(PARTIAL) SUMMARY OF SECTION A:
CONFINEMENT MECHANISM, FLUX TUBES AND STRINGS

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ABSTRACT

An overview of some issues discussed is given, including the following ones: (i) the mechanism of confinement, (ii) the structure of the string, (iii) non-perturbative glue in vacuum and hadrons, and (v) quark-hadron duality. In doing so I try to indicate issues important for JLab. Finally I discuss another issue debated during this conference, namely (vi) whether one can use some common effective potential for description of various hadrons, using heavy quarkonia as an example.

1. Overview

My first remark is about a general situation in non-perturbative QCD/ hadronic physics. It is striking that it has several sub-communities, with rather weak interaction between them. Roughly speaking, there are three different schools which think about those issues in completely different terms.

(i) The first one (naturally, the dominant one at this conference), concentrate on confinement phenomenon and strings. Hadrons are viewed basically as bare quarks connected by strings, and hybrids are string excitations. The vacuum is “empty” of any non-perturbative glue and the glueballs are closed strings. The stronghold of this point of view is physics of charmonium/bottonium and Regge trajectories. There are of course extended claims for all other hadrons as well. (These proceedings is probably the best possible review of development in these directions.)

(ii) The second one (to which I belong) concentrate at hadrons made of light quark. The basic phenomenon underlying it is breaking of chiral symmetry, both spontaneous ($SU(N_f)_A$) and explicit by the anomaly ($U(1)_A$). This school view light quarks as collective excitations propagating on top of the quark condensate $<\bar{q}q>$, and thus obtaining “constituent quark” mass, being the main part of masses of all light hadrons. The residual interaction binds those into hadrons, in some cases even without a confining potential (which is treated as small effect).

Originally this school was using Nambu-Jona-Lasinio-type models. Recent QCD-based development views the vacuum (as well as glue inside glueballs and hadrons) as a superposition of instantons, which naturally provide the (non-local) interaction
between quarks needed for chiral breaking. The obvious stronghold of this ideology is chiral dynamics of pions and its relatives. There is also a claim that all other lowest mass hadrons made of light quarks (and heavy-light ones) are explained by it. For recent review I propose that by T.Schaefer and myself.

(iii) The third school tries to re-sum perturbative diagrams using well known QED tools like Schwinger-Dyson or Bethe-Salpeter equations. They hope that when the coupling constant is not too small, both chiral symmetry breaking and confinement will appear, e.g. from modified propagators. An example is the $G \sim 1/q^4$ behavior of the gluon propagator at small q. Similarly, the one-gluon exchange, if strong enough, may also lead to chiral symmetry breaking, quark constituent masses etc.

The first point I want to make is that these three pictures of hadrons are indeed so different, that they cannot be simultaneously right. And available lattice/phenomenological data should be used to pick the right one. One example (which was several times mentioned at this conference): although the $G \sim 1/q^4$ gluon propagator may provide confinement, it definitely does not correspond to a string-like configuration of the field. Thus it is already ruled out by lattice studies, which do see a rather narrow string between static charges. (Other similar examples are to follow below.)

My next general point provides an argument against the perturbative views (iii). In fact we do not have one QCD vacuum / hadronic spectrum but many, numbered by the famous $\theta$ angle related with the phase of the instanton amplitude. For example, at $\theta = 0$ (the physical case) and $\theta = \pi$ there is no CP violation, but the instanton-induced amplitude change sign. Strong arguments exist showing that there should be drastic differences between these vacua, and as $\theta$ changes different vacua cross each other and cause some phase transitions.

The perturbation theory (no matter how resummed, with or without renormalons) simply does not know anything about the $\theta$ angle! What it means is that there cannot be any unique way of defining QCD starting from perturbative series, unless it somehow includes this parameter.

The same question applies to “abelian projected” version of QCD (much advocated at this conference). It is not a trivial question whether it may or may not generate the topological charge, to know about the $\theta$ angle as well.

Of course this is not a new argument. Still it is worth repeating that $\theta$ dependence is not an academic issue. In fact dependence is very strong. For example, in gauge theory without quarks one can see directly from the lattice data on topological susceptibility that the energy difference between the two vacua mentioned above, $\theta = 0, \pi$ is about a 1 GeV/fm$^3$, larger than energy densities associated with chiral breaking or confinement in all models. And it would be difficult to believe that any model which completely misses such large effects can be successful for description of smaller ones.

2. The Physical Mechanism of Confinement

Impressive lattice studies reported (see especially talks by Bali and Michael) has strengthened the case made by earlier works on the field distributions in static configurations. Properties of glue excitations with different orbital momenta they found leave little doubt that a (rather thin) string is indeed formed in such configurations. This by itself rules out some models of confinement. I already mentioned the
model with the gluon propagator $G \sim 1/q^4$. Another one which I think belongs to the same class is a suggestion that confinement is due to the tail of the instanton distribution over sizes $dn/d\rho$ at large $\rho$: I will return to it at the end of this section.

*What is the internal structure of a string?* It seems that lattice data are consistent with the dual superconductor picture, see talks by Bali, Polikarpov, Ichie and Suganuma. In addition, as M.Baker told us, the dual picture reproduces the spin-dependent potentials well.

The interesting discussion here was initiated by the observation that abelian projected field shows string radius to be only about .15 fm, while the complete SU(2) fields indicate about twice larger radius. The point (emphasized by Suganuma, but also by others) is that the non-abelian fields have to be located outside the abelian one, since the monopole current should wind around abelian field, and the monopoles are made of it.

This convincing argument however seem somewhat to contradict another set of ideas, presented here by Kondo and Suganuma. They proposed that the non-abelian fields can be to first approximation ignored, and then taken into account perturbatively. The reason for that, they argued, is because its phase is a simple “random variable”. However it seems to me that this viewpoint cannot be quite correct, since it would destroy the monopoles and eventually the outer region of the string.

*How the string interact?* Recall that attraction means dual superconductor of the 1-st kind, and repulsion means the 2-nd. Bali has shown that there are clear signals of weak attraction. C.Michael has also studied the issue, and his conclusion is the octet string tension is nearly exactly twice the triplet one thus he sees no significant interaction. It also suggests that QCD strings are close to the border line.

*The theoretical status of the dual superconductor picture* is however still controversial. Its proponents say that recent Seiberg-Witten solution of N=2 Supersymmetric QCD has provided support to their point of view. Well, not quite. In this theory the Higgs VEV does break the gauge group, and both monopoles and dual photons are indeed real excitations of the theory. In QCD we definitely have no “dual gluons” or Higgses with masses $\sim 1\text{GeV}$ The fact that those do appear in the abelian dual Landau-Ginzburg Lagrangian basically invalidate it, no matter how good is its description of the string. It is not the right effective theory of QCD at low energies: we have to look for another one, without the color group being broken.

*Is confinement generated by monopoles?* It was repeatedly mentioned that several abelian projections were shown to preserve the full string tension. Another strong contender (discussed here by Polykarpov) is further reduction of the abelian fields to only $Z_N$ parts of them. As shown it also reproduces the string tension, reviving an old viewpoint that vortices rather then monopoles (or the 2-d objects rather than the 3-d ones) are responsible for confinement. A debate which of the two are more physical objects is still going on, and extensive lattice work is needed to resolve the issue.

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*The famous “Casimir scaling” for the non-triplet strings was debated in literature for ages: it seem not to work. I never understood what it is based on anyway.*

*The data for shown by Suganuma indicate that the abelian part has the propagator which is consistent with zero mass. Has it any specific physics consequences?*
mechanisms which (I think) do not work. The first is based on the correct observations that (i) an abelian projection of an instanton produces a monopole loop (of size \( \rho \)) and (ii) in a sufficiently dense ensemble of instantons those loops merge into a long loops, in a manner similar to a percolation transition. Nevertheless, the potential \( V(r) \) (extracted by calculating the Wilson loops with the ensemble of instantons) show smooth (and approximately linear) dependence on the instanton density, ignoring the percolation point.

The second is the suggestion that large-\( \rho \) tail of the instanton distribution may be responsible for confinement. Indeed, if \( dn/d\rho \sim \rho^{-3} \) one can get it. However, if the origin of confinement are kind of large domains of weak field, we would not get thin strings. Furthermore, if it is due to some tail of the distribution, confinement should be “intermittent” on configuration-by-configuration basis: many lattices should have no confinement and some have very strong one. It is completely contrary to lattice observations, in which confinement fluctuates little.

With a tail \( dn/d\rho \sim \rho^{-5} \) which fits well (still rough) lattice data, one gets approximately linear potential \( V(r) = Kr \) up to \( r \sim 3 \) fm. There was a claim by Suganuma et al.\(^4\) that the tension \( K \) has the right magnitude, but it must be mistaken since our calculation as well as by others\(^5,7\) get in this case the tension \( K \sim 100 - 200 \) MeV/fm, few times less than the empirical/lattice value \( K \sim 1 \) GeV/fm.

### 3. The Non-perturbative Glue

#### 3.1. How the Non-perturbative Glue in Vacuum actually look like?

Some people reject this question\(^\ddagger\), but I think it remains to be the most important issue, a key to the whole hadronic physics. Let me make the question more specific, emphasizing dimensionality of the objects: *Is it mostly made of (i) closed string (2-d membranes), (ii) instantons (4-d objects), (iii) monopoles (3-d objects) or (iv) vortices (2-d objects)?*

Obviously our lattice friends can answer this question simply by making lattice fields smooth (in order to get rid of “quantum noise” of perturbative nature) and then see what they look like. What they see are 4-d “bumps”. Many of them have nonzero topological charge and are identified as instantons or anti-instantons. Some have zero topological charge and smaller action, they look like close instanton-anti-instanton pairs (and disappears under further “cooling”, action minimization). Something of the order of 90 percent of the action is saturated by those objects.

For those who think that a “smoothening” of gauge fields is a questionable procedure, there is another way of looking at the problem, this time from the point of view of light quarks. One can look how the wave function of the quark condensate (which all approaches mentioned above claim to generate). In order to do so, one should take \( |\psi_\lambda(x)|^2 \) for the lowest eigenmodes \( \lambda \) of the Dirac operator and study how the corresponding eigenfunction is distributed in space. What one sees on the lattice are again isolated bumps correlated to instantons.

I never heard that anybody was able to identify 3-d (or 2-d) structures either in action distribution or in quark condensate: for me it put large question mark for\(^\ddagger\) Paton commented for example that in his model the vacuum has no non-perturbative glue at all.
claims that monopoles (vortices) are physical. The “instanton dominance” is clearly established, as far as gauge field action of chiral symmetry breaking is concerned. However it is not so if we for confinement (see the previous section).

In fact, a “slightly smoothened” vacuum preserves confinement with the original string tension (see e.g. 3), and further studies should be able to find out what it is due to.

3.2. Glue in Glueballs and Hybrids

At this conference this subject was discussed from a string picture point of view. It does not work for recently claimed hybrids, as those turned out to be lighter than expected. Let me present an argument that it cannot be true at least for the scalar glueball as well, based on its size and shape.

The size of the scalar glueball is surprisingly small. This was first seen on the lattice from the magnitude of finite size effects 9 and then confirmed directly from measurements of the wave functions 10,11. The size of the scalar glueball (defined through the exponential decay of the wave function) is \( r_{0^+} \approx 0.2 \text{ fm} \), while \( r_{2^+} \approx 0.8 \text{ fm} \). It indicates that spin-dependent forces between gluons are much stronger than between quarks, at least in the scalar channel. Instanton-based calculation done by T.Schaefer and myself 12 have obtained both the mass and size values of the scalar glueball in agreement with those obtained on the lattice. The parameter enhancing gluon interaction with instantons is nothing else but large classical amplitude of the instanton field at the center \( G^2(0) = 192/(g^2\rho^4) \) (basically, a parameter of the same nature as the one enhancing induced photon emission inside lasers).

The wave function of the scalar glueball (defined by a split-point correlator) has a strong peak at the origin. This is in direct contradiction with the closed string picture, suggesting a hole there.

Closing this subsection, let me say that all these arguments do not of course invalidate estimates based on the string picture: it is possible (and even quite probable) that resonant excitations of many different types can coexist, as they do in countless examples from condense matter/nuclear physics.

3.3. How to look for Glue inside Ordinary Hadrons?

One method, is to extract the next twist matrix elements for nucleons from power corrections to deep inelastic scattering. The theory of these corrections based on general Operator Product Expansion (OPE) was worked out in early 80’s 13. As emphasized in these works, while the leading twist operators are related to probabilities to find quarks or gluons with certain momenta inside the nucleon, the next twist ones describe correlations between partons. In particularly, mixed quark-gluon operators of the structure \(<N|\bar{q}...G_{\mu\nu}...q|N>\) tell us what is the gluon field at the same (scattering) point where the quark is. Needless to say, it would be very important to have such information, and JLab experiments are well suited to fill this gap.

Meanwhile, one can estimate such matrix elements from various models of the nucleon. Such estimates based on the instanton liquid model 14 were discussed here by

\[ \text{For comparison, a similar measurement for the } \pi \text{ and } \rho \text{ mesons gives } 0.32 \text{ fm and } 0.45 \text{ fm.} \]
M. Polyakov. His results show that in fact these matrix elements are smaller than a naive estimates based on the magnitude of the fields inside the instantons: estimates have some small factors like diluteness of the instanton vacuum. The reason for that is that the quark operators which appear as corrections to DIS all are vector or axial vectors, so they are not chirality changing and cannot thus be represented by instanton zero modes. It is a part of a general tendency of the instanton effects to be “hidden” in vector and axial channels.

Much larger gluonic effects are expected in scalar/pseudoscalar channels or isosinglet axial channel related with the anomaly. Very intriguing tool in this respect is virtual/real charm annihilation processes. Let me conclude this section with three examples of this kind.

(i) The process $\eta_c \to \bar{N}N$ is forbidden by chirality conservation rules of perturbation theory: nevertheless it is observed and has a surprisingly large branching. Its relation to instantons was discussed in.\[15\]

(ii) Recently, CLEO collaboration has reported measurements of inclusive and exclusive production of the $\eta'$ in B-decays:

$$Br(B \to \eta' + X; 2.2 \text{ GeV} < E_{\eta'} < 2.7 \text{ GeV}) = (7.5 \pm 1.5 \pm 1.1) \cdot 10^{-4}, \quad (1)$$

$$Br(B \to \eta' + K) = (7.8^{+2.7}_{-2.2} \pm 1.0) \cdot 10^{-5}. \quad (2)$$

Simple estimates show that these data are in severe contradiction with the standard $b$-quark decay into light quarks: Cabbibo suppression $V_{ub}$ leads to decay rates two orders of magnitude smaller than the data (both inclusive and exclusive ones). Alternative mechanism, suggested by Halperin and Zhitnitsky,\[16\] is based on the Cabbibo favored $b \to c\bar{c}s$ process, followed by a transition of virtual $\bar{c}c$ into the $\eta'$. The latter transition implies large “intrinsic charm” component of the $\eta'$. Its quantitative measure can be expressed through the matrix element

$$\langle 0|\bar{c}\gamma_\mu \gamma_5 c|\eta'(q)\rangle \equiv i f^{(c)}_{\eta'} q_\mu. \quad (3)$$

and one needs $f^{(c)}_{\eta'} \approx 140 \text{ MeV}$ in order to explain the CLEO data, see.\[16\] This value is surprisingly large, being only a few times smaller than the analogously normalized residue $\langle 0|\bar{c}\gamma_\mu \gamma_5 c|\eta_c(q)\rangle = i f_{\eta_c} q_\mu$ with $f_{\eta_c} \simeq 400 \text{ MeV}$ known experimentally from the $\eta_c \to \gamma\gamma$ decay.

By expansion in inverse powers of charm quark mass, the problem can be reduced to the matrix element of a particular dimension-6 pseudo-scalar gluonic operator:

$$\langle 0|g^3 f^{abc}_{\mu\nu\sigma} \tilde{C}^{\mu}_{\nu\alpha} G_{\sigma\mu}|\eta'(q)\rangle >.$$ It was estimated in\[17\] and found to be indeed large, enhanced by very strong gluonic fields of small-size instantons. Quantitative description of CLEO data still remains to be work out, however.

(iii) The last example (strongly related with the previous one) is the contribution to the spin of the nucleon of the polarized charm “sea” (to be measured at next generation DIS experiments like CERN COMPASS). It is also related with the matrix element of the current $\bar{c}\gamma_\mu \gamma_5 c$, but this time averaged over the nucleon. As explained in recent paper by Blotz and myself,\[18\] using the same gluon operator one can do it in the instanton model. The resulting charm contribution of about -0.03.
4. Quark-Hadron Duality

The issue in early 70’s was a check of QCD itself: and we did found that, for example, the total cross section of $e^+e^- \rightarrow \text{hadrons}$ is close to total cross section of annihilation to all species of quarks. Now the topic was revived, and the issue is the nature and magnitude of the oscillating component, the difference between the two.

One useful direction of recent work is to study what happens in exactly solvable models. At this conference Lebed has presented results\textsuperscript{19} for heavy quark decay in 2-d QCD at large N, known as ’t Hooft model. The issue was also studied by Shifman et al.\textsuperscript{20} Two groups have nicely demonstrated that the duality itself works. However they strongly disagree about the magnitude of oscillating component. Lebed claimed that it is $O(1/M)$, based on numerical fit, while Shifman et al find much smaller effect $O(1/M^3)$ (M is the mass of the heavy quark).

What is the general reason why the oscillating part exist at all? Shifman et al\textsuperscript{20} simply argued that since the exponentially decaying contribution to correlators for space-like (Euclidean) momenta $\Pi(Q) \sim O(\exp(-Q \times \text{const}))$ does exist, such contributions become oscillating $O(\sin(Q \times \text{const}))$ for the time-like (Minkowski) momenta. What I find exciting about this argument is that oscillation period is related with singularities of amplitudes in x space away of x=0 (like the radius of the instanton in expressions like $1/(x^2 + \rho^2)$).

Let me add two important comments on that. First, since the derivative of the scattering amplitude over energy determines the lifetime of the intermediate system, the oscillating component should exist because otherwise we would not be able to get from the amplitude the duration of the final state interaction. (In the canonical case of total cross section of $e^+e^- \rightarrow \text{hadrons}$, this is the time at which the string is broken and final hadrons appear, or widths of corresponding resonances.)

The second: one can ask what happens when resonances overlap\textsuperscript{4}. My general expectation is that oscillating function is changed to a random one. However its correlators like

$$K(\epsilon, \delta\epsilon) = <\sigma(\epsilon + \delta\epsilon)\sigma(\epsilon) >$$

should be regular functions which still display the characteristic correlation scale in energy of the order of the inverse time of final state interaction $\delta\epsilon \sim \Gamma(\epsilon)$. Similar phenomenon known in nuclear physics under the name of Erickson fluctuations.

Summary: the physical nature and accuracy of parton-hadron duality is not yet clarified. Clearly much more theoretical and experimental work is needed. A good place for the latter is Jefferson Lab. which can check on accuracy of/deviations from the so called Bloom-Gilman duality (a difference between sum over hadronic excitations and smooth partonic structure function).

5. Can one use a common Effective Potential for various hadrons?

This issue was not part of section A I have to review, but it was discussed in a very interesting lecture by P.Lepage and, since it is vital to the field, I am tempting to

\textsuperscript{4}For example, in Regge trajectories $M_n \sim \sqrt{n}$ and spacing between resonances is $O(1/\sqrt{n})$ while the width $\Gamma_n \sim M_n$. 
try to clarify the issue somewhat.

Let me briefly repeat what he said in his talk. His basic idea is that lattice simulation allows one to get wave functions of quarkonia states like $J/\psi$ and $\Upsilon$, which can in turn be used to get an effective potentials (by putting those into Schroedinger equation). Among the questions Lepage has addressed are: (i) whether these potentials are universal and (ii) how close they are to the one extracted for static quarks. His results show that (i) for $J/\psi$ and $\Upsilon$ the potentials are very close, but (ii) both are about 20 percent different from the static one.

Let me start with the issue of the time scales relevant for the problem. We discuss heavy quarks $Q$ interacting with a non-perturbative glue, so let us imagine that those have characteristic time scales $\tau_Q$ and $\tau_g$ respectively.

The static potential would be correct if $\tau_Q \gg \tau_g$. It is however easy to see that the heavy quark limit $(M \to \infty)$ in fact leads to the opposite relation. It is true that the velocity of heavy quarks decreases with $M$, but sizes of quarkonia decrease even more and $\tau_Q \sim 1/(M \alpha_s^2)$. From old papers of Voloshin and Leutwyler we know that one can use in this limit dipole expansion, leading to oscillatory $V \sim r^2$ potential with the state-dependent coefficient. This explains why Lepage’s upsilon potential deviates from the static one.

But why it is the same for $\Upsilon$ as for $J/\psi$? My answer to that is that (if we still use the dipole approximation to second order) the effective potential is proportional to the following vacuum average over the gluelectric fields

$$V \sim r^2 \int d\tau < \tilde{E}(\tau) U(0) \tilde{E}^+(0) > \exp(-\tau \omega_{PS})$$

where $U$ are color transport matrices and $\omega_{PS}$ is the energy difference between the original S-wave state ($J/\psi$ or $\Upsilon$) and that of P-wave states the dipole excites.

Although the charmonium states are mostly sensitive to the linear potential while bottomonium are more due to the Coulomb part, by some play of numbers the essential frequency $\omega_{PS}$ is about the same for both. It means that the frequency of rotation of quarks in both states are the same, and we probe the same correlator of the vacuum fields. (Incidentally, it is about coincides with the correlation time of the vacuum field, see e.g. the talk by Dosch in these proceedings.)

Of course, if we go outside the dipole approximation in the second order, there are differences between two cases (e.g. the relativistic corrections with higher power of the velocity). Lepage’s results then imply that those are sufficiently small.

In summary: neither very heavy quarkonia (nor real $J/\psi$, $\Upsilon$) should be described by static potentials. One may construct those using lattice data for wave functions (and/or Gauge field correlators), but those are going to be state-dependent.

References

∥ The reader of the proceedings can of course read the Lepage’s contribution, while now I only have recollections of his talk. In case my points are also made there, I apologize.

∗∗For simplicity I ignore their splittings.
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