## DISCUSSION

Joos - I only want to ask, is that special for SU(3), i.e. compact group, for what you have stated?

Michel - No, it is true for connected Lie groups. I don't know exactly where to find this theorem and I don't want to argue about the general theorem. What I wanted to say is the following: Before starting to read long papers of the Physical Review about computations on bootstraps, for instance, you could have guessed the geometrical answer. Now, I leave for private discussion the precise mathematical statement of the theorem, but, please always look for a special layer.

Joos - Thank you.

Cabibbo - I have a comment on this theorem. This theorem, in the case of SU(3), can be shown very simply.

Michel - Well, let us say that eq. (18) is a proof of the theorem for SU(3).

Cabibbo - Although you have given an exact proof of the theorem on the location of extremal points of a function in the case of SU(3) by explicit derivation, I would like to say that in this case the singular layers $\gamma = \mu = 0$ and $4\gamma^3 = 27\mu^2$ are on the boundary of the region in the $\gamma,\mu$ plane where the function f is defined. In fact, for any real vector x, one can check that $4\gamma^3 \geq 27\mu^2$. The general theorem can thus (in our case) be reduced to the statement that minima of f are either at points of zero gradient, or at the boundary of the region where f is defined.

[Since this boundary is not of finite extension, the theorem can only state that extrema can be found on the boundary, not that they are necessarily there. As an example, $f(\mu,\gamma) = \mu$ has no extremal point. ]*
I would also like to comment to the extent that Michel's negative result, an $\theta$, can be extended to the following: If you assume that in a given frame of SU(3), weak interactions are completely $\Delta S = 0$, then the function of f, considered as function of $\theta$, can be written as

$$f(\theta) = \Sigma a_\ell P_\ell(\cos 2\theta)$$
$$= a_o + a_1 \cos 2\theta + a_2 \tfrac{1}{2}(3\cos^2 2\theta - 1) + \ldots$$

If $f(\theta)$ is of first order in weak interactions (see Michel's talk), then each term $\alpha_\ell$ in the sum corresponds to a term in the weak Lagrangian, which behaves as a U spin tensor with $\Delta U = \ell$. In order for $f(\theta)$ to have a minimum in a position different from 0 or $\frac{\pi}{2}$, the term $a_2$ and/or higher terms must be non-negligible with respect to $a_1$ and $a_o$. So that in order to have a minimum for $f(\theta)$ at point$2\theta \neq 0, \frac{\pi}{2}$, either weak interactions have $\Delta U > 1$, or $f(\theta)$ must be essentially influenced by higher order weak interactions. It is seen that it is the U-spin character of the weak Lagrangian more than its SU(3) character which is important here. It seems that the second possibility ($f(\theta)$ of higher order in weak interactions) is more interecting than the first ($\Delta < 1$ in weak interactions) since this would imply some $\Delta S = 2$ weak transition.

*The portion of the discussion in brackets was added in proof by Professor Cabibbo.

Teller - I would like at this moment to interrupt the discussion. Yuval Ne'eman has a little additional remark for which I would like to give a little time and which is also about symmetry.

Ne'eman - Let us return to the question of what these "y" and "q" interactions could be. There is the possibility, of course, that the symmetry breaking comes entirely from the nonstrong interactions which is probably what Michel (and Cabibbo in some recent work) has in mind. However, there is also the possibility that there is still another term which we have not identified within the strong interaction itself. Some four years ago, I called that the fifth interaction and Gell-Mann corrected me, so that in our book (The Eightfold Way, p. 282) we said that perhaps it's the fourth rather than the fifth interaction. Now that would mean that the input of the strong interaction contains a fixed pole, an interaction in the ordinary sense of field theory. On top of that input we than have unitarity and the bootstrap and all the Berkeley machinery. These create "afterwards" strongly interacting particles, which are not really elementary particles and lie on Regge trajectories. However, there would then be one fixed pole to start with. At the time I had no definite predictions with respect to that kind of model, except for the properties of its intermediate boson, if it existed. I think that recently there has emerged a possibility of observing whether or not this effect exists. Sometime ago, Yang and Van Hove and Feynman discussed the fact that at large momentum transfer, t and at large energy s, it seemed as if the proton-proton differential cross section went like the fourth power of the electromagnetic form factor $G_M(t)$. Now very recently, I have seen a paper, I think still in preprint form, by Abarbanel, Drell and Gilman, who have taken up this suggestion and studied it in as exact a way as possible, using all the available data. They seem to like the idea and find a nice fit to $[G_M(t)^4]$. Now to explain this, you have to think of the amplitude as containing two pieces, a Regge piece, and then something that would be an interaction Hamiltonian, which would have a coupling, which they called a, and then would go like a current squared, i.e. "$aG_M^2(t)$". Now if the Hamiltonian goes like a current squared, it is assumed that the behavior of the amplitude would also go like $G^2$. Now, why should one really expect to observe that kind of term? Wouldn't it be hidden by double poles, etc.?

Well, there is a study by Gell-Mann and Zachariasen in 1961 of a renormalizable pseudoscalar theory in which they showed that at very large t you can observe a renormalized coupling, an effective Born term. It's not messed up by all the other possible exchanges. Now, the proof seems to depend only on the renormalizability so that it could carry over to an interaction mediated by a singlet vector meson, which, even if it is massive, has a renormalizable theory. This means that you could expect, for instance, if the Hamiltonian is a vector current times itself, to identify this term at large t. In the differential cross section you have the Regge term squared, you have the interference term and you have the $G_M^4(t)$ term. Now, the Regge term squared goes away at large t and large s because of the shrinkage. The mixed term goes for the same reason, and you are left with the term that goes like the fourth power of the form factor. Now, this means that if the kind of dependence that Abarbanel, Drell and Gilman have now identified turns out to have the right quantum numbers, it could be the pole that fixes this y direction. It would have to be only pure isoscalar, zero and eighth component, not the third component. In fact, what you want from a current like this is that it should have some kind of dependence like $\cos\alpha\, F^0 + \sin\alpha\, F^8$. You want the square to behave like the eighth component. In the square there will be zero times zero (i.e., zero), there will be zero times eight, which will give you eight, and eight times eight which contains zero, eight and a direction in the 27. By making $\alpha < 45^{o}$, we have a large octet contribution. In this picture, we should not observe the $G_M^4$ term in the π-N elastic scattering. However, there is another possibility: the fixed pole at J=1 could have even signature (i.e., a flat, fixed Pomeranchuk trajectory with $\cos\alpha\ F^{o} + \sin\alpha\ F^{8}$ quantum numbers). This could perhaps be represented in field theory by something like the exchange of an $f^{o}$-like meson with infinite mass. Such a model would contribute to π-N elastic scattering, but not to change exchange. However, we would then need some further reason to explain why the factorized form factor is proportional to $G_M(t)$. Then that's one way of looking for the "y" input. Some of the predictions will be different in the two models (even and odd signatures) and one could determine which is correct, if any, by looking at all the available scatterings, both π-N, K-N, N-N, $\bar{N}$-N, elastic and inelastic. One could identify the quantum numbers of the pole and see whether in any case it is an 8 component. If it's both eighth and third component then we'll have to look back to the weaker interactions and see how they can generate SU(3) in an induced way. Thank you.

Teller - Any comments on this?

Tuan - What do you mean by wrong signature and what is the reason that you say that the meson may not exist?

Ne'eman - Wrong signature means (like the conventional Pomeranchuk at J=1, t=0)the pole has J=1, but does not materialize as a particle of J=1. For a Regge pole, for instance, you have the signature term which just makes the real part of the amplitude cancel there and it would not materialize. The same could happen to a fixed trajectory. It would be at the spin one level of an even signature J(t) trajectory, which corresponds also in that case to an even charge conjugation trajectory, so that it would be the spin 1 extension of the $f^{o}$ meson which is at spin 2. But if it's flat, then you have just pushed the mass of this meson to infinity. That's the picture. I don't know of any other field theoretical equivalent to a flat trajectory with the wrong signature. The realization that such objects could exist has not come from field theory. You do find it in ordinary diffraction and in an ordinary optical picture. This corresponds just to a flat pole at spin 1. There could be some objections about whether or not the Pomeranchuk could be that way. But maybe its not the "true" Pomeranchuk, maybe it's this SU(3) breaking which is coupled with a weaker coupling and perhaps that is then allowed. (This is again a fifth rather than a fourth force then).

Breit - Returning to Professor Michel's talk and Professor Cabibbo's discussion of it, it might help some of those who are not quite in this particular part of the subject (I am sure it would help me) to know just in what sense the word charge is used. Apparently, it has a mathematical sense apart from anything else, I gather that it has, but I don't know what that mathematical sense is. Is it a number that can be assigned to some kind of scalar density or just what is meant?

Teller - I think Professor Michel might very appropriately want to answer this. Are you using it in any sense that is different from that in elementary physics? Can you give an example where the charge as you use it would not be given by the charge as we learned about it in high school?

Michel - Well, if you believe that SU(3) is an exact invariance, so you can have an octet of vector current with zero divergence, then of course you can consider the integrals which are constants independent of time. I would not like to call every integral a charge, because some of those integrals do not have integer eigenvalues. So what I call a charge amongst the constant quantities which are integrals of currents, which are exactly conserved in the SU(3) approximation, are the quantities which have only 1, 0 and -1 eigenvalues (in suitable units) in the Lie algebra of SU(3) in the octet.

Teller - Do you then call a charge every conserved quantity, other than energy and momentum, which with the help of a current can be conserved?

Michel - No, I say that I want to show the quantum number to be integer 1, 0, -1, on the octet. That might be arbitary. I have to give a name to those quantities if you don't want to call it charge.

Teller - Let's have Yuval say it how he would explain the meaning of charge.

Ne'eman - I just want to help to clarify it. It is a matter of normalization, in fact. For instance, if there were quarks then their electric charge would not have had the integer eigenvalues. In Michel's definition, he would have then decided that this is not a "charge" in his new sense. However, he works in the adjoint representation, which is the representation of the algebra itself, (i.e. the octet). In that representation, it happens that you can always diagonalize two quantum numbers that have integer eigenvalues, and these are electric charges and hypercharge in the ordinary sense or strangeness and electric charge (and not the third component of isospin, for instance). That's all.

Teller - And therefore, in this sense, if you introduce something crazy like the quarks, the quarks don't have a charge which is quantized in this particular way.

Ne'eman - In the definition that Michel gave, he could of course change the definition.

Teller - Therefore, the charge as used here means more and means less than usual. It means less because the quarks have no charge, unless appropriate changes are made. It means more because it also refers to strangeness and not only to charge. May I ask Gregory, does this satisfy you?

Breit - I think this question of mine is satisfied as much as it can be in such a meeting. May I ask one additional question? I don't know who it should be addressed to primarily, one of the two last speakers I am sure, and that is: Just what kind of substitute for a clean dynamics are you accepting in the discussion of symmetry breaking? On the one hand, I have heard the word perturbation Hamiltonian, on the other hand the language in the discussion seems to be dispersion relations. Now perhaps it is obvious that everything amounts to the same, but it is not really obvious to me, because you speak of a Born term and presumably you mean first order Born perturbation theory which was invented really for Hamiltonians, but it is mixed up with a Regge trajectory. I would appreciate some illumination of this point of view.

Ne'eman - First, when I use the word "Born term", I really meant a renormalized three point junction squared. Suppose you have a field theory and not bother about the strength of the coupling and whether we are allowed to use perturbation theory or not. Assume we have an interaction mediated by some kind of intermediate bosons. Take the case where a proton is being scattered on a proton. This intermediate boson is being exchanged in a field theory sense. My comment was that according to this 1961 paper of Gell-Mann and Zachariasen you would be able to distinguish this effective (renormalized) exchange, multiple boson exchanges, for instance, with all their cross terms. This happens because at very large s and t, this pole term would dominate. That's the idea. So it does not require the coupling being smaller than one. Even if the coupling is larger than one, the energetics see to the other terms' relative weakening, and you have a hope of observing a 3-point term squared. This was the point. Now to the question of whether this is Regge dynamics, a dispersion relation, or a Hamiltonian theory, the answer is the following. The assumptions are the following: you assume that all hadrons that we observe are non-elementary. They are really sitting on Regge trajectories. They are just poles at integer values of J and at intervals of two units of spin along these trajectories. The existing particles are all "made of" each other, in the sense that if you scatter a pion on a proton you can remake a proton, so that the proton is made, among other things, from a pion and a proton. That's the bootstrap. Now on the other hand, you do use as an input, one single "fixed" pole. It is not a Regge trajectory, and could even be described as one "elementary particle" if it happens to have the appropriate signature, allowing a particle to materialize. In the example I used for the original fifth or fourth force, we would then have as the only elementary particle a vector meson singlet. That would be the only elementary particle that you feed into the system. It couples like the photon does to some current (a linear combination of baryon number and hypercharge, perhaps Schwinger's nucleonic charge). The field theory connected with this elementary particle is sufficient as an interaction to define a direction in the originally completely($\infty^8$ times)invariant SU(3) space. Finally, electric charge fixes the remaining directions in that space, which is no longer invariant. We know that the masses will arrange themselves orthogonally to that y direction.

Breit - May I confirm my understanding of it that you would work personally with such a vector meson as being the only elementary particle? May I ask how does this compare with a recent letter by Sudarshan and others in Physical Review Letters in which they use a vector meson and axial vector in order, well one might say, to construct the universe.

Ne'eman - I have not seen that reference.

Cabibbo - The way in which Michel uses the word charge is very technical. It refers to any vector such that if you write it as a matrix it has two equal eigenvalues and one which is equal to minus two times that. For example $I_3$ is not a charge because it has plus one minus one and zero. Charge is a charge because it has 1/3 and 1/3 and -2/3, so it's very technical. There are these vectors whose invariants lie on the boundary. Those are the ones which Michel calls charge. I don't suggest a name. Breit asked what is a charge and I would say that is answered.

Teller - May I just put in a small plea? I am sure it will not be accepted, but sometime it may be accepted. Whenever you can think of a peculiar name and a common name use the peculiar name, because the common name will give rise by association to some misunderstanding. The peculiar name, (and "quark" is an excellent example) gives a chance for the idea to be sharper. For instance in your talk, I would have liked you to stay with "stratum" rather than with layer; because we use layer more frequently and stratum we are more likely to use specifically for the purpose for which you want to use it. Of course, this also applies to charge. You should use a special name if you have a special idea.

Joos - I would like to ask Professor Ne'eman on this question, how to determine the quantum numbers of this fixed pole. Did I understand you correctly, if this equality between the fourth power of the electromagnetic form factor would hold for the isovector part, would you say the fixed pole is an isovector and if it holds for the scalar part it's an isoscalar? Therefore, it is also very important to study the electromagnetic form factors under this aspect.

Ne'eman - It can be one of two things. Either this fixed pole, if it exists, is parallel to the entire strong interactions and every direction appears in it. Think, for instance, of all eight currents of SU(3) multiplied by themselves. Such a picture has nothing to do with SU(3) breaking. It just parallels the rest of the mess. Or alternatively, it does represent input and in that case it has to be a clear simple SU(3) violation. So its got to be then the 0 plus 8 components multiplied by themselves. In that case you should not see an isovector current. You should see only the isoscalar. Now, as I was saying, it is slightly more complicated by the following. If the only possibility would have been a vector current, then for instance, in $\pi$-N scattering, it would act because in $\pi$-N scattering the only exchange allowed is an isovector object such as the p. That would be very clean. But there is this other possibility of having a fixed pole with even signature, that looks like the Pomeranchuk trajectory, which is completely flat. This couples two pions and contributes to $\pi$-N scattering. But I think it is still definable. There are enough experiments to observe the existence of current-current terms in all SU(3) directions with both signatures and we can hope to pinpoint the effect. Incidentally, in any case, you want the whole thing to have spin one just because of the property that it has to survive at high energy; what is unclear is the signature.

Joos - Thank you.

Freund - I would like just to make a remark about the possibility of this particle being a vector particle. If an elementary vector particle that is a fixed pole at one would exist it has been shown that total cross sections would have to increase logarithmically. This does not seem to agree with experiment.

Ne'eman - Could be, I don't know.