

Chiral Symmetry Breaking and Light-Front QCD

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Abstract

This paper consists of an overview of the discussions on chiral symmetry breaking followed by a transcript of the discussions themselves.

1. Introduction

This article covers discussion sections on chiral symmetry breaking.

First, let us outline the essential questions which must be answered on the way to solving QCD as a few-body problem.

The first thing one needs is the canonical Hamiltonian. The canonical Hamiltonian is well defined except for those peculiar modes which have precisely zero longitudinal momentum, so-called zero modes. In the exact light-front (LF) theory these would surely give rise to a nontrivial vacuum and have a key role in both confinement and spontaneous chiral symmetry breaking. Here we wish to remove these states from the theory but argue that additional effective interactions in the Hamiltonian can restore the same physics.

There are several ways of removing these states from the theory. The simplest one is to introduce a cutoff ϵ such that all $k^+ > \epsilon$. Then one has to introduce counterterms to subtract infrared divergences. The finite parts for these counterterms would be a logical source for the necessary additional terms. Another possibility is to truncate the theory in a finite volume as in DLCQ. In this case one must face the problem of “constrained” zero modes. We prefer the first approach because we do not know how to solve the constrained mode problem.

The next question on the list regards the cutoffs used to regulate the theory. One could use cutoffs that either maintain the separation between longitudinal and transverse degrees of freedom or ones that mix them (for more detailed discussion see the contribution by R. Perry).

A fundamental feature of QCD is that it is a confining theory. Any attempt to solve for QCD bound states must implement this property, which is believed to be related to the nontrivial QCD vacuum. If we try to describe QCD bound states as few particle states, can we identify or implement a confining mechanism which does not require infinitely many wee partons?

The answer to this question is: Yes. As opposed to our earlier work [1], the confining

mechanism no longer requires an artificial potential. If one assumes that the gluon is massive, then the divergent parts of the instantaneous interaction and one gluon exchange do not cancel entirely. A mechanism for obtaining logarithmic confinement due to an incomplete cancellation has been obtained by Perry. Perry's confinement mechanism is independent of the initial cutoff used. I have oversimplified his argument in proposing a k^+ -dependent gluon mass (see my opening talk).

An exact treatment may require new effective interactions as well as the gluon mass. But a gluon mass is sufficient to gotten calculations started. It cannot be ruled out because a gluon mass can be understood as a finite part of an infinite renormalization which is needed anyway.

The assumption that the gluon is massive violates gauge invariance, but it is not unnatural. Apart from the fact that a similar result may be emerging from the zero mode analysis (see Pinsky's comment below and the contribution of A. Kalloniatis), one can get a hint from the hadronic spectrum. If gluons were massless like photons, the spectra would be continuous rather than discrete.

The main subject of these two discussion sections is contained in the question: Where does chiral symmetry breaking come from?

It is important to realize that light-front chirality is not the same as equal-time chirality. On the light front chirality coincides with helicity. It is important to remember that chirality is typically broken only by terms linear in m_q . The light-front free Hamiltonian does not violate chirality on the light front, because it involves only m_q^2 .

In the absence of zero modes, we will argue that spontaneous chiral symmetry breaking has to be realized through explicit symmetry breaking interactions. So there would be two types of explicit symmetry breaking on the light front - one corresponding to a nonzero pion mass and one reflecting spontaneous chiral symmetry breaking.

As opposed to the confining mechanism, which can be obtained as an incomplete cancellation, chiral symmetry breaking requires adding a new interaction. The incomplete cancellation causing confinement preserves the chiral symmetry of the canonical theory.

For an illustration of how complicated the issue of chiral symmetry breaking is, let us consider the canonical Hamiltonian, including the only explicitly chiral symmetry breaking term (the spin-flip part of the one-gluon exchange). If one drops the explicit breaking term,¹ entire $SU(3) \times SU(3)$ multiplets will have the same mass. Therefore, to make the pion massless and the ρ heavy without the explicit breaking, one has to add a new interaction. The precise operator form and the strength can, in principle, be determined from the requirement that in the chiral limit the chiral current is conserved. (There are however some subtleties regarding renormalization of the $-$ and \perp components of the current (see the contribution of A. Harindranath)).

To understand what the structure of the new terms might be, we reason by analogy with the

¹Strictly speaking, this is not a legitimate operation, as explained in more detail in the discussion. Linear and quadratic mass should be linked. We make this operation for the purpose of "playing" with the theory. It enables us to demonstrate clearly a fundamental reason why it is necessary to introduce new symmetry breaking interactions.

sigma model. However, there are some important differences between the symmetry breaking mechanism in the σ model versus QCD.

For a scalar σ field, the zero mode of the σ is shifted by a constant when there is a spontaneous symmetry breaking. Then this constant generates new terms when substituted back into the canonical Hamiltonian which involve the zero mode of σ . In QCD, it is $\bar{\psi}\psi$ that has a nonzero vacuum expectation value, but a constant shift for $\bar{\psi}\psi$ has no effect. However, we do already substitute a formula for the lower components, ψ^{lower} , to derive the canonical Hamiltonian. This provides an opportunity for changes to the zero mode of ψ^{lower} to occur before making the substitution. More details will be provided in the next section.

Finally, I will comment on the picture just presented. Some parts of the program described will be less convincing than others, and unexpected obstacles, which we do not foresee yet, may still arise. This is typical at the beginning of a scientific revolution in Kuhn's sense, even a modest one. You may accept the new approach now, when the revolution is just past its diaper stage, or you may wait. But, as Prof. Susskind said in this discussion: **"If you wait till he can convince you, it'll be too late."**

2. Transcript of the discussions on chiral symmetry breaking

Wilson: We have had a lot of discussions and so forth, but I am sure there are many of you who still don't have a picture of what it is that the OSU group is really doing here.

Well, that just puts you in a situation that we were in for 3 years until sometime this spring. And the first thing I want to say is that we would have never gotten through that period of confusion, in fact we would have never get started on it, except for our host here, Stan Glazek, who came to OSU and said: We had to accept the idea that the proton is 3 quarks. We could not engage in fuzzy thinking about lots of gluons, we had to think of it as of 3 quarks and we had to answer all the questions about those 3 quarks. We have to answer why they are confined and we have to answer why chiral symmetry is spontaneously broken and we can't make the excuse that there are lots of gluons and quark - antiquark pairs so we don't know what's happening. And Stan just hounded us.

There was no way we could do this except by working through all the pieces of the problem, and not just pieces A B C, but pieces A B C D E F G H I J K L M N; until we went through every problem about 3 times each that we could start putting it together and we realized that there is a sensible picture.

So all I can do is to write down what are the labels for "A B C D E F G H" so that you understand why we went crazy.

A The first thing you have to have is a **canonical Hamiltonian**. It is not that there is any real argument about what the canonical Hamiltonian is, except when it comes to the zero mode. You have that question about how **zero modes** change the canonical Hamiltonian itself.

B You need to cut it off. How do you **cut off**? Do we use a cutoff that maintains the separation between longitudinal and transverse directions or do we mix them?

C Can we get **confinement without an infinite number of wee partons**? The whole picture formerly was that you have to have an infinite number of wee partons to cause confinement, in other words, to reflect the nontrivial vacuum of normal rest frames.

D Where does **chiral symmetry breaking** come from?

E **How do you renormalize**? What is the situation when the QCD Hamiltonian has a big cutoff and how do you relate it to a qualitative picture with a small cutoff? How to ensure the physics is independent of the big cutoff?

F **What are the parameters**? You face the problem that when you set things up in the LF theory, all of a sudden there are lots of parameters that you can throw into the canonical Hamiltonian because you do not have the full Lorentz covariance to control the number of parameters. Just look at the canonical Hamiltonian: unless you can connect the mass term that appears in the free Hamiltonian to the mass terms that appear throughout the interaction, you suddenly find yourself with lots of parameters and as soon as you renormalize, it looks like those parameters disconnect from each other. So this was the problem that led to the coupling coherence idea.

Question (Van de Sande): What is a one word answer to C?

Wilson: The answer is yes, if you accept a gluon k^+ -dependent mass. That is, there is a way of constructing the gluon mass where the mass-squared is proportional to k^+ (not a constant, the way it was proposed in our paper) where you get the log potential that we are talking about as a confining potential in the second order, independent of how you do the cutoff. So the answer to this question is yes. That does not say it is physically correct, it simply says: Yes, we can get confinement out of the canonical Hamiltonian without an infinite number of wee partons.

Question (Susskind): Can you get a linear potential?

Wilson: In the second order of perturbation theory, we get a logarithmic potential. A sum to all orders might still give a linear potential. We do not know what happens beyond second order.

Question (Pauli): How do you calculate the gluon mass?

Wilson: At the ABC level it is a parameter. I agree that in the complete theory you can't have a gluon mass that is a free parameter but at this ABC level you can not calculate the gluon mass.

Perry: If you don't like to put in the gluon mass by hand, you can also do the second order calculation with cutoffs and find out what kind of masses you have to put in (i.e. as counterterms).

Question (Susskind): Could it be that these are somewhat dependent on the scheme of renormalization?

Wilson: Let me qualify this: The question C is, can we get confinement in the limit all

cutoffs removed? Thus the answer should not be cutoff dependent. It should not be scheme dependent. The only case that would forbid our k^+ dependent term would be a renormalization scheme that a) preserves longitudinal boost invariance and b) avoids counterterms with an explicit dependence on the total center of mass momentum P^+ . To my knowledge no renormalization scheme meets this requirement.

Pinsky: Let me just illustrate how I see a completely different way up to the valley. We saw in Alex's talk that the topological structure in the $\frac{1}{(k^+)^2}$ interaction had an additional piece which came from topology and looked like the mass of the gluon. So when I see something like that I say maybe the right path is to try to see the connection with the topology of instantons. It has lots of similarity with what you are saying but there is a difference between this and the self-consistent treatment (put in the mass and show that it works).

Pauli: It seems to me a lot more ad hoc to put in the gluon mass by hand.

Wilson: My statement that you can get anything you want out of the zero modes is clearly extreme and unwarranted. What I would say about the zero modes is that from everything I can see it will take you a long time to resolve the problems of the zero modes. Meanwhile I will be working with more ad hoc approaches to the theory without zero modes. But it is the only thing I can say about it.

Glazek: I understand that zero modes are not necessarily unimportant. I don't agree that we know that they will never be important. They may be important in ways we don't know yet. When I was thinking about LF QCD I wanted to include those modes. Actually, I succeeded in designing some zero mode rules that could reproduce some QCD sum rules and I was excited about it. I thought that it was the most important part of LF QCD that we should work on. But when I started to use the LF Hamiltonian that I designed to reproduce QCD sum rules I discovered difficulties and could not overcome problems that completely annihilated the possibility to use this Hamiltonian for calculating numbers for hadrons in a well defined procedure. It went to the extent that I was able to propose a very simple model of mesons and baryons by analogy with the LF QCD Hamiltonian that reproduced Karl-Isgur constituent model confining potential parameters within 10 % from the gluon condensate value. But that model had literally nothing to do with LF QCD, because when I started with LF QCD it was impossible to do anything unless I knew how to renormalize. Before we'll be able to answer questions concerning the vacuum, zero modes and things like that, at least to me it became clear that first we have to learn how to do renormalization.

Wilson: Let me say a few words about the zero modes. I apologize for the attitude I take toward zero modes which is sometimes hard for me to control. But I perfectly agree with what Stan says, that very important things may come out of the zero modes analysis, things to which we may not get to any other way. But at the present time, the only way I have to get through this cycle of all the ABCDEF you have to understand to make the picture, the zero modes are not part of that cycle and I am still optimistic that we can make progress whether or not the insights of the zero modes that we need are forthcoming.

Question (Susskind): I want to ask my question again, because I am not convinced that I believe your answer. This was the question about the scheme dependence in the effective potential. You are finding these effects, that is chiral symmetry breaking and confinement, due to an incomplete cancellation of contact terms at very low momentum expansions. Is that right?

Wilson: No, the chiral symmetry breaking is not an incomplete cancellation. The chiral symmetry breaking term will be bringing in a new concept, a new structure into the canonical theory, that I will claim could come from the zero modes. It will be a new structure which you can't create by any incomplete cancellation, because any kind of cancellation would preserve the chiral symmetry of the canonical theory and I have to put in something that breaks the chiral symmetry. I am saying: to get the confinement we didn't have to put in anything, it came from an incomplete cancellation of what was already there, but for chiral symmetry we have to put in something.

Question (Susskind): ... and have an incomplete cancellation?

Wilson: No. It's just the question of putting in something.

CHIRAL SYMMETRY BREAKING.

Wilson: I want to do a bit of set up on chiral symmetry. I want to show how desperate we became before I show you the finesse. I want to start with the canonical light front Hamiltonian, no zero modes, and I am not going to write out everything, I'll just remind you that there is a free part where the quark energy is

$$\frac{p_{\perp}^2 + m_q^2}{p^+} \quad (1)$$

and an analogous gluon term. Then there are the quark-gluon interactions, one term of which is the explicit chiral symmetry breaking term of the form

$$gm_q \bar{\psi} \sigma_{\perp} \cdot A_{\perp} \psi, \quad (2)$$

where A_{\perp} represents the gluon and σ_{\perp} are the Pauli matrices in transverse light-front coordinates. Color and flavor indices have been suppressed. Among all other terms we will be interested only in the instantaneous quark exchange term - a two quark-two gluon interaction. For example, in this term a fermion and a gluon come in and a fermion and a gluon come out, what you diagrammatically represent as a quark exchange even though it's not an actual particle exchange diagram, just a term in the canonical Hamiltonian.

If $g = 0$, so that all that we have are the free terms, and no zero modes, I remind you that what we have at this stage is an exact $SU(6)$ symmetry of all the 3 flavors. That is assuming that the 3 quarks have the same nonzero mass. Of course the two spins (the two helicities of the quarks) have the same mass. That just simply means that you have a trivial $SU(6)$ symmetry.

In the presence of the interaction, $SU(3) \times SU(3)$ symmetry is maintained if term (2) is dropped. Term (2) is the only term that can break explicit $SU(3) \times SU(3)$, so if I turn on the interaction but not this term, I continue to have full $SU(3) \times SU(3)$ multiplets. (The larger $SU(6)$ symmetry is now broken by terms with $(\sigma_{\perp} \cdot A_{\perp})^2$ type chiral structures.) Of course funny things can happen to the masses due to these interactions but funny things cannot happen within a given $SU(3) \times SU(3)$ multiplet. The $SU(3) \times SU(3)$ multiplets have to always maintain the same mass through out all members of the multiplet. So if I send the π mass to zero, without the term (2) the entire multiplet goes to zero. This does not make any sense. To resolve the situation I need two things. First I want to decouple (1) from (2). We consider what happens if g is nonzero but the mass m_q in (2) is a separate parameter m'_q from the free quark mass m_q . What I have in mind is that the free mass should be a constituent mass ~ 300 MeV, while m'_q would be much smaller or zero, like a current mass. In the true relativistic theory the values of m_q and m'_q would be linked, but for now I will ignore the linkage.

Secondly, we still need an extra term in the Hamiltonian as a result of subtraction or infrared behavior or something else, which does the job of supplying another explicit $SU(3) \times SU(3)$ symmetry breaking which involves constituent masses as coefficients rather than current masses and therefore provides a large separation of the π and ρ masses. This term should arise as a result of eliminating zero modes from the theory and should maintain local chiral current conservation - the true signal of spontaneous breaking only.

Perry: When you throw away the zero modes and look at how the parameters renormalize, you will see that the quadratic mass has an infrared divergence. The linear mass has no such divergence. So they do renormalize quite differently.

Question (Susskind): Does it have to do with anything else than you just have to renormalize everything separately unless they are linked by a symmetry?

Wilson: Right, but it's more complicated because you're getting to the coupling coherence which Bob (i.e. Perry) has mentioned but not really talked about.

Question (Burkardt): This mass (i.e. term (2)) is determined by the pion mass scale. To get the ρ heavy means you have to add another term, right?

Wilson: Yes. That's the whole point that I am saying. We are missing something that would allow this (i.e. the linear mass in term (2)) to stay at the pion mass scale, this thing (i.e. quadratic mass in term (1)) to be at ρ mass scale and yet still not have the $SU(3) \times SU(3)$ near symmetry driving everything down to zero.

Question (Berera): Ultimately, there should be one m_q .

Wilson: The statement that there should be one m_q comes back to what Bob Perry has worked out called coupling coherence. When you look at the renormalization process to higher than to second order, (you have to go at minimum to third order) you find out that all the appearances of m_q are linked exactly the way that you would like.²

²For more detailed explanation of coupling coherence see R. Perry's paper in this volume.

Question (Susskind): But they don't come up the same way, do they?

Wilson: They don't come up the same, but they are linked.

Question (Berera): In your language, does it basically mean that there is one direction irrelevant? If you start with two dimensional space and there is only one parameter at the end ...

Wilson: No, no. It's not a question of relevant versus irrelevant. It is a question of preservation of the full Lorentz symmetry of the theory. The preservation of the full Lorentz invariance translates into something called coupling coherence, that forces you to set up linkages between all the parameters. If you break the linkage by letting m'_q be arbitrary all that happens is that Lorentz invariance is lost.

Question (Berera): So it has nothing to do with the renormalization flow?

Wilson: It has to do with the renormalization flow but the criteria that we put on say that the renormalization flow should be describable in terms of one coupling constant rather than an infinite number of coupling constants all doing their own thing.

Question (Susskind): Is it anything like what would happen in the Hamiltonian lattice gauge theory when you allow different coefficients in front of spatial and time derivatives?

Wilson: That's right.

Question (Berera): I am glad you just made the distinction because when I think about the relevance and irrelevance I think about it from a dynamical point of view. Are you telling me that I am going to get one coupling, I mean one parameter, from the kinematic structure?

Wilson: It's not a kinematic structure. Lorentz covariance is not explicit in this theory, so it is a very subtle dynamical issue to keep those parameters linked. You have to look at the details of renormalization to see what coupling coherence means.

Question (Berera): How does it relate to your old picture of relevant and irrelevant operators?

Wilson: It is not a question of relevance versus irrelevance. Both operators are relevant - the free mass term and the chiral symmetry breaking interaction. There is however a unique linear combination which preserves Lorentz covariance. Any other combination is still relevant, but would destroy Lorentz covariance.

Similarly, there are many marginal operators independent of the mass in the canonical Hamiltonian. But again only one unique linear combination preserves Lorentz covariance. All other combinations remain marginal but violate Lorentz covariance.

AFTER THE LUNCH the discussion continued addressing the question how one can find new chiral symmetry breaking terms.

Wilson: What I am going to do is I'll start with the σ model but I am not going to do any development. I'll put the chiral symmetry breaking in the σ model into one equation and then use that as a starting point for discussion.

Question (Yan): Are we talking about explicit or spontaneous symmetry breaking?

Wilson: What I need is an explicit symmetry breaking term, but it is supposed to be representing the spontaneous. There are two different explicit symmetry breaking terms in this formalism.

I remind you, the way we handle the σ model³ is that we translate the sigma field, but I just want to write that translation in a particular way. I want to take a term in the Hamiltonian, say,

$$\int d^3x \phi^2(x) \sigma^2(x) \quad (3)$$

before translating. And before translating I am going to rewrite this in momentum space in terms of the Fourier transforms:

$$\int d^3k \int d^3k_1 \int d^3k_2 \phi_k \phi_{k_1} \sigma_{k_2} \sigma_{-k-k_1-k_2}, \quad (4)$$

where k , k_1 and k_2 are light cone three vectors.

And now I am going to look at the effect of translation on this term, an absolutely trivial exercise except that I am going to do it in momentum space. I am going to take the σ_{k_2} and look at a translation in momentum space form:

$$\sigma_{k_2} \rightarrow \sigma'_{k_2} + \delta^3(k_2) \cdot C \quad (5)$$

where C is a constant which corresponds to the vacuum expectation of σ . In other words, when you have the condensate (to use the word that everybody likes to use here) and you have to look at it in the momentum space, that corresponds to a delta function added to the field σ .

In a diagram context, if I have a four point vertex where two of the fields are ϕ and two of the fields are σ , what this gains is a term that has three external fields and a fourth leg which has an x on it (i.e. an external field contracted with the vacuum) is the fourth leg pinned at $k = 0$. That is all I want to say about the σ model.

The idea here is that any time you have a σ line, you can replace it with a constant C and pin its momentum to zero. We have no a priori information of what the constant C is and we don't even know whether we should put the same constant in two locations.

Susskind: In particular, it does not have to be a classical constant.

Question (Namysłowski): But when you pin the leg to $k = 0$, isn't it the zero mode?

Wilson: Yes, the pinning is obviously connected to a zero mode, a mode with all components of k vector equal to zero, so the only way you can properly justify this substitution is through the zero mode theory.

Question (Susskind): Is the rule of the game: We don't know what the value of the coefficients is, but we adjust it so that the pion is massless?

Wilson: Yes, but this adjustment must do more than give a zero pion mass. What you want is to ensure a locally conserved chiral current when the pion mass is zero. By insisting that you

³the linear sigma model

have conserved current you can derive the standard shifted Hamiltonian of the σ model without ever explicitly invoking information about zero modes.⁴ Detailed analysis can be found in the appendix of our paper [1].

Wilson: Now turn to QCD. What we are demanding is a nonzero expectation value for $\bar{\psi}\psi$ instead of an elementary field. So you go around diagrams and say: Is there any way that I can arrange to have a constant vacuum expectation value for $\bar{\psi}\psi$ change the canonical Hamiltonian? And as you can well imagine, we could not produce a changed Hamiltonian. There was no way that you could shift the $\bar{\psi}\psi$ and have it mean anything in the context of the canonical QCD Hamiltonian. So now we come to the stretch.

Question (Burkardt): Couldn't you contract through one of those instantaneous fermion lines?

Wilson: You have found the right place. But the way I want to motivate this is I want to look back at how the instantaneous fermion term arises. I haven't taken you through the canonical theory, but in any case, you had in Harindranath's talk this morning the relevant equation, the equation which solves for the lower components of the ψ field.

The instantaneous quark exchange term originates with an ordinary three point vertex, but with one quark leg referring to a lower component. But the lower component is not an independent degree of freedom and it has to be eliminated. It gets replaced by the following structure - somewhat simplified:

$$\psi_q^{lower} = \frac{1}{q^+} \int_p \sigma_{\perp} \cdot A_{\perp p} \psi_{q-p}^{upper} + \frac{1}{q^+} m \psi_q^{upper}. \quad (6)$$

Now I want to add a term to this structure reflecting the effect of the zero modes, because the picture is that the whole physics of chiral symmetry breaking has to come from zero modes. That is the only place where you can have a nontrivial vacuum.

I have to tell you that this is different from the presentation in our paper. I worked out this presentation just for this conference.

People can make all sorts of pictures of how the zero modes might affect the chiral symmetry. But I try to copy what was done in the σ model. So I want to add something that is associated with momentum zero. Let me show you an example of what I add in:⁵

$$+ \delta(q^+) \int_{q'_{\perp}} \vec{f}_{\perp}(q_{\perp} - q'_{\perp}) \cdot \int A_{\perp p} \psi_{q'-p}^{upper} d^3 p' \quad (7)$$

where q' is understood to differ from q only in its transverse components. Now, notice that I do not force the momenta q_{\perp} to be zero. Furthermore, when you are taking an expectation value

⁴There are some subtleties regarding the $-$ and \perp components of the current.

⁵This is a slight modification of the expression Dr. Wilson wrote down first, which was done later in the discussion thanks to a comment by Matthias Burkardt.

of $\bar{\psi}\psi$ you don't demand that both fields have zero momentum, only that the product has zero momentum. Hence my rule is that the A_{\perp} and ψ can separately be nonzero modes, as long as the product lives in the zero mode sector. The form factor \vec{f}_{\perp} reflects the properties of the vacuum. The vacuum knows about the masses, so \vec{f}_{\perp} can have very complex mass dependence, rather than a structure dictated by power counting in the absence of mass dependence.

I am not going to justify the precise term I wrote here. I am saying it is reasonable to consider. You may consider some other terms.

Question (Soldati): But you can have only $q^+ \neq 0$ in an exchange.

Wilson: No, in the instantaneous diagram q^+ can be anything. It just depends on whether the incoming momentum of the fermion is bigger, smaller or equal to the momentum of the outgoing fermion. And all three are possible. But what I am going to say is that for the added term to be associated with the zero modes, I have to put in a delta function that forces q^+ to be zero.

Question (Namysłowski): But at $q^+ = 0$, wouldn't that blow up? (the first two terms.)

Wilson: No, the first two terms remain functions of q^+ , with q^+ not forced to be zero. However, if quarks and gluons actually existed as asymptotic particles, there is no way that I could have the delta function. That would generate a physical nonsensical quark-gluon scattering amplitude. What that says is that the kind of things I am doing you can only do in a confined theory. Because in the confined theory you can never have quarks or gluons as free particles which means they will only appear as constituents in a wave function. That's the only way they will have a physical meaning.

Question (Namysłowski): But even being confined the instantaneous momentum can be equal to zero.

Wilson: That's right.

Namysłowski: Oh, they will be hidden in a hadron.

Wilson: Yes, every one of the quark momenta will be hidden in a hadronic wave function so there is always an integration over these momenta to compute physical amplitudes. But there is nothing to stop the momentum of an incoming fermion to be equal to the momentum of the outgoing fermion as part of the integration. And of course, since there is an integration, there is no problem with the delta function.

Question (Burkardt): Am I right in assuming that after inserting this extra term into the Hamiltonian all external legs are at nonzero p^+ ?

Wilson: Yes, you still keep the infrared ϵ cutoff on the external legs. How I handle any cutoff in the instantaneous exchange is not necessarily linked to what I do on the external legs.

Question (Wyler): Is the mass in eqn. (6) a constituent or current mass?

Wilson: The mass in eqn. (6) starts out as a current mass in the free field limit, but it will renormalize and it may do all sorts of crazy things when you renormalize.

Question (Namysłowski): But it cannot reappear as a constituent mass, can it?

Wilson: I will leave it as an open question because I don't know whether this will still be forced to be a current mass. The trouble is you have to have a formula for the lower component of ψ because in the relativistic field theory you have to know what the lower component of a field is.

However, I think you are correct. From the point of view of behavior of the field, since this is linear in mass, for this to be anything other than the current mass would screw up the chiral behavior of the field. But what you have to be careful about is that you must not take this expression and substitute it back into the Hamiltonian. Once you renormalize, the Hamiltonian itself has broken its connection to the parameters in this equation.

Let me tell you what the trouble we are having is. The second term in (6) will do two things for the light-front canonical Hamiltonian: it will give you the m^2 term of the free Hamiltonian and it will give you the linear m term in the interaction Hamiltonian. The same substitution will give you one term that has to produce a constituent mass and another term that has to produce the current mass and I don't know whether you can handle it except by breaking the connection between eqn. (6) and the renormalized Hamiltonian. You still have to have an eqn. for the lower component because you cannot do Lorentz covariant field theory without it and you must have a Hamiltonian but they may not be connected any more. I want to leave it as an open question.

An easy way to say it: you write down the canonical Hamiltonian, write down the equation for the lower component, but the renormalization happens separately for both. On the other hand, I use the idea that these terms can appear here in the equation for the lower component of the ψ field to justify putting in the kind of terms that would have been generated if I could make the substitution.

Susskind: Do you have to maintain the conservation of color except when the coupling equals the physical coupling?

Wilson: Susskind raises the right question: do you conserve the color locally? Remember, we always conserve color globally, that's trivial. But do you have local conservation of color at any value of the coupling constant other than the one given by the asymptotic freedom?

Susskind: The issue is subtle because you are not separating the tricks and artifacts of the computation and the real physical things that are expected to happen at the physical point.

Wilson: Let's go back to what I said at the start about building the bridges: about linking the qualitative physical picture of the constituent quark model to QCD through the bridge. To generate the qualitative picture I am going to start with a weak g at the hadron mass scale. In order to have something that I can work with I am going to consider a coupling of order e^2 . I am going to make g^2 just as small as e^2 , so I can build the qualitative picture. The bridge is the range from $g^2 = e^2$ to $g^2 = g_S^2$, where g_S is the asymptotically free value for relativistic QCD, measured at the hadron mass scale.

To build the picture at weak coupling there are some qualitative things that I want to get. I want to get confinement. I would not tolerate a weak coupling picture based on the Coulomb

force. I've just given you the argument why I want an explicit chiral symmetry breaking term. I will not tolerate a situation when the $SU(3) \times SU(3)$ multiplets do not split as I turn on the coupling. I want to have these two things. But I am willing to sacrifice almost everything else in order to get those two things.

So, if somebody tells me I don't have locally conserved color current, I say: Fine. I don't care. Because that will have to come back again as we cross the bridge to relativistic QCD and then it should be perfectly satisfied.

Question (Susskind): It's just that the physical pictures that you are making may be to some extent an artifact of the method of computation, which continuously changes over to the physical picture when the coupling approaches the physical value.

Wilson: That's correct.

Susskind: One has to be aware of that.

Wilson: That's right. Remember, if you are living in a house and if you are freezing to death ...

Susskind: I am not criticizing you!

Wilson: I am just giving you a way to think about it. Think about it like this: You are living in a house and you are freezing to death, and you realize you want to save the first floor. So you take your ax to the second and third floor, put them in the fireplace, so that you get yourself warm. I took the ax to the second and third stories of the QCD House so that I could preserve some warmth for the first floor.

It was so devastating to try to do QCD with the Coulomb force and no chiral symmetry breaking ...

Susskind: I am completely with you!

Wilson: Right. Well, you have to show to me that it is just as bad to break the local conservation of the color current as it was to have no confinement and no chiral symmetry breaking before I even worry about your problem.

Susskind: What problem?

Wilson: Lack of local conservation of the color current.

Susskind: It's not my problem.

Wilson: Ok, fine, whoever's it was.

Question (Berera): You start with a bare Hamiltonian which is defined at some large cutoff. And you want to get to an effective theory which has some lower cutoff. Now you let the cutoff go to infinity and you solved the ultraviolet problem. And you hold the cutoff fixed and you hold the parameters at your cutoff at some typical value.

My question is: because of including these terms do you have any feeling whether you are going to change the ultraviolet behavior of the theory? Am I going to flow to some fixed point that is not physical? There might be technical problems with all the operators.

Wilson: What I am expecting is that since the chiral breaking operator reflects a vacuum effect, not an ultraviolet effect, it will have a soft form factor \vec{f}_\perp . It will be something that drops

out very fast, when the q^\perp becomes large, and because it does not affect large q^\perp it will not affect the ultraviolet renormalization at all. Because it is a nonlocal term, I put in a term that will directly impact the low q^2 physics without affecting the ultraviolet renormalization. Now, I may be wrong on that in a sense that when we try to do the calculations we may find that this f (i.e. the form factor \vec{f}_\perp) tries to persist at large q and then I'll have a more complicated ultraviolet renormalization, but at the present time I am very optimistic and I say it does not affect it.

Question (Susskind): I want to see the strategy for going to the physical point. And what kind of physics you expect.

Wilson: Let me give you the strategy that I personally have in mind. It's one of the things we are implementing in the group, not just me at this point. The idea is if you put g^2 of order e^2 and then you calculate the way you calculate for QED, you have nonrelativistic bound states just as in QED. The only difference between this and QED is that we have a confining potential on top of the Coulomb potential, whereas in QED you had only the Coulomb potential. And of course in this case you have a nonrelativistic bound state structure for gluons. So gluonium exists in a whole sequence of nonrelativistic bound states just as quarkonium does. And it's a very orderly picture. You crank it out at order g^2 just the way you do in QED. Then we borrow all the apparatus for doing radiative corrections to QED bound states. (And you know that the record is something like 8th order.) So what we would do is we would calculate bound state properties as a power series in g and then we're going to cook the data helped by the usual round-up of power series extension methods in order to extrapolate the power series result to the relativistic value g_S^2 .

Question (Yan): What about the rotational symmetry?

Wilson: The full rotational symmetry in x, y, z can be expected only at the point $g = g_S$. So one of the tests of whether things work is: when you calculate everything in the Particle Data Table, do all those spin structures of states come into line so that every member of a spin multiplet has the same mass? If you can really do what the papers of Nathan Isgur have done where you have page after page of the tables and it all works based on a consistent framework for extrapolating to g_S^2 then I will be happy.

We have lots of fudge factors here. Every form factor in the Hamiltonian will become a fudge factor. It's going to be an interesting discussion when we come back and we say we are getting the particle data table and you say: You just fudged it. It's going to be a very interesting debate.

As for renormalization problems, we expect to learn how to get rid of all the cutoff dependence, order by order. All cutoff dependence will be removed. You will then not be able to challenge us on the cutoff dependence. You will only be able to challenge us on the finite fudge factors, not the infinite subtractions themselves.

Susskind: You have a computational method, I have a physical picture of what happens. I would like to go on record with what I'm guessing, or speculating on the connection of the two

of them and what you'll find when you go to physical value of the coupling. Can I do that?

I will try to express it in terms of a picture, no equations. We have the rapidity x or η axis. As far as I can tell your picture is something like this: There is the zero mode down here, and a few modes very close to $x = 0$ and we would like to integrate them out. You put the coupling constant small and you integrate them out, but you want to get the chiral symmetry breaking. So the only way to do this is to put some symmetry breaking associated with this mode. The interaction feeds itself right back up to the high momentum particle going up here. So it is a direct interaction between what is at $x = 0$ and everywhere else on the rapidity axis.

That's alien to my picture. In my picture the way it works is that the symmetry breaking is fed up through a chain of degrees of freedom.

But, the coupling of your symmetry breaking interaction (let's call it $C(g)$) is a function of g . Now, there is some dynamics in the system that as the coupling gets large, the system will simply be able to support its own chiral symmetry breaking without the direct interaction (that you put in). In other words, the coupling $C(g)$ will go to zero at the point where g goes to g_{AS} .

That's my guess.

Wilson: Let me give my version of what happens.

I am going to expand $C(g)$ in powers of g . I expect it to look like this:

$$C(g) = g^2 c + g^4 \log\left(\frac{p^+}{\mathcal{P}}\right) + g^6 \log^2\left(\frac{p^+}{\mathcal{P}}\right)$$

where \mathcal{P} is whatever you choose as a reference momentum. You can't have the logarithm of p^+ without dividing it by something.

At the 4th order my expectation is that we start getting logarithms. Then what we'll be arguing about is what happens to the sum of these logarithms. Before we have the logarithms, couplings are completely uniform all along the rapidity line. But as soon as you get logarithms, you will start to see localization in Susskind's sense. Then the question will be, what happens as $g \rightarrow g_S$?

And now I say no more.

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