A RENAISSANCE IN STRONG INTERACTION PHYSICS

Hadrons and Exotics

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A Brief History

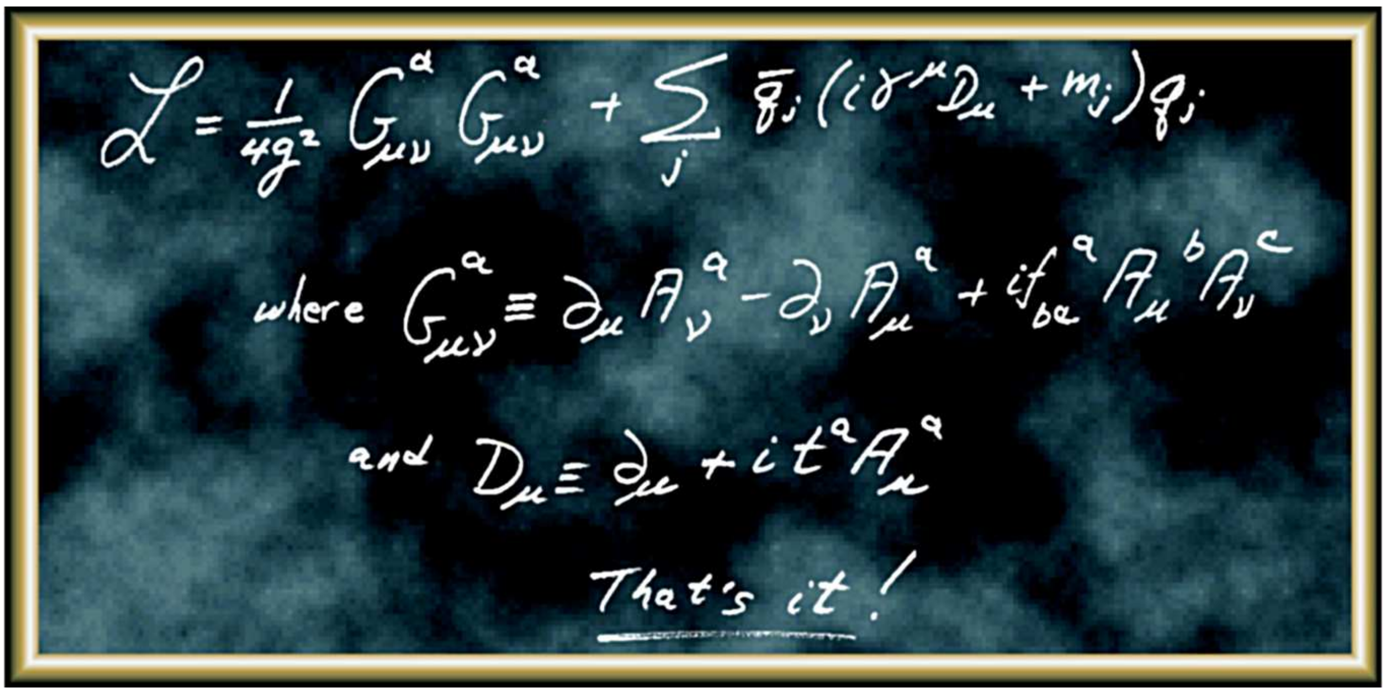
Leaving aside the **Gravitational** Interaction, which nobody really understands, we have the Electromagnetic and **Weak** interactions, which are now comfortably **united** in the **Electroweak** theory, and the **Strong** interaction, which is still **not united** with the rest and has unresolved problems. It is therefore the most interesting to study, at least for some of us. The tool for studying the strong interaction is hadron spectroscopy, and it is having a very exciting **renaissance**. Before I go into it, let me review the history of strong interactions very briefly.

As we all know, the first manifestation of the strong interaction was in nuclei. The binding energy/nucleon in nuclei is $\sim$8 MeV, as compared to the electromagnetic binding energy of electrons in an atom, which is of the order of 10 eV, i.e., a million times smaller. Hence, **the strong interaction**.

- At the beginning of the 20th century the only knowledge we had about the strong interaction was empirical, obtained from the experimental measurements of the properties of nuclei. Then Yukawa gave us the **pion**, and we tried to understand the nuclear strong interaction by the exchange of a pion between two nucleons, giving rise to **OPEP**, or the One Pion Exchange Potentials, and subsequently to **MPEP** and **OBEP**. However, two problems remained.

The success of the potentials was limited to energies below particle production threshold, i.e., ∼300 MeV. Further, the entire edifice was based on phenomenology. No fundamental understanding of the strong interaction was achieved.

- The situation changed with the discovery of quarks, the quark model of hadrons, including, of course, the nucleons, and then of the theory of Quantum Chromodynamics, or QCD(1973). We now believe that **QCD** is **the** theory of strong interactions. To quote Wilczek, "The entire strong interaction physics is contained in the QCD Lagrangian.'



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As true as Wilczek's statement may be, life is not simple for several reasons.

1. The QCD Lagrangian can not be solved analytically. It must be solved numerically by what has come to be called Lattice Gauge calculations.
2. Several constants of QCD, the masses of the six quarks (u, d, s, c, b, t) and the scale parameter Λ(QCD) must be supplied from outside.
3. Since the exact calculations must be made by numerical methods, the Lattice Gauge Calculations require larger and larger computing efforts, and unfortunately **transparency** to the underlying physics **is lost**.

It is often stated that given "enough" computing resources and manpower and time, all strong interaction problems can be solved by Lattice calculations of QCD, and we, experimentalists will become obsolete. Fortunately, the statement is about as true as **colonizing Mars** and mounting a mining industry there to solve the problem of the limited resources on Earth, and we are not in danger of losing our jobs. Besides, there are problems that Lattice can not handle, for example hadron form-factors for timelike momentum transfers, or making ^{208}Pb out of 624 quarks and gluons.

- Continuing with the history, before 1974 there were only three flavors of quarks, u, d, and s. In Dec. 1974 a narrow resonance with mass $\approx$ 3.1 GeV, the J/ψ, was discovered. The next issue of PRL had eight theoretical papers trying to explain J/ψ. Six of the eight papers, including those by Nobel Laureates, were wrong, but two recognized that J/ψ was a particle-antiparticle hadron, and the particle was a new quark, the **charm quark**.
- The only particle-antiparticle system known at that time was **positronium**, the electron-positron system bound by the $1/r$ Coulomb interaction which is mediated by the exchange of the vector photon. The natural suggestion was that $c\bar{c}$ were similarly bound (or glued together) by a **Coulombic interaction** mediated by the exchange of a vector (1^{--}) particle, appropriately named the **gluon**. However, since free quarks are not seen, it was suggested that the quarks are confined inside charmonium by an additional term in the potential called the **confinement potential**, proportional to a positive power of r. Thus the simplest representation of the strong $q\bar{q}$ interaction was born as the central **Cornell Potential**:

$$\mathbf{V}(\mathbf{r}) = -\frac{\mathbf{c}}{\mathbf{r}} + \sigma\mathbf{r}$$

- The Lagrangian formulation of an interaction is doubtlessly more powerful, but the potential model description of the interaction is more transparent and physical, even as the confinement potential is without precedent, and not well understood.

The replacement of the non-Abelian gauge-invariant field theory of QCD by a potential may appear far-fetched and presumptuous, but the fact is that the Potential Model predictions are unexpectedly successful.

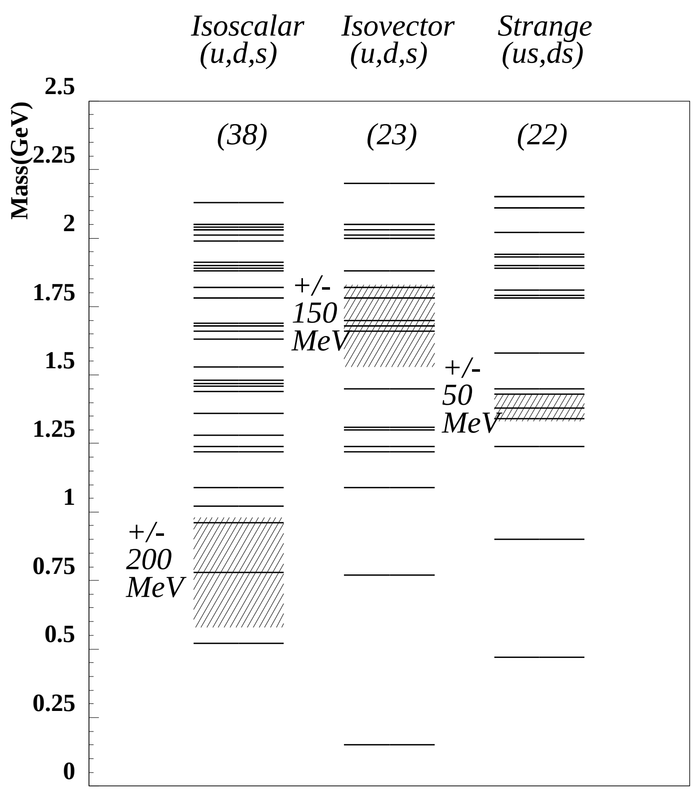
And what works should not be sneezed at.

In the following I will present a sampling of the latest experimental results in heavy quark spectroscopy, and compare them with potential model and lattice predictions.

Let me first explain why I confine myself to heavy quark spectroscopy. There are both experimental and theoretical reasons.

1. The constituent quark masses of the light quarks, u, d, and s are similar (300–500 MeV), so that the light quark hadrons almost always contain admixtures of all three in their wave functions. The result is that light quark states are very numerous, and have **large overlapping widths**, with average spacing $\sim$15 MeV and average width $\sim$150 MeV. This makes experimental spectroscopy very difficult.
2. There are important theoretical problems also. Although the $q\bar{q}$ interaction is flavor-independent, the quarks in light-quark hadrons are very **relativistic** ($\boldsymbol{v/c \sim 0.8}$) and the strong coupling constant is too large ($\boldsymbol{\alpha_S \geq 0.6}$) to make **perturbative treatment** feasible for light quark hadrons.
3. In contrast, in heavy quark hadrons both of the above problems are minimized ($\boldsymbol{v/c < 0.2}$**, and** $\boldsymbol{\alpha_S < 0.3}$), and spectroscopy is clean.

Light Quark Mesons

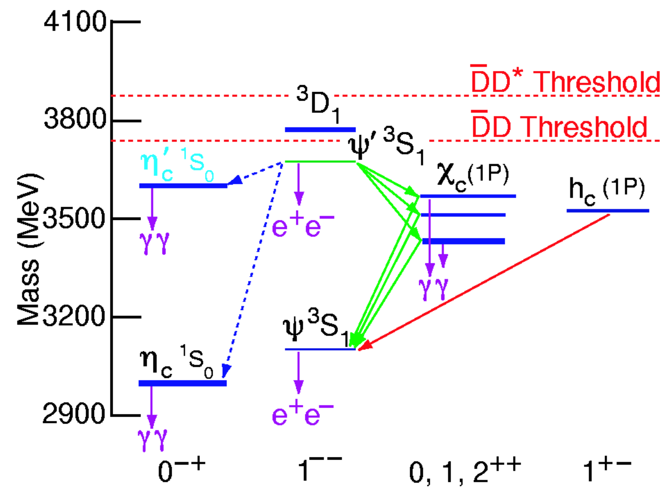


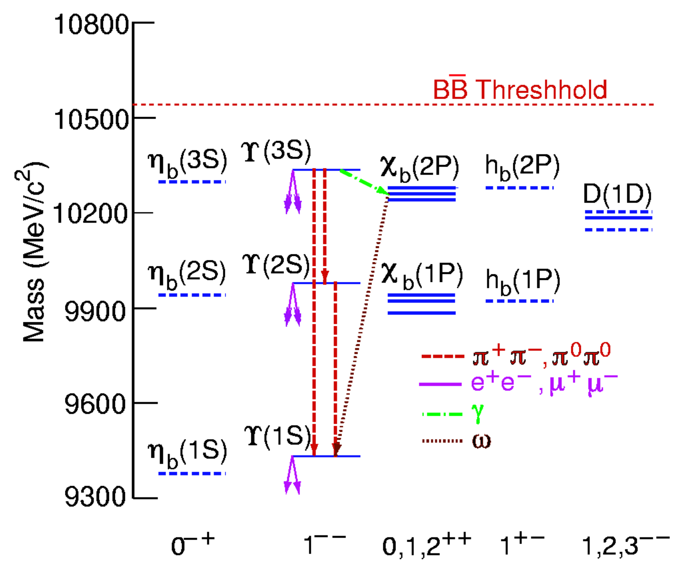
For $M > 1$ GeV

Average Spacing ≈ 15 MeV

Average Width ≈ 150 MeV

Charmonium and Bottomonium





We agree to defer to the **ultimate** lattice calculations to fit the experimental observables. But since we are not there yet, we depend on using QCD–based potential model predictions. It is therefore important to know how consistent with QCD these potentials are. I will present a brief review of this first.

- Then, I will talk about some recent experimental developments in the spectroscopy of **hadrons**, the **conventional $q\bar{q}$ mesons**.
- Then, I will go into **exotica**, i.e., hadrons which go beyond the conventional ones.

Let me begin with comparing potentials, the phenomenological **QCD–inspired potentials** with lattice–derived potentials.

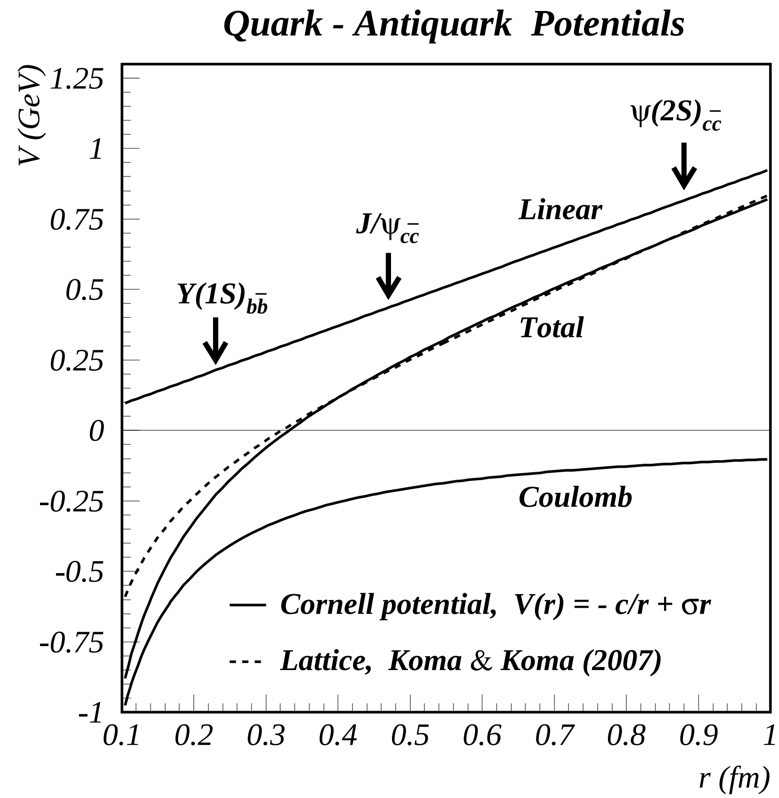
The commonly used **$q\bar{q}$ potentials for physical quarks** are constructed by by fitting the masses of some of the well measured states, usually the S–wave singlet and triplet states of $c\bar{c}$ charmonium and $b\bar{b}$ bottomonium. The **lattice calculations of $q\bar{q}$ potentials** are made in the **static approximation**, i.e., by assuming **infinitely heavy quarks**. In comparing the two types of potentials one has to keep this difference in mind. However, comparison at a qualitative level is very instructive.

Central Potentials

- The figure illustrates the comparison of the Cornell potential

$$V(r) = -\frac{c}{r} + \sigma r$$

with the **static lattice potential** obtained by Koma and Koma (KK)[1]



- It is gratifying to see that the the lattice potential has the general features of the Cornell potential, with both **Coulombic** and **confinement** parts. However, the lattice potential is **less singular** in the extreme Coulombic region, for $r < 0.2$ fermi, where there are no experimental data to constrain the potentials. This could be important, but we have to also keep in mind that the KK lattice potential has been obtained only in the **quenched approximation**.
- How about comparisons of the spin–dependent potentials?

Spin-Dependent Potentials

- The richness of hadron spectroscopy resides in its the spin-dependent features, and it is even more important to see how well the commonly used spin-dependent potentials compare with the predictions of lattice calculations.
- As in atomic physics, the non-relativistic reduction of the Bethe–Salpeter equation results into the familiar Breit–Fermi spin-dependent interaction which has **spin-orbit**, **tensor**, and **spin–spin** parts. Their contribution to the potential depends on the Lorentz structure of the kernel in the B–S integral.
- Both vector and scalar kernels give rise to spin-orbit potentials, but the tensor and spin-spin potentials arise only from the vector kernel. Further, the spin–spin potential for the vector kernel is a contact potential, finite only for S–waves.

  The potential models **assume** one gluon vector exchange Coulombic potential, and an essentially ad-hoc linear confinement potential which is generally **assumed** to be scalar.
- **Questions:** To what extent are these assumptions of the potential model calculations supported by experimental data, and to what extent do lattice calculations support these assumptions? The answers to these questions are important for our understanding of the strong interaction in the QCD era.

Without going into details, let me summarize the important conclusions reached by comparing the spin–dependent potentials obtained by KK in their lattice calculations with those obtained by assuming a purely vector, one–gluon kernel in the Bethe–Salpeter integral.

1. KK claim that in their static lattice calculation an important component $(V_1'(r))$ of the spin–orbit potential, is found to be large and finite instead of being zero. This leads them to the conclusion that "**something other than a one–gluon vector exchange** is needed in the Bethe–Salpeter kernel", This could be important.
2. Both tensor and spin–spin potentials show differences in the **extreme "Coulombic" region**, $\lesssim 0.1$ fm, reminiscent of the differences in central potentials in the same region.
3. There seems to be no evidence for a long–range $(r \gtrsim 0.5$ fm$)$ spin–spin potential. This would justify the assumption that the **confinement potential is Lorentz scalar**.

- The caveat that goes with these conclusions is that they are based on **quenched** lattice calculation of **static** potential.
- For a real comparison with QCD we have to go to the **lattice predictions for the actual observables**, and not through potentials. We do so in the following, with special reference to the hyperfine, or spin–spin interaction.

Quark–Antiquark Spin–Orbit and Tensor Interactions

These interactions first show up in the spin splitting of the triplet P–wave states ($^3P_2(\chi_2)$, $^3P_1(\chi_1)$, $^3P_0(\chi_0)$) of charmonium and bottomonium, whose masses have been measured with good precision. The quantities which are generally used to quantify these splittings are based on perturbative expectations:

Spin–Orbit: $\Delta M(\mathrm{SO}) \equiv (5M(\chi_2) - 3M(\chi_1) - 2M(\chi_0))/9$

Tensor: $\Delta M(\mathrm{T}) \equiv (-M(\chi_2) + 3M(\chi_1) - M(\chi_0))/9$

$\rho \equiv (M(\chi_2) - M(\chi_1))/(M(\chi_1) - M(\chi_0))$

$= 2\left[(\Delta M(\mathrm{SO}) - \Delta M(\mathrm{T}))/(\Delta M(\mathrm{SO}) + 5\Delta M(\mathrm{T}))\right]$

Many potential model calculations and several older quenched lattice calculations exist for P–wave splittings, and their predictions show large variations. However, the most recent Fermilab **unquenched** lattice calculation gives results in remarkable agreement with experiment (within 5–15%, as indicated in Table)

1P States	$\Delta M(\mathrm{SO})$—MeV	$\Delta M(\mathrm{T})$—MeV	ρ
Charmonium–expt.	46.61 ± 0.09	16.2 ± 0.07	0.475 ± 0.002
Charmonium–lattice	43.3 ± 6.6 ($\approx \mathbf{95}\%$)	15.0 ± 2.3 ($\approx \mathbf{95}\%$)	$0.48 \pm (< 0.10)$*
Bottomonium–expt.	18.2 ± 0.2	5.25 ± 0.13	0.58 ± 0.02
Bottomonium–lattice	16.9 ± 7.0 ($\approx \mathbf{93}\%$)	4.5 ± 2.2 ($\approx \mathbf{86}\%$)	$0.62 \pm (< 0.40)$*

* The errors in lattice predictions for $\Delta M(\mathrm{SO})$ and $\Delta M(\mathrm{T})$ are correlated, and they must be considerably less than the above limits obtained by treating them as completely uncorrelated.

Quark–Antiquark Hyperfine Interaction

The importance of the S–wave spin–spin interaction, or hyperfine interaction and the consequent triplet–singlet splitting can not be overemphasized. In QED it is responsible for the **21 cm** line which is the workhorse of microwave astronomy. In QCD it is always used for calibration of potential model parameters.

The spin-spin interaction determines the ground state masses of all hadrons. For $q\bar{q}$ mesons, the masses of the pseudoscalar ground states ($J^{PC} = 0^{-+}$) and the vector ($J^{PC} = 1^{--}$) states are given by

$$\boldsymbol{M(q_1\bar{q}_2, J)} = m(q_1) + m(q_2) + \frac{32\pi}{9}\alpha_S \left(\frac{|\psi(0)|^2}{m_1 m_2}\right)(\vec{s_1} \cdot \vec{s_2}), \quad s_1 + s_2 = S = J.$$

The hyperfine spin triplet-singlet splitting is

$$\boldsymbol{\Delta M_{hf}} = M(n^3S_1) - M(n^1S_0) = (32\pi\alpha_S/9)\,|\psi(0)|^2/m_1 m_2.$$

- The spin–dependent potentials we have been discussing are exclusively those which arise from the one gluon vector interaction in B–S kernel, and that is also what is assumed in potential model calculations. But that begs the question: **"What about the confinement potential?"** The confinement potential obviously does not arise from one gluon exchange, but it is ad-hoc assumed to be **scalar**, and therefore makes no contribution to the spin–spin interaction. Could it have a different origin and different spin–dependent character? We do not know. Only the experimental measurements of hyperfine splittings can tell us.

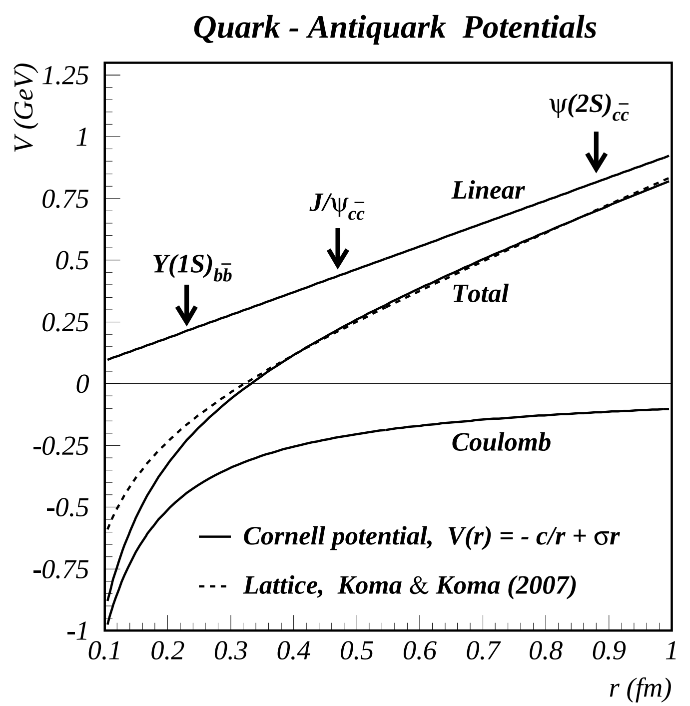
The Spin–Spin Interaction and the Confinement Potential

To put the question about the role of the confinement potential in the nature of the $q\bar{q}$ spin–spin potential in perspective, we note again that different $q\bar{q}$ states sample different regions of the $q\bar{q}$ potential with quite different levels of contribution from the Coulombic and confinement potentials. It ranges from being dominantly Coulombic for the bottomonium 1S states to dominantly confinement for the 2S charmonium states. This raises the following questions. **How does the hyperfine interaction change**

(a) with principal quantum number $\boldsymbol{n}$, for example between 1S and 2S states,

(b) between S–wave and P–wave states, e.g., between $\boldsymbol{\ell} = 0$ and $\boldsymbol{\ell} = 1$ states,

(c) with **quark masses**, e.g., between c–quark states and b–quark states?



Experimental Measures of the Hyperfine Interaction

The answers to the questions posed can be provided only by experimental data about hyperfine splittings. Unfortunately, there is a generic **experimental problem** in measuring hyperfine splittings,

$$\boldsymbol{\Delta M_{hf}(nL)} \equiv M(n^3L) - M(n^1L)$$

The problem is that while the triplet states are conveniently excited in e^+e^- annihilation, either directly (e.g., $^3S_1$) or via **strong E1** radiative transitions (e.g., ^3P_J), the radiative excitation of singlet states is either forbidden, or possible only with **weak M1** allowed $(n \to n)$ and forbidden $(n \to n')$ transitions.

As a consequence of these difficulties, while the spin triplet S– and P–wave states were identified early in the spectroscopy of charmonium and bottomonium, the identification of the singlet states has taken a tortuously long time.

- The identification of the first singlet state, $\eta_c(1^1S_0)_{c\bar{c}}$ took **6 years** and many false steps after the discovery of $J/\psi(1^3S_1)$, the identification of $\eta_c'(2^1S_0)_{c\bar{c}}$ state took **26 years**, the identification of $h_c(1^1P_1)_{c\bar{c}}$ took **29 years**, and the identification of the first singlet state in bottomonium, $\eta_b(1^1S_0)_{b\bar{b}}$ took **31 years**.
- But great progress **has been made in the last five years** with contributions from many laboratories. I do not have time to describe the details of these marathon efforts, but I do want to give you the important results.

Hyperfine Splitting of Charmonium Ground State

- $\Delta M_{hf}(1S)_{c\bar{c}} = M(J/\psi, 1^3S_1) - M(\eta_c, 1^1S_0) = 116.6 \pm 1.0$ MeV.

  This remains the best measured hyperfine splitting in a heavy quark hadron.

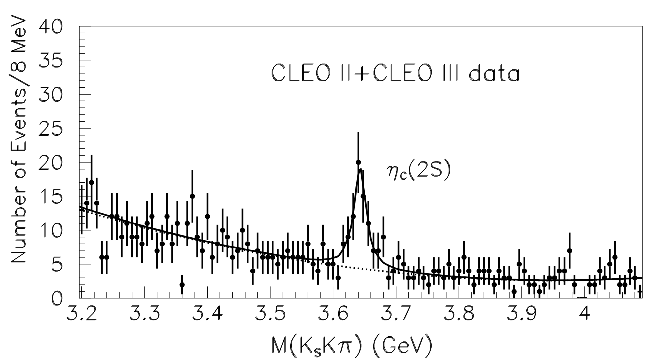
  The recent Fermilab **unquenched** lattice calculation predicts[2] the remarkably successful result $\Delta M_{hf}(1S)_{c\bar{c}} = 116.0 \pm 7.4^{+2.6}_{-0}$ MeV.

Hyperfine Splitting of Charmonium Radial Excitation

- $\Delta M_{hf}(2S)_{c\bar{c}} = M(\psi', 2^3S_1) - M(\eta_c', 2^1S_0) = 49 \pm 4$ MeV.

  η_c' was first identified in 2002 by Belle[3] in B–decay, and confirmed by its formation in two-photon fusion, and decay into $K_S K \pi$, by CLEO[4] and BaBar[5] in 2004.

  The figure shows the CLEO spectrum.



- This is the first measurement of hyperfine splitting in a radial excitation.

The fact that the charmonium 2S hyperfine splitting is a factor 2.5 smaller than that of 1S poses serious problems for theoretical understanding.

- There are numerous pQCD–based predictions for $\Delta M_{hf}(2S)_{c\bar{c}}$, and they range all over the map (and occasionally even hit 50 MeV). However, it is fair to say that nobody expected the 2S hyperfine splitting to be ~ 2.5 times smaller than the 1S hyperfine splitting. A **model–independent** prediction, relating 2S to 1S splitting, gives $\Delta M_{hf}(2S)_{c\bar{c}} = \mathbf{68 \pm 7}$ **MeV**, which is 40% larger than the measured value of 49 ± 4 MeV.
- It has been suggested that the smaller than expected 2S hyperfine splitting is a consequence of the 2S levels being very close to the $|c\bar{c}\rangle$ break-up threshold, and the consequent mixing with continuum levels. However, no definitive numerical predictions are available so far.
- The Fermilab unquenched lattice calculation which is successful in reproducing the 1S hyperfine splitting, is not able to reproduce the 2S hyperfine splitting with better than $\pm 100\%$ error. It ascribes its failure to confusion in distinguishing 2S charmonium levels with "nearby multiple open–charm levels".
- The continuum mixing problem would, of course, not exist for bottomonium 2S levels, but the experimental problem in determining bottomonium hyperfine splittings is a formidable one, even for 1S levels, as we shall see later.

The problem of hyperfine splitting in radial excitations remains open.

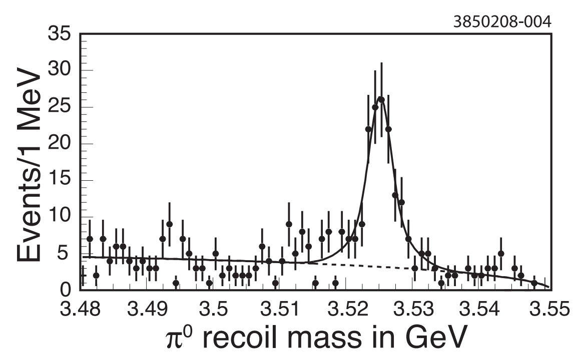
Hyperfine Splitting in P–waves

- $\Delta M_{hf}(1P)_{c\bar{c}} = M(1^3P) - M(h_c, 1^1P_1) = ?$
The masses of the spin–orbit split P–triplet states of charmonium, $\chi_J(1^3P_J)$ were measured with precision by the Fermilab $p\bar{p}$ annihilation experiments E760/E835 nearly twenty years ago, and their centroid,
$\langle M(^3P_J)\rangle = [5M(^3P_2) + 3M(^3P_1) + M(^3P_0)]/9 = 3525.30 \pm 0.04\,\text{MeV}.$
- The identification of $h_c(^1P_1)_{c\bar{c}}$ was, however, extremely challenging. Both its formation by radiative decay of ψ', and its decay to J/ψ are forbidden by charge conjugation invariance. Further, its formation by π^0 decay of ψ' is isospin violating and has very little phase space. Nevertheless, in 2005 CLEO[6] succeeded in identifying it in the latter reaction,

$$e^+e^- \to \psi'(2^3S_1)_{c\bar{c}} \to \pi^0 h_c(1^1P_1)_{c\bar{c}}, \qquad h_c(^1P_1) \to \gamma\eta_c(^1S_0)$$

and made a precision determination of its mass $M(h_c, ^1P_1) = 3525.28 \pm 0.22$ **MeV**. The figure illustrates the spectrum from the exclusive analysis of the CLEO data. If we identify the triplet centroid mass $\langle M(^3P_J)\rangle = 3525.30 \pm 0.04$ MeV with the true triplet mass $M(^3P)$, we get

$$\Delta M_{hf}(1P)_{c\bar{c}}(1P) = 0.02 \pm 0.22\ \text{MeV}.$$



- The theoretical expectation for a delta function spin–spin hyperfine interaction is indeed $\boldsymbol{\Delta M_{hf}(1P)_{c\bar{c}} = 0}$. It is therefore very tempting to assume that $\langle M(^3P_J)\rangle = M(^3P)$, and that $\Delta M_{hf}(1P)_{c\bar{c}} = 0.02 \pm 0.22$ MeV.
- But this identification can not be right because the centroid determination of $M(^3P)$ is only valid if the spin-orbit splitting is perturbatively small, and we have already noted that the perturbative prediction

$$\rho = \left[M(^3P_2) - M(^3P_1)\right] / \left[M(^3P_2) - M(^3P_1)\right] = 2/5 = \mathbf{0.4}$$

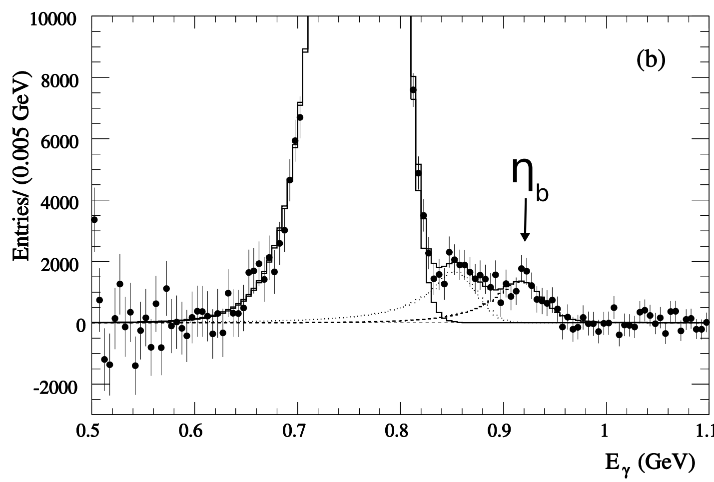
is in large disagreement with the experimental result, $\rho_{c\bar{c}} = \mathbf{0.475 \pm 0.002}$.

- This leads to serious questions.
  - What mysterious cancellations are responsible for the wrong estimate of $M(^3P)$ giving the expected answer that
$$\Delta M_{hf}(1P) = 0$$
  - Or, is it possible that the expectation is wrong? Is it possible that the hyperfine interaction is not entirely a **contact interaction**?
  - Potential model calculations are not of much help because they smear the potential at the origin in order to be able to do a Schrödinger equation calculation.
  - **Can Lattice help?** Not so far.
For example, the Fermilab lattice result[2] is that $\Delta M_{hf}(1P) \leq 10 \pm 10$ MeV.

Hyperfine Splitting with b–Quarks

- $\Delta M_{hf}(1S)_{b\bar{b}} = M(\Upsilon(1^3S_1)) - M(\eta_b(1^1S_0)) = 70.6 \pm 3.5$ MeV.

  Upsilon $\Upsilon(1^3S_1)$ was discovered in 1977, but it took 31 years to identify $\eta_b(1^1S_0)_{b\bar{b}}$. In 2008 BaBar[7] announced its discovery by identifying it in the inclusive photon spectrum for the radiative decay of $\Upsilon(1^3S_1)_{b\bar{b}}$. It was a tour-de-force analysis of the data for the radiative decay, $\Upsilon(3S) \to \gamma\eta_b$ of 109 million $\Upsilon(3S)$. Their spectrum is shown in below. The BaBar result has been recently confirmed in an independent measurement by CLEO[8]. Both experimental results are in agreement with the **unquenched** lattice predictions of $\Delta M_{hf}(1S)_{b\bar{b}} = 61 \pm 14$ MeV[9] and 54 ± 12 MeV[2].



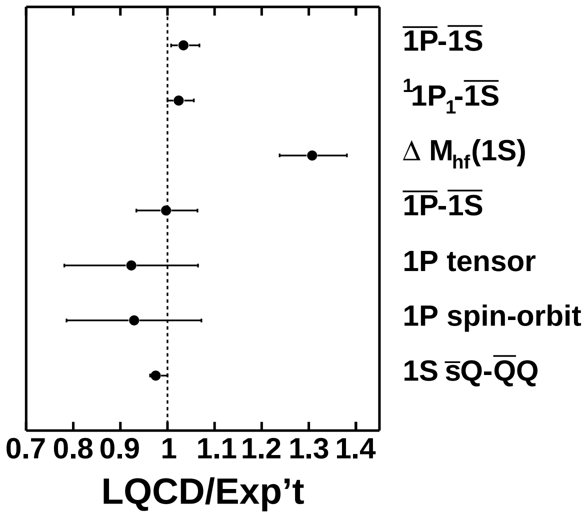
Successes & Limitations of Potential Models

- **In Potential Model** calculations the experimental masses of 1S states are used to determine potential parameters. For the predictions of the **masses** of radial excitations and P– and D–wave states, only broad agreement with the experiments is found. Detailed features like spin-orbit or spin-spin splittings are not well predicted. For unbound states the predictions become more uncertain.
- The potential model calculations do have the advantage over the present lattice calculations in their ability to predict **decay widths**, following the corresponding radiative decays in positronium. However, unlike positronium, the **first-order strong radiative corrections** do not work well for charmonium or bottomonium.
- Because of the large values of the strong coupling constant these corrections are often very large and produce absurd answers. For example, the correction factor for the decay $\chi_{c0}(^3P_0)_{c\bar{c}} \to \mathrm{glue}$ is $[1 + 9.5\alpha_S/\pi] = 1.91$ for $\alpha_S = 0.3$. A 91% correction in the first order is essentially **meaningless and unacceptable**.

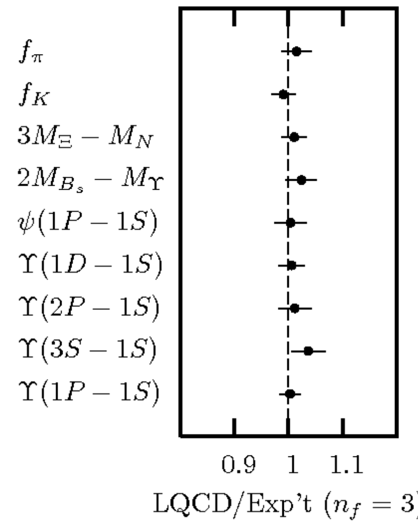
  Unfortunately, higher order radiative corrections are not yet available. I am told that it is now possible to make them, and it would be my strong request to the strong interaction community to make them.

Successes & Limitations of Lattice Calculations

For the first time, we now have rather successful Lattice calculations in the **unquenched** approximations with u, d, s sea quarks.



CHARMONIUM (FNAL/MILC)



BOTTOMONIUM (HPQCD/UKQCD)

Charmonium predictions still have large errors.

- Present successes are mostly limited to excitation energies. Results for a few transition widths are beginning to appear. For example, the HPQCD/UKQCD group obtains $\Gamma_{ee}(\Upsilon(1S)) = 1.28 \pm 0.01$ keV and $\Gamma_{ee}(\Upsilon(2S)) = 0.67 \pm 0.03$ keV, as compared to the experimental results of 1.34 ± 0.02 keV and 0.612 ± 0.011 keV. Similar results for charmonium are not yet available.

Before I leave the conventional "hadrons", and go to "exotica", let me address a conventional hadron dear to all of us, nuclear & particle physicists, the **proton**, the only hadron which never dies, and makes up most of the mass of the visible universe.

Proton Structure

Now that we know that the proton is made of three quarks, the simplest next thing we want to know is what are these quarks doing there. What is their spatial distribution, what is their momentum distribution?

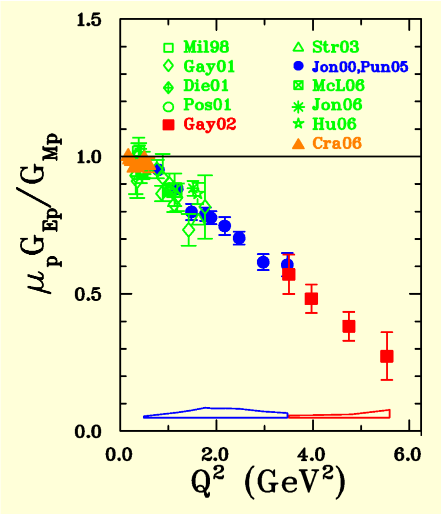
While we all wait for the much acclaimed Generalized Parton Distributions (GPDs) to tell us something, form factors remain the true and tried means of learning what the protons look like.

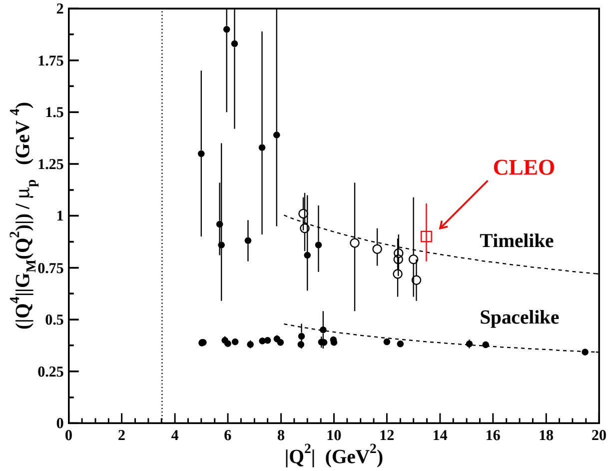
All the textbooks have been assuring us that measurements of elastic scattering of electrons from protons, especially those done at SLAC, and analyzed by the Rosenbluth method, tell us accurately what the electric and magnetic form factors of the proton look like as function of momentum transfer, which is spacelike ($|Q^2|$ positive) in these measurements.

Recently, we have had two major upsets in our understanding of proton form factors.

- JLab measurements have shown us that $G_E(p) \neq G_M(p)$, but $G_E(p)/G_M(p)$ monotonically decreases with Q^2, and becomes as small as $G_E/G_M \approx 0.3$ by $Q^2 \approx 6$ GeV^2.
- Fermilab and CLEO measurements show that $G_M(p)$ for timelike momentum transfers ($|Q^2|$ negative) are **factor two larger** than for corresponding spacelike momentum transfers, even at $|Q^2|$ as large as ~ 15 GeV^2, when they are expected to be equal.

There is really no theoretical understanding of either of these observations. **The proton is apparently not a simple system**, with identical space and momentum distributions of its 3 quarks.





Exotica

The pursuit of **Exotica** holds the same fascination for physicists as **Erotica** does for non–physicists. Both pursuits involve great excitement and frequent disappointments and frustrations. But, it is difficult to shake off the **addiction**.

QCD dictates that only **color neutral hadrons** may exist freely. Hence we have 3–quark baryons and $q\bar{q}$ mesons. But that would imply that other color–neutral objects like

6–quark **dibaryons**,

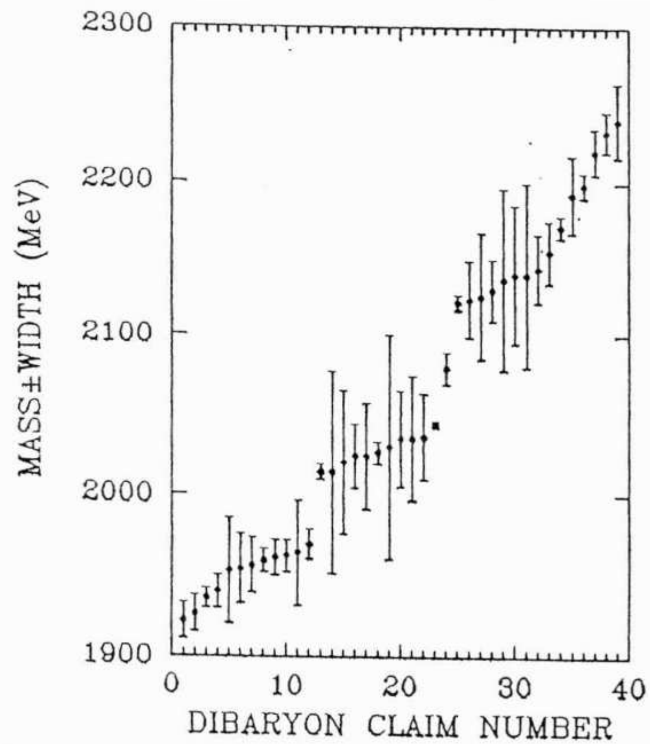
$3q3\bar{q}$ **baryonia**,

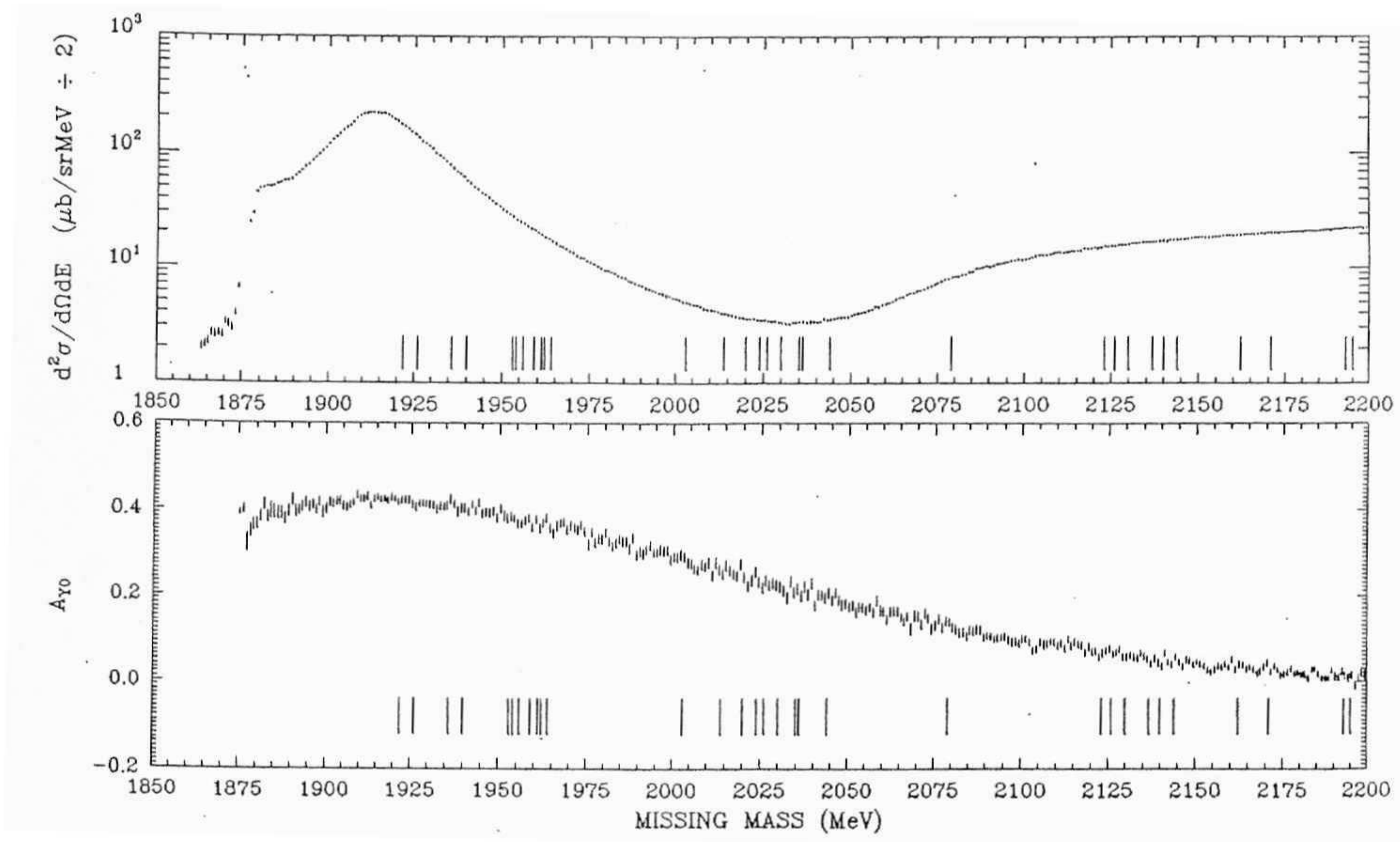
and $4q - 1\bar{q}$ **pentaquarks**

may also exist. The search for such constructs constitutes the **exotica** of hadron physics. Let me give you a birds' eye–view of this terrain.

Dibaryons

The possibility of dibaryons was first put forward in the context of the MIT bag model of hadrons, and very detailed predictions of scores of six–quark (u, d, s) dibaryons were made in 1980. This caused a stampede of experimental claims. Within a few years, claims for as many as 40 dibaryons with masses between 1900 and 2250 MeV were made by enthusiasts. None of them have survived as a result of high statistics, good energy resolution measurements, many of them made at LAMPF. Here's an example.





$\vec{p} + d \to p' + X$ at $T_p = 800$ MeV, $\Theta = 15°$: **No Dibaryons anywhere!**

The H Dibaryon

The $\boldsymbol{uuddss}$ H dibaryon was predicted by Jaffe. Stubborn searches for the H were made for years at Brookhaven and KEK. The u, d quark dibaryons died a long time ago, but the H dibaryon lived longer. By now, however, by common consensus it is also considered dead. It has only 18,000 entries in Google.

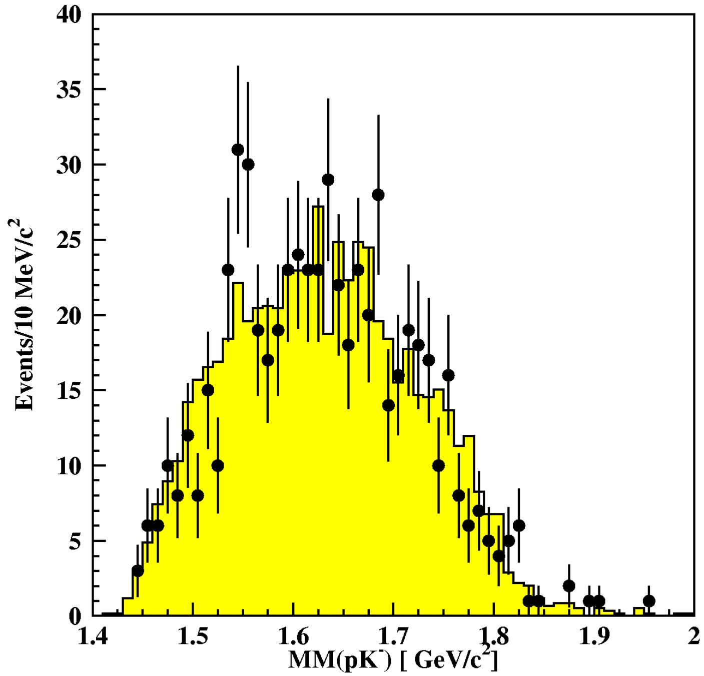
Pentaquark

The pentaquark surfaced in 2003 by the claim by Japanese physicists (Nakano et al.) of a narrow peak, called Θ^+, with a mass of $M(\Theta^+) = 1540 \pm 10$ MeV, $\Gamma(\Theta^+) < 25$ MeV, in the invariant mass of K^+n in the reaction $\boldsymbol{\gamma n \to K^-(K^+n)}$. If true, it would have strangeness $+1$, and at least five quarks/antiquarks, $|uud.d\bar{s}\rangle$.

The pentaquark is so exotic that a stampede of **confirming** claims flooded the literature, including one from the Jefferson Lab (JLab). In a little more time, an equal number of **non-observations** were reported, including one from JLab contradicting their earlier observation. If you go to Google, you find 32,600 entries (used to be 99,800 in 2005) for **Pentaquark**, and it will be difficult to decide whether the pentaquark is alive or dead!

In a high-statistics repeat of their own measurement, JLab found that their own earlier observation of Θ^+ was false and no evidence for the existence of the pentaquark exists.

Dots: Original measurement by JLab
Colored histogram: Large statistics repeat measurement by JLab



So, once claimed, an exotic is difficult to kill!

I end with a quote from PDG08 summarizing the saga of the pentaquark

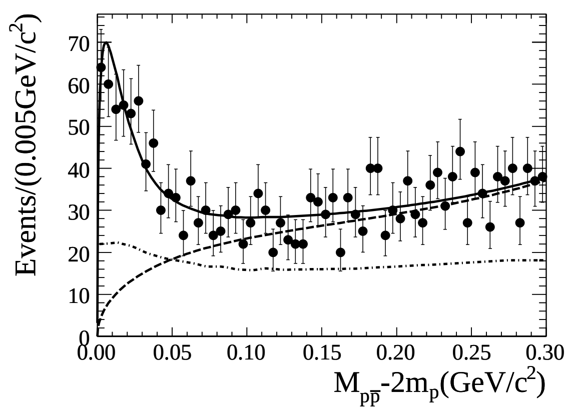
> **"The whole story — the discoveries themselves, the tidal wave of papers by theorists and phenomenologists that followed, and the eventual 'undiscovery' — is a curious episode in the history of science."**

Another "exotic" down the tubes!!

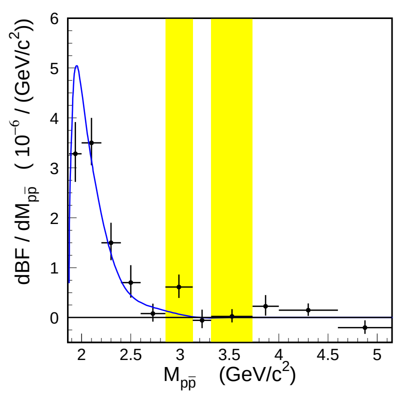
Baryonium

In the 1980's, great excitement was generated for the possible existence of a bound state of proton and antiproton, the $|p\bar{p}\rangle$ baryonium. Successively better measurements at CERN disproved prior claims, and baryonium appeared to have been put to rest. However, very recently it has been trying to rise from the dead. In measurements as diverse as B–decays to several final states containing a $p\bar{p}$ pair and $e^+e^- \to \gamma p\bar{p}$, enhancements have been reported **at threshold**, $M(p\bar{p}) \approx 2m_p$. I show two examples.

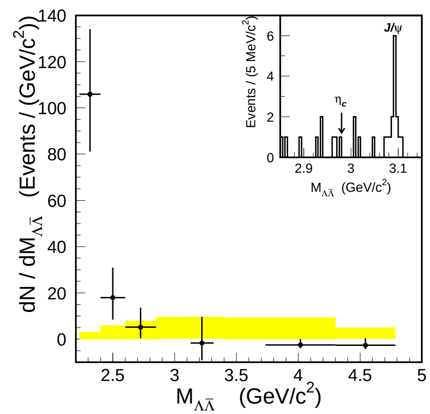
Unfortunately, the interpretation of these threshold enhancements as baryonium has become doubtful because similar threshold enhancements have been reported in a number of different final states, $J/\psi\phi$, $p\overline{\Lambda}$, $\Lambda\overline{\Lambda}$, etc. It is likely that they are manifestations of reaction dynamics rather than resonances.



BES[10]: $J/\psi \to \gamma p\bar{p}$



Belle[11]: $B^+ \to p\bar{p}\pi^+$



Belle[12]: $B^+ \to \Lambda\overline{\Lambda}K^+$

Glueballs and Hybrids

- Quarks and Gluons both carry color. And color is in QCD what charge is in QED. It is **color that carries the strong interaction.**
- And, as stated before, the one absolute rule that follows is that

  Only colorless hadrons can exist in nature.

- Since gluons also carry color, perhaps we can have colorless hadrons containing valence gluons:

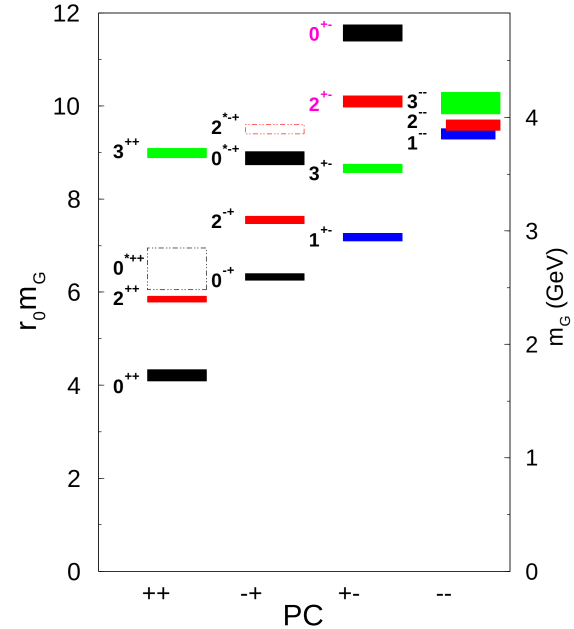
  $|gg\rangle$ **glueballs,** $|q\bar{q}g\rangle$ **hybrids**

- Once again, because theory suggested their possible existence, experimentalists like me rushed to find these exotic objects—hadrons without quarks.

  So what happened?

Glueballs

- Glueballs with various spins have been predicted.
  A lattice QCD based prediction of Glueballs →
- The lowest two glueballs predicted are
  $M(0^{++})$ ≈**1700 MeV**, $M(2^{++})$ =**2400 MeV**.
  These have been extensively searched for.
- The $\mathbf{0^{++}}$ **glueball** has been difficult to distinguish from normal 0^{++} scalar mesons. It could be hiding, mixed up with 0^{++} mesons at 1370, 1500, and 1710 MeV.
- The narrow $\mathbf{2^{++}}$ **glueball** with $M(2^{++}) = 2230$ MeV
  was **suggested** by a SLAC measurement (1986),
  killed by Orsay (Paris)(1988),
  revived by BES I (Beijing)(1996),
  killed by us at CB at CERN (Geneva)(2001),
  disappeared by BES II (Beijing)(2003),
  resurrected by ITEP (Moscow)(2006),
  and **reburied** by us at CLEO (Cornell)(2008).



In summary: **Pure glueballs do not exist. Mixed glueballs are difficult to identify.**

Another disappointment in search of Exotica.

$q\bar{q}g$ Hybrids

Glueballs are certainly exotic, but the fact that they have the same quantum numbers, J^{PC}, as normal $|q\bar{q}\rangle$ mesons, makes their distinction from normal $q\bar{q}$ mesons difficult, if not impossible.

- Hybrids, $|q\bar{q}g\rangle$ have an advantage. Normal $|q\bar{q}\rangle$ mesons **must have**

  parity, $P = (-1)^{l+1}$, charge conjugation, $C = (-1)^{l+s}$,

  i.e., normal $|q\bar{q}\rangle$ mesons **cannot have**

  $$J^{PC} = 0^{--},\ 1^{-+},\ 2^{+-}, \ldots$$

  But, **hybrids can**. So if you find a hadron with one of these J^{PC}, you have found a **manifestly exotic** object.

- The search for a 1^{-+} meson has been going on for nearly 30 years since Gell–Mann stated that they **must exist**.

1^{-+} Hybrids

The discovery of a **manifestly exotic** 1^{-+} meson depends completely on a firm determination of its J^{PC}. It involves a complicated method of analysis of the data for its component partial waves, and finding a resonance in the 1^{-+} wave.

- In 1997 we announced the discovery of a 1^{-+} meson with a mass $\boldsymbol{M(1^{-+}) = 1370 \pm 50}$ **MeV**. It caused tremendous excitement, with notices in the New York Times, Scientific American and elsewhere.
- Then we went on to discover two more 1^{-+} states at

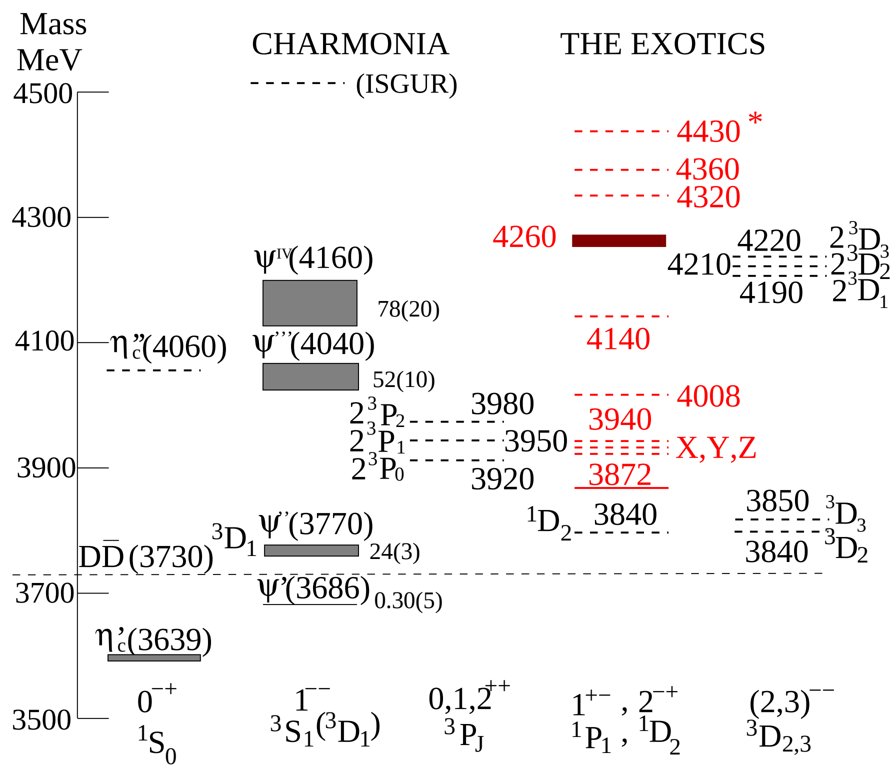
$$\boldsymbol{M(1^{-+}) = 1593 \pm 50 \text{ MeV}, \quad \text{and} \quad M(1^{-+}) = 2014 \pm 30 \text{ MeV}}$$

  The 1593 MeV 1^{-+} hybrid has been recently confirmed by COMPASS.
- The trouble with making an exotic claim is that it invites challenges. Nobody doubts that our three 1^{-+} mesons are exotic, i.e., non–$|q\bar{q}\rangle$. But there is plenty of discussion about whether they are $|q\bar{q}g\rangle$ hybrids, for they could also be 4–quark states.

The Renaissance in Hadron Spectroscopy: The Exotics

Let me now turn to the "Renaissance" in the title of my talk. This refers to the unexpected, and therefore "exotic" states recently discovered above the charmonium break-up threshold at 3.73 GeV by Belle and BaBar, and later by CDF, DØ, and CLEO.

- These states do not generally fit in the charmonium spectrum, but are often called "charmonium-like", because they seem to always decay into final states containing a charm and an anticharm quark.
- There are by now more than half a dozen of them, and they go by X,Y,Z,X′,X″,X‴,Z′. The alphabet soup is getting thicker by the day.



The Veteran of Exotics—X(3872)

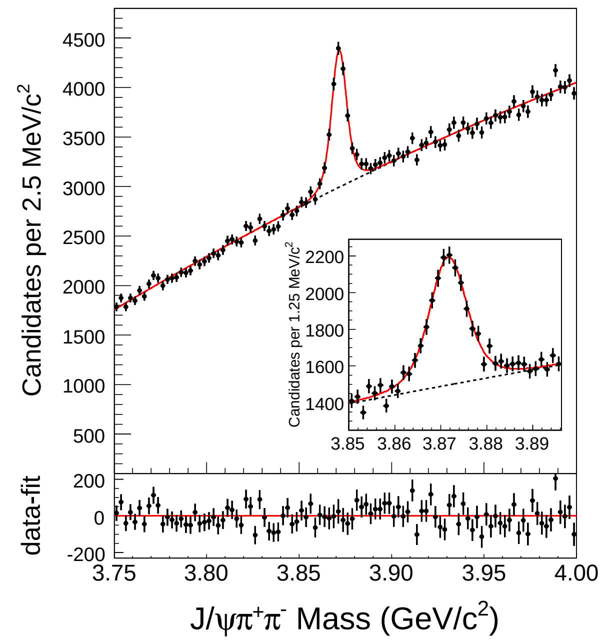
- In 2003, Belle[13] reported a very narrow peak with 37 counts in the decay, $B^- \to XK^-$, $X \to \pi^+\pi^- J/\psi$, and X(3872) was born. Very soon it was confirmed by BaBar[14], CDF[15] (6000 counts), and DØ[16], and by now it has been observed in many decay modes. Its measured parameters are:

  Mass$= \mathbf{3871.56 \pm 0.21}$ **MeV**, **Width**$= \mathbf{1.34 \pm 0.64}$ **MeV**, $\boldsymbol{J^{PC} = 1^{++}}$

- X(3872) decays a factor 10 more strongly to $\overline{D}^{*0} D^0$ than to its discovery mode $J/\psi\pi^+\pi^-$, and its mass is very close to $M(D^0) + M(D^{*0})$. This has given rise to its interpretation as a $\boldsymbol{\overline{D}^{*0} D^0}$ **molecule**.

- CLEO[17] has made a precision measurement of $M(D^0)$. It leads to the binding energy, $\mathbf{BE(X(3872)) = 0.14 \pm 0.28\ MeV}$.

- If the picture of X(3872) as a very weakly bound $\overline{D}^{*0} D^0$ molecule is correct, a very exciting new chapter of **hadronic molecules** has been opened. However, we should keep open its interpretation as the $\mathbf{2^3 P_1}$ **state** of charmonium as a possibility.



The Strange Vector, Y(4260)

The **Y(4260)** has been observed in ISR production

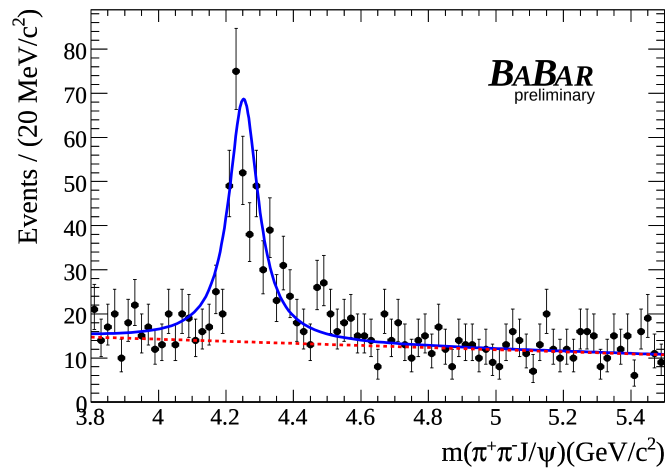
$$e^+e^- \to \gamma_{\rm ISR} e^+e^- \to \gamma_{\rm ISR} {\rm Y}(4260), \qquad {\rm Y}(4260) \to \pi^+\pi^- J/\psi$$

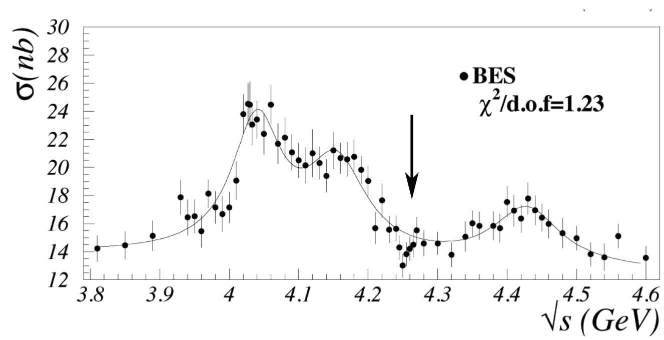
by BaBar[18], CLEO[19] and Belle[20], and in direct production by CLEO[21].

- Y(4260) is clearly **a vector** with $J^{PC} = 1^{--}$, but a very **strange** one, since it sits at a very deep minimum in $R \equiv \sigma(h)/\sigma(\mu^+\mu^-)$, with

  $$\boldsymbol{M({\rm Y}(4260)) = 4252 \pm 7\ {\rm MeV},}$$
  $$\boldsymbol{\Gamma({\rm Y}(4260)) = 105 \pm 19\ {\rm MeV}}$$

  Is Y(4260) a charmonium vector, perhaps 2^3D_1? If not, **what is it?**





- It is suggested that Y(4260) is a $c\bar{c}g$ charmonium **hybrid**.
  If so, where are the 0^{-+} and 1^{-+} hybrids companions predicted by quenched lattice NRQCD?

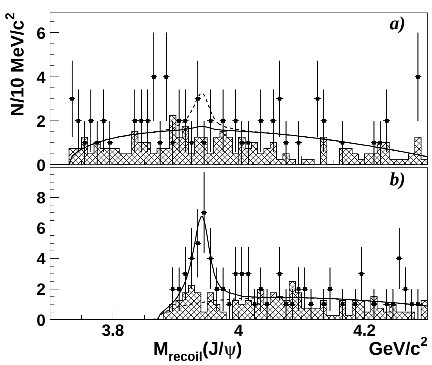
The Saga of X,Y,Z($\sim$3940)

Between 2004 and 2006, Belle reported three new states with very similar masses, ~ 3940 MeV. Besides nearly identical masses, they had other unusual properties.

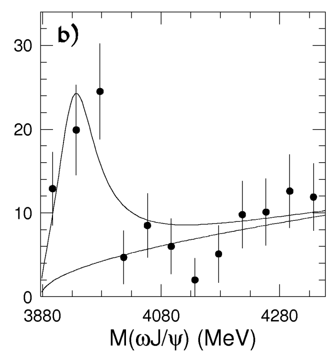
- The three were formed in different reactions
- The three decayed in different final states, but all containing a c and a $\bar{c}$ quark.
- Unfortunately, all three were observed as peaks with poor statistics.

While these gave rise to great excitement, they also made many of us skeptical about their separate reality.

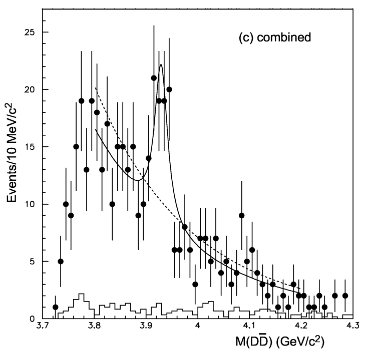
- It has been more than five years since the claims of X,Y,Z.
  Where do we stand now? Are they real? If real, what are they?



X(3940)



Y(3940)



Z(3940)

About X(3940)

X(3940) was observed[22] in $e^+e^-(10.6\ \text{GeV}) \to J/\psi + X$ (double charmonium)

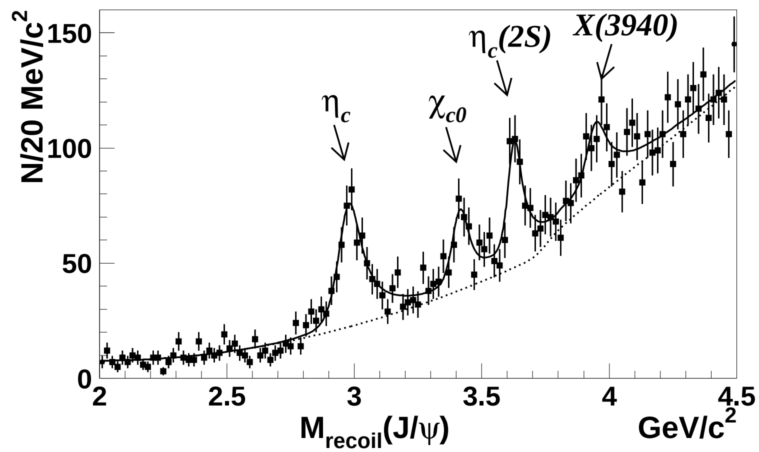
It was found to decay in $D\overline{D^*}$.

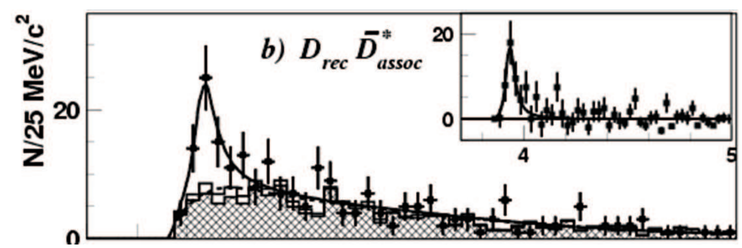
$\boldsymbol{M(\text{X}(3940)) = 3943 \pm 9}$ **MeV,** $\boldsymbol{\Gamma < 52}$ **MeV**

- Its spin was not specified, but is conjectured to be $J = 0$ because only $J = 0$ states, η_c, χ_{c0}, η_c' seem to be excited in double-charmonium production.

  This resonance remains unconfirmed by BaBar.

  So, it is meaningless to speculate whether X(3940) is η_c'' which is predicted to have a mass 100–130 MeV higher.





About Y(3940)

- This resonance was reported by **Belle**[23] in the reaction

$$B \to KY, \quad Y \to \omega J/\psi, \qquad \text{with } 58 \pm 11 \text{ counts}$$

$$\boldsymbol{M(\mathrm{Y}) = 3943 \pm 11 \pm 13} \textbf{ MeV}, \quad \boldsymbol{\Gamma(\mathrm{Y}) = 87 \pm 22 \pm 26} \textbf{ MeV.}$$

- Recently, **BaBar**[24] has reported it in the same reaction with 1980^{+396}_{-379} counts.
  $\boldsymbol{M(\mathrm{Y}) = 3914.6^{+3.8}_{-3.4} \pm 2.0}$ **MeV**, $\boldsymbol{\Gamma(\mathrm{Y}) = 34^{+12}_{-8} \pm 5}$ **MeV.**
  The mass and width are different but statistically consistent with Belle's.

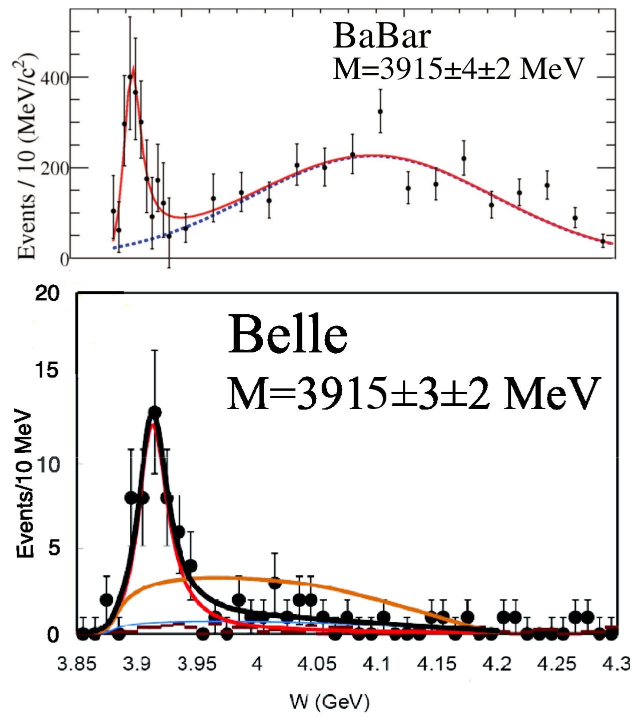
- A further confirmation of this resonance has been now reported by Belle[25] in the reaction

$$\gamma\gamma \to \omega J/\psi, \quad \text{with } 49 \pm 14 \pm 4 \text{ counts}$$

$$\boldsymbol{M(\mathrm{Y}) = 3915 \pm 4} \textbf{ MeV, } \boldsymbol{\Gamma(\mathrm{Y}) = 17 \pm 10} \textbf{ MeV.}$$

- So, this resonance appears to be real, and has $J^{PC} = J^{++}$. Is it the **charmonium** $\boldsymbol{2^3P_0(3920)}$ state?

- Or is it an exotic? A $|c\bar{c}g\rangle$ **hybrid**, or a rather deeply bound $D\overline{D}^*$ molecule?

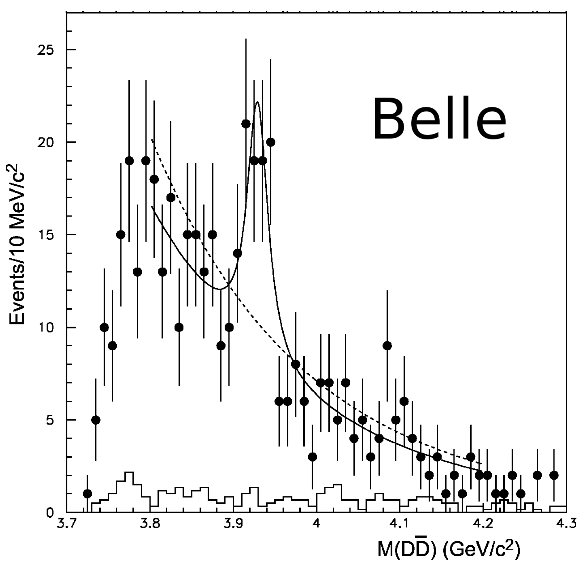


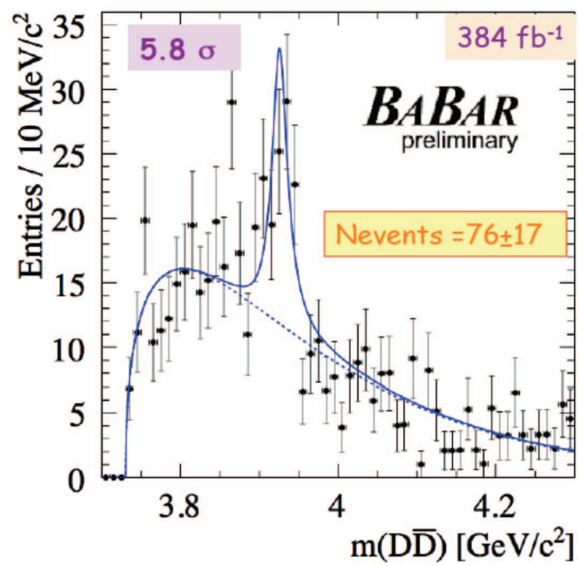
About Z(3930)

This resonance was reported by Belle[26] with formation in $\gamma\gamma$ fusion and decay in $D\overline{D}$, $\gamma\gamma \to \mathrm{Z}(3940) \to D\overline{D}$. It was recently confirmed by BaBar[27] in the same reaction.

$$\begin{aligned} M(\mathrm{Z}) &= 3929 \pm 5 \pm 2 \text{ MeV (Belle)}, & 3926.7 \pm 2.9 \pm 1.1 \text{ MeV (BaBar)} \\ \Gamma(\mathrm{Z}) &= 29 \pm 10 \pm 2 \text{ MeV (Belle)}, & 21.3 \pm 6.8 \pm 3.6 \text{ MeV (BaBar)} \\ N(\mathrm{Z}) &= 64 \pm 18 \ \text{ (Belle 395 fb}^{-1}\text{)}, & 76 \pm 17 \ \text{ (BaBar 384 fb}^{-1}\text{)} \end{aligned}$$

- This is now the best confirmed of the three X,Y,Z resonances.
- Both Belle and BaBar find that its spin $J = 2$.
- Z(3930) is a candidate for 2^3P_2 state of charmonium, but this is difficult if Y(3914) is 2^3P_0. A 15 MeV $^3P_0 - 3P_2$ splitting is rather unlikely.





Exotics? — Unconfirmed & Rare

CDF(2009)[28]: $B^+ \to K^+\mathrm{X},\ \mathrm{X} \to J/\psi\phi$

$\boldsymbol{M(4140)} = 4143.0 \pm 3.1$ MeV $\Gamma = 11.7^{+9.1}_{-6.2}$ MeV, $N = 14 \pm 5$

Belle(2010)[25]: M(4140) not seen in the same reaction, or in $\gamma\gamma$

Belle(2009)[25]: $\gamma\gamma \to J/\psi\phi$

$\boldsymbol{M(4350)} = 4350.6 \pm 5.1$ MeV $\Gamma = 13^{+18}_{-9} \pm 4$ MeV, $N = 8.8^{+4.2}_{-3.2}$

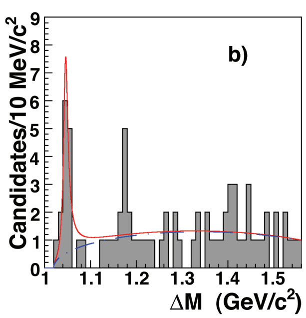
Babar(2009)[29]: $e^+e^- \to \gamma_{\mathrm{ISR}}\mathrm{X},\ \mathrm{X} \to \pi^+\pi^- J/\psi$

$\boldsymbol{M(4320)} = 4324 \pm 24$ MeV $\Gamma = 172 \pm 33$ MeV, $N \approx 68$

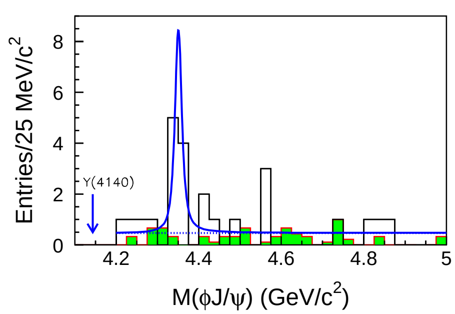
Belle(2007)[30]: $e^+e^- \to \gamma_{\mathrm{ISR}}\mathrm{X},\ \mathrm{X} \to \pi^+\pi^- J/\psi$

$\boldsymbol{M(4360)} = 4361 \pm 13$ MeV $\Gamma = 74 \pm 18$ MeV, $N =\sim 45$

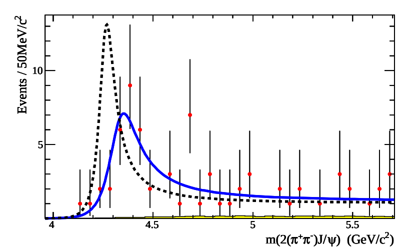
$\boldsymbol{M(4660)} = 4664 \pm 12$ MeV $\Gamma = 48 \pm 15$ MeV, $N =\sim 35$



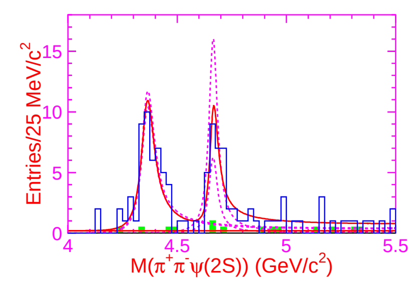
CDF–$M(4140)$



Belle–$M(4350)$



BaBar–$M(4320)$



Belle–$M(4360, 4660)$

The Super Exotics

All the exotic states I have so far talked about are uncharged. Below 5 GeV the only charged mesons which are known are either entirely made of the (u, d, s) light quarks or a light quark and a charm quark (the $D-$mesons).

So a charged exotic with mass between 3 GeV and 5 GeV would indeed by a **super exotic**.

- Two years ago, **Belle**(2007)[31] dropped a bombshell of a claim of observing a charged exotic, the $Z^+(4430)$

$$B^0 \to K^{\mp}Z^{\pm}, \quad Z^{\pm} \to \pi^{\pm}\psi(2S)$$

$$\boldsymbol{M(Z^+) = 4433 \pm 4 \pm 2 \text{ MeV}, \ \ \Gamma(Z^+) = 45^{+18+30}_{-13-13} \text{ MeV}, \ \ N = 121 \pm 30 \text{ evts}}$$

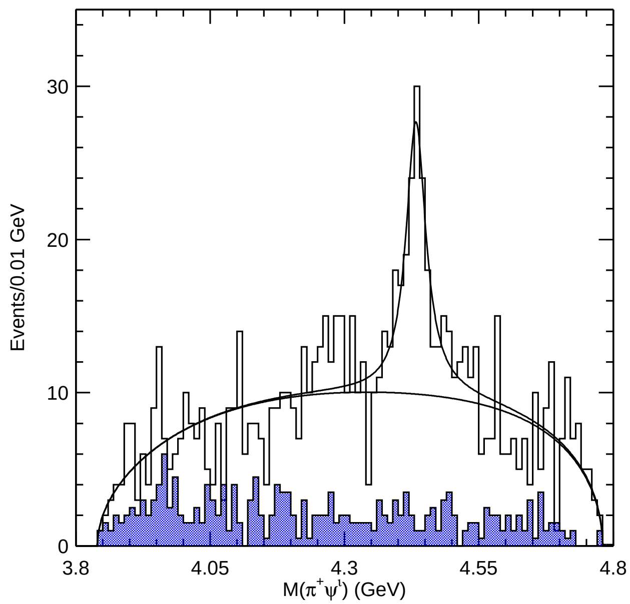
  If true, this would be a fantastic discovery, opening a new chapter in hadron spectroscopy.

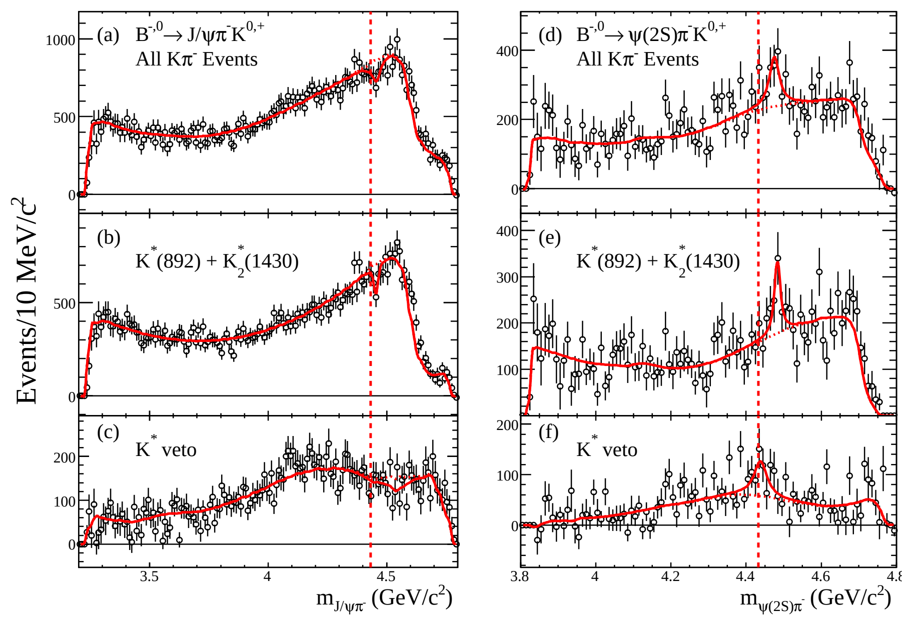
- BaBar(2009)[32] has searched for the Z^- decaying to $\pi^- J/\psi$ and $\pi^-\psi(2S)$, done very detailed Dalitz plot analyses, and finds **no statistically significant evidence** for the charged Z.

- Belle[33] has also announced two more charged exotics with masses of 4051 and 4248 MeV observed in the reaction

$$B^0 \to K^- Z^+, \qquad Z^+ \to \pi^+ \chi_{c1}$$

  but it does not make sense to dwell on these until the dust about $Z^+(4433)$ settles.





$Z^{\pm} \to \pi^{\pm}\psi(2S)$ $\qquad Z^{-} \to \pi^{-}J/\psi$ $\qquad Z^{-} \to \pi^{-}\psi(2S)$

Belle, $M(Z) = 4433 \pm 5$ MeV $\qquad$ BaBar, no evidence for $Z(4433)$

Summarizing the Exotics

As many as a dozen new states have been reported in the 1 GeV mass region, 3.8 GeV to 4.8 GeV.

- The evidence for some of them is shaky, and not all of them may eventually survive. But many are firmly established.
- The states are variously populated in $B-$decays, two–photon fusion, and ISR e^+e^- annihilation.
- They all decay in final states containing a charm and anticharm quark, as J/ψ, $\psi(2S)$, or $D\overline{D}$.
- Their masses and widths do not fit **easily** in the predicted spectrum of chamrmonium states, hence the label **exotic**, and the proposals to identify them as **hadronic molecules**, $q\bar{q}g$ **hybrids**, **four quark states**, etc.
- There are no firm proofs of the exotic explanations, but some are more likely than others.
- Even if only a few of these survive as true exotics, they will open new chapters in hadron spectroscopy. **A true Renaissance indeed!**

[1] Y. Koma and M. Koma, NP B769, 79 (2007)
[2] T. Buch et al., PRD 81, 034508 (2010)
[3] Belle, PRL 91, 262001 (2003)
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