July 31, 2004

Dr. Michael Barnett, PDG

Re: Minireviews in PDG

Dear Michael,

we want to communicate our grave concerns to you about the process of producing minireviews for PDG. We have read the two PDG04 articles on $V(cb)$ by Artuso & Barberio (AB) and on the CKM matrix by Gilman, Kleinknecht & Renk (GKR) with particular interest, since we are all working in these areas. Furthermore some of us had been asked by PDG to comment on the drafts of the minireviews for the '04 PDG issue (and previous ones as well) and we had provided lengthy and detailed critiques on time. Minireviews – though not PDG's primary mission – can be a very useful asset, when one wants to gain a first orientation in a new field. Yet that requires that these articles do reflect the state of the art, are factually correct and provide a fair overview of the relevant literature. These requirements are particularly important in view of the fact that PDG is widely seen as a neutral body of our whole community defining complete or almost complete consensus on subjects without ignoring disagreements. We believe that the $|V(cb)|$ article is completely inadequate in these respects, and also the CKM article falls short of it.

We are fully aware that writing and editing truly satisfactory articles requires a considerable amount of work by many people on different levels. Yet we think that an unsatisfactory article is counterproductive, since it impedes the scientific discourse, and thus is worse than no article. Therefore we are making another effort to address the serious problems we have encountered in our dealings with PDG. Yet based on our past frustrating experience with the PDG04 and PDG02 editions we decided to go to the very 'top', namely you, Michael. We will send this letter also to various collaborations doing analyses in these areas – like BABAR, BELLE, CLEO, DELPHI etc. – for their information before the summer conferences and to some of the rapporteurs there.

Our unhappiness and irritation concerns the results as well as the process:

- The original draft, in particular for the $V(cb)$ article, contained many questionable, misleading or even incorrect claims concerning theoretical issues.
- Such claims had been addressed and refuted in the existing literature.
- Highly relevant theoretical as well as experimental papers that had been published

at the time of writing of these articles were not listed among the references.

- When several of us received these drafts, we sent reports with long lists of detailed and specific comments. Yet we received almost no feedback, and when we read the final versions of these minireviews, we saw that our criticisms had been either completely ignored (even such comments that could not have been seen as controversial), or led to changes that at best could be characterized as cosmetic – in some cases the changes exacerbated the problem.
- Some of us had gone through the same frustrating and irritating routine for the PDG02 edition, sometimes concerning the same problems.

Rather than resending you a full listing of our critique now we will provide you merely the most telling examples. Let us also state clearly that our aim is not to get a few more citations for some of our papers; neither do we claim that these shortcomings will obscure scientific truths forever or even for long times. However they cause some unnecessary work in correcting them – and they could turn into a drag on PDG's reputation for objectivity and solidity.

$|V(cb)|$ **Minireview**

$B \to l\nu D^*$ – *the 'exclusive' method*: The referencing is very sloppy here. For quark model estimates of the zero-recoil form factor $F_{D^*}(1)$ the authors list two papers by Falk & Neubert from 1993, of which only the first one is actually relevant here; in it an estimate $F_{D^*}(1) \simeq 0.98$ is given; i.e., nothing like 0.91 ± 0.04 used by AB. The first paper that gave an estimate $F_{D^*}(1) \sim 0.9$ was the one by Shifman, Uraltsev & Vainshtein listed here; other estimates then followed suit.

Some of us have pointed out explicitly sources for additional theoretical uncertainties that cannot be addressed adequately by existing computations in lattice QCD; the reader will not find any reference to those concerns.

$B \to l\nu X_c$ – *the 'inclusive' method*: Let us list just some of the major omissions, inaccuracies and mistakes in content as well as correct and proper referencing.

The 'HQET approach on one hand and the 'Wilsonian approach introduced by Shifman, Uraltsev, Vainshtein and Bigi on the other are treated as completely equivalent. Certainly for the time being, this is demonstrably wrong. Explaining it in detail might go beyond the scope of a minireview, but the authors should refer explicitly to the literature, where it is explained, like Refs. [9] and [13]. The latter in particular – the summary from the CKM workshop, hep-ph/0304132 – provides a very useful and convenient listing of relevant formulae, where different definitions and treatments get a 'fair hearing'.

The discussion of the complex subject of 'quark-hadron duality' is particularly hard to swallow. The authors fail to even mention that the claim of large duality violations by the late Isgur they refer to has been explicitly addressed in considerable detail and refuted by Le Yaouanc *et al.* and by Bigi & Uraltsev in papers published in 2000 & 2001; this issue has been resolved. The last paper – the 2001 'Vademecum' addressing duality and its limitations in a comprehensive way – is not even listed among the references by AB. As explained in the 'Vademecum', it is misleading to say that "quark-hadron duality is an important *ab initio* assumption in these calculations". Buchalla refers to this paper in his lecture notes concerning duality in semileptonic B decays, which is of relevance for extracting $|V(cb)|$, not duality in nonleptonic decays.

Of course it is essential to probe for limitations in quark-hadron duality in the data. This can be achieved best by extracting a small number of heavy quark parameters like quark masses etc. from a large number of observable moments like lepton energy moments and hadronic mass moments. It is quite inexcusable that AB basically ignore that the 2002 moment analysis of their Ref. [58] using DELPHI data was already a pioneering step in that direction, when they fitted six measured moments with four heavy quark parameters, found consistency in their fits and – very importantly – found that their fit values were in full agreement with extractions from other processes. The most impressive example is the value of m_b that agreed with what was found from B production near threshold in e^+e^- annihilation within the stated small uncertainties. Particularly pioneering features of the DELPHI data were the measurements of more and higher moments and the lack of a hard cut on the lepton energy. To deal with that achievement in a single sentence "Moments of the M_X distribution without an explicit lepton momentum cut have been extracted from preliminary DELPHI data [58] and give consistent results." is grossly inappropriate and unfair.

In their 'Conclusions' the authors list

$$|V_{cb}|_{excl} = (42.0 \pm 1.1_{exp} \pm 1.9_{theo}) \times 10^{-3} \; vs. \; |V_{cb}|_{incl} = (41.0 \pm 0.5_{exp} \pm 0.5_{\lambda_1,\bar{\Lambda}} \pm 0.8_{theo}) \times 10^{-3} \tag{1}$$

We have sketched above our criticisms about some aspects of these numbers. But let us for the moment accept them at face value. The authors then go on to write "In addition, non-quantified uncertainties are associated with a possible quark-hadron duality violations when using the inclusive method. A first conservative assessment of these uncertainties may be obtained from the difference between the two values of $|V_{cb}|$ extracted from $B \to D^* l \bar{\nu}$ and from inclusive measurements. These data imply about 6% uncertainty for non-quantified assumptions in the inclusive determination. This result is largely affected by the quantified theoretical errors in the two determinations and thus does not give a very stringent bound."

We have to admit to be deeply mystified by these statements. Even accepting the numbers in Eq.(**??**) at face value, we would have concluded first that the numbers actually nicely agree within the stated errors with their central values differing by merely 2.4%. Where does their 6% number come from? Adding in for good measure the second and third error quoted for $|V_{cb}|_{incl}$ and doing it linearly? This is a gross misrepresentation of the meaning of duality. *Duality does not mean that hadronic rates are equated with parton model rates – it means describing hadronic rates on the quark-gluon level including nonperturbative effects to the degree that the employed algorithm empowers us to do so!*

We also do not understand the logics here. The value for the form factor $F_{D^*}(1)$ used here is based on quark *models*, a single *quenched* lattice computation etc. – hardly well established precision tools. Yet the quoted theoretical uncertainty is treated as sacrosanct here. In the days when it was claimed that $F_{D^*}(1) \simeq 0.98$ was a firm prediction, this logic would suggest that duality violations had to be even larger by an additional 7%, right?

The very recent and comprehensive BABAR analysis of the lepton energy and hadronic mass moments as a function of the lower cut on the lepton energy provides even more accurate and still consistent values of the heavy quark parameters and even more sensitive experimental checks of quark-hadron duality.

The CKM minireview

$V(cb)$: It is not surprising that the authors of this article use the PDG article on $V(cb)$ as an input. Nevertheless they have to accept responsibility for their product. In this context we want to address two points:

• Their statement that in analysis "of inclusive decays, where the ... width is assumed to be that of a b quark decaying ... duality" is an untenable simplification that ignores a decade of careful studies. While they refrain from invoking the bizarre 6% 'claim' for duality violation mentioned above, they just state an ad-hoc uncertainty of 1% in $|V(cb)|_{incl}$ due to duality violation, without explaining, why they disagree with the findings of the 'Vademecum', which they do not even list among their references; likewise for the summary from the CKM workshop. At the same time they accept the stated uncertainty on $|V(cb)|_{excl}$ as gospel – despite the criticism listed above, which can be found in the existing literature.

• They also fail completely to recognize the novel elements in the new generation of moment analyses based on DELPHI's data for the extraction of $|V(cb)|$ *and* $|V(ub)|$.

$V(us)$: Their discussion of V_{us} has become quite obsolete due to the very recent KTeV results. Of course, they could not know about it, since the KTeV analysis was a well kept secret, while it was ongoing. Yet they cannot escape blame completely. The problem

of how to properly treat soft photon radiation had already been addressed in a series of theoretical papers and become manifest with the 2003 BNL-E865 result on the K^+ semileptonic branching ratio. Contrary to the new BNL-E865 and KTeV data, it is quite unclear how to apply radiative corrections to the old measurements. Averaging over the old and new data as done in this article thus seems to be quite inappropriate. The review also ignores the inconsistency of theK_{l3} formfactor slopes, as extracted from electron and muon modes. Yet this problem can no longer be ignored.

A suggestion: Finally we would like to make the general suggestion that PDG employ more flexibility in its choice of subjects for reviews and in its publication schedule.

Choice of subjects: Of course it would be inappropriate to have every two years a separate minireview for all nine CKM parameters. Which ones are thus covered should be determined by the amount of new experimental and/or theoretical activity, not by the content of the previous PDG edition. V_{cb} and V_{ub} are obvious choices – yet so would have been V_{us}. For the next PDG edition V_{cs} and V_{cd} very likely deserve their own minireviews as well due to the anticipated CLEO-c data and the hoped-for progress in lattice QCD.

Publication schedule: Having a fixed schedule where every two years all minireviews have to appear at the same time is clearly not optimal since advances do not always happen at a steady pace. The BABAR moment analysis and the KTeV V_{us} analysis are two topical examples of studies that establish new benchmarks that make even the best prior reviews at least partly obsolete. These days the vast majority of physicists download PDG publications from the Web rather than get it in hardcopy. Then there is no longer a technical reason against adjusting the publication of PDG products individually, in particular when people in the field know about a pending new analysis – like in the BABAR case – or rewrite and reissue a minireview, when a very significant new study comes as a surprise.

Looking forward to hear from you,

Ikaros Bigi, email:ibigi
Gino Isidori, email:
Gino.Isidori@lnf.infn.it
Olivier Pene, email:
Olivier.Pene@th.u-psud.fr
Patrick Roudeau, email:
roudeau@mail.cern.ch