At Arcetri, on the steps of the laboratory; sitting from left to right: the author, Giuseppe Occhialini, Gilberto Bernardini, and Daria Bocciarelli.

Early days in cosmic rays

Memories of four years of physics in the Florentine hills at Arcetri a half century ago that did much to shape the future of cosmic ray research.

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Bruno Rossi

Early in 1928, shortly after receiving my PhD at the University of Bologna, I was offered the position of assistant at the Physics Institute of the University of Florence. The Institute rose among olive trees, on the hill of Arcetri, a short distance from the villa where Galileo had spent the last years of his life as a political exile. The chair of physics was held by Professor Antonio Garbasso who, in earlier years, had done some creditable scientific work. But the first world war and subsequent events had diverted his interests toward politics. He was now a senator and the mayor of Florence. However, he still went to Arcetri three times a week to deliver his lectures, and he still had a strong desire to see the Institute, which he had built, become an important center of research.

The group I found in Arcetri was quite small, but quality made up for the size. Gilberto Bernardini, a recent PhD in physics from the University of Pisa, had joined the group a short time before my arrival. Among the students were Giuseppe Occhialini, Giulio Racah, Daria Bocciarelli, Guglielmo Righini, and Lorenzo Emo. We also had the benefit of regular visits by Professor Enrico Persico, who had undertaken the task of unraveling for us the mysteries of wave mechanics.

Life at Arceti was austere. My monthly salary was 600 lire (30 dollars at the then-current rate of exchange). There wasn't enough money in the laboratory budget to buy heating fuel even though Florentine winters are severe. To combat the cold, we would wear heavy woolen linings inside our laboratory smocks. The laboratory was always far behind in paying the electric bills, and the only reason our electricity was not cut off altogether was that the director of the laboratory was also mayor of the city.

Both Bernardini and I were very anxious to start some experimental program, and we spent our first year in Arcetri exploring, without much success, a few different lines of research.

Interest in cosmic rays

For me, the turning point in the search came in the Fall of 1929 with the appearance—in *Zeitschrift für Physik*—of the historical paper "Das Wesen der Höhenstrahlung" by W. Bothe and W. Kolhörster.[1]

Until then, I had not been particularly interested in the phenomenon of the "Höhenstrahlung" or "cosmic radiation," using the suggestive expression introduced by Robert Millikan. I had not thought that it would offer, to me at least, a profitable field of research.

I had not been seduced by Millikan's well-publicized theory, which maintained that cosmic rays were the "birth cry of atoms" in cosmic space, being born, in the form of γ-rays, when hydrogen atoms "fused" to form the heavier elements. To my skeptical mind, this was a romantic idea, lacking sound experimental support.

On the other hand, I had accepted uncritically the prevailing view that primary cosmic rays were high-energy γ-rays. Therefore I read with particularly keen interest the paper by Bothe and Kolhörster relating the first attempt to submit this assumption to a direct test.

The idea behind the experiment is well known. Gamma rays do not ionize directly. They do so through the intermediary of the secondary electrons which they generate in matter (at that time, Compton collisions were the only known interaction processes of gamma rays). The secondary electrons were thought to have a much smaller penetrating power than the parent γ-radiation. It followed that they would soon reach equilibrium with this radiation, so that ordinary absorption experiments would measure the attenuation of the hypothetical γ-radiation, although, presumably, the ionizing agent recorded directly by the measuring instruments was the secondary corpuscular radiation.

Bothe and Kolhörster saw that a direct study of this corpuscular radiation offered the most crucial test of the current views about the nature of cosmic rays.

In their experiment (figure 1), they used "tube counters" of the kind that H. Geiger and W. Müller had invented the year before. Two counters were placed one above the other, a small distance apart. Simultaneous pulses (or "coincidences") were frequently observed; their occurrence was interpreted as a result of the passage through both counters of ionizing particles. According to current views, these were Compton electrons generated by the cosmic γ-rays in the matter above the counters or in the walls of the counters themselves.

If this had been the case, a very small thickness of absorber between the counters would have been sufficient to stop all coincidences, owing to the small penetrating power of Compton electrons. Instead, a 4.1-cm gold absorber produced only a moderate decrease in the counting rate. The authors concluded that the ionizing particles were not Compton electrons, and that the primary cosmic radiation did not consist of gamma rays. In fact, they argued that the cosmic radiation observed at sea level consisted of charged particles and, moreover, that it was to be identified with the radiation falling upon the Earth's atmosphere from outer space.

The fact that this view later proved to be an oversimplification still does not detract from the pioneering quality of the work by Bothe and Kolhörster.

For me, the paper by Bothe and Kolhörster opened a window upon a new, unknown territory, with unlimited opportunities for exploration. I quickly realized these opportunities and started working, enlisting the help of my students, particularly Giuseppe Occhialini and Daria Bocciarelli. And thus began what I still remember as one of the most meaningful and exhilarating periods of my life. Was it because of the excitement of venturing into a still virgin field of science? Was it because of the exceptional human and intellectual qualities of the young people I found myself associated with? Was it because of the subtle, poignant beauty of the Tuscan countryside?

Our work proceeded rapidly. Within a few weeks we had our first Geiger–Müller counters in operation. To build a Geiger–Müller counter was, at that time, a kind of witchcraft. The tube was supposed to be made of zinc. Since no zinc tubing was available in Italy, we had to prepare it by bending a zinc sheet around a cylindrical surface and soldering it at the junction. The anode, according to the prescription, was to be a thin steel wire, slightly oxidized by immersion in nitric acid. The wire was held by two hard-rubber stoppers, closing the ends of the tube and made airtight with some sort of wax. The tube was evacuated through a thin glass pipe inserted in one of the stoppers and then filled to 1/10 of an atmosphere with dust-free dry air. Finally the glass pipe was sealed by melting its walls on a flame and the G.–M. counter was taken off the filling system. To quench the discharge, we had to ground the wire through a very high resistor, over 10^9 ohms, which we prepared ourselves by filling a small glass tube with some appropriate mixture of organic fluids (remember that self-quenching counters were still in the future).

Coincidence counting

Bothe and Kolhörster had recorded the coincidences by connecting their counters to two separated fiber electrometers, which were imaged on a moving film. By some clever device, involving the use of a fast-oscillating screen, they had obtained a time resolution of $1/100$ of a second. I felt that the power of the coincidence method would be greatly enhanced if one could devise a method for recording coincidences

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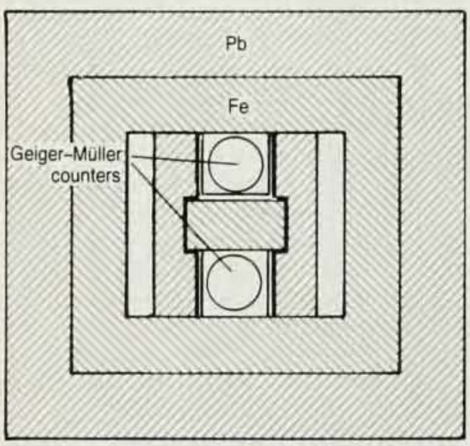
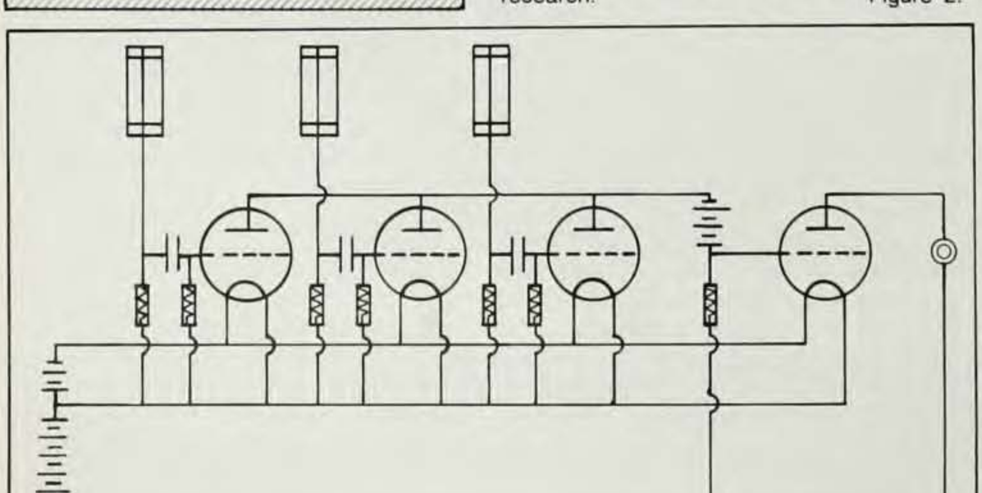
that would be less cumbersome than that used by Bothe and Kolhörster, and would provide a better time resolution. Thus the now classical coincidence circuit was born, which was to remain my main research tool—and also the main tool for many other experimenters—for years to come.[2] In its original version, it consisted of two or more triodes with the plates connected in parallel and the grids coupled electrostatically to the wires of the G.–M. counters (figure 2). Only when the grids of all triodes were simultaneously driven to a negative potential by the coincident discharges of the G.–M. counters would the current in all triodes stop and a large potential drop develop across the common resistor in the plate circuit of the triodes. In my earliest experiments, we detected this event acoustically as a click in a headphone; this meant that I or one of my collaborators had to sit by the equipment to count the coincidences. For all our enthusiasm, this turned out to be rather boring. So we soon replaced the headphone with a galvanometer whose deflections were recorded photographically on a moving film. Later still we did away with the galvanometer and used in its place a mechanical counter. (I should note that, before I published my invention, Bothe reported the development of a coincidence circuit making use of a two-grid vacuum tube. This circuit, however, was not competitive with mine. It required a much more delicate adjustment and, more importantly, it could only be used to record twofold coincidences, while mine was capable of recording coincidences between any number of G.–M. counters.)

So, with a satisfactory technique in hand, I began some preliminary experiments. Before a year was over, I had measured the efficiency of G.–M. counters and its dependence on voltage[3] by comparing the rate of threefold coincidences between three counters placed vertically, one above the other, with the rate of twofold coincidences between the uppermost and lowermost counters. I had also checked that the penetrating particles came preferentially from the vertical direction (by comparing the coincidence rate between two counters placed vertically one above the other or horizontally one next to the other).[4]

In another experiment I had tried to detect a deflection of the penetrating particles in their passage through a bar of magnetized iron.[4] Not having obtained any significant result from this experiment, I later tried to observe the deflection of cosmic-ray particles in magnetized iron, using a different, more sensitive arrangement suggested by Professor Puccianti of Pisa. This arrangement, which one could describe as a magnetic lens, consisted of a closed-circuit magnet formed by two oppositely magnetized iron bars that were arranged next to one another. Two G.–M. counters were placed horizontally, one above the other, below the magnet. Depending on the direction of the magnetization and on the sign of the charge of the particles, it was expected that the particles crossing the upper counter would be either concentrated upon the lower counter or deflected away from it.[5,6]



Early experiment by W. Bothe and W. Kolhorster found that cosmic rays were not gamma rays.(Reference 1) Figure 1.



Coincidence circuit, designed by the author in 1930, became a primary tool for cosmic ray research. Figure 2.

I observed a small effect, corresponding to positive particles, but I did not feel that this effect was statistically signficant. Of course, I did not guess that my ambiguous result was largely due to the presence of both positive and negative particles in the cosmic radiation. In this connection, I may recall that several years later the "magnetic lens" was successfully used to separate positive from negative μ-mesons, in experiments designed to study the different behavior of the two kinds of particles.

Working in Berlin

In the meantime, I had communicated with Bothe, describing what I had been doing, and I expressed my strong desire to work in his laboratory for a while. Bothe's answer was exceedingly kind and encouraging. My boss, Professor Barbasso, who had been following my work with great interest, procured a fellowship that enabled me to spend the summer of 1930 in Bothe's laboratory at the Physikalische-Technische Reichsanstalt in Berlin-Charlottenburg. The memory of that summer is still vivid in my mind. Berlin was, at that time, the very heart of contemporary physics. There I met Max Planck, Albert Einstein, Otto Hahn, Lise Meitner, Max von Laue, Walther Nernst, and Werner Heinsenberg, to name just a few. Some of these scientists were at one or the other of the several institutes in or around Berlin. Others would come from the neighboring cities for the weekly seminars. At that time also began my friendship with Patrick Blackett, who was also visiting there from England.

Bothe himself was most friendly and helpful; something I learned to appreciate more and more as I realized that, by nature, he was not overly outgoing and trustful. In this connection, I still remember a curious episode. I had just begun to work in his laboratory and had realized that his G.–M. counters were noticeably better than mine, in that they were more stable and had a longer plateau. I puzzled about this matter until one day Bothe took me aside with a mysterious air and began: "I will tell you a secret, but you must promise not to give it away to anyone." After I had promised, he continued: "My counters do not have a steel wire, as advertised; they have an aluminum wire!" I must confess to my shame that, when I returned to Italy, I felt that I could not keep the secret of the aluminum wire from my friends in Florence and in Rome; but I relieved my conscience by requiring of them the

Daria Bocciarelli building a Geiger–Müller counter in the University of Florence's Physics Institute at Arcetri.

same oath of secrecy that Bothe has required of me.

During my comparatively short stay at the Reichsanstalt, I repeated, in an improved form, the experiment of Bothe and Kolhörster by comparing the coincidence rates of two G.–M. counters with a given thickness of lead (9.7 cm) placed alternately above and between the counters. It turned out that the coincidence rate was not exactly the same in the two cases. With the lead above there was an excess of about 4% over the coincidence rate observed with the lead between. Thus things were not quite as simple as suggested by the assumption of Bothe and Kolhörster.[7]

East-West asymmetry

While I was at the Reichsanstalt, Bothe went with Kolhörster on an expedition to the North Sea and the northern Atlantic Ocean, in an attempt to discover a dependence of the cosmic-ray intensity on geomagnetic latitude (between 51° and 81°).

The prediction of such an effect was based simply on an argument of analogy with the phenomenon of the northern lights or aurorae. This phenomenon was then believed to be caused by high-energy electrons originating from the Sun and channeled toward the circumpolar regions of the Earth by the Earth's magnetic field; it was thought that if primary cosmic rays were indeed charged particles, the geomagnetic field should exert upon them a similar focusing action.

The experiment gave a negative result. But it aroused my interest in understanding more precisely what would actually happen when an initially isotropic stream of charged particles entered the magnetic field of the Earth.

Talking with Bothe, I learned that, in an attempt to explain the observed features of the aurorae, Störmer and his students had been working for many years on the mathematical problem of the motion of charged particles in a dipole field. But, as Bothe and Kolhörster remarked in the paper describing the results of their expedition, the theory developed by Störmer appeared to be so complex that it seemed hopeless to obtain from it quantitative conclusions pertinent to cosmic rays. In studying Störmer's papers, however, I found that this was not at all the case and that, by just asking the proper question, it was quite easy to derive from Störmer's theory some simple and highly significant results.

Störmer and his collaborators, through years of painstaking numerical calculations, had computed the trajectories of hundreds of particles of different energies entering the geomagnetic field from the direction of the Sun, in order to determine where and from what directions they would hit the Earth (electronic computers, of course, were undreamt of; Vannevar Bush, at MIT, was still in the process of developing his mechanical differential analyzer). But we cosmic-ray physicists were primarily interested in a simpler problem: We wanted to know, in the first place, whether particles of a given energy could or could not reach a given point of the Earth in a given direction. I found that at least a partial answer to the problem was contained in a simple formula derived by Störmer, which read

$$\sin\theta = \frac{300\ M}{R^2V}\cos\lambda - \left(\frac{300\ M}{R^2V}\right)^{1/2}\frac{2}{\cos\lambda}$$

On the right-hand side, M is the magnetic moment of the Earth (in gauss·cm^3), R the Earth's radius (in cm), λ the geomagnetic latitude of the point of observation, and V the magnetic rigidity (in volt) of the particles under consideration. On the left-hand side, θ is the angle between the trajectory of the incoming particle and the magnetic meridian plane, positive toward the east for negative particles, positive toward the west for positive particles. This angle is the semiaperture of a cone (known today as the Störmer cone) which separates the directions of incidence of the particles whose trajectories, followed backward, reach to infinity, from the directions of the particles whose trajectories always

Author at Arcetri working on early experiments, with several counters on his right. He had not yet learned to build power supplies and relied on dry cells in series to provide high voltage for the counters and batteries to provide plate voltage for vacuum tubes.

Experiment by author was first to use threefold coincidence and showed that cosmic ray particles could penetrate meter of lead. Figure 3.

remain in the vicinity of the Earth. The latter trajectories cannot be possible trajectories of cosmic-ray particles, while the former are possible trajectories, unless they happen to cross the Earth. The "forbidden" directions are to the East or the West of the Störmer cone, depending on whether the particles carry a positive or a negative charge.

This result (published in the summer of 1930—see reference 5) led me to predict an east–west asymmetry in the angular distribution of cosmic-ray particles and to estimate the energy of the particles for which a sizeable effect should be expected (depending on the latitude, it turned out to be of several times 10^9 or several times 10^{10} eV).

Upon my return to Arcetri, I attempted, unsuccessfully, to detect the predicted east–west asymmetry.[8] Having realized that the effect would become more pronounced at low latitude and high altitude, I began to plan an expedition to Asmara (Eritrea; geomagnetic latitude 11°30′; elevation 2370 m). Sergio de Benedetti joined me in this enterprise, which was made possible by the generous support of Professor Garbasso. Preparations took a fairly long time and, in fact, the experiment was completed after I had left Arcetri. Finally, in the fall of 1933, Sergio and I reached East Africa and set up our experiment in a cabin on a hill near Asmara. Soon we found that cosmic-ray intensity in the western directions was considerably greater than in the eastern directions, which proved unambiguously (1) that primary cosmic rays were, at least in part, charged particles and (2) that the charge of these particles was *positive* (a surprising result because most of us who had been supporting the corpuscular hypothesis, thought, more or less unconsciously, that primary cosmic rays would turn out to be electrons.)[9]

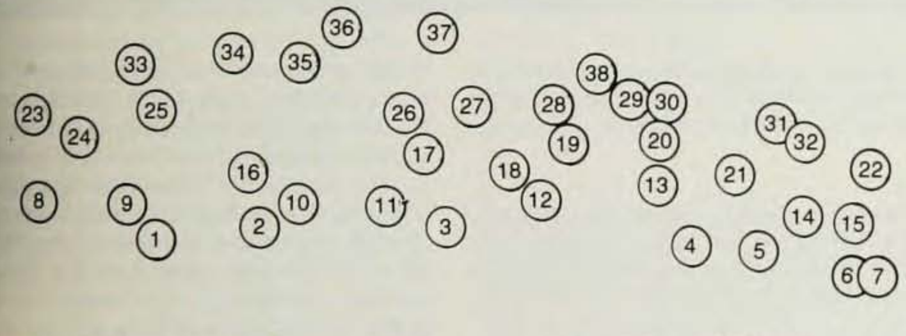
Here I must admit that we were rather painfully disappointed when we found that, by just a few months, we had lost the priority of this important discovery. It happened that, just as we were about to set out on our expedition, we read in *Physical Review* two articles, one by Thomas Johnson and another by Louis Alvarez and Arthur Compton, reporting the observation of an east–west effect in Mexico City. Moreover, my 1930 prediction of this effect was ignored, and credit for this prediction was given to Lemaitre and Vallarta, whose paper had been published three years later. Though I am sure it was an oversight, it still added to my frustration.

One further result of our experiments at Asmara may be worth mentioning. I shall do so by quoting from my own paper: "It would seem therefore (since doubts about possible disturbances were ruled out by appropriate control experiments) that once in a while there arrive on the instruments very extended showers of particles which produce coincidences between counters even rather far from each other. Unfortunately I lacked the time to study more closely this phenomenon in order to establish with certainty the existence of the supposed corpuscular showers and investigate their origin."[10] This, I believe, was the first observation of the air showers that Pierre Auger was to study extensively a few years later.

After this digression, let me return to 1931.

The great debate

In the fall of that year, the Italian Royal Academy sponsored a Conference on Nuclear Physics that brought to Rome, from all over the world, the most illustrious scientists interested in this and related fields of physics. At that time, under the direction of Fermi, the Rome group had just begun the research program in nuclear physics that was to produce such momentous results a few years later. At the invitation of Fermi, I gave an introductory speech on the problem of cosmic rays.[11] The main thrust of this talk was to present what, to my mind, were irrefutable arguments against Milli-



1. Richardson	9. Debye	17. Perrin	25. Townsend	33. Mott
2. Millikan	10. Compton	18. Corbino	26. Ehrenfest	34. Rapp
3. Marconi	11. Curie	19. Trabacchi	27. Fermi	35. Majorana
4. Bothe	12. Bohr	20. Canione	28. Beck	36. Garbanzo
5. Rossi	13. Aston	21. Parravano	29. Persico	37. Lo Surdo
6. Lise Meitner	14. Ellis	22. Rasetti	30. Vallanri	38. Carrelli
7. Goudsmit	15. Sommerfeld	23. Heisenberg	31. Giordani	
8. Stern	16. Wataghin	24. Brillouin	32. Bonino	

Participants in the conference on nuclear physics in Rome in 1931, which marked the beginning of the great debate on the nature of cosmic rays. (Reference 11).

kan's theory of the "birth cry" of atoms. Such a brash behavior on the part of a mere youngster (I was then 26 years old) clearly did not please Millikan, who, for a number of years thereafter, chose to ignore my work altogether. On the other hand it awoke a strong interest on the part of Arthur Compton, who had never worked on cosmic rays before. He kindly told me, some time later, that my 1931 talk had provided the initial motivation for his research program in cosmic rays.

The Rome Conference provided the first occasion for the proponents of the new corpuscular hypothesis to present their case before the scientific community, still strongly attached to the old γ-ray hypothesis. So this conference marked the beginning of the great debate on the nature of cosmic rays, which was to continue for several years.

In the United States the debate was at times bitter, involving the personal prestige of scientists committed to opposing views. For some reason, this did not happen on the other side of the ocean. In fact two of the strongest advocates of the γ-ray hypothesis—Lise Meitner and Eric Regener—were among my dearest and most respected friends.

From the previous discussion it is clear that the evidence concerning the penetrating power of the ionizing particles was a crucial argument in the controversy about the nature of cosmic rays. From direct experiments, it was known that most ionizing particles had ranges greater than 10 cm of lead. However, by indirect arguments, I had become convinced that many particles must have much greater ranges.

I felt that it was important to verify experimentally this conclusion; if the result of the experiment confirmed my expectations, it would kill once and for all the γ-ray hypothesis.

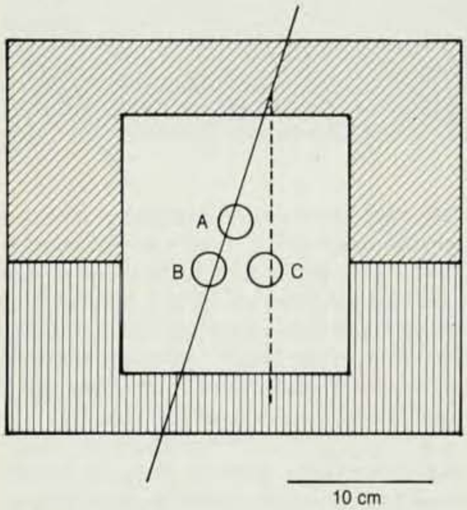
I planned to do this experiment by the usual method of counting coincidences between Geiger–Mueller counters arranged vertically one above the other and separated by a suitable absorber. The difficulty was that, with two counters far apart, the rate of "true" coincidences would have been smaller than the rate of chance coincidences. To overcome this difficulty, I decided to use threefold rather than twofold coincidences, thereby reducing the rate of chance coincidences to an almost negligible value.

The experimental arrangement is shown in figure 3. By varying the thickness of the lead absorber, I found that while the decrease in the rate of coincidences was fairly rapid in the first 10 cm of lead, it then became very slow, so that about 50% of the particles emerging from 10 cm of lead had ranges in excess of one meter of lead.[12,13]

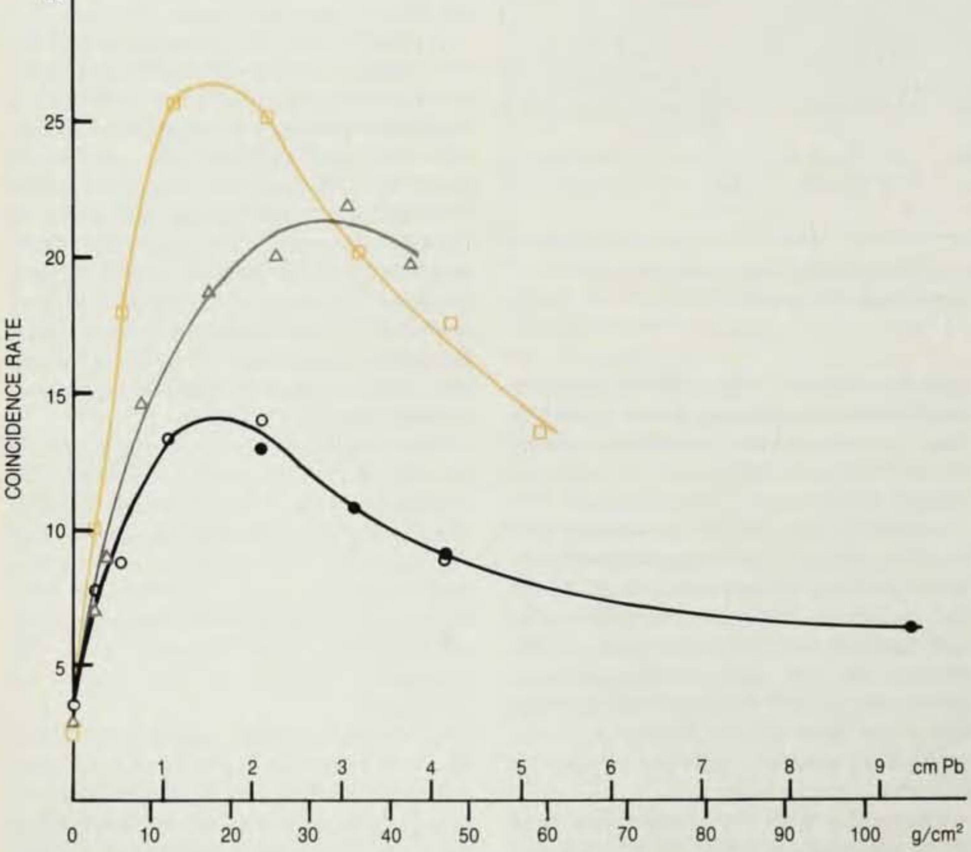
It is difficult today to appreciate how hard it was for the majority of the scientific community to accept this result. After all, the most penetrating particles known at that time (β-rays from radioactive substances) had ranges of a fraction of a millimeter of lead. Doubts were expressed as to the legitimacy of the coincidence method, and I had to perform further experiments to dispel these doubts.

Perhaps, at this point, I may be allowed to introduce a personal note. In retrospect, I realize today that an important asset in those early days was my completely open-minded approach to the problems I was working on. Unlike many other scientists, I did not feel constrained by concepts and results derived from the study of other phenomena, believed to be more or less akin to cosmic rays. At the Rome conference I began my discussion with the words:

"The most recent experiments have brought to light such strange facts that we are almost led to ask ourselves whether the penetrating radiation [cosmic radiation] may not be something fundamentally different from all other known radiations, or at least, whether, in the transition from the energies encountered in the ordinary radioactive processes to the energies encountered in the phenomena of the penetrating radiation, the behavior of particles and photons does not change much more deeply than one might have thought until now."



Triangular arrangement of GM counters in threefold coincidence detected production of secondary cosmic ray radiation. Figure 4.



Secondary effects of cosmic rays in lead at 14.6 cm (black curve) and 1.2 cm (grey curve) from counters and in iron (colored curve) at 1.2 cm from counters. (Reference 15) Figure 5.

Secondary radiation

The discussions at the Rome conference provided the motivation for another experiment I performed shortly after my return to Arcetri. At the conference, both Bothe and I had emphasized the growing evidence for the production of a secondary radiation by cosmic-ray particles in matter. This evidence, however, was still of a rather indirect nature. So, upon my return to Arcetri from the conference, I decided that I would try to detect the production process directly. For this purpose I arranged three Geiger–Müller counters in a triangular configuration (figure 4) so that a single particle traveling along a straight line could not traverse them all. With the counters enclosed in a lead shield a few centimeters thick, I did in fact observe a large number of threefold coincidences. Removing the shield reduced the coincidence rate considerably. It was thus clear that most of the coincidences observed with the shield in place were due to associated groups of particles (at least two) arising from

Sitting on the laboratory grounds, A. Colacevich, Bernardini, Bocciarelli and Emo Lorenzo at Arcetri.

interactions of cosmic-ray particles in the shield itself.[13,14] Qualitatively, this is the effect I had been looking for. But the magnitude of the effect, that is, the high rate of coincidences, was astounding. It showed that cosmic rays were capable of producing an enormously more abundant secondary radiation than any other known rays. So incredible were my results that a German magazine (if I remember correctly, it was *Naturwissenshaften*) refused to publish my paper. The paper was then accepted by *Physikalische Zeitschrift* after Heinsenberg had vouched for my credibiity.

I was, of course, greatly excited. Here was a new, unexpected phenomenon, a surprising property of the still mysterious cosmic rays.

I continued my experiment using a variety of configurations of the counters, changing the position and the thickness of the layers of matter where the secondary particles were produced, comparing the behavior of different materials, placing absorbing shields in different positions. The most significant results of this work are summarized in figure 5, which shows the dependence of the coincidence rate of three counters in a triangular array upon the thickness (in mass per unit area) of the layer of matter (lead or iron) above them. One sees that the lead curves (I and II) reach a maximum between 10 and 20 g/cm^2 of lead, which shows that the secondary particles have a range in lead of this magnitude.

One then finds that, beyond the maximum, the curves drop much more rapidly than the absorption curve of cosmic rays at sea level; I interpreted this result as showing that the coincidences were at least in part produced by comparatively soft, secondary rays generated by cosmic rays in the atmosphere.

Comparison of the curves for lead and iron, obtained with the shields placed at the same distance from the counters (curves II and III), showed that the rate of production of secondary interactions (per g/cm^2) was an increasing function of atomic number while the range of the secondary particles (again in g/cm^2) was a decreasing function of this quantity.

The next basic step in the understanding of the secondary interactions, which followed closely after my counter experiments, was the well-known cloud chamber work by Blackett and Occhialini at the Cavendish Laboratory. It so happened that in 1931, at my suggestion, Occhialini had gone to the Cavendish Laboratory to work with Blackett. He was bringing with him the experience in the coincidence technique developed in Arcetri and was planning to learn from Blackett the cloud chamber technique, with which, at that time, no one in Italy had any experience. The collaboration of Blackett and Occhialini had produced the counter-controlled cloud chamber. Among the first pictures obtained with this instrument, many showed groups of particles, which they named *showers*; these were clearly identical to the groups of particles produced in the secondary interactions which I had detected with my coincidence experiments. These cloud chamber pictures and the results of my counter work were to form the experimental basis of the theory of showers developing shortly thereafter.

In the late fall of 1932, having won a national competition for a chair in experimental physics in the Italian Universities, I was called to fill a vacancy at the University of Padua. I left Arcetri with a heavy heart. There would be other periods of interesting and rewarding work in my life. None, however, will have the very special flavor of my years in the Florentine hills.

* * *

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Author with Eric Regener and Lise Meitner on the Bodensee in a boat used for underwater experiments. Regener, who had baptized the boat "Undula" to reaffirm his faith in the wave nature of cosmic rays, told the author, "If it turns out you are right, I will have to rename my boat 'Korpuskel,' which does not sound as nice as 'Undula'!"