HYPERFINE INTERACTION IN HEAVY QUARKONIA

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Introduction

The spectroscopy of a composite system of fundamental particles becomes richer by the presence of spin. For mesons spin–dependent interactions lead to four–fold increase in the richness. Classically, the spin–dependent interactions are classified as **spin–orbit, spin–spin, and tensor**. Of these the spin–spin $\boldsymbol{\vec{s}_1 \cdot \vec{s}_2}$ interaction, also called the hyperfine interaction, is perhaps the most important one because it gives rise to the ground states of all mesons. It is the hyperfine interaction between heavy quarks that I am going to talk about.

The existence of a spin-spin interaction between the spin of a nucleus and that of an electron was first proposed by Pauli in 1924. For two spin$=1/2$ particles the $\vec{s}_1 \cdot \vec{s}_2$ interaction gives rise to **the hyperfine splitting between $\boldsymbol{S = \vec{s}_1 \cdot \vec{s}_2 = 0}$ and $\boldsymbol{1}$ levels**. The most famous of these is the hyperfine splitting in hydrogen with the transition between the two levels giving rise to the famous **$\boldsymbol{\lambda = 21}$ cm (1420 MHz) line** which is the staple of radio-frequency astronomy. We will not be talking about it, but about the similar transitions between the spin-triplet $(S = 1)$ and spin-singlet $(S = 0)$ states in charmonium and bottomonium.

At the tree-level in QED and QCD a non-relativistic reduction of the Bethe–Salpeter equation makes the hyperfine interaction as a contact interaction, with the result that it is **finite** only for $L = 0$ S–wave states, **and zero** for states of all higher L.

Thus, hyperfine splitting for S–wave is [1]

$$\Delta M_{hf}(nS) \equiv M(n^3S_1) - M(n^1S_0) = (32/9)\pi\alpha_S|\psi_n(0)|^2/m_q^2 \quad (1)$$

For P–wave states at **one–loop level** Pantaleone and Tye [2] obtained a small correction to the zero splitting,

$$\Delta M_{hf}(1P) = [M(^3P) - M(^1P)] = -\frac{3}{4}[M(^3P_2) - M(^3P_0)]\left(\frac{10\alpha_S}{81\pi}\right) \quad (2)$$

which amounts to a sub–MeV splitting.

- Note that in Eqs. (1) and (2) the triplet masses $M(^3S,\ ^3P)$ are **not necessarily the centroid masses** of the three spin–orbit split triplet states. I return to this point later.
- Also, keep in mind that these expressions are obtained assuming that there is **no long–range spin–spin interaction**, as might arise from the exchange of anything other than a single vector gluon.

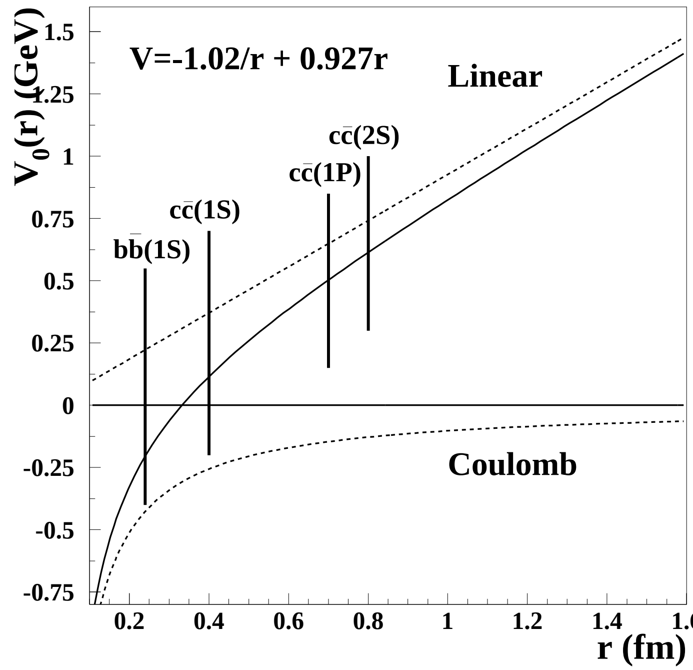
It is a fact of life that while Lattice may provide the ultimate answers in QCD, at this point most existing theoretical predictions are from **QCD–inspired potential models**. In one way or another they require assuming a central potential. The most popular potential is the so-called **“Cornell potential”** [3] which consists of a one-gluon exchange “Coulombic” central potential and a linear “confinement” potential:

$$V(q\bar{q}) = V(\text{Coulomb}) + V(\text{confinement}) = \frac{k}{r} + Cr \qquad (3)$$

Variants often consist of different prescriptions for smoothing the Coulomb potential, making different assumptions about the Lorentz structure of the confinement potential, and including relativistic effects in various approximations.

The spin–dependent contributions arising from the Coulomb part are represented by the well-known Breit–Fermi Hamiltonian. The uncertain part is the contribution that the confinement interaction makes to the spin–dependent interaction.

The relative contributions of the Coulombic and confinement parts to the central potentials are, of course , very r–dependent, as illustrated. It is clear that the 1S states of bottomonium are least affected by the confinement potential and the 1P and 2S states of charmonium are most sensitive to it. The same follows for spin–dependent potentials.



Since one of the important open questions is about the Lorentz structure of the confinement interaction, and whether it makes any long–range contribution to the hyperfine interaction, it is important to study how the the hyperfine splitting varies with variations in

- **angular momenta: S–wave states versus P–wave states,**
- **radial excitations: 1S versus 2S, 1P versus 2P,**
- **quark flavor: charm quark versus beauty quark.**

This is what I am going to talk about.

Experimental Problems

Most of what we know about heavy quark spectroscopy comes from **electron–positron annihilation** experiments done at the e^+e^- colliders, with the early ones being at SLAC, DESY, Frascati, Orsay, and Novosibirsk, followed by those at Cornell and Beijing, and the B–factories at SLAC (PEP–II) and KEK (Belle). High precision measurements of charmonium states were done with $p\bar{p}$ collisions at Fermilab, and may soon be done at the PANDA facility under construction at GSI in Darmstadt.

- **In e^+e^- annihilation** into a virtual photon the spin–triplet S–wave states with $J^{PC} = 1^{--}$ are directly excited, and their spectroscopy was done in great detail for $c\bar{c}$ charmonium and $b\bar{b}$ bottomonium early after the discovery of J/ψ in 1974 [4], and the discovery of $\Upsilon(1S)$ in 1977 [5], but the identification of their partner spin–singlet states presented serious problems, because they could only be reached indirectly in transitions from higher excited states, the obvious one being via weak **M1 transitions** from the triplet states. And there lies one of those rare and interesting stories of the **triumph of theory over experiment.**
- In 1978 DASP [6] announced the identification of $\eta_c(1S)$ at a mass of $M(\eta_c(1S)) = \mathbf{2830 \pm 50}$ **MeV**. This was immediately and forcefully challenged by Shifman et al. [7] who made a firm prediction of $M(\eta_c(1S)) = \mathbf{3000 \pm 30}$ **MeV** based on QCD Sum Rules, which is of course where it was later found by the Mark II and Crystal Barrel [8] at SPEAR (SLAC).

Fun and Games with the Hyperfine Interaction

Before we go into details of hyperfine splittings, let us consider a toy model. The simple QED result for hyperfine splitting is

$$\Delta M_{hf}(nS) = (32/9)\pi\alpha_{em}|\psi_n(0)|^2/m_1m_2$$

We replace α_{em} by α_{Strong}, and borrow its mass dependent values from Godfrey and Isgur. We make the simplifying assumption that in a Schrödinger equation $|\psi_n(0)|^2/m_1m_2$ is nearly a constant, independent of the quark masses. We set it $= 31$ MeV, so that $\boldsymbol{\Delta M_{hf}(nS)}$ **in MeV** $\boldsymbol{= 346\alpha_s}$. The table shows that the predicted $\Delta M_{hf}(^3S_1 -^1 S_0)$ which result are in remarkable agreement with their experimental values (jumping the gun a bit about charmonium and bottomonium):

$\Delta M_{hf}(1S)$	(η,ω)	(D,D^*)	(D_s,D_s^*)	$(\eta_c,J/\psi)$	$(\eta_c(2S),\psi(2S))$	$(\eta_b,\Upsilon(1S))$	$(\eta_b(2S),\Upsilon(2S))$
α_s(GI)	0.65	0.42	0.42	0.34		0.21	
$\boldsymbol{346\alpha_s}$	**225**	**145**	**145**	**118**	**49**	**73**	**34**
Expt. (MeV)	234	145	144	117	49	64	**?**

- For the 2S excitations $|\psi(0)|^2/m^2$ is reduced by the experimentally determined factors $[|\psi(0)|^2/m^2]_{2S}/[|\psi(0)|^2/m^2]_{1S} = \Gamma_{ee}(2S)/\Gamma_{ee}(1S) = 0.42(c\bar{c}), 0.46(b\bar{b})$.
- **This level of success of a frivolous(?) prediction ought to set a goal for the Lattice!!** We will see how close it comes.

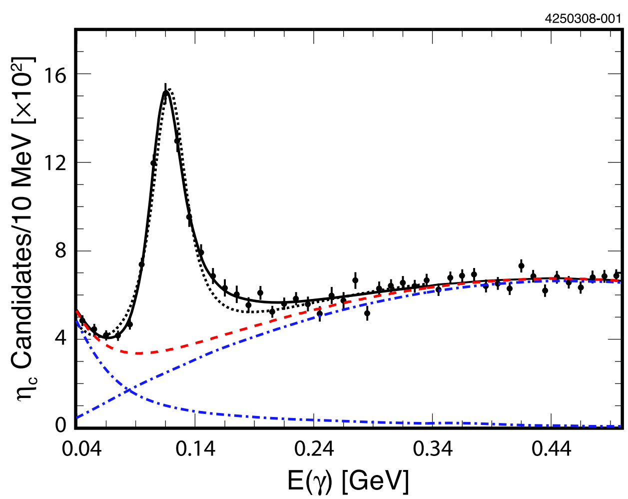
Hyperfine Interactions in Charmonium (Experimental)

Hyperfine Splitting in Charmonium 1S States

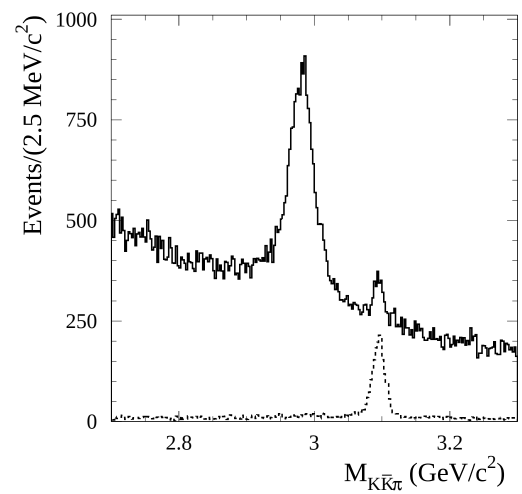
In 1980 Mark II and Crystal Barrel at SPEAR (SLAC) [8] finally identified the charmonium ground state, $\eta_c(1S)$. Better measurements have continued since, and the PDG2010 average of hyperfine splitting is

$$\boldsymbol{\Delta M_{hf}(1S)_{c\bar{c}} = M(J/\psi) - M(\eta_c(1S)) = 116.6 \pm 1.2\ \mathrm{MeV}.}$$

This is by far the best measurement so far of hyperfine splitting in a quarkonium system.



CLEO [9]: $e^+e^- \to J/\psi \to \gamma\eta_c(1S)$

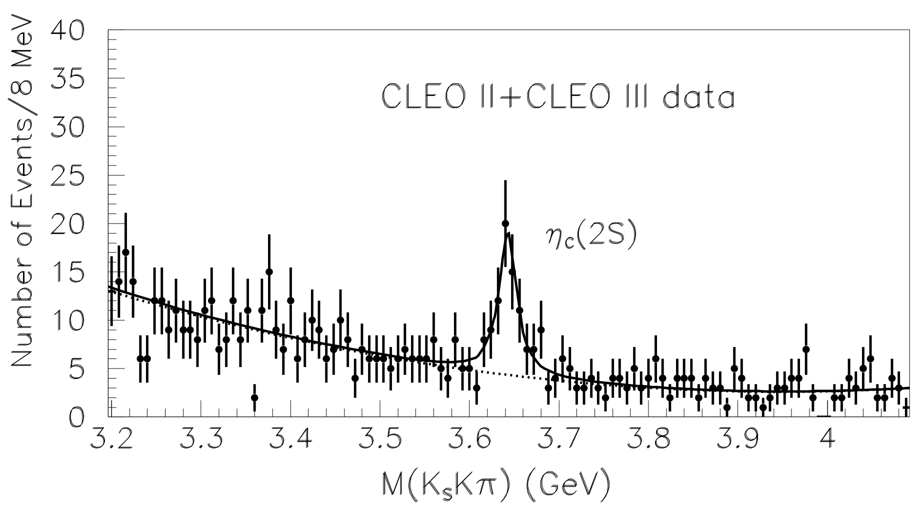


BaBar [10]: $e^+e^- \to e^+e^-\ \eta_c$, $\eta_c \to K_S K\pi$

Hyperfine Splitting in the Charmonium 2S State

Measurement of hyperfine splitting in the radial excitation of the charmonium ground state defied many attempts because the photon in the radiative transition, $\psi(2S) \to \gamma\eta_c(2S)$ was expected to have very small energy, ~ 60 MeV. One had to wait for 22 years for the successful identification of $\eta_c(2S)$ by Belle [11] in B decays in 2002.

It was confirmed by us at CLEO [12], and simultaneously by BaBar [13], in its formation in two photon fusion and decay in $K_S K\pi$.



The result for hyperfine splitting is

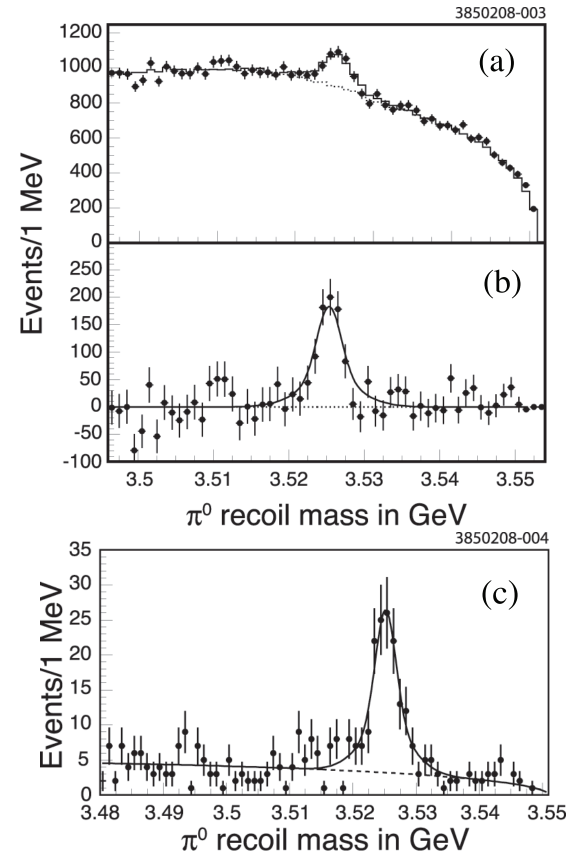
$$\Delta M_{hf}(2S)_{c\bar{c}} = M(\psi(2S)) - M(\eta_c(2S)) = 49 \pm 4 \text{ MeV.}$$

Hyperfine Splitting in Charmonium 1P States

Determining hyperfine splitting in P–wave states required identification of h_c, the $^1P_1$ singlet state. It was even more challenging, because the transition from the 1^{--} $\psi(2S)$ to the 1^{+-} h_c is only possible with a 0^{-+} π^0. But it is forbidden by isospin, and has very little phase space. However, we identified it in 2005 at CLEO [14] via $\psi(2S) \to \pi^0 h_c$, and obtained

$$\Delta M_{hf}(1P)_{c\bar{c}} = M(\chi(1^3P)) - M(h_c(1^1P_1)) = -0.10 \pm 0.22 \text{ MeV},$$

which is, of course, consistent with the lowest order prediction of zero.



(a) **Inclusive**, E1 tagged, (b) background subtracted, (c) **Exclusive**, η_c decays

- The 2P states of charmonium all lie above the $D\overline{D}$ breakup threshold at 3730 MeV, and there is little hope of identifying them anytime soon. So, let me now move on to bottomonium.

Hyperfine Interactions in Bottomonium (Experiment)

As for charmonium, the problem of studying the hyperfine interaction in bottomonium consists of identifying the spin singlet states, $\boldsymbol{\eta_b(n^1S_0)}$**, and** $\boldsymbol{h_b(n^1P_1)}$.

The spin singlet states $\Upsilon(n^3S_1)$ have been known for nearly 34 years, but even the spin singlet ground state $\eta_b(1^1S_0)$, of bottomonium was not identified until three years ago. The main problem is that the radiative M1 transitions from $\Upsilon(nS)$ states have even smaller energies than in charmonium and backgrounds are far worse. Nevertheless, remarkable success has been achieved recently.

The $\boldsymbol{\eta_b(1S)}$**,** $\boldsymbol{h_b(1P)}$ **and** $\boldsymbol{h_b(2P)}$**, and as of today** $\boldsymbol{\eta_b(2S)}$, have been successfully identified, and we can now discuss hyperfine splitting in bottomonium.

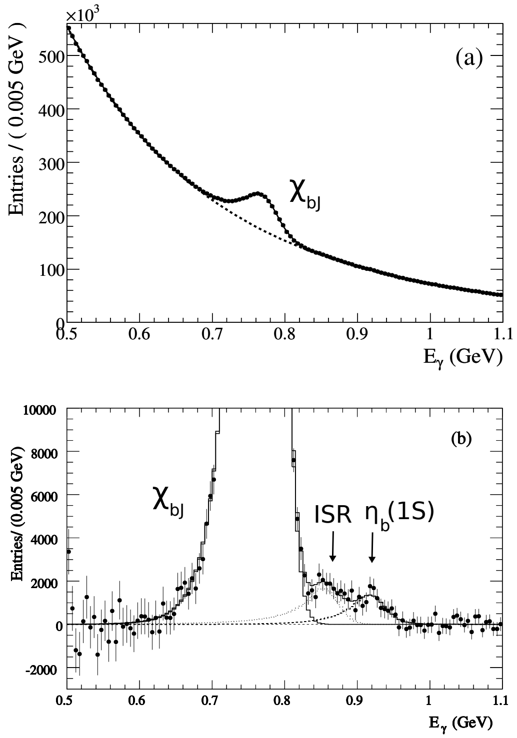
Observation of the Ground State of Bottomonium

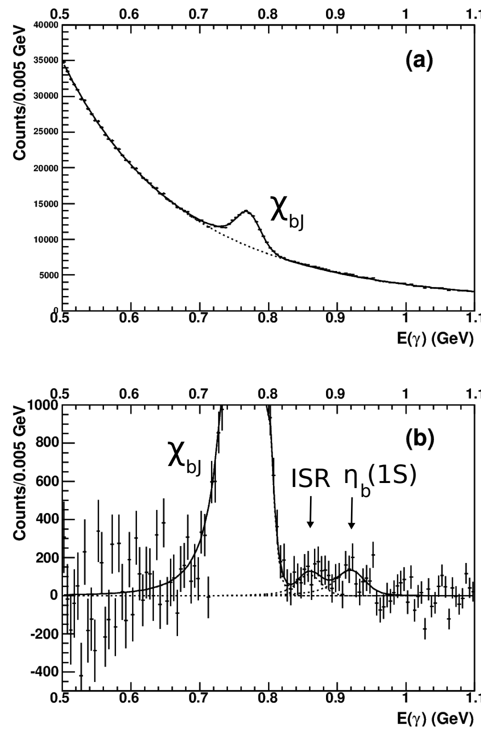
- An early attempt to identify $\eta_b(1S)$ in the inclusive allowed M1 radiative decays, $\Upsilon(1S) \to \gamma\eta_b(1S)$ by detecting the low energy (< 100 MeV) transition photons was unsuccessful [15].

- Successful identification of $\eta_b(1S)$ by BaBar [16], and its confirmation by CLEO [17], was only made possible by detecting instead the ~ 920 MeV transition photons in the **"forbidden"** M1 decays $\Upsilon(3S) \to \gamma\eta_b(1S)$.

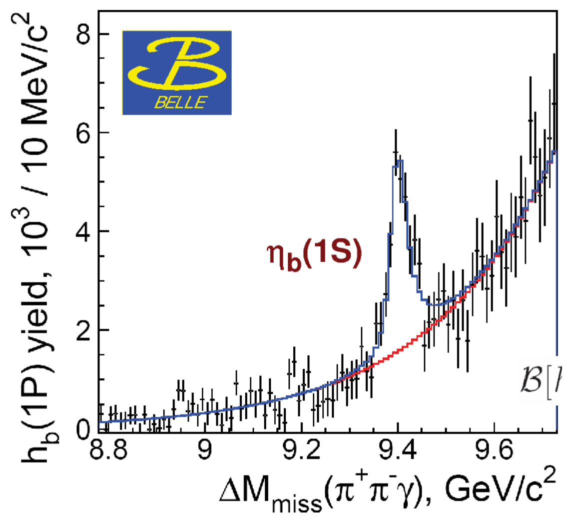
  The **forbidden** M1 transitions have zero overlap between the initial and final states in the lowest order, and they become finite only because of relativistic and higher–order effects. Fortunately, this weakness is partially compensated by the E_γ^3 increase in the width. However, it is still very challenging to identify the transition photons in the inclusive radiative decay of $\Upsilon(1S)$ as was done by BaBar and CLEO.

  The following figure illustrates the difficulty in identifying the $\eta_b(1S)$ signal in presence of the huge yield of $\chi_{bJ}(1P)$ and the ISR excitation of $\Upsilon(1S)$.





And now by an unexpected means—



BaBar [16]: $\Upsilon(3S) \to \gamma\eta_b(1S)$ CLEO [17]: $\Upsilon(3S) \to \gamma\eta_b(1S)$ Belle [18]: $h_b(1P) \to \gamma\eta_b(1S)$

The net result is

$$\Delta M_{hf}(1S)_{b\bar{b}} = M(\Upsilon(1S)) - M(\eta_b(1S)) = 64.1 \pm 2.0 \text{ MeV}$$

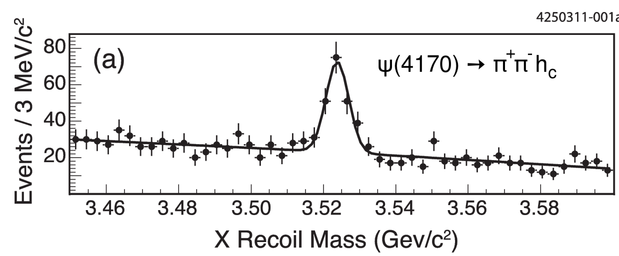
Observation of the Singlet P–States of Bottomonium

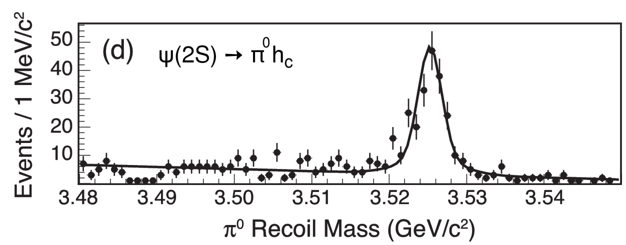
As I already mentioned, we made the discovery of h_c, the singlet P–state of charmonium by means of the doubly difficult transition $\psi(2S) \to \pi^0 h_c$ which is forbidden by isospin conservation and has very little phase space.

Five years later CLEO [19] found another, rather unexpected way to populate h_c through the **unbound charmonium state, $\psi(4160)$**. The reaction

$$\boldsymbol{\psi(4160) \to \pi^+\pi^- h_c(3525)}$$

was found to populate h_c stongly.





- Frankly, the mechanism for this successful reaction is not well understood if $\psi(4160)$ is a pure 1^{--} state as is generally believed, because the transition,

$$\boldsymbol{1^{--} \to 1^{+-} + (\pi^+\pi^-)}$$

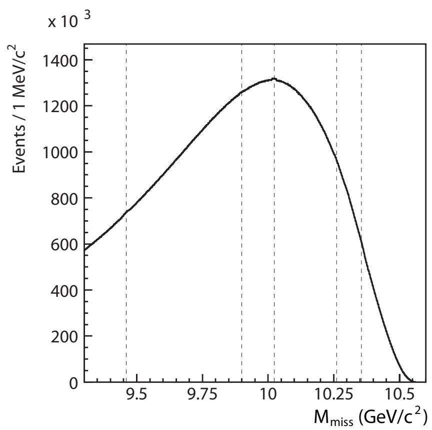
  can not be a one-step transition.

Observation of the Singlet P–States of Bottomonium

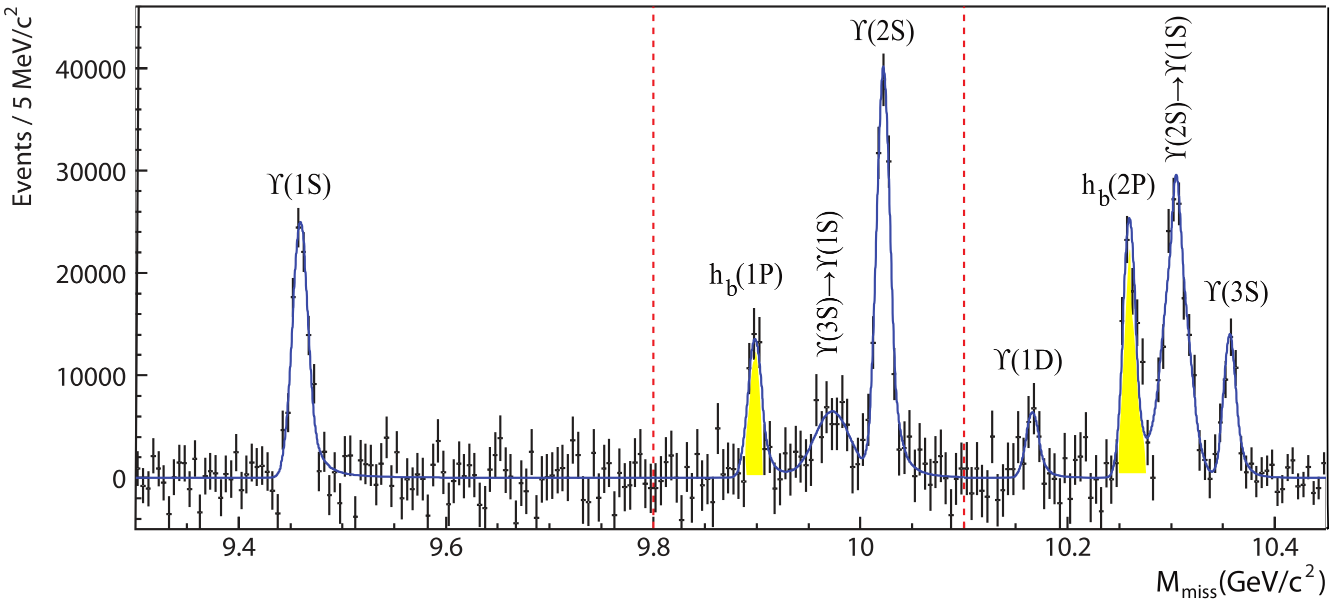
The success of CLEO in reaching h_c from an unbound state of charmonium suggested to Belle [20] that perhaps they could also find the h_b states of bottomonium by searching for them in the corresponding $\pi^+\pi^-$ transitions from the unbound region of bottomonium. They tried, and strangely enough, while it did not work at $\Upsilon(4S)$, it worked beautifully from e^+e^- annihilations near $\Upsilon(5S)$

To understand how difficult it is to find the h_b, first look at the raw $\pi^+\pi^-$ recoil spectrum for

$$e^+e^- \to \Upsilon(\sim 5S) \to \pi^+\pi^- X, \quad X =?$$



You can see nothing. But if you have monstrous statistics you can look with a microscope. Lo, and behold, a whole world is revealed, with beautiful peaks corresponding to $h_b(1P)$ and $h_b(2P)$!!



The identifications lead (Belle [20]) to

$$\Delta M_{\rm hf}(1P)_{b\bar{b}} = 0.8 \pm 1.1 \text{ MeV}, \qquad \Delta M_{\rm hf}(2P)_{b\bar{b}} = 1.6^{+1.6}_{-1.2} \text{ MeV}$$

Both results again confirm the rather naive pQCD expectation that ΔM_{hf}(P-wave)$= 0$.

And now for the "stop the press" news!

Observation of the Radially Excited $\eta_b(2S)$

We have now made successful observation of the radially excited $\eta_b(2S)$ state, and the measurement of the hyperfine splitting of the 2S state of bottomonium [20]. Since this is the **first announcement of the observation of $\eta_b(2S)$** let me give you some details.

- As mentioned earlier, the inclusive radiative transitions in $\Upsilon(nS) \to \gamma\eta_b(nS)$ are essentially impossible to measure. The only hope was to identify $\eta_b(nS)$ in radiative decays of $\Upsilon(nS)$ by "tagging" $\eta_b(nS)$ by their exclusive hadronic decays. Since individual exclusive decays are expected to be very weak, with product branching fractions in the 10^{-5} range, many such decays need to be measured in order to obtain a statistically significant result. We have done just that. We have measured 26 exclusive decays of $\eta_b(2S)$ into charged hadrons

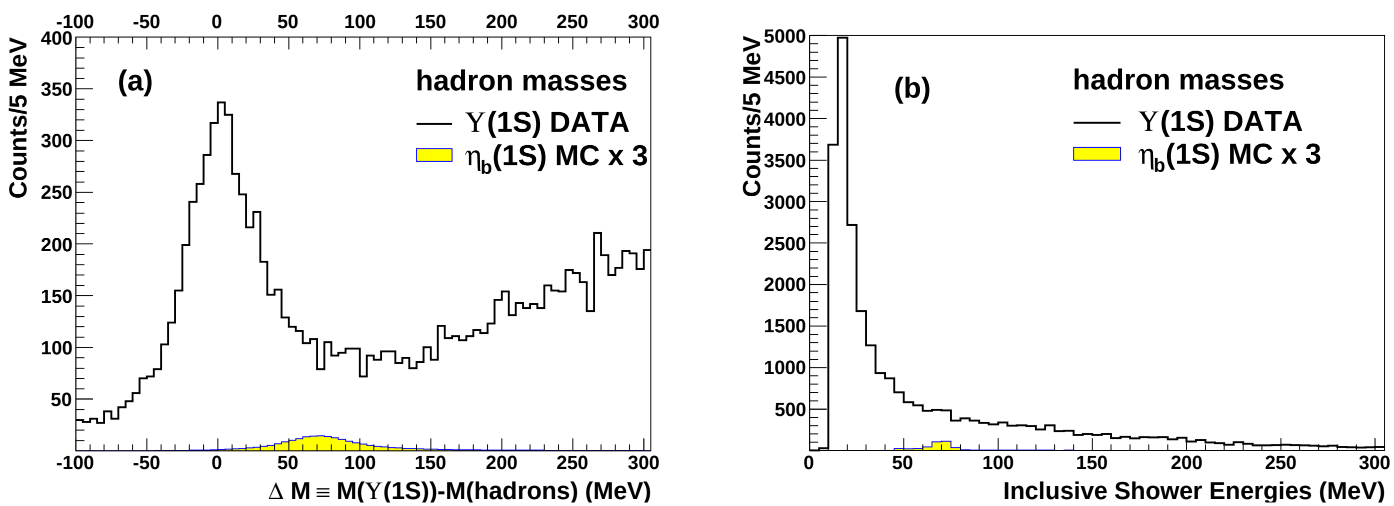
  $$\boldsymbol{\Upsilon(2S) \to \eta_b(2S), \qquad \eta_b(2S) \to X_i,}$$

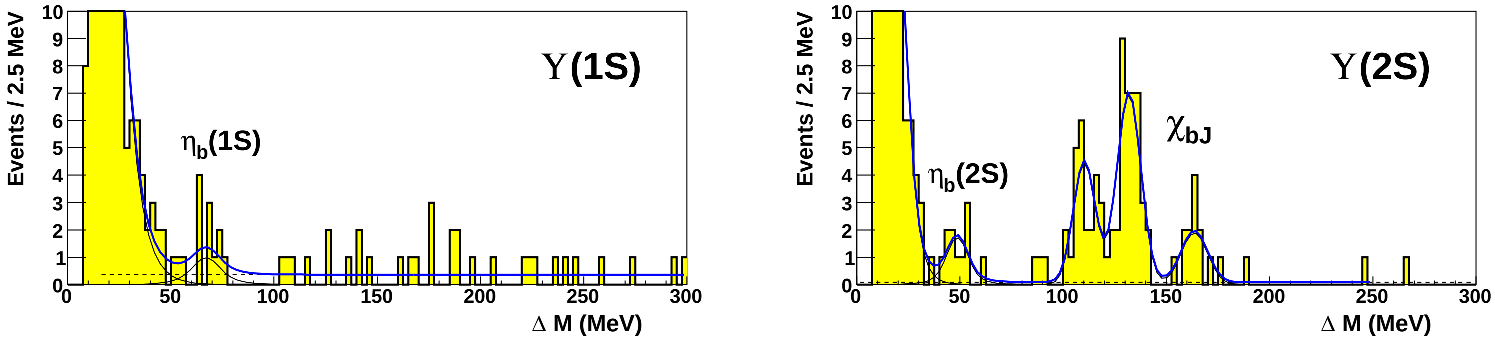
  with X_i comprising of up to ten $\pi^\pm$, $K^\pm$, and $p/\bar{p}$. To validate our analysis procedures we have made identical analysis of

  $$\boldsymbol{\Upsilon(1S) \to \eta_b(1S), \qquad \eta_b(1S) \to X_i,}$$

  We use CLEO III data consisting of 9.3 million $\Upsilon(2S)$, and 20.9 million $\Upsilon(1S)$.

- The exclusive final states in the reaction we construct are: $\Upsilon(nS) \to \gamma X_i$, $X = 2(\pi^+\pi^-)$, $3(\pi^+\pi^-)$, $4(\pi^+\pi^-)$, $5(\pi^+\pi^-)$, $K^+K^-\pi^+\pi^-$, $K^+K^-2(\pi^+\pi^-)$, $K^+K^-3(\pi^+\pi^-)$, $K^+K^-4(\pi^+\pi^-)$, $2(K^+K^-)$, $2(K^+K^-)\pi^+\pi^-$, $2(K^+K^-)2(\pi^+\pi^-)$, $2(K^+K^-)3(\pi^+\pi^-)$, $p\bar{p}\pi^+\pi^-$, $p\bar{p}2(\pi^+\pi^-)$, $p\bar{p}3(\pi^+\pi^-)$, $p\bar{p}4(\pi^+\pi^-)$, $p\bar{p}K^+K^-\pi^+\pi^-$, $p\bar{p}K^+K^-2(\pi^+\pi^-)$, $p\bar{p}K^+K^-3(\pi^+\pi^-)$, $K^0_S K^\pm\pi^\mp$, $K^0_S K^\pm\pi^\mp\pi^+\pi^-$, $K^0_S K^\pm\pi^\mp 2(\pi^+\pi^-)$, $K^0_S K^\pm\pi^\mp 3(\pi^+\pi^-)$, $2K^0_S\pi^+\pi^-$, $2K^0_S 2(\pi^+\pi^-)$, $2K^0_S 3(\pi^+\pi^-)$.
- In the first step of analysis, for example for $\Upsilon(1S)$, if you look at the raw spectrum of reconstructed hadrons alone, presented in terms of the mass difference $\Delta M = M(\Upsilon) - M(\mathrm{hadrons})$, or the inclusive spectrum of photons, you can not expect to see any enhancement for $\eta_b(1S)$, and you do not.





- We have to reconstruct the full event, i.e., the **hadrons and the photons**, and impose the energy-momentum constraint for the full event. When we do that, most of the background events are removed, and we see resonance enhancements in both $\Upsilon(1S)$ and $\Upsilon(2S)$ data.
- Fit to the $\Upsilon(1S)$ data yield the mass of the enhancement as $\boldsymbol{\Delta M = 67.8 \pm 3.9}$ **MeV**, which unambiguously identifies it to be due to $\eta_b(1S)$.
- Fit to the $\Upsilon(2S)$ data yields the mass of the enhancement as $\boldsymbol{\Delta M = 48.7 \pm 2.7}$ **MeV**, with **significance level** $\boldsymbol{\sim 5\sigma}$**.**

  We cannot find any other explanation for this enhancement except to **attribute it to** $\boldsymbol{\eta_b(2S)}$.
- Thus, we conclude that the hyperfine splitting for the bottomonium $2S$ state is

$$\boldsymbol{\Delta M_{hf}(2S)_{b\bar{b}} = M(\Upsilon(2S)) - M(\eta_b(2S)) = 48.7 \pm 2.7\ \mathrm{MeV}}$$

Summary of Experimental Results

$M(^1S_0, ^1P_1)$ (MeV)	Hyperfine Splitting (MeV)	
$M(\eta_c(1S)) = 2980.3 \pm 1.2$	$\Delta M_{hf}(1S)_{c\bar{c}} = \psi(1S) - \eta_c(1S)$	$= 116.6 \pm 1.2$
$M(\eta_c(2S)) = 3637 \pm 4$	$\Delta M_{hf}(2S)_{c\bar{c}} = \psi'(2S) - \eta'_c(2S)$	$= 49 \pm 4$
$M(h_c(1P)) = 3525.20 \pm 0.15$	$\Delta M_{hf}(1P)_{c\bar{c}} = \langle\chi_{cJ}(1P)\rangle - h_c(1P)$	$= -0.06 \pm 0.15$
$M(\eta_b(1S)) = 9396.2 \pm 2.0$	$\Delta M_{hf}(1S)_{b\bar{b}} = \Upsilon(1S) - \eta_b(1S)$	$= 64.1 \pm 2.0$
$M(\eta_b(2S)) = 9974.6 \pm 2.7$	$\Delta M_{hf}(2S)_{b\bar{b}} = \Upsilon(2S) - \eta_b(2S)$	$= 48.7 \pm 2.7$
$M(h_c(1P)) = 9898.3 \pm 1.6$	$\Delta M_{hf}(1P)_{b\bar{b}} = \langle\chi_{bJ}(1P)\rangle - h_b(1P)$	$= 0.8 \pm 1.1$
$M(h_b(2P)) = 10259.8 \pm 1.3$	$\Delta M_{hf}(2P)_{b\bar{b}} = \langle\chi_{bJ}(2P)\rangle - h_b(2P)$	$= 0.5^{+1.6}_{-1.2}$

As experimentalists **our job is done**, and we can assign the job of understanding these hard–fought numbers to the theorists.

But no! Let us point out some of the important problems, as we see them.

The Problems

The first problem relates to what we are calling the experimental measure of hyperfine splittings in P–wave . We have been calculating, for example,

$$\Delta M_{hf}(nP) = \langle M(n^3P_J)\rangle - M(n^1P_1),$$

with $\langle M(n^3P_j)\rangle$ calculated as the **centroid** of the three spin-orbit split states, $^3P_0$, $^3P_1$, and $^3P_2$,

$$\langle M(n^3P_J)\rangle = [M(^3P_0) + 3M(^3P_1) + 5M(^3P_2)]/9$$

but this is not equal to $M(n^3P)$ except in the limit of perturbatively small spin-orbit splitting. The fact is that the measured spin–orbit splittings are hardly small, $M(^3P_2) - M(^3P_0)$ in charmonium being 142 MeV. An often used measure of spin–orbit splitting is the ratio,

$$R = [M(^3P_2) - M(^3P_1)]/[M(^3P_1) - 3M(^3P_0)]$$

For a Coulombic potential the perturbative prediction is $\boldsymbol{R = 4/5}$**, or** $\mathbf{0.8}$.

For charmonium 1P states the experimental value is $\boldsymbol{R(1P)_{c\bar{c}} = 0.475(2)}$.

For bottomonium $\boldsymbol{R(1P)_{b\bar{b}} = 0.583(2)}$, and $\boldsymbol{R(2P)_{b\bar{b}} = 0.574(4)}$.

Thus the measured values of $R(nP)$ are all telling us that the centroid $\langle M(n^3P_j)\rangle$ is not a proper measure of the unsplit triplet mass $M(n^3P)$.

So, we have been calculating the P–wave hyperfine splittings using the wrong triplet state masses. But these wrong masses are giving us the expected result of zero hyperfine splittings for P–wave states. This is rather weird. **Doing the manifestly "wrong" calculation is giving us the "right" answer.**

This raises two possibilities. Either the calculation is not "wrong", or the answer is not "right". To be more specific, the possibilities are:

1. The observed spin–orbit splittings receive large contributions from the non–Coulombic part of the potential, i.e., from the confinement potential, which in some magical way makes the centroid $\langle M(n^3P_J)\rangle$ an excellent measure of the unsplit triplet mass $M(n^3P)$.
2. The contact nature of the hyperfine interaction, based on non-relativistic reduction of the Bethe–Salpeter equation, and the assumption of a Lorentz scalar nature of the confinement potential lead to the expectation of zero hyperfine splitting for non–L=0 states. This expectation may not be correct.

The second problem concerns theoretical predictions of singlet state masses.

An infinite number of potential model calculations for charmonium and bottomonium exist in the literature. Their predictions run all over the map. Let me illustrate the point by reproducing the compilation of potential model predictions of bottomonium hyperfine splittings from the Ph.D. dissertation of S. Dobbs [21].

Even in the post–1990 papers the potential-model predictions for the bottomonium hyperfine splittings range from $\Delta M_{hf}(1S)_{b\bar{b}} = 46$ MeV to 87 MeV and from $\Delta M_{hf}(2S)_{b\bar{b}} = 23$ MeV to 44 MeV. The situation is not much better with the Lattice predictions, and they range from

$$\boldsymbol{\Delta M_{hf}(1S)_{b\bar{b}} = 20\ \mathrm{MeV\ to}\ 70\ \mathrm{MeV\ and}}$$
$$\boldsymbol{\Delta M_{hf}(2S)_{b\bar{b}} = 12\ \mathrm{MeV\ to}\ 30\ \mathrm{MeV.}}$$

Potential Model Calculations

Reference(year)	$\Delta M_{\rm hf}(1S)_{b\bar{b}}$ (MeV)	$\Delta M_{\rm hf}(2S)_{b\bar{b}}$ (MeV)
Potential Models		
CGM(78)	90	40
BT(81)	46	23
EF(81)	95	41
BJ(82)	27	13
GRR(82)	35	19
MB(83),ZB(83)	101	40
MR(83)	57	26
GOS(84)	67^a, 78^b	31^a, 37^b
GI(85)	63	27
GRR(86)	44	26
IO(86)	$40-45$	—
PTN(86)	$39-49$	$21-24$
GRS(89),ZSG(91)	48	23
F(91)	46	23
EQ(94)	87	44
ZOR(95)	49	26
GJR(96)	43	—
LNR(99)	79	44
EFG(03)	60	30
RR(07)	47^d, 68^e	24^d, 36^e
Effective Field Theories		
CO(96)	$36-49$	$20-23$
BSV(01)	$36-55$	—
RS(04)	44	21
KPPSS(04)	39 ± 14	—
Model Independent	79 ± 3	36 ± 1

Lattice Calculations

Reference(year)	$\Delta M_{\rm hf}(1S)_{b\bar{b}}$ (MeV)	$\Delta M_{\rm hf}(2S)_{b\bar{b}}$ (MeV)
BSW(97)	44,50	—
MBD(01)	40,44	—
LM(02)	59 ± 20	—
TWQCD(07)	70 ± 5	—
*CP–PACS(00)	$20-33$	—
*HP–UKQCD(05)	61 ± 14	30 ± 19
*BE(07)	37 ± 8	13 ± 19
*FNL–MILC(10)	54.0 ± 12.4	(12 ± 60)
*RBC–UKQCD(10)	60.3 ± 7.7	23.5 ± 4.6

* Unquenched calculation.

I hope to hear a lot from the assembled Lattice experts in the next few days, but before then I will make my credibility cuts among the lattice predictions as follows:

I will only consider

- unquenched lattice calculations.
- those calculations which include continuum extrapolation.
- those calculations which succeed in predicting the known 1S hyperfine splitting of 64 ± 2 MeV within errors, and make prediction of the 2S hyperfine splitting.

With these non–expert subjective criteria only four predictions survive.

	$\Delta M_{hf}(1S)_{b\bar{b}}$	$\Delta M_{hf}(2S)_{b\bar{b}}$
HP–UKQCD (2005)	61 ± 14 MeV	30 ± 19 MeV
FNAL–MILC (2010)	54 ± 12 MeV	12 ± 60 MeV (from graph)
RBC-UKQCD (2010)	60 ± 8 MeV	24 ± 5 MeV
HPQCD (2012)	70 ± 9 MeV	35 ± 3 MeV
Experiment	$\mathbf{64 \pm 2}$ **MeV**	$\mathbf{49 \pm 3}$ **MeV**

I am sure you see the big problem. Our measured 2S hyperfine splitting is considerably larger than the existing lattice predictions, even though all of them make the caveat that predictions for radial and P-wave excitations are not very reliable at this point.

The experimental measurement has two parts:

1. Our observation of the enhancement. How sure?

   with $\sim 5\sigma$ significance, we are sure it is real.

2. Our assignment of it to $\eta_b(2S)$

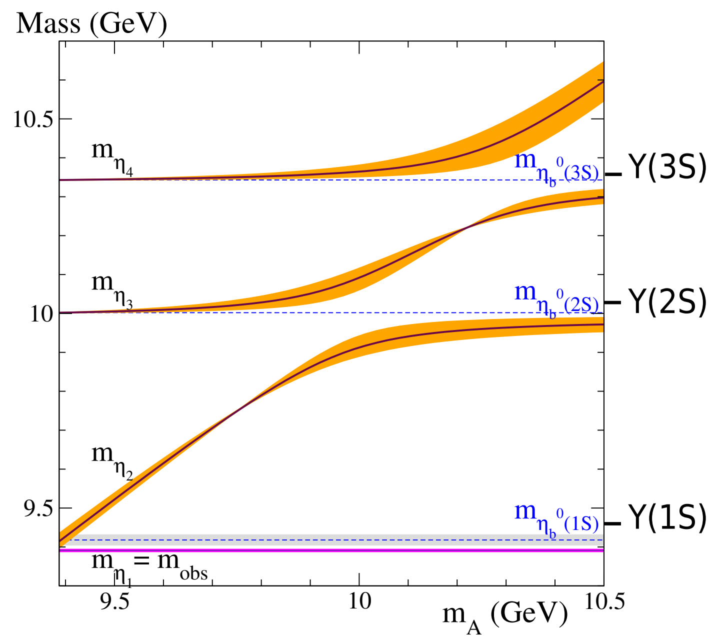
   This is, of course, a best explanation call,

   because we can not think of what else it could be.

What about the apparent discrepancy with the existing Lattice calculations?

Should one seriously look into speculation a la Domingo et al. (PRL **103**, 111802 (2009)) which invokes:

- **Mixing of η_b States with a Light CP–Odd Higgs Boson**
  to predict larger hyperfine splitting.



The third problem concerns theoretical predictions about formation and decay probabilities of the singlet states, or what we experimentalists call branching fractions. These are obviously more difficult to predict than masses because they involve details of overlaps between initial and final state wave functions. As the nuclear physicists among us know very well, predicting level spectra is relatively easy compared to predicting spectroscopic factors.

One of our important reasons for studying the **allowed M1** transitions $\Upsilon(nS) \to \gamma\eta_b(nS)$ was to get away from the difficult problems associated with the theoretical understanding of the **forbidden M1** transitions $\Upsilon(nS) \to \gamma\eta_b(n'S)$. The hope was that at least for these, the theoretical predictions would be reliable. However, this turns out to be not true.

There are absolutely no predictions of hadronic widths or branching fractions from Lattice. And, as usual, potential model predictions are numerous and they vary all over the map. This is illustrated in the following table.

Reference	$\Gamma(\eta_b(1S) \to \gamma\gamma)$ (keV)	$\Gamma(\eta_b(1S) \to gg)$ (MeV)	$\Gamma(\eta_b(2S) \to \gamma\gamma)$ (keV)	$\Gamma(\eta_b(2S) \to gg)$ (MeV)
GI(85)	0.38	10.21	—	—
AB(92)	0.17	4.57	0.13	2.21
AM(95)	0.52	13.97	—	—
M(96)	0.22 ± 0.04	5.91 ± 1.07	0.11 ± 0.02	1.87 ± 0.34
GJR(96)	—	12.46	—	—
SBG(98)	0.46	12.36	0.20	3.40
EFG(03)	0.35	9.40	0.15	2.55
F(03)	0.47 ± 0.08	12.52 ± 2.71	—	—
PPSS(04)	0.66 ± 0.09	17.71 ± 2.47	—	—
KLW(05)	—	6.98 ± 0.85	—	3.47 ± 0.45
KLW(05)	0.38 ± 0.05	10.32 ± 1.26	0.19 ± 0.03	3.25 ± 0.43
LS(06)	0.23	6.18	0.07	1.19
LP(07)	0.56	15.04	0.27	4.58
KPS(10)	0.54 ± 0.15	14.52 ± 4.03	—	—
CLY(11)	0.51 ± 0.10	13.71 ± 2.69	0.24 ± 0.4	4.00 ± 0.73
Range	$\mathbf{0.17 - 0.66}$	$\mathbf{4.57 - 14.52}$	$\mathbf{0.07 - 0.27}$	$\mathbf{1.19 - 4.58}$

	$\boldsymbol{\Upsilon(1S) \to \gamma\eta_b(1S)}$	$\boldsymbol{\Upsilon(2S) \to \gamma\eta_b(2S)}$
Reference	$\mathcal{B} \times 10^4$	$\mathcal{B} \times 10^4$
ZB83	8.3	0.83
GOS84	3.3, 3.5	0.54, 0.63
GI85	2.2	0.24
ZSG91	0.74	0.31
LNR99	3.5	0.88
EFG03	1.1	0.44
BJV06	2.8	1.7
Non–Relativistic	$\mathbf{0.17 - 8.7}$	$\mathbf{0.02 - 0.72}$

So, what are the prospects of doing better. Are we experimentalists laboring in vain to make the awefully difficult measurements of formation and decays of bottomonium states. **Can Lattice really help?**

Once again, I am here to hear your answers to this challenge.

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