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# KENNETH I. GREISEN
1918-2007

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*A Biographical Memoir by*
VIRGINIA TRIMBLE

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*Biographical Memoir*

Kenneth Greisen

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*January 24, 1918–March 17, 2007*

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KENNETH INGVARD GREISEN (variously Greisen, KG, and KIG below) was the G of the GZK effect (Greisen, 1966a; Zatsepin and Kuzmin, 1966), and this has probably overshadowed his other contributions to cosmic-ray and gamma-ray physics and astronomy. The GZK effect is the predicted steepening of the power law spectrum of cosmic rays for primary energies above $5 \times 10^{19}$ eV, finally seen in 2007 (Abbasi et al., 2008; Abraham et al., 2008). The loss of these high-energy primaries occurs because they have to get to us through a sea of 2.7 K photons, the cosmic microwave background (CMB), and, colliding with the photons, they shed energy by producing pions.[1] At lower energies, around $10^{18}$ eV, electron-positron pairs are produced, but with smaller cross-section, so that only a dip, not a cutoff, in the spectrum is predicted, and seen. It is poetic justice that the expected cutoff was finally confirmed by detectors (HiReS near Dugway Proving Ground in Utah and AUGER in Argentina) using a technique that KIG had independently co-invented and pioneered, atmospheric fluorescence by $N_2$ molecules.[2]

Greisen, born in Perth Amboy, New Jersey, on January 24, 1918, was the middle of three children of Signa and Ingvard Greisen. The previous generation had come from the Schleswig-Holstein area and thought of themselves as Lutheran and of Danish heritage (Greisen, E. W., 2010). The "en" ending is a giveaway for Danish names, shared by my grandmother's father, a Paulsen, and her mother, a Rasmussen, whose father also came from Schleswig-Holstein, as did the ancestors of Edward Ohm, prediscoverer of the CMB, whom you will meet again soon (Ohm, 1961, 1995).

KG began his postsecondary schooling in religious studies at Wagner College on Staten Island, then closely connected with the Lutheran and Reformed Churches. (Even now it has only a two-person department of chemistry and physics.) After a year, he transferred to Franklin and Marshall College in Lancaster, Pennsylvania, also originally founded to educate clergymen and connected with the same churches until the 1960s. But it had instituted a science curriculum as early as the 1880s, and KIG graduated from this in 1938, summa cum laude, with the prestigious Henry S. Williamson Medal and election to Phi Beta Kappa (the liberal arts honorary), Sigma Xi (the scientific honorary), and Phi Kappa Phi (an honorary extending over all disciplines). These and his 1974 election to the National Academy of Sciences were the only honors that Greisen typically mentioned in autobiographical passages.

The European heritage was preserved in what KG called his parents' siblings, Tante Marie and Tante Agnes, and in the names of his own siblings, Sigurd (an accomplished but troubled studio photographer) and Agnita (later Dupree). Both parents, both siblings, Greisen's first wife, Elizabeth ("Betty") Chase (m. 1941, d. 1975) and second wife, Helen Wiltberger (m. 1976, d. 1996), predeceased him. He was survived by son, Eric W. Greisen (a radio astronomer who spent much of his career at the National Radio Astronomy Observatory), and daughter, Kathryn, born in 1944 and 1946, while the family was at Los Alamos, and by two Wiltberger stepchildren and several step-grandchildren.

Greisen's own later life was dogged by ill health, including a heart attack in 1971, a stroke in 1984, and colon cancer in 1991 (Greisen, 1995). This perhaps contributed to his absolute retirement from physics when he reached emeritus status at Cornell in 1984. But almost until the end of his life (on March 17, 2007, at the hospice care residence in Ithaca, New York) he maintained an active involvement with the surrounding community, as a member of church choirs, a driver for the elderly and infirm, an income tax adviser for less numerate friends, and much else.

## GRADUATE SCHOOL AND LOS ALAMOS

With his impressive B.S. fresh in hand, KIG arrived at Cornell for graduate work in physics, though what sort of physics he was not at all sure. He had declared for theoretical work, having no laboratory experience, but an encounter with Bruno Rossi was transforming (Greisen, 1970). Rossi had arrived at Cornell in 1940 from Chicago (where he had, briefly, one student, so KIG was not quite his first in the United States) and from Italy in 1938. Greisen became Rossi's first Cornell student. The apparatus they built, centered around a Geiger-Müller counter, was used to distinguish the altitude dependence of hard (mesotron[1]) and soft (electron) cosmic-ray secondaries (Greisen, 1942) and to confirm that the lifetime of the mesotrons (mu mesons, or muons) depended on their energy as predicted by special relativity (Rossi et al., 1942). The data had been taken at altitudes of 249, 1616, 3240, and 4300 meters around Echo Lake, Colorado, and the Greisen family returned to Echo Lake on holidays after World War II. One of KIG's more private discoveries was that vacuum tubes have polarity (when he accidentally plugged one in the wrong way around).

The theoretical work was also a baptism of fire, with the resulting review of cosmic-ray theory much cited for decades afterwards (Rossi and Greisen, 1941). If your memory is no better than mine, you may be surprised to find the authors describing the theory of the electromagnetic interactions that they used as quantum electrodynamics and crediting the name as well as many of the ideas to Dirac (1927).[3] They also cite Maxwell, Heisenberg and Pauli, and Wendell H. Furry and J. Robert Oppenheimer. KIG described his relationship then with Rossi as "master and flunky," each day beginning with Rossi giving him a list of things to read and to calculate, KG working madly to get it all done by the next day, only to be given another list. The calculations were done with hand-cranked calculating machines, log tables, slide rules, and approximations; the drawings for the publication were done by KG using India ink, Bristol board, and French curves; and much of the final language was his—he had edited school newspapers and had firm views about English grammar. Reminiscences by Greisen's own students indicate that he established much more equal relationships with them.

It is worth noting that Greisen finished graduate school with a considerable background in both experimental and theoretical physics. Indeed, his most important later contributions came from both sides of the now almost unbridgeable divide between experimental and theoretical problems.

The 1942 Ph.D. and promotion to instructor at Cornell came in time for KIG to head out to the Manhattan Project at Los Alamos, though he and Rossi (who was still officially an enemy alien) had already been involved in some radar tracking work at MIT (unpublished). Rossi, declared "friendly," arrived at Los Alamos a bit later, though they were in different divisions and saw rather little of each other. Once there, Greisen moved up the ladder and from one division to another, starting as a junior researcher in the Van de Graff group, but ending up in charge of the group that designed, built, and figured out how to test the electronic detonators for the plutonium bomb. Replacing Primacord fuse with more sophisticated detonators had been his idea, and it solved the problem of setting them all off at the very precise times needed to make the "Christy bomb" design implode all at once (Hoddeson et al., 1993). The design was used for the July 16, 1945, Trinity test and then at Nagasaki. KIG was therefore "personally nervous, (for) if the shot turned out to be a dud, it might possibly be our fault" (Rhodes, 1986). He had also driven the detonators to the test site, receiving a speeding ticket en route, and was the next to last person up the tower preparing for the test explosion. "My God, it worked!," someone else there recollected his saying.

## BACK TO CORNELL AND COSMIC RAYS

Upon returning to Cornell in 1946, KG was appointed assistant professor, becoming associate professor in 1947, and professor in 1950. He returned just in time to inherit the student Rossi had left behind on moving to MIT, William Kraushaar, who went on to develop the instrumentation that first clearly recorded cosmic gamma rays from pion decay (Kraushaar and Clark, 1962). Greisen himself turned to gamma-ray searches in 1966.

It is worth remembering that cosmic rays have frequently been forefront physics, so that cosmic-ray papers often appeared first in issues of Physical Review in the 1940s. (By the 1950s pride of place generally went to nuclear physics.) Cosmic rays were important in at least three areas. First, between the wars and after World War II a number of new particles were found among cosmic-ray secondaries (positrons in 1932, mesotrons[1] in 1936, heavy mesons with charge in 1947, neutral pions in 1950, and some of the strange particles). Then accelerators and colliders gradually replaced cosmic rays as the main sources of new sorts of particles, though there were occasional cosmic-ray false alarms (one of which KIG participated in ruling out). The second topic was determining the nature of the primary particles. Pinning down most of them as protons is generally credited to Schein et al. (1941). Freier et al. (1948) reported the first heavier nuclei. The third issue was the spectrum—that is, number vs. energy—of the primaries, determined from the nature and number of the secondaries and their spread across the land when they reach the earth's surface and below. This last, especially for the highest energies, was Greisen territory for much of his career.

The problems of trying to understand the origin, acceleration, and propagation of cosmic rays have been with us since their discovery. Greisen (1971) addressed some of these issues in his book, though not often elsewhere. The focus of his own work and other reviews (Greisen, 1960, 1966b) was on the use of shower data from both above and below sea level to understand the electromagnetic (etc.) physics of shower development, and thus to determine total energies of the primary particles.

And cosmic-ray physics is yet again in the forefront with speculations that the highest-energy particles could reveal some new physics, complementary to that being sought with the Large Hadron Collider.

One conclusion, bridging across the war years (Greisen, 1943; Kraushaar, 1949, his thesis with KIG), was that data on muons plus electrons in the showers were best explained by a muon decaying to an electron plus two neutrinos, rather than an electron plus a photon or single neutrino. This feels remarkably prescient, given that flavor conservation was a later idea.

By 1948-1949 Greisen and his early collaborators (of whom Giuseppe Cocconi and Vanna Cocconi-Tongiorgi were particularly important co-authors) had spread arrays of plastic scintillator detectors across the tops of Cornell buildings, around the surrounding countryside, and beneath 1600 meters of water equivalent at the Cayuga Rock Salt Company mine. Others who contributed their hard work to the EAS (Extensive Air Shower) arrays around Ithaca included Edith Cassel, Goro Tanahashi, Alan Bunner, and Peter Landecker. An important conclusion was that, if a primary produced a shower of secondaries covering a wide area of the ground (EAS), then secondaries would also penetrate underground, and conversely (Greisen et al., 1949, 1950). Since these are independent signatures of large primary energy, analyzing both phenomena permitted better determination of the primary spectrum. This turned out to be a smooth power law, ruling out, for instance, a particularly odd model of cosmic-ray origins, put forward by Millikan, Neher, and Pickering (1942), which predicted spectral features from abundant elements like Si, S, N, C, and He. Luckily we remember these three gentlemen for other contributions to physics.

Presumably the description of stuff between you and the cosmic-ray secondary as centimeters of lead arose because lead was quite frequently used as a shield. The meters-of-water equivalent is convenient because water is a good deal more uniform in composition than rocks. But, in addition, the first experimenters to take their cosmic-ray detectors to altitude much less than 0 meters (Clay and Clay, 1935) really did go under 300 meters of water in a fjord near Bergen, Norway, quickly discovering that this reduced many sources of noise, though not all.

KG and his colleagues contributed regularly to the main stream cosmic-ray literature up until 1966, with progress marked by larger arrays of better detectors and higher energies for the primary particles. He dipped his toes back into theoretical mud at the 1957 Varenna Cosmic Ray Conference

(Cocconi et al., 1958) in an attempt to understand how solar flares can accelerate particles inside a magnetic bottle and yet release them to reach us quickly and more or less isotropically. Anyone who met three or more of those co-authors might suspect that the Rayleigh-Taylor instability invoked was not the most spectacular instability concerned.

## CLEARING OUT UNDERBRUSH

Predictions, discoveries, and confirmations are all important in science, but so is falsification.[4] Greisen was involved in three examples of this. First was a new sort of cosmic-ray secondary particle reported by Auger et al. (1948), who called it the lambda meson. It was supposedly made in pairs by gamma rays (Janossy and McCusker, 1949). Cowan (1948) even published a photograph of a track made by a particle of 10 $m_e$. No, said Cocconi and Greisen (1949). Based on their EAS arrays at Ithaca and Echo Lake, Colorado, they concluded that the particles reaching the ground are all mesons (which can penetrate 7 inches of lead) and electrons and gammas (which cannot).

Next were the issues of gamma rays of radioactive origin and neutrons in the secondary particles reaching ground, reported by Barnothy and Forro (1936, 1939, 1940), who had taken their detectors down to 1000 m of $H_2O$ equivalent in the Salgotarjani coal mine. Their results had perhaps even been confirmed by Jiesowicz et al. (1949), working down in the Wieliczka Salt Mine (the famous beautiful one near Krakow). No, said the Greisen team, after a false start or two. The secondaries include pions (which decay to muons before reaching ground) and electrons, while the gammas are mostly radioactive contamination; and an occasional neutron gets knocked loose from the experimental apparatus (Barrett et al., 1949; Greisen, 1949, 1950; Greisen et al., 1950). That set of papers included one (Greisen, 1950) in which KIG very graciously acknowledged that the conclusion of Greisen (1949) was too sweeping, and one of the Barnothy and Forro mechanisms could still be correct. The Greisen, et al. (1950) paper ruled that out as well.

Third was a mechanism for powering synchrotron radio emission from compact sources. One needs enormous numbers of relativistic electrons, and Burbidge (1956) and Burbidge and Hoyle (1956) had proposed that these might be secondaries to a sea of relativistic protons, containing about 100 times as much energy as the electrons (as they do in our local cosmic rays). The problem was that these sources would then have been overwhelming gamma-ray emitters from proton-proton collisions. Ogelman et al. (1966) showed that they were not.

Somewhere between “falsification” and “orderly progress of science” come frequent reports of a slight anisotropy in arrival directions of UHECRs (Ultra High Energy Cosmic Rays), which might have allowed their sources to be identified. Then the next round of better observations removed that anisotropy and perhaps found another (Delvaille et al., 1962a, b). Something similar happened in papers addressing whether fluxes detected underground depend on air temperature and time of day. Actually they do, but not at the level that had been claimed at the time (Barrett et al., 1954).

## TWO HIGH-PROFILE CONTRIBUTIONS: ATMOSPHERIC FLUORESCENCE AND THE GZK EFFECT

Predictably, two of the items with which Greisen was most often identified have at least mild priority disputes associated with them. The first of these is atmospheric fluorescence induced by high-energy particles. This must not be confused with Cerenkov radiation, though Greisen and his collaborators built detectors for both that could differentiate the two because the Cerenkov light arrives first, by about 10 microseconds (Bunner et al., 1967). The primaries seen with this experiment extended up to $10^{19}$ eV. Cerenkov light is what happens when a relativistic particle moves through a medium at velocity larger than the local speed of light. It was predicted by Blackett in 1947, was seen from cosmic rays by Galbraith and Jelley (1953), and has a continuous spectrum. Fluorescence is what happens when secondary particles excite $N_2$ molecules in the upper atmosphere (because they are the commonest sort of molecules there) and the molecules radiatively de-excite, emitting near-ultraviolet photons in three bands. Pure $N_2$ would actually be better, because $O_2$ molecules tend to collisionally de-excite the $N_2$'s, but in that case there would be no cosmic ray physicists to observe the process.

The question of who first thought of fluorescence from cosmic rays probably cannot be resolved (Watson, 2010) because some of the early thinking was apparently inspired by things that happened during atmospheric bomb tests. In any event, KG, Koichi Suga, and Alexander Evge'evich Chudakov each thought of looking for the emission at some time before 1962. The topic was discussed then at the Fifth Interamerican Seminar on Cosmic Rays in La Paz, according to references provided by Nagano and Watson (2000). Curiously, it does not appear in Greisen's (1963) report of the meeting, but George W. Clark (2009) recalls discussing fluorescence with KG on the plane back from La Paz. The report does mention other contributions at the meeting from Suga, Chudakov, and the Greisen group (Delvaille et al., 1962a, b), and all three are in the conference photo, as is Clark, who in due course established the high-altitude air shower experiment at Chalcultaya (in case you were wondering "why La Paz?").

And it was in 1962 that KIG put in the first proposal to build an array to look for the fluorescence. He and students Ed Jenkins (2009) and Alan Bunner installed a bunch of photomultiplier tubes fed by Fresnel lenses in a potato field on a hilltop named Mt. Pleasant. Unfortunately the site suffered from very frequent cloud cover and also from particle pollution in the air that created reflected light noise. The Japanese site, Mt. Dodaira, which was developed by Suga's group (including Goro Tanahashi, who had worked as a postdoc with KG), had the same problems, though they reported detections and they were congratulated for doing so by KG (Watson, 2010). It is possible, however, that what they saw was Cerenkov radiation, as was the case for Chudakov's installation.

The Ithaca group felt confident only in upper limits, which appear in Jenkins's 1966 thesis (Jenkins, 2009) and in the group's final report to the Atomic Energy Commission in 1972, which also expresses considerable relief at the termination of a long period of arduous and rather unrewarding effort.

What was needed was a much less cloudy site with much clearer air and, KG recognized in retrospect, better information-processing technology to identify the shower signatures. The end of the story is happy, though long delayed. In spring 1966 KG spent one of his few sabbaticals at the University of Utah, where skies were much clearer. In due course his friends Jack Keuffel and George Cassiday submitted a National Science Foundation proposal, funded in 1974, for the first "Utah Eye." They saw one event at $E \approx 10^{20}$ eV (Cassiday, 2010). And, skipping to the present, Cerenkov and fluorescent detection of UHECRs is still not perhaps quite routine, but HiReS in Utah, under the leadership of Pierre Sokolsky and Eugene Loh, and the AUGER array of particle detectors on the ground and four fluorescent telescopes in Argentina have now both seen enough $10^{20}$ eV events to confirm a deficiency of these relative to a smooth power law spectrum continued from lower energies. The results appear as Abbasi et al. (2008) and Abraham et al. (2008), from which we learn that both were large collaborations.

Most famous was, and is, the GZK effect. Zatsepin (1951) had thought about interactions of cosmic rays with interstellar and intergalactic photons long ago, and there was a discussion of processes from Hayakawa and Yamamoto (1963). Particularly prescient, Zel'dovich (1965) considered the specific case of microwave photons and cosmic rays as energetic as the $10^{20}$ eV event reported by Linsley (1963) from the EAS array at Volcano Ranch in New Mexico. Zel'dovich's conclusion was that a Gamovian hot big bang could not be the right picture of the early universe, because the cosmic rays could not have reached us from any reasonable cosmic source. He focused then briefly on a cold big bang cosmology, also partly because a map of the microwave sky, made by Ohm (1961) for the purpose of understanding backgrounds for communication satellites, seemed to Zel'dovich to set a 1K limit to any CMB (Cosmic Microwave Background), while Ralph Alpher and Robert Hermann (1950) predicted 5 K.

The first paper after the news of the CMB emerged from New Jersey (too complex a story to tell here; see Peebles et al., 2009) came from Fred Hoyle (1965) and was cited by Greisen (1966a, b). But Hoyle considered only inverse Compton scattering, which is significant only for the particles above $10^{21}$ eV. The CMB was first widely publicized by Walter Sullivan in the May 21 , 1965, *New York Times,* and its existence was mentioned at the summer 1965 Cosmic Ray Conference in London, attended by Igor Dmitriyevich Novikov, who brought the news back to Moscow (Peebles et al., 2009, p. 99). Greisen's group was also represented at that meeting and may well have included readers of *The New York Times.* The issue of *Astrophysical Journal Letters* reporting the 3 K radiation discovery was nominally published during summer 1965 but not distributed until September. The

Greisen (1966a) and Zatsepin and Kuzmin (1966) papers were not submitted until April and May 1966, and Alan Watson (2007) actually expressed surprise that it had taken the three so long to do their calculations and write up the papers. Others of us probably feel that, even when doing the calculations and writing up a paper are fairly easy, the hard and perhaps time-consuming part is to have an idea!

## GAMMA RAYS FROM BALLOONS

The third innovation with which KIG's name has been most often associated was the search for (and in the case of the Crab Nebula, detection of) cosmic gamma rays above about 50 GeV using balloon-borne spark chambers and gas Cerenkov detectors. Because the flux of primary cosmic rays hitting the upper atmosphere exceeds the flux of such gamma rays from astronomical sources by a factor of 100 or more (Greisen, 1971, p. 61), anticoincidence shielding was an essential part of the successful flights.

Greisen switched quite abruptly from cosmic-ray to gamma-ray experiments at a time when the number of review articles exceeded the number of confirmed sources by a factor of infinity. And it is probably relevant that one of those reviews was by Cornell colleague Philip Morrison (1958) and another by future co-author Giovanni Fazio (1967).

The spark chamber detector was flown about six times from 1966 to 1976, last from Holloman Air Force Base in New Mexico. The only thing that ever showed up was a 5 standard-deviation count excess over the background in a direction close to that of the bright galaxy M87 (Koch, 2007).[5] This was not published in any of several papers of upper limits (e.g., Ogelman et al., 1966). The Mansfield amendment put a sudden end to the Air Force Office of Scientific Research (AFOSR) funding that had supported the spark chamber, but a proposal to NASA to develop a gas Cerenkov telescope was funded and led to an interesting instrument (Albats et al., 1971). The telescope and gondola design were David Koch's thesis, and Fazio up at the Smithsonian Astrophysical Observatory built the pointing and suspension system.

This was the instrumentation that yielded the first detection of pulsed ultrahigh-energy gamma rays from the Crab Nebula (McBreen et al., 1973), upper limits on six other sources published a couple of years later, and a report that the Crab flux had faded and changed its spectrum over a couple of years (Greisen et al., 1975). A last upper-limits paper appeared in 1976, after the field of gamma-ray astronomy had pretty much been taken over by the satellite COS-B, launched in 1975. The GeV regime wasn't reached again until EGRET (with a detector similar to Greisen's design) went up on the Compton Gamma Ray Observatory in 1991. EGRET provided no evidence of variable gamma emission from either the nebula or its pulsar. MAGIC (2008) has now detected pulsed Crab gammas at about 25 GeV.

Quite by chance, as this memoir was being tidied up, the AGILE detector on the Fermi gamma-ray satellite reported a major (nonpulsed) Crab flare (Weekes, 2010; Fonseca, 2010). It is, therefore, just barely possible that the Greisen flights occurred at a time of some very rare event in the life of the Crab and its neutron star.

## A GOOD CITIZEN

The record of KG's contributions above and beyond his research is incomplete; neither the Cornell archives nor his family ever had a complete curriculum vitae. He was part of a group that worked with Philip Morrison and Hans Bethe (both then at Cornell) to prepare the Physical Sciences Study Committee one-year high school physics course for use starting in 1959. Initially the chapters were mimeographed, and the experiments came in paper bags. His commitment to teaching extended to redesigning the Cornell introductory physics courses (Greisen, 1977) and teaching them for a number of years. He supported colleagues who were, like him, outstanding teachers, and served on several student-oriented committees. Former undergraduate student Irwin Shapiro, later director of the Harvard-Smithsonian Center for Astrophysics, was among many who responded to queries with stories of KG's devotion to seeing that his students understood what was happening in class, received suitable fellowships, and were admitted to high-profile graduate schools. He consoled a first-year graduate student who was worried about flunking the required graduate lab, Physics 501, by saying that he had done badly in it himself, because most of his attention was going to someone named Betty, who was much more important at the time.

Other service to Cornell included a term as university ombudsman (1975-1977), chairing the astronomy department (1976-1979) when it was suffering from some internal dissension between the terms of Martin Harwit and Yervant Terzian, and a five-year stretch (1978-1983) as dean of the faculty. Greisen provided input to the Apollo, Space Shuttle, and other NASA programs on instrumentation, data analysis, and interpretation of gamma-ray fluxes (recorded in final reports to the sponsors) as well as to the National Science Foundation and AFOSR. He was a founding member of the organizing committees of both the Commission on High Energy Astrophysics of the International Astronomical Union (1970) and of the High Energy Astrophysics Division of the American Astronomical Society (1968-1969), chairing the latter in 1970 and 1971. He generously provided some of his records for a history of the division (Trimble, 1999), though it was his former student and successor as division chair, Bill Kraushaar, who had kept the most detailed records. KIG remained a member of the IAU Commission until shortly before his death, but gave up his membership in the High Energy Astrophysics Division after retiring in 1984.

## SUMMARY

Greisen, whose expertise in trouble-shooting apparatus was attested to by numerous students, influenced the long-range development of cosmic-ray (and to a lesser extent gamma-ray) astronomy through innovative instrumentation whose long-term descendents are still in use; through the prediction that UHECRs either would not get to us through the CMB or would reveal new physics by doing so; and through the at least 21 graduate students, 6 postdocs, and numerous undergraduates he mentored from 1946 until the mid-1970s.

## ACKNOWLEDGEMENTS

I was first introduced to cosmic-ray physics by the late Maurice M. Shapiro at conferences in Cambridge, U.K., in 1968 and 1974, and it is thanks to him that I knew enough cosmic-ray vocabulary to be able to read very nearly all the papers in the list of references, the bibliography, and the rest of KG's publications. Gaurang Yodh generously provided a number of technical corrections to the first draft of the memoir. Others to whom I am very much indebted are KIG's son, Eric W. Greisen, who provided an extended obituary he had written for the American Astronomical Society in 2007 and some additional family material; George W. Clark, who had somehow managed to hang on to, and sent me, hand-written documents from KIG; David Cassel, who provided an assortment of obituaries and lists of publications that he had compiled shortly after KIG died; Alan Bunner, Irwin Shapiro, David Koch, David Gilman, George Cassiday, Peter Landecker, Paul Albats, Ed Jenkins, Trevor Weekes, and Maria Victoria Fonseca, who sent tales of their interactions with Greisen and updates on several of the science topics; and especially Alan Watson for a last-minute bailout on the history of atmospheric fluorescence. The keyboarding was expertly done by Lt. Col. Dawn Deshefy.

NOTES

1. When particles with mass between those of protons and electrons were first found in cosmic-ray secondaries, they were called mesotrons. These became mesons and with the recognition that there were two kinds, pi mesons and mu mesons. Only the former is strongly interacting. Common names now are muons and pions, or just $\mu$'s and $\pi$'s.
2. The idea is that secondary particles from ultrahigh-energy cosmic rays (UHECRs) entering the upper atmosphere collisionally excite molecules, mostly $N_2$. These radiatively de-excite, giving three bands of near-ultraviolet photons. Some early papers call the process scintillation.
3. Because the 1965 Nobel Prize in Physics went to Richard Feynman, Julian Schwinger, and Shinichiro Tomonaga for research in quantum electrodynamics, folks who came to physics after World War II typically think they invented all the concepts and the name. I did.
4. Falsify, falsification, and so forth should be thought of as technical terms in philosophy and history of science, with meanings slightly different from the commonest dictionary ones (like energy and momentum in classical mechanics). The underlying issue is that, although one can prove a theorem in mathematics, one can never absolutely prove a theory or hypothesis in the empirical sciences, since the next experiment might show it is wrong. This is a possible definition of what it means for an idea to be a scientific one (Popper, 1963). It has been claimed against string theory and the multiverse concept (but also against both biological evolution and creationism) that they could not be falsified by any observation or experiment that one can conceive of carrying out and are, therefore, not scientific. The three sets of ideas mentioned in this section, while all sounding a bit off-the-wall at least today, were definitely falsified by the Greisen et al. observations and were, therefore, authentically scientific, even though the last of the three was motivated by a desire to make radio galaxies and quasars sound impossible in a conventional universe where redshifts are caused by expansion.

5. M87 is the central active galaxy in the nearest large cluster, Virgo. It is a strong source of X rays and radio waves, powered by accretion on a compact object (black hole), whose mass of about 6 billion solar masses is the largest measured to date. It is also a gamma-ray source; active galaxies are often highly variable at gamma-ray energies; hence, just possibly, this unpublished excess was a real event.

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