

The future of charm physics: a discussion

B.D. Yabsley (ed.)

School of Physics, University of Sydney, NSW 2006, AUSTRALIA.

We closed the CHARM 2007 workshop with a lively panel discussion on the future of the field. This document presents a summary of the key points, and a lightly edited transcript of the discussion itself.

1. Introduction

The CHARM 2007 organisers (and advisory committee) deliberately chose to close the workshop not with a summary talk, but with a panel discussion. This document, accordingly, tries to respect the idiosyncrasy of that discussion, rather than seeking to be comprehensive or to be too “on-message”.

The panel discussion followed four talks on future facilities — the relevant speakers were all panel members — and could helpfully be read together with those presentations. The raw material for the discussion was a set of questions from workshop attendees, and some absent committee members.

2. Key points

- Charm physics is intellectually alive as a field, and we plan to keep it that way.

Mixing and CPV

- We now have good evidence for mixing, and may even breach the 5σ threshold in individual channels, with the 2 ab^{-1} that’s currently within reach. With twice that dataset, the chance will be good. (And CDF will help!)
- There’s no obstacle (in principle) to calculating potential beyond-the-Standard-Model contributions to mixing on the lattice. But current methods won’t cope with the nonlocal operators that we believe dominate the SM contribution.
- Finding and studying CP violation in the charm sector is (we hope) the next big thing. CPV-in-mixing has long been a goal, but is only now within reach, with evidence for mixing established; controlling systematics at the 10^{-3} – 10^{-4} level may be challenging. And let’s not forget direct CPV studies ...

QCD, spectroscopy, lattice ...

- The charm sector is a good laboratory for QCD; we’re still learning from it, and continued to do so at this workshop.

- Not every bump is a new state: it’s possible to get complicated structure from a single pole. Beyond the mere “discovery” of “new states”, experimental determination of quantum numbers and decay properties is important to provide enough information for theoretical analysis. Interpretation can be difficult even so.
- Very large data samples, and improvement in quality (*e.g.* BES-II \rightarrow BES-III), will help.
- A really good precision test of lattice QCD requires the use of CKM-free quantities, *e.g.* the ratio of semileptonic to leptonic decay rates. Very large data samples will help here, too.
- Experimentalists and non-lattice theorists should interpret any lattice calculation in an informed critical spirit. Is the calculation unquenched? With $2 + 1$ sea quarks? How thorough is the systematic error analysis? If the result is to be compared to experiment, there should be a positive answer on all three fronts.

Facilities in the future

- There are plans at BaBar to maintain an analysis effort (including priority-setting) after data-taking stops. Still, it may be tough.
- There should be active experiments in the flavour sector, to complement the LHC.
- There is strong support for a so-called super-B or super-flavour factory in our community, in addition to BES-III. While the B-physics programme is its selling point, it will be highly capable in charm (& charmonium & ISR & ...)
- The way forward in hadronic $p\bar{p}$ physics is to take a global approach, with a full-capability detector: PANDA. (Earlier-generation experiments were already at their limits.)
- **It would be very helpful to form a “shopping list” of measurements we want LHCb to make in the charm sector.** It could have real influence. Analysis will be limited by manpower and interest as much as the trigger ... and note that the upgrade will likely have a software trigger, to the benefit of charm studies.

3. Dramatis personae

Panel members:

[DA] David Asner

[DB] Diego Bettoni

[AE] Aida El-Khadra

[EG] Eugene Golowich

[PS] Patrick Spradlin

[YW] Yifang Wang

and Bruce Yabsley

(chair: comments listed in italics)

Contributors from the floor:

[EE] Estia Eichten

[BM] Brian Meadows

[KP] Klaus Peters

[AP] Alexey Petrov

[JR] Jonas Rademaker

[AS] Alan Schwartz

[KS] Kamal Seth

[??] at least one significant contributor is currently anonymous

4. The discussion

Thank you everyone for joining us for this last session. You know everyone on the panel: they don't need any further introduction. Let me just show you what we're going to do. You can see the outline here, divided into

- *current issues:*
 - *mixing and CP violation, and*
 - *everything else;*
- *the future of the field:*
 - *in general, and*
 - *concerning future facilities.*

We'll try to give about 15 minutes on each of those categories, picking out questions out as we go. But to begin, I'm going to throw the microphone to Gene: we've heard four talks from experimentalists, I thought we'd get a few remarks from the other side of the divide; and then we'll head to questions.

4.1. Overview

[EG] Bruce asked me to be provocative and nasty, but it's really not in my nature. Actually over lunch we were talking about the future of the field, and I was drifting off, and ended up in a fantasy world where things were done the right way. And in this world the LHC was in fact built and came on the air, and found the Higgs, and found many new events that we couldn't explain with the Standard Model. And people had realised that in order to interpret these possible signals of new physics, we would also have to have flavour physics studies of rare phenomena, so that we could start to see patterns emerging ... and working symbiotically together, the LHC and the flavor sector would get to the root of what was happening, something that would be very difficult if not impossible to do with the LHC alone.

But then I woke up. And I thought about a colloquium I'd given recently, where one of the chief experimentalists there took me into his office and shut the door and said to my face, "Flavor physics is dead!" and apparently he's not the only one who said it: some pretty important people have said it. And when something like that is said over and over it begins to have a truth of itself.

So, anyone who's been at this workshop realises that that's not the case: that not only are we getting wonderful results, there are all sorts of ideas — arguments even — about what to do for the future, about what's going on, and that's the sign of an intellectually alive effort. And part of what we're doing on this panel, I think, is trying to see whether or not we can realise the potential of the field.

I know that in my own shopping-list I tend to divide it between electroweak physics and QCD, and I'd like to see efforts in both halves; I'm interested in both. My number one wish, as far as the electroweak sector is involved, is that we **find and are able to study patterns of CP violation in charm physics**. As regards the QCD part of things, I really find that **charm has turned out to be a terrific laboratory for QCD and its workings**, we're still trying to figure out what's going on: all the data's not in yet, and I think we're going to need every bit of data to understand what's going on, and that's been a terrific part of this workshop.

So the completion and growth of those two areas would be what I'd hope most for, but by no means saturates what I want to see. Now Bruce has handed out to us a bunch of questions that we've gathered from various folks; so

let me throw it back to Bruce now — and make sure you have a live microphone.

4.2. Mixing: (x, y) , the lattice, and CPV

The first question is a reasonably simple one: whether we can firmly establish that mixing occurs — that x and y are not equal to zero — with (say) 2 ab^{-1} of data. And going further than that, whether we can determine (x, y) themselves with twice as much data. Anyone want to take that?

[DA] Sure. If the current central values hold up, then 2 ab^{-1} is close to sufficient for 5σ signals in two or three channels. If the current central values don't hold up, one might hope that at least 4 ab^{-1} is ... so yes, that's certainly the right ballpark.

Brian, you don't disagree with that? Either of the Brians [Petersen and Meadows], since you're sitting together? No?

[JR] Just one comment: CDF will contribute to these measurements very soon as well. So there will be some help even if the situation is marginal with 2 ab^{-1} : contributions from hadron colliders [will help resolve x and y].

*Alright, so the next question may or may not be easy because I genuinely don't know the answer to it — I had a pretty fair idea of the answer of that first one — and that's the following: **Calculations of New Physics contributions to mixing rely on determinations of four-fermion matrix elements: what can we expect the lattice to contribute to that?** So that's a question for Aida.*

[AE] So the methods for calculating 4-fermion operators that contribute to $B\bar{B}$ mixing are quite mature and ongoing — there's a result by the HPQCD collaboration using NRQCD heavy quarks — and in principle there's no barrier towards repeating these calculations for charm. HPQCD would use the HISQ action, and our collaboration [Fermilab] also has an analysis in progress for $B\bar{B}$ mixing, and we can certainly do this for the charm sector too.

I should add a note of caution however: what we can do with current methods are calculations of matrix elements of local operators. So we cannot say whether the observed $D\bar{D}$ mixing is Standard Model or not, because that involves non-local operators — because you have light particles propagating — and that's something that we can't really calculate with current methods. But if one wants to know the expectation values of local operators that would come from beyond the Standard Model: those can be calculated.

OK so let me see if I understand the caveat: What about the extent to which the observed pattern of mixing comes from the interference of SM and NP contributions? Does your caveat about long-distance operators mean that one has a grey area there? Or do they not interfere because they are short and long distance?¹

[AP] In New Physics, ΔM or mass-difference comes from heavy intermediate states, and even those intermediate states are not present in the SM: squarks, gluinos, whatever. So regardless of what happens, at the end of the day you get a bunch of matrix elements at the charm scale ... and there are only 8 of them. So regardless of the New Physics model, you will get those 8 operators. And so the matrix elements of those ...

[AE] And they can be calculated ...

[AP] Yes.

It may not all be this easy.

*We often say that CP-violation-in-mixing is the real New Physics signal, and this is part of the progaganda, but up until now the focus has essentially been on x and y . **Do the experimental plans that the existing and future collaborations have, reflect the need to measure CPV in mixing?** Comments?*

[DA] The short answer is of course yes. Every mixing paper in the last five years has had CP-violating limits although they've been trivial; and one of the most active efforts now of the HFAG-charm mixing group is to come up with meaningful CP-violating limits which are actually posted on the site, if you choose to go look at it ...

So yes.

But that's a nominal yes, isn't it? I understand that (say) for the $D^0 \rightarrow K^+K^-$ channel we included a CP-violation measurement, but that was essentially trivial, in that it came almost for free with our y_{CP} measurement ... aren't there more difficult CPV studies?

[DA] Well it comes for free with a D^* tag as well, for other measurements ...

Well, at an e^+e^- machine it does.

¹The discussion moved on from this question, and it wasn't followed up at the time. For the record, the answer is that in addition to the problem of hadronic uncertainties in the SM predictions, "we do not know the relative *phase* between the SM contribution and that from any NP model, so that x_D will lie between the extreme limiting cases of constructive and destructive interference." [1]

[DA] Yes. Well, perhaps the fact that the LHCb mixing studies are currently CP-conserving might indicate that the experimentalists haven't caught up with this ... but it's certainly on the agenda.

[PS] Yeah. Our plans have recently changed focus, to look more at CP violation. What I was presenting was some of the more complete studies, incomplete as they were. And our initial efforts were towards mixing measurements. But there's been a fundamental shift in what we've been [studying], towards CP violation, and CP-violation-in-mixing.

The statement is always in the papers but the emphasis isn't always in the slides, if you see what I mean, or in people's spoken remarks. So I guess that's what was behind the question.

[??] Just related to that, to do the CP violation, at least the direct one, you need to get asymmetries also from detector effects and so on, so how well are LHCb and SuperB geared towards handling this? (And I guess you have similar issues in B-physics, right?)

*This is actually the next question, or part of the next question, which is about systematics: **It's one thing to do a percent-level measurement of mixing, but it's quite another to do a 10^{-3} or 10^{-4} measurement of CPV. That's a whole lot more demanding, and it's going to be systematics that are the problem.** Has LHCb thought about this?*

[PS] Yes, but unfortunately we've only begun thinking about systematics. We have a lot of ideas on how to evaluate them in the data itself. But that's a work in progress.

Alan wanted to speak to that.

[AS] Actually I just had a comment on [the earlier point]. You have to keep in mind that to see any CP-asymmetry effects, you need a nonzero x and y . But it's only been since the spring, since March, that we've had such evidence in hand [2, 3]. So that, so when you say do experimental plans reflect going after [the CPV phase]: since March yes, but before March, even in Belle when we measured those things, as you said, they were not the headline measurements, because we didn't know what x and y were.

[AP] Well this is true if you stick to CPV-in-mixing. In both of those cases. I was actually wondering why the person who asked the question wanted to know about time-dependent CP asymmetries,

I mean of course you have CP-violating asymmetries that are independent of x and y — direct CPV effects — and you know, those are easy to study, although experimental issues are similar; I don't know, if you look at D^+ decaying to a state that contains charged kaons, detector effects for kaons of different charges are significant ...

But the point is that you can have CP violating signals that don't have [time-dependence] ...

[BM] I just had something ... I think the last mixing paper that I read — experimental paper — that I read that didn't say anything about CP-violation in mixing must have been at least 10 years ago.

4.3. Rare decay measurements

*We're going to hop forward to some stuff on rare decays now: something not unrelated to CPV. This actually cropped up in one of the talks earlier: that there hasn't been much work at the B-factories on rare decays. Despite our propaganda, and despite the fact that we put it in all of our proposals and statements, and all the rest of it — and I certainly did, for the charm programme at Belle — so the question is **what the prospects are for rare charm decays in the rest of the B-factory era, or perhaps for a super-flavor factory.** Why aren't there more rare decay measurements?*

[DA] Everybody complains about manpower, right?

[Well I've seen rare decay measurements done, and then not turned into an ongoing programme, because the judgement was that other work would better advance people's careers. And that judgement is probably correct.] So, you know, "manpower" ... there are manpower issues but they're not always just the sheer number of people.

[DA] To be fair: until rare decays can probe SM sensitivities they are just not that interesting. We have a long history of not finding new physics, and if you want to sign up a grad student to work on something where we know he's not going to find new physics ... well we can do that, [and there are many ways of doing it, but there's little motivation for it.] But when we do have sensitivity [at the level of the SM predictions], I suspect there'll be rather a large line of people who want to perform the measurements.

But I wonder about this argument, because we've been busy doing charm physics at B-factories, and I've had guys bowl up to me at conferences and say "The beauty sector is the most rich in physics at the moment (which is true), and offers the best prospects for

finding physics beyond the SM at the moment (which is also true), and therefore it's the most exciting thing, so why would you work on anything else? And in particular, why are you working on charm?" And in the terms in which that question is posed, I don't know that there is an answer to it. And isn't [the statement about rare decays] a species of that same argument?

[DA] Well I'd rather address your first part first: Rare decays are much more interesting in charm than they are in B, because rare decays actually include CP violation and mixing, which are also rare in charm but not in B; so from the get-go rare charm processes are a broader field. And I'm not actually certain how what I said translated to the analogy [with charm-versus-beauty].

[KS] As long as you have this up, I have a question for David Asner. When you were comparing what could be done at LHCb and the super-B-factories, most of the time the things were sort-of comparable, but there were a couple of instances in which you said you really need SuperB for that ... Now I have to ask you a question whose flavour is completely different from [that of] your talk. Does one, for those rare cases for which SuperB is essential, spend \$500 million?

[DA] That's a good question: it depends what part of the programme of SuperB you think is most important. If you are a tau physics enthusiast, there is no comparison between the tau physics programme at SuperB and the tau physics programme at LHCb. [And for] anything that requires a primary vertex (including, probably, charm semileptonic decays) the performance will simply be better at SuperB. To be fair, I've probably represented LHCb in a rather favourable light. After all, it's going to be an existing experiment.

Will the world end if we don't have the charm physics programme from SuperB? No. We're not trying to sell SuperB on its charm physics programme. But if you can motivate SuperB based on the way that the B-physics interacts with the LHC programme, then you get the charm and the tau and the charmonium and the ISR programmes *etc.* for free, assuming that you can assemble the workforce.

[KS] But if there's one thing I have learned, it's that when someone is blessed with huge luminosities, they have the advantage of making cuts and throwing away so much data that they can [do almost anything]. I always used to say that the B-factories, if they wanted to get at whatever I might be doing, they can cut everything out and

still beat us on total statistical and other kinds of errors.

And that is what makes me wonder about comparison between LHCb and the SuperB. LHCb will have so much production of whatever-you-want, that you wonder whether they have the ability to throw away a lot of it, and become, you know ...

[DA] Well they do throw away a lot of events: it's called the trigger, right?

[KS] Yes, the trigger, and whatever else you want ... and still reach the statistical precision [of SuperB].

There are also issues of systematics, remember: things that a hadron experiment by its nature doesn't do as well. Alan?

[AS] There are many final states that would be very challenging at a hadron collider: π^0 and η and charged ρ , you know, those will be very difficult at a hadron machine. You will not measure $B^0 \rightarrow \pi^0\pi^0$ at LHCb — I don't think — and I don't think you'll see $B^0 \rightarrow \rho^+\rho^-$; and many of the η and η' decays will be very hard. I mean, obviously they can be simulated, but there are many many final states where I don't know how it's going to happen in a hadron environment.

[BM] Just one thing to add to what Alan was saying: One thing you probably want to do in looking for CP-violation in mixing is, you want to see if there's a difference in x and y for D^0 and \bar{D}^0 . And then you want to see if they're the same in different decay modes, in all the different decay modes. And I think that at SuperB that will be much easier to do.

4.4. Do we claim too many new states?

*I'm going to move on, to something that's going to induce a different kind of argument, but this is also about perceptions of what is exciting and what's not. One of our questioners wanted to know **whether we are claiming too many new states, either quarkonium-like or in general. And what about dynamics? Particularly near threshold, since a lot of these states do seem to be near threshold, ... An awful lot of new states are being claimed, some of them on our experiments' websites. [I think Yifang has his hand up to answer this.]***

[YW] I think, in fact, we do see a lot of resonances, or particles, or structures — whatever you want to call them — near the threshold, and I think there's a very simple experimental reason, in that near the threshold the background is much

lower, so wide resonances are much easier to see
...

OK, but let me pick you up on the words that you used there: yes it's easy to see "it", and you said "resonances", "new particles", or "structures", but just because you see nontrivial structure, it doesn't mean that there's a new pole sitting [on the complex plane].

[YW] I agree: I said all these possibilities, because now, particularly at BES [where there are so many things that we see], we have a lot of arguments about whether new particles or structures or enhancements (or whatever) exist, and I think shows that we need more statistics, and we need a much better detector.

No, but hang on: don't we also need better interpretation?

[YW] Of course, later on; I think the important thing, right now, if we see a signature of (say) 5σ , 4σ sometimes, 6σ , then many people believe it, some people don't believe it, and for both experimentalists and theorists, [the confusion] makes it hard to work together. Now if you have a much better detector, and significantly improved statistics, if you see a signal at a level of say 20σ , or 50σ , then there's no question of whether these things exist or not, and it makes it easier for both experimentalists and theorists.

*Perhaps it will clarify the point [if we consider another example. There have been many claims of new states at Belle, and one of the most interesting pieces of recent work concerns the extra enhancement below the 4260 , in the $e^+e^- \rightarrow \pi^+\pi^-\psi$ cross-section. Yes? I think everyone has seen this. Now in the paper [4] we were very careful not to say "this is a new state". But in *arXiv* postings and on the web, it is being given a name, a "Y" and a number, with it being implied or stated that it's a new state.] And that reflects the fact that for (I think) 80% of the people in the community saying that you see a significant enhancement — and it is an enhancement, and it is significant, OK, that part's not in question: a better detector won't change that — but the first thing we always lean to is, "it's a new state". And that's been the first interpretation rather than the last one, for lots of things from Belle, from BaBar, from BES, and what this question is asking is, **Is that really a good thing — and if it's not, how do we get past it?** And Estia wants to speak to that.*

[EE] I mean, when you have wide structures, it's always rather difficult to figure out what's going on, and you can certainly show examples where a single pole will give rise to complicated-looking structure.

So there's two separate questions. For the theorists, they have to figure out how to interpret the structures that are seen in experiment, in terms of what's underlying them. I don't know how the experiments can do that, but what experiment can certainly do is describe the properties of those structures — make sure you understand its quantum numbers, if you interpret it as a state, its decay properties *etc.*: these things will certainly make it a lot easier for theorists to try to disentangle wide structures into what actually is underlying them [and whether] there are additional states.

Every bump is not a new state. You can show examples when it is clearly not ...

And was it you that showed that point about the $\pi^+\pi^-\psi$ and the $\pi^+\pi^-\psi'$ [cross sections, in ISR data from the B-factories], and that maybe ...

[EE] Yes, it may very well be that there's one state with different decay modes at different energies, whose branching fraction varies as a function of energy. Now, I can't calculate that one, because I don't know what it is — if it's a hybrid of some sort, then I don't know how to do the calculation of how much it'll decay into those two modes — but that's (in a sense) a theorist's or a phenomenologist's job to try to figure that all out, but experimentalists can certainly try to disentangle any other decay modes that are in the same region, for example, besides the ψ or the ψ' , maybe there's another mode in between that can fill us in on what's going on. But that's all an experimentalist should [be expected to do]. The interpretation is difficult.

[EG] This goes back as long as I can remember in hadron physics: the $\sigma(600)$ is something that people call different things ... I don't know what to call it, but if I do know how to use it, you can call it whatever you want.

[AP] There's actually a little counter-example to that: You all know the story of the pentaquark, which was very well predicted by several groups, and very well observed by several different experiments, and yet it's gone. And so it's natural that you see so many states, well not states, but structures. Time passes, and you sort out which of those are real states, and which of those are just threshold things ...

[YW] I think I haven't finished what I want to say, so let me finish, and then we can go forward. I think it's very important to have more data. If you have 4σ , 5σ , how do you disentangle whether it's a real particle, structure, a fake, or so on? But once you have significantly improved

statistics, you can certainly establish whether this is a real structure, and you have the possibility to understand its decay properties, dynamics and so on, and then you can have a much better understanding.

Put it this way: up to now, we have studied a lot of electroweak physics, but we understand QCD much less well. And we know that we should see a lot more resonant states, hybrids or whatever, glueballs, beyond the conventional $q\bar{q}$ or baryon states: certainly more than what we see.

So with significantly improved statistics . . . put it this way, we now see many 4σ signals. Over time, BES-III will increase statistics by a factor of a hundred, and on top of that the detector is much better — corresponding to an improvement, for the same statistics, by a factor of two or four — so this is an improvement of almost a thousand in total. So you can imagine: with one thousand more better signals, I think it will be revolutionary, and for many unresolved problems or questions, [it will give us] a much better understanding. So I think it's important to have more data. And by that time, once we have firmly established particles and their properties, theorists can work together, and figure out all these details. And [regarding] QCD: by that time, we'll have a much better understanding.

4.5. Comparing the lattice to experiment

*We're going to move on. This is a question I think you'll even like: **Suppose experimental and lattice values for decay constants disagree by 2σ . Is that a discrepancy, and should we be worried about it? And if 2σ is not, then what about 3σ or 4σ ?** [To AE:] Do you have a view from your side?*

[AE] I would say that at present, the situation is not yet completely clear, whether or not there is indeed a discrepancy.² I'd also like to point out that the experimental values for the decay constants have to assume a value for V_{cs} , which is not very well determined at this point. It's hard to imagine right now that V_{cs} could change too much, but you can have at present a few small effects which would bring experiment and theory back [together]. I think once the error bars decrease more, then we would be more worried.

To really do a precision test of lattice QCD without any assumptions about CKM angles, we need to compare the CKM-free quantities like the ratio of the semileptonic decay rate to the leptonic decay rate, where the CKM angle does not contribute. So [at the level of precision reached by HPQCD] I don't think that just looking at the decay constant itself is really [enough]: you want to look at the ratio of semileptonic decays to leptonic decays; you want to look at the shapes of the form factors; you want to see if you extract V_{cs} from the semileptonic, does that agree with the result from the leptonic case, to see if there really is a discrepancy. So I think we need more information than just the decay constant. And I think that's part of the programme.

And that's a point where Yifang's argument about overwhelming statistics is important, because with overwhelming statistics you have the luxury of doing those assumption-free tests that you're talking about.

[AE] That's certainly true. I mean, two sigma statistical fluctuations are not unheard of; and while Peter Lepage might vehemently disagree with me for saying this, it's not absolutely impossible that (say) the HPQCD result might have slightly larger errors than they [claim], in which case the discrepancy is not as large as it currently is. So I think it's certainly important to monitor and keep in mind, but ah . . .

Does anyone else want to speak to that? It's a fairly complete answer, but . . .

[AP] I'd like to ask a question about the errors that lattice calculations are assigning to their numbers. I mean, I used to see nice lattice predictions with very small error bars that were completely overtaken by new lattice predictions, and people would say that they're inconsistent, but "That's OK, because the previous error bar did not include quenching errors." Now, how can one claim an error bar if there is an error bar sitting on top of that, which you don't even know what it is? Now I understand that with the new techniques this [is no longer the case]. But when you now do things at the level of a percent, then none of these errors, they're not independent, and you have to take into account correlations between them.

[AE] And we do. And we do. We have learned a lot in the last ten years on this. I want to make this point very clearly, and I think it requires non-lattice theorists and experimentalists maybe to have some knowledge, but if you read a paper and it says we are doing a quenched calculation

²The reference is to the disagreement in f_{D_s} between the new lattice calculation (241 ± 3) MeV [5] and the CLEO-c average ($275 \pm 10 \pm 5$) MeV presented in Steven Blusk's talk at the workshop.

and we get this result with this error, just, don't even ... they did an error analysis ...

But it's quenched.

[AE] But it's quenched. And even if they have some estimate for the quenching error, but if it's not anywhere close to 20% or so, you just shouldn't believe it. The same thing with calculations that have two flavours of sea quarks, that's not enough: there are three in the real world. So, you know, I don't think it makes a whole lot of sense to compare quenched lattice calculations to experiment. I also don't think it makes a lot of sense to compare lattice calculations based on two flavours of light sea quarks to experiment. [These comparisons are not interesting to people outside of lattice field theory.] And that's the reason I only showed results based on $2 + 1$ sea quarks.

And the second point to your question, it's a valid point: we have a long history as a community in lattice field theory, certainly [more than] 10 years ago people were not worried about systematic errors and as we got more sophisticated we became more worried. And not everyone in the field understands that they need to be very careful with the systematic error analysis. But I think that people who are claiming to do serious calculations need to be able to show you plots: Do you understand the light quark mass dependence of your result? Have you looked at the lattice spacing dependence? I mean you need to have a very sophisticated systematic error analysis, and if you don't, you shouldn't believe the result. [Certainly not at the few percent level.]

And it's always good to have more than one person, because these analyses are complicated. I mean more than one group — one person never does these analyses by themselves — but more than one group do these calculations with different methodologies and see that you have that consistency. So, you know, we have the HPQCD result, we have our result, which is a factor of two less precise than their result, but within our error, the two results are certainly very consistent.

[DA] Can I jump in real quick?

Quickly ...

[DA] Speaking to the experimental determination of the CKM-independent quantities, at best I can tell from the two reports I mentioned earlier, it looks like the measurement is at about the 10% level. Part of the BES-III programme will get that to, you know, a few percent, and SuperB will be required to get 7%, but these are part of ...

We might come back to that later.

4.6. BaBar data after datataking ends

*Before Gene goes, I just thought we'd ask the question you see here: **whether there will be an analysis effort on BaBar beyond 2009. And what the plan is for doing such an obviously difficult thing?** I mean, we have some BaBarians or ex-BaBarians here in the room: anyone want to speak to it?*

[??] As far as I understand, there will be three years of ... at least the infrastructure for doing analysis for three years after datataking.

What does "infrastructure" mean?

[??] All the computing, so you can process all the data, do the simulations, and put new decay modes in.

Right, I mean, if you want to do ten decay modes, will you have a supply of slaves to do it, or will you have to do it all yourself?

[??] Clearly, there is a very limited number of new graduate students coming on, already now. So, ... they will be graduating in the three years ...

So what's being done to prioritise measurements?

[??] We have a core list of results which were scheduled to come out ... somewhere next year or the year after that. And they are being prioritised within B-physics and charm physics, so for instance, the mixing analysis will be updated with the full dataset. So there is a list of people, with their names assigned to core analyses ...

I don't know, does someone who's lived through this part of the life-cycle of a big experiment want to speak to this question?

[DB] In addition to that, there is also serious consideration being given to conserving the data for future use well beyond 2011, because it is recognised ...

*This goes to the second question posted here actually, **what to do when the data becomes a heritage matter.***

[DB] I'm not aware that there's a solution to that yet, but certainly that question is being considered.

My question is: when it's several years out from the running of the experiment and you've got one or two dedicated groups at a slow burn working on this data, how [do] you maintain quality? How do you stop people with agendas and nothing better to do, and a lot of BaBar data ...

[KS] You don't avoid that.

That just happens?

[KS] It's natural. Things die, and there's a rate of dying, and then there is that not that close supervision, and the quality suffers.

[AP] This might open a can of worms, but are there any plans to make the data publicly available on the internet, just like astronomy?

But that then multiplies the issue that I was just raising, doesn't it. I mean, if some high-school student can run a job can you imagine the number of new states we're going to be seeing ... [laughter]

[AP] But what's wrong with that? It's not going to be signed by BaBar ...

[DB] ... but the answer is yes.

[Gene leaves]

4.7. Fermilab: charm, and antiprotons

We've perforce been skipping around here, so I'll just continue on this slide for a moment, [can we hear] what the plans are at Fermilab for charm studies — at D^0 and CDF?

[JR] [inaudible] certainly mixing is going to be out soon, with comparable sensitivity to ...

OK, I already knew the answer to that question, but what about non-mixing topics?

[JR] I have no good overview, except for charm production ... [from elsewhere: "and the rare decays"].

... and the rare decays. I guess the question is whether there will be an update or successor to that analysis, or whether that's the last thing we'll see. [to AP: "You have a lot of comments!"]

[AP] No, I just happen to be at an institution that participates in CDF, and which actually does rare-decay charm analysis. So, you know, the first paper from CDF-II was on $D^0 \rightarrow \mu^+ \mu^-$ [6] ... and right now they're doing an update of that.

There was a second question posted here — it's outside my awareness — about what the support in the community is for the antiproton source proposal at Fermilab. Does anyone want to comment on that?

[DB] I can say my personal opinion — I cannot speak for the community, of course — because I have been following a little the proposal for the antiproton source, to make a dedicated experiment at the pbar accumulator — my personal impression is that the next step, the next generation of hadronic $p\bar{p}$ physics will have to be a global approach like PANDA, with a detector like PANDA. A detector like we had in E835 has more-or-less said whatever it could say, and I don't know whether Kam would agree, but we were discussing the other day: already at the end we were sort of at the limit of what could be done in this kind of experiment.

So I don't know what was the point of the question: my impression is that there is very little one can do in the limited space which is available there, unless one can dig a bigger hole, and really do a general-purpose detector.

[KS] As a former worker in the field: I can tell you that there is just not enough constituency and it doesn't rank high enough in the priority, so at least my feeling is that it is very sad that the only place which has currently the ability to produce antiprotons is not planning [anything for the future]. But that's the way life goes: GSI is coming, [the current facility has] outlived its capabilities, and I don't see much future for that at Fermilab. That is my personal opinion, but I make that opinion after having heard the arguments and the things that have been proposed.

OK, I'll take that as an answer. I'm going to skip to the second part ...

[DA] About SuperB?

Yep. [i.e. What is the interest and the support of our community for the Super-B project?]

[DA] OK I'll start with a question to Alan — or maybe to you, Bruce — as to how many names are on the Belle letter of intent for SuperBelle?

At least half [of the collaboration].

[DA] Because SuperB has three-hundred-and-twenty signatories, and there's some overlap between the two, but it's not overwhelming [*i.e.* the overlap is not the dominant part], but we're talking about hundreds, five or six hundred people in our field, theorists and experimentalists, who are excited to do work, who have produced these documents ... so I think there is considerable support for a super-B-factory.

[YW] I'd like to say that although we are not part of the super-B [project, we strongly support it] ... we think it's very good for the field, and very good for the community. Without super-B ... at least for us, we at BES, five years from now, the charm field would only be at BES, and um, we don't want that.

4.8. What to measure at LHCb

*There was a pessimist's question on exactly this point, which you saw, which I'm not going to put up, because we're running out of time. [Instead I want to consider] a couple of LHC-related questions — essentially LHCb — the first one was answered by Patrick's talk; so the second one is **whether we should be making a shopping-list of the measurements that we want LHCb to make** — the people in this room, this community — how much [would that help?]*

[PS] I think that that's an excellent idea. People complain about the manpower issue at the B-factories: at LHCb it's even worse right now. So having a list of things to look at — and a prioritised list — would be helpful for our planning. As scientists come off of the building phase of the experiment they're going to be looking for analyses. And if we have an enumerated list of charm physics [topics] we might be able to woo them into this field.

*OK, so a specific question, as to **whether there's a charmed baryon programme at LHCb.***

[PS] Right: Not now. Not yet.

Will there be?

[PS] I don't know. It would take more than just me to be doing it.

OK, but realistically: would the people in this room saying "We want such-and-such to be measured" make a difference to that, or is it just the people in the collaboration?

[PS] I think it's more getting individual scientists and groups in the collaboration interested in that work.

[JR] I just wanted to second that. I think that at the moment, suggesting exciting analyses might actually still have an influence, for the very reason that many people who have worked very hard on building the detector are moving into physics at this moment, and we can have influence over [the choices that they make].

Maybe we should look into the list idea.

4.9. On being a successful sideline:

4.9.1. ISR work at a "super-B-factory"?

*Going to another sideline point about the future, this is one for SuperB: the ISR programme at the B-factories has become quite an important part of the work lately, and through accumulated accidents that kind of dropped out of this meeting. [But there's good physics being done using ISR at present.] So, **what ISR programme is foreseen for the super-B or super-flavor factories?***

[DA] There are a couple of pages in the SuperB CDR precisely on this physics, and I can tell you that at every SuperB meeting that is about the physics (not the accelerator) there are talks on this subject. So it's on the radar. The data will get read out: it will be there on tape. But it will require people who are interested in this physics from the current experiments showing up and doing that physics ... I don't think the ability to do this physics, as interesting as it is, will drive whether or not a super-B machine gets built ...

No, I think that's right ... Because it took us a long time to warm up to doing ISR physics. It was in the BaBar book — we didn't have a book, but it was in the BaBar book — but it even took them a while to warm up to it, so I guess that's what's behind the question.

4.9.2. Charm studies at LHCb?

*Some general questions about the future now: This is actually a question that I care about as well, as to **how easy it's going to be doing charm studies parasitically at facilities that aren't dedicated to it.** This is really an LHCb question: we managed it fine at Belle and BaBar, but Belle and BaBar run with open triggers. Now ...*

[PS] Yes. We are in some sense competing for bandwidth. And we may have to make a case for every exclusive charm channel that we put in, and certainly it needs to be optimised so that it doesn't eat into the total bandwidth too much.

But I think that anything that someone is genuinely interested in doing can be put into the trigger at this stage. It's more of a manpower issue, even at this stage. And if I may make a comment about the poor neglected LHCb upgrade — I'm sorry I didn't mention anything about that — the trigger is going to have to be completely overhauled for an LHCb upgrade in order to accommodate the increased luminosity, and although it's by no means finalised, the prevailing idea for the trigger there is to go with a full software trigger. And once you've done

that, it opens up the whole field: if you can efficiently create a charm trigger, you don't have these p_T cut limitations that are applied at the hardware level.

So, yes. It's a matter of manpower and interest and influence, I think.

Anyone else on that one? No?

4.10. Charm & the heavy ion community

*OK this is a question that I wasn't expecting, but it's a potentially useful one, and it's **whether a closer collaboration with the heavy ion community would be beneficial, either for charm or quarkonium physics.** Anyone with experience of heavy ion work? Or friends in heavy ion work?*

[AP] There is already close collaboration. First of all, they are already going to share an accelerator: the LHC. And you know, those guys need fragmentation functions, things like that: they get it from us.

[KS] Already at Brookhaven, people are beginning to do spectroscopy, which was not their [original] goal. So they are of necessity coming into the field, and they often ask questions: it is not quite a collaboration yet, but yes, each is beginning to get interested in the other. I think it's a developing thing.

Right, I assume that's the thought behind the question: that the communities are somewhat disjoint.

[KS] At RHIC it's beginning. At LHC it remains to be seen, because it is still in the future.

[AP] There are already a couple of groups: I mean, at our university, Michigan State, the groups are both ... there are groups on both CDF and D0, CLEO, and STAR and PHENIX and so people are ... maybe it needs to be formalised.

[KP] Also at GSI with the FAIR project we have two distinct communities ... [but] it's exactly ten metres from my office to [that of] my colleague who leads the heavy ion community there, and our intention is to place an application for a virtual institute ... to manifest [our cooperation] a little more: to really concentrate on charm and the various issues with the nucleus ... things are moving together to some extent.

4.11. On charmed baryon measurements

OK. We are ... running out of time. I thought I'd just throw one slightly odd question in. There

*was a comment along the way **about the measurements that are being made in baryons, and that there appear to be some kind of imbalance in it:** that you can have some of these quite sophisticated studies that measure the spin and parity of some of the baryons, or search for new states or whatever, but there are quite fundamental things that we know poorly, particularly about the Ω_c . And some of those measurements aren't being made. Is there a way around that? Or do we need a super-motivated group to just do the work at the B-factories? I mean, CLEO-c was going to run at Λ_c threshold at one point, and famously ...*

[DA] SuperB — the INFN proposal — has plans to spend a month at the Λ_c threshold. But it's at Λ_c threshold.

Right: they talk about it, but CLEO-c did as well. Is it going to be the first thing that gets dropped, in the same way that ...

[DA] I think the answer is, "of course".

Right. OK. That's not necessarily a criticism ...

[DA] The entire 4 GeV programme would probably be the first thing to get dropped if the 10 GeV luminosity wasn't up to snuff.

[KP] To give you one example, our group started the idea to measure ... the $D_{s1}(2536)$ which we have seen this morning [7]; and it really has reached excellent resolution ... But to give you an idea of the timescales, this PhD student has spent three years on it, to get really all the systematics nailed down to that point where you can reach these 100 keV systematic errors ... and that's exactly what kills a lot of these mass measurements. If there is not really an important issue, you just drop it because these systematic studies ... there are so many other things you could do, which are more geared to the mainstream.

Less mainstream and more difficult.

[KP] Yes.

4.12. Thanks

OK look, I ... don't want to keep you here any longer, and I think we've covered a fair amount of territory on a range of things. So thanks to the people who served on the panel, and to all of the speakers I guess. And um, I would certainly like to thank the committee, for what's been a good workshop.

References

- [1] E. Golowich, J. Hewett, S. Pakvasa, and A. A. Petrov, [arXiv:0705.3650 \[hep-ph\]](#). Submitted to Phys. Rev. D.
- [2] B. Aubert *et al.* (BaBar Collaboration), Phys. Rev. Lett. **98**, 211802 (2007).
- [3] M. Starič, B. Golob *et al.* (Belle Collaboration), Phys. Rev. Lett. **98**, 211803 (2007).
- [4] C. Z. Yuan, C. P. Shen, P. Wang, S. McOnie *et al.* (Belle Collaboration), Phys. Rev. Lett. **99**, 182004 (2007).
- [5] E. Follana, C. T. H. Davies, G. P. Lepage, and J. Shigemitsu (HPQCD and UKQCD), [arXiv:0706.1726 \[hep-lat\]](#).
- [6] D. Acosta *et al.* (CDF Collaboration), Phys. Rev. D **68**, 091101 (2003).
- [7] B. Aubert *et al.* (BaBar Collaboration), [arXiv:hep-ex/0607084](#).