GLUEBALLS AND OTHER EXOTICA IN $p\overline{p}$ ANNIHILATION

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SUMMARY

Theoretical predictions for exotic states are reviewed and it is found that there are few areas of agreement between the various models. It is argued that a $p\overline{p}$ experiment with suitable luminosity and detectors could provide much help in discriminating between the models. Various specific final states are discussed.

INTRODUCTION and CONCLUSION

To avoid repeating myself I have combined my introduction and conclusions[2]. In the two sections following I will first flesh out the theoretical arguments and then discuss some phenomenological details.

This talk will address the questions: "What does theory tell us about exotic states in QCD?" and "What can low energy proton – antiproton collisions tell us about exotic states in QCD ?". Exotic states expected in QCD are glueballs, meiktons, baryonia ($qq\overline{q}\overline{q}$), and dibaryons. I will concentrate mainly on glueballs and meiktons, and I will not say anything about dibaryons.

In my view present theoretical expectations concerning exotics are weakly founded. This can be seen in two ways – either by comparison of the various model predictions, or

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[2] For this practice there is good precedent [1].

by examining the models themselves. The only common prediction for glueballs is that the scalar glueball is lighter than the others, but the overall mass scale is very much in doubt; it may be anywhere in the range 0.7 - 1.7 Gev. For the meiktons the only common prediction is of a nonet of exotic 1^{-+} states; their masses vary from 1.4 GeV to 2.1 GeV. In the long term, lattice calculations offer the best hope of well founded predictions. In fact I am involved in a lattice calculation which I shall have more to say about below. But "long term" here means at least a few years.

Clearly, then, we need experiment to step into the breach. And that is has been doing most successfully. In fact, too successfully – a lot of new states $(\iota, \theta, g_s, G(1590), g_t, \xi, \ldots)$, and unexplained enhancements $(J/\psi \to \gamma\rho\rho, \ldots)$ have been found, but theorists cannot agree on their interpretations. This will doubtless change, and one very important catalyst will be more data. In particular, some of these states have only been seen in one channel and by one experiment. Confirmation of such states, and, hopefully, the discovery of new resonances, is a general reason why it is very important to build the $p\bar{p}$ machine.

For such a machine to be useful, however, it is important to have detectors that can easily separate pions from kaons, and which have good neutral detection. Also good resolution in energy detection is needed because many of the interesting channels have large combinatorial backgrounds. As studies at the J/ψ have shown, it is very important to get high statistics in order to disentangle the myriad states that have and might appear. Thus a crucial feature of the machine will be a large enough anti-proton luminosity. I will try and say something more quantitative about this below, although it is really guesswork at this stage.

It is natural to wonder whether a $p\bar{p}$ machine is particularly suited for producing exotic states, and in particular states containing excited gluonic degrees of freedom. It seems plausible that this is so – a drastic rearrangement of the quarks and anti-quarks is occurring – but it is not so clear as in radiative J/ψ decay. However, it should not be forgotten that $p\bar{p}$ was the first experiment to come up with a glueball candidate back in 1963 [2]. So there is good reason to suppose that with more statistics and better experiments, much more can be found out now.

As I see it there are two main modes of operation in which one will search for exotics. The most straightforward is to look for exclusive production of states heavier than $2m_p$, such as the ξ and the $\phi\phi$ states. This method also applies to the heavy meikton states. The second method is to look for exotics produced in association with one or two pions, and perhaps also kaons. This is the $p\bar{p}$ equivalent of the radiative J/ψ decay which has been so successful in providing us with new states and structures. The main disadvantage of the $p\bar{p}$ reactions, compared to the radiative J/ψ decays, is that they *need* not involve hard gluons, so there will be a larger background from the production of conventional resonances. Because the "trigger" particle(s) are not photons, there will also be larger combinatorial backgrounds. Compensating for these problems is the possibility of much larger rates, and thus much higher statistics, than those obtained in radiative J/ψ decay. This is very important because only with high statistics can spin-parity analyses be done, and such analyses are crucial if the data is to be unraveled. Also some channels, in particular the exotic $J^{PC} = 1^{-+}$, are suppressed in radiative J/ψ decay but not in $p\bar{p}$ annihilation.

Thus if the detectors and machine are good enough $p\bar{p}$ annihilation is a source of exotics potentially exceeding radiative J/ψ decay. If so it will be an excellent machine for exploring the unsolved puzzles of the spectrum of QCD.

THEORY

(a) *Glueballs.* I have little doubt that QCD contains glueballs in its spectrum. My confidence comes from the lattice regularisation of QCD, which has been simulated numerically with some success. I would like to spend some time discussing the pros and cons of lattice calculations because, unlike the other models used to describe glueballs and meiktons, lattice calculations use an approximation (discretization) which becomes exact in a well defined limit. Thus to interpret lattice work and estimate its utility one has to use different criteria than, say, for the bag model. Since I'm now involved in the lattice business and thus think I know something about it, I will try to provide a filter through which the non-lattice expert can pass lattice results.

Let me start by considering pure gauge QCD, i.e. gluons with no quarks; in fact all numerical work to date on glueballs has been done on this theory. The important questions are, first, whether the theory has a mass gap between the lowest energy (vacuum) state and the first excited state (as opposed to the possibility of a theory of massless gluons). I think that this question has now been definitely answered in the affirmative [**3**]. The second question is whether the continuum limit can be taken, i.e. can one tune the parameters, in this case take $g(a) \to 0$, so that the mass gap diverges in lattice units, in a way specified by the renormalization group. To this question there is not yet a definitive answer, although I hasten to add that there is no evidence to which suggests a negative answer. There *is* strong evidence [4][5] that quantities other than the mass gap – the string tension (see below) and the deconfinement temperature – do diverge in the appropriate way (show "asymptotic scaling" in the parlance), and I think these calculations are very impressive achievements of the lattice approach. But it is simply a matter of making the (more difficult) measurement of the mass gap on bigger and better lattices in order to check asymptotic scaling, and this takes time.

On the lattice one gets at masses by measuring two point (spectral) functions – much like one measures the electromagnetic two point function experimentally with an e^+e^- machine. The main difference is that one works in Euclidean space on the lattice, so that a given state contributes a term $Ae^{-m|t_1-t_2|}$, where A is the product of the coupling of the initial and final operators to the state, and m is the mass. Thus to extract the mass of the lightest state in a channel one needs to go to "large enough" times, and convincingly demonstrate that one is only seeing a single exponential. One can help matters by finding a combination of operators which projects maximally onto the lightest state, and minimally onto higher states, so that the amplitude for the higher states is relatively reduced. This technique is called the Monte Carlo Variational method.

Now, the original glueball estimates [6] were made with data rarely extending out to three timeslices, so even with a variational calculation, it is extremely hard to draw convincing conclusions. To do so in $q\bar{q}$ meson mass calculations one often needs many more timeslices (of order 10). The reason that the signals are so poor for glueballs is that there are fluctuations at both ends of the correlator. Both the operator which creates the glueball state, and that which destroys it, fluctuate. With $q\bar{q}$ mass calculations, part of the calculation is done deterministically, and much better results ensue. Various techniques have now been developed to reduce this problem for glueballs. The most useful to date has been that of doing the functional integral in the presence of a fixed glueball source [7].

Then there are fluctuations only at one end of the correlator, and signals which extend out to timeslices comparable to those in $q\bar{q}$ calculations have been found. This technique does have problems – it does not give an upper bound on the mass, it can be done only for one J^{PC} at a time, and it is less amenable to variational methods – but it has given the only credible mass gap measurements to date. Unfortunately it has not been done successfully on lattices on which the other physical quantities have shown asymptotic scaling. I will quote the results nevertheless: the ratio of the scalar glueball mass to the square root of the string tension is 2.5 ***and rising*** at $\beta = 5.9$ [3]. If one naively sets the scale using $\kappa \approx (.42GeV)^2$ for the string tension this gives a glueball mass of greater than 1 GeV. No mass value has yet been found using sources with $J^{PC} = 2^{++}$.

The collaboration of which I am a part [8] has attempted to circumvent the problem of needing to go to smaller couplings and thus larger lattices by using an improved action. The idea is to add more terms to the gauge action and tune them to remove, as far as possible, the artifacts of the lattice approximation. The hope is that one can get better results from the same size lattices, at the relatively small cost of a decrease in the monte-carlo update time. Our action also takes us further away from the singularity in the fundamental-adjoint plane, which singularity may be the cause of the late onset of asymptotic scaling. Our results have some unresolved problems, but we do seem to find that $\frac{\sqrt{\kappa}}{m_G} \approx 3 - 3.5$. This would put the scalar glueball mass between 1.2 - 1.5 GeV. Unfortunately we also have found that the signal for the tensor glueball disappears in the noise at the third timeslice, so we cannot extract a mass.

I do not wish to trumpet this result too loudly, however, for a number of reasons. It is preliminary: we are performing better analyses and hope to get more data. More importantly, though, the relevance of results obtained in the pure gluon theory is not at all clear. Including fermion loops will no doubt change the values of the string tension and the glueball mass, and will allow the glueball to mix with $q\bar{q}$ states, and also to decay. These changes could well be by a factor of two [9] and there is no good reason why ratios such as $\frac{\sqrt{\kappa}}{m_G}$ should remain unchanged. So a pessimistic point of view would be that we have learnt little or nothing about the real world from lattice calculations to date. There is also a suicidal viewpoint: that glueballs and $q\bar{q}$ states are not different – but this seems unlikely as one can construct glueball operators with spin-parities such as 0^{--} not available to $q\bar{q}$ states. Or finally, there is the optimistic point of view that the main effect of including dynamical fermions is to change the coefficient in the β function and thus change the way that physical quantities diverge as one takes the continuum limit. Ratios are unchanged, and the results discussed above would apply, except for possible mixing and unitarity effects. This view receives some support from the small scale calculations done with dynamical fermions [10]. As I said in the introduction it will be a few years before we know for sure.

Before I move on I want to comment on a recent preprint of Berg *et al* [11] claiming to find evidence that the tensor glueball mass is lighter than the scalar glueball mass in pure QCD. This result is suprising from a symmetry point of view, but also I think wrong. The authors use small lattices, most of which are deconfined or nearly so, i.e. the theory is in the gluon plasma phase which has no glueballs.

I started by saying that the lattice presented the only hope of results that we could believe in, and now I've almost discounted the lattice results obtained to date. This leaves us with a motley assortment of models which have been reviewed extensively by me [12] and many others [13][14]. Since very little new work on these models (that I'm aware of) has been done in the last two years I refer the reader to these earlier reviews. I will briefly recap the conclusions of my survey of the models.

Bag, constituent gluon, sum rule, and flux tube models all agree that the scalar glueball should be the lightest. Beyond that there is little agreement in the ordering of the states, and there is little agreement on the overall scale. The flux tube and sum rule models tend to give much higher masses with lots of states lying above $2m_P$, states which might be found in an exclusive pp search. The width of glueballs is not clear, varying from the relatively narrow $\sqrt{OZI}$ prediction of $O(10MeV)$, to the very broad width expected (I think quite reasonably) for a scalar glueball decaying to two pions. One interesting possibility, suggested by the bag model [15] and, in a different sense, by the J/ψ radiative decay data [16][17], is that constituent gluons have an enhanced coupling to strange quarks. This, at the very least. is meant to make one suspicious of the naive prediction of flavor singlet decays.

Another naively expected property of glueballs is a depressed coupling to photons both in radiative decays and in two photon production. This together with the naively expected enhanced production in the "gluon rich" radiative J/ψ decay has lead Chanowitz [13] to suggest the quantity "stickiness" (a term which grabs you and won't let go) - $S = \frac{\Gamma(J/\psi \to \gamma X)}{\Gamma(X \to \gamma\gamma)}$ (corrected for phase space). S should be large for glueballs and is large for the ι and θ. Unfortunately, it is hard to know how to fit $p\overline{p}$ production into this scheme. In any case, I want to stress (see refs. [12] and [13] for more detailed discussion) that stickiness may not be a good guide for the scalar and psuedoscalar channels. Due to the trace and axial anomalies, respectively, there is large non-perturbative mixing between

$q\bar{q}$ and gluon components in these channels, at least for the lightest state(s). This is unfortunate, as it obscures the naive signature, but it is one of the few well founded predictions that one can make.

As far as the radiative decays go, the glueball widths will be smaller than those for the $q\bar{q}$ states which are not radially excited, with the possible exceptions of the scalar and pseudoscalar channels, but the widths may be comparable to or larger than those of radially excited $q\bar{q}$ states. Thus radiative decays may provide a useful discriminator, but not in the way naively expected. Thus if at all possible the $p\bar{p}$ detectors should be able to study radiative decays.

(b)*meiktons.* Meiktons are hybrids between $q\bar{q}$ states and glueballs, and thus are known by some as hermaphrodites. They contain both valence quarks and gluons. They don't quite have the "caché" of glueballs, which is due in part to the dryness of editorial arbitration, but they may be just as important phenomenologically. Their existence as states separate from glueballs and $q\bar{q}$ mesons is not universally accepted: it may not be correct to think of glueballs as made of valence gluons, rather as an assemblage of an infinite number of gluons. However, an appeal to the lattice again makes it seem likely that such states exist, at least for heavy quarks. Here I am explaining some very nice work of Michael and collaborators [5].

To measure the potential between infinitely heavy quark and antiquark on a lattice one measures the lowest energy of a line of flux as a function of it's length. One can couple to this flux by using, for example, the product of gauge link matrices along the straight line joining the quark and antiquark. What Michael and co-workers do is "twist" the flux line between the quark and antiquark such that it belongs to a non-trivial representation of the cylindrical group. Thus they force the flux line to be excited, with the quarks themselves still fixed. This gives rise to a potential of higher energy than the usual one, but it still yields bound states. These then are the equivalents of meiktons for heavy quarks. This picture has been developed into a phenomenological model by Isgur, Kokoski and Paton [18], about which Nathan Isgur will talk here.

For light quark meiktons one must allow the quarks to fluctuate. One can construct $q\bar{q}g$ operators and in principle measure their correlation functions. However, it is very difficult to get the statistics needed for a good signal, and so far no results have been obtained. However, since one can construct simple operators with the exotic quantum numbers (e.g. 1^{-+}) expected of meiktons, the only way that they cannot exist is to have infinite mass or zero amplitude. If the exotic meiktons exist, then it is most likely that the others do too. Notice that none of these arguments really bare upon the question of whether valence glue is a valid concept. If it is, then it gives information about the order of the many states for which one can write down interpolating operators. A correspondence between interpolating operators and the ordering of states has been investigated by Jaffe and collaborators [19].

For any guide to their masses and properties, though, one must turn to the models which I have already tried to put down above. The most important common prediction is that among the lightest meiktons there is a nonet of exotic 1^{-+} states. Bag models [20][21][22] put the isovector at 1.4 - 1.8 GeV, the flux tube model (FTM) at [18] 1.9 GeV, and the sum rule calculations at 1.3 [23] or 1.6 - 2.2 GeV [24]. Perhaps, then, a reasonable range is to bracket these, i.e. 1.4 - 2.1 GeV. The bag model has $0^{-+}, 2^{-+}, 1^{--}$ nonets within 400 MeV of the 1^{-+}, while the FTM predicts that states with all the spin-parities

$(0,1,2)^{-+}, 1^{--}, (0,1,2)^{+-}, 1^{++}$ will lie in this range. Thus the FTM has the extra exotics 0^{+-} and 2^{+-} among the lightest meiktons.

I mention in passing that for heavy quarks the 1^{-+} and 1^{--} states are both predicted in the ranges 4.0 - 4.3 GeV for $c\bar{c}g$, and 10.5 - 11.0 GeV for $b\bar{b}g$, and that this agrees with the lattice estimates. Again there is disagreement on the quantum numbers of other states present close to these masses, and furthermore there is disagreement on the ordering of levels between bag and sum rule calculations. In any case these states will lie above the open flavor thresholds, and so will be difficult to find. But they are certainly worth searching for in the scanning mode of a $p\bar{p}$ machine.

Unlike glueballs, meiktons always come in flavor nonets, which is both good and bad. Good because it allows one experimentally to keep away from the higher background isoscalar channels; bad because if one tries to identify an experimental state with a meikton, then one must find candidates for the rest of the states in the nonet.

Turning to the decay of the meiktons, confusion continues to reign. Calculations have been done in the FTM and using sum rules. To be fair I should say that the FTM and the associated flux breaking mechanism for meson decay have been extensively tested by Isgur and co-workers. By comparison, the sum rule method has been less rigorously tested in its application to decays. Despite this the differences in the predictions are so large, as you will see, that it is difficult to believe in the detailed predictions of either model. I will simply summarize the results.

The FTM finds that meiktons are very broad and that the dominant decays are to two mesons, one of which is orbitally excited, the other not. Decays to two $L = 0$ mesons with some relative angular momentum are suppressed. These properties make them very hard to find. One of the best cases is the $I = 1, 1^{-+}(\tilde{\rho})$; it has $\Gamma(\tilde{\rho} \to \pi D + \pi B) \approx 200 MeV$ in the FTM. The sum rules have as yet nothing to say about this decay, but do find substantial decays to two $L = 0$ mesons: $\Gamma(\tilde{\rho} \to \rho\pi) \approx 10 - 100$ MeV [23] or 600 MeV [24], $\Gamma(\tilde{\rho} \to K^*\bar{K}) \approx 300 - 1300$ MeV [24] (sic). I deduce from this that it is reasonable to expect meiktons to be broad, possibly very broad. Furthermore, the theory is not in good enough shape to tell us which modes to look for, and so both types of decay should be tried.

Another decay mode is $\tilde{\rho} \to \eta(\eta')\pi$. Because the final mesons are in a p-wave, this process is described only by quark diagrams which are disconnected, the $\eta(\eta')$ being connected to the valence gluon. This mode may be suppressed by the $\sqrt{\sqrt{OZI}}$ (sic) rule, but is still a good channel to try.

Two other properties of experimental relevance which have been suggested are that the gluon in the meikton will have and enhanced coupling to strange quarks (just as for glueballs above), leading to decays which are OZI violating for ordinary mesons such as $\tilde{\rho}(1^{+-}) \to \phi\pi$, and that the octets may not appear ideally mixed, the $\tilde{\omega}$ (isoscalar) being heavier than the $\tilde{\rho}$ (isovector) by $\approx 100 MeV$.

Finally, if mixing with $q\bar{q}$ states is a potentially confusing factor for glueballs, then it is even more so for meiktons, except for the exotic ones. Thus in non-exotic channels state counting may be the only way we can tell that something is new.

(c) ***Baryonia or 4-quark states.*** I want to say something about these states, since they may not be covered in any other talk here. These are the least convincing of the exotics I've discussed so far. Lattices provide no insight here, and present lattices are in any case too small to accomodate what are most likely quite extended states. The problem with these states is that by construction they nearly all lie close to their two body "fall apart" thresholds, and if above them they are like attractions, not resonances. It is hard for me to say anything not said, either for or against, in the original papers by Jaffe [25]. They do not have exotic J^{PC}, but some have the exotic isospins $\frac{3}{2}$ and 2. Unfortunately the exotics tend to be heavier, and thus lie above their fall apart thresholds. Even if below the threshold there are in most cases many OZI allowed decays available, so they are likely to be quite broad. This makes the proposed identification of the $\xi(2220)$ [26] with an $ss\bar{s}\bar{s}$ state seem unlikely at first sight, but arguments have been presented [27] in support of this identification. These authors also argue that higher mass baryonia, in particular $cs\bar{c}\bar{s}$ with $J^{PC} = 0^{++}$, should have similar properties. Such states could be scanned for with a $\bar{p}p$ machine.

These negative comments are all true with two notable exceptions, the $\delta(980)$ and the $S^*(980)$. Here the bag model and non-relativistic quark models converge and predict a state below $K\overline{K}$ threshold. The bag model has trouble in suppressing the fall apart decay $\delta \to \eta\pi$, but the non-relativistic quark model picture of essentially a $K\overline{K}$ bound state has no such trouble because the K and $\overline{K}$ are well separated most of the time. This picture explains many things[25], but it should not be forgotten that there is an alternative approach – the quark model with unitarity corrections [28] – which also claims to accomodate these states. However, Barnes[29] has pointed out that recent two photon production of the δ has yielded results quite different from quark model predictions, yet much closer to the baryonia predictions.

These states are not without their mysteries still, though. The branching ratios $B(\delta \to \eta\pi)$ and $B(\delta \to KK)$ appear to be process dependent. Furthermore, a K-matrix analysis of the existing $\pi\pi \rightarrow \pi\pi$, $\pi\pi \rightarrow K\overline{K}$, $pp \rightarrow pp(\pi\pi)_{central}$ and $pp \rightarrow pp(K\overline{K})_{central}$ has yielded an extra S-matrix pole very close to the S^*[30]. All this means that more data is needed.

Although it is not strictly relevant, I want to mention a recent paper of Lipkin [31] which argues that baryonia containing two heavy quarks, and two light antiquarks should, for sufficiently heavy quark, be stable against fall apart decay. Unfortunately, these states have to be pair produced, but if there were any way of scanning for them, it would be most interesting.

PHENOMENOLOGY

The previous section should have made clear that theory can provide few definite predictions, though it has given many guidelines, which if at all true, could be useful experimentally. I imagine that a sort of bootstrap is possible, data sorting out the good ideas from the bad, leading to more reliable predictions, etc. . This cycle can take place without understanding the reason that one model is preferred over others. The OZI rule is a case in point.

To sort out the good models and ideas from the bad, it is clearly essential that we get more data. This is not just a ritual cry for *more* which covers up a lack of understanding – on the contrary we are approaching a great leap in our understanding of meson spectroscopy, thanks to new results from radiative J/ψ decay and other experiments. But some crucial pieces in the puzzle are missing, and a $p\overline{p}$ machine could provide them.

Two examples spring to mind, and they will begin what amounts to a series of examples of how a $p\overline{p}$ machine can help in the search for exotics. This series is not, by any means, supposed to be exhaustive. The first example is the $\xi(2220)$, seen in K^+K^-

and $K_S K_S$ in radiative J/ψ decay by the Mark III [26], but not seen by DM2. Mark III barely has enough events in the background free channel $K_S K_S$ to do a spin-parity analysis. The ξ can be produced exclusively in $p\bar{p}$, and can be mapped out in the scanning mode. Even if the fraction of the total cross section is 10^{-4}, with $5.10^5 \bar{p}$/sec a planned LEAR experiment expects to get 50 events/hour [32]. If so, a reasonably short run can determine the spin-parity, width, and search for other decay modes. Although perhaps the most reasonable interpretation of the ξ is a $L = 3, 2^{++} s\bar{s}$ meson [33], exotic interpretations have been suggested. Determining the properties of the ξ can help settle these issues. In particular the $L = 3$ model requires a state at about 2 GeV decaying to two pions. For further discussion of the ξ see the talk of Mike Chanowitz.

The other example that stands out is the $\iota(1460)$, which state deserves pride of place in this talk. Over the last few years, the isoscalar, pseudoscalar channel above 1 GeV has become more and more confused. A careful reanalysis of the original channels $p\bar{p} \rightarrow \pi\pi(K\overline{K}\pi)$ and $p\bar{p} \rightarrow \pi\pi(\eta\pi\pi)$ at a variety of beam momenta is essential. Since there may be two psuedoscalars and one axial vector in this mass region [17], a search in as many final states as possible should be done. Even if one of the states decouples from some channels, it will not do so from all. More discussion of the ι is given in the talk of Harry Lipkin.

Part and parcel of the ι mystery is the problem that the $\delta(980)$ seems to have different decay branching ratios in different production reactions. The δ can be studied in $p\bar{p} \rightarrow \pi\delta$, where the $p\bar{p}$ annihilate from a $0^{-+}, I = 0$ state[3] . The similar decay to $\eta\delta$ might g. e

[3] As discussed by Jaffe in his talk, if one looks in the $\pi^0\pi^0\eta$ mode, $0^{-+}, I = 0$ is the only initial channel allowed. This will help to reduce the background. Similar arguments apply in the $\pi^0\pi^0\phi$ and $\pi^0\eta\eta$ decay modes.

some information about the structure of the δ. Other exclusive decays produced with non-zero $\bar{p}$ momentum such as $\omega\delta$, $\rho\delta$ and $\phi\delta$ are also interesting, particularly in light of the Mark III finding that $J/\psi \rightarrow \phi S^*$ occurs at a much greater rate than $J/\psi \rightarrow \rho\delta$[17]. Similar decays to the S^* may help clarify the T-matrix pole structure.

It may be, however, that the only way to convincingly demonstrate the existence of exotics is to discover states with exotic quantum numbers. The 1^{-+} is easily accessible in $p\bar{p} \rightarrow \pi 1^{-+}$, with the final state in a p- or s-wave, depending on whether the initial state is in an s- or p-wave. All non-strange members of the exotic nonet are accessible. These reactions can only proceed from annihilation at rest if $m_{1^{-+}} < 1.7GeV$, which may well not be heavy enough. Notice that for the production of the exotic 1^{-+}, $p\bar{p}$ annihilation is favored over radiative J/ψ decay because in the latter the two gluons cannot couple (on mass shell) to 1^{-+}.

Having produced the exotic, it should be looked for in $\eta\pi$, $\rho\pi$ (there is probably a large background in this channel $p\bar{p} \rightarrow 4\pi$), $K^*\bar{K}$, πB and πD. The last two channels have large combinatorial backgrounds, and can only be accessed from annihilation in flight because the particles must be in a p-wave. But it has been claimed that using appropriate decay channels a signal could be reconstructed, if it existed[34]. The remaining members of the exotic nonet could be searched for in association with a kaon.

Similarly the state G(1540) seen in $\eta\eta$ and $\eta\eta'$ [35] could be searched for in association with a pion in the annihilation at rest.

Finally, a scan for the exclusive decay $p\bar{p} \rightarrow \phi\phi$ should help clarify the existence and properties of the tensor g_t states found at Brookhaven.

I have not talked at all about rates, mainly because they are hard to estimate theoretically, and because I do not know the planned machine parameters. The branching ratio $BR(p\bar{p} \rightarrow \iota\pi\pi)BR(\iota \rightarrow K\bar{K}\pi)$ is about 2.10^{-3}. If radiative J/ψ decay is any guide, this will be one of the largest exclusive branching fractions. Thus a sensitivity to branching ratios down to 10^{-5} or lower should be aimed for. If this can be achieved, and a general purpose detector built, I foresee a low energy $p\bar{p}$ annihilation machine at FNAL yielding lots of interesting physics.

References

[1] See for example P. Nelson and S.Coleman, Nucl. Phys. **B237**(1984)1.

[2] P. Baillon *et al* , Nuovo Cimento **50A** (1967)393.

[3] Ph. de Forcrand *et al* , Phys. Lett. **152B**(1985)107; Phys. Lett. **160B**(1985)137.

[4] S. A. Gottlieb *et al*, Phys. Rev. Lett. **55**(1985)1958 ;
R. Gupta *et al*, Phys. Lett. **161B**(1985)352.

[5] C. Michael, Talk presented at the Workshop on Lattice Gauge Theory, Wuppertal, November 1985 (Illinois Preprint P/86/2/17), and references therein.

[6] K. Ishikawa *et al* , Z. Phys. **C21**(1983)167 ;
B. Berg and A. Billiore, Nucl. Phys. **B226**(1983)405.

[7] Ph. de Forcrand and C. Roiesnel, Phys. Lett. **151B**(1985)77.

[8] R. Gupta *et al* , work in progress.

[9] M. G. Olsson and C. Suchyta III, Madison Preprint MAD/PH/256 (July 1985).

[10] J. B. Kogut, Illinois preprint ILL-TH-86-19 (Feb 1986).

[11] B. Berg, A. Billoire and C. Vohwinkel, Florida preprint FSU-SCRI-86-06 (March 1986).

[12] S. R. Sharpe, Talk presented at the Symposium on High Energy e^+e^- Interactions, Vanderbilt Univ., 1984 (published in the proceedings).

[13] M. S. Chanowitz, Lecture presented at the VI International Workshop on Photon-Photon Collisions, Lake Tahoe, Sept 1984 (published in the proceedings).

[14] P. M. Fishbane and S. Meshkov, Comm. Nucl. Part. Phys. **13**(1984)325 ;
K. A. Milton, W. F. Palmer and S. S. Pinsky, Moriond Conf. 1982 v.2:67 ;

T. Barnes, Rutherford Preprint RAL-85-005 (1985) ;

and many others listed therein.

[15] M. S. Chanowitz and S. R. Sharpe, Phys. Lett. **132B**(1983)413.

[16] Particle Data Tables.

[17] R. Partridge (Mark III), seminar at Univ. Washington, April 1986.

[18] N. Isgur and J. Paton, Phys. Rev. **D31**(1985)2910 ;

R. Kokoski and N. Isgur, Toronto preprint UTPT-85-05 (Oct 1985) ;

N. Isgur, R. Kokoski and J. Paton, Phys. Rev. Lett. **54**(1985)869.

[19] R. L. Jaffe, K. Johnson and Z. Ryzak, MIT preprint MIT-CTP-1271 (Aug 1985).

[20] P. Hasenfratz *et al*, Phys. Lett. **95B**(1980)299.

[21] M. S. Chanowitz and S. R. Sharpe, Nucl. Phys. **B222**(1983)211.

[22] T. Barnes, F. E. Close and F. de Viron, Nucl. Phys. **B224**(1983)241.

[23] J. Govaerts *et al* , Nucl. Phys. **B248**(1984)1.

[24] J. I. Latorre, P. Pascual and S. Narison, Montpelier preprint, IC-85-299 (Nov 1985).

[25] R. L. Jaffe, Phys. Rev. **D15**(1977)267; **D15**(1977)281.

[26] R. M. Baltrusaitis *et al* , Phys. Rev. Lett. **56** (1986)107.

[27] S. Pakvasa, M. Suzuki and S. F. Tuan, Phys. Lett. **145B**(1984)135; **D31**(1985)2378.

[28] See for example N. A. Tornqvist, Phys. Rev. Lett. **49**(1982)624.

[29] T. Barnes, Phys. Lett. **165B**(1985)434.

[30] K. L. Au, D. Morgan and M. R. Pennington, Phys. Lett. **167B**(1986)229.

[31] H. J. Lipkin, Argonne preprint ANL-HEP-PR-86-15 (Feb 1986).

[32] D. W. Hertzog *et al* , Proc Int Conf on Hadron Spectroscopy, Univ. Maryland, 1985 (ed. S. Oneda).

[33] S. Godfrey, R. Kokoski and N. Isgur, Phys. Lett. **141B**(1984)439.

[34] Crystal Barrel Proposal, CERN/PSCC/85-56 (Oct 1985).

[35] F. Binon *et al* , Nuovo Cimento **78A** (1983)313; **80A** (1984)363.