Tracking Louis Leprince-Ringuet's contributions to cosmic-ray physics

We were a bit surprised that Per Carlson's otherwise well-documented review, "A century of cosmic rays" (PHYSICS TODAY, February 2012, page 30), did not mention the contributions of Louis Leprince-Ringuet. We write to correct that omission.

Leprince-Ringuet (1901–2000) began studying cosmic rays in Maurice de Broglie's laboratory in the early 1930s, during a time of controversy over whether cosmic rays were charged particles or gamma rays, since experiments with ionization chambers had produced conflicting evidence for geomagnetic effects. In 1933 Pierre Auger and Leprince-Ringuet used a more direct technique based on coincidence counters aboard a ship traveling from Le Havre, France, to Buenos Aires, Argentina, and back. With that approach they counted 170 000 individual cosmic rays and accurately measured the geomagnetic effects.[1]

Later, Leprince-Ringuet and his collaborators built a cloud chamber that was triggered by counters, and they inserted it in the big magnet of the French Academy of Sciences in Bellevue. In 1936 Leprince-Ringuet was appointed professor of physics at École Polytechnique in Paris, where he created the school's first research laboratory, initially devoted to cosmic-ray physics. In March 1939, shortly after the discovery of the muon, he and coworkers used the cloud chamber to directly measure its mass.[2] With the onset of World War II, however, the researchers could not submit their paper until the end of 1940.

During the war the cloud chamber and the magnet were moved to L'Argentière-La-Bessée in the French Alps. In a series of 10 000 cloud-chamber photographs they took in 1943, Leprince-Ringuet and his collaborators found the first evidence for a heavy meson[3] with a mass some 870–1110 times the electron mass. The new particle could not be interpreted as a muon but was not unanimously accepted as a heavy meson; as Hans Bethe argued, the upper limit on the mass could reach the proton mass if all the probable errors were doubled and added together with the same sign. However, as J. Robert Oppenheimer stated in his concluding remarks at the Symposium on Cosmic Rays at Caltech in 1948, it was "hard to disbelieve Leprince-Ringuet's evidence for a very heavy meson." Despite the low probability of the proton hypothesis, his evidence was not deemed decisive, and George Rochester and Clifford Butler were credited with the discovery of the K^+ in 1947. The next year Leprince-Ringuet and his team presented further evidence[4] for the K^+, obtained using the emulsion technique at the Laboratoire des Cosmiques, a high-altitude facility he had created in the French Alps.

In 1949–50 the search for strange particles and the study of their properties became primary goals of the physicists at École Polytechnique. Two large superposed cloud chambers were installed at the observatory of Pic du Midi de Bigorre, 3000 m above sea level in the French Pyrenees. The chamber on the top was located inside a magnet, whereas the bottom chamber was equipped with a series of metallic plates. The setup allowed incident particles to be identified and their energies measured.

At the time, Leprince-Ringuet, as secretary of the cosmic-ray commission of the International Union of Pure and Applied Physics (IUPAP), was in charge of organizing the 1953 Cosmic Ray Conference at Bagnères-de-Bigorre. A recent summary by James W. Cronin (Carlson's reference 14) of that conference and its consequences emphasizes Leprince-Ringuet's role. In view of early results from the first big US accelerators, Leprince-Ringuet insisted that the conference focus on particle physics aspects. A new nomenclature for strange particles was proposed by a commission that included the best specialists in the field. Most were quoted in Carlson's article, but not Leprince-Ringuet, who had coined the word "hyperon."

In the following years, Leprince-Ringuet's focus turned toward accelerator experiments. He was instrumental in the creation of CERN, and several physicists from his laboratory at École Polytechnique were major actors in the scientific developments of CERN's first 20 years.

Through his affiliations with the French Academy of Sciences and the Commissariat à l'Énergie Atomique and his valued participation in IUPAP and CERN, Leprince-Ringuet was a major force in cosmic-ray physics and in the transition to particle physics, at both the national and international levels.

References

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James Clerk Maxwell, a modern educator

Many of the works dedicated to the life of Scottish physicist James Clerk Maxwell contend that his scientific accomplishments were much more significant than his teaching achievements. During his lectures, for example, he would often correct himself and double-check what he'd written, which tended to frustrate and confuse the few students he had (from two to eight per class). As a result he had limited communication with the students, and in his classroom there was excess noise and a lack of discipline. He developed a reputation as a poor teacher who lacked self-confidence.[1]

However, we obtain a very different perspective if we step away from the details of his teaching personality and look instead at his own writings. They reveal that Maxwell's pedagogical ideas and approaches were in general agreement with many of today's views.

In his inaugural lecture[2] at Cambridge University in October 1871, Maxwell presented concrete and concise concepts as a part of his methodical plan for classroom teaching. Here are a few of his ideas.

When Maxwell arrived at Cambridge, the traditional method of education, concentrating on mathematics and theory, was the norm; students were to trust the proofs that professors provided in their lectures. "The familiar apparatus of pen, ink, and paper will no longer be sufficient for us," he said, "and we shall require more room than that afforded by a seat at a desk, and a wider area than that of the black board." He suggested a new approach that he called the "experiment of illustration." He wrote,

> The aim of an experiment of illustration is to throw light upon some scientific idea so that the student may be enabled to grasp it. . . . To exhibit illustrative experiments, to encourage others to make them, and to cultivate in every way the ideas on which they throw light, forms an important part of our duty. The simpler the materials of an illustrative experiment, and the more familiar they are to the student, the more thoroughly is he likely to acquire the idea which it is meant to illustrate. The educational value of such experiments is often inversely proportional to the complexity of the apparatus (reference 2, page 15).

Maxwell was certain that much of a future scholar's training must take place in a laboratory, where the student could administer his own experiments and become practically familiar "with all kinds of scientific methods, to compare them, and to estimate their value" (reference 2, page 20). Cavendish Laboratory, developed by Maxwell, realized that notion and became an outstanding school of experimental physics.

Maxwell's remarks about using mathematical tools in research and education can be readily accepted by today's scientists: "It is therefore natural to expect that the knowledge of physical science obtained by the combined use of mathematical analysis and experimental research will be of a more solid, available, and enduring kind than that possessed by the mere mathematician or the mere experimenter"(reference 2, page 18).

Furthermore, Maxwell noted the value of history of science in teaching: "The history of the development . . . of ideas is of all subjects that in which we, as thinking men, take the deepest interest" (reference 2, page 21).

In the lecture, Maxwell said, "It must be one of our most constant aims to maintain a living connexion between our work and the other liberal studies of Cambridge, whether literary, philological, historical or philosophical" (reference 2, page 20). His idea of interdisciplinary study is fully consistent with the view of science education espoused today by liberal arts colleges.

Many modern educators in colleges and secondary schools may be surprised by Maxwell's textbook *Matter and Motion*,[3] written in 1876 for beginning students in mechanics. The organizational structure of that textbook is nearly identical to texts used in colleges and high schools today, some 130 years later. They have the same introduction of vector algebra in the beginning course, the same sequence of material, and the same end-of-book review of the important points.

Maxwell was thus, both in terms of what he said and what he wrote, far ahead of his contemporaries in the teaching of physics.

I am indebted to Yitzhak Sharon, of the Richard Stockton College of New Jersey, for his input.

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Big Bang paternity in question

In his informative but somewhat hagiographic article on Alexander Friedmann's contributions to modern cosmology (Physics Today, October 2012, page 38), Ari Belenkiy suggests that the Russian physicist should be recognized as the father of Big Bang cosmology. I believe the suggestion is unsupported by historical documentation and that Belenkiy misrepresents to some extent the work of the other pioneer of modern cosmology, Georges Lemaître. It seems to me that Belenkiy, when discussing priority, does not make a sufficiently clear distinction between expanding models and finite-age models of the Big Bang type. Of course, Lemaître's 1927 paper proposing a universe expanding from a static Einstein state does not earn him credit for fathering Big Bang cosmology—but has anyone said that it does?

Friedmann's 1922 paper was not an argument for the expanding universe, which appeared as a mathematical possibility only and not as the solution that corresponds to the actual universe. Although Friedmann wrote in his 1922 paper of a "beginning of the world" in connection with solutions that correspond to expanding world models, he did it in an uncommitted way and without referring to astronomical or physical data. Neither in the 1922 work nor in his companion paper of 1924 did he mention physical terms such as "energy" or "radiation," not to mention "nebula" or "redshift."

If later commentators have presented Friedmann's cosmological papers as basically mathematical and unconcerned with physics and astronomy, it is because that is how the papers were. Belenkiy objects by referring to "Friedmann's considerable achievements in meteorology and aerodynamics"—hardly a relevant argument with regard to his two cosmological papers. It was precisely in that respect that Lemaître went beyond Friedmann. The Belgian physicist did not "fail" to consider all the solutions that Friedmann had discussed. He focused on the expanding solution that seemed to correspond to the redshift data and thus to the real universe as studied by the astronomers.

At any rate, Lemaître's candidacy for paternity of the Big Bang universe rests on his "primeval atom" hypothesis of 1931 in which he offered for the first time a *physical* picture of the nonsingular beginning of the universe.[1] While one can perhaps call Friedmann the father of the Big Bang in an abstract, uncommitted, and mathematical sense—although that would stretch the notion of paternity—it was only with Lemaître that the physical Big Bang entered the history of science.

Finally, when Belenkiy deplores the lack of recognition of Friedmann and refers to Yakov Zeldovich and other Russian scientists as exceptions, one should keep in mind the context of the cold war in the 1960s and later: In the Soviet Union, there was a political need to credit Friedmann as the true and only founder of relativistic evolution cosmology.

This brief letter is in no way an attempt to belittle the great work of Friedmann, only to offer a more balanced view. Friedmann's seminal contributions to cosmology are beyond question, but there is no need to exaggerate them or to read later advances into them.

Reference

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I very much appreciated the article by Ari Belenkiy on Alexander Friedmann. However, I noticed a typographical error just under Belenkiy's equation 1 on page 39: The constant k should be divided by c^4, rather than by c^2 as in the text.

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Belenkiy replies: As an applied scientist, Alexander Friedmann was keenly interested in testing his theories with available data. An interpretation of the right side of Einstein's field equations as the "flow of matter" was particularly appealing to him as a meteorologist. Several of his recently discovered letters to Paul Ehrenfest in June 1922 (Physics Today, March 2013, page 9) clearly show Friedmann knew he had made an important discovery.

While Friedmann had some *a priori* estimates of Λ and Λ *critical*, he was wise enough not to include them in the final draft of his 1922 paper; he remained uncommitted to a particular scenario until the empirical data provided information.[1] He was unaware of Vesto Slipher's data on the nebulae's high radial velocities, publicized by Arthur Eddington in 1923. As the letters to Ehrenfest testify, Russia was so devastated at the time Friedmann was writing his 1922 paper that he was still unable to obtain a copy of Willem de Sitter's original 1917 paper, which first highlighted Slipher's data. (Most likely, Friedmann learned of de Sitter's solution of general relativity equations indirectly from a secondary source.)

Furthermore, in his 1923 book, Friedmann discussed the possibility of the universe's birth from a "singularity." Thus all components of the Big Bang cosmology were present in Friedmann's writings long before Georges Lemaître entered the field.

As for Peter Hammerling's comment, Friedmann's metric, in contrast to the modern one, had the additional factor c^{-2}, thus forcing renormalization of the relevant constants k and λ by the inverse factor c^2.

Reference

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