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Title: 2000 CKM-Triangle Analysis A Critical Review with Updated Experimental Inputs and Theoretical Parameters

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In this paper experimental determinations of ϵ_K, $|V_{ub}/V_{cb}|$, Δ_{md} and limits on Δ_{ms} are used along with theoretical determinations of QCD matrix elements to produce a restricted region in the $\bar{\rho}-\bar{\eta}$ plane (Wolfenstein parameterization, from now on the bars are omitted for simplicity). According to the paper abstract: "The purpose of the analysis is to infer regions where the parameters of interest lie with given probabilities."

Now this is a laudable goal. Two groups whose previous work has been published, Parodi et al (ref 14) and Ciuchini et al (ref 19) have teamed up to produce a unified document. In this review I will discuss whether or not the goal stated above has been reached, and whether or not their results should be taken seriously.

I am not going to question the experimental data that they are using. However, each experimental measurement needs to be combined with a theoretically determined parameter in order to place a constraint in the $\rho-\eta$ plane. The error distributions of the experimental quantities are generally "Gaussian" which means that we know how to deal with them as statistically distributed quantities, although for a part of the experiment, the distribution

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of systematic errors is less well known. For the theory errors the probability density functions (PDF) are not known in most cases.

At this point there are two separate issues that I want to discuss. The first is the actual assessment of the theoretically determined parameters and their errors. The second is the use of Bayesian statistics to extract values.

Let us start with the first issue-theoretically determined parameters. 1) f_B, B_K, $f_B B_B / f_{Bs} B_{Bs}$: In principle these parameters can be determined quite accurately from UNQUENCHED lattice QCD calculations. What we have are quenched calculations with, in some cases, an estimate of the quenching errors and predictions of other models such as the quark model. The authors do not like the NRQCD value for f_B so they don't use it. Their reason is that two groups get different results (147±20 MeV versus 191±12 MeV). Perhaps the two different results reflect the actual uncertainties. Perhaps this should be a lesson for all the calculations? For $f_B\sqrt{B_B}$ they use 220±25±20 MeV, where the last error due to quenching is guessed at as 102) $|V_{cb}|$: The exclusive determination is done fine. However use of the inclusive determination leads to an unknown error because the theory (HQE) used to extract the value in this case is based on an untested assumption of quark-hadron duality in $b \to c\ell^- \nu$ decays. This may introduce a large error in the V_{cb} determination. It would be much better if they used only the exclusive results. 3) $|V_{ub}|$: Here again we have a competition between the exclusive and inclusive results. Except in this case the exclusive results have a large theoretical error because here there are only imprecise models. The inclusive results also rely on quark-hadron duality as commented upon above. Here though many final states contribute to $b \to u\ell^- \nu$, (unlike $b \to c\ell^- \nu$ where there are only a few) making this assumption less of a burden. However, the inclusive results don't measure the full rate because of the necessity to combat $b \to c$ backgrounds thus allowing the duality violation to bite back in manner that's hard to quantify (see Ligeti hep-ph/9908432). This paper takes V_{ub}=$(35.5 \pm 3.6)\times 10^{-4}$ while Hocker et al take $(34.9 \pm 2.4 \pm 5.5)\times 10^{-4}$. The discrepancy in the error is about a factor of two! Note that taking only the exclusive result $(32.5 \pm 2.9 \pm 5.5)\times 10^{-4}$ gives a similar value and error as Hocker et al. (The last error on the exclusive result is due to theory. Here the error represents the uncertainty quoted on a specific model and does not represent the spread among models. The latter procedure is dangerous if the models make similar assumptions.) 4) Δ_{ms} the PDF. Here experimentally we have only an upper limit. The authors choose a new method that uses the function defined in

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equation 50 for the PDF. ($\Delta\mathcal{L} = .5((A-1)/\sigma A)^2 - (A/\sigma A)^2$, where σA is the error on the measured amplitude A. This is used to subtract off the Likelihood that is "measured" at infinite mixing. However the validity of the normalization of the likelihood which allows the identification of $\mathcal{L}$ at infinity with a probability density function is questionable. (See Hocker page 32). The effect of using this type of PDF is to peak the Likelihood rather sharply near a fluctuation in the data near Δ_{ms} of 17.5 ps^{-1}. One should be wary of this because the errors in the mixing result are such that a positive at the 4σ level could only be seen if is for Δ_{ms} of about 7 ps^{-1} or less.

Next let us discuss the interesting subject of Bayesian statistics. While the subject of statistical inference using experimental data is an old an established one, there are still disagreements. The Bayesians believe in using the data themselves to establish the PDF's. This can in some circumstances be the correct procedure and usually the differences between this procedure and the normal Lagrangian approach based on apriori known PDF shapes, usually taken as Gaussian, are small. Here however we are dealing with highly non-Gaussian theoretical errors, sometimes not even quoted (i.e. assumption of quark-hadron duality), and usually unknown. The Bayesian approach tends to underestimate the errors. Furthermore it was shown by Hocker p52 that use of Bayesian PDF's for more than one parameter could essentially distort the distributions. Hocker also remarks that the probabilities in the Bayesian approach are only relative ones not absolutely defined (see Hocker equations 56 and 57). This paper does not even address this issue.

This is particularly important in the treatment of the theoretical errors. Our authors claim: "...we cannot find any conceptual difference which would force us to treat experimental and theoretical uncertainties on a different footing and claim that the standard method is a perfectly justified scientific approach able to establish confidence levels for the quantities of interest." I DISAGREE WITH THIS STATEMENT. (So does Hocker.) It is on this point that the analyses procedures really do differ. The Babar approach is to scan over "reasonable" ranges of the theoretical parameters (the so called scanning approach). Hocker et al use a hybrid scheme that allows the theoretical parameters to vary within their ranges but calculates a chisq for each point.

The authors resort to a "reasonable man" argument in several places: P8 "This makes the results largely stable against variations within reasonable choices of models and parameters used to describe the uncertainties due to

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theory and systematics." P11 when referring to errors on lattice calculations: "These evaluations are unavoidably subjective, though not arbitrary, as long as we use the judgments of responsible experts for each input quantity."

I believe that I am a "reasonable man," yet I do not believe the quoted results, the allowed regions are too narrow. This is due to the authors using very aggressively small experimental quantities (i.e. V_{ub}, V_{cb}, a narrow range of theoretical parameters and treating everything, especially the latter in a Bayesian manner. I don't agree that they have made reasonable choices for errors. The authors spend time criticizing the Babar 95

Falk in his plenary Lepton-Photon talk hep-ph/9908520 has: 'Combing the limits in the (ρ, η) plane from ϵ_K, V_{ub}, Δ_{md} and Δ_{ms} requires a consistent treatment of experimental and theoretical errors. This is problematic because the dominant theoretical hadronic uncertainties are difficult to quantify meaningfully and are certainly not in any sense Gaussian distributed about their "central values." ...Thus the Babar collaboration uses a scanning method to deal with the theoretical errors ...' Other, more restrictive regions in the (ρ, η) plane have appeared in the literature. However, it is important to note that they differ...from the scanning method...almost entirely in a less conservative treatment of theoretical uncertainties ....Until the lattice calculations of hadronic matrix elements improve, with quenching corrections under serious control, a conservative treatment of theoretical uncertainties is appropriate.

Stone in hep-ph/9910419 has : Some groups have tried to narrow the "allowed region" by doing maximum likelihood fits, assigning Gaussian errors to the estimated theoretical parameters. I strongly disagree with this approach. The ..scanning technique.., shown at this conference, while imprecise, is more justifiable.

It is clear to me that both Falk and Stone, one a respected theorist and the other a respected experimentalist are criticizing the technique used in this paper and saying that the scanning approach is preferred but they are not necessarily advocating it as the last word on the subject.

In summary I find this paper to be provocative and addressing important physics issues. However, I find the judgment used by the authors on assigning values and errors to be quite radical in choosing the smallest possible errors. I also find that their use of the modified Likelikhood function for B_s mixing and their Bayesian minimization to be suspect. Furthermore, the errors on the theoretical parameters must be treated with a great deal of caution,

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judiciously, and they treat them the same as the experimental quantitites. Any one of these grounds would be grounds for rejecting this paper, which I do.

Postscript: The use of English in this paper is not good. Besides the spelling errors there are unfortunate clauses such as: i) Since several years... ii) ...which is claimed of being too optimistic. iii) This also allows to answer to several... iv) ...and it is thus not founded to qualify as too optimistic.... v) ...f(cj,x) has been splitted.... vi) ...it is in order a discussion... vii) ...(until it will not be clarified)... the "not" does not belong here!

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