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СОКРАЩЕННЫЙ ТЕКСТ ТЕОРЕТИЧЕСКОЙ ДИСКУССИИ 7 ИЮЛЯ 1962 ГОДА НА МЕЖДУНАРОДНОЙ КОНФЕРЕНЦИИ ПО ФИЗИКЕ ВЫСОКИХ ЭНЕРГИЙ

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Дубна 1963.

7 January 1963

To : Professor D. Blokhintsev
Professor N.N. Bogoliubov
Professor G. Breit
Professor G.F. Chew
Professor M. Gell-Mann
Professor W. Heisenberg
Professor R. Jost
Dr. C. Lovelace
Professor S. Mandelstam
Professor J.R. Oppenheimer
Professor V.F. Weisskopf
Professor C.N. Yang

From : L. Van Hove

This is a slightly abbreviated version of the theoretical discussion held on Saturday 7 July during the 1962 International Conference on High Energy Physics at CERN. We feel its contents is interesting enough to be mimeographed and sent to the participants of the Conference.

If you wish to correct the remarks you made during this discussion would you kindly send your corrections by return mail.

VAN HOVE

We thought it might be useful to organize this informal session on theoretical aspects of elementary particle physics because of two reasons: first it might give us the opportunity to have a look at the theoretical developments in a broader perspective, going perhaps a little further back in time; and secondly, it should allow us to have longer discussions than is possible in the regular sessions of the conference. The intention, namely, is that a few people will speak, more raising questions than presenting results or developing theories, and that there will be a lot of interruptions.

If you allow me I start by listing a few points which have struck a number of us, certainly me, as very interesting developments in the theory of elementary particles and high energy phenomena, and probably many of our discussions will concern them in some way. It would also be extremely nice if additional points would be brought in by you later on. My list is the following.

I. Regge poles. I would like to distinguish four points:

- Regge poles in potential theory.
- Their significance for the asympotic properties of scattering amplitudes, also the relativistic amplitudes with their supposed crossing relations and analyticity properties. This raises in addition the more comprehensive question of a complete set of boundary conditions for the scattering amplitude when its two variables tend to infinity.
- Their relation to the mass spectrum of particles and resonances.
- Do Regge poles occur in field theory?

II. The dynamical theories of elementary particles. Here we distinguish:

- The non-linear spinor theory, as developed by Heisenberg and collaborators and as studied also by Namby and Jona-Lasinio.
- The so-called "bootstrap mechanism" proposed and studied by Chew and his group.
- The idea of degeneracy of the vacuum which Nambu and Heisenberg have introduced for the first time in the theory of elementary particles. If this idea, closely connected with the problem of broken symmetries, would turn out to be part of a future theory of elementary particles, this would be of absolutely fundamental significance not only in theoretical physics but also from the standpoint of philosophy of science.

III. Recent progress in quantum electrodynamics.

- The most remarkable development here is the work by Yang and Lee on the electromagnetic properties of vector bosons, especially the result that the first radiative correction to the quadrupole moment of a vector boson is in $\alpha \log \alpha$, where α is the fine structure constant. For the first time, a finite result has been obtained in electrodynamics containing an essential singularity in α. This, I think, if it is there to stay, is of very great significance.

I will now ask Professor Heisenberg to open the discussion by presenting a number of considerations and questions.

HEISENBERG

I would like to make a few remarks about the relation between Chew's assumptions in field theory and our own assumptions. But, to start, may I perhaps say that we all, when quantum field theory was started, have neglected one fact which really turned out to be extremely important. This is that Einstein, when he introduced the theory of special relativity, changed from forces at long distance to local forces. And we had not realized that this was such a tremendous change. Quantum mechanics, of course, was like the old Newtonian theory with forces at a distance, and when we had to go over to quantum field theory we had to introduce local interactions, and this apparently has made all the trouble.

One could perhaps classify the different attempts at an axiomatization of a theory of fields or elementary particles in the following way.

1. The minimum which we require to describe experiments is an S matrix with certain properties. So our first axioms are existence of the S matrix, and this S matrix must be unitary and it must have so much analyticity that it represents what is observed as causality. Also this S matrix, of course, must have invariance for the Lorentz group, the isospin group and so on.

2. While this is, I think, definitely the minimum which we have to take in order to describe the experimental situation, one can be more optimistic and add other axioms, requiring the existence of a local field operator, call it $\chi(x)$, commuting or anticommuting at space-like distances, and the existence of a corresponding Hilbert space. So far, we have not stated anything yet about the metric in Hilbert space. Causality will be represented by some kind of differential equation in $\chi(x)$.

3. Finally one can still be more optimistic and add the postulates that the metric in Hilbert space shall be positive, and that the asymptotic operators shall be sufficient to construct the complete Hilbert space.

Using this rather rough classification, I would say that Chew apparently tries to start with the kind of axiom system mentioned under 1., and he omits 2. and 3.. Then, of course, in order to get definite results, one must stress more the properties under 1., requiring as much analyticity as possible and, for instance, an assumption concerning Regge poles is an assumption in that direction. Postulates 1. and 2. actually correspond to what we assume in our non-linear spinor theory, we do assume that there is a local field but we do not assume that there must be a positive metric, we leave it to the mathematics to decide, and actually it comes out that the metric is indefinite.

OPPENHEIMER

This order of axioms seems to me to describe the relation of your theory to others, but is it not also possible to include in 1. some elements of 3. on the existence of the asymptotic states and their completeness in Hilbert space, and from this to deduce the existence of field with some elements of locality, without insisting on total locality. Perhaps the field concept introduced by you may do this if you go to axioms 3. or some modified form in which an indefinite negative metric is allowed, but is curbed, is made innocent, and then the question of how local fields are may still be an open one. It does not seem to me that locality, as we formulate it, has to be a logical precondition for axioms 3..

HEISENBERG

You would actually say that if we have 1. and 3. we have in some way already a kind of field operator from the asymptotic states.

OPPENHEIMER

Right, from the interpolation between the asymptotic states.

VAN HOVE

I would like at this point to ask a question concerning weakened forms of locality. I assume that what Professor Oppenheimer refers to is the fact that if the S matrix is Lorentz and TCP invariant, one can then, for every interpolating field between the incoming and the outgoing fields, write down a relation of weak locality or weak commutativity. What is unclear to me is how much this has to do with any locality at all. We could ask Jost to comment on this question.

JOST

I agree, I think, with the statement which Van Hove implied. It is a result of a paper which I wrote that the weak locality is completely equivalent to the existence of an anti-unitary involution which leaves the S matrix invariant, that is, to TCP invariance. Now, the work "weak locality" is I think from Dyson, and it is really a very, very weak locality, it has not the essential elements of locality in so far as it does not lead to analyticity properties of the S matrix. It may be a somewhat unhappy work, but certainly strong locality implies weak locality and in this sense, I think, the work is not too bad.

HEISENBERG

If you assume analyticity for the S matrix, so that you actually do represent causality, would you still feel that the field could possibly be non-local ?

JOST

That is a matter of definition. It is clear that if you define causality just as the restriction which you get from locality for the matrix elements of the S matrix, there is nothing to be discussed. It is then just a tautology.

MANDELSTAM

I have just a small point but it may clarify things a bit. I wonder if the causality postulate should not be included in the second group of axioms rather than in the first group, because I think that people who do not want to use fields have to take analyticity as a postulate not connected with causality, because one cannot use simple Kramers-Kroning-like arguments to get analyticity of the S matrix.

HEISENBERG

Is it not so that you can grove at least some kind of analyticity property from macroscopic causality, like Aage Bohr ana Wanders have tried to do ?

MANDELSTAM

I am not familiar with Wanders' work, but I do not agree that Aage Bohr's assumption is macroscopic causality, I think he is really assuming much more, in fact, he is assuming so much that he can conclude dispersion relations which are contradicted in perturbation theory.

WEISSKOPF

I do not know what Aage Bohr has proved, but it seems to me clear that a theory based on axioms 1. must have some kind of causality in it, because even in such a theory the signal cannot come out before the incoming wave came in.

MANDELSTAM

I think that a theory based on 1. does not really include the time as one of its concepts, does it ?

WEISSKOPF

Yes, it does. You can construot incoming wave packets.

HEISENBERG

I also feel that this is a strong argument. You can make wave packets come in, and we want them not to come out before they have come in. This requirement should have come kind of representation.

MANDELSTAM

Yes, I agree it should have some kind of representation, but I do not think that one could prove dispersion relations from it, and Professor Bogoliubov has made the same remark.

BOGOLIUBOV

It seems very difficult to obtain dispersion relations from macroscopic causality only. The difficulty is: how can one technically define what macroscopic causality is ? There are very many ways to define microscopic causality, mostly equivalent, but I do not see any technical definition of what macroscopic causality is.

GELL-MANN

The question is, given an S matrix theory with all the necessary analytic properties, can you embed it in a theory which has all the Fourier components, including the ones off the mass shell, in such a way that they correspond to a field theory, the field being local. It seems to me that on both sides we have some ignorance. If we want to deal exclusively with the S matrix on the mass shell and try to guess, or abstract from the Feynman diagrams, suitable analyticity properties and use them for calculation, we do not know whether such a theory can be properly embedded in a local theory by forming analytic extensions of some kind off the mass shell. But it is not necessary that it be so embedded, and this is the answer to the people who would attack that kind of procedure. For the people who try to defend that kind of procedure as the only one to be used, I would say they do not know that it cannot be embedded in a field theory of the local type. I do not see that we have enough knowledge to engage in any arguments on this subject.

VAN HOVE

Could we use this point to let Professor Heisenberg continue ?

HEISENBERG

I have not much to say, I just wanted to raise questions and I am very happy if the questions actually find answers, but that seems - as one sees - pretty difficult. I would like to make two more remarks, and just turn them into questions.

One is: how do theories with a Lagrangian formalism, like quantum electrodynamics, stand when one renormalizes these theories ? In the case of the Lee model we know that an infinite renormalization reduces the delta functions on the light cone to zero, and therefore by the process of infinite renormalization we apparently get from 3. to 2., and the question is that generally true, also for instance in quantum electrodynamics ?

The second one is a question to Chew: if one makes the assumption that all poles are of the Negge type, or more generally that there are no really elementary particles, or that no particles have a bare core in the middle, then I would feel that one can hope to find a formalism in 1., or 1. and 2., but one probably cannot find a formalism in 3., because with 3. we always get the delta functions on the light cone. I would like to ask Chew what he thinks about this consequence of the Regge poles, I would like him to say at least his feeling about it.

CHEW

I feel very apprehensive about this question. I have been accused, I think quite justly, of injecting an èlement of religion into the whole subject, but Heisenberg has been kind enough to permit a somewhat religious answer, so maybe that is all right. I think that I would refer to the remark of Gell-Mann that maybe the S matrix postulates, including the postulate that all poles are Regge poles, will correspond to a situation which does not allow itself to be embedded in a field theory with axioms 2. or 3., and I agree completely that no one knows the answer at the present time, certainly not me. Heisenberg asked me for my opinion and before I give it, let me say the following. For years I did strong interaction calculations believing that I was doing field theory, this is what I had been taught and all the people I worked with used fields. But at a certain point I realized that absolutely no use was being made of the field concept in anything that I was doing. And not only that, but I did not really understand what a field was. I worked for years with scattering amplitudes, analyticity properties, unitarity, and saw that these things worked. Of course unitarity we expect to work, but the analyticity properties of the S matrix are quite remarkable, and it is extremely significant that so many of them have by now been experimentally verified. This is something which I do not think people feel, who have not been deeply embedded in the problem of strong interactions. The fact that these analyticity properties where they have been tested - actually work, is most impressive. I would say that the most impressive examples are the forward dispersion relations, which Goldberger proposed and which have stood up including the poles. Since that time there has been the detailed verification of the high angular momentum parts of the nucleon-nucleon scattering. Another point which is less precise but which was terribly impressive - to me at least -was the fact that, when these arguments were applied to the nucleon electromagnetic structure, one really came to believe that there must be meson resonances present, otherwise one just could not understand the electromagnetic structure, and they eventually developed.

HEISENBERG

May I protest against this statement ? I think that from the form factors in the nucleon-electron scattering you only can conclude that there must be a continuous spectrum with the symmetry properties which you are looking for, but I think there is absolutely no reason from the measurements to say that within this continuous spectrum there must also be a resonance state. This conclusion in favour of the resonance states, I think, was only derived because it was much more convenient for the theoreticians to calculate with one line, instead of taking the continuum. Now, I agree that when you go into details of the calculation, you may perhaps from Hofstadter's experiments find some indication that you have not only a continuous spectrum, but also some resonance state in it. But I think that the role of these resonances has been widely over-emphasized.

BOGOLIUBOV

This is not a question but a statement. You say, Professor Chew, that you do not utilize field concepts, but you for instance work with ordinary dispersion relations and with the Mandelstam representation. Dispersion relations could be proved, either from general principles or by the diagram technique of ordinary perturbation theory. Mandelstam representation, at least, can be proved for simple diagrams. But you do not work with Nambu representations, which were in contradiction with the ordinary diagram technique. So in your mind you have criteria of diagram technique, you use only those analytical principles which at least cannot be disproved easily by the diagram technique.

VAN HOVE

To continue one minute on this point, I think it is indeed important to realize how complicated the logical injections are, that are made in the practical applications of dispersion relations. Another point is that one has to make this sharp distinction between what one might call the rough qualitative successes of dispersion relations and the very definite quantitative success which is obtained for forward pion-nucleon scattering. As far as I know, this particular example stands alone on a quantitative scale of accuracy. Now, one can raise the question: if you believe only that and leave aside all the other cases, how much can we conclude as to the validity of analyticity properties? Forgetting about field theory, can one from this single instance conclude that our assumption of analyticity is on the right track ?

CHEW

I certainly do not think one could, in any completely logical way. As I say, the element of religion here is very strong. I find the principle of TOP symmetry or of crossing so very compelling that once I have seen the demonstration of analyticity properties in the energy variable it is to me almost obvious that there will be analyticity properties in momentum transfer as well. And the Mandelstam representation then, in my mind, is based on the fact that it is the only consistent realization of the combination of unitarity and crossing which has yet been written down. Professor Bogoliubov mentioned that it is motivated by diagrams in field theory, and this is completely true, but I think that these diagrams simply represent the realization of the combination of unitarity and analyticity. Polkinghorne and Stapp, and others, have made it very plausible that this topological structure is really nothing more than a combination of these two principles.

FUBINI

I have a few points to make. First, the forward dispersion relations which work so well, have also been proven rigorously from the principles of field theory, so they can be taken both as a proof of the good philosophy of just analyticity and of the good philosophy of field theory. In the case of the nucleon form factor I would like to correct a little the statement made by Professor Heisenberg. I remember that in 1957 the Berkeley group did actually compute the contribution of the two pion continuum. It was computed on the assumption that no pion-pion resonance existed, and this computation was actually a very good one. Now I remind you that in Drell's talk at the 1958 Rochester conference it was shown that these computations could not in any way agree with experiment, so that actually the theoreticians have been forced to assume a pion-pion resonance, just because a very reasonable calculation without it did not agree with experiment.

I would also like to make a third point. There is a practical question which has not been emphasized so much in this discussion , and it is the following: you can start from various sets of axioms, but there is a common thing in all situations, it is very hard practically to use these sets of axioms. If you start from the field theoretical axioms, you are only able to prove a few dispersion relations. The causality condition has only been used in a very small region of validity, and much of the information is not used. A similar situation is met when you start either from perturbation graphs or from general analyticity arguments.

Thus it is very hard, for the moment, to apply any of our reasonings in a region which is beyond the case of two-body problems. We know that field theory is a complicated theory because there is production. Now we are almost completely helpless in dealing with production.

In this respect, I would like to say a word of caution in the use of potential scattering. Potential scattering has been extremely helpful, first in giving a rigorous derivation of the Mandelstam representation, and secondly, in giving Regge poles. But we must remember that potential scattering contains a very small fraction of the information contained in field theory. So I would say that I am a little hesitant in adopting the religion that properties which are proved in potential scattering hold exactly, without any change, in field theory. It is very reasonable to assume that properties in potential scattering can be in some way transferred to field theory, but with the caution that something else may happen.

CHEW

I agree completely, but I would say that any complications in field theory which are not present in potential theory, and there certainly are going to be many, I believe will arise entirely from the combination of unitarity and analiticity. I do not think there will be any other source of complication.

MANDELSTAM

I would like to make two remarks. One is in connection with the remark last made by Gell-Mann: if you look at the equations you get from the so-called axiomatic field theory, by that I mean axioms 2. and 3., you will find a set of horribly non-linear, integral equations which of course involve the whole Green functions on and off the mass shell. Now one can isolate the non-linearity into those parts of the Green functions that are on the mass shell, namely the scattering matrix, and therefore solve the scattering matrix equations without mentioning the other parts of the Green functions at all, and that is essentially what we do in S matrix theory. Having done that, we have a much simpler set of equations to solve, a linear set, to get the rest of the Green functions. Whereas it may be possible that we can solve the complicated equations to get the S matrix and for some reason or other cannot go further and solve the remaining very simple set of equations to get the Green functions, I myself would think this to be an unlikely possibbility.

My second remark refers to Heisenberg's question. I would say that the assumption that all poles are Regge poles does not make it any less likely that you are going to be able to fit it with a definite metric. We know that we can get operators corresponding to non-elementary particles within the space with a definite metric, and of course the propagators associated with such particles will have singularities on the light cone, and therefore you would expect the propagators associated with all particles to have singularities on the light cone. The reason why this is not serious is that one just does not use the propagators for non-elementary particles, but I do not think that we have to bring in an indefinite metric to round off these singularities.

HEISENBERG

I would like to ask one question in this connection: I had always the impression that if you use a definite metric you will at least have some particles which are so to say bare particles with a cloud around them, and what you would have to do is always to eliminate the bare particles in the middle of the real particle by an infinite renormalization factor. But if that is true, then of course you would have come to an indefinite metric. I do not see how you could not have an infinite renormalization factor and still avoid completely this part of the bare particle being present. But I may be wrong, I certainly cannot prove what I say now.

JOST

I want to point out that it is of course terribly difficult to answer the question of Professor Heisenberg. Namely, with axiom involving 3., more exactly with the Wightman axioms, we have the very difficult problem of compatibility of the axioms with an S matrix which is different from one. I think that if you make in any way precise the axioms which are under I., and I think here of such things like saturation of unitarity, which I do not understand, and maximal analyticity, if you formalize these axioms in any rigid way, then you might easily get into the same problems of compatibility. If you ask the S matrix to be very analytic and simultaneously to have singularities which I guess would be implied by the statement that you have saturated unitarity, I do not at all see that you can avoid the danger that the S matrix then finally becomes completely analytical, for instance - I. So I would say that the problem of compatibility is certainly in a very well defined way present in the axioms 3., but somehow it is also in the background in the rather vague set of assumptions, which I hate to call axioms, under 1. But maybe Professor Heisenberg can be convinced a little by the folllowing argument: if you take non-local theory, you can satisfy of course all the axioms 1., 2. and 3. except locality, and under this condition the propagator will have a delta function singularity on the light cone and the scattering matrix will be arbitrary. You can take any scattering matrix and interpolate it with a LSZ procedure.

GELL=MANN

Let me say first that I agree entirely with Mandelstam's remark on the embedding of an S matrix with the right analyticity properties into a local field theory, by solving the linear equations; this seems to be very likely. Secondly, I agree with him that the answer to Heisenberg's oft repeated question about indefinite metric seems to be that, when you reggeize all the particles, you no longer have any need for the propagators of any of them, they do not enter into the S matrix. I would like to make a couple of other remarks. Practically speaking, we have at the moment three ways of getting analyticity properties. One of them is to start with the complete set of postulates of a local field theory. The second is to abstract the properties from the Feynman diagrams, which are a kind of laboratory of theoretical physics, but a more sophisticated laboratory than the Schroedinger equation, both of them being still very valuable laboratories. The third way is to fiddle around until we get analytic properties that seem to work, and we can then postulate that they are the right ones. Each of these ways has advantages, I think. It is not clear so far that there is any difference whatsoever among them.

It is only that the methods are quite different, the people using them are different people with different psychologies, and the rates of progress are of course very different. If you start from the axioms and you require mathematically valid proofs, the rate of progress is very, very slow, but I have not seen anything to indicate that it will be particularly hard to prove in that way any of the relations which seem to come out of the Feynman diagrams. It just will take a long, long time and a lot of effort on the part of the mathematically sophisticated people. Then, comparing the diagrams with the analyticity properties of the S matrix postulated under 1., there really is not any difference so far, because all those properties have been really abstracted from the diagrams. However, we might ask whether this situation will continue, whether it will continue to be true that all of the properties will be the same. As far as boundary conditions at infinity are concerned, there might be some differences, that is, the axioms of field theory might conceivably admit several boundary conditions, or none. Feynman diagrams seem to give fairly definite ones, at least in each order, but of course nobody can tell what happens when you sum them. The Regge poles, used together with axioms 1., seem to give at infinity quite different results from the Feynman diagrams in each order. So, as far as boundary conditions are concerned, there are some differences. But how about the unitarity and dispersion relations themselves, that is dispersion relations and unitarity relations extended out of the physical region? So far, no differences have appeared among the three points of view. The only danger that there might be some differences seems to arise from the situation that on the physical sheet, in formulating the analyticity properties from the diagrams, we deal with the stable particles and we try to work out the consequences of applying the analyticity condition and the unitarity condition over and over again to the stable particles. We get in that way poles on the physical sheet corresponding to stable particles, and all sorts of cuts. But we never get in that way, by proceeding in each order of perturbation theory, the unstable particles which are treated there as resonances. Of course, they come in on the second sheet, and the big question seems to be whether that will ever come back in the sum of the whole series to make some difference on the first sheet. And there some differences might arise between the analyticity properties abstracted from the diagrams and the analyticity properties that one will be forced to assume under 1. Chew, I believe, is working very strenuously on this question. But so far, I do not think there is really any ground for quarelling, except different psychologies of different people.

HEISENBERG

I want to make a remark in connection with what Gell-Mann just said. Of course, it is quite possible that actually the traditional axioms of a local field theory with positive metric have solutions, but I am not convinced. I personally rather feel that there is no non-trivial solution. But let us leave that uncertain for the moment. If we, for a moment, just make the assumption that these axioms have no non-trivial solution, what do we do then? I understood Gell-Mann thought that then we have not much advantage from using axioms 2. instead of just using 1., because if we cancel the delta functions on the light cone we actually come back to something which is more or less an S matrix theory. Now I cannot quite agree to this point because even if we have to omit 3. and just stick to 1. and 2., we have very much use from a local field operator. By means of a local field operator we can actually, in a simple way, construct laws which have to do with all elementary particles. We can say something about the field operator and can say how it interacts at small distances. If, however, we only speak about the S matrix, and this is now a question to Chew, I always feel the following difficulty which actually for myself was the reason to go away from the pure S matrix theory. If we study the S matrix, we must use to a large extent arguments of analyticity. Now that means, as Chew has pointed out one of the last days, that we cannot use approximations in any simple manner because, if two functions are approximately equal on the real axis, they may be entirely different at some other place in the complex plane. This would mean that in order to derive laws on the elementary particles, we would need the theory of functions of enormously many variables, say of 200 variables if we have 200 elementary particles. That is of course a hopeless task. So I just felt it is extremely difficult to work only with the S matrix, because I do not know how one can use the concept of analyticity in the more general problems like multiple production, etc. And therefore I think it would certainly be of great practical use to have a local field operator, even with an indefinite metric.

JOST

I would like to add something to the remark which I made before on the connection between elementary particles and fields. Ruelle in Zurich has recently completed a paper which proves that under extremely natural conditions the Wightman field theory, which does not start at all with the particle notion, allows the introduction of particles. However, there is no indication at all in Ruelle's work that there is a very intimate connection between the fields in the theory and the particles which come out. For example, you can easily conceive a Heisenberg-like theory which starts with a spinor field and which has no stable spinor particles, but it may have particles of other spin which are stable.

Then I think I have to answer briefly Gell-Mann's comment on the slow rate of progress in axiomatic field theory. I agree completely with him, and I would not dare to say that the axiomatic field theory was in any way successful. However, I feel that maybe one can speak about progress. For instance Ruelle's paper strikes me as progress. But of course it is a matter of taste how fast one wants to go.

I have one final comment. The conclusions from the Wightman axioms have been, until now, to a large extent trivial. Every physicist would say that what one has proved is completely trivial. In my opinion this speaks very much for the Wightman axioms, because of the following consideration. As far as we know, the Wightman axioms form a very natural frame, they start with highly abstract mathematical notions and what you can construct from them are just the things you expect, and therefore I think it speaks for the Wightman axioms that thihgs like particles come out. Of course, every physicist knows that particles exist, why does one have to prove it ? I would like to introduce here a religious remark of myself, and I do this with very great hesitation because I think science should not be religious. The following might be true: it might be true that actually the fundamental structure of a complete or semi-complete theory is extremely involved, so involved that you cannot calculate anything, that you cannot even decide a compatibility problem, but that you believe the theory simply on the strength of its internal structure. We have such theories, for instance general relativity. Nobody knows whether general relativity taken seriously as non-linear theory actually describes things correctly.

Still, we believe that this is in fact a very beautiful theory, full of physical content. Now, if this should be the case, one would always have to make very daring approximations in order to get results. To take another example, consider the properties of copper, of a copper crystal. We believe that we have the fundamental equations for such a system, but nobody in his right mind would ever dare to make a rigorous calculation of any property of a copper crystal. We have auxiliary notions which are very good, like the band model, and with this band model, despite the fact that we do not quite know why it is so good, we can explain a lot of things, It could be, if you philosophize in this direction, that the hypothesis of Regge poles, for instance, is something which you never can quite grasp and justify, but that it could play a similar role as the band model in solid state physics. This would of course mean that both the people who go slow and the people who go fast have a very good justification for their respective speeds.

BREIT

Regarding the form factors of nucleons, the general picture which they suggest fits in with reasonable simple notions in which you could have any kind of emission of a particle produce the general features of what is observed at present. It seems to me that the way in which the subject developed is somewhat irrelevant to the conclusions. It is true that at one time a resonance was useful, but at a later time three resonances or so had to be used. If you use three resonances, you might as well go over to a simpler picture unless you know that the resonances have to be brought in. I think also that for fundamental purposes a resonance is really not a valid thing to be brought into the situation because a resonance is really part of a continuum, in which the wave function changes in a certain way. This relates to fundamental problems which belong in an entirely different category of thought. On the other side of experimental indications, high angular momentum states in nucleon-nucleon scattering have been mentioned. It seems to me that this evidence is very tenuous at present. You cannot claim any accuracy for the agreement. Putting results in terms of an equivalent one-pion exchange potential, you cannot claim that its form has been obtained with any accuracy, now that the coupling constants of charged pions and neutral pions are really known to be accurately the same. From a phenomenological point of view we are pretty much in the dark and, employing calculations of Cupta's, one can explain the results about as well by theoretical means other than the dispersion theoretical calculations reported in a paper by Amati, Leader and Vitale at this conference. One might ask, to what extent do the different approaches differ? For the sake of concreteness suppose you take the two-nucleon problem. If you deal with elastic colisions below the pion production threshold, all that you need is the S matrix; that is, these days, a triviality. Suppose you consider the photo-disintegration of the deuteron; you now have two nucleons plus another particle. Consider the relationship of the two problems to each other. It is obvious that if you can deal with a wave function, the first problem is of definite use for the second, because then it has sense to speak of a wave function for the deuteron and to calculate results in the usual manner. At this point, the S matrix approach has a bit of difficulty because it has to bring in another rule for forming the S matrix when you extend the problem by bringing in another particle. The question arises: will the pure S matrix approach be able to do this process of extension, of adding more and more particles to each other, which amounts to exploring the wave function in the usual approach? Will it be possible to produce a set of rules which will not be just a purely arbitrary one ? Certainly, from a field-theoretical approach, if you believe in Feynman diagram calculations, it is simple enough. But then you also have to know that the usual field theory makes sense, and you have Professor Heisenberg's old objection that the usual formulations of field theory have the artificiality of the great many coupling constants. It is at this point, it seems to me, that one will have to make a decision, not so much on the particular techniques involved, but on what one considers to be a structure of nature, a method of thought about nature which is acceptable to us.

WILSON

I perfectly agree with Chew's remarks that the analytic properties of the elastic scattering amplitude are very impressive. But I think that we are somewhat misled because we have concentrated our attention in the last few years almost exclusively on elastic scattering, to the exclusion of multiple production for example. It seems to me that it is precisely in a problem of multiple production where field theory can come into its own, because we are originally introduced to the field in classical physics, in Maxwell equations where you are describing a process involving very many photons, all coherent. It may well be that in a process of multiple production at high energies we again have a situation where there is coherent production of many pions, and in this case the pion field can have a classical value and be of real importance in describing the process.

OPPENHEIMER

I have two brief comments. I think the first is rooted in what Jost, Gell-Mann and Mandelstam have said, but it is a formal answer to Heisenberg's question. We do not know whether the axiomatic equations either on the mass shell or taken as a whole have any solutions. It does seem they are not going to be found very easily. We do not know what the unwritten equations under axioms 1. really portend and whether they have solutions or how we would find them. But granted that either of these have content, then it seems to me not at all assured that the extension off the mass shell of the axiomatic equations, or of the equations of 1., would have the causal properties which cause the trouble in the field theories with which we have started. This, I believe, is also what follows from Ruelle's work. The second point is very minor. When Feynman first explained his diagrams, it was very disturbing to Bohr, because Bohr kept thinking of fluctuating quantized electric and magnetic fields, these little things on paper were just a method of calculation and he said that this is not physics Well, we have learnt that it is very, very much physics and a very good way to think. It seems to me that one works with the S matrix, one becomes as fond of it as of the Feynman diagrams. But there is one aspect of physics which so far has not been proved wrong, and that is the notion of how things are in space and time. I believe that when we think of the structure of nucleons it is a useful notion, and Breit referred to it, which is permitted only to those who believe that the S matrix can be extended off the mass shell.

BLOKHINTSEV

As far as I understood, Chew hopes to proceed without field theory. The S matrix theory of today is rather of a kinematical nature than of a dynamical one. Therefore we need to have a dynamical principle to find the S matrix elements. If not a field, then what ?

CHEW

There have been so many questions that I do not quite know where to begin, but I think I will concentrate on the remark of Oppenheimer made a moment ago, the remark of Blokhintsev just now, and a remark of Jost a little while ago. Let me first say that one of the striking features of the development of quantum mechanics, of course, was to try to throw away concepts which had no possible experimental verification or experimental content. It may be that this is a point which we should consider here, namely that a continuum in the momentum variables seems to be something with experimental significance. We do not know of any ultimate limit on our ability to resolve momenta. But it has been understood for a long time, ever since quantum mechanics was united with relativity, that there is no experimental way of checking up on the space-time continuum. The latter, as Professor Oppenheimer says, is a very nice concept, but we really do not have any way of checking up on it, and that of course is just another way of saying that it is possible in fact to answer all conceivable experimental questions by staying on the S matrix level. You never have to come down to axioms 2. or 3. answer any questions which the experiments may raise.

The second point then is that of Professor Blokhintsev. Is it possible to have a complete dynamical theory without crossing the line from 1. to 2. and 3.? I am sure that many of you understand that it is possible, but it is also - I think - quite probable that many of you do not, because it seems very surprising that such general notions as unitarity and analyticity could have dynamical content. The fact is that they do. As Gell-Mann said earlier, within the old-fashioned framowork in which we allowed certain particles to be elementary (there were particles which had spin less than, or equal, to one), from the corresponding poles of the S matrix the combination of unitarity and analyticity would allow you to generate exactly the same dynamical equations as you get from conventional field theory. Now, this point is no longer physically of great interest because we do not believe that these low spin particles are any different from their higher spin cousins. But it illustrates the fact, in a very concrete way, that there is dynamical content in the principles of unitarity and analyticity. The answer which I would give to Professor Blokhintsev is that there has never been a calculation which one could think of doing within the field theoretic framework which one could not also do purely within the S matrix framework. You could ask certain questions which perhaps you could not answer in the S matrix framework, but they also would not be questions that had any experimental content.

Let me now come to the remark of Jost that the situation might be so complicated as to never permit a systematic approach. I am not sure that this is the case, but I am willing to contemplate with some reluctance that it might be. And certainly the way we look at things now, it is not improbable. The essential point about the S matrix theory, at least with regard to strong interactions, is that there is no central point in the S matrix at which you can begin. It is all or nothing. This is the distinctive feature, from the physical point of view, between this and Heisenberg's approach. Heisenberg also treats all the particles on an equivalent basis, but he does have one central field at which he can start his thinking. There is nothing like that in the S matrix approach. All regions are of equal importance. There is, of course, the question about which eigenvalues lie lowest, these are the most stable particles, and then as you go up you come to the resonances, etc., so that from a practical standpoint there is a hierarchy of simplicity which will emerge, but it is no more than the usual distinction in any eigenvalue problem between the low states and the higher states. In such a situation how can any progress be made ? In just the way that it has been made and is being made. The point is that you can start anywhere in this very complicated S matrix with some experimental information. The crucial distinction between the S matrix and the field theory, from the practical standpoint, is that the basic quantities with which you deal in the first case are in certain regions experimentally measurable. The experimenter gives you some numbers and you know that these are the values of S matrix elements in a particular region. You cannot do that with field theories where no one can ever pin down any point in the whole structure.

So you begin with some experimental knowledge of a small region and then you apply the principles of analytic continuation and unitarity to try to make a prediction about what will happen in some nearby - but still distinct - experimental region, and then you see whether you have succeded, because again a measurement can be made at this point. There have, so far, as has been brought up in the discussion, been relatively few situations where clean predictions of this kind could be made. The most outstanding, as has been said, is the pion-nucleon forward dispersion relation where for example you can measure the total cross-sections and then you can predict what the forward scattering amplitude should be. But now we have hopefully come upon a second, where we find the close adjacency between the low energy region and the high energy region. They are very close together in the sense of analytic continuation, and we have some very exciting possibilities for experimental checks, which are of course receiving intense discussion at this conference. If these are borne out successfully, as I am confident they will be, the S matrix approach will of course get a very strong push. Now, your ability to calculate only allows you to go a small distance at a given time, because you always have to neglect lots of singularities that are very complicated. But you have working in your favour two principles, I suppose these could be expressed mathematically, but I would not know how. I would rather put it this way: first the Cauchy relations have the general form of a Coulomb law, the contribution of a given singularity to a particular region goes inversely with the distance of the singularity. Second, you have the unitarity condition which puts an absolute bound on the strength of any of the singularities. This is a feature which is lacking in the usual analysis of field theory, you do not have any bound on how large things can get. But the analytically continued S matrix will everywhere have these restrictions, so that you have generally working in your favour the fact that these local continuations can be made with some degree of certainty if you really can assess the nearby singularities. You know that the ones that are very far away cannot hurt you too much. This is so far the only principle which is available for making calculations in the S matrix framework. It is the idea that only the nearby singularities need to be taken into account in each small excursion. Suppose now you want to go further, you want to make a big excursion. You will go stepwise, and after each small step you can start again putting in the experimental values; this is truth, this is nature after all, and the experimenters tell you what these values are. And then you can go on and see whether things continue to work. By this sort of crab-wise approach where you make little steps constantly checking up with the experiments, one can explore a substantial portion of the S matrix. With enough success of this type it is conceivable that one might become convinced of the general principles involved, even though it would be far too complicated to ever make a complete calculation, starting from any one point.

VAN HOVE

We shall now have a few remarks by Professor Weisskopf and Professor Bogoliubov, after which the discussion will continue.

WEISSKOPF

My remarks are really very short and I would like to emphasize that I am speaking as an outsider in this field. I am looking at it from the distance. I would like to take up one point which Jost has made, namely his comparison with the situation in the theory of metals. That seems to me a very lucky example, and in this sense I would like to speak somewhat against what Chew has said. I get the impression that the new concepts that have arisen from looking at the experiments and at the theory, such as Regge poles and S matrix continuations, have definitely the character of some simplification which seems to offer itself for a good discussion not only of the experimental material, but also of the theoretical situation. There seems to be there an element of simplicity, we do not know why this element of simplicity is there, but it seems to be there and it provides a useful language to talk about what we have before us. If this is so, and I think it is certainly that, although it might be more, then of course one must say: more power to those who are busy with these things, because it is useful and it is stimulating, just as the theory of metallic bands opened up a completely new field of physics and technology. We are not yet that far, but I would like to say that there is one question which one must raise from this point of view. It is this: if it is a "band theory" kind of thing, there must be a connection with the fundamentals, because after all we know why bands exist and there is a simple explanation which we find in all books. This explanation is certainly not very good. Still, as an outsider and as an amateur who likes simple although not very good explanations, I am very pleased with it and I must say I have not yet found anything similar for Regge poles. I think it would be very good if one would see a little more of a bridge, and I think that the attitude which has been expressed in the last ten minutes by Chew is actually against this, because he says there should be no connection and this is a theory that stands on itself. Maybe it does, but before one says this, one must first really go very deeply into the other attitude and show whether it does not really follow from the field theoretical concepts, although I am of course very well aware that the field theoretical concepts are much less clear than the Schroedinger equation is, on which the band theory is built up. We all feel sort of disgusted by field theory. It is too complicated, that is the reason why I am disgusted, but others are disgusted because there are infinities and contradictions in it. Still it is a basis on which we have lived for a long time and which has a deep value in itself, in fact it is quantum mechanics after all, it came from quantum mechanics.

Now I must say, there is of course a possibility that Chew is right and that this simple concept which one vaguely sees in its contours if one looks at the experiments is really the nucleus of something new. Probably people before 1924 had sometimes feelings like this when they saw quantum orbits emerge, and it might be that there is really a new theory behind this. But, to my mind, one must be extremely careful before one jumps into it, because a new theory is a very great thing, and, although we hope we will get it sometimes, I still would think that one has first to investigate the concepts very thoroughly and see how they connect with concepts which we had before.

BOGOLIUBOV

I shall make a few remarks. First, concerning axioms 1. of Professor Heisenberg, it seems to me that it would be better to put causality first, and analyticity second. For instance, when you consider the problem of analyticity for vertex parts with many ends, for multiple production, analytical properties have not yet even been postulated. You may say also that causality conditions are formulated only out of the mass shell, they really need some space-time relations. I may draw your attention to the fact that maybe there is some deep physical reason for this because for instance you formulate the causality condition mathematically by introducing classical fields. Physically the same situation will arise when you have weak interactions. Suppose you have a physicist establishing the theory of strong interactions only on the mass shell and ask him: calculate a process where weak interactions are involved. He immediately draws the vertex part taken from strong interactions, only this vertex part will have a virtual end where the weak interaction is attached. So he needs virtual vertex parts with virtual ends in order to answer the physical question of how the strongly interacting particles will behave under weak interactions. It will be possible to construct a theory limited to the mass shell only when you have a unified theory containing strong, electromagnetic and weak interactions, but I think that nowadays such a theory is "nur in Gedanken möglich"!!!

Now I ask a rethorical question: which approach to the problem has been successful? I may say that the greatest success in field theory has been booked by ordinary perturbation theory with Feynman diagram techniques, but by means of the right summations, that is really by means of the right way of extracting information out of perturbation theory ! By the right way, I mean of course the way which is crowned by success !!! For instance, in ordinary dispersion relations, a very vast region was established by this perturbation treatment. The Mandelstam relations are just established by considering some very important class of graphs. But you may say that Regge poles cannot yet be inserted in this scheme. Still I shall briefly mention the work of Logunov, Tavkhelidze and collaborators in Dubna, who study the asymptotics of the four particle vertex part on the mass shell for large s by applying the renormalization group to the t variable. Considering some model examples with small interaction, they obtained a Regge formula with exponents which exhibit the right analytical structure. This work is in progress. It seems to me that even in the theory of Regge poles, it is very interesting to go to the calculational procedures and ask the theory of perturbation - or diagram technique - how it must be modified, how it must be summed in order to obtain the Regge pole structure. In addition, it will have some practical applications, e.g., for electrodynamics. I personally like more the axiomatic approach, but to be objective I must say that most of the results have come out of the diagram technique, including the modern approach leading to a synthesis of Regge poles with field theory.

HEISENBERG

I want to ask whether Professor Bogoliubov could not say something on the question of the degeneracy of the vacuum because he has himself worked so much about it.

BOGOLIUBOV

The concept of vacuum degeneracy is a very beautiful concept. But how to materialize it technically? There resides the difficulty. One can construct many models where the divergences are not so strong. For instance, Dr. Tavkhelidze has considered with success such models, and then you can see with your eyes the existence of the degeneracy of the vacuum. But for the general situation in field theory, in order to discuss the properties of the vacuum, you must introduce a cut off, as Nambu did, in order to get rid of the divergences.

HEISENBERG

May I just say one word. I do not see that one should connect this problem with the problem of the divergences. The problem of the divergences has to do with those very fundamental problems which we have discussed all this morning, but I think that the problem of degeneracy of the vacuum is entirely separate. If one wants to work on the degeneracy of the vacuum one should of course either take some indefinite metric to avoid the divergences, as we do, or take some classical example like ferromagnetism or superconductivity, which also is without divergences But I think that one should really try not to mix two problems which have nothing whatsoever to do with each other.

BOGOLIUBOV

I agree that these problems are quite different. But when you wish to materialize the idea of degeneracy, you consider for example a field theory with Pauli-Villars regulators or with relativistic cut off, and you introduce in such a way an indefinite metric. Suppose now you have a situation in macroscopical physics, like the theory of superconductivity. You have a normal state and you obtain a superconductive state which is lower, has a lower energy, and this is good. But when you have an indefinite metric mixed in and you obtain an energy less than normal, is it good or is it bad ? You do not know because this indefinite metric may be so manipulated that you get the energy to $- \infty$. I completely agree with you that this is quite another problem, but all the difficulty is how to consider this problem. Maybe one must consider it non-relativistically, to take a non-relativistic approach and have a regularization which is not Lorentz invariant. A non-Lorentz invariant regulatization is subject to the indefinite metric criticism and when you obtain there some results, you have a better realization of the idea. It is true that these two problems are entirely different, but the divergences in the existing theory make it very difficult to develop it mathematically.

LOVELACE

This is a comment on Chew's remarks. We left out one of the best predictions when he made his list. That is, the Regge pole theory predicts that the diffraction peak should get sharper at high energies. This is such an extraordinary prediction that, at first, some people used it as an argument against Regge poles. Nevertheless, we had a lot of new experimental data at this conference, and the experimentalists themselves have authorized me to say that this prediction has now been quite conclusively confirmed.

MANDELSTAM

I wonder if I could ask Professor Bogoliubov to amplify his remarks on Logunov's work about getting the Regge poles from renormalization groups. Have they actually got some rigorous consequences of field theory, or do they just say it is consistent ?

BOGOLIUBOV

They are not rigorous, because the renormalization group is only a means of summing diagrams. If you do not like renormalization groups, you may obtain the same results by summing a class of diagrams. But this seems to me to be the beginning of a very important trend to calculate the Regge poles, maybe by entirely inexact methods, but leading to some intuitive understanding, because their results exhibit in their approximation all the properties of Regge poles, analyticity, etc.

At this stage, OPPENHEIMER and FUBINI drew attention to the fieldtheoretical investigations of Regge poles by B. Lee and Sawyer in Princeton and by Amati, Fubini, and Stanghellini at CERN. Also GELL-MANN raised questions on Regge poles in electrodynamics.

FUBINI

Since the discussion has come once more to Regge poles, I want to call attention to a point made by Goldberger, Blankenbecler and Cook which shows some of the paradoxical aspects of the new theory. If, for example, one uses for the vacuum trajectory the one determined at CERN from proton-proton scattering, then one obtains a very striking result. If the photon is still the old-fashioned particle to which we are accustomed, then in elastic scattering at infinite energy the electromagnetic effect will dominate on the strong interaction effect everywhere, with the exception of the forward direction. So, if you construct let say a million GeV machine, then you might just learn that $e^2 = 1/137$, that is all. Now this is very paradoxical and there are many ways out. The first way out is that the Regge trajectory behaviour is just the first approximation to something more complicated. A second possibility is that electrodynamics has also to be reggeized, but then it has to be very strongly reggeized, because it must have a slope of $\alpha(t)$ steeper than the one determined at CERN, and this is very unpleasant because electromagnetism is a theory which works pretty well and it is very unpleasant to change it. So at least I think this new behaviour of Regge poles is really bringing our theories to a stress.

VAN HOVE

I would like to raise a final point for the 20 minutes that we have left, and this actually comes back in a way to a remark made by Wilson. When Professor Heisenberg wrote down his list of axioms, point 1. was existence of the S matrix, point 2. was existence of a local field $\chi(x)$ commuting for space-like distances. I then got the impression that he was writing a list which I could compare with the following list of the ethical rules: rule 1: be good, rule 2: when you are in your car drive on the right-hand side of the road. What I mean by that is that 1. is extremely closely and unavoidably connected with the very basic principles of quantum and relativistic physics, whereas 2. is very technical and very special, it looks even quite arbitrary taken on its face value. Now the historical origin of this is of course quite clear and I think it is the point which was mentioned by Wilson, namely that in the whole growth of physics the concept of field has played such an absolutely predominant role in electromagnetism, and then has kept that predominant role in the quantum version of electrodynamics. There we have a very specific field theory using specific field operators, with their commutation rules describing local commutativity or local causality. I think the reason why we still worry so much about the use or the rejection of this concept is the fact that electrodynamics has worked so well. If electrodynamics had turned out to be no good in comparison with experiments on the sub-atomic scale, we would not be worried so much about the concept of field and might have abandoned it quite a while ago.

JOST

I would like to point out a general property of all the field theoretical axioms. I take now the Wightman axioms which do not fit completely in what you are saying, but I think it is all right. Namely, these axioms taken together form, that is our experience, an extremely rigid framework. If you cross out any one of them, you get a completely amorphous system which allows certainly much too many possibilities. For instance, if you cross out locality, then the consequence is simply that every S matrix can be interpolated with the LSZ procedure. So I think that there is an additional reason to keep to axiom 2., namely that if you give it up completely you just get nothing interesting. In other words, I have the very strong feeling that we have to face, maybe for some time to come, the possible contradiction of the axioms with the S matrix being different from 1, because in all of these axioms there are certainly elements of truth. If you reject one axiom, you are bound to lose everything one has understood until now. This is a very disagreable situation and I understand very well that many people think one should change this situation by a vigorous revolution, but here in Europe one does not believe so strongly any more in revolutions !!!

OPPENHEIMER

I could not agree more. Chew said this morning we should be prepared to give up space and time, and this is very much what one must mean of course by giving up causality. But think back, nobody wishes to give up determinism, no one wishes to give up a deterministic mechanics. We should not be too ready to give something up without knowing what it is. It is hard to make the principle of correspondence to go from an S matrix through quantum theory to the macroscopic world in which we live, because there is no way to formulate it till we know where there is something wrong. I do not think we are likely to find it out, except by keeping the elements that we have until we see where they go wrong.

VAN HOVE

In quantum electrodynamics, good as it is, there is the whole question of the necessity of renormalization and we have now the new point that in a non-renormalizable theory, where divergences have been believed for a long time to be unavoidably present in observable quantities, Yang and Lee, by the use of a complicated limiting procedure, have found that the quadrupole moment of the vector boson can be given a finite value non-analytic in alpha. I would like to ask Yang to comment one moment on the significance of this result. Because of time limitation this will be the last contribution to our discussion.

YANG

Consistent with the tone of discussion of this morning, let me first make a philosophical remark. We have found that such works like religion and psychology have crept into our discussion, I think it is indicative of something that characterizes our present style and trend, an I feel very strongly that this is a collective responsibility of all of us. Now as to our work, let me say that both Dr. Lee and myself admit freely to thinking mostly in terms of Feynman diagrams. It is upon such a way of thinking that we approached our problem and I will just describe to you what was the essential point. We know that a vector meson cannot be renormalized in the usual sense because its propagator does not have the desired characteristics at small distances or large momenta. So we introduced a scalar meson field in a way which I am sure has been done many times before, and which serves as a regulator, and then we asked whether such a formalism could be renormalized in the usual sense. It still could not. Then we found that if you introduce negative probabilities for the scalar mesons, the theory indeed becomes renormalizable. Now, the square of the mass of the scalar meson introduced is m^2/ξ, being the mass of the vector meson and ξ a positive parameter. If the parameter goes to zero you push the scalar meson mass to infinity and, while the whole S matrix now is not unitary, that part of it which does not contain the scalar meson becomes unitary in the region where ξ goes to zero. On the other hand, as long as ξ is finite, the theory is renormalizable. That means that you have a power series in α, each coefficient of which for any process would be a function of ξ, but of course if you make ξ go to zero the theory goes back to the original one and it diverges term by term, that divergence being manifested by the fact that the series is a power series in α/ξ^2. ξ entering the theory in the combination m^2/ξ this merely states the fact that the divergence is a quartic one as you go to higher and higher orders of α. When one meets with a situation like this, with a power series of the type $\Sigma c_n (\alpha(\xi^2)^n$ m, one asks whether this theory can make any sense at all. We have only tried to make the best of it by saying that if the theory does make sense, then perhaps, although term by term the series is divergent, the sum is convergent. This is a statement which is a little strange but we have seen previous cases in which it worked very well. In the many-body problem, if you take a large collection of small hard spheres, a dilute gas of hard spheres of radius a, you can calculate for example the ground state energy. You find a power series in a and the coefficients of all the terms are divergent. However, because in this case all coefficients are computable, you can explicitly demonstrate that they add up to give a convergent result in $\sqrt{a^3}$, i.e., a fractional power of a. In the vector meson case we speculated that perhaps if one knew how to calculate the coefficients one would find a similar behaviour. Fortunately, for practical results, one did not have to be able to do that. This arises because of the fact that in the very low order terms there are logarithmic dependences on ξ coming in, and, as you well know, when that happens it is often easy to extract the logarithmically divergent term without being able to calculate the series to infinity, although you still have to make the assumption that the sum converges to a finite number. If one carries out this idea, one in fact is able to obtain the corrections to various processes involving an intermediate vector boson, in particular for example one gets the quadrupole moment of the vector boson, which contains a α log α term. This has also been carried out for the radiative correction to the muon decay, the intermediate boson decay and the decay of a non-strongly coupled neutron. All this exists in the form of two preprints, so I do not think we need to go further into details.