Fifty Years of Cosmic-ray Science — a Personal Retrospective*

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Professors Yoshimura and Kajita have graciously asked me to share with you some reflections on the history of our discipline, and some recollections of the twenty-six ICR conferences that I have been privileged to attend.

My first conference of the ICRC series took place just fifty years ago, at Bagneres di Bigorre in France, but I was lucky to attend a world conference on cosmic radiation fourteen years earlier, in 1939.** I was a graduate student of Arthur Compton at the University of Chicago when he organized this meeting, which attracted many eminent physicists; (see Fig. 1. Most of the participants are identified in Fig. 1A.)

It is noteworthy that not one of the attendees in this photograph would—at that time—have described himself as an astrophysicist.

Let us now go back a few decades: in 1912 Victor Hess in Austria and Ernest Rutherford in England hardly knew each other. Yet their epochal discoveries were to intersect, some twenty years later, in a new discipline—elementary-particle physics. Hess’ cosmic rays would serve as nature’s accelerator, while Rutherford’s atomic nuclei foreshadowed the disciplines of nuclear physics and high-energy physics.

Over the next twenty years there were few notable cosmic-ray developments, and there was not an overwhelming interest in the “Höhenstrahlung.” One indication of this is the fact that Victor Hess was apparently not among the eminent scientists who attended the prestigious Solvay conferences in the 1920’s. To be sure, Clay discovered the latitude effect, which Millikan could not verify. Compton’s confirmation of this effect of the geomagnetic field showed that the primaries must be charged particles. But Millikan insisted that, since the primaries were so penetrating, they had to be gamma rays.

The situation changed dramatically in 1932 with the cosmic-ray discovery

*After-dinner address at the Conference banquet.
**Actually, I also attended another international conference on cosmic rays prior to 1953, i.e., the one in Echo Lake, Colorado, in 1949.
International Cosmic-Ray Conference, Chicago, 1939

Fig. 1.
of the positron in a cloud chamber by Anderson and Neddermeyer. Cosmic rays sprang into the limelight: they were good for something: they had revealed the existence of a new particle anticipated by Dirac. This initiated a major industry: the discovery of a bewildering array of so-called elementary particles—which Oppenheimer dubbed “the nuclear zoo.”

Four years later Anderson won the Nobel Prize for a discovery that had used the cosmic rays. He shared the prize with Victor Hess—whose crucial and basic discovery was finally recognized after a lapse of 24 years!

The same year 1932 brought a raft of other great discoveries, e.g., Chadwick’s (the neutron), Landau’s anticipation of neutron stars, etc. But for cosmic rays, the impact of Anderson’s achievement was stunning: in the 1930’s cosmic-ray research proliferated—both in theory and experiment. Now the question naturally arose: if the cosmic rays are charged particles, then what charge do they have—positive or negative? The East-West experiment proposed by Bruno Rossi was to show that the particles are mainly positive. But what kind of positive particles?

Curiously, it was not until 1941—some three decades after Hess’ manned balloon flights—that we finally got the first answer: mostly protons. This was learned in a balloon flight by Schein, Jesse & Wollan, of the University of Chicago.
Marcel Schein has never been properly recognized for this great achievement. [In 1941 most physicists were too busy with war research to read the Physical Review.]

Speaking of Marcel Schein, permit me to digress from my chronological account to tell you of a project that involved a protégé of Schein’s who is a prominent speaker at this conference.

In the late 1950’s Schein organized the International Collaborative Emulsion Flights (ICEF) to study interactions of cosmic rays up to $10^{15}$ eV; the detectors were enormous stacks of photographic emulsions. After a successful balloon flight, Schein died, and his young research associate—Masatoshi Koshiba—was left with the task of developing many square meters of emulsion. The University of Chicago, aware of my early research using the photographic method, asked me to ensure that the emulsions were fairly distributed and well utilized by laboratories around the world. I could spare only a limited amount of time from the research of my own group in Washington; it was my friend Koshiba who really saw the project through. As you know, Toshi later returned to Japan and made historic contributions to neutrino astronomy.

After the discovery in 1941 that the primaries were protons, another six
years elapsed until it was found that in addition to a predominant proton component, the primary cosmic rays also include heavier nuclei—at least up to iron. Thus a new sub-discipline—that of primary cosmic-ray composition—was born. It was only later that the source composition became a subject of inquiry.

In the 1920’s the development of quantum mechanics had paved the way for the subsequent elucidation of cosmic-ray atmospheric showers in the ‘30’s. Among theorists who contributed to this understanding were Bethe, Oppenheimer, Bhabha and Heitler. Experimentally, this development was based on Rossi’s and Auger’s research on cosmic-ray showers.

Also in the 30’s the muon was discovered. Since it was so penetrating, it could not be the Yukawa particle, postulated to serve as nuclear glue. At that early time it could hardly be anticipated that the muon would serve another purpose—as a tracer for mu-neutrinos. [Perhaps this is a response to the famous question of Rabi about the muon: “Who ordered that?”]

In the same decade of the thirties, a Viennese physicist, Marietta Blau,
pioneered in applying photographic emulsions to cosmic-ray research. I was intrigued by this approach, and wrote a review, “Tracks of Nuclear Particles in Photographic Emulsions.” Prof. Compton warned me that if I were to use this unfamiliar technique in research for my thesis, he would be unable to help me. Fortunately, he liked my review paper and had it published in the Reviews of Modern Physics. Apparently the paper was widely read, and it stimulated an interest in using nuclear emulsions for cosmic-ray research. In the post-war period the technique became a principal tool for studies of cosmic-ray composition.

In 1947 two great discoveries were made—both involving the use of nuclear emulsions. One was the discovery of the pi meson by Lattes, Occhialini, and Powell. [This became possible with the development of sensitive nuclear emulsions at British Kodak]. The other, as already mentioned, was the discovery of “heavy primary nuclei” by teams at the Universities of Minnesota and Rochester.

By 1955, with the advent of multi-GeV accelerators, nature’s accelerator was eclipsed, and many of us turned to the astrophysics of cosmic rays.

Only two years earlier, however, at the ICRC in Bagneres di Bigorre, many of the papers were devoted to elementary particles discovered among the cosmic-ray secondaries. For example, three different cosmic-ray groups—in Milan, Bombay, & Washington, D.C.—reported on their independent discoveries of the sigma hyperon.

The next ICRC, in Guanajuato, Mexico, was a perfect reflection of what had just happened to the field of cosmic-ray research: for the first time, a goodly number of professional astronomers and astrophysicists were present, among them Chandrasekhar. On a long bus ride from Guanajuato to Mexico City I had the unforgettable experience of sitting with Chandra, and hearing of his collaboration with Fermi. This conference was also notable for the participation of three physicists from the USSR—Zatsepin, Dobrotin, and Vernov. George Zatsepin (of Greisen-Zatsepin-Kuzmin fame) was to become a good friend. He has been Russia’s preeminent theorist on neutrinos.

Two years later, in 1957, at the meeting in Varenna, I first met Minoru Oda and Sasha Chudakov. Prof. Oda was an impressive representative of Japan’s cosmic-ray community. Chudakov later headed the experimental program in the underground Baksan Laboratory.

The next ICRC, in Moscow was, for me, an exceptionally memorable experience. I met Ginzburg, Skobeltsyn, Landau, Tamm, Shklovsky, and Feinberg, among others, and enjoyed a guided tour of the famous Pulkovo Observatory in Leningrad by its director Mikhailov. At this meeting our NRL group reported convincingly that Li, Be, and B were actually part of the primary beam, not just atmospheric secondaries. By the next ICRC in Kyoto, we had increased our
statistical significance 6-fold, thus putting the ten-year old controversy to rest. Bernard Peters, chair of the session, graciously called attention to the difficulty of the project.

While in Kyoto, I was invited to lecture at the Yukawa Institute. On the appointed day, my Japanese hosts appeared, and I was there, but no one else showed up—a violent typhoon was raging. Later on the same day I was scheduled to go by rail to Nagoya with Sekkido, Hayakawa, and Kondo, but tree trunks and other debris were sprawled over the railroad tracks, so we traveled to Nagoya by taxi.

The Jaipur conference in 1963 gave me a second opportunity to visit India. “Goku” Menon, whom I had met when he was a student at Bristol, now chaired this meeting. He was to become director of the Tata Institute in Bombay, which had been founded by Homi Bhaba thanks to his rich uncle Tata. On this trip I was invited by Vikram Sarabhai to visit Ahmedabad, a pleasure I enjoyed on each of my five visits to India. I had met Vikram in Bagneres di Bigorre and again in 1958 when he and his family came to visit me in Washington. He was soon to become science adviser to Indira Gandhi, and he presided over both atomic energy and space research.

In 1965 the ICRC met at Imperial College in London (where Bill Penney, my tennis partner in wartime Los Alamos, was soon to become rector and to sit in the House of Lords. Penney had succeeded Cockroft as head of UK’s atomic energy establishment). When I moved from my hotel in order to be closer to Imperial College, my mail was forwarded with the notation “M. Shapiro, c/o Conference on Cosmic Radiation.”

I skip over to the Budapest conference in 1969, when Hungary was still behind the Iron Curtain. Janossy was chairman of the meeting, and his co-chairman, Ervin Fenyves, had served as a co-director of the Dubna Center in the USSR. Curiously, Prof. Fenyves was not to be seen; he and his wife had just managed to escape from Hungary. Subsequently—and for the last three decades—he has been a mainstay at the Univ. of Texas in Dallas and an indefatigable organizer of the Texas Symposia on Relativistic Astrophysics. [Texas—already a large State—has since extended its reach as far as Jerusalem!]

At the ICRC meeting in Budapest our NRL group reported the first extensive results on the source composition of cosmic rays, their path-length distribution and their residence time (“age”) in the Galaxy, and predictions of their isotopic composition. It is, of course, the source composition that is of interest in questions of origin.

The meeting in Budapest featured a spurious report claiming to have detected free quarks in a cloud chamber. This “discovery” was promptly demolished
by Wilson of the U.K., and by Cowsik, a young Indian physicist, who is now director of the Indian Institute of Astrophysics in Bangalore.

The first ICRC in the USA took place in Denver, Colorado in 1973. It seems strange that the U.S. should have waited twenty-two years to host an ICRC. This was due to the disease of McCarthyism that afflicted America in the early fifties. Two prominent and influential cosmic-ray gurus had been denied invitations to the U.S. because of their leftist political leanings. Cecil Powell and Patrick Blackett managed to keep the ICRC from meeting in the U.S. Powell was so offended that he never came to the States. When Marshak invited him to attend the first Rochester Conference on High-Energy Physics in 1950, Powell asked me to report the latest Bristol findings in his behalf. Subsequently, Blackett did come to the States, and I was pleased when he visited my laboratory and my home.

For me the Denver meeting in 1973 proved to be special: Vitaly Ginzburg was prevented by the Soviet authorities from attending this conference, and I was asked to replace him as rapporteur on “Origin.” At the outset I used the opportunity to call attention to the violation of international norms in science [as I did subsequently at the ICRC in Munich]. It was also at this meeting in Denver that we sketched the first plans for DUMAND, the Deep Underwater Muon and Neutrino Detector. Eventually, the project was deemed by the U.S. Department of Energy to be too expensive. It was, however, the forerunner of the active arrays for high-energy neutrino astronomy now underway in Antarctica and the Mediterranean. Its basic methodology and instrumentation proved to be a lasting legacy. Among the leading contributors to DUMAND were Reines, Learned, Peterson, Roberts, and Greider.

As my time is running short, I move on to Moscow, in the year 1987. I believe it was the first time that Zel’dovich addressed an ICRC. We had become friends at a meeting in Moscow on “Cosmic-ray Astrophysics” some sixteen years earlier, organized by Zatsepin and Chudakov. A few months earlier Andrei Sakharov had been released from Gorky by Gorbachev, and I had a memorable four-hour discussion with Sakharov in his Moscow apartment. Somehow, during his painful exile, he had kept abreast of many developments in science. Prof. Sakharov was a unique combination of a Landau and a Gandhi; I feel privileged to have known him.

Many of you have attended the ICRC over the past decade. Hence, with your indulgence, I shall close by displaying a few photos that will remind you of colleagues whom you have known—or at least heard of.
1. Acknowledgment

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