IBM Watson Scientific Laboratory at Columbia University, where I could do whatever I wanted in science and technology—just so that it was valuable to the IBM company or outstanding science, and would fit into a small lab.

At IBM, I worked with the tools of nuclear magnetic resonance (spin echoes) on liquid and solid He^3 and its alloys with He^4, down to about 0.3 K, but that is not what I want to tell you about.

In August 1956, two of my former graduate student colleagues from Chicago, T.D. Lee and C.N. Yang proposed a solution to the puzzle of the τ and θ mesons decaying with the same lifetime and apparently with the same productions cross-sections, no matter how they were formed. Because the θ decayed into two pions and the τ into three, they could not be two decay paths for the same particle, or so all the wisdom of physics taught us. Because if a single particle decayed with some branching ratio on these two paths, it would not have a definite parity; or parity wouldn't be conserved. Lee and Yang created a revolution in our physical understanding with their rightfully famous October, 1956, paper that won them the Nobel Prize in December 1957, after confirmation in the beta decay of Co^{60}, and in the pi-mu-e decay chain.

At the time of the Lee-Yang parity paper, I was leading some 80 researchers at three IBM locations, to build a superconducting computer of thin-film cryotrons, operating in liquid helium at 4 K, with perhaps 10 ns cycle time. So I resisted involvement in experimental work in my old field of particle physics, even though I had been asked by Columbia Prof. C.S. Wu whether I could help her with a demagnetization refrigerator to polarize Co-60 so as to test whether the beta decay occurred preferentially along the spin or opposite to it, as was suddenly conceivable under the Lee-Yang hypothesis. And I similarly resisted the urge among my Columbia particle-physics friends somehow to look at a sample of stopped muons (in photographic emulsion) and to see whether electrons were emitted preferentially along or opposite to the spin, although it was not known, really whether muons had spin ½ or perhaps spin 3/2, and the extent to which each muon born of the 20ns lifetime pion at rest would be polarized along its initial velocity vector—an impossibility if parity is conserved.
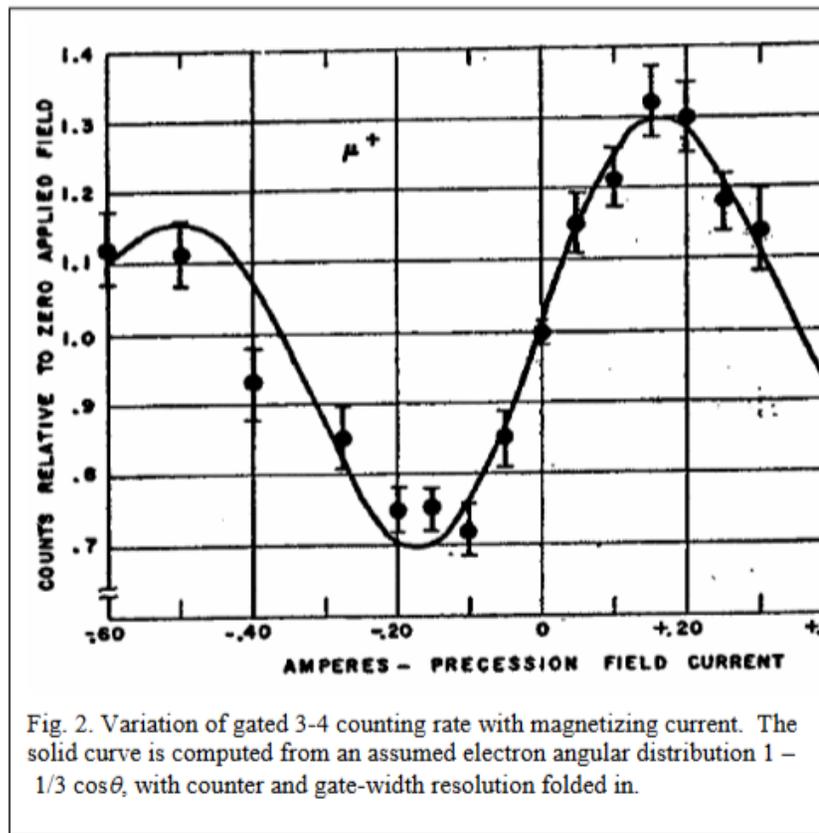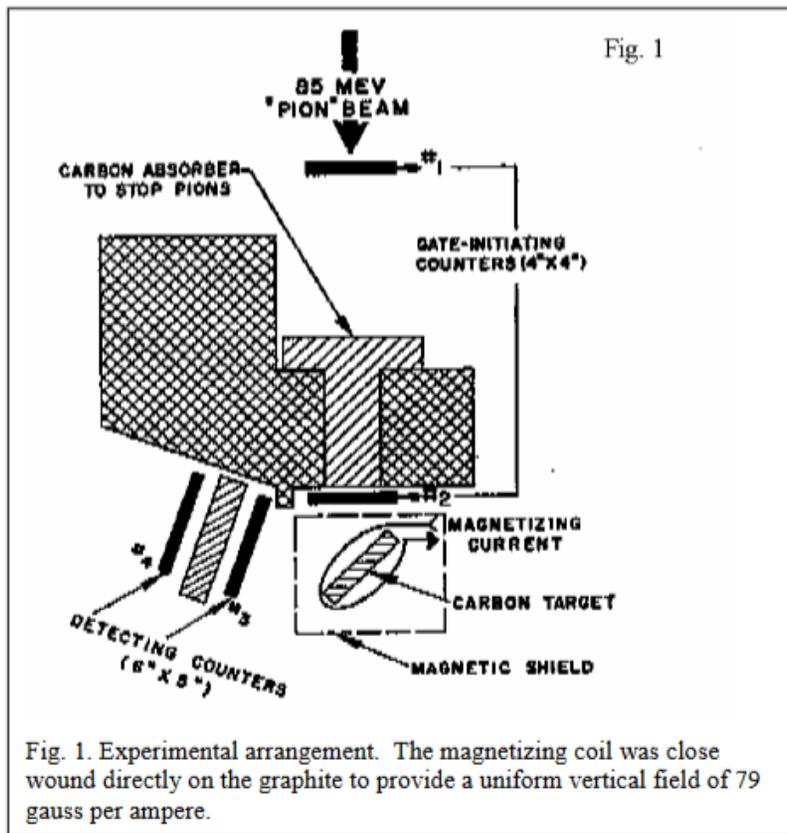
In moving to New York City in December 1952. I had had the foresight to have IBM buy from Chicago's excellent electronic shop one of the coincidence-anticoincidence analyzers that I had built and had been using at the Chicago 450-MeV cyclotron, and I eventually contributed it to Leon Lederman at the Nevis Cyclotron Laboratory of Columbia. Leon had a graduate student, Marcel Weinrich whose thesis wasn't going very well, despite his using my coincidence circuits and photomultiplier-detected scintillation counters with which I was very familiar. Somehow, Marcel's measurements of the decay of muons stopped in a graphite block in the external muon beam of the Columbia cyclotron did not show exponential decay with a fixed half-life as was dictated by quantum mechanics.

On Friday night, January 4, 1957, Leon called me at home to tell me that he had realized that the muons that fed the external beam from the Columbia cyclotron were already polarized because they had been born from the forward decay-in-flight of the pions created by the protons striking the internal target of the cyclotron, and that therefore the muons stopped in Marcel's apparatus might already be polarized; if so, all we needed to do was to find a counter that could be swung around the graphite stopping block, in order to determine the spatial distribution of the decay. This assumed, of course, that the muon spin direction survived the substantial equivalent electric field of the "stopping process" – some 10MV/cm--and the internal fluctuating magnetic fields of the material in which the muons had been stopped.

I agreed to meet Leon at 8 p.m. at the cyclotron that night, and we worked until perhaps 10 am Saturday morning when the cyclotron shut down for weekend maintenance. We could find no way Friday evening to remotely swing a counter around the distribution of stopped muons, so I employed my knowledge of nuclear magnetic spin resonance-- not in a resonance form but in a simple precession experiment, having a "decay-electron telescope" at fixed angle and winding a coil around the graphite stopping block in order to provide a steady magnetic field that would precess the muon spin (if there were one and if it were not disturbed by internal fluctuating magnetic fields). In fact, by 7 a.m. Saturday, as I recall, we had a good signal, but in another hour it vanished. When we went down to turn off the 2-kV power supplies to the counters in the experimental hall (which

we could not occupy while the beam was on), we discovered that the thermal expansion of the coil of fine copper wire I had wound on a Lucite cylindrical coil form had allowed the coil to drop to the bottom of the vertical cylinder, so it no longer produced the desired axial magnetic field. Haste makes waste.

By Monday evening when the cyclotron resumed operation after its long weekend shutdown, we had wound the coil directly on the rectangular graphite block and by early Tuesday morning we had 22 standard deviations of significance and had written the paper to be published February 15, 1957 in Physical Review Letters, accompanying report of the Co-60 anisotropic beta-decay by Ms. Wu and her colleagues at the National Bureau of Standards.

Fig. 1. Experimental arrangement. The magnetizing coil was close wound directly on the graphite to provide a uniform vertical field of 79 gauss per ampere.

Fig. 2. Variation of gated 3-4 counting rate with magnetizing current. The solid curve is computed from an assumed electron angular distribution 1 − 1/3 cosθ, with counter and gate-width resolution folded in.

Serendipity was important to our success—that I had devised these experimental techniques in the previous decade, but also the two independent recognitions—by Leon that the muons were already polarized, and mine—that the muon spin and the decay distribution with it, could be swung by

the ambient magnetic field, and that the decay distribution could be averaged in a classical way—a perception disputed by W. Pauli when, soon after, I presented the results of our experiment in a colloquium at the ETH in Zurich. Of course, our result explained the problems that Marcel Weinrich had been having, as caused by the 2:1 forward/back decay asymmetry of the muon, together with the precession of the muon spin in fringing magnetic field of the cyclotron at the site of the stopping block.

So that's the story of our demonstration of failure of parity conservation in the weak-interaction decay of the pion and of the muon.

Example 2 of serendipity: perhaps of current interest in view of the launch of LightSail II by the Planetary Society and the deployment of its solar sail on July 24, 2019. As physicists do, I had been thinking about how things worked or could work and learned about radiation pressure, as did everybody in high school. Then I worked at Los Alamos, beginning in 1950 as a faculty member of the Chicago Physics Department, and dealt with much larger radiation pressure from nuclear explosions—truly large pressures because of the few kilo-electron-volt temperature in the plutonium or uranium core of a fission explosive[1].

When the first issue of the IBM Journal of Research & Development was to be published in 1956, I wrote up an idea that had been percolating with me for a long time, and which I had explained to the rocket and satellite folks at General Dynamics Corporation in San Diego, for which I served briefly on an advisory committee; here is a photo of a session of 09/21/56.

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[1] One eV as kT is 11,600°K, so the Sun's surface temperature of 5,800°K is about 0.5eV, compared with about 5 keV in the metal core of a fission explosive. This factor 10^4 in temperature, together with the Stefan-Boltzmann T^4 law, corresponds to a factor 10^{16} in radiation pressure or black-body energy density of the electromagnetic field.

GRAD
CHOU   NICHOLS GARWIN GAMOW   WHIPPLE WIGNER SHERWIN   SEBOLD   FERRI   PLESSET   WHEELER   TELLER CRITCHFIELD
                              COURANT   VON KARMAN   CASE   NEY MEHL

I believe that Wernher von Braun was also on the committee, together with people I had known from Los Alamos, and I suggested to them that solar radiation pressure would be a useful and practical means of propulsion within the solar system—from LEO to solar orbit, to visiting the planets.

Needless to say, people were too impatient at the time, although solar sailing would have *shortened* mission durations compared with chemical propulsion in visiting Mars, or any of the other inner planets. The editorial staff of the IBMJ judged that my paper should not appear in the first issue, because it was not the sort of serious research that should be presented there, so I submitted it to the Journal of the American Rocket Society, where it was published in 1958[2]. Here is the beginning of the paper,

Solar Sailing—A Practical Method of Propulsion Within the Solar System

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It is shown that commercially available metallized plastic film can be used as a solar radiation pressure sail for propulsion of space vehicles within the solar system. The method of propulsion is of negligible cost and is perhaps more powerful than many competing schemes.

IT IS difficult to exaggerate the importance of solar radiation pressure for the propulsion of satellites or space ships within the solar system; but since I have never seen any allusion to this powerful method, while less practical and more difficult schemes are frequently cited, I feel it desirable to publish this paper.

The principle involved is simply to make use of the pressure of the sun's light on a sail to propel a space ship as desired through the solar system. What is important is the area of sail per unit mass, and if, just as a practical example, we use a commercially available 0.1-mil-thick plastic sail equal in mass to the rest of the "space ship," the mass per unit area will be 5×10^{-4} g/cm^2. The sail may be aluminized without significant additional mass, in which case the sun's radiation

sence of holes, tears, flaws, etc. The reflective coat plays a perhaps important role in eliminating electrostatic forces, which might easily prevent opening of the sail. The sail may be attached to the ship by "shroud ribbons" ~ 200 meters long. Since the forces on the shroud ribbon will not exceed 2 grams weight, the ribbons may be narrow strips of the same material as the sail. Let us for the moment grant that the sail may be expelled from its packing container after the "space ship" is in a satellite orbit at reasonable altitude. It is of interest to compute the time required for the sail to "fill with sunlight." This is the time for the sail to go from a flat circle to a dished configuration of the order of 10 meters deep and is

$$t_f \sim \left(\frac{2 \times 10^3}{0.32}\right)^{1/2} = 80 \text{ sec}$$

Thus the sail may be furled and unfurled quickly compared with the 90-min period of its orbit around the earth.

The simplest program for increasing the altitude of the satellite, at the same time increasing its energy and *reducing* its velocity about the earth, is to furl the sail by slacking half the shroud lines while the satellite approaches the sun and to unfurl the sail while receding from the sun. Clearly then, the mean projected acceleration along the velocity vector is $\pi^{-1} \times 0.16$ cm/sec^2 or 5×10^{-2} cm/sec^2, and the rate of decrease of orbital velocity is then $\theta = 5 \times 10^{-2}$ cm/sec^2. Thus, an initial velocity of 8×10^5 cm/sec will decrease to zero in a time on the order of $(8 \times 10^5)/(5 \times 10^{-2}) = 1.6 \times$

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[2] https://fas.org/rlg/030058-SS.pdf

I recall that when the Chief Scientist of the U.S. Air Force was asked about this proposal at a press conference, he explained that even if it would work, it could only be used for going outward beyond Earth orbit around the Sun and not for going inward, because radiation pressure was radially outward from the Sun. What he missed, of course, was that the fact that the sail was in Earth *orbit* or, for that matter solar orbit, meant that a reflective sail could be angled so as to provide a force perpendicular to the sail, that would have a component either along the velocity vector or in the opposite direction, so that the orbital velocity component could be increased or reduced; thus, the SS could either gain or lose energy and so spiral in or out from the Sun, or in Earth orbit.

In November 1957-January 1958, I was in Geneva for six weeks on the U.S. delegation to the Ten-Nation U.N. Conference on Prevention of Surprise Attack, and I had occasion to propose the deployment of telegraph-relay satellites in GEO, in support of a potential treaty on prevention of surprise attack. The relay satellite would be fed by U.S. "airfield watchers" or "silo watchers" in the Soviet Union. I visited the first head of ARPA, Herb York, whom I knew from my work at Los Alamos in 1950 on nuclear weapon testing, and also the first head of NASA, T. Keith Glennan, whom I knew because I had earned money by drafting building plans for him when he was President of Case Institute of Technology in Cleveland, from which I graduated in 1947, but neither one of them picked up the opportunity.

Can't say that solar sailing has yet made it as a practical means of propulsion of spacecraft within the solar system, but it may—after 60 years of gestation.

However, although the Conference on Prevention of Surprise Attack agreed only on its title and not on its agenda, the simultaneous Conference of Experts on the Detection of Nuclear Explosions (in space, underground, in the oceans, and in the atmosphere) did have much more substantive discussions, and I was involved with teleseismic detection of underground nuclear explosions as a result, as well as the detection of distant space explosions—but that is a longer story.

In the early 1960s I continued the decade or more of work I had done at IBM in spin-echo measurements of self-diffusion in He^3, and as we squeezed the solid helium to double density, the exchange interaction that we used to explain self-diffusion decreased from 10 MHz to a few Hz. We looked for an explanation of this striking result—having expected the exchange interaction between nuclear spins to increase as the adjacent atoms approached one another.

More serendipity. I used to sit next to John Tukey, famed statistician at the Bell Telephone Laboratories and Princeton University, in monthly meetings of the President's Science Advisory Committee (PSAC) and this time he was writing Fourier sums, which seemed to me to be of help in analyzing an internal spin wave in my *hcp* He^3 crystals. Tukey explained to me that one of our colleagues in Britain, I.J. Good, had an idea for doubling the number of points in a Fourier series without quadrupling the number of multiplications required for the calculation; more generally, the number of multiplications would go as N log N, and not as N^2. For my 20,000 spins, this would be the difference between 400 million multiplications and something like 200,000, and in other emerging applications of which I knew from my work with PSAC, perhaps involving digital images of a million pixels or more, the difference between 10^{12} and 3×10^7 multiplications—quite a difference. I suggest that the FFT is as big an advance over the previous computer calculation of Fourier transforms as arithmetic is an advantage over counting.

I disavow any claim of intellectual contributions to the result, but I did arrange to have an IBM mathematician/programmer visit John Tukey and produce a Fortran program for the Fast Fourier Transform published as the Cooley-Tukey paper in 1965. The history of the FFT was written up from

the viewpoints of several participants in a special session of the IEEE Transactions on Audio and Electroacoustics and has long been available in my archive, at https://fas.org/rlg/690600-fft.pdf

You might believe that Cooley's FORTRAN implementation of the Cooley-Tukey FFT would be avidly adopted by researchers and others, but I think that if I had not written some scores of letters, individually pointing out to various people who had published work that would benefit from the FFT, it would have taken much longer to gain currency.

We learned that similar methods had existed since the 19th century, and had even been used in the 1930s by L.H. Thomas, who had the office next to mine at the IBM Watson Scientific Laboratory, when I first arrived in December, 1952.
    So that's the origin story of a tool as powerful in its not-so-limited domain as many generations of computer development.

Finally, something that you all have used, perhaps without realizing it, and that is low-noise amplifiers and charge detectors, in this case for digital imaging. Since I was a kid, working in the home shop my father had had built behind the double garage of the home to which we moved when I was 12 years old (1940, from which he operated a business with his brother—Gartec, the Garwin Theater Equipment Corporation—which installed and serviced audio and visual equipment for schools, universities, and industry in the area of Cleveland, Ohio, I had repaired and built audio amplifiers, and was fascinated with low-noise, sensitive microphones. I soon learned about thermal noise, as well as shot noise. I learned also that thermal noise, although it could not be eliminated, could be pushed outside the frequency band of interest. I did this later in some work on ion chambers at the University of Chicago cyclotron, copying designs of (William) Elmore and (Matt) Sands from their 1949 book on electronics, reflecting their wartime work at Los Alamos.

When IBM introduced the laser supermarket scanner, using a He-Ne laser illuminating a spinning disk that carried holograms, to read the "grocer's code" on packages; it used photomultiplier tubes to detect the reflected light with typical 10 MHz components, I took note and may even have helped a bit.

But then IBM progressed to more convenient and cheaper solid-state diode lasers, with detection by large-area solar-cell semiconductor diodes, and lo and behold, the capacitance of the solid-state junctions, gave inadequate signal-to-noise ratio—SNR.

One of my colleagues on the program told me about the problem, and I asked them to bring the amplifier to my lab. I looked at it, and, of course, it was built the way any sensible person would build an amplifier, with a resistor of small value across the detecting capacitance, in order to preserve the rapid transitions (10 MHz) of the photocurrent as the laser beam scanned the grocer's code.

I had suspected this was the problem, and snipped the resistor (you see how long ago this was!), replacing it by one a hundred times larger, and adding a small differentiating capacitance and resistor at the end of the amplifying chain, restoring the same transfer function, but moving the thermal noise (which was still of magnitude Q^2/C) but was now far down from the signal in the signal-frequency band. Really this is the equivalent in LIGO of mounting the mirrors on very soft pendulum supports, which does not reduce the kT kinetic-energy noise in each mirror degree of freedom, but it moves it entirely out of the relevant band of, say, 100 Hz. The fix for the second-generation supermarket scanner was gratifying, but it would not have been a disaster if that program had failed or if one had to retain PMT detectors.

But when I was working on a program for digital imaging, with many pixels, scanning an image or a scene over a line of photodetectors, each with its own amplifier, I recognized there might be some similarity between this problem and the others with which I had dealt in the realm of analog signals.

My previous work, although it was applied to the digital grocer code, was considered from the analog, frequency-spectrum point of view, but was there some similar approach that would be helpful in reducing the noise (charge fluctuation) acquired by each pixel capacitor from the few-ms exposure of the photo-detecting pixel? The straightforward approach was to reset each pixel to zero charge, then measure the charge accumulated during the exposure to the scene (the Moon, for instance) and to record or transmit that information, pixel-by-pixel as is done in monochrome TV. But the "reset" does *not* reset to zero charge, but to zero voltage, and the capacitor is inevitably left with an energy *kT/2,* at room temperature, *T,* Boltzmann-distributed.

Shorting the capacitor and then opening the circuit to accept charge from photons detected ensures that after each reset there is a sampling of charge noise from the Boltzmann distribution, with a noise charge Qn such that the noise energy is $Q_n^2/2C$, where C is the stray capacitance associated with the photodetector. We are assured that Q_n, positive or negative, is such that this quantity squared, averaged over multiple samples, equals *kT/2.* In this case, as I recall, the stray capacitance was such that the rms noise charge was on the order of 200 *e*.

It occurred to me to read the charge on the node immediately after unclamping the node from ground and then read it again at the end of the exposure time, resetting the node to ground only after the second read. As I recall, this reduced the rms noise charge to about 20 *e*, allowing one to have a noise-equivalent signal of that magnitude rather than 200 *e*. This enabled 10 times faster scanning, or scanning at the same speed with ten-fold dimer light.

Ultimately this became standard, not only in astronomy (where it was proudly shown to me decades later by the supernova search team at Berkeley), but also in all consumer digital cameras.

"Write once, read twice," is what I had written in my notebook in December, 1969.

I've been a practitioner of scientific research and innovation, not a philosopher, so I draw no explicit conclusions from these tales. But I'm eager to discuss further with you.