A SEND-OFF AFTER DIF06: WHAT DO WE NEED TO KNOW TO UNDERSTAND MORE?

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Abstract

After a brief look back at the roles played by hadronic machines and $e^+e^-$ colliders I emphasize that continuing dedicated studies of heavy flavour transitions should be central to our efforts of decoding nature’s ‘Grand Design’. For studies ‘instrumentalizing’ the high sensitivity of CP violation will presumably be essential to identify salient features of the New Physics anticipated for the TeV scale and hopefully discovered directly at the LHC. An $e^+e^-$ Super-Flavour Factory would provide the optimal platform for such a program.

This is not a summary – to my considerable relief I was asked not to give one. These are not the conclusions either. You can never give the conclusions when the ‘boss’, in this case M. Calvetti, is speaking right after you. ¹ Instead I will offer merely my personal reflections.

1 A Short Look Back

Let us look at two exhibits from the past.

Exhibit A:

The weak bosons were first found at the CERN SPPS – a hadronic collider. It was LEP I & II – an $e^+e^-$ collider – that established the electroweak gauge sector of the Standard Model (SM) with a quantitative accuracy well beyond original expectations.

¹A Justice on the US Supreme Court once said: "We are not the Supreme Court, because we are infallible. We are infallible, since we are the Supreme (i.e. final) Court." Working at a Catholic University I am not unfamiliar with this issue.
Exhibit B:
Indirect and direct CP violation were first uncovered at BNL, CERN and FNAL – all hadronic machines. The $B$ factories at KEK and SLAC – $e^+e^-$ machines – established the Yukawa sector of the SM: CKM dynamics are able to describe (at least almost) all CP violation as observed in particle decays to a quantitative accuracy well beyond original expectations. The CKM paradigm has thus become a tested theory; CP violation has been ‘demystified’ in the sense that if the dynamics are sufficiently rich to be able to support CP violation the latter can be large; this demystification will be completed once CP violation is found anywhere in the lepton sector.

Let us remember also what happened with strangeness changing transitions: (i) The $\tau - \theta$ puzzle lead to the realization that $P$ symmetry does not hold in the weak interactions. (ii) The observation that the production rate exceeds the decay rate by several orders of magnitude (giving rise to the name ‘strangeness’) lead to the notion of associated production and in due course to the concept of quark families. (iii) The huge suppression of strangeness changing neutral currents – as inferred from the tiny size of $\Delta M_K$ and of BR($K_L \rightarrow \mu^+\mu^-$) – encouraged some daring minds to speculate about charm quarks. (iv) The observation of $K_L \rightarrow \pi^+\pi^-$ revealed CP violation and suggested the existence of a third quark family.

All these features, which are now pillars of the SM, were New Physics at that time!

2 What to Do?

The SM is a very peculiar creation. On one hand more and more victories have been attached to its banner. The years around the turn of the millenium have witnessed novel successes in its description of heavy flavour dynamics: most spectacularly the ‘Paradigm of truly large CP violation in $B$ decays’ has been verified. We can even see the next triumph of CKM theory emerging: Consider the constraints on the CKM triangle as of the end of 2005 shown in Fig.1. If you remove the constraints from CP violation, in particular from $|\epsilon_K|$ and $\sin 2\phi_1$ (as well as from $\phi_{2,3}$), thus retaining only those from $|V(ub)/V(cb)|$ and $\Delta M(B)$, then a completely flat triangle – i.e. one with zero area – while not favoured, is not ruled out, as indicated by the broken circle.

If – and that is admittedly a nontrivial ‘if’ – the evidence presented this spring by D0 [1] for an upper bound on the rate of $B_s$ oscillations holds up – $\Delta M(B_s) \leq 21$ p$^{-1}$ – one has a qualitatively new scenario as can be seen from Fig.2 [2]:

- CP insensitive observables – namely $|V(ub)/V(cb)|$ and $\Delta M(B_{d,s})$ – imply a CKM triangle of non-zero area, i.e. the existence of CP violation.

- This triangle is fully consistent with the observed CP violation as expressed through $\epsilon_K$ and $\sin 2\phi_1$.

Yet none of the successes of the SM – including those just sketched – invalidate the arguments for it being incomplete. This represents a virtual consensus in the community. New Physics is expected at the TeV scale.

2
A clear majority opinion holds that we need more answers from nature to figure out the variant(s) of New Physics realized in nature and to establish that we are ‘on the right track’. After all ‘experiment is the Supreme Court of Physics’.

Given the past history of searches for Physics beyond the SM this might remind you of Samuel Beckett’s dictum:

> Ever tried? Ever failed?
> No matter.
> Try again. Fail again. Fail better.

But we can cheer up – we know there is New Physics: With CKM dynamics being utterly irrelevant for baryogenesis, there has to be a ‘New CP Paradigm’. Likewise the SM cannot account for neutrino oscillations, dark matter and dark energy. In addition there are the SM’s well known explanatory deficits. Thus we will not fail forever.

Yet history rarely if ever repeats itself in an identical manner. Recognizing that the SM has succeeded in putting a vast array of phenomena occurring at very diverse scales under its roof, we cannot count on numerically massive manifestations of New Physics in heavy flavour transitions – unlike what happened in the physics of strangeness before the era of the SM. It appears that nature has read the SM’s book on flavour changing neutral currents – at least for down-type quarks.

Accordingly we need some luck to find this New Physics. Being lucky has of course to be part of the job description for a high energy physicist (in particular of the experimental variety). Accuracy in acquiring and interpreting data will do wonders for enhancing our luck.
3  The Future

I do not intend to beat around the bushes: in my view studies of heavy flavour dynamics

• are of fundamental importance,

• their lessons cannot be obtained any other way and

• they cannot become obsolete.

I.e., no matter what studies of high $p_\perp$ physics at FNAL and the LHC will or will not show – and I am confident they will show a lot – detailed and comprehensive analyses of flavour physics will remain crucial in our efforts to reveal ‘Nature’s Grand Design’.

I see three possible scenarios of the next five to eight years:

Scenario A – the ‘optimal’ one:
New Physics has been observed at high $p_\perp$. It is then mandatory to study their impact on flavour dynamics, which is greatly facilitated by the mass scales of the New Physics being known. Even a negative result – i.e. no discernible impact on heavy flavour decays – would be a highly important result in this scenario, however frustrating it would be for our experimental colleagues.

Scenario B – the ‘intriguing’ one:
Deviations from the SM have been established in heavy flavour decays. Recently considerable excitement has been created by the ‘$b \to s\bar{s}s$ anomaly’, i.e. by experimental evidence that modes driven by this effective ‘Penguin-like’ operator exhibit markedly lower CP asymmetries than predicted by the SM. While the spectacular discrepancies have faded away, some have remained [3], and I find those actually more believable than the original ones. Therefore we better keep a close watch on them,

Scenario C – the ‘frustrating’ one:
No further deviation from the SM has been established at high or low $p_{\perp}$.

While I am optimistic it will turn out to be Scenario A, I would like to emphasize that none of these scenarios weakens the importance of continuing heavy flavour studies for our quest of finding out about nature’s basic forces.

Yet we do not live and work in isolation. Following what was said by Nakada at this meeting I would like to formulate a ‘Generalized Nakada Concern’: While more than ever before we have many promising avenues for exploring fundamental physics, while we have more technical tools and capabilities than ever, we live in a world with immense political, social and environmental problems; furthermore we have to deal with governments with less interest in basic research to a degree that goes well beyond a justified preoccupation with these problems. How do we choose our priorities?

This is an excellent question, and I do not have a general answer to it. I can offer only one criterion, namely to aim for comprehensive research goals. In the case under discussion here I want to emphasize that a Super-B factory is also a Super-Tau as well as Super-Charm factory of truly unique capabilities. It allows precise studies of a third family down-type quark, a third family down-type lepton and a second family up-type quark. In this context the studies of CP violation, oscillations and rare decays are instrumentalized to probe TeV scale New Physics.

To achieve

- a luminosity of $10^{36}$ cm$^{-2}$ s$^{-1}$
- with tiny beams and a hermetic detector allowing to study transitions with large amount of ‘invisible’ energy – like $B \rightarrow \tau^+\tau^-$, $\tau\nu$, $\tau\nu X$, $\tau \rightarrow l \nu \bar{\nu}$ etc.
- maybe even with one beam being polarized,
- ‘soon’ and
- ‘here’, i.e. near Rome –

to me sounds better than paradise. There is the promise that such a Super-Flavour Factory can be run not only at the $\Upsilon(4S)$ and $\Upsilon(5S)$ resonances, but even at much lower energies close to charm and $\tau$ thresholds at an affordable cost in luminosity. While the primary goals in $\tau$ and charm physics – searching for and probing CP violation as well as rare decays – can profitably be pursued at the $\Upsilon(4S)$, future studies might show that lower energies are optimal for dealing with certain backgrounds. The statistically as well as systematically ambitious goals that need to be pursued in $B$ physics have been listed repeatedly at this workshop. Let me remind you also of equally challenging goals in $\tau$ and charm studies [4], namely to go after CP asymmetries as small as $O(10^{-3})$ or even smaller.

It has often been noted that when ‘all is said and done’ usually a lot more is said than done. We are seeing how the dream of a Super-Flavour Factory is turning into a vision. A whole lot needs to be done before it can be transformed into a project and finally elevated into reality.
I had mentioned in my opening remarks at this workshop [5] that the area around Rome has a long tradition of linear structures and shown you a historical example. In Biagini’s talk [6] we have seen a brand new and very different ‘Solution 2’ for an ILC-inspired Super-Flavour Factory. The fascination of Rome is such that you can find an example for almost anything in its rich heritage. ‘Solution 2’ reminds me of the almost 2000 years old structure shown in Fig. 3 full of tunnels with round as well as straight beam lines. Another intriguing aspect of it is that such a design would be portable to other places – like KEK, which has its own heritage, see Fig.4.

4 A Final Thought

In my opening remarks [5] I had shown a picture as an allegory on the future of high energy physics. I am showing Fig.5 again for a related reason. You notice the sun between the two rocks. Just looking at the picture without taking recourse to additional information you might have about these structures (in particular if you are Italian) you cannot tell if the sun is rising or setting. To me one of the most impressive aspects of experimental high energy physics is that when you give experimentalists resources and time it is most amazing what they can achieve. As an example just have a look at the ‘Blue Book’ that was written during the planning and constructing of LEP with a lot of quality time spent...
on it by theorists as well and compare it with what was actually done – which was so much more. This speaks most highly of the intellectual vigour of the field.

Likewise I firmly believe that if a Super-B factory is built, a proper time span is provided to utilize it and young people can fully participate in shaping its program, we will learn even more than what we envision now. In that sense I am confident the picture shows a rising rather than a setting sun.

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References
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[4] I.I. Bigi, these Proceedings.
[5] I.I. Bigi, these Proceedings; hep-ph/0603087.
[6] M. Biagini, these Proceedings.