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BOOTSTRAP PHYSICS: A CONVERSATION WITH GEOFFREY CHEW

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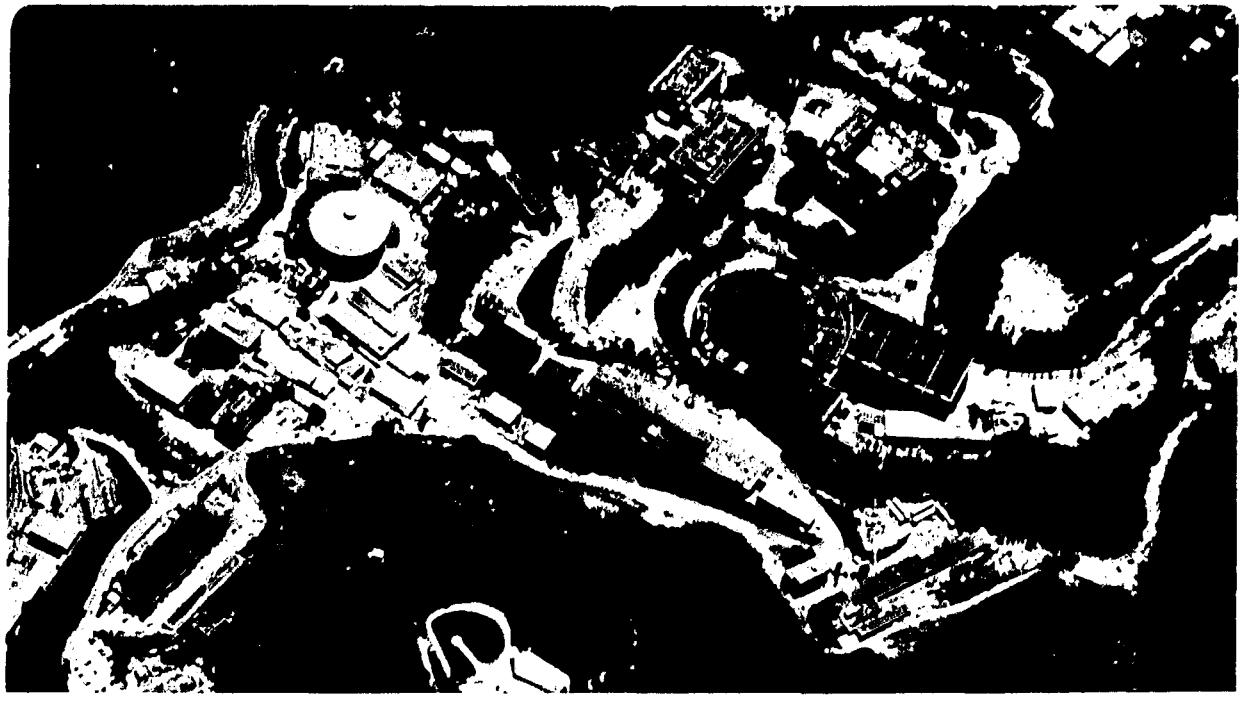

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BOOTSTRAP PHYSICS: A CONVERSATION WITH GEOFFREY CHEW*

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ABSTRACT

The history, present status, future potential, and philosophical implications of the bootstrap approach to particle physics are discussed in a wide-ranging conversation with its originator and main advocate, Geoffrey Chew.

This paper is dedicated to Geoffrey Chew in celebration of his sixtieth birthday.

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I. INTRODUCTION

For Geoffrey Chew, the year 1984 is a double jubilee. It is the year of his sixtieth birthday and, at the same time, the twenty-fifth anniversary of his celebrated bootstrap hypothesis. It seemed therefore appropriate to review the history, present status, future potential, and philosophical implications of the bootstrap approach to particle physics. The present paper is the transcript of a wide-ranging conversation about these topics between Chew and the author, which took place in July 1983. The text of the transcript has been edited only minimally in order to preserve the spontaneity of the conversation, and it has been organized as follows.

II. THE BOOTSTRAP IDEA

CAPRA: Geoff, I would like to begin by asking you: What is the bootstrap? What does it say?

CHEW: The idea tends to evolve in time, and the way I describe it now will probably be different from how I would have described it five or six years ago, or from how I would describe it five or six years in the future. The key notion certainly is consistency; the idea that the laws of nature are controlled by consistency and are not arbitrary. From a bootstrap standpoint, one is not content to see any arbitrary aspect in a theory.

CAPRA: Arbitrary meaning what? Do you mean by that, for example, that the mass of the proton has its reasons?

CHEW: Yes, that is certainly the sense in which I mean it. However, when you actually apply the bootstrap idea, you always accept certain ideas, you have to, and within the context of these accepted ideas you then try to remove arbitrariness as much as possible. For example, at the beginning we simply accepted the idea of particles, the idea of an S matrix, which brings with it the ideas of energy-momentum, superposition, and also the Poincaré group and ideas of analyticity. All these ideas were accepted as the basis of our thinking about consistency.

CAPRA: They were part of your scientific framework, part of your language.

CHEW: Yes, and within that framework an effort was made to understand the properties of the hadrons, just the strongly interacting

particles. That was what we operationally called the bootstrap. But then, as time went on, we became more demanding, and we asked: could we also understand the electroweak particles? And this evolved to the feeling that one needs also to understand the origin of space-time, the Poincaré group, probably; ultimately also superposition, the presence of complex numbers, analyticity, all these things.

CAPRA: The presence of complex numbers?

CHEW: Yes, why complex numbers are appropriate to understanding physics. There are lots of other formalisms you might think of. No matter how far you go, because of human limitations you will always have to accept, at any stage, a certain set of ideas.

CAPRA: But apart from those ideas, which are provisionally accepted as fundamental, you don't accept anything arbitrary in your theory. You want to derive everything from overall self-consistency.

CHEW: Yes, that's the idea.

CAPRA: Now, what would you say about the nature of the bootstrap idea? Is it a scientific hypothesis, which has now, maybe, turned into a theory? Is it a philosophy? How would you characterize it?

CHEW: Well it is certainly a philosophy, and I think operationally it has turned into a scientific program. I suppose this scientific program has now enough substance to call it a theory. It is very hard to say when you make these transitions from one category to another.

CAPRA: It is also something like a principle, something like Occam's razor, for example.

CHEW: Yes, or like Mach's principle.

CAPRA: However, there seems to be one problem with this notion of self-consistency. One could say that it is a fundamental principle of an approach which does not accept fundamental principles.

CHEW: That's right. That's the ultimate paradox.

CAPRA: Does that bother you?

CHEW: Well, it disturbs me vaguely, but I don't expect to get to the bottom of the whole thing in my lifetime anyway . . . All is relative; it's a matter of making a certain amount of progress.

CAPRA: I have sometimes worried about this problem, and I have thought that one could maybe put this principle of self-consistency together with the scientific framework, with the scientific language. It is certainly an important aspect of science that you don't want to be inconsistent, which is again related to the way human beings think and to the way we observe.

CHEW: Yes, that would be a way to talk about it.

CAPRA: The notion of self-consistency brings to mind the celebrated paradoxes of quantum mechanics. How do you see the role of these paradoxes?

CHEW: I think that this is one of the most puzzling aspects of physics, and I can only state my own point of view, which I don't think is shared by anybody else. My feeling is that the principles of quantum mechanics, as they are stated, are not satisfactory and that the pursuit of the bootstrap program is going to lead to a different statement. I think that the form of this statement will include such things as: you should not try to express the principles of quantum mechanics in an a priori accepted space-time. That is

the flaw in the present situation. Quantum mechanics has something intrinsically discrete about it, whereas the idea of space-time is continuous. I believe that if you try to state the principles of quantum mechanics after having accepted space-time as an absolute truth, then you will get into difficulties. My feeling is that the bootstrap approach is going to eventually give us simultaneous explanations for space-time, quantum mechanics, and the meaning of reality. All these will come together, somehow, but you will not be able to begin with space-time as a clear, unambiguous basis and then put these other ideas on top of it.

CAPRA: By the way, I know that some people are confused when they hear you use the term "reality". Whenever you say "reality," you mean Cartesian reality, right?

CHEW: Oh yes, I'm sorry, I should always use "objective reality," or "Cartesian reality".

CAPRA: You don't mean to say that the quantum reality, or the reality of emotions, or of the spiritual realm are any less real?

CHEW: No, no, no! I am just being careless. I mean objective reality, the explicate order, as David Bohm calls it.

CAPRA: Coming back to quantum mechanics, you are saying, in fact, that these paradoxes exist, as Bohr and Heisenberg already emphasized, because we are talking about atomic phenomena in a language which is inappropriate. They were referring to the Cartesian language of classical physics, and what you seem to be saying is that space-time is still a remnant of the classical way of thinking.

CHEW: Absolutely. I don't think the meaning of space-time has ever been separate from classical notions. Somehow, we are trying to

grasp the connection between the real world and the quantum principles, and we have to understand that space-time is part of the real world and not something that pre-exists before quantum principles are stated.

CAPRA: I would now like to discuss with you the general significance of the bootstrap idea. I have recently been more and more impressed by the idea that the major shift and the deepest change in our thinking may be the shift from an architectural metaphor of a building, with firm foundations upon which one builds, to the metaphor of a network, which has no foundations but represents a web of interrelated events and, correspondingly, consists of a web of concepts to describe these events. That seems to be the major shift. The notion of a basis, of fundamental concepts, building on strong foundations, etc. -- all that runs through Western science and philosophy. Descartes wrote that the knowledge of his time was built on sand and mud and that he was going to build new firm foundations for a new science; and three hundred years later Einstein wrote that the foundations of classical physics, that is of this very edifice of Descartes, were shifting and that he did not see any firm ground upon which he could build a theory. I think, maybe, since the bootstrap it is now the first time in Western science that we are not looking for firm ground and solid foundations any more.

CHEW: I think that is true, and it is also true that because of the long tradition of Western science the bootstrap approach has not become reputable yet among scientists. It is not recognized as science precisely because of its lack of a firm foundation. The whole idea of science is, in a sense, in conflict with the bootstrap approach,

because science wants questions which are clearly stated and which can have unambiguous experimental verification. Part of the bootstrap scheme, however, is that no concepts are regarded as absolute and you are always expecting to find weaknesses in your old concepts. The bridge, however, between standard science and the bootstrap approach lies in the commonly shared awareness of the approximate nature of all experiments. Even people who are dedicated traditional scientists recognize that no measurement can be completely precise.

CAPRA: But these are two things, the approximate nature of measurement and the approximate nature of concepts.

CHEW: Right, and both are recognized.

CAPRA: By the way, do you have any idea when the appreciation of the approximate nature of scientific theories emerged in the history of science?

CHEW: I don't know for sure, but I suspect that it came along with quantum mechanics. I suspect that in the 19th century people might well have believed that theories like Newton's could have absolute validity.

CAPRA: Anyway, now the approximate nature of sicence is generally accepted.

CHEW: Yes, but in spite of that the traditional point of view in science is that at any given stage of acitvity there is supposed to be a consensus about certain principles whose validity has not yet been disproved or even challenged. All scientists are supposed to conduct their activities within this framework of accepted principles until some measurement comes along which is accurate enough to show

that some principle has to be abandoned. The bootstrap approach recognizes from the start that the principles used are not going to be absolute, that everything is approximate. Nevertheless, it is incumbent upon a bootstrap theorist to get an understanding of the degree of approximation.

CAPRA: But now you have said more or less the same thing about bootstrap science and orthodox science.

CHEW: Well, that's why it is possible for them to coexist. Psychologically, however, there is a difference which causes great misunderstanding. Let me give you an example. At the present time, the overwhelming majority of the theorists working in high-energy physics accept an absolute notion of local fields. They do this because it is to them the only known way of combining the quantum principles with the space-time continuum. They accept the space-time continuum as an absolute and accept quantum superposition as an absolute, and they only know one way to put these two things together, which is through the local quantum field, and so they take for granted that whatever the description of natural phenomena is going to be, it will be done through local fields. Now, if you get them in a philosophical discussion such as this one, and if you push them, the more talented ones will agree that probably local quantum fields do not represent absolute truth. But they would say: "So far that has not been shown."

CAPRA: So they would think it <u>might</u> be the absolute truth?

CHEW: I suspect that if you took <u>the</u> most talented ones -- people like t'Hooft, Gell-Mann, Weinberg, or Salam -- when they are in a philosophical mood they would probably agree that local fields are not

the ultimate truth. But they are guessing that within their own lifetime nobody is going to go beyond the capacity of the local field to describe high-energy phenomena. Somehow or other I have come to the belief that it is not too soon to go beyond local fields. What that means is that in trying to develop a theory I don't start with a local quantum field. I start with other ideas, and all these people find this incomprehensible. They say: "Why don't you use local fields? They have never shown to be wrong." Now, the reason why I don't like them is because they bring in an inherent arbitrariness. Nobody has ever found a way to use local quantum fields without introducing an unpleasant arbitrariness.

III. HISTORY OF THE BOOTSTRAP

CAPRA: Geoff, I would now like to turn to the history of the bootstrap idea. A little while ago you said, "Somehow or other I came to the belief . . . " Since the shift from orthodox physics to bootstrap physics is so radical, I am extremely curious to know how you developed these ideas and to what extent you appreciated their radical nature.

A. Fermi's Influence

CHEW: I made an attempt not long ago to reconstruct some of these developments, and I believe that the beginning came right at the time of my Ph.D. thesis with Fermi. Now there is an irony here, because Fermi was an extreme pragmatist who was not really interested in philosophy at all. He simply wanted to know the rules that would allow him to predict the results of experiments. I remember him talking about quantum mechanics and laughing scornfully at people who spent their time worrying about the interpretation of the theory, because he knew how to use those equations to make predictions. But Fermi suggested as a thesis problem for me an extension of an approximation which he had discovered in connection with the scattering of slow neutrons by molecular systems. He had realized that the molecular binding was important in this process but that nevertheless the interaction of the neutron with the nuclei was overwhelmingly strong compared to its interaction with the rest of the system. While the neutron was interacting with the nucleus you could ignore the molecular forces. It was a very subtle thing which, eventually,

became called the impulse approximation. Fermi's idea was that the nuclei behave in response to the atomic forces until the neutron arrives; then, when the neutron is in contact with a particular nucleus, the nucleus forgets that it has any other things around it, until the neutron departs when, once again, it responds to its environment. Now, all of this is done quantum-mechanically, so it's not trivial. But it led Fermi to a certain set of formulas, a recipe of how to know the molecular wave functions, and then all you had to know in addition was the scattering amplitude of the neutron by the nucleus, as if the nucleus were free. And then you could put these two things together to do your computations.

CAPRA: So that was what Fermi had done.

CHEW: Fermi had done that and he suggested that I extend the same idea to scattering of neutrons by nuclei, where you think of the nucleus as being made up of neutrons and protons. The point of the idea was that if the neutron was moving very fast, there might again be something like a neutron interacting with a single nucleon. What Fermi had done here was really to make a practical application of an S-matrix idea. He did not recognize that but he intuitively understood that there was a complex number which characterizes the scattering of the neutron, which you can measure, and you can use that number in computations. You don't have to say that there is a potential between the neturon and the nucleus; you don't have to go through the apparatus of the Schrödinger equation. All you have to know is that one number, which is an S-matrix element. So that idea got into my head.

CAPRA: And you worked it out?

CHEW: I worked it out for the case of scattering of neutrons by deuterons and various other things.

CAPRA: And it worked?

CHEW: It worked, and it also persuaded me that it was not necessary to have a Schrödinger equation and a potential. Previously, people had always thought that when you computed something you had to have a detailed microscopic interaction between the particles together with a Schrödinger equation.

CAPRA: In other words, you had to have a temporal sequence for the wave function.

CHEW: That's right. Fermi simply produced formulas. You saw no time; you saw no Schrödinger equation. He simply worked directly with amplitudes.

CAPRA: And he did this because he was a pragmatist.

CHEW: That's right. He somehow knew intuitively what he had to do. Now, he did not describe it that way. He described it in ways that very much obscured the S-matrix interpretation. But nevertheless I began to think that a large part of what we normally associate with the Schrödinger equation is simply done by S-matrix principles, and you don't need all this microscopic space-time.

CAPRA: But these S-matrix principles were not formulated at that time.

CHEW: No.

CAPRA: Was the S matrix itself known?

CHEW: Yes. John Wheeler had identified it, I think, in 1939. Heisenberg had written papers about certain of its properties in the mid-

forties, and he had actually called it the S matrix. Then Christian Møller wrote some review papers which propagated Heisenberg's thinking.

CAPRA: Were you familiar with these papers at that time?

CHEW: Well, that's a very funny thing. I had Fermi's idea, and I knew about the S matrix abstractly, but I did not connect the two; not for a long time. It's very strange. I found the S-matrix theory at that time kind of forbidding. It used an apparatus that was difficult, and I simply did not connect it to those other ideas. But I did become aware of the S matrix while I was a graduate student.

After I left Chicago, I continued to work on this impulse approximation for a couple of years, but it was done within sort of a bastard framework. It wasn't S-matrix theory, it was something in between. I was picking up Fermi's intuition and trying to generalize it, and I produced a series of papers in which Murph Goldberger and Giancarlo Wick were also involved. Then I went to the University of Illinois in 1950 and started thinking about pi mesons which had been discovered not long before that. For some reason -- I wish I could recall that precisely -- I was completely persuaded that the idea of local fields was inappropriate for describing pi mesons. Up until then, people had been dominated by the idea of Yukawa, which was that pi mesons were the analogue of photons. Yukawa had said that the electromagnetic force, which is due to the exchange of photons, was the analogue to the nuclear force due to the exchange of pi mesons. So people were writing down equations just like electromagnetic equations, except that they would have fields associated with the pi mesons.

Now I had been in contact with the early experiments on pi mesons, and it was clear to me that these were particles just like

any other nuclear particle, like neutrons or protons, and it seemed silly to me to use fields to describe them. The kinds of experiments you were trying to describe were just like any other nuclear reaction. You didn't use fields in connection with nuclear physics before that; why should you use fields for the pi mesons when they were just another kind of nuclear particle? But people said, pi mesons are not nuclear particles; they are field quanta; they are like photons. It is very strange when you look back now to understand that psychology. So in 1950, when I went to the University of Illinois, I decided to try to make a little model to describe scattering of pi mesons by protons, based on the same idea that Fermi had. I said to myself, suppose the proton is some kind of a structure that contains pi mesons within it and then we shoot pi mesons at it from the outside . . . Although I did not know that the word S matrix was relevant at that time, it was a model in the spirit of S-matrix theory. It was a model in which you did not use the Schrödinger equation; you just used the superposition of amplitudes. Looking back now I can see that it had much of the Feynman idea that you can build amplitudes by superposition.

B. Collaboration with Low, Analyticity and Pole-Particle Correspondence

CHEW: The model had a certain amount of success, and then Francis Low came to the University of Illinois, and after a year or so we started to work together. He had made a certain discovery in axiomatic field theory and for some reason either he, or I, or both of us, recognized that his discovery might be relevant to this model that I had developed. So we started to work together on it, and I was so pleased

to have somebody of Low's talent to work with that I put aside my feelings about the nature of my model and tried to re-express the content of it in field-theory language. It turned out that, to a large extent, this was possible. Then we wrote a paper together, which many many more people could understand. Not so many people could understand the thing I had written first, but when it was re-expressed in the language of field theory it could be appreciated by many more people. The mathematical structure that came along with it was, in fact, much improved, so that we could see a lot more things.

Now Francis understood that the additional content was, in fact, of a general nature associated with analyticity. It was at that point that I began to be aware of analyticity as a principle. Francis and I had, somehow, come upon the notion that analytic continuation is very powerful. We still did not think of it as S matrix; we thought of it as analyticity suggested by field theory. But, in fact, what we did was to sort of forget the field theory at a certain point and start working with analytic functions. Most of the content of what we did was just based on analytic functions. We started to recognize the complex plane explicitly at that point.

CAPRA: Did the S-matrix framework, as it existed at that time, have analyticity in it, or was this your discovery?

CHEW: I am not quite sure what the honest answer to that is, but I'll tell you what Landau said to me. He was a very dramatic person, very outspoken with no hesitation to express his views about anything. In 1959, at a meeting in Kiev, he expressed annoyance to me about the work that I was doing with Mandelstam on the pi-pi dynamics. He said we wasted our time with approximations, dealing with a system that was

incomplete. He was partly right, but through our effort we discovered general things which we would not have discovered had we not made that effort. In any case, in the course of criticizing me for putting so much effort into this pi-pi dynamics, Landau said: "You know, you have discovered an absolutely crucial point, which is that particles correspond to poles, that the S matrix is an analytic function, and that the poles of the S matrix are the particles." He attributed that discovery to me. I didn't think of that as my discovery, but when I look back and ask myself, who was it who first really appreciated that particles correspond to poles, maybe it was me; I am not sure. It was an idea that was floating; it occurred in various special forms here and there, but somehow the generality was not recognized. For example, a few people, such as Wigner, had come to the idea that the notion of an unstable atomic state could be associated with a complex pole in something or other, and that the imaginary part of the pole location was associated with its lifetime.

CAPRA: What that the Breit-Wigner resonance?

CHEW: The Breit-Wigner resonance formula was an example. It's hard to tell, how general Breit and Wigner thought these concepts were. At that time, such ideas were always presented as if they could be derived from perturbation theory, but the smartest people knew very well that they had to be general; they couldn't rest on perturbation theory.

CAPRA: Now, what about the analyticity of the S matrix?

CHEW: Well, the S matrix itself was not a well-recognized notion.

CAPRA: It seems that what you contributed, then, was the emphasis on analyticity which put the whole notion of a pole in a different context. Without even mentioning the S matrix, it was nevertheless

a step in that direction.

CHEW: That's right; that's true. I certainly contributed something, but it's hard to say exactly what it was. I remember being puzzled at the time that there weren't lots of people recognizing these points, and I felt there must be something the matter with me, because it seemed so evident to me that we were dealing with an analytic function which has poles. But nobody else . . . and then Landau! That was a tremendous thing. Here I go to Russia, and here comes Landau and congratulates me for exactly recognizing this. Then there was one other place that I know of where I was given credit. This was in a paper by a Berkeley mathematician who was studying the abstract mathematical problem of how to extrapolate a function which you only know incompletely. He was focusing on the use of the idea that the function had poles with known locations, and he attributed the use of this information for the extrapolation of an incompletely known function to me. In presenting the history of this problem, he referred to a paper of mine, which might be the first paper in which a definite statement about the association of particles with poles is made. This was a paper on the problem of deducing the pion-nucleon coupling constant from nucleon-nucleon scattering data, which I had written in 1958.[1]

Just as Francis Low and I were doing that work, Gell-Mann and Goldberger had started to develop their dispersion relations, which had a big influence on me. They believed the relationships that they employed were all based on field theory, but I remember I was quite convinced that it was analyticity that counted and that field theory was not really necessary. I still did not make the connection with

the term S matrix. It's very strange; that was already in 1955-56.

C. Collaboration with Mandelstam; Origin of Bootstrap

I left Illinois in 1957 and came here to Berkeley, and then I met Stanley Mandelstam, who had discovered double dispersion relations and had thereby solved a problem I had been struggling with for a long time. I had become aware of the fact that analytic continuations in energy needed to be extended to angle, that you had to continue both in energy and angle. I could not figure out quite how to do it, and Mandelstam did.

CAPRA: He developed that whole framework of the s and t variables, didn't he?

CHEW: Yes, that's right. Well, I got Mandelstam to come to Berkeley and we worked together. He was at Columbia and had gotten his Ph.D. at Birmingham with Peierls. At Columbia nobody knew what he was doing; nobody paid any attention to him. I heard him give a talk at the Washington meeting of the American Physical Society, and I remember that I said to myself when I heard his talk: Oh, this young guy, he doesn't know how hard his problem is. He thinks he solved it, but I'm sure he hasn't solved it because he doesn't know this difficulty and that difficulty, and so on. And I thought, in kindness to him I'll point out some of these difficulties after his talk. But when I started to ask him questions he just answered every question. I was totally overwhelmed; he had really solved the problem! So I persuaded him to come out to Berkeley, which he happily did, and we collaborated on two papers extending the whole idea to pion-pion scattering. Up until then it had always been pion-nucleon scattering,

but now that Mandelstam had extended the analytic continuation it was possible to think about pion-pion scattering.

We did not get a satisfactory theory, of course, because the pion is not the end of the story, although at that time we thought it was. It is funny to look back at this now. We thought, somehow, that the pi meson was the key, and if you could understand how pions interacted with pions, you really got it. But we discovered that something was loose; the system did not close.

CAPRA: In all this there was no S matrix yet, and of course no bootstrap?

CHEW: That's right, but this was where the idea came into my head, and in 1959 the word "bootstrap" appeared in print for the first time, although rather casually. Mandelstam and I had pushed our pi-pi analysis to the point where we could see there might be a solution of the following character. The pions would interact to produce either a bound state or a pseudo-bound state, and that bound state by crossing would then constitute a force which would be the agent for making a bound state in the first place. We could see this possibility quite clearly in the way the coefficients of the equations arose.

CAPRA: Had the crossing property been identified at that time?

CHEW: Yes, crossing had been discovered by Gell-Mann and Goldberger, but it had not been used in the sense of dynamics to make a theory of forces. It was understood that ingoing particles became outgoing antiparticles by crossing, but the crossing property had not been applied to talk about forces in the cross channel. Somehow it needed

Mandelstam's representation to do that. Well, Mandelstam and I figured those things out, and in 1959 there was that conference in Kiev at which I was a rapporteur. When I reported on our work, together with the work of others, I used the term "bootstrap" for the first time in the text of this report, referring to that possibility that we had noticed. Now, you have to realize that the rho meson had not been discovered at that time; Mandelstam and I thought of a bound state of two pions being simultaneously, through its exchange, the force that holds the pions together. A corresponding particle was not yet experimentally known. So all this was very tentative. Nevertheless, we had noticed this possibility and we described this as a kind of bootstrap dynamics.

CAPRA: "We" meaning Mandelstam and you?

CHEW: I said it. I don't think Mandelstam would have used the term, but it certainly came out of our joint work. I think in discussions with Mandelstam I had used the term "bootstrap". Stanley never endorsed it but, being a very mild person, he did not fight it, and the term also appeared in one of our papers.[2] What then happened was that a number of other people, in particular Zachariasen and Zemach, and some others, used the term once they had grasped the idea. Going along with it was an approximation called the N/D approximation, in which you use S-matrix principles to do computations within the framework of scattering amplitudes, which you analytically continue without using the Schrödinger equation. If you think of Mandelstram's subsequent interests, he never picked up the bootstrap idea. He allowed me to use it in one of our joint papers, but he never felt comfortable with it. Stanley is a beautiful example of the kind

of physicist I was talking about earlier. He feels a need for something fundamental. I think he always believed that he had firm ground under his feet.

CAPRA: Even though you could say that his double dispersion relations were really the first tool that pulled out the firm ground from under you.

CHEW: That's right, that's right; that's exactly right!

CAPRA: If you disperse in one channel, and then you turn everything around and disperse in the other channel, that is very much connected with that whole network idea that was later to emerge.

CHEW: That's right, but Stanley thought that it was based on field theory. By the way, Francis Low's earlier work was of somewhat similar status. Low did the same thing in one variable, and then Mandelstam did it in two variables.

D. Regge Poles; Chew-Frautschi Plot

CAPRA: Where did Frautschi come in?

CHEW: Frautschi came a year or so after that, in 1960. Mandelstam and I had been frustrated in our N/D calculations by a certain divergence that appeared in these equations. It turned out to be impossible to avoid this with the methods that we were aware of. This was associated with a power behavior that goes along with the spin 1 of the rho meson. We had to use the spin 1 in order for anything interesting to happen, and then we got into this difficulty in connection with asymptotic behavior. I was furious because I felt intuitively there should not be any divergence. The rho meson was not an elementary particle, it was coming out as a composite, and it was

ridiculous that it should produce a divergence just like in field theory. It was most irritating to have a difficulty characteristic of field theory just because the rho meson had spin 1. When Frautschi came -- now this is a very important historical question -- somehow or other we became aware of a paper by Tullio Regge. I forgot who told us about that paper, maybe it was Mandelstam. I am not sure, but it was probably Mandelstam. Anyway, somebody told us about the paper by Regge, which seemed to have something to do with our difficulty of the spin-1 asymptotic behavior. So we tried to read Regge's paper. In the beginning we did not understand it very well, but we did grasp the idea of an angular momentum which depends on energy. Regge somehow made an analytic continuation away from the integer angular momentum so as to make it smooth.

CAPRA: Was this one of his basic papers on complex angular momentum?

CHEW: It was practically the only one. As far as I know, he just wrote one paper, and he did it in the context of potential scattering. Frautschi and I, in frequent consultation with Mandelstam, came to the belief that this kind of behavior was general, that it would apply to the relativitic problem. I remember that Mandelstam was not very keen on this at first. We had long, long arguments about this question, which went on into 1961, I think. Frautschi was enthusiastic, and he and I worked together and developed some phenomenological applications before we understood clearly what was going on. Strangely, Murray Gell-Mann played a big role in this. He got interested, and it was Gell-Mann, I think, who said you should call these things Regge poles. He thought it was a big joke, because Regge himself did not have a clue as to what we were doing with them. He had simply written

that one paper, and from his point of view it was mathematics based on the Schrödinger equation, and there was no connection with a more general problem.

CAPRA: Did you see the relevance of crossing to Regge's formalism at that time?

CHEW: Yes, we certainly did. There was a confused period there, in which we were sure we had come upon something of generality and importance, but we weren't clever enough to get it really straight. We kept talking to other people about it and getting their advice, and gradually a number of other people became interested in the development. Gell-Mann certainly did; he was very enthusiastic about it; also Goldberger and some other people. Then Frautschi and I wrote a paper applying the idea. There were just enough baryon masses that had been measured at that point, so you could begin to see a Regge trajectory developing. But my real interest, and I suppose also Frautschi's, was to apply this to the bootstrap idea. We wanted to take the equations that Mandelstam and I had developed and apply this Regge boundary condition to them, so that we would get away from that divergence. Well, there was progress made in that respect, but in retrospect you can see that the understanding of Regge behavior still did not close the problem. During that period we worked up a lot of enthusiasm for the bootstrap notion, and other people picked it up and started to work on it from a variety of standpoints. So in the early sixties the term "bootstrap" was very widely spread, and a lot of different approaches to it were developed.

CAPRA: It seems that there were a number of simultaneous developments that generated great interest and enthusiasm at that time: the

bootstrap, Regge poles, S-matrix theory, and all that.

E. Recognition of S Matrix

CHEW: That reminds me of when the S matrix finally became recognized. It was not until I tried to write a book in 1961. I wrote a little book for the Benjamin series, S-Matrix Theory of Strong Interactions.[3] When I prepared that book and a talk for a conference that was held in La Jolla I said to myself: after all these years of pretending that what I was doing was field theory I finally want to be honest and say that I don't really believe in it; that what is important is analytic continuation. I kept looking for a word to contrast it with field theory, and suddenly I became aware that the S matrix was the point. It was not until 1961 that I really grasped that this was the concept Wheeler and Heisenberg had discovered twenty years before. I think at that talk in La Jolla I used the term "S matrix," and I certainly did in the book, and from then on I kept using it. Henry Stapp provided amplification very quickly by extending the idea to the description of spin.

CAPRA: When was the axiomatic work by Stapp, Iagolnitzer, and others done?

CHEW: That was somewhat later, but in 1962, and probably starting in 1960/61, Henry worked on the problem of spin.

CAPRA: What about Polkinghorne? He and some others wrote a book about S-matrix theory.[4]

CHEW: That's a tricky business. There was a team of four authors: Polkinghorne, Eden, Olive, and Landshoff. They had been working on

dispersion relations and analytic continuation, and their book, which finally appeared around 1966, comes actually in two parts. Part of it is sort of straight S-matrix theory, which was mainly written by David Olive, and the other part is Feynman diagrams. I remember, at the time I didn't like that. I thought the book ought to be just on straight S-matrix theory, that they should not spend all this effort on Feynman diagrams. Well, subsequently I've changed my mind; what they were doing was very relevant.

CAPRA: Around the mid-sixties, then, the S-matrix framework was more or less established.

CHEW: Yes.

CAPRA: This was the time when you wrote The Analytic S Matrix.[5] So that must have represented the culmination of your ideas at that time.

CHEW: At that time, it did. Yes.

CAPRA: Did you have any new insights while working on this book? You said that while working on the first book you really recognized the S matrix for the first time. Was there anything like that connected with the second book?

CHEW: I think there was a good deal less. As I remember, the book was a disappointment to me, because it was not able to move past two-particle channels. When Mandelstam and I developed our techniques we knew how to discuss poles and we knew how to discuss the two-particle branch points, but that was it. We did not know how to discuss anything higher -- and we still don't! This impasse has never really been overcome. Essentially, what the second book did was to add the Regge theory in a good deal more detail.

CAPRA: And I suppose you also presented things in a more systematic way.

CHEW: Yes.

F. Emergence of Bootstrap Philosophy

CAPRA: Now what about the philosophical side of the bootstrap idea? First of all, it seems that the bootstrap idea was always tied to S-matrix theory, even before you knew that you were dealing with S-matrix theory.

CHEW: Yes, that's right.

CAPRA: It seems that the whole idea emerged out of a pragmatic position and in a sort of technical way.

CHEW: Right.

CAPRA: You did not sit down to think how the world was built; you did not entertain general philosophical thoughts?

CHEW: No.

CAPRA: So when did the whole bootstrap philosophy emerge?

CHEW: I think it was during my collaboration with Mandelstam. I particularly remember one item in those discussions, which focused the issue. In developing the equations for the pi-pi system with Mandelstam we not only encountered the possibility of the rho meson being generated as a bound state and also producing the force necessary to sustain the bound state; we encountered a parameter in connection with the s-wave scattering, which Mandelstam wanted to associated with a standard field-theoretical parameter. In standard scalar field theories there is a $\lambda\phi^4$ term in the Lagrangian, which corresponds to an s-wave interaction for spin zero particles.

The parameter which showed up in our equations could be interpreted as such a coefficient in a Lagrangian. That's how Mandelstam wanted to interpret it. We had long arguments about this, and I don't remember how exactly the paper finally was written, but I didn't like that at all. I couldn't believe this system was going to admit a fundamental parameter of that character. It seemed absurd to me that it would. Mandelstam felt this was just a representation of field theory, and he thought λ was the fundamental parameter and that the rho meson would then, somehow, emerge driven by that parameter. Our equations did not indicate that; they indicated that the rho meson was driving itself. There was no real connection between that parameter and the rho meson. The parameter was just dangling out there, and subsequently it has been understood that it is no more fundamental than anything else. Nowadays one would not dream of referring to λ as a fundamental parameter. Because of that particular aspect of our theory I was forced to think hard about fundamental parameters. In this example it seemed clear that λ could not be a fundamental parameter. And I said to myself: but here is Mandelstam who believes that it's fundamental. Why? Because he believes in a Lagrangian. So, at that point I said: there is something sick about the whole Lagrangian idea that causes people to think there have to be parameters sitting there, which you are not going to be able to understand. I think in trying to defend myself, in trying to find a language that would express my idea, somehow the term "bootstrap" was helpful. I tried to explain to people why I felt that way. That parameter should not be there, because this was a bootstrap system which would not allow such things. I am pretty sure that up until then I had been vague;

I had been rather unclear in my own head as to what I believed concerning fundamental parameters, elementary particles, and the like. At that point I somehow crossed the bridge.

CAPRA: The fact that you should be able to derive the masses, or rather the mass ratios of particles seems to be much more intrinsic to the bootstrap framework, crossing, and all that. Didn't you feel that?

CHEW: Yes, but you know it is remarkably hard to put aside ideas you grew up with. When I was a student it was accepted that neutrons and protons were fundamental particles, and nobody dreamed of explaining their masses. I was aware that the logic of the bootstrap said you had to be able to determine them, but still, because nobody believed that you could determine them, the possibility was hard to accept. But then Frautschi and I were led to make our Regge plot, and -- my God! -- there was the proton sitting on the same curve with these other things. That was, somehow, a real punch -- to see the mass of the proton in what was clearly a dynamical context.

CAPRA: So the Regge formalism really helped you to work out your philosophy.

CHEW: Oh, yes, tremendously! When you see the mass of the proton sitting on the same curve with a lot of other things, that tends to dissolve prejudices.

G. Break with Convention

CAPRA: Now the bootstrap idea is extremely radical compared to the whole scientific tradition. Was this radical nature apparent to you, and was there a struggle? You know, when you read Heisenberg and Bohr,

you realize that they struggled like hell. Did you go through a similar phase?

CHEW: I remember going through something like that before that conference in La Jolla, asking myself: do I really believe this? Am I really prepared to back up this position? I went over all the developments that I had been exposed to until then, and I could only come to one conclusion, and that was the idea of nuclear democracy. None of the nuclear particles really could be said to have a fundamental status; they all had to be bound states of each other. It's true I was aware this was a radical idea, but nothing else made sense to me at that point. I couldn't see any alternative. The fact that the Schrödinger equation had not to be taken as a fundamental statement of dynamics had been working gradually on me over these years. I had seen how much the S matrix could do. Mandelstam, by the way, drove the final nail into that coffin, because he explicitly showed, using his double disperson relations, how you could recapture the Schrödinger equation as an approximation. So I had no feelings any longer that one needed an equation of motion.

CAPRA: Several years ago you told me that this was very crucial in your thinking, because with the equation of motion you also give up the notion of "things". When you have an equation of motion there is a thing that moves.

CHEW: That's correct.

CAPRA: I have heard people refer to a talk of yours where you were very enthusiastic and very radical. You said: "From now on you can forget about Lagrangians" and things like that. Was that the La Jolla talk?

CHEW: Yes, that was the La Jolla talk in 1961. I suspect there was a lot of emotional stress associated with preparing that talk and giving it. Up until then I had continued to operate as if I accepted field theory, even though I didn't believe it. I felt torn and dishonest, but I was so anxious to get problems solved that I didn't want to let arguments with my colleagues get in the way of solving these technical problems. But for some reason I decided . . .

CAPRA: . . . to come out of the closet, as they say these days.

CHEW: That's right (laughs), and having done that I was probably more inclined to think about philosophical questions. Once you have said to yourself: conventional wisdom does not have to be accepted, that's a big psychological break. I guess I was sort of expecting that as a result of that talk somebody would come and give me an overwhelming argument to the contrary. But they didn't. They didn't like what I said; they were furious, but there was no counter statement that was substantial. I remember a story about Arthur Wightman, who was doing axiomatic field theory at that time. He was furious at my La Jolla talk, but he also had a sense of humor. He put up a sign at his office door in Princeton, which said "Closed by order of G. F. Chew."

H. Decline of Bootstrap in Late Sixties

CAPRA: During the late sixties, there was a decline of the bootstrap idea, probably because of the difficulties you mentioned before, the inability to go beyond two-particle channels and to find the right kind of approximation.

CHEW: Yes, right!

CAPRA: At the same time the quark idea gained momentum. How did you feel in those years? You must have been disappointed, of course, but did you actually have doubts as to whether the bootstrap program could be carried out?

CHEW: I did not have doubts about the ultimate story; I certainly had doubts about the time scale, about whether I was going to see any significant part of it. I resisted the quark business very strongly at the beginning, because I felt that it was abandoning the whole bootstrap idea.

CAPRA: In those days people thought of quarks as particles, I suppose.

CHEW: Well, there was a confused period at the beginning, from 1962 to 1966 or so, in which Gell-Mann exercised a very big role. He did not call them particles.

CAPRA: He was talking about mathematical quarks, I remember.

CHEW: That's right; he didn't think they were particles. Then gradually naive but phenomenologically successful models were developed, by Dalitz for example, and, I guess, when people discovered the color concept to resolve the difficulty of the symmetry of the baryon wave function, they started to be less inhibited about calling the quarks particles. Then, finally, when QCD was invented, they lost all their inhibition.

CAPRA: So what was your attitude in those years?

CHEW: I resisted the quark idea for quite a number of years, but I began to be more receptive when the dual models began to show up.

CAPRA: So that was quite late.

CHEW: That's right; 1968-69.

CAPRA: Did you sense then that there was something behind the quark idea other than quarks as particles?

CHEW: I certainly resisted the idea of quarks as particles. I have never been able to swallow that, and I couldn't fit the quark idea into anything that made sense to me until the dual models appeared.

CAPRA: There is an interesting coincidence here. I remember you giving a talk at a conference in Irvine in 1969. I thought that this was a very pessimistic talk, very subdued. Actually, it was the only pessimistic talk that I have ever heard you give. At that time you must have been at the end of a long stretch of years where there did not seem to be much hope for the bootstrap.

CHEW: Yes.

CAPRA: And yet, it was at that very conference that the Harari-Rosner diagrams were also discussed. So that was the lowest point, and from then on it went uphill.

CHEW: Yes, I think that's about right.

CAPRA: However, at the same time you wrote two general more philosophical articles about the bootstrap,[6] one of them called "Hadron Bootstrap -- Triumph or Frustration?" In these two articles you expressed, basically, a positive outlook. Now, what made you keep your faith?

CHEW: I think, by that time there were so many philosophical elements in the picture which seemed to be stronger than the difficulties. I always felt that the difficulties were just lack of imagination. It wasn't that the bootstrap idea itself was wrong; it was just that we were without a good technique for pursuing it. I never really changed from that attitude. But, you know, when you are speaking at a meeting of physicists, and you haven't got anything to present . . .

CAPRA: That's not much fun.

CHEW: That's right. It is much easier to write a philosophical article and express your enthusiasm.

CAPRA: It is interesting that by that time, by the end of the sixties, the philosophy had become so strong that you could actually do that. In 1961, say, you couldn't have done it.

CHEW: Right, that's true.

IV. PHILOSOPICAL INFLUENCES

CAPRA: Given the radical nature of this bootstrap philosophy, I have always been very curious about your philosophical background. You are obviously a very philosophical person in the way you do science. Were you always interested in philosophy?

CHEW: No, I was not aware of being interested in philosophy. I tended to model myself after Fermi. I find this paradoxical in retrospect, but for a long time I tried to think that I was going to behave as much as possible in the spirit of Fermi. As a matter of fact, I recall that during the period of collaboration with Francis Low, one day we were riding in a car back from a conference, and Francis brought up the question of whether quantum mechanics was really understood, and we began discussing some of the crazy things about quantum mechanics. And I remember feeling what a waste of time this was to think about such things. I couldn't respond to Francis' arguments, but I was still very much a student of Fermi at that point, and I just didn't believe that scientists should spend their time worrying about issues like that.

CAPRA: You know, it's interesting that the S-matrix approach does have this pragmatic aspect, and it also has a very deep philosophical aspect. It's a very curious mixture.

CHEW: That's absolutely right. The S-matrix idea is the clearest expression of the Copenhagen interpretation.

CAPRA: You must have been interested, though, in the whole mystery of quantum mechanics, the Bohr-Einstein debates, and so on.

CHEW: No, I wasn't. I think I appreciated that there was a difference between quantum mechanics and the Schrödinger equation. Fairly early on I knew that quantum mechanics really meant the S matrix. So my war at that point was with the Schrödinger equation or, if you like, with the use of the space-time continuum as the underpinning. I felt that the S matrix was completely capable of doing everything that needed to be done, and I didn't worry very much about the philosophical significance of that position. It really was only a good deal later, when I had to write and give talks, that I started to think about that.

CAPRA: This is very difficult for me to imagine. I met you in 1969, when I was at UC Santa Cruz, and I remember that you came and gave a talk about the significance of small parameters from the bootstrap point of view. I was very impressed, already then, by your way of presenting things and by the depth of your thinking. So I have always known you as a very deep thinker and a very philosopical person. Did you turn into that at some stage?

CHEW: Hmm! (smiles)

CAPRA: You see, this is really a surprise to me that you say you weren't interested in philosophy, nor even in the philosophical aspects of physics.

CHEW: Well, somewhere in the 1960s, I guess, there must have been . . . Well, okay, I can remember, when I was in England in 1963, that I was asked to give a lecture. I was beginning to become more philosophical during that year. I remember that in addition to this big lecture, which had a certain amount of philosophy in it, I also gave a small lecture to a Cambridge college, in which I tried

to persuade them that there was no absolute truth in science. I remember that this was a pretty radical thing to do, at that point, in Cambridge.

CAPRA: You see, I always had the idea that there must be something in your interest -- some philosophical tradition, some religious tradition, or something in the world of art -- something that influenced your thinking. We know that Niels Bohr was influenced by Kirkegaad and by William James, that Heisenberg was reading Plato. Some of the ideas from these traditions influenced them and helped them in their conceptual crisis. But there doesn't seem to be anything of that kind in your life.

CHEW: I can't identify anything like that. That's quite true.

CAPRA: Maybe that just means that you are really an original thinker.

CHEW: No, I don't think so. The influence was there, coming in various ways that were not so obvious. Let's see, maybe I can identify a few roots. You know, Edward Teller was somebody who had a substantial influence on me in addition to Fermi. Fermi was not interested in philosophical questions, but Teller really was. When I was a student at the University of Chicago, Teller made me aware, either in formal lectures or in private conversations, of some of the great philosophical issues associated with quantum mechanics. In particular he told me a few things which somehow stuck. I haven't thought about this for a long time, but either Teller or somebody else made the point that the quantum theory of electromagnetism, which had been analyzed by Bohr and Rosenfeld and which implied some extremely puzzling aspects in connection with electric charge, measurement, and so on, that all this only made sense because of the zero mass of the

photon. You could understand the known facts about electromagnetism and also the presence of quantum principles only because of the zero mass of the photon. There was an approximation involved that had to do with the dimensions of the measuring apparatus, and you couldn't expect the notion of a local quantum field to have any final, definite meaning. I guess I've never forgotten that. So I got this idea early on that we really depend on approximations. And not only do we depend on approximations, but the nature of the theories that we construct depends on certain physical parameters. There is a remark, along the same line, attributed to Bohr, which I recall. If the fine-structure constant were not small, our whole way of looking at quantum mechanics and the real world would be totally different. It is very, very important that the fine-structure constant be a small number in order for matter to be involved in a way that allows us to think about it the way we do. We depend on the smallness of 1/137 very, very much. I found it troubling that most physicists, when they carry out their activities, ignore those considerations, that they never stop to think about the significance of the parameters.

Also, George Gamow had an influence on me. I met Gamow extremely early, when I was only 18 years old, at George Washington University. His courses were anecdotal and not very systematic; he picked out the spectacular and glamorous aspects of physics. You know that he also wrote a series of popular books.

CAPRA: Yes, of course.

CHEW: From him, I guess, I must have learned some of these peculiarities of quantum mechanics very early. And those were things you could not grasp within the traditional view. That's right. When you put your

finger on it, the fact that quantum mechanics makes sense has a strong bootstrap implication. If the parameters were not right, it wouldn't make sense. I believe that Gamov might have gotten that across in his discussions of some of the paradoxes that arise when you suppose that quantum principles govern the phenomena of our ordinary world. In discussing these examples he must have taken the parameters of the real world and shown that some very good approximation was involved. So that idea that approximation was crucial and that parameters were always important must have come very early.

CAPRA: So all these philosophical influences on you really came from scientists. There was no parallel influence, apparently, from any school of philosophy.

CHEW: Well, I am certainly not aware of any. I realize, when I talk to philosophers, that I know so little about philosophy it is embarassing.

V. RECENT BREAKTHROUGH IN THE BOOTSTRAP PROGRAM

A. Topological Expansion; Ordered S Matrix

CAPRA: Now I would like to come to the recent history of the bootstrap. What was the actual breakthrough, and when did you become aware of it?

CHEW: There were many steps which impressed me, and the cumulative effect is a little hard to break up into pieces. I became seriously interested in the new developments in 1974 when I ran into Veneziano at CERN and learned about his notion of a planar approximation. He identified the idea that there was a level in something like a topological expansion, which was topologically planar, where some remarkably simple things happened and the bootstrap became really very much clearer and simpler to understand. Not only that, there were also experimental facts which supported the usefulness of this topological expansion. Shortly thereafter Carl Rosenzweig showed up here in Berkeley. He had been in contact with Veneziano and we started to work together and wrote a few papers which looked quite promising. We were also in touch with a fair number of other people who worked on related things, and then in 1977 we undertook to write a review of those new developments.[7] In the course of getting ready for this review we discovered the concept of what we then called the ordered S matrix. It was a formalization of Veneziano's thinking, but I remember that, when it was presented to Veneziano, he was quite clear in saying that this was something new, that it was something added to his ideas. Again, it is a little hard to say in which way it added, but he did make that statement. Certainly in my own thinking,

seeing the concept of the ordered S matrix appear was a big support. It meant that there was a mathematical area which was suitable for bootstrap theory. Up until that point there were a lot of vague statements floating around and we didn't know how to convert them into something that was really a discipline.

CAPRA: Now I want to backtrack a little bit. In the years between 1969 and 1974, in those five years, there were a lot of ideas which were precursors of the new development -- duality, the Veneziano model, etc. Did you recognize those as being relevant to the bootstrap?

CHEW: That's a good question. Between 1969 and 1974 I was aware of these dual models and very interested in them, but I didn't know how to take them and do something with them in terms of the S-matrix framework.

CAPRA: These models were very much associated with quarks.

CHEW: That's right, but it was apparent to me that they were not field theory. There was something else that was going on. Unfortunately, from my standpoint, people succeeded in translating a lot of that into something that was called a string model. The string model has a funny in-between status, which just threw a fog over everything. It is not field theory, but it is Lagrangian theory with arbitrary parameters and uses the space-time continuum as a base. I was quite confused and put off by all the activity that went into string models; it did not seem the right thing one ought to be doing.

CAPRA: In a sense this seems to be a parallel to the early history of S-matrix theory, where people were doing something that would later turn out to be relevant to a new development in S-matrix theory

but were doing it from a field-theory perspective.

CHEW: Yes, that's right. So I listened to what people were saying about the string model, but I didn't work on it. What I did during those years was work related to the concept of the pomeron. There were several papers with Pignotti and Snyder on trying to clarify the status of the pomeron, and that set me up for the influence of Veneziano in 1974. What I became sensitive to in those years was the fact that this phenomenon that was called the pomeron had a lot of simplicity to it, but that there was a mysterious weakness associated with it. This was perplexing to me because it seemed to contradict one of the assumptions of bootstrap theory, which was that strong interactions were self-generating. Here was a piece that was clearly strong interactions with some very simple properties but with a strength that was very weak. I was puzzled; where could this small number come from? I think I wrote several papers in which I made an effort to understand what it was that made the pomeron weak. Well, in 1974 when I went to CERN and talked to Veneziano, that was the thing that really hit me. His topological expansion didn't have the pomeron at the planar level. The pomeron was a correction, and that immediately appealed to me as a natural explanation for its weakness. You see, that was kind of symbolic of the whole new development. Even though strong interactions are strong, nevertheless it is profitable, via the topological expansion, to make some sort of a classification of different levels of strength. I think that was for me a very, very big step; to get over the idea that <u>all</u> of strong interactions need to be understood at the same time. Maybe we could do bootstrap but nevertheless have hierarchies.

CAPRA: At that time you must have felt a tremendous surge of enthusiasm, after these ten years or so of "crossing the desert," as it were.

CHEW: Yes, that's absolutely right. Of course, the enthusiasm did not suddenly come in 1974. It began, and then, as Rosenzweig and I started to work, it built up as we saw more and more things that wanted to emerge.

B. <u>Topology -- The Language For A New Science?</u>

CAPRA: So the really new development was then the recognition of order as a new ingredient in particle physics; and topology, of course, is very closely related to order.

CHEW: Right.

CAPRA: From the most general point of view you could say that, when you have that philosophy of "no foundation," you deal with relationships and topology seems to be the language of relationships par excellence. Therefore, it would seem to be the language most appropriate for this whole web philosophy and for the bootstrap idea. I really see a tremendous potential here. Topology could really be the mathematical language for a new science.

CHEW: Yes, I agree. That's my feeling about it. I tried to say that while David Bohm was here. Bohm has emphasized the importance of language, and I suggested to him that in this problem of getting beyond explicate order, as he calls it, the order of the the ordinary real world, our language is extremely prejudicial, because so much of the language that we use is based on explicate order. Bohm knows this and he tries hard to get around it. So what I was proposing to

him is that the language of topology, and in particular of graphs, seems very suitable. It is my feeling that in the future some extremely deep questions are going to be approached using the language of graphs.

CAPRA: Now, topology and graph theory are two distinct even though closely related languages. Topology seems the more general framework.

CHEW: I have asked myself that question many times, and I am not sure. I have also asked Poénaru and he is not very definite on this subject. Certainly, graphs without "thickening" are not sufficient, but what I don't grasp is whether this thickening of a graph is all of topology or whether it is only a teeny bit of topology. At the moment I find it still a puzzling feature of the topological bootstrap theory that there is so much redundancy. Sometimes we find it appropriate to talk about graphs, sometimes about surfaces, and the two are so interlocked that it's hard to know . . .

CAPRA: Coming back to the problems of language, you have mentioned several times in discussions we had over the years that the question-and-answer framework of ordinary scientific investigation will be found unsuitable when we want to go beyond the Cartesian framework. Have you given that any more thought?

CHEW: Well, I have come to believe that topological language is a very good candidate for going beyond the question-and-answer framework. The way our theory has developed in the last few years, we quite typically don't know what question to ask. We don't get into the posture of saying: Here is the question; let's try to answer that question! We simply . . .

CAPRA: . . . sort of muddle along . .

CHEW: . . . muddle along and use consistency as the guide. Each increase in the consistency then suggests something that is incomplete, but it rarely takes the form of a well-defined question. We are constantly downgrading concepts that in the recent past would have been considered fundamental and would have been used as the language for questions. You see, when you formulate a question you have to have some basic concepts that you are accepting in order to formulate the question. This is also related to something that you and I have already discussed. The description of our subject can be begun at a great variety of different places. There isn't any clear starting point. So it's very hard to say, when you are writing about this business, this is the problem that is being attached and then go from there.

CAPRA: Right. Since the whole system represents a network of relationships without any well-defined foundation, you can start almost at any arbitrary point.

C. Achievements of Topological Bootstrap Theory

CAPRA: Geoff, I would now like to ask you to give an assessment of the topological bootstrap, sort of a summary of the results achieved so far.[8] Which puzzles, actually, did it really solve? What were the most important achievements?

CHEW: I am almost sure that what I am going to say will not be what I would say a year hence, because this is a subjective question. As a matter of fact, some of the things that impressed me a number of years ago no longer impress me now, because I have gotten so used to them. Let me see if I can overcome that.

One of the important developments has been explanation of the distinction between strong and nonstrong interactions. The connection of this distinction with the topological expansion has been, I think, a big success. I believe that in the future it will also illuminate the meaning of space-time, because the notion of a continuum, which has some connection with space-time, does not emerge until you get to the electroweak level. The ingredient that characterizes the strong interactions qualitatively is the contraction idea, which goes with zero entropy and which is also very much connected with inaccessible degrees of freedom. We can now see not only how these things cause the strong interactions to be strong; we also understand the order of mangitude of the observed strength. I think that's an enormous step forward. However, it will take further development of the theory to convince other people of this.

We also understand why the machinery of quantum electrodynamics with its reliance on space-time should work and why strong interactions can coexist without having any such underpinning. All interactions share the S matrix -- the analyticity properties, unitarity -- all these notions coexist, and yet we get separation between strong and electroweak interactions.

CAPRA: Now let's talk about the strong interactions themselves. I think the understanding of the nature of quarks would have to be counted as one of the big successes there.

CHEW: Oh yes, and going into more detail I would say the meaning of the mysterious color degree of freedom has been exposed and the meaning of quark generation tentatively brought out, although we are still not completely sure about quark generation.

CAPRA: Baryon number conservation has also been understood.

CHEW: Yes, baryon and lepton number conservation, electric charge quantization -- all these famous quantization characteristics have been explained.

CAPRA: How would you now characterize the quark concept?

CHEW: The quark concept is an extremely useful way of describing part of the order, which is present in the strong interactions. A more detailed description would be the following one. In contrast to the standard approaches we do not identify the quarks with the lines of the Feynman graph. When you express the order you thicken the Feynman graph by embedding it in a bounded surface, and then it is the <u>boundary of the surface</u> which houses the quarks.

CAPRA: More generally, can you say that, if particles are relationships then quarks are patterns in these relationships?

CHEW: Yes, you can say that.

CAPRA: And this is what has been understood. The relationships are not arbitrary; there are constraints, which produce patterns, and these patterns are the quarks. Now the confinement is also something that has been understood.

CHEW: Quark confinement is automatic from that standpoint; because quarks don't carry momentum; they are not identifiable as particles.

CAPRA: Finally, the fact that all particles are built of two topological elements seems to be a major result.

CHEW: Yes, the theme of twoness is pervasive in this theory, and the full significance of that feature has yet to be understood. We are finding more and more two-valued quantities to be required by consistency, and their combinatorics control the whole business.

CAPRA: Might that be connected with the two dimensions of the surfaces we are using?

CHEW: I don't think so. It is rather that the idea of an orientation is two-valued. We have two kinds of boundary units, and these units come with a variety of two-valued orientations. Out of these patterns of twoness we think we have understood the major characteristics of the strong interactions and quite a number of things about electroweak interactions.

CAPRA: So this pattern of twoness is now more important than the threeness that was emphasized before -- the three sides of the triangle, the three colors, etc.

CHEW: I am hesitant to say that. The threeness comes from the triangle or, you can also say, from the cubic vertex. A graph becomes nontrivial as soon as it develops a cubic vertex, and from the cubic vertex you can build everything. So you need threeness in addition to twoness.

CAPRA: As far as particles are concerned, the theory gives a finite number of particles, right?

CHEW: If you speak of elementary particles, that's right, a finite but large number. I once counted how many elementary hadrons there are. There are about 18,000 of them.

CAPRA: 18,000?

CHEW: Yes, most of them are particles with 6 topological constitutents that we have begun to call "hexons". There are maybe 200-300 elementary mesons, 1,000 elementary baryons and all the rest are hexons. We are expecting the majority of physical hexons to have too large a mass to be detected with present accelerators.

D. Outstanding Problems

CAPRA: Now, Geoff, what would you say are the major outstanding problems in the topological bootstrap? Let's first talk about hadron physics.

CHEW: In hadron physics the identifiable major outstanding problem is the breaking of generation symmetry. We have not yet understood how that comes about. We have some candidate ideas, and they all have to do with the coupling of strong and electroweak interactions. Without going to electroweak interactions, we do understand the breaking down of the hadron supermultiplets, for example the differences between mesons and baryons, or the difference between a rho and a pi meson. But for distinguishing between the generations, for example between the pi and the K meson, you have to go to electroweak interactions. The infrared phenomena is another major area to be developed, both from the practical and conceptual standpoints. The fact that we start with zero-mass electroweak particles is of great significance, both quantitatively and qualitatively, and one of the main things for the future is the development of a real theory of measurement.

CAPRA: I want to come to this whole question later, because that is a big question.

CHEW: Right.

CAPRA: In connection with hadrons, can you think of other major outstanding problems?

CHEW: For the hadrons . . . the hadrons . . .

CAPRA: What about the zero-entropy bootstrap?

CHEW: That's a technical problem. I don't see it as a tremendous puzzle. I think it's a challenge to our ingenuity to figure out

better ways of solving that problem. We do need some new ideas, but I wouldn't put that in the category of major outstanding problems.

CAPRA: That really attests to the tremendous success of the topological bootstrap for hadron physics.

CHEW: Yes, I am hard put to think of qualitative questions that have not been explained. All the quantum numbers -- the parities, the spins, and all that -- all of that seems to be understandable.

CAPRA: With respect to electroweak interactions, of course, the whole theory is still changing.

CHEW: That's right.

E. Resistance of Orthodox Physicists

CAPRA: Now, if the topological bootstrap has been so successful in hadron physics, why is there such tremendous resistance among orthodox physicists? Why do they hesitate so much to accept it and what will convince them?

CHEW: I think there are various ingredients in the answer. One is that QCD was very successfully sold as the theory of strong interactions. Although the deficiencies of QCD are recognized by many people, there is this prevailing sense that it is the correct theory. Some of the things that it doesn't explain and which the topological theory explains would be, for example, the origin of quarks, the origin of three colors, the origin of all these quantum numbers which you simply have to put into QCD. But people have gotten used to putting things in, and they have forgotten that one needs to answer questions about why such ingredients are there in the first place. At some point they will come around to thinking about that. Part of the problem, Fritjof,

is that the topological theory has not been presented in any easily understandable way. You wrote a review and I wrote a review,[8] but in neither of these reviews did we go into any detail about Feynman rules.

CAPRA: In other words, nobody can apply the theory.

CHEW: Nobody can apply it, that's right. I think there are lots of young people around who would do things with it if they could get their hands on it. Maybe I made a mistake in not sticking with the strong interactions; I became deeply involved in this problem of the electroweak interactions.

CAPRA: So what happens is that practically all the people working on the topological theory work on the fundamental development of the theory, and nobody worries too much about applications.

CHEW: Well, there are some who do, like Nicolescu.

CAPRA: Yes, but since the entire number is very small those are just one or two.

CHEW: That's right. On the other hand, given the state of QCD at the moment, I think that people are subconsciously aware that QCD is not doing the job, but without an alternative they are not recognizing this explicitly.

CAPRA: And you think the feeling that QCD is not living up to the expectations is spreading?

CHEW: Well, my best measure is the graduate students here. During this last year I repeatedly found graduate students who were not satisfied with QCD; there was a lot of interest in the topological approach at the level of graduate students.

F. QCD and Weinberg-Salam Theory

CAPRA: Maybe, at this point, I could ask you to summarize your criticism of quantum field theory, that is both of QCD and of the Weinberg-Salam theory.

CHEW: My position derives from the way the topological theory has been evolving and from the fact that it makes so much sense. What has developed is that you can associate with both weak and strong interactions a Feynman expansion. That goes somewhat beyond what we used to say. We used to think that strong interactions would be pure S matrix and would not admit an off-shell continuation. The way the theory has evolved, there is an off-shell continuation. This was painful to accept; we did not really want it; we thought that it was contrary to the S-matrix spirit, but after long discussions we came to the conclusion that you really had to extend off-shell and that this did not upset the essential ideas. More importantly, the theory tells you how to go off-shell; it isn't something that is left open. But this extension does not imply a field theory. This is the strange thing. For a long time people said: if you go off-shell this is equivalent to field theory. That's not true.

CAPRA: So you don't have local interactions?

CHEW: You don't have local interactions, even though you have Feynman rules. The big difference is that the vertices for strong interactions are not simple polynomials in momentum.

CAPRA: Because of contraction?

CHEW: Because of contraction. Each hadronic vertex represents the contraction of an infinite number of topologies, so that the function associated with the vertex has an infinite sequence of singularities.

This is qualitatively different from what you get in ordinary field theory. If you Fourier-transform a topological vertex function you will not get a local spacetime interaction. This is the situation for the strong interactions. Now, the extension to electroweak interactions, right from the beginning, has not admitted contractions. The logic of this situation has never been explored as carefully as would be appropriate. Next year I hope to start writing a complete and systematic account of the whole thing, and then I will try to work out why the electroweak topologies do not admit contractions. There are all sorts of indications that they should not admit contractions; therefore the vertices for their Feynman rules are local if you Fourier-transform.

CAPRA: So then you do have local fields.

CHEW: Yes, we think that you can define local fields. There is a tricky point here. The locality really is not a property of a single field; it's a property of their interactions; for example, you have three fields interacting at the same point in space-time with each other. So it's a little bit misleading to talk about just local fields individually; it's a matter of the Lagrangian.

CAPRA: And this locality is a feature of the electroweak topologies?

CHEW: Right. Without having insisted on locality, it seems that the non-contraction associated with the electroweak interactions implies that the interactions are local.

CAPRA: Regarding your criticism of the orthodox theories, there seems to be two key issues involved then, the locality and the arbitrariness.

CHEW: That's right, and the two are connected, because it was contractions that eliminated the arbitrariness for the strong interactions, where the zero-entropy level is completely controlled by contractions. Now, you can then ask: what is it that makes the electroweak theory non-arbitrary if you don't have contractions. Here we are in a fuzzy area, but there are at least two recognized sources of non-arbitrariness for electroweak theory. Firstly, segments of surface boundary are building blocks for all elementary particles. They control strong interactions and you use the same objects to build electroweak particles. If you just said that, however, there would still be a fair amount of arbitrariness.

CAPRA: Well, the other link would be via the "naked cylinder."

CHEW: That's right, but I think it's probably true that I am the only one so far who has paid much attention to this. Of course, the argument is not tight; it has been loose. Finkelstein and Poénaru have been sympathetic to my reasoning, but . . .

CAPRA: This is probably because they came in later, after you had worked out the whole cylinder business. So you probably have a better feeling for it.

CHEW: That's probably true. We were talking before about this strange period between 1969 and 1974, when I was working on pomeron theory. Now that was all cylinder! So later, when the electroweak stuff came along, I immediately saw it as cylindrical. It was always cylindrical, right from the beginning.

CAPRA: So the arbitrariness is reduced, or even eliminated, by that link to the naked cylinder.

CHEW: I think that it will eventually be eliminated, but it is reduced already.

CAPRA: So what, then, is your assessment of QCD and the Weinberg-Salam theory on the basis of the topological theory?

CHEW: I can see Weinberg-Salam as basically O.K. It's a question of what the full group is; what they have done is to identify the dominant low-energy subgroup. The idea of using a gauge field theory for describing the electroweak interactions seems to be supported by the topological approach.

CAPRA: So you expect the Weinberg-Salam theory to be recuperated more or less completely by the topological approach.

CHEW: Yes, it almost has been already. We have come close to recapturing the whole thing. What we are doing now is trying to figure out the part of the story that has not already been guessed by Weinberg and Salam.

CAPRA: And what about QCD?

CHEW: QCD is a local field theory, which treats strong interactions exactly the same way as electroweak. It is a naive extension of Weinberg-Salam; you take Weinberg-Salam and you replace SU(2) by SU(3). Otherwise, you do very little to it.

CAPRA: Which means, you ignore the fundamental difference between strong and electroweak interactions.

CHEW: Right.

CAPRA: They would argue, of course, that this is justified by the phenomenon of asymptotic freedom.

CHEW: Yes, they say that there is another scale at which these things are unified, but there are qualitative differences beyond that. The

degree of freedom associated with SU(2) is isospin, or electric charge, which is an observable degree of freedom labeling the particles. The analogous degree of freedom for SU(3) is color, which is not observable. Now that is a clear qualitative difference between these two situations, and there isn't any logical explanation why you should get confinement in the one case and not in the other. They set up the two theories in completely parallel fashion, and then they just say you get confinement in one case and not in the other.

CAPRA: Now, in the topological theory, can you relate confinement to contraction?

CHEW: Yes. We are talking here about color confinement, which in topological theory means that color is an inaccessible degree of freedom, similar to the cyclic order of lines around a vertex. It is another kind of order which you sum over. Now, if you don't have contractions, you don't have inaccessible degrees of freedom either. The inaccessible degrees of freedom and the idea of contraction go together. There are no contractions without inaccessible variables and in electroweak interactions all variables are accessible.

CAPRA: That is an interesting connection.

CHEW: Yes, it is tremendously interesting. This is one of the things I am going to try hard to get straight next year.

CAPRA: Coming back to QCD, do you think that it will be thrown away entirely?

CHEW: Yes, as a fundamental theory, because it is fundamentally wrong. As a phenomenological theory for GeV-scale physics, part of QCD will survive. It gets certain things right, such as energy-

momentum conservation and Lorentz invariance, and people have put in by hand the right number of degrees of freedom. They looked at experiment and said, we see there are three colors, and there are two spins on the quark, and they inserted these features, so the theory is bound to have phenomenological utility, but it's profoundly wrong.

CAPRA: Now if QCD is wrong, then the grand unification schemes will have no future either.

CHEW: No, they will not. By the way, topological bootstrap theory predicts absolute stability for the proton, in contrast to most grand unification schemes.

VI. OUTLOOK

A. Space-Time Continuum and Electromagnetism; Gravity

CAPRA: I would now like to talk about future developments, and to begin with I would like to discuss the concept of the space-time continuum with you. During the course of our conversation you have repeatedly been critical of the use of a space-time continuum, and you have also said that the paradoxes of quantum mechanics arise, in your view, because in the framework of quantum mechanics atomic phenomena are embedded in a continuous space-time. Now, evidently, atomic phenomena are embedded in space-time. You and I are embedded in space-time, and so are the atoms we consist of. Space-time is a concept that is extremely useful, so what do you mean by the statement that one should not embed atomic phenomena in space-time?

CHEW: Well, first of all, I take it as obvious that the quantum principles render inevitable the idea that objective Cartesian reality is an approximation. You cannot have the principles of quantum mechanics and, at the same time, say that our ordinary ideas of external reality, the explicate order, are an exact description. You can produce examples showing how a system subject to quantum principles begins to exhibit classical behavior when it becomes sufficiently complex. That is something which people have repeatedly done. You can show how classical behavior emerges as an approximation to quantum behavior. The WKB approximation is a famous example, and there are lots of others.

CAPRA: Well, more generally, you can derive the basic laws of Newtonian physics from quantum physics . . .

CHEW: . . . as approximations, when things become adequately complex. So the classical Cartesian notion of objects, and all of Newtonian physics are approximations. I don't see how they can be exact. They have to depend on the complexity of the phenomena which are being described. If the phenomena become too simple, the classical description won't work.

CAPRA: So you have a quantum level at which there are no solid objects and at which those classical concepts do not hold; and then, as you get to higher and higher complexity, the classical concepts somehow emerge.

CHEW: Yes.

CAPRA: And you are saying, then, that space-time is such a classical concept.

CHEW: That's right. It emerges along with the classical domain and you should not accept it at the beginning.

CAPRA: And now you have also some ideas about how space-time will emerge at high complexity.

CHEW: Right. The idea, actually, was present in a qualitative form long before and probably goes back right to my graduate-student days. When you learn quantum field theory, you learn that the quantum fields are introduced as operators, just like the operators in standard quantum mechanics, but they don't correspond to observables.

CAPRA: Except for the electromagnetic field.

CHEW: Except for the electromagnetic field; that's right! That's the only exception. And that just hit me like a bomb. Why should the electromagnetic field be different? I never forgot that, and I was constantly amazed that my colleagues, who did field theory, never

seemed to pay any attention to that. It never fell out of my consciousness that there had to be some very special feature connected with the elctromagnetic field.

In my student days the idea was put into my head that the possibility of the electromagnetic field being an actual observable, in the same sense, as say, momentum, was associated with the zero photon mass. It is actually associated with more than that, but I remember being told that zero mass was the essential ingredient. And as time went on and I began to think about the S matrix in connection with strong interactions, I was much impressed by the fact that electromagnetic fields which could be described classically would not tolerate an S-matrix description. There was a funny complementarity. The zero photon mass, which allowed the electromagnetic field to have the status of a classical observable, also means that you cannot describe photons by the S matrix.

CAPRA: Is this statement still true in view of the recent successes of topological electroweak theory?

CHEW: It is still true because of the infrared phenomenon. At our present level of understanding, we have to think of the electroweak particles as massless, and that leaves this very important infrared problem still to be faced.

CAPRA: And we might have to go beyond the S matrix to solve it?

CHEW: I am sure. Henry Stapp has already taken the first step in that direction by showing how you can start, basically, with an S-matrix approach but then, when you are faced with these zero-mass particles, have to change the basis from the asymptotic states which are based on counting individual numbers of particles to asymptotic

states where you superpose coherently different numbers of particles. Now, when you are superposing states containing differing numbers of particles you are not, strictly speaking, going outside the S-matrix idea, because the S matrix allows superposition, but it is not the way we normally use the S matrix. The idea that these states are then measurable as superpositions, that's where you get into a new area. What is it you are measuring when your apparatus is sensitive to a superposition of states with an indefinite number of particles? But that is exactly what a classical apparatus does. When you get to the classical level, you are using the fact that soft photons are coming in whose number is not well-defined.

I want to emphasize again that the idea of the S matrix implies such an extension should be possible. The statement that the S matrix is unitary is based on the idea that you can superpose the basis states. You can always give a meaning to an arbitrary superposition. In fact, you can't prove unitarity unless you assume that an arbitrary superposition of states is possible. That means also states of different numbers of particles. It is not a notion that you normally use in S-matrix theory, but it really is implied by unitarity. The thing which is perhaps qualitatively new is this idea that the superposition should involve states of arbitrarily large numbers of particles.

CAPRA: So the very special nature of electromagnetism stayed in the back of your mind during all those years while you were developing the S-matrix framework?

CHEW: Oh yes. Because the zero mass kept electromagnetism out of the standard framework and because it allowed the status of a classical

observable for the electromagnetic field, I have always been very much impressed by the importance of electromagnetism, and you will find occasional references to it in my papers, here and there.

CAPRA: I know. In fact, I would like to read to you a passage from a paper that you wrote in 1971:[9]

> Electromagnetism is deeply mysterious and its origin unlikely to be explained within our current scientific framework because the unique attributes of this interaction are inextricably enmeshed with the framework itself.

In view of the recent progress, have your views changed since you wrote this passage?

CHEW: No.

CAPRA: But you expect now to unravel a little more of that mystery, don't you?

CHEW: That's right, but we <u>are</u> departing from the scientific framework. That's one of our problems of communication, because in challenging the standards of space-time we are off into the unknown, to some extent. Now we can conceal it when we publish papers and talk to people, and people won't know that we are really . . . off the deep end (laughs). But the fact is, you <u>are</u> as soon as you say that the meaning of space-time is coming from the graphs and not vice versa.

CAPRA: So your statement still stands, and you are saying the more we learn about electromagnetism the more we are forced to give up the standard scientific framework. It is at that expense that we can learn about electromagnetism.

CHEW: That's correct.

CAPRA: Now we got into this discussion of electromagnetism by talking about space-time.

CHEW: I know. You see, the understanding of what a measurement is, of what space-time is, of what an observer is, of what electromagnetism is -- all these are tied together. The language that is useful for discussing this is the language of graphs, and the key notion is the idea of gentle events.[10] This idea is uniquely associated with photons. What it means is that in the Feynman graphs there is a special kind of vertex where two of the lines are very closely connected -- what is carried in on one line is almost exactly carried out on the other line -- and the third line is the photon, which is bringing almost nothing in or out.

CAPRA: That is, a very slight disturbance.

CHEW: Very slight disturbance, and it is in the analysis of why this is possible that you understand the special features of the photon. It has to have zero mass, and it also turns out that it has to have spin 1. It has to flip the chirality, that's the strange thing. Connected with spin 1, furthermore, is the attraction of unlike charges and the repulsion of like charges, which produces the effect of large clumps of matter that are almost electrically neutral. That is necessary for the appearance of classical reality. A classical object is recognizable as such partly because it doesn't carry an enormous electric charge.

CAPRA: So there are these gentle events, and they pile up.

CHEW: Yes, and they pile up coherently, and these coherent superpositions of photons generate the classical fields.

CAPRA: And how is this connected with space-time?

CHEW: Well, from the point of view of a quantum starting point, everything is discrete in the beginning; there is no continuum, and the discreteness is represented by the vertices of graphs. The graphs have no metric associated with them; there is no meaning to the distance between two vertices. These vertices I would call the "hard" vertices, and they are interspersed with all the "gentle" or "soft" photon vertices. There will be an infinite number of superpositions of these gentle vertices, whereas the hard vertices will remain finite and discrete. What Stapp showed, in order to solve the infrared problem -- the specific mathematical problem associated with the zero mass of the photon -- is that you are led to certain particular superpositions which, in effect, approximately localize the hard vertices. So after you have added this infinite coherent superposition of soft photons a hard vertex, which to begin with had no sense of space-time localization, acquires an approximate localization.

CAPRA: And the more soft vertices you have, the preciser the localization?

CHEW: Well, it doesn't ever localize beyond a certain limit.

CAPRA: You mean the limit given by the uncertainty principle?

CHEW: Yes. You don't ever get arbitrary localization, but you discover that you get as much localization as you expect in order to have a classical interpretation.

CAPRA: Now, you are saying that you could <u>derive</u> the uncertainty principle from that?

CHEW: Oh sure. You can derive the uncertainty principle <u>and</u> you can derive space-time. The idea of deriving space-time, of course, is really the more striking.

CAPRA: So what emerges is the notion of continuity and the notion of approximate localization.

CHEW: Right.

CAPRA: And with it the uncertainty principle.

CHEW: Yes. And, I would say, with it a meaning for measurement.

CAPRA: I see. All these steps are still tentative, though.

CHEW: That's right. But out of this will also come, at the same time, the capacity for recognizing certain patterns of events as representing an observer looking at something. Once you have the gentle-photon idea in the picture, you can begin to do that. In this sense, I would say, you can hope to make a theory of objective reality. But the meaning of space-time will come at the same moment. You will not start with space-time and then try to develop a theory of objective reality.

CAPRA: Now what about the dimensions of space-time? That does not follow from the gentle events.

CHEW: No. I think that comes out of the topology, and we are getting some very very strong hints about that right now. The fact that momentum has four components is associated with a 2 x 2 matrix, and we suspect very much that this matrix is related to these pairs of two-valued orientations, that is to the apparent twoness inherent in the nature of elementary particles. So it seems that these topological notions can be translated into the statement that momentum must be a four-component object.

CAPRA: What about the conservation of energy and momentum?

CHEW: Well, that is something we don't have such a specific idea about, but I have a conjecture about it. Momentum appears to be built from indices which are closely connected to spin indices, and the same kind of topological symmetry that gives rise to SU(2) isospin invariance also seems to give rise to SU(2) spin invariance, which is rotational invariance and means angular momentum conservation. So the topology does definitely promise to explain why angular momentum is conserved. Now, if spin and momentum are built basically from the same topological indices and you can get conservation of angular momentum, it seems to be very plausible that you are also going to get conservation of linear momentum and energy. I feel that sooner or later we will understand that.

CAPRA: Now I have another question. Considering that you envisage deriving the basic properties of space-time, what about the constancy of speed of light? Will that be derived too?

CHEW: Well, that somehow just comes along with the Lorentz invariance. If you can get rotational invariance, the analytic continuation of the rotation group will lead you to the Lorentz group, and the Lorentz group effectively contains this notion of the speed of light.

CAPRA: So you foresee that all this could really come out of the topology:

CHEW: Oh yes. I don't think that's implausible.

CAPRA: You have often said that you expect gravity, too, to emerge together with continuous space-time in the high-complexity limit.

CHEW: Yes, that's true.

CAPRA: If so, then what about gravitons?

CHEW: From the topological standpoint you don't start with gravitons; you don't have gravitons on the same footing with the elementary particles. Gravitation is expected to emerge as a manifestation of extremely high complexity, beyond the level at which space-time becomes recognizable. If gravitation has meaning only at a classical level, there may be no gravitons.

B. Extending the S-Matrix Framework

CAPRA: Looking into the future, I am curious about the necessity of either extending or abandoning the S-matrix framework.

CHEW: We have already extended it in two ways, actually. We have extended it off-shell and also by changing the basis in connection with the infrared problem.

CAPRA: It seems now that this extended framework of the S matrix is appropriate for all of particle physics.

CHEW: If it is understood in the sense of being supplemented by a topological expansion, I do think so. However, there is something else. When you say "the S matrix is the basis for understanding everything," I would object and say that the S matrix is defined in terms of momentum. But the whole notion of momentum, as we have been discussing in the last ten or fifteen minutes, promises not to be something you have to accept on an a priori basis, but will come from somewhere else. Now where does it come from? In the end, the meaning of momentum has to come from large-scale objective reality. The striking point about momentum is that in order to measure it precisely, you need large-scale apparatus. The more accurately you measure momentum, the bigger is the apparatus you are using. And

the fact that high complexity is involved is somehow essential. So there is some elusive quality to the conceptual structure.

There is another dimension which should also be mentioned, and that is cosmology. It is the aspect of the story which says that the ideas of physical phenomena and physical laws are somehow localized in time. That also has to be an approximation. Any sophisticated cosmological approach will tell you that the conditions which lead to our sense of Cartesian reality don't necessarily always exist.

CAPRA: So there is no abstract, eternal validity of the laws of nature?

CHEW: No, I think we are surely making an approximation.

CAPRA: Now, coming back to the bootstrap program, in the past you have often expressed the idea of a mosaic of interlocking models. This idea has been of tremendous value for people outside physics. I know many people who have been inspired by it. But it seems now that in particle physics it might no longer be necessary.

CHEW: Well, let me think whether I agree with that or not . . . . I think there are two considerations to bear in mind. One of them is that already at the level of the zero-entropy bootstrap one may not be able to find an exact solution. One may always have to use some kind of model. So the notion of a mosaic of models could be relevant right there at zero entropy. For example, Espinosa has been doing just that in his thesis. He is making models which are appropriate to certain portions of the zero-entropy space.

CAPRA: So what you felt would apply to the entire hadron physics applies now to the nonlinear part, which is confined to zero entropy.

CHEW: Could be. It may that we shall be condemned to always understand zero entropy in a piecemeal fashion.

The other point is that somewhere consciousness has to enter the picture. So far I have been talking about using soft photons to develop an understanding of space-time, objective reality, and the Cartesian-Newtonian view. But you know that in this domain of high complexity there is something else which, in a vague way, people describe by this term "consciousness," and which has an actual impact. The fact that I am raising my hand to gesticulate . . .

CAPRA: Of course, but there is much more to it. There is the hand itself, which you will not explain in the S-matrix framework. There is the whole world of living organisms.

CHEW: That's right. But that has to affect the accuracy of our description of the physical world. You can't have a completely accurate description of the physical world which leaves out consciousness, because it is clear that consciousness interacts with the physical world. How can I possibly have a complete description of the physical world if I don't include consciousness in the story? At the moment we work with a model that neglects consciousness; it's an approximation.

CAPRA: It's obvious that the inclusion of consciousness would give a more complete picture, but will this actually be necessary in or to make progress in our understanding of physical phenomena?

CHEW: Eventually, surely.

CAPRA: At what point do you think that we will be forced to go into this domain?

CHEW: I don't have well-developed views about at what level consciousness will be necessarily the concern of physical scientists, but I feel sure that the time has to come. It is clear that if you push your desire for complete understanding far enough, eventually you will have to bring it in.

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