New Techniques in the Cosmic-Ray Field and Some of the Results Obtained With Them*

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EACH new field of scientific study goes through a period of development which may be likened to the development of a man child. Up to the age of six or seven a boy interests everybody intensely. In babyhood he is new and sweet and altogether charming because of the awakening qualities which bloom forth as he becomes a real person, and from then on up to seven or eight he is intensely interesting because he is revealing day by day what kind of a person he is likely to be. Then from say eight to thirteen he goes through a period in which he is in general best described as a whining brat who knows no law, is always against the government, and has no real friend except his mother. From about thirteen on he begins to find himself as a member of the human family, and by the time he is twenty-one is ready to don the toga and be admitted regularly into the ordered society of adults.

It is now just over twenty-one years since Gockel[1] made the balloon ascent which revealed the new and quite unexpected fact that there were penetrating rays at an altitude of 4 kilometers of greater intensity than at the surface, i.e., at a height to which no rays could possibly reach, as everybody then knew, if they came from the earth. The cosmic rays ought therefore to be now of age. But the war robbed them of six or seven years of their life, and this may account for the fact that they are just now emerging from the lawless, friendless period into the period of orderly, recognized behavior. But until this emergence is more or less complete, the less public attention is focussed on the "ugly brat" the better it is both for him and for the public. In the lawless or controversial state, before the simple facts have been determined and sufficiently checked to become generally recognized by physicists, the child is of no interest or value to the public. Better for him to have no friend except his scientist mother until he learns to behave himself.

I shall then have little to say today about the controversial aspects of the cosmic-ray field. These will not be controversial long, nor are they anything like as controversial now as the public, for some reason, seems to think. Indeed so far as all the major and really significant facts of observation are concerned, there is general agreement now. There are differences as regards accuracy of measurement and there undoubtedly are differences in interpretation, but so far as I myself am concerned, nothing has happened which alters in any essential or fundamental way the views which I expressed in my last comprehensive report written more than a year ago and printed in the Proceedings of the International Electrical Congress[2] held last July in Paris. I hope very much that anybody who is really interested in the subject will take the pains to read that report before expressing or even forming an opinion, for within the past ten months the American newspapers have been quite consistently misunderstanding and misrepresenting my findings, and I suspect that even some of my physicist friends, who have been too busy with their own work to look up the facts, have been pretty thoroughly misled by incorrect reports.* However, I shall not consume precious time by making the necessary corrections now, but content myself with the mere statement *that the present report is to be regarded as a supplement to the aforementioned one*, for that report, insofar as it goes, will actually stand today with scarcely a change beyond the deletion of a single line which represents merely a slip that should have been deleted anyway, even if no further work had been done (see below). I shall indeed present today new experiments, new techniques, and new results, but these have actually served in the main merely to confirm formerly expressed views, though adding some new and important experimental facts, especially in the fields of energy measurements and of geographical distribution. So far as possible, too, I shall confine myself to results which have been so fully checked and so often repeated that they will now meet, I think, with general acceptance; for this is the stage at which they become both of interest and of value to the public.

The first important and non-controversial fact is that the cosmic rays ionize through the mechanism of the passage

* Address delivered at Atlantic City on December 30, 1932, as part of a symposium on cosmic rays.

[1] A. Gockel, Phys. Zeits. **10**, 845 (1909); **12**, 595 (1911).

[2] Etat Actuel de nos Connaissances sur le Lieu et le Mode de Production des Rayons Cosmiques, Congrès International d'Electricité, 1932. See also Annales de Institut H. Poincare.

* Even A. H. Compton (Phys. Rev. **41**, 682 (1932)) altogether erroneously reports Bowen and myself as finding but 6 ions at 16 km, and then draws conclusions from this fictitious number which have not the remotest relation to our findings. We actually have never made any estimate as to the ionization at 16 km. Our only statement has been that our flight "seemed to give the *mean* value of the ionization between 5 km (where the value is 15 ions) and 15.5 km as 46 ions." For three years we have repeatedly stated that up to 9 km Kolhörster and ourselves are in substantial agreement so that Dr. Compton's observations at about half that height have not the remotest bearing on 16 km values.

with speeds approaching that of light of charged particles which shoot through electroscopes or other observing instruments and ionize in the well-known fashion of electron rays. In the term electron rays I shall include both positives and negatives, i.e., protons and negative electrons. What seemed to Bowen and myself unambiguous proof of this mechanism was first worked out in the spring of 1931, this proof being found in the observed pressure-ionization relations in pressure electroscopes. We did not publish our findings until I had worked out in July and early August, 1931, also the predicted influence of diurnal changes in temperature upon the observed ionization within a pressure electroscope. When these also checked we sent our findings to *Nature*, which published them in the October 3d issue, 1931, but on September 14th I had presented them orally to Steinke and, a few days later, to Hoffmann in Europe, who at once agreed with our conclusions. Also, a short time after our first publication Bennett, Compton and Stearns, who had independently reached similar conclusions, published their findings, so that, so far as I know, this conclusion, namely, that the cosmic-ray ionization that we observe at the earth's surface is due to high-speed charged particles, is now considered as definitely proved. At least it seems to be universally accepted.

However, the very significant fact that *proton* rays, as above stated, as well as negative electron rays, are the immediate ionizing agents and in something like the same proportions was first brought to light in September and October, 1931, by Carl D. Anderson's measurements[3] on the curvature of these charged-particle-rays in a very powerful magnetic field with the aid of a special vertical Wilson cloud chamber devised by himself and the writer more than two and a half years ago, and permitting 6-inch ray-tracks to be obtained in a uniform magnetic field of strength up to 18,000 or even 20,000 gauss. This is one of the new techniques mentioned in the title and one that extended the range of the measured kinetic energies of charged particles from about 15 million volt-electrons, the limit prior to 1931, up to the order of from two to four billion (10^9) volt-electrons, where it now is. The possibilities of measurement go even higher but no higher energies have been found. Also much caution must be used in studying these nearly straight tracks because of minute sudden changes in direction which sometimes appear. For example, we have cases of nearly straight tracks which seem to show a very slight positive curvature before traversing a lead block a centimeter thick and a slight negative curvature after so doing—an obviously secondary influence of some kind, probably a minute deflection due to a close encounter and not a real curvature at all, and of course such an influence may produce either an apparent increase or decrease in a curvature.

But that positive electrons or proton rays constitute something like half of the immediate ionizing rays, and that some 15 percent of these positive ray tracks are associated with negative ray tracks starting from the same center *constitutes the strongest sort of evidence that both tracks arise immediately from the disintegration of the nucleus of an atom which has been hit by a primary ray of some sort*, in other words, that the *immediate ionizing agents in the cosmic rays are themselves secondaries released from the atoms of the atmosphere by non-ionizing primaries.* I say non-ionizing primaries because we have placed strips of lead a centimeter thick in the middle of the cloud chamber and have repeatedly directly observed ionizing tracks, some times negatives, some times positives, some times both, simultaneously to spring out of the lead when no visible ionizing track enters the lead.[3] This is direct, visible proof of what Hoffmann, Geiger and others long ago showed by placing thin lead sheets above a cosmic-ray electroscope and noting that the ionization in the electroscope was increased instead of decreased by the interposition of the lead, a phenomenon long known with gamma-rays and meaning merely that new secondary charged particle rays are created in the lead by the absorption by the atoms of the lead of the incoming gamma-rays.

Dr. Workman, one of the National Research Fellows in our laboratory, has recently completed an elaborate study of the ionization produced in an electroscope by the hardest gamma-rays as a function of the thickness of a given substance (expressed generally in terms of number of electrons per square centimeter) interposed between the source of the gamma-rays and the ionization chamber. Each material, of course, has its own characteristic curve. If the aluminum curve, for example, is being plotted and successive sheets of aluminum have already been introduced, and if, from a given thickness on, lead sheets are used in place of aluminum, the new points at once leave the characteristic aluminum curve, rise above, then fall below it, and slowly move over to the characteristic lead curve, and reach it as soon as the secondaries released from the originally interposed aluminum sheets are no longer able to get through the added lead sheets. Now, Schindler[4] in Steinke's laboratory has recently published exactly the same sort of curves taken with cosmic rays instead of with gamma-rays, and has obtained exactly the same relation that Workman has obtained with gamma-rays, thus demonstrating again and in a new way that precisely as in the case of gamma-rays the ionizing cosmic-ray-particles are indeed secondaries released by incoming non-ionizing primaries.

But the third and perhaps the most complete demonstration of this conclusion is furnished by Carl Anderson's measurements of the energies of the actually observed cosmic-ray particles; for these measurements show that the majority of these energies lie below 600 million volts, Dr. Anderson and I having published the estimate that not more than a tenth of these tracks reach appreciably above the billion (10^9) volt range.[3] But now we know with great definiteness and great certainty, too, the maximum distance of penetration of billion volt-electron rays through the atmosphere; for such rays, be they positives or negatives, (for at these energies the differences are small) make at least 30 ions per cm of path (45 ions seems now a better mean), and each ion removed from the molecules of air

[3] Millikan and Anderson, Phys. Rev. **40**, 325 (1932); and C. D. Anderson, Phys. Rev. **41**, 405 (1932).

[4] Heinz Schindler, Zeits. f. Physik **72**, 625 (1931).

represents an expenditure of 32 volt-electrons of energy; or such a billion volt-electron as we are considering, and in the case of electrons the same is true of all lower speeds so long as the energy is as high as a million volts (in the case of protons the penetration at low speeds is much less)—loses at least a thousand volts per cm of air path, which means that the maximum range of a billion volt-electron in air cannot be more than a million centimeters or 10 km, more accurately 9.6 km. Indeed Heisenberg[5] makes it but a third that much for billion volt particles. But the depth of a homogeneous atmosphere of the density existing at ordinary temperatures and pressures is 1033 divided by 0.00117 and therefore equals 8.8 km. In other words, a billion volt-electron, if it happened to come in normally to the earth surface, might possibly just get down to sea level and then pass through a roof and an electroscope-wall having a combined thickness equal to a cm of brass. Since the rays come in from all directions, even if all of them were billion volt-electrons, a wholly negligible number of them could possibly reach such an observing chamber. *Since something like nine-tenths of the actually observed ionizing rays have energies under this value, nearly all of the rays observed in such a chamber must then be secondaries, not primaries.* This conclusion seems to me altogether inevitable from the foregoing facts alone. The only possible escape is to say that our measurement of the energies is wrong. It might be slightly wrong in the higher volt range, but it cannot be wrong in the only significant range which is the lower volt range, as anyone can see if he will examine our photographs. Further, Kunze[6] in Fuchtbauer's laboratory in Rostock, has duplicated our measurements in Germany and gets the same distribution of energies as we do. He lists a negligible number of energies up to 4 or 5 billion volts, but, as heretofore indicated, this listing perhaps represents less caution about his high energy measurements than we use, though this is of course quite unimportant for our present considerations.

So far as I can see then the only escape from this argument that these ionizing particles observed at sea level originate practically wholly in our atmosphere is not only to deny the validity of these direct measurements of energies but to postulate, as some have attempted to do, incoming charged particle energies at least ten times, or even a hundred times, higher (10^{10} or 10^{11} volts) for the usual calculations require these energies if the rays are to get through the earth's magnetic field into the equatorial regions at all. This postulate clashes head on and altogether disastrously both with the observed distribution of the cosmic rays in altitude and in azimuth (inclination with respect to the vertical). For 10^{10} volt or 10^{11} volt incoming rays of this sort will produce an atmospheric ionization *very nearly independent of altitude*, instead of increasing very rapidly as the facts require; and as to azimuth, *such rays would produce a uniformity of distribution* of the observed ionization at sea level over the whole celestial dome. But for at least seven years I have been testing this last point in mountain valleys and basins, and I have never found any measurable decrease in the observed ionization when the horizon was screened by mountains up to 30 degrees or even 40 degrees from the horizon. In other words, practically all the rays come in at sea level through a vertical cone of apex at the observer and of 100 degrees or at most 120 degrees opening.

But I am perhaps spending time unnecessarily upon the point of the atmosphere origin of the observed ionizing cosmic-ray particles, for I do not see how it can be doubted for a moment by any one who is at all generally familiar with cosmic-ray facts. There is, however, one further closely related point which should be mentioned. For the main facts which have at any time led anybody to postulate incoming electrons of an energy of 10^{10} or 10^{11} volts are, first, so-called Geiger counter coincidence measurements on absorption coefficients, and second, the aforementioned geographical distribution of the rays. With respect to the first, I have been pointing out for two years in Pasadena seminars, in the Rome congress on nuclear physics in October, 1931, in New Orleans last Christmas at the A.A.A.S. meeting, and in the report for the Paris Electrical Congress, that these counter experiments never in my judgment actually measure the absorption coefficients of anything. I shall presently show that no appreciable number of these observed ionizing particles ever go through more than 30 cm or at most 60 cm of lead, and yet both Regener and Cameron and I have proved that the cosmic rays penetrate through the equivalent of more than 20 feet of lead. These figures cannot both be correct without carrying with them the conclusion that the primary rays at sea level and below are not charged particles. The mechanism of these coincidences is, I think, as follows:

When two counters are separated only by a foot or two of interposed air the coincidences are due indeed wholly to these observed electrons (using now this word to cover both negatives and positives) which shoot through both counters. When, however, 15 cm of lead are interposed between the counters, as in Mott-Smith's experiments, the electrons of energy five hundred million volts or more, of which there are actually a considerable number, do indeed shoot in succession through the upper counter the lead and the lower counter just as is usually assumed: *but the number of coincidences thus produced is now augmented by new coincidences due to the practically simultaneous emission of a succession of secondary particles released along the path of a photon by Compton and other encounters*, the chance for the emission of such secondaries being now enormously increased by the large amount of new matter interposed between the counters. The result is that the number of coincidences with the interposed lead is very much larger than that which corresponds to the absorption coefficient of the charged particles that get through. Messrs. Pickering and Sharp in our laboratory have just now obtained certain confirmations of the correctness of this explanation. It will be seen that if this view is correct these so-called absorption coefficients when thick layers of matter are interposed mean then nothing at all, and the conclusion about particle energies that have been drawn from them are quite invalid. The conclusions to be drawn from latitude effects will be treated below.

[5] W. Heisenberg, Ann. d. Physik **13**, 430 (1932).

[6] Paul Kunze, Zeits. f. Physik **79**, 203 (1932).

The foregoing interpretations gain further support from the new measurements made in the Norman Bridge Laboratory by Carl Anderson on the actual losses in energy experienced by cosmic-ray secondary electrons in going through lead sheets one or more centimeters thick. These measurements are upon the changes in curvature of the same electron tracks (plus and minus) above and below strips of lead a centimeter thick through which they have passed. The average loss in energy is 35 million volts per centimeter, which agrees within less than the uncertainty of measurement with Heisenberg's computed value of 39 million volts per centimeter.[5] It will be seen from this that a billion volt ray has a range in lead of but 30 cm. The great majority, then, of all coincidences obtained through more than this thickness of lead are due if this theory is correct to successive secondaries released along the path of a photon.

Measurements of the type I have been considering in the foregoing paragraph yield other important information; for *even the straightest of these tracks always show measureable changes in direction in traversing the lead.* The statistical way in which these deviations vary with incident energy furnishes a new way of measuring energies, supplementary to the method of magnetic-deviability, and applicable to higher energies. This method alone seems to show that no tracks corresponding to energies of 10^{11} or even 10^{10} volts are actually present among those thus far worked with. Really quantitative conclusions, however, must await fuller statistical data upon such energy losses in traversing lead.

With all these lines of approach, and still others to follow, the two conclusions that seem to me no longer controversial are thus far (1) that the observed ionization at sea level is due to charged particles, and (2) that certainly more than nine-tenths of these particles are secondaries formed in our atmosphere. A third thus far non-controversial result has to do with the general nature of the curve representing the way in which the ionization varies with altitude. I have repeatedly pointed out for the past two years, both in speech and in print, that the curve obtained by Dr. Cameron and myself relating the observed ionization to depth beneath the top of the atmosphere and carried by us with great accuracy up nearly to 5 km runs sufficiently closely into Kolhörster's balloon observations, which he carried up to 9 km. It also runs into the more accurate airplane observation up close to 9 km recently made by Mott-Smith and Howell[7] and Millikan and Neher (see below). In other words, Kolhörster and ourselves are in reasonably close agreement up that far. I have further based the conclusion that this exponential rise, observed by all of us, and reaching in its upper part a value close to $\mu = 0.6$ per meter of water could not possibly continue to the top wholly upon the integrated value of the ionization in the 15.5 km balloon flight with self-recording instruments which Bowen and I made in 1922, and I have repeatedly drawn the important conclusion from this and other phenomena that the incoming rays could not be in complete equilibrium with their secondaries upon entering the atmosphere, since incoming rays in such equilibrium should yield such a continuous exponential rise. *The absence of this exponential rise, and of the equilibrium of the primary beam with its secondaries upon entering,* is the sole conclusion which I have ever drawn from our 1922 flight. If the entering rays are *entirely unmixed* with secondaries upon entering, i.e., if they constitute a pure photon-beam, there should of course be a maximum in the depth ionization curve somewhere on the way to the top, and a year or more ago when Piccard reported a lower observed ionization at 16 km than at 9 km, I thought that this maximum must be found within those limits and so stated, chiefly on the basis of Piccard's briefly-reported findings, in my review for the Paris congress. This is the one line that should now be deleted from that review since Piccard has now in his final report[8] changed his judgment about these two readings. But I further reported in that review that current cosmological theories "seemed to lead to rays which should be in equilibrium with their secondaries (particle rays) upon entering the atmosphere; that *one must then regard this question as not yet settled*" and that Bowen and I were repeating these high balloon flights for the sake of get-more accurate data near the top of the atmosphere, upon the degree of purity, or freedom from secondary particle rays, of the incoming beam.

With the second of our new techniques, for which Dr Neher's skill is largely responsible, and the cooperation of the Weather Bureau, which is also very important, Bowen and I have been repeating then during the past summer our high balloon flights with self-recording instruments, and in the meantime Regener[9] has succeeded in getting one such flight, too, in which he reached an even higher altitude than ours, his altitude record being 28 km while ours is 26 km, though the returns from our last flight are not yet in, and we of course hope that it reached his altitude or better. However, the two flights that are already in and computed check reasonably Regener's up to 20 km, where our electroscope records stop, and all three of these flights seem to confirm the conclusion of lack of equilibrium which we drew from our 1922 flight, though the temperature effect on the electroscope used in 1922 now turns out to have been considerable. How beautiful is the new electroscope technique in the matter of freedom from temperature effects is shown by the fact that a change in temperature from minus 77 degrees C to plus 23 degrees C causes a change of not over 2 percent in the observed readings in our test chamber.

This lack of equilibrium of the incoming rays with their secondaries,—a subject discussed more fully in the report to the Paris Electrical Congress—is attested first by the shape of the depth ionization curve as found now both by Regener and by Bowen and Millikan, for this curve is not even concave upward as the upper part of the atmosphere is traversed as it should be, and very strongly so for rays coming in from all directions, in case these rays are in equilibrium with their secondaries. Instead of this the curve is actually found to have bent over so that it is

[7] Mott-Smith and Howell, Phys. Rev. **42**, 314 (1932).

[8] A. Piccard, Comptes Rendus **195**, 71 (1932).

[9] E. Regener, Naturwiss. **20**, 695 (1932).

somewhat concave downward. It is attested in the second place by the fact that the ionization at the highest point reached (computed by assuming that the coefficient of absorption actually found by all of us, Kolhörster, Regener, Bowen and Millikan, Mott-Smith and Howell, at about 9.0 km, namely about 0.6 per meter of water, maintains its value unchanged to the top, as it should for incoming rays in equilibrium with their secondaries) is actually not half the computed value, the computation of course being made for the actual case of rays which come in from all directions. Further, if the observed coefficient 0.6 is the composite result of rays of different hardnesses, like helium rays, oxygen rays, etc., as Cameron and I have suggested, the discrepancy with the observed ionization becomes much greater. Regener himself, then, draws from his curve precisely the conclusions which we drew from our 1922 data, as well as from our present data, so that the whole field of ionization at very high altitudes is one in which there is as yet no disagreement either as to experimental findings or as to conclusions drawn therefrom by the observers themselves. It should be noted in this connection that the discovery that the secondaries are positives as well as negatives means that even with a quite small admixture of incoming particle-secondaries—though no approach to equilibrium—these positives, released from the nuclei of atoms by the relatively soft incoming primaries which seem to exist in the upper layers, say of 27 million volt energy, would ionize heavily and thus tend to push up the maximum in the ionization-altitude curve to close to the top of the atmosphere. The data close to the top are, however, not yet reliable enough to make quantitative conclusions possible.

But why is it so important to determine whether the incoming rays are in equilibrium with their secondaries or not? Because, if the cosmic rays originate in the stars they should be in such equilibrium, the primaries being mixed with secondary electron rays just as they are at sea level, and if they are not in such equilibrium, then the conclusion that the primaries originate in interstellar space gains added support, though even so no definite proof, since even if they do so originate they might still be in equilibrium (see above). In any case the incoming rays should be mixed *to some extent* with their secondaries, as I pointed out in 1931, since they must at least come through some of the matter, which small though it be, is distributed throughout space. I also pointed out in the paper written for the Paris Congress, before latitude effects had become in any way a controversial subject, that "On ne peut se reposer entièrement sur ces seules indications"; that new light should be thrown upon this important question by looking more carefully for such latitude effects, especially at high elevations and high latitudes; for at sea level under no assumptions which seemed either likely or natural could the secondary rays make their effects felt save as they had energies much greater than a billion volts, and no very considerable number of such energies were either then or now thought to exist. In other words, practically all such secondary incoming electron rays would in any case, no matter where they entered the atmosphere, be filtered out before the earth's surface was reached. But, at an elevation of seven kilometers, for example, or 23,000 feet, secondary entering rays of energies greater than 400 million volts, and many such were known to exist, might make their effects felt in electroscopes so far as penetrating power alone was concerned, and according to my own reasoning and Epstein's calculations[10] the earth's magnetic field should let these high altitude effects through in the high magnetic polar latitudes, but keep them out of the equatorial magnetic latitudes.

I did, indeed, suggest in the Paris paper that incoming secondary charged particles might produce latitude effects even in temperate latitudes through the possible agency of photons arising from the nuclear impacts of such high-speed electrons plunging through the upper part of our atmosphere, and since no such latitude effects were found between Pasadena, California and Churchill, Manitoba, the very region in which such effects were to be expected, I stated that there seemed to be as yet no evidence that the incoming rays were mixed in any measurable degree with their secondary electron rays. This line of approach involves the assumption that very penetrating gamma-rays can be produced by the passage of high-speed electrons through air, an assumption pretty certainly contrary to fact, for, first, although very soft gamma-rays are indeed produced when K electrons are knocked out of atoms by high-speed bombarding electrons, and although somewhat harder gamma-rays might be occasionally produced by similarly knocking out nuclear electrons, I think no gamma-rays capable of penetrating kilometers of atmosphere, as would be necessary if such secondary gamma-rays become observable at sea level, are to be expected from the plunging of high-speed electrons through the light atoms of the atmosphere. The energy available from the mere rearrangement of the parts of such a smashed atom is wholly inadequate. Even if such secondary gamma-rays might thus be produced they could not account for Carl Anderson's observed effects no matter what the energy of the incoming particles, *for his ray-tracks in the great majority of cases come in from above, while 10^{10} volt electrons shooting through the atmosphere should produce at the earth's surface photons shooting out equally in all directions from the bombarded nucleus in case it be assumed that they can produce such high energy photons at all.*

Nevertheless, if the incoming rays are actually mixed to any appreciable extent with their secondary electron-rays of the observed energies the effects of the latter should of course be observable at high altitudes *in polar latitudes* and absent in the equatorial latitudes, and accordingly more than a year ago, i.e., in 1931, I laid before the Carnegie Corporation of New York a definite and detailed program for the carrying out of this high altitude exploration program and also described it explicitly in the aforementioned report for the Paris Congress long before any latitude controversy was ever dreamed of. I was not primarily interested in further sea-level observations because, first, the foregoing reasoning showed that if any magnetic latitude effects could be found at sea level they would of necessity be small, and second, Cameron and I

[10] Paul S. Epstein, Proc. Nat. Acad. Sci. **16**, 658 (1930).

had in 1927 carried sea-level observations from latitude 34 north to 17 south and found no variation which was outside the limits of our experimental uncertainty, which we estimated at 6 percent. Further, the next year I had helped Dr. Gish to calibrate electroscopes which were taken on the good ship "Carnegie" and with which 500 observations were made in the equatorial latitudes from 20 degrees north to 20 degrees south, and an equal number from 20 degrees to 60 degrees south, and also half that number from 20 degrees to 60 degrees north, without bringing to light any sea-level latitude effects which were greater than the observational uncertainty which I estimated again, upon privately getting this data from Dr. Gish, to be of the order of 6 percent. However, these Carnegie observations showed, if anything, slightly lower equatorial values than higher latitude values, the mean of 507 readings between 20 north and 20 south being 3.32 ions/cm^3/sec., of 262 readings between 20 north and 60 north being 3.39, while the mean of 509 readings between 20 south and 60 south was 3.44 ions/cm^3/sec. The estimated zero was here about 2 ions/cm^3/sec. The readings in the equatorial belt are indeed 5 or 6 percent lower than those in the north temperate belt, but those in the south temperate belt differ from those in the north temperate by nearly as much. Further all of these differences might have been due to barometric differences, for which at that time dependable correction could not be made. It was for these reasons that in 1930 I made an elaborate and accurate comparison of sea-level intensities at Pasadena (lat. 34) and at Churchill within 700 miles of the North magnetic pole, for here I expected the maximum differences to appear, but I found no difference that exceeded my observational uncertainty which was stated in these measurements to be not over 1 percent.

For these 1930 observations from latitude 34 northward both at sea level and on mountains were taken with improved, very sensitive electroscopes and were very much more accurate than our South American ones. Bennett and Dunham,[11] as well as Neher and myself, have this last summer completely confirmed these findings, not only at sea-level but on mountains from five to nine thousand feet high, so that there is now entire agreement upon the only point having to do with latitude effects upon which I have at any time been insistent or even claimed to have made accurate measurements (see also below) namely that *from Pasadena northward, within the limits of our observational uncertainty, there is no effect of latitude so far as observations on terra firma, high or low, have gone.* It is to be noted, too, that according to Epstein's[10] and Lemaitre's and Vellarta's[12] calculations, these high latitudes, north or south, are the only regions in which latitude effects on intensities can be expected as a result of the action of the earth's magnetic field on incoming charged particles of the observed energies. There is no doubt at all about the abundance of secondary charged particles up to energies of 500 million volts. If the effects of such secondary incoming rays do not appear about the magnetic poles, the alternative inferences will be (1) that the incoming primaries are not appreciably mixed with their magnetically deflectable secondaries, or (2) that we have not extended our observations to high enough altitudes in the atmosphere or to high enough latitudes to bring these effects to light, *for the earth's atmospheric skin is so thin that no latitude effects of secondaries formed in our atmosphere can be expected.*

A third possible hypothesis might be made, namely, that the earth's magnetic field, like the sun's, does not extend out as far into space as that of such a theoretical magnetic dipole should. This would shift the expected effects of the incoming secondaries to lower latitudes, where we should find them if only we carry our observations high enough.

As to the mere observational facts at sea levels divorced from all theorizing, it should be reported that Clay[13] has two or three times during the past five years reported from 10 to 20 percent of equatorial lowering on voyages from Batavia to Europe via the Suez Canal, lowerings which he has related to magnetic latitudes and that A. H. Compton[14] has reported similar results obtained in the western hemisphere. On the other hand, E. Oeser[15] of the University of Göttingen has just repeated the sea-level observations between latitude 50 degrees north and latitude 7.1 degrees north (Hamburg to the Panama Canal) and reports no change whatever in his electroscope No. 1, that was airtight, but a lowering of 17 percent in the equatorial regions for a second electroscope (No. 2) which was definitely proven to be leaking and which therefore contained more rarefied air in the hotter areas than in the colder ones. But the most significant and dependable measurements of all have just been made by Dr. Neher, who has carried one of our new sensitive recording electroscopes by sea from Pasadena (34 N.) to Mollendo (17 S.) and back without finding at sea level any changes appreciably larger than the limits of the uncertainties indicated above, namely, of the order of 6 or 7 percent. However, the mere uncertain *indications* previously found of about this amount of equatorial lowering are now confirmed by these quite exact and dependable readings, which show an increase in coming from Mollendo (17 S.) to New York (41 N.) of 6.8 percent correct to one or two percent.

With respect to all my former sea-level observations, I have always been very careful to make no other statement than this: First, that "I myself have never been able to find any sea-level effect of latitude which was outside the limits of my observational uncertainty." That statement I shall obviously never have to retract, and it cannot properly be made the subject of controversy. But, further, my estimated uncertainty of 6 percent between Pasadena (34 degrees north) and Mollendo (17 degrees south), and

[11] Bennett, Dunham, Bramhall and Allen, Phys. Rev. **42**, 446 (1932).

[12] Lemaitre and Vellarta, Phys. Rev. **43**, 87 (1933). See also Nature **128**, 704 (1931) for Lemaitre's superradioactive theory of cosmic-ray origins.

[13] J. Clay, Proc. Roy. Acad. (Amsterdam) **30**, 1115 (1927); **31**, 1091 (1928); **33**, 711 (1930); and J. Clay and H. P. Berlage, Naturwiss. **37**, 687 (1932).

[14] A. H. Compton, Phys. Rev. **41**, 111 (1932).

[15] E. Oeser, Zeits. f. Geophysik **8**, 242 (1932).

of 1 percent between Pasadena and Churchill may of course some time have to change, but I think there will be general agreement that with the observations of Millikan and Cameron, the Carnegie Institution of Washington, Oeser and Millikan and Neher all in reasonably good agreement it would be quite premature to change them appreciably now.

But with respect to *high-altitude* latitude effects, as already indicated, I have thought them so likely as to publicly state in several addresses in Europe, delivered in October and November of 1931, and in the aforementioned article written in 1931, that I was undertaking a rather elaborate high altitude latitude survey. The carrying out of such a survey required the development of the second new technique referred to in the title of this paper, for the observations had obviously to be made in airplanes which can hold the same level for long periods, and they had to be as accurate as those made in a laboratory. It is largely Dr. Neher's extraordinary skill which has made the success of this program possible. The instruments are self-registering and work just as well in an automobile or a railway train running over rough roads, or even in a diving airplane, as in a laboratory. They yield the most accurate airplane observations of which I myself have any knowledge, though I have assisted in making many inaccurate airplane tests. They are very much more accurate than any balloon observations made by ourselves, and I think these compare well with the balloon observations made by others. Further, any one who henceforth wishes to check for himself our results need only come to our laboratory and measure up our films, which of course are without prejudice or preconception of any kind.

There are two main results of these high altitude airplane tests that I wish to report. With the aid of Colonel Arnold and the staff at March Field (latitude 34), we took up one of our new electroscopes inside a four-inch lead shield to an altitude of 21,000 feet, or about 7 km, and then we repeated the observations without the lead, so that we now know exactly how much of the rays at each altitude are cut out by the lead, not roughly but quite accurately. At 21,000 feet the ionization inside the lead is less than one-third what it is with the lead removed. This means that more than two-thirds of the ionizing secondary rays existing at that altitude are unable to pass through four inches or 10 cm of lead. This means in turn, since many new secondaries are of course produced within the lead, that very much more than 66 percent, *probably as much as 80 percent of the secondary ionizing rays existing at 21,000 feet, have insufficient energies to carry them through 10 cm of lead, and that means energies below about 350 million volts.* This checks very beautifully our conclusion that practically all the ionizing rays are secondaries produced in the atmosphere, for 350 million volt rays cannot possibly penetrate one-third the thickness of the atmosphere. This way of finding the distribution of the energies of the ionizing particles cannot, I think, be seriously in error. It shows that at least 35 percent of the ionizing particles existing at sea level, to take another example, have energies under 350 million volts and this checks well with Carl Anderson's direct measurement of the distribution of particle-energies. Again, Mott-Smith and Howell's high altitude measurements[7] tell precisely the same sort of a tale, for at 25,000 feet these observers report that a one-inch lead screen cuts off 40 percent of the ionization. This means that certainly much more than half the particle-rays existing at 25,000 feet have an energy under 90 million volts. All these new airplane observations, then, confirm most emphatically the banded structure of the cosmic rays, upon which I have been insisting since 1925. *Indeed, the rapidly progressive softening of the rays with altitude, here brought sharply to light, is I think inexplicable on any charged particle theory as to the nature of the primary rays.*

The other result of the airplane tests relates to the high altitude geographical findings. Exactly the same sort of flights in which the pilot flew for an hour at 10,000 feet, an hour at 14,000, an hour at 19,000, and an hour at 21,000, were made first at March Field (latitude 34, magnetic latitude 50 reckoned from pole position at 70°); then at Spokane (latitude 47, magnetic latitude 64), where the flights were most skillfully made by Major Breene, then at Cormorant Lake, Manitoba (latitude 55, magnetic latitude 75), where Flight Commander Gordon of the Royal Canadian Air Force generously and skillfully flew for us. Also, a similar flight was made in Peru (latitude 17 south, magnetic latitude 2° N.), and still another, with the assistance of the U. S. Army at Panama, 7.1° N., magnetic latitude 24.1 N. In these flights the electroscope is automatically recharged once every 15 minutes, and it is so sensitive that it moves over the 100 divisions of the scale in 15 minutes at sea level. *The Cormorant Lake and the Spokane curves show no differences whatever, and yet this is where the magnetic effect of the earth's field on negative electrons of energies up to 10^9 volts should be most felt.* Its failure to appear there may perhaps be considered as pointing again toward the absence of appreciable numbers of secondary deflectable rays of the above-mentioned, directly observed energies in the incoming beam, but an alternative explanation is that the earth's magnetic field is not as strong as is assumed in the Epstein, Lemaitre calculations.

Quite unexpectedly, however, the results at March Field at 21,000 feet are 11 percent lower than those taken at the same altitude at Spokane and at Cormorant Lake. With respect to the high altitude tests in the equatorial belt, the Peru flight and the Panama flight are in perfect agreement and yield distinctly lower high altitude readings than any of the others, 40 percent lower at 21,000 feet than the Spokane and Cormorant Lake readings. It is interesting to observe, also, that this diminution in the high altitude intensities of the cosmic rays in the equatorial regions actually stands out clearly from the published readings which Cameron and I took in the High Andes in 1926,[16] though we did not then call attention to it, nor ourselves assign to it special significance; for these readings were taken before we had developed sensitive high pressure electroscopes and we were unable at that time to lay much claim to precision. Further, we had taken but a dozen or so under-water readings at Lake Maguilla at an altitude of 15,000 feet when, as indicated in our published article,

[16] Millikan and Cameron, Phys. Rev. **31**, 165 (1928).

leaks developed in our electroscopes. We therefore relied mainly for our latitude findings upon our sea-level readings, of which we had a great many and which were quite free from suspicion as to possible effects of leaks. However, in view of the thinness of the earth's atmospheric skin and the universally assumed character of the earth's magnetic field *no latitude effects due to the earth's magnetic field could possibly be expected at high equatorial altitudes if they failed to appear at sea level.* This is why we based our conclusion as to a lack of a latitude effect chiefly upon our sea-level readings and drew one single ionization-depth curve through all our points taken up to that time at high altitudes both in the northern hemisphere and in Peru and Bolivia and assumed that the points that fell off this curve represented observational uncertainties, though, as any one can now see by looking at them, these points actually do show clearly the differences between readings at a given altitude in Bolivia and in the United States. For our published composite curve embracing all readings taken up to the end of 1926[16] is actually considerably lower at its upper end, e.g., at 7 m beneath the top of the atmosphere, than is the corresponding curve taken in the northern hemisphere either in 1925[16] or in 1931.[17] Indeed, the mean values of the ionization at a depth of 7 m beneath the top of the atmosphere is 16 percent higher in the Muir Lake data than in the Lake Maguilla data, and when we compare the more accurate 1931[17] curve we find just as does Compton, a difference of more than 30 percent at this same altitude. In other words, from this last curve the reading corresponding to a depth of 1 m beneath the surface of Lake Maguilla should have been more than 6 ions instead of the observed 4.1 ions. In other words, the *published Bolivia readings are actually at least 30 percent lower than the readings taken at the same altitude in the central part of the United States.* I have often discussed with others this difference between our curves taken in South and North America, and it was chiefly because of this discrepancy and of the aforementioned readings taken on board the Carnegie that I decided in 1931 to make what I regarded as a crucial test of this latitude question by comparing intensities at Pasadena and at Churchill, the only latitude stretch in which I estimated that the effect of the earth's magnetic field could cause large differences at any altitudes. In a word, then, *the lowering of the observed cosmic-ray intensities at high altitudes in the equatorial regions is consistent with all the high altitude observations which have yet been taken by anybody, so that there is no chance for controversy about this point.* Only at sea level is the observational data not yet in quantitative agreement. In the matter of interpretations there are quite naturally differences of opinion.

With respect to these interpretations it is quite clear that this new high altitude data in different geographical areas cannot easily be brought into consistency merely by invoking the earth's magnetic field, for there are real difficulties which a magnetic interpretation encounters. The first is to explain the fact that the small sea-level dip in intensity occurs only in quite low, i.e., in equatorial latitudes. According to a hypothesis which Epstein and I have considered and which he will presently publish in quantitative form, it is sought to explain this dip as follows: The photon hypothesis as advanced by Millikan and Cameron is adopted to account for the whole of the observed ionization in the equatorial belt, for the earth's magnetic field would in any case prevent the penetration of particles of the observed energies into this belt. But of the ionization observed at sea level between Pasadena and Churchill, only 93 percent would be due to incoming photons while to account for the 7 percent equatorial dip it is assumed that there is a small admixture of charged particles superimposed upon these photons. From Epstein's point of view these particles might be looked upon as having the same relation to photons as beta-rays have to gamma-rays in radioactivity, or they might be secondaries produced by the incoming photons and therefore entering our atmosphere as a beam either in partial or total equilibrium with its secondaries. But to enable the earth's magnetic field to remove them only in equatorial latitudes and leave the sea-level intensities from Pasadena to the pole constant, Epstein is obliged, following his own and Lemaitre's computations, to postulate that these particles have energies as high as from three to eight billion volts. Such particles, however, should have so high a penetration that, in the first place, they should be observable at all angles from the vertical clear down to the horizon, which they do not seem to be, and, in the second place, they should appear in greater numbers than they seem to do in Dr. Anderson's direct measurement of energies.

The third difficulty is as follows: Although the assumed Millikan-Cameron photon hypothesis can take care of the larger part of the enormous rise in the intensity of ionization with altitude, these high energy entering particles must take care of the whole *difference* observed at any and all altitudes between the ionization in the equatorial belt and the temperate belt. This is a difference which near the Equator increases from 7 percent at sea level to 40 percent at 21,000 feet, but since the actual ionization at 21,000 feet is seventeen times that at sea level and the percentage of particle-rays is, as above stated, five times that at sea level, the ionization at 21,000 feet due to the incoming particle-rays alone must be $5\times 17=85$ times greater than the incoming-particle-ionization at sea level. But the particle-rays which must produce this sort of ionization are also required to get through the earth's magnetic field and reach the earth's surface between Balboa (latitude 7.1) and Pasadena (latitude 34), and it is this condition that limits their energies to the range from three to eight billion volts. If such high energy rays are to produce anything like the observed ionizing effects, then the particle energy necessary to just get through the earth's atmosphere must be assumed to be some five or six billion bolts, instead of but about one billion as calculated above. This seems to be somewhat higher than the present rather incomplete experimental situation allows. If it should be found permissible the three difficulties mentioned might all disappear.

But there is still a fourth difficulty. Since incoming particle-rays are continually losing energy along their

[17] Millikan and Cameron, Phys. Rev. **37**, 244 (1931).

paths their energy or penetrating power should increase, not decrease, with altitude, but as was shown sharply by the foregoing measurements of Millikan and Neher in their airplane flights made with and without their 10 cm lead shields the mean penetrating power actually decreases rapidly with altitude, being about one-third as much at 21,000 feet as at sea level. This behavior defies any natural interpretation on the basis of incoming charged particles alone, but fits nicely the theory of incoming photon bands made up of He, O, Si, Fe, etc., rays. Now, according to the foregoing hypothesis as to the effect of the earth's magnetic field, the ionization in the equatorial belt is due wholly to such bands of photons, but in the temperate belt, as is shown by the difference between the readings in temperate and equatorial latitudes, as much as 30 percent of the ionization at 15,000 feet arises from the 3 to 8 billion volt incoming particle rays. Hence, at 15,000 feet a much larger percentage of the observed rays should get through the 10 cm lead shield in the temperate and polar belts than in the equatorial belt. Actually Neher and I found by quite accurate airplane measurements in the United States at 15,000 feet 38.7 percent of the ionization inside the 10 cm lead shield that we found outside it and in the equatorial belt Neher got the same way 39.2 percent, i.e., *the rays at 15,000 feet are of very nearly the same mean hardness in Peru as in the United States. In a word, then, both the observed very rapid change in ionization with altitude and the observed lack of effect of latitude upon hardness, or mean penetrating power, especially at high altitudes, present rather serious difficulties to any sort of an incoming-particle theory even though these particles be secondaries which are required to account for but 7 percent of the observed sea-level intensity.*

From my point of view, however, these difficulties would be reduced and perhaps entirely avoided if it might be assumed that the earth's magnetic field does not have the generally assumed strength so that secondary particles of energies more nearly like those actually observed might be mixed, in temperate as well as in polar latitudes, with the incoming photon beam, for the wide distribution of energies among these secondary-particle rays would help in understanding the increased ionization with altitude, which must be attributed as well to the particle component as to the photon component of the rays. If this weaker magnetic field cannot be postulated, nor the low penetrability that Epstein suggests, then there is no further recourse other than to postulate some secondary influence of a non-magnetic sort to account for the observed dip in the equatorial belt. It is barely conceivable that the following hypothesis might have value.

The earth's negative electrical field must be taken as equivalent to an additional resistance to the inflow of negative particles, so that if these predominate, as they ought to do in view of Compton encounters, an increase in the electrical field would result in a pushing down of the curve and a decrease in the field to an elevation of it. Now, water vapor, plus the difference in mobility of newly formed negatives and positives, is presumably the great underlying cause of the electrical separations which result in strong electrical fields in the upper air, and it is in the equatorial regions that most of the water is lifted into the atmosphere and to relatively great altitudes. The absence of an important effect at sea level might then be due to the fact that the rays that get down to sea level are so hard that the influence of the electrical field is negligible in comparison, though not so at the higher levels where the rays are as shown relatively very soft. Clay's strange results between Genoa and Suez might possibly be due to the tropical temperatures existing at higher latitudes here than in the western hemisphere. It should be possible to test the correctness of this suggestion. A few months hence we shall have more and better information upon this point.

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