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CP Violation in B Physics — What have we learned, and what comes next? *

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Abstract

This talk is a personal reaction to the Osaka results from the B factories. I review the ranges of Standard Model parameters currently allowed, with the theoretical uncertainties discussed from a skeptical point of view. I comment on the improvements expected both from new measurements and from further theoretical work. My conclusion is that it is unlikely that the measurement of $\sin(2\beta)$ alone will present a clear challenge to the Standard Model, and that, more likely, continued work on both the experimental and theoretical fronts will be needed before we can assess whether there is new physics required to explain the CP-violating sector of B physics.

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I accepted the invitation to give this talk before the Osaka results from the B Factories were known. Like many others, I was optimistic that even those early results might teach us something new. We all now know that is not the case [1, 2]. All we have learned so far is that both B factories are functioning well, and that we need more statistics before we can say anything definite about even the simplest $\sin(2\beta)$ measurements. And I have learned it is dangerous to promise a talk when you are not sure what you will have to say!

One thing the new results clearly teach us is the risk of over-interpreting the central value given by a low-statistics result. Both Belle and Babar have presented an overall result for $\sin(2\beta)$ and also the results of several sub-samples (in the Belle case divided by CP final state, and in the BaBar case divided by tagging sample). The central values of the sub-samples in both experiments are all over the map! Looking at these sub-samples is a sure reminder that their combined central value is only meaningful together with its associated two-sigma uncertainty range. That, of course, is always true, but the theory community seems to have developed the habit of excessively focusing on the central value and analyzing what will happen if the errors shrink but the central value remains unchanged. That is not a particularly likely future. Indeed, both experiments have larger uncertainty than one would naively have calculated for the size of their samples. This is, I think, another reflection of the disparities of the sub-samples, and shows that the statistics are as yet too limited. A few events at relatively large time difference can have inordinate pull in the current small samples. The first lesson of this data is the need for patience and for continued hard work from the experiments. Another lesson I draw from this experience is the value of blind analysis. No matter how honestly people try to remove their biases in deciding what data to include in a given analysis, only the answer-blind decisions can be truly unbiased. Particularly when such variable sub-sample results appear the tendency to find arguments to discard one or the other may be high. The possible biases in choosing whether or when to do this are minimized in a results-blind analysis.

This talk comes after two talks on experimental results [3, 4] and before talk from Fabrizio Parodi [5] who will review the latest constraints on the Unitarity triangle found by combining all data and theoretical predictions into probability contours. Therefore, I will not review the standard formalism here. Rather I will concentrate my remarks on some reactions to the data, and to the general question of how theoretical uncertainties play into the issue of testing the Standard Model. I warn you immediately that mine is a conservative approach. I will try to ask where we stand now with a more skeptical view of the status of theoretical predictions than you will see elsewhere, for example in the next talk. That is not to be construed as a criticism of the work to be presented there. That work is essential; it establishes our best knowledge of Standard Model parameters, by combining all inputs in the only way possible, namely statistically. One can argue about small variants of the method, for example flat vs. gaussian distributions for theoretical uncertainties, but in fact, as long as all results are consistent with one another, these details make very little difference.

However my approach is to ask a different question. Suppose results do not fit together so neatly, then what level of conflict can be taken as a definitive indication of new physics? Asking the question this way leads me to a different level of skepticism about the "confidence levels" that emerge in an analysis that includes a statistical treatment of theoretical uncertainties. My skepticism has a historical base. In many, though not all, cases theoretical uncertainties are subjective rather than objective judgments about the range of possible values for a given theoretically-calculated parameter. Before there is any conflict in the data the commonly expressed ranges tend to be dominated by the optimistic voices, who believe the errors are quite small and relatively well-controlled. After measurements appear to be in conflict one hears more readily the skeptical voices who assign a larger uncertainty. Consider the history of measurements and theory for ϵ'.

Of course in the experimental world there are uncertainties that are not purely statistical too –we call them systematic errors. These too need to be approached skeptically. However it appears to me that the methods for studying these uncertainties and making grounded estimates for them is much better developed in the experimental community than in the theoretical one. A large part of any experimental talk is a discussion to convince the listener that this job has been carefully and honestly done. When did you ever hear a theorist (aside from one doing lattice calculations) spend more than two minutes in a talk discussing how he or she arrived at error estimates. One of my points here is that we need to take more time to do this. I may be overly skeptical of some uncertainty estimates, simply because I have not heard any detail of the arguments on which they are based. Others may be over accepting, for just the same reason!

In many cases the distinction between a theoretical error and an experimental systematic uncertainty is not so clear. The estimate of the impact of a certain cut or correction to the measurement often depends on modeling based on theoretical inputs. This is unavoidable. However whenever it is possible to quote an experimental result in a way that removes the theory uncertainties from the number given, this should be done, in addition to the exercise of folding that number together with theory to extract, for example, a range for Standard Model parameters. Then, if the theory gets better, re-analysis is straightforward. This statement should be obvious. But today it is not always easy to find the un-mingled results, even when they could readily be given. We have become so used to folding together theory and experiment via monte-carlo modeling that we do not always take the time to ask whether the impact of the model can be reported as a separate rather than an integral step in the analysis.

It is very hard to find any test of the Standard Model where none of the measurements to be compared is subject to theoretical uncertainties. CP Violation is B physics offers one beautiful and clean measurement, that of $\sin(2\beta)$. The interest in this measurement is high, precisely because it is so free from theoretical uncertainties. For that reason alone it is a valuable measurement, one which can truly improve our knowledge of the Standard Model. However the same cannot be said of the all the measurements that currently constrain the Unitarity triangle, and hence give an expected Standard Model Range for $\sin(2\beta)$. In the remainder of this talk I will review those predictions, and discuss for each two points –the current uncertainty ranges, both those usually accepted and my conservative (and biased) versions, and secondly, and more importantly, what theoretical or combination of theoretical and experimental progress can be expected in the coming years to better define these quantities.

If you look at the Babar Physics Book [6] analysis of these predictions, an attempt is made to separate measurement errors and theoretical uncertainties in defining the allowed region in the complex plane for the apex of the unitarity triangle, that is for the point (ρ, η). The theory uncertainties are then treated as flat distributions, with sharp edges, whereas the experimental uncertainties are treated statistically. This approach is of course no less arbitrary than the approach of treating all ranges as gaussian errors; it is like debating about angels dancing on pins to choose between the approaches. Rather one needs to look at both and then make judgments. But one thing the Babar analysis does make clear, from the relative sizes of the experimental error ellipses compared to the range of theoretical uncertainties (see the figure below), that in every case our uncertainty in knowledge of the Standard Model parameters is at present dominated by the theoretical uncertainties.

This does not mean there is no role for experiment in improving the situation. Often, the easier measurement is more difficult to interpret theoretically than an alternate harder measurement for which the translation to Standard Model parameters is theoretically cleaner. So sometimes new measurements will yield large improvements in theoretical uncertainties, even when the current errors appear dominated by theory. There is another impact of further experimental input, and that is in testing implications of the theory or model in other channels. The prediction which is used to give the translation of a particular measurement to a Standard Model parameter may be one of many predictions made by a given theoretical approach. Our confidence in the reliability with which we understand the range of uncertainties of that approach can be much improved by multiple comparisons of that method with data, much of it data that does not yield a new measurement of a particular standard model parameter. So one of the joint asks for theory and experiment in the coming years is a systematic comparison of multiple predictions with data. This is not the kind of work that wins any prizes, but it is essential if we are to understand how well the Standard Model works, or to have confidence when we claim it does not work.

Now I turn to a discussion of a series of quantities that determine allowed regions in the complex plane for the point (ρ, η), the apex of the unitarity triangle. There are four types of measurement; the value of the CP-violating parameter ϵ in K decays, the ratio charmless B_d decays to those with charm, the mass difference between the two B_d eigenstates, compared to their average width, and likewise the mass difference for B_s eigenstates compared to average width of these states. I will discuss each of these in turn below. My error estimates are as subjective as anyone else's. In fact in what follows I will err on the side of skepticism, in order to make my point. The best estimates are always those you get by asking the experts, but you should insist that you understand how they got their answer, and what is and is not included in it.

At present, the quantity ϵ' has larger uncertainty in its theoretical interpretation than those mentioned above. Hence, it is always ignored in these discussions. Eventually, as the predictions become better constrained, it may play an important part of testing the theory. At present it provides the object lesson for my caution here. The current measurements are well outside the range predicted by the dominant theoretical approaches before the measurement. Yet, after the measurement, other calculations and more conservative error estimates became more visible. Today one cannot say that there is a conflict with theory. But new lattice calculations of the B_6 and B_8 parameters (terms whose definition is not important here) could clarify this situation soon. This is an important task.

To translate the quantity ϵ, which is measured from the mass difference of the two neutral kaon mass eigenstates, into a range of Standard Model parameters one needs to calculate the matrix element of a four quark operator between the K^0 and $\bar{K}^0$ states. This matrix element is typically quoted by giving the parameter B_K, which is the ratio of the true matrix element to the naive estimate obtained by dividing the four quark operator into two two-quark terms and inserting the vacuum state between them. While this is a convenient way to parameterize the result, it has the disadvantage that it brings into the picture another theoretically-uncertain parameter, namely the mass of the strange quark, m_s, which enters in the naive estimate. The different theoretical approaches for calculating B_K have different approaches to how uncertainties in m_s enter their answer. Ideally a direct calculation of the matrix element, such as a lattice calculation, avoids this problem altogether. However one must take care if this calculation is quoted as a value for B_K that it is used in conjunction with the corresponding lattice value for m_s, and not one calculated by quite different methods. Since lattice values for m_s are turning out to be quite low compared to traditional estimates this can lead to significant differences in the overall results.

Not all quantities can be calculated on the lattice. However I think that it is fair to say that for any hadronic quantity that can be approached by lattice calculation this method is eventually going to be the most reliable. Lattice calculations are a subfield with a set of conventions all their own, and it is important to recognize it in reading their error estimates. Like experiments of other kinds, these numerical methods have two types of uncertainty, statistical and systematic. The statistical uncertainty is well-defined and clearly stated in their papers. The systematic effects generally have three major sources, one is from extrapolation from the lattice to the continuum and the matching of lattice parameters to continuum parameters, the second is the extrapolation from the set of fermion masses at which the calculation is made to physical numbers and masses of light flavors, and the third is the error that occurs when a quenched calculation is used instead of the full calculation that allows quark- antiquark loop effects to be treated. Quenching, which removes all such loops, is an approximation that greatly simplifies and speeds up lattice calculations, but at the price of introducing this third, not well controlled, uncertainty. Nowadays we are beginning to see results for certain quantities from unquenched calculations. That is a direction that will yield reduced uncertainties in the interesting quantities for B physics in the near future. Many lattice papers discuss but do not give clear numerical estimates for the systematic uncertainties. In fact you may have to read carefully, the statement is made that a calculation is done with these particular input fermions and is a quenched calculation, and then, because these effects have been discussed in detail in earlier papers, no further discussion is given of the impact of these approximations on the uncertainties in the result. Within the lattice community this is all well understood, no-one is trying to deceive anyone here, but an outsider reading these papers to try to extract uncertainty ranges has a lot of work to do. I find the best place to look is at the annual lattice theory conference talks, such as Lattice 2000, which in recent years have plenary talks that summarize the current status of the various quantities.

For the quantity B_K the current values lattice calculations quote an uncertainty of about $\pm 15\%$. There are beginning to be some unquenched calculations. New calculation by a somewhat different method that use what are called "domain wall fermions" yield somewhat different results, outside the range of statistical errors quoted for earlier work [7]. The favored lattice value for m_s is quite low, and this needs to be better understood. To be conservative, until these two issues settle down, I will show in my figure what happens to the predicted range in the complex plane if we use instead an error of $\pm 25\%$ on this quantity. Fortunately this is one area where it is quite clear that progress is possible. It will not be long before we have reliable lattice estimates with smaller errors, but I think today one cannot yet be quite so certain.

The measurement of $|V_{ub}/V_{cb}|$ from the ratio of charmless to charm-containing weak decays of the B gives an allowed range of values for ρ and hence a circular band in the ρ, η plane, centered at the origin. There are several classes of measurement, each with different experimental difficulties and different theoretical uncertainties. First, and theoretically simplest, is the total inclusive rate for semi-leptonic B decays with and without charm. If one could measure this it would translate with very little theoretical uncertainty into the desired parameter ratio. However, since charm quarks decay and are not directly observed, one can only be sure a decay has no charm by making a kinematic cut that excludes charmed final states. Then one is measuring not the total rate but some fraction of it. The impact of the cut requires a theoretical estimate. There are two approaches, using a cut on lepton energy, or one on hadronic invariant mass. Each introduces some theoretical uncertainty. Improved experimental mass resolution could reduce the theoretical uncertainty in the second method by allowing a higher mass cut. These two methods are the leading ones today and probably will continue to dominate the results even if the other suggested methods can be tried. Some proposals have been made for using fully hadronic decays, but they are, in my opinion, prone to larger theoretical uncertainties. Finally theory offers another option which is to use the ratio of similar exclusive semileptonic B decays and D decays, such as $\rho l\nu$ or $\pi l\nu$ with the same kinematics for the B or D to ρ(or π) transition. In the leading heavy quark approximation the uncertainty that arises from the form factor for this transition cancels in this ratio. However Λ_{QCD}/m_c corrections to that ratio will still give relatively large theoretical uncertainties. Furthermore the experiments are very difficult, it will require much more data before this method is feasible. Perhaps by then theorists will have reasonably reliable calculations of the Λ_{QCD}/m_c and Λ_{QCD}/m_b corrections; if so this method may provide an interesting alternative.

At the present time the uncertainty quoted in the particle data book [8] for $|V_{ub}/V_{cb}|$ extracted from inclusive semi-leptonic B_D decays is of order 18%. I show in my figure a more somewhat conservative number of $\pm 20\%$, to demonstrate what happens if this uncertainty is increased a little..

The remaining side of the triangle (with base normalized to 1) is fixed by the value of V_{td}/V_{cb}. This can be extracted from the mass difference between the two B_d mass eigenstates. Like the similar calculation for the K case this method requires calculation of a hadronic matrix element, which leads to significant theoretical uncertainties. Again the matrix element is usually expressed as the ratio of a vacuum insertion estimate to the actual value. Here the two-quark vacuum to B meson matrix element f_B is not as well measured as the corresponding quantity for kaons, while the quark mass m_b has a small percentage uncertainty compared to m_s. So the quantity for which theory estimates are made is $f_b^2 B_B$. Since the square root of this quantity enters in the value of V_{td}, it is the errors on that square root that control the uncertainties. Again the leading calculations are lattice calculations, but with the added complication that one must treat a system with one heavy quark. The quoted uncertainties are typically of order 16% [9]; my more conservative version is shown for an uncertainty of 20%. Note that this corresponds to 40% uncertainty on the matrix element itself. Unquenched lattice calculations are still needed here, and I believe they will provide considerable improvement. Direct measurement of f_B in a purely leptonic B decay would provide a good test of the lattice accuracy, but it is extremely difficult to achieve.

However the measurement of mass difference between the two B_s mass eigenstates would allow a much cleaner extraction of the ratio V_{td}/V_{ts}, from the ratio of the two mass-difference results. At present there is only an experimental lower limit on this quantity, and even this plays an important role. Since V_{ts}/V_{cb} is well-constrained by unitarity, this approach will give much smaller theoretical uncertainty in V_{td} than the B_d measurement alone. The measured ratio of mass differences is directly related to the ratio of CKM parameters, times a correction factors usually called ξ_s^2. This quantity is 1 up to QCD and SU(3) corrections. The QCD corrections have been calculated to next to leading order, giving $\xi_s^2 = 1.30$. The SU(3) corrections, coming from differences in B_d and B_s wave functions should be of order m_s/m_b, so I think the usual estimate that these, plus any residual QCD uncertainties are not more than $10 - 12\%$ is, in fact, quite conservative [9]. This means that a measurement of x_s, the ratio of mass difference to average width for B_s states is important. It is expected that it will be measured by CDF in the next year or so of TeVatron running. Even the current lower limit plays an important role in constraining the allowed Standard Model parameter range.

The situation shown on this figure may, at first glance, seem a bit depressing. The likelihood that a single measurement of $\sin(2\beta)$ can convincingly demonstrate the need for physics beyond the Standard Model is small. All this says is that testing the Standard Model is a long term program both for experiment and for theory. Clearly the experiments know this; no-one sees measurement of $\sin(2\beta)$ as the only task, merely it is the first important and interesting one. The theory community is also facing up to its share of the work. New ideas for calculations of many B-decay modes, such as those presented here by Matthias Neubert [10], are constantly being explored. Theorists are laying out more clearly what measurements give complementary results, and how to test aspects of the theory or model that can help reduce uncertainties in extracting CKM parameters. Much ongoing work by lattice calculators is now focused on quantities of interest for this physics. We can expect continued improvement from that work.

The message here is not that nothing can be done to improve the situation, but that much can be done, and that it must be done if we hope to be able to see signatures for new physics in B physics. Many results must build together to see whether or not the picture is consistent with Standard

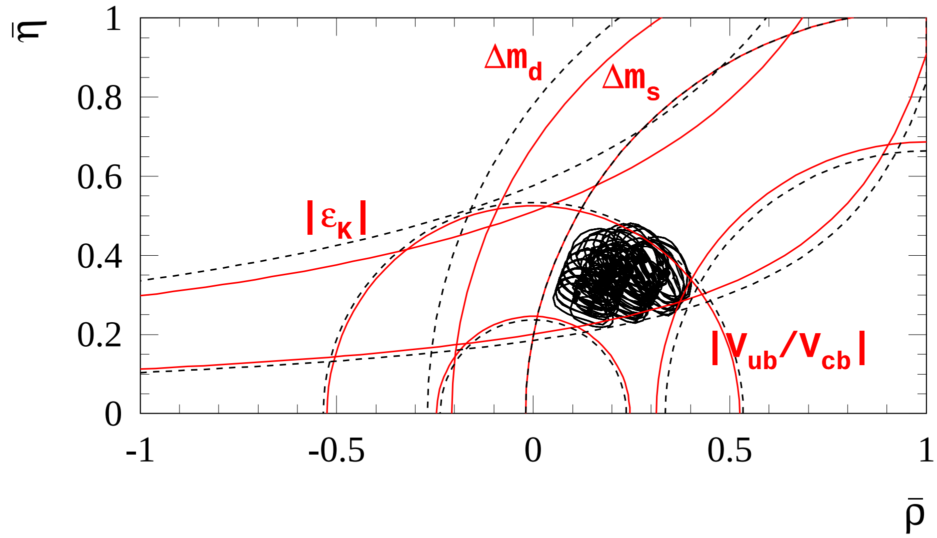

Figure 1: The constraints on the Unitarity Triangle. The basic figure is an updated version of the Babar Book figure (thanks to Marie-Helene Schune), with theoretical uncertainty ranges described in the text as the usual current estimates. Each black contour shows the 2 sigma region for the point (ρ, η) given by combining experimental errors, for a given choice of theory parameters within the range. The dotted lines show how the region grows if the more conservative estimates for theoretical uncertainties described in the text are used.

Model predictions. One dramatic discovery would be much easier to explain to the world. It is still possible that this could occur. But much more likely, with what we now know, we will have to keep working for some time on both the theory and the experiments to explore this question. Only after several years more work, after combining all the various pieces of information, will the answers emerge.

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