Comments on the Workshop on $B \to D^{**}$ transitions ; some themes of discussion.

Alain Le Yaouanc, LPT

February 20, 2013

Abstract

Warning. We do not quote name of authors in the text on purpose. There are some personal points of view, as well as most common to many of us, and a synthesis of their particular contributions. References are given. The comments try to reflect the discussions in Orsay and possible disagreements outside. Experimentalists, in SL especially, may not agree on several comments, but they may lead to doubt.

1 The "1/2 semileptonic puzzle"

Note that this formulation is better than "The 1/2 vs 3/2 puzzle" because it locates the problem where it is really present up to now : not in 3/2, which are well predicted, not in NL, which agree with theory, but in SL decay to $j = 1/2$ $q\bar{q}$ states.

As to discrepancies, it must be clear that if errors were rightly calculated in exact theory (QCD) and experiment, there would not be any discrepancy. We believe all (within some range) in QCD and experience. So, to avoid metaphysics, let us stress what is the real question.

1.1 Preliminaries on theory and experience and on error estimate

-"Theory" does not mean QCD in itself but rather one of its concrete avatars, the **quark models**, and specifically our QM with GI wave functions inserted in the Bakamjian-Thomas framework for transitions. Presently, only this approach is able to formulate predictions for BR, the only ones which can be confronted to experiment. Present lattice QCD does not provide large w, which is what counts for BR; all the less at finite masses.

Up to know, the BT results for matrix elements have been presented only at $m_Q = \infty$, one important motivation being that only there is the approach covariant. The next step for QM is to give predictions at finite masses, which is straightforward, but one will loose covariance.

Theoretical errors. Let us stress a point which is contrary to certain prejudices : there is no way to give an a priori "error" on the predictions of QM, unlike in lattice QCD. Such a concept like a priori uncertainty is not well defined or not calculable. In particular, we do not have any free parameter to vary. The innumerable parameters of the GI potential model have been fixed by Godfrey and Isgur a long time ago, from an immense fit to hadronic states, which it is not possible to redo easily as would be required to estimate possible variations [1]. We simply borrow their value. Also, what error should one allow on hadronic masses ? Etc...

Only **a posteriori comparisons** with safe experiment, where experiment is known, can be easily made, or, where available, with lattice QCD. An extensive comparison with experiment for transition rates within the BT approach, which has not been performed, could give a valuable qualitative idea. This could be termed an experimental measurement of errors, and is a rather general and well-known method for estimating theoretical errors. Comparison with lattice QCD is another mean.

The comparison with lattice QCD on current densities suggests a good agreement at infinite mass, rather extensive in spatial dimensions. The $\xi(w)$ is a good fit to experimental h_{A_1} or F, G as well, to the rough precision which is relevant.

But the most relevant predictions for experimental tests -in the present discussion- are the physical BR, calculated in the same way as for the controversial 1/2, from $m_Q = \infty$ matrix elements, [2] : the BR of $B \to Dl\nu$, which is the most safely measured (if a little less accurate), is quite good : theory 2% against expt. 2.2%. Still more relevant is the BR to $j = 3/2$, which is not controversial (narrow states): theory 1.15% against expt. 0.9% (average by Patrick is used, with sum over the doublet, as we advocate). From these numbers, one suspects that the theoretical error cannot be 500% as it should be to agree with the experimental numbers for SL decay to $j = 1/2$.

-As to experiment, let us be precise :

1) what is under discussion first is the exclusive NL or SL rates to **resonances**, not the $D^{(*)}\pi(\pi)$ semi-inclusive SL rates for which we have simply no prediction

2) The question of experimental error bars is crucial ; we start from the experimental error bars as they are quoted for resonant states, which are claimed to be identified. Otherwise, one gets into confusion about what is meant by experiment. What we would guess or like the error bars to be is another question, discussed at the end of next subsection. Indeed,in the SL case, they are surprising small for the broad states $j = 1/2$: around (20% as given by Patrick,

[1] One cannot vary one or two parameters separately. One has to reproduce the full spectrum, which constraint establishes a connection between all the parameters.

[2] Of course, this requires a recipe relating the physical form factors to the $m_Q = \infty$ matrix elements. This generates an uncertainty, which could be evaluated by varying the recipe. However, once more, the range should be restricted by confrontation with experiment...

for the 0^+, where Babar and Belle are compatible; the same for 1^+ in Babar).

1.2 The main problem

-What is clear is that there is no **positive** indication of a serious problem in NL. Even more, there is **positively** an impressive semi-quantitative success, especially considering class III/class I, and taking into account $1/m_Q$ effects as discussed by Leibovich et al..

-Let us then reiterate that the main problem is in the **SL rate** to the $j = 1/2$ **resonances** (not in NL, neither in semi-inclusive SL, for which there is no theoretical prediction).

The discrepancy of experiment with theory for $j = 1/2,\ 0^+$ state in SL is a factor 5 with lower edge of experimental error bars, and 7 with central value (see the table pp. 12 or 13 in the talk ; the average proposed by Patrick would give rather 6 instead of 7). The discrepancy is enormous because the experimental error bars are so small.

Could we have a theoretical failure in SL so large as to comply with the claimed SL experimental numbers and errors ? we doubt much from what has been said above about similar BR.

Moreover, **multipying the theoretical predictions for NL by a factor 5 to accommodate the SL would give numbers completely contradictory with the NL data** (as given in the table p.17 in the talk, table due to Patrick). This reflects the fact that the experimental data **are completely at odds with the expected relation between SL and class I NL decays** (Neubert) , which is totally independent of the quark model.

It is therefore very unlikely that the QM is to be blamed for the core of this enormous discrepancy.

We would rather suspect the very small error bars in **SL experimental rates to $j = 1/2, 0^+$ resonance**. **The most probable is that a very broad resonance with small BR cannot be well identified with so few events as observed in SL**. The statistics is small, and the systematics would be expected to be very large, due to the difficulty of separating resonance and continuum for very broad, almost flat states. One may be surprised that with much more statistics, the errors in the NL case (charged B) are larger. In strong formation of broad baryonic resonances, such an accuracy is seldom obtained, although raw data are very much better, with Argand diagrams obtained.

This central point has not been discussed much in the colloquium- it also implies discussion on the Breit-Wigner forms far away from the peak (damping factors), and of the continuum.

1.3 Conclusions

That there exists a clear contradiction between certain theoretical approaches and experiment about the estimate of the rate to $j = 1/2$ **resonances** in **semileptonic** decays seems the only clear statement. From here on, up to Section 6 excluded, one enters into another type of discussions, more confused, and perhaps confusing.

-Preferably, one should concentrate on narrow resonances, in SL and NL as well. This is the reason to propose the experimental study of the $Bs \to D_{sJ}$ transitions, which offers precidely the opportunity to measure $j = 1/2$ with very narrow widths. Once we have these, we could almost forget about the experimental nightmare of D^{**} in SL as to the magnitude of transition amplitudes, as to excesses of all sort,..., and use $SU(3)$.

-Moreover, from the discussions, one important point should emerge : that we must not try to **"solve"** the problem of SL decays to D^{**} in an isolated fashion, without paying attention to what would happen correspondingly in NL decays (see below, for instance, the case of radial excitations).

2 NL versus SL

This seems to a central question for the future.

The general advantage of NL is : much more observed events. However, we must admit the important drawback : in Class I, where we could see directly the $B \to D^{**}$ transition, it interferes with a non exotic, therefore large, crossed channel $\pi\pi$ contribution. In Class III, one is freed from such a channel, but there is interference with the D^{**} emission contribution. The latter is controlled by f_D^{**}. f_D^{**} is perhaps not very well suited for BT, but it is the place for lattice QCD, and it has been calculated at finite mass and then one can tighten the predictions for class III which have been formulated previously.

One gets a powerful instrument of analysis by combining class III and class I decays. For instance class III has allowed a rather clean determination of the mass and width of broad resonances, which can be inserted in the analysis of the other processes.

3 Inconsistencies or differences beetween Belle and BABAR

They are obvious in SL, especially in the $D^*\pi$ channel. They are also present in the NL ; here, only the $D\pi\pi$ channel has been studied by both ; there is a very significant difference, but within compatibility- the error being very large in BABAR. It would be very interesting to have an analysis of $D^*\pi\pi$ from BABAR.

Resolving these differences is certainly one of the main tasks on the experimental side. Could theoreticians have something to say about the modelling (e.g. : phase)?

4 "Excesses" of events in SL decays

Much has been said about excesses. It is useful to warn that one must distinguish two questions and **two types** of excesses :

1) admitting the experimental analyses of Belle and Babar, one can perceive definite excesses or other discrepancies of raw data with respect to the experimentalists fit at low masses, especially at Babar. Babar seems also to find such an excess in the electronic spectrum. The $D\pi$ data of Belle at low mass do not seem simply to be fitted by the claimed 0^+ BW.

2) quite another question is : if and only if it were recognized that experimental estimates of SL rates to $j = 1/2$ resonances are mistaken, which is our only sharp point, there would then arise **another** question, quite different in nature, and **which should not be confused with the above controversy** : how would one explain the events previously included in the high experimental rate for the resonance, and which appear as $D\pi$ or $D\pi\pi$ events ? Let one find some comment on this new question below.

We have presently no answer to such questions. Not even an indication on the partial waves which are involved.

4.1 Are "excesses" also present in NL experimental analyses ?

The question of whether excesses similar to the ones considered in Section 4 for SL are present also in NL has been considered in the discussion. One has a vague impression that "excesses " of types 1) and 2) described for SL have no clear counterparts in NL.

"excess" of type 1), outside the fits. It is a fact that in NL, **fits pass through the data point much better than in SL, where excesses are seen at low mass away from the fit line**. There therefore no excess of type 1) in NL. One must stress that **factorisation assumption is not implied at this stage**, only the quasi-two body approximation.

As to "excess" of type 2) (events outside the resonances): although the rate to 0^+ is much smaller in Belle NL than in the SL (measured with respect to the 3/2), one does not find more events outside resonances. We must take into account the D_V^*, it contributes as much as the 0^+ resonance, but not much more. In Babar, a particular strong NR continuum is used, but it is **destructive** ; then the rate to resonance is larger, but it does not mean more events that at

Belle ; this is anyway quite different from SL, where Babar does not see such a continuum.

In fact, nothing definite can be said by non experts as one should also consider the reflexion of the $\pi\pi$ channel, which is large in class I decay, and which may absorb a part of events ; also, it should be clarified whether the D_V^* is to be fittted, etc ....

5 Theory : should we insist on explaining non resonant continuum ("background" in old terms) and all that ?

In relation to the latter questions, a more general question : should we really insist on explaining theoretically NR continuums, since it is very difficult ? The tendency of modern hadronic physics has been to leave more and more aside this difficult task, except for very particular and crucial cases such as $\pi\pi$ or $K\pi$ scattering. One concentrates on resonances and, still more specifically, on their decay properties. A further step could be to concentrate on narrow states. This could be termed in french "un replis stratégique", which however has a pejorative tone. In short, should we insist on explaining everything, or rather prefer clean situations ?

Anyway, there is not much of safe theory in this question of continuum. Soft pion theorems are useful constraints, but they call for extrapolations and corrections (unitarity corrections, higher order expansion..).

One has to be aware that, much like for baryonic resonances, "non resonant" could include contributions from the remote states, which indeed act as a non resonant continuum in the vicinity of the resonance. Could one calculate such contributions ?

5.1 Virtual $D^{(*)}$

The so-called $D_V^{(*)}$, virtual $D^{(*)}$ contributions, can be counted as some sort of continuum. Attention must be drawn to this contributionbecause of different possible treatments. It is included in the background of SL Babar $D\pi$ events. Belle considers, apparently, as an alternative to the 0^+ signal. It is also included in Belle and Babar analysis of NL data, this time as a component of the signal.

Where is the truth ? What is the magnitude of this contribution in SL ? Should it be fitted or predicted ? In the experience, it seems to be fitted. But the normalisation, i.e. the residue of the pole, seems fixed from the now well-known $g_{D^*D\pi}$ coupling and the known amplitude $B \to D^*$. Then the contribution is calculable, and it is indeed included in Goity-Roberts. What is the impact on the experimental analysis if this calculation is inserted in the experimental analysis ?

6 Spectroscopy of $D^{**}, L = 1$; phase of scattering

It seems very encouraging that the two lattice methods, with or without inclusion of four quarks operators, converge and find the experimental low mass of the 0^+ : 2300 GeV. A useful proposition could be that one abandons the misleading notation $D_0(2400)$

One important point has been the first results on $I = 1$ phase. Further steps can be envisaged : 1) to have a more detailed lattice prediction of $D\pi$ scattering phase, 2) to extract it experimentally from $B^- \to D\pi\pi$ (class III). The question would then be : is there any serious deviation from the Breit-Wigner form ? 3) to use this phase as a constraint in the analysis of class I NL and SL decays.

7 Spectroscopy of $D_s, L = 1$

One has also learned in the colloquium that after many improvements of the lattice calculations, the discrepancy of experience with GI quark model in the location of D_{sJ} is not solved . This is in contrast with the similar $D^{**})0^+$, which is now found at 2300. At least the $1^+_{1/2}$ is still predicted much too high. The more accurate method gives also a too high result for the 0^+.

Although there is little doubt that these are $\bar{q}q$ states, could it be that rescattering effects alter the spectrum ? This possibility is a motivation, in my personal opinion, (amateuristic), to consider also in this case the method including scattering states.

Another reason for doing this is the fact that the mixing of 1^+'s has been found to have a large imaginary component, unlike for D^{**} : this is an indication of large coupling channel effects.

8 Radial excitations

A lot of interesting discussions are in course, and experimantal tests are possible.

8.1 Missing pions

It has been suggested that states decaying into $D^{**}_{1/2}\pi$, like radial excitations, could enhance the apparent rate into $D^{**}_{1/2}$ if the additional pion is soft and then missed. In this case, this should be included in the error on the rate.

8.2 $B \to D^{(*)'}$

Once more, in theory, $B \to D^{(*)'}$ is presently accessible only to QM. In our opinion, whatever the wave functions, and with many variations on the transition model, we find much smaller BR than Galkin et al.. to the first excitation. **We doubt their calculation**. Our BT result is in agreement with the sum rules. As suggested, sum rules (curvature sum rule) can also be used roughly to bound from above the contribution of radial excitations if they are sufficiently regular ($m_Q \to \infty$). Once more, this suggests small contributions of the radial excitations (from their slope at $m_Q = \infty$)

As to experiment, it has been underlined that they are rather narrow states, so that they should be seen if their BR were sizable.

Babar has attempted to see them in NL class I and found no signal. Class III is a completely different question (see subsection 8.3).

Patrick has given other arguments tending to the conclusion that the contribution in SL should be small

8.3 $B \to D^{(*)'}\pi$ in class III of NL

In this case, there is a diagram controlled by $D^{(*)'}$ emission, therefore by completely different quantities : the annihilation constants $f_{D^{(*)'}}$.

8.4 OPE

Sums over transition to radial excitations can be calculated through OPE. It would be interesting to confront this with QM and other estimates.

Bibliography de "Proposal", à compléter

References

[1] Memorino on the '1/2 versus 3/2 puzzle' in $\bar{B} \to \ll \bar{\nu} X(c)$. I.I. Bigi, B. Blossier, A. Le Yaouanc, L. Oliver, O. Pene, J.-C. Raynal, A. Oyanguren, P. Roudeau. e-Print: hep-ph/0512270

[2] I.I. Bigi, B. Blossier, A. Le Yaouanc, L. Oliver, O. Pene, J.-C. Raynal, A. Oyanguren, P. Roudeau. Memorino on the '1/2 versus 3/2 puzzle' in $\bar{B} \to \ll \bar{\nu} X(c)$: A Year Later and a Bit Wiser. Published in Eur.Phys.J.C52:975-985,2007. e-Print: arXiv:0708.1621 [hep-ph]

[3] New exact heavy quark sum rules. Nikolai Uraltsev. Published in Phys.Lett.B501:86-91,2001. e-Print: hep-ph/0011124

[4] One interesting new sum rule extending Bjorken's to order $1/m_Q$. A. Le Yaouanc, D. Melikhov, V. Morenas, L. Oliver, O. Pene, J.C. Raynal. Published in Phys.Lett.B480:119-128,2000. e-Print: hep-ph/0003087

[5] Lattice measurement of the Isgur-Wise functions $\tau_{1/2}$ and $\tau_{3/2}$. D. Becirevic, B. Blossier, Philippe Boucaud, G. Herdoiza, J.P. Leroy, A. Le Yaouanc, V. Morenas, O. Pene. Published in Phys.Lett.B609:298-308,2005. e-Print: hep-lat/0406031

[6] Dynamical lattice computation of the Isgur-Wise functions tau(1/2) and tau(3/2). By ETM Collaboration (Benoit Blossier et al.). Talk given at 27th International Symposium on Lattice Field Theory (Lattice 2009), Beijing, China, 25-31 Jul 2009. Published in PoS LAT2009:253,2009. e-Print: arXiv:0909.0858 [hep-lat]

[7] Futur calcul reseau a masse finie

[8] Quantitative predictions for B semileptonic decays into D, D* and the orbitally excited D** in quark models a la Bakamjian-Thomas. V. Morenas, A. Le Yaouanc, L. Oliver, O. Pene, J.C. Raynal. Published in Phys.Rev.D56:5668-5680,1997. e-Print: hep-ph/9706265

[9] Exclusive semileptonic B decays to radially excited D mesons. D. Ebert, R.N. Faustov, V.O. Galkin. Published in Phys.Rev.D62:014032,2000. e-Print: hep-ph/9912357

[10] Semileptonic B decays into charmed p wave mesons and the heavy quark symmetry. Pietro Colangelo, Giuseppe Nardulli, Nello Paver. Published in Phys.Lett.B293:207-215,1992.

[11] Universal tau(1/2)(y) Isgur-Wise function at the next-to-leading order in QCD sum rules. P. Colangelo, F. De Fazio, N. Paver. Published in Phys.Rev.D58:116005,1998. e-Print: hep-ph/9804377

[12] Semileptonic B decays into excited charmed mesons from QCD sum rules. Yuan-ben Dai, Ming-qiu Huang. Published in Phys.Rev.D59:034018,1999. e-Print: hep-ph/9807461

[13] Theoretical analysis of $\bar{B} \rightarrow D^{**}\pi$ decays. M. Neubert. Published in Phys.Lett.B418:173-180,1998. e-Print: hep-ph/9709327

[14] The Decays $\bar{B} \rightarrow D^{**}\pi$ and the Isgur-Wise functions $\tau(1/2)(w)$, $\tau(3/2)(w)$. F. Jugeau, A. Le Yaouanc, L. Oliver, J.-C. Raynal, Published in Phys.Rev.D72:094010,2005. e-Print: hep-ph/0504206

[15] Study of $B^0 \rightarrow anti{-}D^{(*)}\ {}^0pi{+}pi{-}$ decays. By Belle Collaboration (K. Abe et al.). BELLE-CONF-0460, ICHEP-04-11-0710, Dec 2004. 24pp. Presented at 32nd International Conference on High-Energy Physics (ICHEP 04), Beijing, China, 16-22 Aug 2004. e-Print: hep-ex/0412072

[16] K. Nakamura et al. (Particle Data Group), J. Phys. G 37, 075021 (2010) and 2011 partial update for the 2012 edition.

[17] B. Aubert *et al.*, BaBar Collaboration, Phys. Rev. **D74** 031103R, 2006.

[18] W.Bardeen, E. Eichten and C. Hill, hep-ph/0305049.

[19] S. Godfrey, hep-ph/0305122.

[20] http://www.slac.stanford.edu/xorg/hfag/index.html

[21] C. Schwanda *et al.*, Belle Collaboration, Phys. Rev. **D78** 032016, 2008.

[22] P. del Amo Sanchez *et al.*, BaBar Collaboration, Phys. Rev. Lett. **107**041804, 2011.

[23] K. Abe *et al.*, Belle Collaboration, Phys. Rev. **D69** 112002, 2004.

[24] B. Aubert *et al.*, BaBar Collaboration, Phys. Rev. **D79** 112004, 2009.

[25] K. Kuzmin *et al.*, Belle Collaboration, Phys. Rev. **D76** 012006, 2007.

[26] B. Aubert *et al.*, BaBar Collaboration, Phys. Rev. **D74** 012001, 2006.

[27] V. Balagura *et al.*, Belle collaboration, Phys. Rev. **D77** 032001, 2008; hep-ex/0709.4184.

[28]