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INTERNATIONAL CONFERENCE ON COSMIC RAYS AND THE EARTH STORM Part III

III-6-21. General Discussion

Powell, C. F.: I think that we are now free to begin to discuss the implications of the papers we heard in the last three days and to begin to see what basic conclusions they lead to and to ask whether there are serious contradiction between the results obtained by different workers with different methods. I know that there was a discussion suggested yesterday on the questions of the work on the μ-meson spectra, the γ-ray spectra particularly whether there was any serious contradiction here or not.

Fujimoto, Y.: Let me summarize the discussions of this morning about primary spectrum, gamma-spectrum and mu-spectrum. About gamma rays we have three contributions, two from aeroplane altitude and one from mountain. Let me write down figures they quoted, the exponent of spectra.

Exponent of gamma-ray spectrum

		$10^{11\sim12}$ ev	$10^{12\sim13}$ ev
Balloon	Bristol	2.0 ±0.5	
	Japanese	2.0 ±0.3	
Aeroplane	Bristol	2.3 ±0.2	2.8±0.3
	Soviet	1.85±0.02	1.5±0.5
Mountain	Japanese	2.0 ±0.2	2.5±0.2

Powell: The crucial point is that at aeroplane altitude there seems to be very substantial difference of view and I wonder there was any discussion about the origin.

Pinkau, K.: I think yesterday Prof. Smorodin stressed the points that at high energy the measurements in the ionization chamber are not compatible with the view that some or many of these events are produced by one particle; they can only be explained if he has at higher energies multiple particles incidence.

Powell: In that case two experiments are measuring different things.

Koshiba, M.: Do I understand correctly, then in the Russian experiment they are rather measuring $\sum E_\gamma$.

Pinkau: Yes, something like that.

Fujimoto: About mu-mesons things are not so clear as the case of gamma-rays. Up to certain energy close to 10^{12} ev, we have contribution from Durham of direct measurement of momentum. Beyond this, we have to rely upon the depth-intensity curve and use range-energy relation for mu-mesons. Making use of the depth-intensity relation, and refering the data from various laboratories, particularly including new data from Tata, the spectrum seems to be as follows.

exponent of μ-meson spectrum

direct	$\sim 5\cdot10^{11}$ ev	$\sim$2.9
depth-intensity	$\sim$ 10^{12} ev	3.0$\sim$3.3

Powell: Is it really useful to put them in the form of the power spectrum when you measure bending curves? We've got to make an absolute measurements under the certain assumptions in the interpretation. And this gives the rough picture of an increasing slope in comparison to π-mesons or gamma-rays.

Pinkau: There seems to be an indication of some steeper slope for the Bristol gamma-ray spectrum than that of the mu-meson spectrum. Furthermore, if one follows the argument of Dr. Nishimura, the discrepancy is a little bit larger because he claims that we should really compare our gamma spectrum at higher energies with mu-spectrum of still higher energies.

Koshiba: Now, is it then agreed upon the view that number of aeroplane altitude 2.8 ± 0.3 of γ-rays should be compared with high energy side of μ, that is, $3.0\sim3.3$ minus approximately 1 ?

I do like to know a little bit more clearly among those numbers Dr. Nishimura pointed out, which number should just be compared. We have to be a little bit careful. So rather than dividing all the altitude data at the magic number of 10^{12} ev, I think people ought to spend more thought, at which altitude, at what energy data should be compared. Energy range to be compared directly to the mu data should be different according to the altitude. Considering this point, I'm not clear at all so I'd be very grateful if anyone can—

Powell: Dr. Nishimura will illuminate this point.

Nishimura, J.: At aeroplane altitude, say at 200 mb, gamma rays come on the average from 100 mb. $\pi^{\pm}$ mesons responsible to μ mesons come also from around 100 mb. So, these $\pi^{\pm}$ and π° are produced at the similar altitude, and we can directly compare these two spectra. As for the energy range to be compared, the energy of $\pi^{\pm}$ meson should be similar to that of the observing $\mu^{\pm}$ meson, as the Q value is qute small. For gamma rays, the situation is different. The energy of the gamma rays should be degraded by suffering the cascade showers, when they pass through the atmosphere. If we are observing the gamma rays of 10^{12} ev, about 50% of parent π° mesons should have the energy higher than $3\cdot10^{12}$ ev. This is a consequence of our Monte Carlo calculations. If we wish to make comparison of gamma rays with 10^{12} ev, then we should compare μ mesons with energy $3\cdot10^{12}$ ev, at which now we are lacking clear data of μ meson spectrum. Looking this situation, the comparison could not be a support of the depression of the gamma ray spectrum, but is only supporting that the exponent of gamma rays with less than 10^{12} ev is 2.0.

Pinkau: One must bear in mind that we measured at the same altitude nuclear spectrum and gamma spectrum in the same stack. The difference of these two slopes is very remarkable. I think it is in the limits of coincidence. Really what I want to know is the physical conclusion derived from this data. I think it might be justified now to start speculating on that.

Nishimura: Our nucleon spectrum seems to bend gradually at high energy side. Referring the data at low energy side obtained by Russian people, nucleon spectrum at our mountain altitude goes as like as shown in the Fig. 1. In the same Fig., our gamma ray and Bristol data are also shown. If the slope of nucleon spectrum does not change, and the slope of gamma rays does change at a certain energy, as in the case of the Bristol data, some change of nuclear interaction should be expected. For instance, sudden increase of the multiplicity of π° mesons. However, we have also direct data about the π° spectrum produced in an individual event. There was a

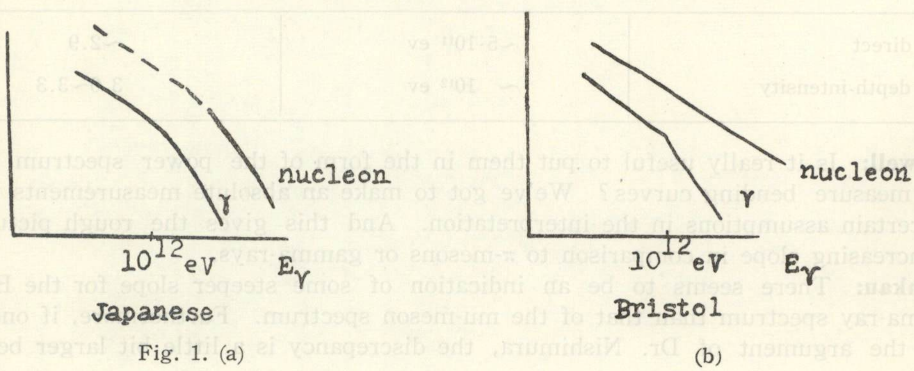
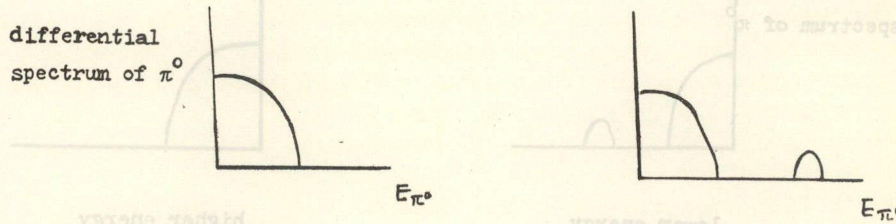


Fig. 1. (a) (b)

change of the spectrum as talked yesterday.

The energy concentrates to one π° with increasing primary energy. (Fig. 2) So the difference of the slope between gamma and nucleon spectrum should be smaller at higher energy side. Thus, about interpretation of the data, there is a great discrepancy between Bristol and ours.



low energy
Fig. 2. (a)

high energy
(b)

Fujimoto: Could I just write down the nucleon spectrum presented by different groups at the time of this morning session?

	exponent of the nucleon spectrum	
	ΣE_γ	Carolimetric
Aeroplane altitude		
Bristol	2.0±0.2 (2·10[13]∼10[14]ev)	
Soviet		1.7±0.15 (2·10[11]∼2·10[13]ev)
Mountain altitude		
Soviet		1.98±0.09 (∼10[13]ev)
Japanese	2.3±0.3 (5·10[12]∼10[14]ev)	

Powell: May I just make a remark about the point raised by Dr. Nishimura about which I wasn't clear. I think it is a powerfult point about Bristol work that when you observe γ-rays, you don't know what they are coming from. You see the streak of cascade and you pick it out. Then you find, in closer examination, that the nuclear active ones are distributed throughout the stack in the way that you would expect from the nuclear interaction length. On the other hand those which have no nuclear interaction are distributed rather close to the edges of the stack corresponding to the much earlier conversion; corresponding to the fact that in Bristol assemblies the cascade unit is very much less than the interaction length. Then at this stage only when you've measured all the energies you put them together, and you find that nuclear interactive one has the shape E^{-2} and there is a marked efficiency at higher energies for the pure γ-rays entering the stack. What I like to mention about this feature is that it seems to me improbable for unsuspected biases to play a role. Now what I want to question is this. Dr. Nishimura says, if I understood him right, the change of the slope must correspond to some change in the interaction. But the essential explanation of this effect is at first sight in terms of the idea that in nuclear interation one has an excited baryon. The essential point is that this production spectrum looks very much, as Dr. Nishimura drew it, that these low energy π's are due to a pionization process in the CMS of interaction between nucleons, whereas this group of high energy π's is produced by the decay of an excited baryon of some kind which is a common, this is what Prof. Peters will affirm, a common result on high energy interaction. Under this view the dropping off of the spectrum here is a consequence not in a change in a characteristics of nuclear interaction but the consequence of the following fact. These γ-rays are rarely produced simply because the baryon makes, at a given density of the atmosphere, interaction with another nucleus before it radiates its π-meson. We've got to be very careful of relating the spectrum of secondary particles necessarily to change in the feature of the primary nuclear interaction. Such decay processes can also distort it.

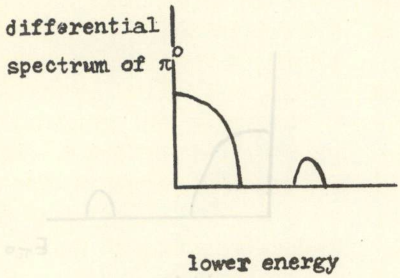
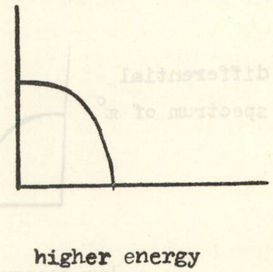


Fig. 3. (a)



(b)

Pinkau: I think that's quite true but we have a point of disagreement and I hope maybe I can now localize it. This is that if the hypothesis is right about the hyperons, then at any one altitude, say at mountain altitude, we should have the following feature like Fig. 3, if I draw here lower and higher primary energy. Because, hyperons have to have time to decay to π°. Now, this stresses just the opposite what Japanese has found and this in fact is our disagreement. You see this is exactly what Dr. Nishimura has drawn. Therefore, he drew that you should exageratedly get the gamma ray spectrum like Fig. 4. I hope, now it becomes very clear where we disagree.

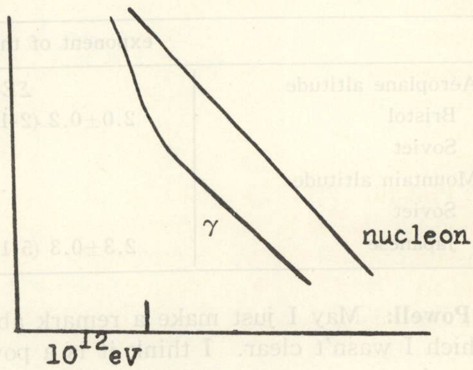


Fig. 4.

Fujimoto: I think now this part ($<10^{12}$ eV) is nearly all right for everybody. For the high energy side ($>10^{12}$ eV), the slope of gamma ray spectrum (2.8 ± 0.3 for Bristol, 2.5 ± 0.2 for Japanese) seems to agree within the experimental error. But the point is the interpretation of these data. If I take the difference of the exponent of gamma and nucleon spectra, of Bristol group, it becomes as high as 0.7, but the Japanese data does not show such big value. I think this is the point which we have to argue.

Powell: So the conclusion is that there is a satisfactory compatibility with the experimental observation obtained in different laboratory by different people by different methods.

Fujimoto: I am saying that there is no reason to believe that the slope of the spectrum should have the same value at the different altitude.

Pinkau: I think that, if I understand rightly, they would wish to stress that the slope of gamma and nucleon should be similar by the reason of π° meson spectrum produced in an individual event which they directly measured in their experiment. The difference of the slope of gamma and nucleon spectra in Japanese data is lower than ours. Under their viewpoint, our gamma spectrum should have the slope very near to 2.0 at the high energy part, because, more and more energy concentrates to the high energy π° with increasing primary energy. So, this is the point of contradiction between Japanese and our data, but I think we are getting down the crucial point. I must inquire more, how they can get the production spectrum of π° meson.

I think we can do it in the plenary session. That, I think, is a very crucial point.

Nishimura: It is my great pleasure, that these experimental values are coming near and near, and we are just inside of the experimental error. Still there is a discrepancy about the interpretation. But we are going to extend our ECC and also perhaps Bristol group is doing much bigger expedition. So in the next conference I hope we will agree completely even on the point of interpretation.

Powell: I think Dr. Daniel was telling me that, we have a very good stack recently flown in India and which will be shared between Bombay and Bristol group.

Daniel, R. R.: Size was 4 ft. by 2 ft. consisting of emulsions and tungsten, which are surrounded by graphite of thickness 1″, and it was floated for 36 hrs. The idea is to catch the nuclear events produced in graphite and also started in the atmosphere. So, we can directly compare the feature of their nuclear interactions. Gamma rays produced in graphite are such as to result in individually coming to nuclear emulsions. Therefore one is able to see the π° spectrum separately.

Powell: I think it will be very nice if we would discuss other aspects of the papers we heard. In particular, there was a very interesting early suggestion that the multiplicity of π-meson production by π-mesons as shown in ICEF stack and others indicated rather slow change with primary energy. I don't know Dr. Koshiba would like to comment upon the various views put forward and how compatible are one another.

Koshiba: I'm not sure how much it is slower than $E^{1/4}$. All the possible effect of the secondary nucleon cascade and so forth is to make it look like slower; it may be slower than $E^{1/4}$ but I don't know. My feeling is that it could be compatible with $E^{1/4}$ but perhaps Prof. McCusker will disagree with me.

Pinkau: Prof. McCusker tried to contrast difference between p-n and π-n interaction on the basis of different cross sections for the two processes and on the basis of assumptions that proton inside the nucleus would make a cascade but pion would not do so. Now this argument depends on the assumptions on the various cross sections of various processes. I have asked in the previous session to check up these numbers. From the recent Geneve Report I found that at 16 Gev total cross section for π^--p is 25 mb (in hydrogen bubble chamber) and elastic is 4 mb so inelastic cross section is 21 mb, and for p-p of 24 Gev one gets 43 for the total cross section and 9 for the elestic. So really the difference between the proton and pion inelastic cross section is only 3 to 2. It's not quite a factor of 4 to 2 which Prof. McCusker was assuming.

Powell: Prof. Fretter directed the attention to the fact that average multiplicity as shown by the analysis that Prof. McCusker made was very much greater in the low energy range than the known values measured for homogeneous groups of π-mesons at CERN. I understand you had a discussion with Prof. McCusker on this point, and reached a certain conclusion. I don't know whether you can give it.

Fretter, W. B.: The facts about this particular points are the following. The crosssection for π-N interaction is about 25 mb at 16 Gev. The cross-section determined by Prof. McCusker in his analysis of secondary interaction also gives a value about 25 mb per nucleon. This means that he is not missing very many interactions. He does not have strong bias for high multiplicity interactions, because if there are twice as many of very low multiplicty interactions which wasn't seen, then you will get a different value for cross-section. On the other hand his multiplicity of all interaction, not of those $n_s > 7$, gives $\langle n_s \rangle \sim 8$ and he believes this is not very biased against low energy, and so do I, between 10~100 Bev, and doesn't change very fast with the energy. Whereas at 16 Bev $\langle n_s \rangle = 4.2$ at CERN.

It seems clear big enough difference here to worry about. Two possibilities can explain this. One is that π strikes a nucleon and leaves behind an excited meson cloud which subsequently blows up. At energy, say 25 Gev, this meson cloud doesn't move very fast and it's quite possible that the cloud can blow up within the nucleus.

When you have a meson cloud that consists of 4 or 6 mesons blowing up within the nucleus then we have a very big possiblity for additional interactions of the pion of the cloud within the nucleus, so what started to be a collision with average multiplicity 4 can easily develop in the collision of multiplicity 8. The other point is the energies of the mesons from a cloud. Here the excited fire ball mesons. Those are low energy mesons, not of 16 Gev but of 1 Gev. At lower energy the cross-section of meson is much larger than 25 mb. You can easily set cross-section of 60, 80, 100 mb with the mesons of proper energy. These two effects, that is, the explosion of meson clouds within the nucleus because it's not moving fast at these energies, and the fact that cross section is larger than 25 mb in secondary events, I think, they are responsible for this difference of cross section.

Powell: This means that lower energy π-mesons produce higher multiplicities in emulsion than faster one.

Fretter: Prof. McCusker gets the flattening of multiplity with energy but actually it goes down. It looks fine.

Koshiba: I think his collection of low energy events is actually higher energy events. Simply because of the facts that it did happen development of nuclear cascade inside the nucleus then the apparent energy looked like low energy, so that it was classified as low energy region, and at the same time because of very fact that it develops nuclear cascade and it gave higher multiplicity than usual.

Fretter: I just like to make one more comment. It seems to me that this discussion on the multiplicity should be a rather important lesson to us and especially those who are working in emulsion. The multiplicity in p-p collision determined at CERN at 25 Gev is also about 4.2. If we take that result seriously and you believe that the variation of multiplicity with energy should be something like $E^{1/4}$ in L.S., multiplicity at 1000 Gev, for example, shouldn't be much more than 10. Therfore it looks very suspicious upon the sort of data as nucleon-nucleon collision at about 1000 Gev giving average multiplicity of 20. So I think by results of the machine you know exactly what you have, but we would not be too sure when we have zero heavy prong and nice clean jet as a nucleon-nucleon interaction.

Powell: We have to break a discussion at the moment regularly. I must say that my impression of the situation is that a profit is very much by the variety of experiment which have been done in the different conditions, with different apparatus and although it's clear that we need better information, we can see a way greatly improving the experimental information at our disposal. We have some experience with new method which again to be profitable. We heard from Prof. Dobrotin already in Moscow and we heard more here. We have a reliable way of getting a pretty good estimate of the energy of a primary interactions and then we can see what happens when the original particle strikes in light nuclei. I believe we have similar possibility from fragmentation in ICEF stack. We also have the possibility of relating between the measurements on the observed γ-ray spectrum in the atmosphere with the μ-meson spectrum and here again we greatly improve experimental results of improved statistical weight. In general we begin to see a way forward by concentrating on gaining firm and reliable information, and certainly already informations we've got allow us to make some distinction between the great variety of theoretical explanations which you can put forward when you have an indefinite number of parameters as you can get. So it seems to me we have rather rapidly improving situation that will be improved still further by our industry, and this is much no doubt to be said.