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PANEL DISCUSSION ON THE FUTURE OF HADRON SPECTROSCOPY

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Panel Discussion on the Future of Hadron Spectroscopy†

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Abstract. This contribution addresses two of the questions which were submitted to our panel on future developments in light hadron spectroscopy. Specifically, these were the extent to which glueball spectroscopy should be explored and how far it is appropriate to continue experimental studies of the light hadron spectrum. We suggest that at a minimum three particular glueball states should be identified, that hybrids should also be identified, and that experiments on hadrons should continue for as long as the theoretical community remains unable to predict their outcome.

I. DISCUSSION

Our panel was requested to share their views on several questions relating to the future of hadron spectroscopy. The two questions I would like to address are in effect

- How many glueballs do we need?
- When, if ever, would it be appropriate to terminate the experimental program in light hadron physics?

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†Presented at HADRON95, Manchester UK (9-14 July 1995).

First I will address the question of glueballs. At this meeting we have heard much about a glueball candidate, the $f_0(1500)$, which has been reported in $P\bar{P}$ annihilation by several collaborations, notably by Crystal Barrel [1]. This state has the quantum numbers and mass predicted for the lightest scalar glueball in lattice gauge theory simulations [2]; indeed, this agreement is one of the best arguments in favor of the glueball assignment. The branching fractions however do not appear consistent with a flavor singlet, indeed preliminary LGT predictions for two-body decay modes are more consistent with the $f_0(1710)$ as the scalar glueball [3]. So, it is not clear that we have even one glueball at present.

There are also interesting reports by BES [4] of a very narrow tensor state, usually referred to as the $\xi(2230)$. This state has been reported in $P\bar{P}$, $\pi\pi$ and $K\bar{K}$, and in the meson modes appears to be approximately flavor symmetric. Although there were previous suggestions that this might be a conventional $L = 3$ $s\bar{s}$ quarkonium state [5], subsequent work has shown that the $s\bar{s}$ states become rather broad when decay modes such as K_1K are included [6]. Consequently the narrow $\xi(2230)$, if confirmed and found to be 2^{++}, is a good candidate for the tensor glueball.

In future experimental work we should attempt to identify enough gluonic states to confirm (or refute) the detailed theoretical predictions for the spectrum of these unusual states. For glueballs the three lowest-lying states are expected to be an $f_0(\approx 1600)$, an $f_2(\approx 2300)$, and a pseudoscalar roughly degenerate with the tensor. If glueballs are found to be as narrow as reported for the $\xi(2230)$ and to have flavor-symmetric decays, then their identification will be straightforward. We may instead find that glueballs are not narrow and their decays strongly violate flavor symmetry, like the $f_0(1500)$ and $f_0(1710)$ candidates. In this case the task of identifying glueballs will be much more difficult and even ambiguous, because the physical states might be highly mixed linear combinations of $|glue\rangle$ and $|q\bar{q}\rangle$ basis states. Since LGT assumes pure glueballs, the LGT mass predictions then might include important systematic errors and could not be compared reliably to experiment without the incorporation of corrections due to mixing with quark states. It will be important to study glueball candidates in many strong modes to test flavor symmetry, and electromagnetic couplings should also be determined for comparison with $q\bar{q}$ and to search for $q\bar{q}$ components.

Glueballs may be difficult to identify convincingly because the lightest ones are predicted to have conventional $I = 0$ $q\bar{q}$ quantum numbers. Hybrid states in contrast are expected to have exotic quantum numbers in the lightest multiplet, and decay calculations in the flux tube model predict that the $I = 1$, $J^{PC} = 1^{-+}$ hybrid should be straightforward to observe. This exotic hybrid is expected near 1.9 GeV [7] with a typical hadronic width of ≈ 0.2 GeV and dominant decay modes $f_1\pi$ and $b_1\pi$ [8]. To complete the picture of gluonic

hadrons we should identify several of these hybrids, especially J^{PC}-exotic ones, in addition to the glueballs.

Thus, the answer to the first question is

- Three. (The 0^{++}, 0^{-+} and 2^{++} glueballs should be found.) Hybrids should also be identified, especially J^{PC} exotics.

The final question is in effect "When can we quit doing experiments on light hadron spectroscopy?". Presumably this remarkable question was motivated by the increasing constraints on funding for experiments, the competition with very high energy facilities such as the LHC, and the theoretical view that since QCD has already been established nothing new is being learned from experiment. It may also be suggested by the good agreement between the experimental spectrum and the predictions of $q\bar{q}$ potential models such as the Godfrey-Isgur model [9].

Although QCD has indeed been confirmed as the theory of the strong interaction, this alone does not imply that experiments in the subject should stop. The ultimate goal of physics is to predict all observables, which in practice means to calculate the S-matrix. In hadron physics we know that QCD is the lagrangian, so the determination of the S-matrix is a well defined problem, but this does not tell us what the S-matrix is! The best efforts of lattice gauge theory have given us predictions for the masses of some low-lying resonances (which tells us the location of some S-matrix poles, *albeit* in the quenched approximation), but higher excitations such as radial quarkonia are notoriously difficult to determine using Monte Carlo methods, and LGT is only just beginning to produce results for other observables such as scattering lengths [10]. Without an understanding of scattering amplitudes the entire subject of multiquark systems and hadronic molecules will remain poorly understood. Until such time as we can predict the S-matrix starting from QCD, the subject cannot be regarded as solved, and an improved understanding will as usual follow from comparisons of the existing lattice QCD results and QCD-inspired models with experiment.

This suggests a general rule for when one can stop doing experiments in *any* field of physics:

- It is time to stop when you are confident that an experiment, if done, would only confirm theoretical predictions.

We are not there yet. [11]

For future experimental progress one would prefer to develop new facilities which are especially designed for hadron physics, such as a higher-energy LEAR or a TCF. A Tau-Charm Factory would be a very interesting option

because the studies of light hadron spectroscopy using J/ψ radiative and hadronic decays at e^+e^- machines in the 1980s were not able to accumulate adequate statistics for definitive studies of resonance branching fractions, decays and quantum numbers. In lieu of such a dedicated facility, one should consider the possibilities at other accelerators now operating or under development that can make contributions to light hadron spectroscopy, although not designed especially for that purpose. At the lower energy scales these include DAPHNE (which will study molecules, KN scattering and vector spectroscopy) and CEBAF (which will use photo- and electroproduction to study baryon and meson resonances, perhaps into the charmonium mass range). At higher energies the e^+e^- B-factories, LEP, RHIC and even the LHC can contribute; at all these facilities one can carry out experiments in $\gamma\gamma$ collisions, in some cases with very impressive luminosities. Thus, although the departure of LEAR will end high-statistics data taking in $P\bar{P}$, this may only lead to a change of emphasis in spectroscopy from purely hadronic reactions to other production mechanisms such as photoproduction and $\gamma\gamma$ collisions. In view of the very interesting contributions already made to light hadron spectroscopy using $\gamma\gamma$, and the progress which could be made in separating $q\bar{q}$ from non-$q\bar{q}$ states by comparing $\gamma\gamma$ and $\gamma\gamma^*$ couplings, such a change of emphasis could be a net gain.

II. ACKNOWLEDGMENTS

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[1] C.Amsler (Crystal Barrel), these proceedings. See also V.V.Anisovich *et al.*, Phys. Lett. B323, 233 (1994); C.Amsler, in Proceedings of the XXVII Int. Conf. on High Energy Physics (Glasgow, 20-27 July 1994), Zürich report UZH-PH-50/94; C.Amsler and F.E.Close, "Evidence for Glueballs", Rutherford Laboratory and CERN report CCL-TR-95-003 (April 1995); *ibid.*, Rutherford Laboratory reports RAL-95-036 (May 1995) and RAL-TR-95-003 (July 1995).

[2] G.Bali *et al.*, Phys. Lett. B309, 378 (1993); H.Chen *et al.*, IBM report IBM-HET-94-1 (contribution of A. Vaccarino to Lattice 93).

[3] J. Sexton *et al.*, IBM report IBM-HET-94-5 (contribution to Lattice 94).

[4] Y.Huang (BES), these proceedings. See also T.Huang, contribution to the Argonne Workshop on a Tau-Charm Factory (June 1995) and T.Huang *et al.*, CCAST report BIHEP-TH-95-11.

[5] S.Godfrey, R.Kokoski and N.Isgur, Phys. Lett. B141, 439 (1984).

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[7] See for example T.Barnes, F.E.Close and E.S.Swanson, Oak Ridge National Laboratory / Rutherford Laboratory Report ORNL-CTP-95-02 / RAL-94-106, hep-ph/9501405, Phys. Rev. D (to appear); N.Isgur and J.Paton, Phys. Rev. D31, 2910 (1985).

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[9] S.Godfrey and N.Isgur, Phys. Rev. D32, 189 (1985).

[10] See for example R.Gupta, A.Patel and S.R.Sharpe, Nucl. Phys. B383, 309 (1992).

[11] As S.U.Chung notes (summary talk, these proceedings) in hadron physics we will 'Probably never' be there.