

Round Table Discussion at the Final Session of FPCP 2008: The Future of Flavor Physics and CP

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The final session of FPCP 2008 consisted of a round-table discussion among panelists and audience. The panelists included Jeffrey Appel(moderator), Martin Beneke, George W.S. Hou, David Kirkby, Dmitri Tsybychev, Matt Wingate, and Taku Yamana. What follows is an edited transcript of the session.

1. Question: What are the big questions in flavor physics at FPCP08?

Jeff Appel

Many of us from many places have to write trip reports when we get back. And perhaps when writing the trip reports we could start with the big questions in flavor physics that came up here. This is meant to help to you write the trip report as well as to focus the discussions to come. A number of topics were suggested by people who sent an email. So you can read them here.

- CP violation in charged vs neutral B decays?
- Mixing induced CP violation in the B_s system?
- $D-\bar{D}$ mixing: How soon can we measure mixing parameter x ?
- Spectroscopy: What are the XYZ states in the charm sector (counterparts in the bottom sector?)?

I don't need to go through them one by one, but I will ask our panel members to begin with what among these topics they found most important; what they think missing from the list. Martin why don't we begin with you?

Martin Beneke

The list includes most of the hot topics discussed at this conference. The first two items refer to phenomena connected with $b \rightarrow s$ transitions, where the window to new physics is still open widest. However, we have learned in the past few years that the standard flavor theory is working quite well. The much discussed hints in the $b \rightarrow s$ sector are either not conclusive (second item) or possess alternative hadronic standard-model interpretations (first item). The actual observation of $D - \bar{D}$ mixing is exciting as a phenomenon, but because of theoretical uncertainties, does not tell us much that we did not know before about new physics.

Matt Wingate

From the lattice QCD perspective, the most interesting thing discussed here was the discrepancy between the HPQCD calculation of f_{D_s} and the experimental measurement. The lattice result is quite sound: the non-strange decay constant f_D is the one which requires more work, namely extrapolating lattice data to the physical up/down quark mass. The fact that f_D agrees with experiment while f_{D_s} does not is an interesting puzzle. The precision quoted for the lattice result is very impressive, and further details from the authors will allow other lattice experts to judge the quality of the fits involved. It doesn't seem plausible to me that the source of the discrepancy could be blamed on the fourth-root hypothesis used in staggered-quark calculations.

One thing which I am investigating is: What more can be done on the lattice in studying $b \rightarrow s$ decays? There are difficulties for the lattice here which are not present in $b \rightarrow u$ decays or neutral B meson mixing. Nevertheless, the $b \rightarrow s$ decays are of such great interest that all approaches, including lattice QCD, should be pushed as far as possible. I think there are calculations we can do which will add to the picture.

Dmitri Tsybychev

I just want to add that whether there is mixing induced CP violation in the B_s system will remain a hot topic for next couple of years, and hopefully both D0 and CDF experiments will have updates on their results; if not in the summer 2008, then in the fall. There is room for improvement on the precision of measurements of ϕ_s for both experiments. With continuing successful running of the Tevatron, both experiments plan to collect up to $8 fb^{-1}$ of data. CDF already has a sample of $3 fb^{-1}$. Their current result is based on a data sample of only $1.3 fb^{-1}$. The D0 experiment has already used the full sample of $2.8 fb^{-1}$ available to date. Therefore it will be able to increase its sample only when new data are collected. However, D0 plans to improve the selection of B_s mesons decaying into $J/\psi\phi$. As was already mentioned, D0 can increase the statistical significance of its sample by 20% through a better selection. This will directly translate to an improvement of the measurement.

Additionally, a question still remains involving

SU(3) or U-symmetry. D0 constrains the strong phases involved in the B_s angular analysis to the similar phases that appear in $B_d \rightarrow J/\psi K^*$ decays, and are measured at B-factories. The constraint is rather weak, and allows for SU(3) symmetry breaking, which may be as big as 10%. Polarization amplitudes, measured in B_s and B_d decays, are compatible within measured uncertainties. This may indicate that such symmetry exists. The result on phi_s does not change significantly if the phase constraints are removed. However there is no consensus whether such a constraint should be applied, and one can benefit from a stronger theoretical motivation.

Jeff Appel

You think that the systematic errors are not coming soon for how well you can do on this?

Dmitri Tsybychev

The fit result for the case of free strong phases is provided in the D0 article in PRL, and agrees very well within statistical uncertainty with result of the constrained fit.

Jeff Appel

Anybody else? Does anybody in the audience want to add to this list?

David Kirkby

I think the main question in flavor physics is where the new physics is going to show up, if anywhere. We should remember also that there are certainly topics in flavor physics that have intrinsic interest: spectroscopy, for example. But how likely is it for new physics to show up there? To get the audience more involved, how about a show of hands? Where do you think that the new physics is likely to come from? Raise your hand once at which one of these four you think is the most promising. So, how about the first one?

Jeff Appel

You have to leave your hands long enough for count. 1, 2, 3, 4..

David Kirkby

So how about the second one, the B_s ? Which one of the four is the new physics most likely to show up?

Rahul Sinha

They are connected. If you find Δ_S not equal to zero in B mixing you are likely to find other signals of new physics such as a deviation in the small B_s mixing phase among other things. They are connected, since Δ_S can be written in terms of the small $B_s - \bar{B}_s$ mixing phase.

David Kirkby

The second one. What is generated from the B_s system? Can we find new physics there? How about $D - \bar{D}$ mixing? Well you don't know, but what's your intuition? What's your gut feeling?

Choong Sun Kim

I thought of the story of the $D - \bar{D}$ mixing. There is no standard model prediction. How can you find new physics?

David Kirkby

How likely do you think you are going to find something there?

Rahul Sinha

Yes, you can measure the $D - \bar{D}$ mixing phase with a precision of about 1 degree at Super- B , but we need 50 inverse attobarns.

David Kirkby

How about spectroscopy? Beyond the standard model? QCD is not new physics.

Unidentified voice

It's kind of obvious that new physics will show up there, and new particles can contribute to the amplitudes like penguin decays or $B \rightarrow \tau$ decays. In my opinion, this will be the best place to look for new physics.

Jose Ocariz

I agree with the previous comment that it's a necessary condition; but it's not sufficient. For example, if we think of item 1, I have a feeling that this is more or less motivated by the measurement of the different CP asymmetry in $B \rightarrow K\pi$ decay. This is a non-controversial measurement, but the interpretation is not uncontroversial. There is no way of falsifying the standard model by this kind of measurement despite the fact there is potential sensitivity to contributions from non-standard physics.

[Comment by Tom Browder added in preparing this report: The discussion seemed to imply that there is no possible future resolution of this issue. However, the isospin sum rule proposed by Gronau and Rosner is a model-independent test for new physics. It requires much more data (> a factor of ten) and much more precise measurements of $A_{CP}(B \rightarrow K^0\pi^0)$.]

Gerald Eigen

Martin, you brought up the $b \rightarrow s$ transitions. I agree with you that these are important. Since point 1 is rather general, wouldn't you rather split them into subtopics that are associated with different points than including them all under point 1?

Martin Beneke

I was thinking of mixing-induced CP violation. Would you like to include $b \rightarrow s\ell\ell$?

Gerald Eigen

Yes, and also the b transitions involving $s\bar{s}$, like ϕK_s , $\eta' K_s$, etc. The leptonic penguins clearly belong under point 1, while the gluonic penguins fit better under point 2.

(George) Wei-Shu Hou

I know I am viewed as a fanatic, saying that fourth generation this and that., fourth generation for everything. I actually quite agree with what Martin and Jose said, and that this kind of discussion can be endless, and we are not going to go very far. But the converse is not true; that if you show that in some new physics model you can generate an observed effect, it would still be of interest. So [going into a short presentation] this result here is published in 2005, and

I give you the diagram. I have said many times during the conference, that having a t' would bring in large Yukawa couplings and new CP violating CKM elements. Our study was re-done at next to leading order in PQCD, and the effect on DCPV difference was not diluted. And there is another thing, that it does push down Δ_S .

It's not sufficient to generate the central value of the experiment, but to me that is very interesting. The two things mentioned in this conference are of note to me. One thing. Maybe I pull this slide [from Derek Strom's talk] back. If you look at this, here is the Standard Model expectation for ϕ_s , and here are all four different, related measurements. All the measured values fall to the left. And here is the actual published prediction from 4th generation (which is smack in the middle of the experiments). I already stated something like this, large $\sin(2\Phi_{B_s})$, in 2005. This is on the record. At the moment, I am not a UT-fitter fan, and nobody here is. I am not on the IAC. I would have voted for them to be here, just for the debate. At the moment, you know, experimentally one can not yet say too much. It's not inconsistent with the Standard Model. I do point out that these numbers normally would be scattered (if the SM is correct), but they are not. The error bars will get reduced, say in next two years, from 1.35 inverse femtobarns of data, to 3 to 5 to 8. In the last year or two, I used to say that if the central value stays, I would then be willing to bet a good bottle of red wine that the 4th generation is real. Starting a year ago at FPCP in Slovenia, the data seem to be heading in this direction. Now here [another slide on A_{FB} from Eigen's talk] is one thing that Gerald brought up but didn't really go through. The green line in the Belle plot is marked "C9, C10 sign-flipped", which is equivalent to C7 sign flip. The blue line in the BaBar plot is for the Standard Model, almost zero, but slightly negative. Now the upper figure was actually shown by Dmitri [Tsybychev] in his talk. The blue dashed curve, is the fourth generation differential A_{FB} , and the marked red line gives roughly the lower q^2 bin here. So you can understand why the Standard Model is slightly negative and close to zero; because below the zero is negative. Sorry that the sign convention is opposite to the B-factory experiment. And above the zero is positive but there is a bit more negative than positive so that you get the blue zero, or close to zero, of the SM in the BaBar plot. But in our fourth generation analysis the line moves down. So the zero moves further down, and there is not much negative part but large positive part; so it's more consistent with Belle/BaBar results. And I think it was Uli who raised this issue, you know, complaining what is still called by experimentalists the C7 sign flip. This is basically a way that experimentalists say that there is a deviation. And this is why I stressed that I want to treat these things more generally, to allow com-

plex Wilson coefficients. This gives the shaded area. I don't want to go into any further details. Let me change tone and say — I am willing to bet a good bottle of Champagne now, if you want to take up the order. Why? Now this [yet another slide] is the standard folklore that Standard Model CP violation is 10^{-20} . Here is the Jarlskog invariant, and A here, the invariant CPV area, is like 10^{-5} . But the real suppression is coming from these small masses. So if you put in numbers, when you normalize properly with, say, the electroweak phase transition temperature, you get this 10^{-20} . Now you see the fourth generation does miraculous stuff here because it naturally has large Yukawa couplings. So if you shift by one generation, this $m_c^2 - \mu^2$ becomes $m_t^2 - m_c^2$, etc. This gives rise to a very large enhancement. Well, it is still a suppression factor, but the $m_b^2 - m_s^2$ alone is the only suppression. So this gives a 10^{15} gain, where about a factor of 30 is from the $b \rightarrow s$ CP violating analysis. OK, but the factor of 30 compared to 10^{15} is nothing, so long that this factor of 30 is not 10^{10} , or something. So we have a very large enhancement factor compared to the Standard Model three generation Jarlskog invariant. I think this is another proof that Nature is more ingenious than anyone of us here. But for me, to be able to jump back to put the CP violation within Yukawa sector to be relevant for baryogenesis, that's why I say I am willing to bet a good bottle of Champagne now, ... but only for ten people, OK?

Choong Sun Kim

I do not know all the details of fourth generation, but I have some simple questions. First, as you know, and as everyone knows, this fourth generation neutrino mass is quite heavy, > 45 GeV. So why do we have such a heavy neutrino, much different from the first three generations? That's very strange to me. This kind of thing comes out more naturally if we have something like a string-inspired E(6) model, which predicts rather heavy vector-like quarks, unlike the fourth generation.

(George) Wei-Shu Hou

Well, there is a very simple answer to that. Vector like quarks will not have this enhancement. These are not masses, these are Yukawa couplings. Dirac masses go into the denominators, propagators and decoupling. So we can not have enhancement.

Choong Sun Kim

Something like Kaluza-Klein or some other excited states. I think probably a similar result will come out generally without a so-heavy neutrino problem.

(George) Wei-Shu Hou

Yea, OK. I can not argue with Kaluza-Klein. They are all legitimate, but this one (4th generation) is within Standard Model dynamics! Now for neutrinos, we firmly know there are only three light ones. But since 1998, as compared to 1989, we also learned that neutrinos have mass. So it's a much richer sector than we knew of. Furthermore, you didn't mention

electroweak precision tests, right? There is a recent paper by Kribs et al (Plehn, Spannowsky and Tait), which refutes the very stringent application of precision test against the fourth generation in the PDG list. So it's not ruled out. But whatever you say, I am just saying this more than ten-order-of-magnitude gain is so enormous. I use this to argue that, despite electroweak precision tests, even the neutrino stuff, the 4th generation is fairly legitimate. The thing is, when you have high scale CP violation for baryogenesis, such as leptogenesis, you tend not to have a laboratory test. It's a matter of physics in the usual sense.

Taku Yamanaka

I didn't vote for any of the four items up there. Since I am an experimentalist, and since I work on kaons, I will vote for kaon physics experiments. The sensitivity of a $K_L \rightarrow \pi^0 \nu \bar{\nu}$ experiment will first go down by three orders-of-magnitude, from $O(1E-8)$ to $O(1E-11)$. Even beyond the Grossman-Nir limit, there is a two-orders-of-magnitude parameter space for new physics to appear. So, do you want to vote for a 10 percent effect, or do you want to vote for a large parameter space with two orders-of-magnitude? I would vote for a two-orders-of-magnitude effect.

2. Question: What are the big flavor-physics questions to come?

Jeff Appel

There is another way to continue this discussion which is the second question. That is, what are likely to be the big flavor-physics questions after the first Tevatron or LHC signal beyond the standard model? And a corollary question is what would be the flavor-physics questions if we don't see a new signal at LHC?

The answer given for the first part is that the interesting flavor-physics question will depend on what you see. However, almost anything you see will have multiple possible answers, multiple models which can explain it. This may mean that there are sensitivities to flavor physics across the board. In fact, I don't think a signal in a particular channel will lead to only one flavor-physics parameter that you want to look at. That's how I guess I would put it.

Tom Browder

If a signal really shows up early at the LHC, I think the big question will be how any new particles at LHC do not produce flavor changing neutral currents. The theorists will have to find brilliant ways for cancellations to not produce flavor changing neutral currents, not just produce a new model.

Jeff Appel

So you don't think there will be big signals from LHC? I didn't mean to put too many words into your mouth. Anybody on the panel want to respond to this more ambiguous question?

David Kirkby

I think it is easy to imagine new physics at LHC where you wouldn't really know what to do at the Super-B-factory. So maybe the challenge to the audience is "Can you think of something we may find at the LHC where it would be unclear what to do in flavor physics?" Are there other scenarios? Let's talk about that.

Rahul Sinha

If you see a signal of something at the LHC, you want to make sure that the theoretical parameters corresponding to your favorite model/scenario, and that are consistent with the signal, are not actually ruled out by precision tests; and B physics would provide a precision constraint, through loop contributions. Therefore, you want to make sure that B data is consistent with the scenario and the observed signal. That is one way again of using flavor physics.

David Kirkby

There are strong constraints from the data we already have.

Rahul Sinha

This is not enough. As to whether the current flavor constraints are good enough - let me say we need to improve; we need as much improvement as possible. With the LHC alone, we may see a signal of new physics, but we may not be able to figure out what kink of new physics it corresponds to. Here is where flavor physics comes in, ruling out or finding consistency among different models given a particular signal. The better the precision, the better the constraints. One requires flavor physics to enable pinning down what is the new physics.

Martin Beneke

We discuss flavor physics in the context of the TeV scale. In doing that, we almost always implicitly assume that electroweak symmetry breaking is caused by some weak-coupling phenomena. That's not guaranteed. An entirely different way of seeing things would be needed if it turns out that electroweak symmetry breaking happens through some QCD-like strong-coupling mechanism. Then the flavor-physics puzzle is more severe, because if there is no weak coupling at the TeV scale, we would know that flavor physics is probing much higher scales which are disconnected from TeV scale. So, indirectly, one of the big flavor-physics questions to come and be answered is what causes electroweak symmetry breaking.

Keh-Fei Liu

I wonder if one of you could comment on neutron electric dipole moment in terms of its discovery potential, and if there can be some effect found in the next couple of years. Will the new physics be orthogonal or complimentary to this flavor physics?

Jeff Appel

The coupling to the neutron electric dipole moment for any of these questions. George?

(George) Wei-Shu Hou

You mean something specific. I actually asked Junji Hisano, the expert. Basically 4th generation effects can enter through loops. I think that's what you are referring to. So that should be studied, yes.

Keh-Fei Liu

In the program of looking for new physics, I want to see whether there is a discovery in a channel that would be complementary to the study here in flavor physics. Or, is there some orthogonal result?

Dmitri Tsybychev

I think it's a general problem. If you see something at the LHC, how do you reconstruct the underlying physics? Say that 200 models can give you the same signal. It will take more than just one significant deviation in one channel to really understand the nature of the new physics.

Rahul Sinha

Typically, one talks about the missing E_T signal at the LHC. I would like to get an opinion as to whether flavor physics can help in pinning down the nature of the new physics. With flavor-physics constraints included, it would be interesting to come up with signals that can help to say whether it is SUSY or not SUSY.

Dmitri Tsybychev

If you have missing E_T , it could be anything. It could be supersymmetry. It could be a leptoquark. It could be extra dimensions. There are a number of scenarios that will result in large missing E_T .

Rahul Sinha

Sure. But, what is it that should be really watched out for, say, for SUSY or other new physics, and what kind of measurements in flavor physics can actually help distinguish between the kinds of scenarios. Is it possible to do that? Anybody?

Jeff Appel

I think the point is that too many things have missing-energy signals to say that this or that is the specific answer.

Tom Browder

There is a sort of a worldwide effort, at CERN and other places. People are writing very thick yellow books about the connection between flavor physics and the physics at LHC. They do consider lots of different scenarios in the possible impact of all the observables in B physics. You may find reading these articles boring now because we don't have a new physics signal at the LHC to look at. But there have been pretty substantial efforts and a lot of papers on this.

Enrico Lunghi

I have a general comment on the first two questions. ATLAS and CMS are mostly "flavor-diagonal" experiments. On the one hand, they will tell us the mass scales and the tree-level structure of whatever new physics model is realized in nature. On the other hand, the quantum structure of the theory (e.g. loop effects) will be hardly accessible. The latter task is perfectly suited for flavor-physics experiments, that will act as a tie-breaker among the several equivalent

new physics models that will emerge from the first analyses of LHC data. Of course, these kinds of studies require inputs from ATLAS and CMS. Once a few masses and processes are known, one can construct complete models and predict which flavor observables are expected to deviate from the SM predictions. It is also possible that ATLAS and CMS will not find any new physics. In this case, flavor physics (including lepton flavor violation) will allow us to access to much higher scales (e.g. hundreds of TeV). There are two scenarios. If ATLAS and CMS find TeV-scale new physics, flavor physics will help to find out the detailed structure of the theory. If, on the other hand, new physics turns out to be beyond the reach of direct production at the LHC, we can still explore it via super-rare processes (e.g. lepton flavor violation).

Choong Sun Kim

I have some unrelated questions for Hsiang-nan and Martin about the previous discussions. Everyone knows that we, within the standard model, can not calculate the B to $\pi^0\pi^0$ branching fractions. Is that new physics?

Martin Beneke

No.

Choong Sun Kim

Because the error is quite small. The experiment error is small.

Martin Beneke

But the theoretical error is not so small.

Choong Sun Kim

But you can explain all others except for $\pi^0\pi^0$. Even B to $\rho^0\rho^0$, which has exactly same quark diagrams as $\pi^0\pi^0$, can be predicted rather well. When the measurements began, it was quite different - theory predicted only 1/3 of the experimentally measured branching fraction. So I think today's value is kind of a post-diction. You just changed the input parameters. Therefore, even though we think it is rather trivial, like the color-suppressed tree, it can be something else - like beyond the standard model.

Martin Beneke

We have learned that the dynamics behind the color-suppressed tree amplitude is very different from the naive factorization picture, and also understand why the theoretical uncertainties are large for this amplitude.

Rahul Sinha

I just want to ask something since you raised the question about factorization and naive factorization. Naive factorization works so well in D decays. We all remember the classic paper of Bauer, Stech and Wirbel. Factorization, however, does not work so well in B decays as is evident from data. Is there a good explanation for that? Why does factorization work better for D decays and not that well for B decays?

Martin Beneke

I wouldn't say that this is true. In B decays, we discuss many more challenging observables than just

branching fractions of tree-dominated decays; such as penguin-dominated decays, CP asymmetries, and strong phases.

Rahul Sinha

Let us just go back to branching ratios for modes like $K\pi$, $\pi\pi$ and ... These things work so well in D decays, but not that well in B decays.

Hsiang-nan Li

But I think this question does not belong to this category. I think it's still too early to have any concrete conclusion because currently the theoretical precision is just up to next-to-leading order, right? So there is next-to-next-to-leading order, next-to-next-to-next to leading order. There is a long way to go.

3. Question: What are the connections between observations in the quark and lepton sectors?

Jeff Appel

This is pretty technical for the round-table level of discussion. I guess I'd like to move on to our next question. I don't have a lot of questions. Don't get too scared. I wonder about the connection between the flavor observations in the quark and lepton sectors. Do we understand these? Or, do we have to wait to get to Planck scale to figure it out.

Choong Sun Kim

The $\sin(\theta_{12})$ in neutrino-sector mixing and $\sin(\theta_{12})$ in the quark sector, now adding up those 2 mixing angles comes up to about 45 degrees. It could be an accident. Or maybe there is some kind of connection between the quark sector and the lepton sector. People say it's complementarity, something like that. Quark-lepton complementarity. Maybe there is some reason behind it, or is it an accident?

4. Question: Is there a flavor-physics community, and if so, has it articulated its case well enough?

Jeff Appel

One reason why I put this question in here is to address the nature of this conference and our community. I use the singular form, our community, the flavor-physics community which covers quarks and leptons. This is the physics we have discussed at FPCP 2008. Have quarks and leptons been brought together at this meeting more strongly than in the past because of CP violation only, or there is something more fundamental that makes them part of the same community? And if so, has this community articulated the case for support of both axes strongly enough? I am thinking of the priorities that have been expressed in the United Kingdom and in the United States. We

also have heard about the delay in kaon physics at J-PARC, and so on.

Taku Yamanaka

Well, let me first speak about the situation in Japan. The High Energy Physics Committee in Japan, of which I am also a member, wrote up a report on what to do in the future. In that report, we stated two things. One is, approach the high energy frontier, including LHC and ILC etc. We also stated that the intensity frontier, especially flavor physics, is important. This is especially true because in Japan we have Belle and the neutrino program. The experiments are very popular and are being supported. J-PARC is the key facility for neutrino and kaon experiments. Even the people pushing for the ILC are supporting the J-PARC program, because if J-PARC fails, then there is no linear collider. From the viewpoint of the funding agency, that's very clear.

If the question is, is there a flavor-physics community in Japan, the answer is yes. The people working on kaons, B physics, and neutrino physics, experimentalists and theorists, have joined forces and won a "Grant-in-Aid for Scientific Research on Priority Areas", titled "New Developments of Flavor Physics". The project is supported for 6 years, and the fund is being used for building the T2K and Opera experiments, the J-PARC kaon experiment, B physics at CDF, and Belle. We get together every year to have a small workshop to present all the new findings. This is really making a close community of people ranging from young students to older professors working on various experiments and theories, all on flavor physics.

Martin Beneke

It may be unpopular to say this, but talking to people outside and even within the flavour physics community, one may get an impression that flavor physicists had their chance to find new physics. They did not, so it is time to move on to the next thing - LHC physics. If something shows up there, then we can go back to flavor physics to try to sort things out. We may be blamed ourselves for that because we have been talking too much about new physics and obscure 2σ effects, and didn't succeed to create interest in the intrinsic physics itself, in the phenomena.

I am fascinated and mystified how neutrino physics is succeeding in this respect - measuring a mass matrix in the lepton sector, which is after all not so different from measuring the CKM matrix. And there is even less prospect of discovering new physics by determining θ_{13} than there is in V_{ub} !

Jeff Appel

There is an interesting corollary to the way you put it. In terms of selling the physics these days, one tries to sell physics as "paradigm-changing" discoveries. What is the argument you would make to sell our physics, whatever it is? The first thing one looks for is what people call paradigm-changing discoveries, right? How would you sell the physics that you are

talking about in the world?

Martin Beneke

Once neutrinos have masses, there is no paradigm change in measuring mixing angles or even CP violation. Nevertheless, there is some intrinsic interest in investigating neutrino properties, because neutrinos are considered mysterious, while quarks are not. In any case, returning to your question, advertising paradigm change is dangerous, since paradigms usually change by observations that come unexpectedly, not because of a systematic search.

Jeff Appel

Martin, that's in fact exactly right. The neutrinos are interesting because we were surprised. And it's more a matter of surprise which sells newspapers, rather than continuing to observe the things we expected to observe, the so called standard model. That doesn't sell newspapers. The articles we read about 600 physicists "failing" to find this, "failing" to find that. We have a problem selling the more precision measurement and standard things.

Bruce Yabsley

You asked how do you sell something more subtle when someone else is pushing the paradigm-changing. The answer is: it's damn hard! If you send your children to their grandparents, and the grandparents feed them candy, they are attacking kids' weakness. I am sorry. We always push that this would change the world, whatever. Think of all that has been happening in spectroscopy, some of the most interesting stuff to come up in the B factory. I don't believe that it's nuclear physics.

Jeff Appel

And it's interesting because it was a surprise? Or interesting for another reason?

Bruce Yabsley

Again, it's interesting because it's a surprise. Now, if we get into a position where we discover something that is both surprising and interesting! Maybe we have to spend a few years in training on how to talk to guys from the newspapers. Maybe we just do.

David Kirkby

Maybe one way to answer your question is to look at the nuclear physics community because they are, at least in the US, well funded; and what they are doing is not so different from spectroscopy in heavy quarks.

Rahul Sinha

The fact is that we initially set up the B factories to test the CKM hypothesis. We have succeeded; we have done that. We have not only succeeded in doing that, but we have learned a lot more. We have new resonances and many puzzles about them. This is at the very least "surprising". So in that sense, there is no way to say that we have not actually had very good physics output. Somehow, B physics efforts have become the victim of various constraints dictating the directions in physics, e.g. our desire to find a way probe the Planck scale as fast as possible.

Eli Rosenberg

Let me say something that has already been said. The first slide you put up there. It all had to do with where new physics is going to be found. You already brought in the concept that to sell anything, it has to be something new. And on your second slide, the reaction to what happens in the Tevatron and LHC. God help this field if nothing is found in those places. This conversation becomes entirely irrelevant simply because we have oversold the idea that we have to find something new. Now I have a feeling that if we went back to 30, 40 or 50 years ago, when particle physics was a virgin, people were working on precision measurements of electromagnetic interactions. We must have felt exactly the same way you are feeling in this room now - that somehow we were undervalued by looking at things where you could make precise measurements. And the real argument is, we are working in the area where you can make precise measurements, where you can look for new things like lepton flavor violation. We'd like to measure D mixing because we didn't expect to see much of it. It's interesting, and it has intrinsic interest of its own, period. Whether it's going to be something new or not, that is a different issue. Now, how you sell that to our funding agencies is where the problem seems to come in. The same thing happens in the K meson sector. The K meson sector had a resurgence at one point after being pushed down for a long time. So this has been a continuing problem. But I think part of the problem is that we have gotten so big and so expensive that we oversell everything. The field as a whole has oversold everything. This is what you have to do. That's why you read the headline about 600 physicists failing to find this or that; because we said we were going to find it. You know, we sold the SSC as if we could do everything except cure baldness. So I think we just have a PR-reality problem about what science is about.

(George) Wei-Shu Hou

I would like to make several remarks touching on all that has been said. I think that on neutrino physics, I held back on one question that I used to ask. If I take V_{ub} , it's very hard to extract, correct? But if we take the V_{ub} analogy, because neutrino people have had ten spectacular years, this is in part because of the very large mixing angles. They could not ordain that, right? So if I take V_{ub} or even V_{cb} , our θ_{13} or θ_{23} for neutrinos, I don't see a program yet to measure something of that strength for θ_{13} . They are entering a hard time. Without that (a large θ_{13}), forget about CP violation in the lepton sector. OK, Majorana neutrinos, (neutrinoless) double beta decay, there is always some discovery potential. But they are not really doing better than we are. I don't know how, in the last ten minutes, we entered such a very gloomy mood. I think we actually have a good situation. The LHC is starting. The Tevatron is still working very hard. We are seeing things here and there. We are,

of course, used to seeing things disappear from before our eyes. But that's how it is; right? Seeing something emerge and then disappear; hoping that one of them is true. And I think this mixing-dependent CP violation phase (in B_s) is of course the way to go. But, maybe we are overselling it. It may be a PR problem, but we do have genuine indications, not just challenges. So I like what Enrico said, I think at LHC, ATLAS and CMS analyses will find the scale. The New Particles will likely be extrinsic to flavor. However there is also LHCb, right? I guess we go back to Question 2. What if we see nothing beyond the Standard Model? Maybe we see the Higgs, maybe we don't. But if we see nothing, and LHCb will measure $\sin(2\phi_s)$ to plus or minus 4 percent, no charged Higgs, no SUSY. Then no matter what PR we do, you can not get the next big machine.

But I am optimistic about both LHC proper, the high energy frontier, and LHCb also. I am also a full supporter of the Super KEKB or Super B -factory. Because it's really a PR question again. A Super B -factory is a multi-purpose facility. And speaking from Asia, I am even more supportive of this. Asia is rising. It has the population, etc. I would fully support it even just only on that account. That it is a project to work on, to go forward. And if not a discovery at this stage, then there will be a discovery at the next stage. To me, Super KEKB is a regional cooperation concern.

Jeff Appel

In order to move to a more positive direction, perhaps there are other questions people would like to address to the panel, or to each other before I get to my last question?

Bruce Yabsley

Just inspired by the previous discussion, I would note that when the LHC turns on, the field is going to undergo a kind of basic change. The kind of information we are using to decide what studies to do, and how to do it, is going to change. Now, the moment someone puts a preprint on hep-ph, everyone at the B -factories, drops everything they are doing to pursue the suggested analysis, or something like this. What's going to happen now is that we are going to get some sign of a particular mass scale. And that's going to have an influence on the things we should be studying. But the influence is going to be a hundred percent, because it's going to determine what the new physics is, presumably, and more than one model will be possible. Here my question is: do we have a mechanism so that we have a while to think about how we are going to be influenced by the new information. Or, are we going to be driven by some prejudice that what we see, is what we know, and so suddenly everyone rushes in particular directions like ten-year-olds playing soccer. The situation really is going to change. I am wondering if we thought forward to what happens when we see data.

Jeff Appel

I think there is plenty of evidence that the community is a very good at rushing together in singular directions, in effect, ten-year-olds playing soccer as you called it. We also did this when the J/ψ was discovered. Every experiment asked if they could see evidence of that signal.

Enrico Lunghi

I would like to make a comment on the impossibility of a null result at the LHC. In fact, unitarity tells us that either we'll find at least one Higgs particle. Otherwise, some other phenomena have to happen (e.g. strongly-interacting vector bosons, new strong resonances, ...). In any case, even if just a SM-like Higgs is found, we still need a linear collider to study its properties. I don't think that we can really go there and see nothing.

Eli Rosenberg

Because you are so convinced, and we have been told that we will. And if we see nothing, it is the most fascinating physics result of all - except that it will kill the field. Aside from that, ... I rest my case. Then why do we have to build it? Because we are going to see something.

Choong Sun Kim

I have one question not related to politics or anything like that. We know that George proposes a fourth generation. But if there is a fourth generation, it is supposed to violate unitarity in three generations because, effectively, the 3 by 3 part of the larger 4 by 4 matrix is non-unitary. OK. What I want to ask is another thing, about gamma in the unitary triangle. The gamma or alpha measurement is not actually measuring gamma or alpha. It's beta plus gamma, for example. So my question is, is it possible that LHCb, or Super-B, or any future B-factory can find the 3 by 3 CKM matrix non-unitary? Is it possible to find non-unitarity or not?

Jeff Appel

Yes, anybody working in B factories would say yes, you can find the triangle does not close. It is not unitary and there is going to be something else, some new physics.

Chung-Sun Kim

The measurement of gamma or alpha is from the $\pi - (\alpha + \beta)$. So by definition, you are just taking the angles as from a triangle.

Jeff Appel

The sides have to work too, right?

Eli Rosenberg

Gamma, perhaps from a Dalitz analysis, maybe from the $\Upsilon(5S)$. Does that beta plus gamma match the beta plus gamma you get when they interfere? That's the test. And that's equivalent to test unitarity; whether the standard model is working. That's what is, in short hand, called alpha. I agree with you. Nobody measures alpha, but measures $\pi - (\beta + \gamma)$.

Rahul Sinha

The one thing to note is that γ , or one of the angles measured, has to be from outside of the B_d , to see a breakdown of unitarity. It is well known that if you measure the three phases using just B_d , the effect of new physics will cancel out. In addition, the other thing one can do is to measure both sides and the angle and then check if the triangle closes.

Jeff Appel

The same triangle.

Choong Sun Kim

The measurement of γ or α is from $\pi - (\alpha + \beta)$ or $\pi - (\beta + \gamma)$. So, by definition, you are just forcing a triangle if you do not measure the 3 angles independently. Also, beta from $B \rightarrow J/\psi K_s$ can effectively include new physics, too.

Rahul Sinha

Yes. But, you can also measure gamma, outside the B_d system. There is a method to measure it using $B_s \rightarrow DK$. If you do that, then there is no problem. You can detect the breakdown of unitarity without measuring the side.

Jose Ocariz

Another way of saying it is that you are measur-

ing 4 parameters with 10 observables. If you have no consistency, you have no unitarity.

5. A final question.

Jeff Appel

We are reaching the end of our scheduled time. I do want end on one new question, our last question, which is "How can we thank our hosts enough for their hospitality, the careful and caring organization of this meeting, their holding back the worst of the rains for the excursion, and clearing the sky as well for the highest level of FPCP banquet ever held? So, I think we should close this session and thank our hosts very much.

(applause)

And I personally want to thank the panelists and all of you in the audience for the very stimulating discussion. Good luck on your trips home, whether near or far. And, again, thank you all.