THE BATTLE OF ALBUERA, THE FC LIVERPOOL AND THE STANDARD MODEL

I.I. Bigi

Department of Physics, University of Notre Dame du Lac
Notre Dame, IN 46556, USA
email: ibigi@nd.edu

Abstract

The Standard Model despite its well-known shortcomings is unlikely to yield without offering stubborn resistance. There are compelling arguments that New Physics lurks around the TeV scale. Continuing comprehensive studies of beauty, $\tau$ and charm transitions can be instrumentalized to reveal it and shed light on it. They are thus complementary to findings obtained at the LHC and presumably essential in clarifying the true nature of that New Physics. A Super-B facility seems to provide the cleanest environment for pursuing such an ambitious program. I list desirable features of such a setup as well as challenges for the accelerator and detector designs and for the theoretical analysis.

1 The Verdict on the SM

After losing the battle of Albuera in 1811 Marechal Soult declared: "I had beaten the British – it was just they did not know when they were beaten." Soult was actually one of Napoleon’s best generals, and experts agree he was right on both counts. For those with a much shorter memory span one can point to a similar experience just last year: at halftime in the finals of the European Champions League AC Milano was leading FC Liverpool 3:0 with truly gorgeous play, yet the pesky Brits while still being outplayed in the second half – except for those magic eight minutes – refused to concede.

This is the story as well with the Standard Model (SM): We all know how to design an extension to the SM that is greatly superior to it – now we have to overcome the SM’s refusal to concede defeat.

Even in the last few years since the turn of the millenium the SM has scored unprecedented successes in flavour physics: CKM dynamics describe a vast array of very
diverse phenomena culminating in CP violation as observed in particle decays as CKM’s signature achievement. Yet we are in search of a ‘New CP Paradigm’: for we know that CKM dynamics is grossly inadequate for baryogenesis, which posits the observed baryon number of the Universe as a dynamically generated quantity rather than an initial input value. There are further shortcomings of the SM as revealed mostly by heavenly data: (a) ν oscillations; (b) ‘dark matter’; (c) ‘dark energy’.

In addition there are serious explanatory deficits of a general nature (I am even not including the so far unresolved ‘Strong CP Problem’):

(i) Electro-weak Symmetry Breaking and the Gauge Hierarchy: What are the dynamics driving the electroweak symmetry breaking of $SU(2)_L \times U(1) \rightarrow U(1)_{QED}$? How can we tame the instability of Higgs dynamics with its quadratic mass divergence? I find the arguments compelling that point to New Physics at the $\sim 1$ TeV scale – like low-energy SUSY; therefore I call it the ‘confidently predicted’ New Physics or cpNP.

(ii) Quantization of Electric Charge: While electric charge quantization $Q_e = 3Q_d = -\frac{3}{2}Q_u$ is an essential ingredient of the SM – it allows to vitiate the Adler-Bell-Jackiw or triangle anomaly – it does not offer any understanding. It would naturally be explained through Grand Unification at very high energy scales implemented through, e.g., $SO(10)$ gauge dynamics, where leptons and quarks are placed in the same multiplet. I call this the ‘guaranteed New Physics’ or gNP.

(iii) Family Replication and CKM Structure: We infer from the observed width of $Z^0$ decays that there are three (light) neutrino species. The hierarchical pattern of CKM parameters as revealed by the data is so peculiar as to suggest that some other dynamical layer has to underlie it. I refer to it as ‘strongly suspected New Physics’ or ssNP. We are quite in the dark about its relevant scales. Saying we pin our hopes for explaining the family replication on Super-String or M theory is a scholarly way of saying we have hardly a clue what that ssNP is.

2 On Finding What Drives the Electroweak Symmetry Breaking

The next big challenge to which we have to rise is to find and identify the cpNP. It has provided the justification for the LHC and drives the motivation for the ILC – an excellent one in my view.

Let me make two judgment calls. While I have reflected on them, I understand that reasonable people can honourably disagree.

• Any future facility has to be justified by its ability to find New Physics and identify its salient features – learning new lessons on QCD will no longer suffice. This applies also to a new τ-charm factory beyond BESIII.

• Heavy flavour studies might provide insights into questions (ii) & (iii) listed above – but we cannot count on it. Therefore we cannot justify a new facility with such a hope.
Instead I advocate *instrumentalizing* studies of flavour dynamics as expressed below through five statements:

1. *Comprehensive and detailed* heavy flavour studies will be *crucial in identifying* the cpNP.

2. I remain skeptical that studies in hadroproduction can be fully competitive with those at $e^+e^-$ machines in $\tau$, charm and even beauty transitions as far as precision and comprehensiveness are concerned.

   In this context I want to emphasize that $B_d$ and $B_s$ decays represent truly different, yet complementary chapters in ‘Nature’s Book on Fundamental Dynamics’.

3. A Super-B factory allows comprehensive precision studies of $B$, $\tau$ and charm decays.

   A very detailed plan for such a project has been developed by the KEKB team [1].

4. To be competitive in $\tau$ & charm studies a *future* $\tau$-charm factory has to be of the Super-$\tau$/charm variety, i.e. with a luminosity of at least $10^{34} cm^{-2}s^{-1}$.

5. I am convinced that a compelling justification for a Super-B facility can be given – yet one cannot merely follow the lines of argument given originally in favour of a $B$ factory. There

   - one had so-called ‘killer-applications’, namely CP violation in $B_d \rightarrow \psi K_S$, $\pi^+\pi^-$ and $K\pi$
   - with predictions of reasonable accuracy
   - requiring a luminosity in the $(10^{33} - 10^{34}) cm^{-2}s^{-1}$ range.

While history repeats itself, it never does so in an identical fashion. For a Super-B project we face a fundamentally different ‘landscape’:

- We cannot count on killer applications.
- We cannot count on a numerically massive intervention by New Physics.
- ‘Merely’ finding New Physics is not enough – we must identify its salient features.
- There is no clear benchmark for the needed luminosity.
- Thus our guidance has to come from what – rather unkindly – has been referred to as the ‘Wall Street mantra of greed’: ”Lots is good, more is better, aim for the sky!”

There are basically two kinds of research:

- One has a more or less well-developed theoretical framework, where one has at least clarified the categories of relevant questions. Answering those (with the help of experiment) can be called ‘hypothesis-driven’ research. In that case one has always something to show for one’s efforts. Not surprisingly such projects are most popular with funding agencies.
• Alternatively one has a situation that is unsatisfactory in a conceptual or even phenomenological way, yet with no compelling theory candidate to fill the gap. Without such guidance one performs ‘hypothesis-generating’ research in the hope that more analyses will point to a new paradigm. Such work thus has the potential to lead to a revolution – alas funding agencies display markedly less enthusiasm for it.

The program at the $B$ factories has primarily been of the hypothesis-driven variety – and a most successful one at that. Yet at a Super-B factory (with $\tau$ and charm) we have to conduct hypothesis-generating research with one of the goals being the search for the ‘New CP Paradigm’.

3 Challenges for a Super-B Facility

Precision in acquiring and interpreting data is essential if we want to draw the desired lessons from heavy flavour studies. The ‘conditio sine qua non’ is to have huge statistics of comprehensive high quality data. A large body of well measured transitions is more important than a few rates determined with infinite precision.

There is the (in)famous challenge from Sanda: "We need a luminosity of $10^{43}\, cm^{-2}\, s^{-1}$!" While it is certainly ‘tongue-in-cheek’, it is not just frivolous; it has more than a kernel of truth, in particular when combined by Sanda’s empirical conjecture that every second ‘3 sigma’ effect goes away.

Yet, as already stated, statistics is not all. For the goal of a Super-B facility has to go beyond ‘doing more of the same’. We need not only more data, but also data of a different and higher quality. New observables have to be opened up to detailed study. This requires a hermetic detector operating in a low background environment with superb $\mu$ vertex resolution. This would allow to study transitions like $B \rightarrow \tau\nu, \tau^+\tau^-l, \tau\nu X, \nu\bar{\nu} X$ and $B \rightarrow \gamma X_s$ vs. $\gamma X_d$.

Also energy flexibility would be very desirable, i.e. to study $\Upsilon(5S) \rightarrow B_s\bar{B}_s$ and even $\psi(3770) \rightarrow D\bar{D}$ etc. in addition to $\Upsilon(4S) \rightarrow B\bar{B}$.

Finally it would be quite desirable to have a polarized electron beam available. It would lead to the production of polarized $\tau$ leptons and probably also of polarized charm baryons. This polarization would be a powerful tool to enhance the sensitivity to CP violation in the decays of those states; at the same time it would help to control systematics.

The area around Rome has an ancient history of superbly engineered and long lasting linear structures, see Fig.1. The stated aim for an ‘ILC inspired’ Super-B facility [2] is to achieve a luminosity of $10^{36}\, cm^{-2}\, s^{-1}$ (or more) with tiny beams and a hermetic detector, maybe even with a polarized beam, ‘soon’ and ‘here’, i.e. near Rome.

Life teaches us all too often that if something is too good to be true – it usually is. Is it in this case? Keep in mind we cannot afford failure.
4 Questions and Challenges

Let me pose to you some questions that I would like to see addressed – or better still answered – at the workshop or in the near future.

- What integrated luminosity can be achieved at a Super-B factory by 2016, i.e after mature data taking has taken place at the LHC, and by 2020, when a realistic optimist can hope for the ILC to begin running?

- How and when can the feasibility of the linear Super-B concept be established?

- What will be the quality of the $e^\pm$ beams, and how hermetic can the detector be?

- What kind of integrated luminosity can be achieved in, say, a two year run at the $\Upsilon(5S)$ run?

- How many precision measurements can be made by (an upgraded) LHCb?

- Can (an upgraded) LHCb do competitive CP studies in charm transitions?

- How feasible is a Super-$\tau$-Charm factory with $(10^{34} - 10^{35}) cm^{-2} s^{-1}$, and what is its price tag? Would it be competitive with respect to CP searches in $\tau$ decays?

- While the items listed so far mainly concern experimental and technical issues, there is a lot to be done by interested theorists as well: to identify the features of the conjectured New Physics one has to state the required

  - benchmark observables \(^1\);
  - benchmark accuracy and

\(^1\)I realize that considerable work has already been done in this direction.
validation checks for establishing control over theoretical uncertainties.

It is also crucial to interpret findings from the LHC – including ‘no-shows’.

Allow me one final comment illustrated by Fig.2. The future landscape of high energy physics is dominated by two huge landmarks, namely the LHC and hopefully the ILC; those landmarks are represented by the two rocks on the picture. I believe there is still some pathway left between them for dedicated studies at a Super-Flavour facility as indicated by the gap between the two rocks. Heavy flavour studies thus resemble a passage between Scylla and Charybdis. It requires a crew and a skipper that combine experience with some daring to navigate through this strait – where can we find them?

References

[1] See Letter of Intent at http://belle.kek.jp/superb/.

[2] See INFN Roadmap Report at http://arxiv.org/abs/physics/0512235.