1) F(1) B->D(*) form factors (more input expected): difficulty for the lattice computation to achieve a high enough precision. Uraltsev BPS limit vs lattice? The small deviation between exclusive and inclusive determination of Vcb ?

→ **Damir:** (i) Understanding whether or not it is possible to find a window in "w" (or in "q2") in B-->D(*) SL decays that is both accessible experimentally and computable in LQCD

(ii) Experimental feasibility of Bs --> Ds(*) mu nu at LHCb [it is also better for LQCD as it would aleviate the issue of chiral extrapolation in the light spectator quark mass]

(iii) Further experimental research of B-->D(*) tau nu and Bs-->Ds(*) tau nu to understand if there is a disagreement with SM [there again lattices can check the behavior of the scalar form factor discussed in our paper http://arxiv.org/abs/arXiv:1206.4977]

(iv) Understanding a disagreement between the LQCD results for the form factors F(1) and G(1) with the recent estimate by Uraltsev et al.

(v) Study the full angular distribution of the semileptonic B-->D(*) and Bs -->Ds(*) decays [we are preparing with Andrey a paper on that where we define the quantities that could be revelatory of NP (if there is any! ;)), and Vera Luth also projected that this would be an are in which the further experimental progress could be made]

2) B->D: Many uncertainties both in theory and experiment are discussed but it turned out that it is difficult to quantify. Nevertheless, it would be important to write them down as much as we can say. Those uncertainties include: theoretical (infinite mass limit, …), experimental (role of background model, resonance, model, etc). See also comment 13).**

→ Patrick: Lattice result is $\tau_{3/2}(w=1) = 0.52 \pm 0.03$; $\tau_{1/2}(w=1) = 0.29 \pm 0.02$ corresponds to the ratio $\tau_{3/2}/\tau_{1/2} = 1.8 \pm 0.2$. Important to quantify the error more conservatively.

→ Patrick: importance of the q^2 dependence

→ Patrick: form factors are evaluated in the infinite mass limit. In practice form factors are not universal.

→ Marc: The J=1 D** states need to be included in the form factor computation. To this end one first needs to resolve these two states from a larger correlation matrix. This should give suitable linear combinations of operators for these two states, which then can be used for the computation of the three-point-functions, i.e. the form factors.

→Damir: concernant le tau(1/2)(1): le resultat BT est confirme par les reseaux dans la limite statique et l'espoir TRES realiste est que Orsay-Clermont verra bien le signal pour le cas ou tous les deux quarks (b et c) sont "dynamiques" [dynamique dans le sens ou les quarks ne sont pas statiques mais se propagent], et donc la question des corrections en puissance sera fixee ==> tau(1/2)(w=1) theorique sera tres prochainement connu tres bien.

→ Damir: la pente de tau(1/2)(w): l'exercice avec la combinaison des resultats non-leptoniques devient maintenant important parce qu'on connait la valeur de f(D0*) --

que j'ai presentee pour la premiere fois et sera publiee avant la fin de l'annee.
La, on trouve donc tau(1/2)(w-max), que l'on pourra ensuite combiner avec le tau(1/2)(1) theorique pour deduire la pente facilement. Pour cela manque le resultat exp. pour B(B- --> D0* pi-), cad. si on pourrait reduire l'erreur qui est actuellement 50%.

3) Proposition to use the hadronic decay in order to understand the ½ vs 3/2 problem in the semi-leptonic decay. The problem is less significant in Class I (and Class III?).
→ Patrick: The ratio of D2*pi/D0*pi is 1.3±1.0 (Babar), 6±3 (Belle), more statistics needed and detailed study of Dalitz components. Belle is close to the theoretical prediction but the accuracy is not enough to draw clear conclusions.
→Alain: 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 → D** transition, it interferes with a non exotic, therefore large, crossed channel ππ contribution. In Class III, one is freed from such a channel, but there is interference with the D** emission contribution. f **D is perhaps not very well 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.

4) We should summarize the experimental status of semi-leptonic and hadronic decays including D. For example the consistencies in Belle vs Babar as well as theory vs experiment should be discussed for each channels. And future possibility to clarify these issues in the future.**
→Patrick: D*' -> D* pi large! What if soft pion from D* escape from detection (any simulation available for B-> D*'->D*pi, D1pi D2* pi?)? To check this possibility it would be nice if BaBar can produce D π mass distributions for events selected with a tighter cut on the missing mass to avoid leakage from soft pions. It can be noted that Belle has already used this type of selection and has observed a similar result as BaBar.
→Patrick: In B^0 →anti-D π lν, BaBar and Belle experiments agree on the rate. Once the contribution from the D^*_2 is removed it remains a branching fraction of 0.42 ± 0.06 % for the broad component. This value is similar to the measured decay branching fractions of the B^0 into D_1 or D^*_2. If these events are interpreted as coming from the decay of D^*_0 mesons there is some contradiction with theoretical expectations.
→ Patrick: Hadronic D*pipi, small D1 broad component (Belle and theory good agreement)
→ Patrick: Leptonic B-> D* pi lnu, Belle found small D1 broad component while Babar measures large D1 (3 sigma discrepancy)
→ Vava: Tom Latham summarized our current work on this at the workshop. Certainly we are pursuing this topic vigorously. LHCb benefits from a fully inclusive trigger so we can e.g. trigger on part of the B->D**X decay and explore the full

possible range of X. Again things with Pi0/Gamma are of course much tougher for us but you saw some ideas in this direction from Justine as well.
→Vava: These were some of the first measurements which we made. We certainly have an interest in pursuing this further and Philipp Urquijo explained I think quite well what could be done with LHCb at the workshop; he is the expert here so if you would like more information I would advise to ask him to write about it. Again, we have a fully inclusive trigger so we can e.g. trigger on the muon + one of the hadronic tracks and look for the rest. Of course we have the missing mass problem but a lot of this can be overcome and the signals are actually rather clean as our first paper on this topic shows.
http://cdsweb.cern.ch/record/1326409?ln=en
→Alain: The inconsistencies between Belle and Babar are obvious in SL, especially in the D∗ π channel (in 1+ rate). They are also present in the NL (in 0+ rate); here, only the Dππ channel has been studied by both ; there is a difference, but within compatibility. It would be very interesting to have an analysis of D∗ ππ from BABAR.
→B. Loiseau: For three-body decays one can introduce an approximation where one assumes that two of the three produced mesons form one state (which could be a resonance) in a S , P or D wave. Assuming that this state originates from a quark-antiquark pair one can then apply the QCD factorization hypothesis to this quasi two-body final state. In this framework, the final state meson-meson strong interaction can enter through the knowledge of the meson-meson form factor (meson- meson transition to the vacuum). Field theory and dispersion relations show that the knowledge of strong interaction meson-meson form factors is available if the meson-meson interaction is known at all energies. So far our knowledge of meson-meson interaction is incomplete and the needed form factors can be phenomenologically extracted from semileptonic processes as, for instance, the ππ vector form factor from the Belle Collaboration analysis of the $\tau^- \to \pi^0 \pi \nu_\tau$ decays. They can also be built theoretically from our knowledge of the strong meson-meson interaction to which one applies theoretical constraints such as unitarity, analyticity and chiral symmetry using experimental data from processes other than heavy meson decays. This has been applied to calculate the ππ scalar form factor in the studies of $B^\pm \to \pi^\mp\pi^\pm\pi^\pm$ and $D^0 \to K^0_S\pi^+\pi^-$ decays. It has also been performed for the πK scalar and vector form factors in the analysis of the πK channel of the $B \to K\pi^+\pi^-$ and $D^0 \to K^0_S\,\pi^+\pi^-$.

5) Existence and relevance of the radial excitation state in semi-leptionic as well as hadronic decays. Have we seen some hint (which mass and what significance)? What is the issue to identify this state?

→ Patrick: if radial excitations are invoked to explain the large B →$D^{(*)}\pi\pi$ lν (close to 1%), then I would expect a similar signal in B →D*π lν. This is not observed. Maybe, Belle finds some events at the expected mass. So I would expect instead 0.1% contribution from radial excitations.
→ Damir: Excitations radiales: On n'a pas encore compris les excitations orbitales, et on s'inquiete sur les excitations radiales!
Uraltsev fait une pirouette et dit qu'il est partage si ce sont les ondes D (3/2 ET 5/2) ou les exitations radiales -- ou bien
les deux-- qui manquent pour saturer la largeur semileptonique inclusive.
Nous aurons le D' sur reseau avec le fD' correspondent et on verra au moins ca...
→ Alain: 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 suggest small contributions of the radial excitations (from their slope at $m_Q = \infty$)
→ Alain: As to experiment, it has been underlined that the radial excitations 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.

6) Lattice computation of the fD'/fD and (mD'-mD)/mD
→ Damir: A l'immediat --et ca sera ecrit dans notre papier qui sortira "bientot"-- le <D'| Vmu|B> ne peut pas etre calcule sur reseau
parce que c'est tres petit et c'est difficile de projeter sur D' uniquement. MAIS, SI la IW radiale xiR(wmax) etait non-negligeable
on aurait du voir plein de "bar B0 --> D' pi-". Je m'explique: une fois de plus, c'est un mode de classe-1 qui factorise tres bien
et comme les usines a B ont tres bien mesure Br(B0-->D pi-), en utilisant la valeur de xiR(wmax) de Galkin et al [prise aussi
par Bernolcher et al], alors on trouve Br(barB0-->D' pi-) ~ 10^(-4), ce que devrait etre bien mesurable.
Si on ne les voit pas --ce qui me semblait etre le cas, d'apres ce que nous a dit T. Latham-- l'interpretation en termes des excitations
radiales ne tient pas la route.
Ceci dit il faut peut-etre mieux chercher les "barB0-->D' pi-" pour etre sur, mais...
→ Alain: Lattice calculation of annihilation constants $f_{D(*)'}$ in course allows to evaluate class III $B \to D(*)' \pi$ through $D(*)'$ emission -pion emission being presumably small for the above reason-. This could be tested experimentally.

7) The experimental problem of semi-inclusive semi-leptonic decay vs exclusive one? What did we miss?

8) Treatment of the non resonant states: modelling uncertainty.
→Alain: 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.
→Alain: This central point of broad resonance has not been discussed much in the colloquium- it also implies discussion on the Breit-Wigner forms far away from the peak, and of the continuum.

8-1) For SL: Is there any other non-resonant continuum model than Goity-Roberts?
→Alain: There is not much of safe theory on this question. So far, the heavy quark chiral-perturbation theory, which is the basis of this model, must be improved by including the various corrections. For example, in reality, the unitarity is not easily implemented due to broad resonances and the proximity of the threshold. Simple recipe is to introduce the width but is it sufficient?
→Alain: Nevertheless, the advantage of this model should be more carefully

considered when applied by the experiments. I) the so-called virtual D(*) contribution is explicitly calculated. One should stress that the D*Dπ coupling is well known so that “$D(*)_V$” is not a free parameter. II) the remote resonances, including the D’ contribution, are included. The problem is that some unrealistic parameters are introduced for L=1 states and for the radial excitations.

→Alain: Attention must be drawn to this question of $D(*)_V$, because it is included explicitly in the background of Babar Dπ events (SL), in Belle it’s considered only as an alternative to 0+ in the signal. What is the magnitude of this contribution ? Is it fitted or predicted as it should from $g_{DD^*\pi}$ coupling?

8-2) For NL: One can hope to measure the Dπ phase in the Dalitz plot. Is there anything other than the 0+ state?

→Patrick: this can be investigated using B^-→D+π-π- (i.e. class III). The analysis of the Dalitz distribution may allow to measure the phase of the Dπ system versus its mass in a model independent way !! and check if it corresponds to the variation expected from a Breit-Wigner distribution.

→Alain: This phase should be the same in SL and in NL class I since it is in the elastic region.

9) Lattice : interesting proposition of the spectroscopy computation by Sasa. Question of chiral extrapolation, threshold effect, broad resonance

→Sasa: A comment on lattice studies of broad scalar D0*(2400) resonance:
I would like to point out again our experience: if this resonances is above threshold in a lattice simulation (m_D0* > m_D+m_pi), our ground state from variational analysis is D(0)pi(0), not D0*(2400) (our mpi is 266 MeV).
This is true also if we take only qbar q interpolators in the variational interpolator basis. The same applies for the simulation of the broad axial D1(2430) - our ground state is D*(0)pi(0). The presence of the scattering energy levels D(0)pi(0) might be the reason for certain problems in simulation of the spectrum and form factors related to these states.

→ Marc: The critical remarks by Sasa that the broad 1/2 D** need to be treated as resonances on the lattice need to be checked. This could be done by computing a Matrix element like < D + \pi | D_0^*>; with this number one can hopefully argue that a possible decay of D** --> D + \pi is irrelevant at the temporal separations we consider (cf. e.g. hep-lat/0404010 for a similar computation).

10) Spectroscopy

→ Sasa: Confirm by an independent experiment the BaBar 2010 results on excited D-meson spectrum (radial and orbital excitations).

→ Alain:

It seems very encouraging that the two lattice methods, with or without inclusion of four quarks operators, converge and explain the experimental low mass of the 0+ : 2300 GeV. A useful proposition could be that one abandons the misleading notation D0 (2400) One important point has been the first results on I = 1 phase. Further steps can be envisaged : to have a more detailed prediction of Dπ scattering phase, and to extract it experimentally from B → Dππ. The question would be : is there any serious shift with respect to the Breit-Wigner forms at low mass?
→ Alain: One has also learned that the discrepancy with GI quark model in the location of DsJ is not solved, in contrast to D∗∗ . At least the $1^+1/2$ is still predicted much too high. The more accurate method gives also a too high result for Ds0 . Although there is little doubt that these are $\bar{q}q$ states, could it be that rescattering effects alter the spectrum ? This is why, in my personal opin-ion, (amateuristic,)it could be worth considering 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 cupling channel effects.

11) Question of "excess events"

→Alain: There are two distict questions. 1) SL: admitting the experimental analyses of Belle and Babar, one can perceive definite excesses of raw data with respect to the fit at low masses, especially at Babar. Babar seems also to find such an excess in the electronic spectrum. The Dπ data of Belle at low mass do not seem simply to be fitted by the claimed 0+ BW. 2) admitting small values of tau1/2 as suggested by theory, how would one explain the events previously included in the high experimental rate for the resonance ?
→Alain: One has a vague impression that "excesses " of types 1) and 2) described for SL have no clear counterpart in NL. 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. One must stress that factorisation assumption is not implied at this stage, only the quasi-two body approximation. As to excess of type 2), although the rate to 0+ is much smaller in Belle than for SL (with respect to 3/2), they do not require a large compensating component. In Babar, a particular strong NR continuum is used, which is destructive, and the rate to resonace is then larger.

12) Theoretical request to LHCb

→ Vava: LHCb has made measurements of excited baryons, e.g.
http://cdsweb.cern.ch/record/1449721?ln=en
We are actively pursuing these further, but again it would be useful to understand how these measurements and measurements in Λ_b->Λ_cXμμ help to clarify the situation in B->D**Xmunu etc.

13) ½ semi-leptonic puzzle by Alain (unvertainties in quark models)

→Alain: 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, and not in NL.

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. So, to avoid metaphysics, let us stress what is the real question.

-”Theory” is meant not to be QCD but rather the quark models, and 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. Specific prediction of our quark model with GI wave functions inserted in the Bakamjian-Thomas framework for transitions and with m_Q=infinity . The next step for QM is to give predictions at finite masses, which is straightforward.

Let us stress a point which is contrary to prejudice : there is no way to give an a priori error on the predictions of QM. Such a concept like a priori uncertainty is not well defined or not calculable. Only a posteriori comparisons with experiment, once experiment is known, can be made, or, where available, with lattice QCD. An extensive comparison of the BT approach with experiment for hadronic transitions, which has not been performed, could give a qualitative idea. The comparison with lattice QCD on current densities suggests a good agreement at infinite mass, rather extensive in spatial dimensions. The ξ (w) is a good fit to the physical h_{A1} or F, G as well, to the rough precision which is relevant. And the BR of B → Dlv , which is the most safely measured (if a little less accurate), is quite good : 2% against expt. 2.2%

-As to experiment, 1) what is under discussion first is the exclusive NL or SL rates to resonances, not the D(∗) π(π) semi-inclusive SL rates for which we have simply no theoretical prediction.

2) we start from the experimental error bars as they are quoted; for the SL, note that they are unexpectedly narrow for the broad states j = 1/2 : around 20%, although the statistics are small in SL, and the systematics are expected to be very large, due to the difficulty of separating resonance and continuum for very broad states (cf. baryonic resonances) ; one may be surprised that with much more statistics, the errors for the NL case (charged B) are much larger .

-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/mQ effects as discussed by Leibovich et al..

-Let us then reiterate that the main problem is on the SL, not NL.

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).
Could we have a theoretical error in SL so large as to comply with the claimed experimental numbers ? we doubt much from what has been said above. Moreover, enlarging the theoretical predictions for NL by a factor 5 would give numbers completely contradictory with the data (as given in the table p.17 in the talk, table due to Patrick).
-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 → DsJ 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 S U (3).
2 -Moreover, from the discussions, one important point should emerge : that we must not try to ”solve” the problem of SL decays in an isolated fashion, without paying attention to what would happen correspondingly in NL decays (see below).

14) Proposition to test the ½ vs 3/2 problem in another channels: Bs-> Ds**? Bs**?

→ Alain: Importance of the measurement of **Bs-> Ds**π. Combine the advantages of NL decay (being only a three body final state, e.g. Ds π0 π+ without neutrino) and narrow resonance (no problem with non resonant events nor the ππ crossed channels). Safe estimate of Bs ->Ds0+ transition.**
→Vava: We have published papers on Dsj and Bs** states, measuring Q values and in some cases widths and branching fractions
http://arxiv.org/abs/1211.5994
http://arxiv.org/abs/1207.6016
as well as a conference note on B0** and B+** states
http://cdsweb.cern.ch/record/1374165?ln=en
We are actively looking for higher mass states. So far we have only looked at B/D+charged track combinations, but I have taken away from the workshop that we should also be looking for B/D+gamma/pi0 modes and I will transmit this forward. It would be very helpful to understand more quantitatively how direct observations of these resonances and measurements of mass/width/relative production would feed back into understanding the B->D** puzzles outlined at the workshop.