Summary and Outlook for KEKTC5 *

Helen Quinn

Stanford Linear Accelerator Center Stanford University, Stanford, California 94309

Abstract

This is the summary talk of a meeting held at the KEK, Tsukuba, Japan, November 20–22, 2001. I give a brief summary of the talks we heard, and a few comments of my own on various points that have been made. Other talks given at this meeting are referred to by the speakers last name; these talks will appear in the same proceedings. See these talks for the detailed references for each subject.

Talk presented at KEK Topical Conference 5 KEK Tsukuba, Japan November 20-22, 2001

^{*}Work supported by the Department of Energy, contract DE-AC03-76SF00515.

1 INTRODUCTION

This meeting has been a wonderful testimony to the success of particle physics in Japan. Our hosts are involved in all aspects of flavor physics, both with the worldclass facilities in this laboratory and elsewhere in this country, and through their participation in experiments elsewhere. The results they have shown us these past few days are at the forefront of B physics and neutrino physics, and the plans for the future we have seen are equally impressive. It is particularly sad that all this success and enthusiasm should be overshadowed, at least for the moment, by the terrible accident to the SuperK detector a week or so ago. The sudden loss of so much of this beautiful piece of equipment was a shock to the particle physics community wordwide, and a particular loss to the next few years of neutrino physics research. We have heard that there are already plans for recovery being formulated. The hope is to have the K2K experiment up and running in about one year with about half the original number of phototubes in the detector, which will, most probably, be rebuilt to its full strength within about five years. I am sure everyone here joins me in expressing our sympathy to everyone involved in the SuperK and K2K effort, our hope that this recovery can be achieved, and our expectation that we will once again learn interesting things from this program once that is done.

I turn now to my summary of the program. We had one day on B physics, one on neutrino physics and today has covered a range of other flavor physics and some future plans.

2 B PHYSICS

I cannot resist the temptation to begin with a little reminiscing on the phenomenal progress this field has seen. Twenty years ago when Tony Sanda and his collaborators [1] first discussed the interesting possibility of large CP violations in the decays of neutral B mesons I remember that I thought "that's very interesting, but you will never be able to measure it". However, sometimes we are lucky. Approximately 18 years ago we discovered experimentally that the B lifetime is relatively $\log [2]$, and it is that fact that makes all current B physics experiments feasible. We had no reason to expect that this would be so. Indeed the pattern of the masses and mixings of the quarks that determine this fact is by now well-described, but in no sense understood or predicted. (It now appears we have a second such pattern to decipher, that for the neutrinos, and from the little we now know it looks quite different. But that is for the second day of the meeting.) Once we knew the B lifetimes, plans for B physics experiments could begin to take shape, and indeed did so. Today over 1000 physicists world-wide are actively engaged in this research. Both the KEKB and the PEPII Bfactories are working extremely well; their detectors Belle and BaBar are efficiently and reliably recording data. We are beginning to see many beautiful results. I want to remark that, as a theorist watching the development of these projects, I have been extremely impressed by the ingenuity and the tenacity of both teams, and by the way they have shared information and learned from one another at essentially every stage of the game.

Now, turning to my real job, the stage for this day was beautifully set by the talk by Falk, who stressed that an ongoing program of B physics must be pursued, producing many measurements which are redundant in the Standard Model so that consistency checks can probe for new physics contributions. He also addressed the issue of hadronic effects and their ability to cloud the relationship between measured

quantities and underlying theory parameters. He advocated that this fact too argues for multiple approaches to the measurement of each parameter. I want to distinguish between multiple tests that measure the same underlying quantity independent of whether we extend the theory (*e.g.* inclusive and exclusive rates for radiative decays) and multiple measurements that measure the same Standard Model parameters but are unrelated effects in a more general class of theories. We can use the first to gain understanding of how well we can control theoretical uncertainties, and the latter to test for new physics, but only if the new physics effects are large compared to the theoretical uncertainties. Eventually this murky situation will resolve itself at some level. Falk's conclusion was that we will typically need new physics effects that are of the same order of magnitude as the Standard Model contributions before we will be able to convince ourselves that these are really new effects.

In his later talk on possible future upgrades of KEKB Yamauchi defined two phases in the B physics CP violation program. Phase 1, discovery, has been achieved [3]. We heard from Sakai for Belle and Mazzoni for BaBar a review of the results that the experiments released last summer (with some minor updates.) Not only are there measurements that show definite evidence of CP violation, but also each of these experiments has produced B lifetime and mass-difference measurements competitive with prior world averages. These talks also showed us that the work is ongoing; new results from larger data samples are to be expected next spring or summer. Additional measurements, such as the addition of new modes to the Belle mass difference analysis and limits on the CP violating parameters in the two pion modes from the BaBar collaboration, show that there is a healthy ongoing program of analyses of the timedependent decay rates in both experiments. Phase 2, a detailed study, is also well begun. We heard a number of new results on branching ratios. Yamamoto presented Belle results for many modes, and likewise Willocq for Babar. New analyses are appearing regularly from both teams and they are pushing branching ratio sensitivities as low as a few times 10^{-6} in some channels [4]. The interplay of this rich resource of data with theoretical calculations is beginning to be interesting, and will continue to be so for some years to come I expect.

We then heard from Bornheim on CLEO results, where the more mature analysis stream is indeed beginning to be guided by more theory. A good example is the moment analysis to extract heavy quark theory parameters from and $b \rightarrow s\gamma$ decay spectrum and use them in the analysis of $B \to X_u l \nu$ decay to improve the accuracy of the extraction of V_{ub} [5]. This is very nice work, I am sure we will see more of such methods to reduce theoretical uncertainty by combining the analysis of multiple data sets. One caution however is to remember that not all theoretical assumptions are removed by this method, in particular there remains the issue of corrections to the quark-hadron-duality assumption. One test of that would be to use an even larger, redundant set of moments and test for the consistency between them as a check that the duality assumption is working. (This may get a little tricky, first because it is experimentally more and more difficult to do, and second because higher moments are effectively more local quantities, and so are progressively more vulnerable to duality violation effects, but it is worth some study.) This talk also presented the program for CLEO C, as a facility for τ , charm and QCD physics. This would provide, among other things, some needed inputs for various B physics analyses that look at specific D decays, and a realm to test some lattice calculations in D decays as a cross check on their application in B decays. This path offers a reasonable future for CLEO in a period when B physics will clearly be dominated by data from Belle and BaBar and eventually also the TeVatron detectors.

The next talk, by Palla, reviewed B physics results from LEP and SLC. While the data is not new, the analyses are continuing to be refined. The major effort that we heard about here was the ongoing improvement of the bounds on Δm_s , the mass difference between the two B_s eigenstates. The combined results now set a bounds at 95% confidence level of greater than 14.6 ps^{-1} for the oscillation frequency. We even see a maximum in the log likelihood plot at around 17.4 ps^{-1} but with only about 2.6 σ of significance. These results can perhaps be pushed a little further we were told, but it remains to be seen whether the result remains a bound, or translates into a measurement of a value for this important quantity. If not we will all eagerly await results on this quantity form CDF in the TeVatron Run II.

Indeed that was the topic of the next talk, by Papadimitriou, namely what the TeVatron can do in B physics with the upgraded detectors in Run II. We heard an updated number for $\sin(2\beta)$ based on an improved tagging analysis of Run I data, and prospects for many new results, particularly on the B_s modes, which are inaccessible to Belle and BaBar. If Δm_s is in the range now expected from Standard Model theory we can look forward to a measurement with 5 σ significance before then end of run II. Many new B_d mode measurements will be possible as well, not to mention B_c and Λ_b decays. There is much to be learned in these experiments about doing B physics in a high-luminosity hadron environment; it is likely this will be a productive source of results in the future.

Turning to theory we heard two talks on calculating two-body exclusive hadronic B decays. I want to take a little time here because this topic seems to be in a very confusing state. We have two groups doing what looks superficially as if it is the same calculation, but getting quite different results. The work is a technical tour-de*force*, but clearly, we need to understand why the results differ. This is not a simple situation where we can say one is right and the other wrong, but rather turns out to be a matter of differing input assumptions. Let me try to explain what I perceive to be the differences. Both groups start with a formal expansion in powers of Λ_{QCD}/m_b and $\alpha_s(m_b)$. However even the power counting of powers of m_b for one diagram relative to another depends on what is assumed about the end-point behavior of the hadron quark distributions and transition matrix elements. The differences arise from the assumptions made on this point. The major difference is whether or not to include the terms known as Sudakov suppression factors, which arise from soft and collinear gluon radiation and, in the true $m_b \to \infty$ limit are known to modulate this end-point behavior by a double logarithm of the transverse momentum of the quark in a hadron (in the light-cone frame). This factor is scaled by some QCD-determined scale (which we can always write Λ_{OCD}) but with a numerical coefficient that has some arbitrariness).

The BBNS group [6], whose work was beautifully explained here by Martin Beneke, try to keep the rigorous part of the calculation and the part that requires some model input as separate as possible. They frame the calculation with a minimal set of assumptions. This results in expressions which contain unknown functions for the Bto one meson transition matrix as well as for the quark distribution functions within both the B and the final state mesons. They then input both data from semileptonic decays and a range of assumed forms for these functions to get numerical results. They do not include any Sudakov behavior in the quark distributions. They argue that, at the physical value of m_b , this factor is not meaningful, it is subsumed in the higher-order corrections. Their power counting assumes that the quark distribution vanishes at least as fast as a single power of (1 - x) as $x \to 1$. This then allows them to characterize the contributions and gives a systematic approach to the calculation of the leading term in the $1/m_b$ expansion. The calculation requires some input as explained above. The next order term in $1/m_b$ must be estimated as well. There are some contributions to this term which may be large because they are chirally enhanced, which is the jargon for a term where the suppression factor of Λ_{QCD}/m_b is multiplied by a factor such as $(m_{\rm meson}^2)/(m_u + m_d)^2$. For pseudoscalar mesons (the case for which the calculations have been most pursued) such factors are large. Hence it turns out that the terms formally of order $1/m_b$ play a numerically significant role in the final result. The group estimates these terms, and also estimates the possible range of values given what they feel are reasonable ranges for the input quantities that play a role in their estimates. This is a somewhat subjective exercise. The fact remains it is an essential step in this work, without it the predictions are meaningless. If the results fall within the predicted ranges then we will gain some confidence in these estimates.

The alternative approach is known as pQCD, the leading exponents are Sanda, Li and Keum [7], was represented by the talk of Li. Li explained how their assumed Sudakov factor gives an evolution of the input quark distribution functions to the extreme $k_{perp}^2 = m_b \Lambda_{QCD}$ scale, at which point they link to the perturbatively calculated hard-scattering kernel. This assumption removes the contribution with no hard gluon exchange to the spectator quark from the leading behavior. The approach is then to choose a parameterization of the quark distributions for the B-meson, and to fix the parameter that appears in this assumed form so that one gets a B to π transition amplitude, with the one hard gluon exchange, that gives the correct exclusive semileptonic decay rate. Hence, while both groups use the measured semileptonic decay amplitude as an input, it plays a rather different role in the two cases. In this second method it requires a rather large probability for a high-transverse-momentum (or short-distance) component of the B wave function to overcome the order α_s suppression of the hard gluon exchange. (In contrast in the BBNS method it is assumed that an order 1 contribution, with no hard gluon exchange is the dominant part of the B to π transition.) The assumed distribution functions in the Sanda, Li and Keum method then have other consequences, in particular they lead to an enhancement of the formally (Λ_{QCD}/m_b) -suppressed annihilation graphs, and of certain penguin graph contributions. This then gives much larger predicted strong phases than the BBNS estimates. Sanda, Li and Keum have not yet published estimated ranges for their numerical results. It appears these may be very sensitive to details of their input assumptions and parameters. Others authors yet [8] take the tack of parameterizing all Λ_{QCD}/m_b corrections, but then we truly lose predictive power.

Unfortunately what all this long story comes down to is the fact that, at least in the two-pseudoscalar decay case, the Λ/m_b terms are not unimportant, and the results obtained are sensitive to the detailed assumptions made. If the strong phases turn out to be as large as the pQCD group predicts I fear it is bad news, for it then means the unknown physics of the quark distribution end points is really dominating the result. In that case, I do not think we will ever gain good control of theory uncertainties. If the BBNS numbers are found to be correct perhaps there is some better hope for eventual predictive power here. It is true that here too, in the two pseudoscalar case, the chiral enhancement, and sensitivity to the end-point behavior of the wave function makes non-leading terms important, and so limits the precision of the prediction. Eventually we need to study many modes. We will begin to accept that the theory uncertainties are under control only when a clear pattern of good predictions is seen. This, unfortunately, makes the hunt for non-Standard Model effects a difficult game.

However, to end this story on an optimistic note, I should remind you how much progress these calculations represent compared to the state of the art a few years ago. Now we are arguing about the role of the next leading term, before we had only the crude first approximation known as factorization. In the end, the theory uncertainties will never be completely removed, so we will probably never be able to see new physics effects in these two-body hadronic modes if they turn out to be only a few percent correction to the Standard Model. I am a bit more optimistic that Falk, I would guess that new physics effects of about 50% of Standard Model will be readily recognized. With some luck, and with continuing hard work by theorists, we may eventually be able to achieve stable predictions at this level, or even do a little better.

The next talk, by Onogi for the JLQCD collaboration, detailed progress made, and expected, in a different area of theory relevant to B decays, that of lattice calculation of matrix elements. Lattice calculations of physical 1-to-2 particle decay amplitudes are still some way off. The fact that the calculations are done in the Euclidean region means that an extrapolation to Minkowski space is needed to determine final state phases. It does not look likely that step will be easily achieved. However there are many important one particle to one particle, or one to zero, transition amplitudes or matrix elements that can be calculated. The prospects for high accuracy results for these are steadily improving. Progress requires unquenched calculations and the capacity to deal well with both the heavy and the light quarks. Quenching, that is the neglect of all non-valence quark effects (or quark-loops), is an approximation that greatly reduces computation time at the price of introducing a poorly quantified error in the calculation. Unquenched calculations, those that do not make this approximation, are now beginning to appear for the relevant quantities. However they are still not made with physical light quark masses so some work will be needed to find the right extrapolations to the physical values. This work is informed by chiral limit calculations for the lightest quarks, calculations which show there is some logarithmic dependence on these masses to be dealt with. This is a subject in its infancy and so there are still significant theoretical uncertainties associated with this extrapolation, but the expectation is that these can eventually be well-controlled. Recent advances include a better method for dealing with the heavy quarks so a large extrapolation in that mass is no longer needed. The combined matching to heavy quark theory at the one end of the quark mass scale, and to chiral effective field theory at the other, has already yielded lattice results for quantities such as f_B and B_B that are estimated to be reliable at the 10% level. Improvements from unquenched calculations which include all these features can be expected, but few lattice theorists are willing to place bets on what eventual accuracy can be reached, or when.

We next heard a talk from Nir who told us that new physics effects seem to be inevitable if we wish to explain both the matter-antimatter asymmetry of the Universe and the observed flavor structures of the quark and neutrino sectors. He highlighted decays that are rare or forbidden in the Standard Model as a particularly good window to search for new physics effects. Whether the new physics will yield detectable effects on B physics experiments depends on the theory. In particular he categorized SUSY theories by the degree of universality of their mass spectra, as universal, partially universal, or not universal. He then showed that the size of new CP-violating and flavor-symmetry violating effects depends on the degree of universality, reaching the conclusion that we will learn something about the possible classes of SUSY theories regardless of whether we do or do not see discrepancies with Standard Model predictions in B decays. This is a way to say that a result that fits the Standard Model limits the possible additional physics just as much as a result (of the same precision) that does not. Of course, we will all be much more excited if we find a result that does not fit the theory, but it is well to remember that we learn much from these experiments even when such dramatic results are not found.

Our rich day of B physics ended with a look to the future. Schneider told us of plans for b experiments at hadron colliders, including eventually BTeV and LHCb. Yamauchi described a program of upgrades of KEKB to reach a luminosity of 10^{35} . I can only remark that the ingenuity and persistence of the machine and experimental physicists continues to impress me. The challenges are large, but the ideas to overcome them continue to appear. Given the fiscal resources to continue, the B physics program has a long and physics-rich future still ahead.

3 NEUTRINO PHYSICS

The second day of our program devoted to neutrino physics was kicked off by an excellent and comprehensive review of the current status of the subject by Lisi. Neutrino physics is another area where years of work have payed off with very interesting results in the past year or so. Kamiokande and SuperK played a leading role in establishing this subject as an active area of experimental science and in contributing some of the key measurements. This makes the recent SuperK accident a sad setback, not just for Japanese physics, but for the world wide neutrino physics community. The topic of interest is of course the pattern of neutrino masses and mixing. Current evidence strongly supports the conclusion that neutrinos do indeed have masses, and that the mixing patterns in the neutrino sector, whatever they may be, do not closely mirror the patterns in the quark sector. Eventually the topic of CP violation in this sector will be one we would like to explore. If present indications that the third mixing angle is quite small are correct, this may be a long way off.

Talks on solar neutrino results from SuperK by Guillian and SNO by Helmer repeated the recently announced results that show a discrepancy between the flux measured in the two experiments. This provides the most direct evidence yet for neutrino oscillation as SuperK measures a combined flux with contributions from all flavors while SNO is sensitive only to electron neutrinos. Since the sun produces electron neutrinos any mismatch of the two fluxes can only be explained by oscillation, that is by neutrino masses and a mismatch between the mass eigenstates and the flavor eigenstates. There is more to come from SNO, we look forward to results soon on neutral currents and the day/night variation (or lack of it) and in the longer future to an improved neutral current result from the salt added to the detector to increase its sensitivity to this effect.

I cannot refrain from observing what a victory these results are for John Bahcall and his collaborators. The combined world neutrino results fit well with the solar neutrino flux that his calculations have predicted for years! They confirm his original interpretation [9] of the anomaly in the pioneering neutrino experiments of Davis and collaborators [10].

We then heard from Suekane on the plans for the Kamland experiment which will be sensitive to fluxes from a number of reactors around Japan (and even in China). Flux predictions are critical for this experiment, but prior reactor experiments with short baseline have seen the predicted fluxes, thereby verifying the validity of these calculations. The range of base-line distances for this experiment will provide very interesting constraints. If the large mixing angle solutions (favored somewhat by present data) are correct, then this experiment will quickly achieve a significant signal.

We then heard a summary of shorter baseline experiments given by Link. The challenge here is to confirm or exclude the results of the LSND experiment which reported 3.3 σ significance for an oscillation corresponding to a neutrino mass difference

of order 1 eV, with small mixing angle [11]. This result cannot be accommodated with the solar and atmospheric results within a three neutrino scenario. As Lisi explained earlier, adding a fourth, sterile neutrino flavor is also disfavored as either one or the other of the solar and atmospheric effects would have to include a significant mixing to this fourth flavor, but both sets of results disfavor any significant sterile neutrino contribution. The Karmen experiment has excluded much of the range of parameters allowed by the LSND result. MiniBoone, observing a neutrino beam from Fermilab with both a near and a far detector, is sensitive to the rest of the range, so within a couple of years should be able to either confirm or exclude the LSND result. If the result is confirmed we will have a real puzzle on our hands; this is why most of us adopt a "wait and see" attitude, with something of a prejudice that the LSND result will not be confirmed.

Next we heard another lovely theory review talk form Murayama, on the topic of the connections between CP violation and the various possible baryogenesis or leptogenesis scenarios. As he told us, with the current Higgs mass limit a pure Standard Model picture for electroweak-scale baryogenesis is ruled out. The theory does not even exhibit a first order phase transition. He also concluded that the minimal supersymmetric Standard Model could still possibly give a satisfactory picture of electroweak baryogenesis, but he considered it a long shot. The parameters are pushed to a limited and somewhat uncomfortable region, which could be excluded by small improvements in the limit on electric dipole moment of the neutron or by observations of B_s mixing. He then described some leptogenesis scenarios that can also work, at least with current constraints.

Murayama also gave us a brief summary of the status of grand unified theories. Limits on proton decay long since ruled out the simplest SU(5) theory; more recent SuperK limits have now excluded the simplest (MSSM) SUSY generalization of this theory. However many non-minimal SUSY GUT theories can evade these constraints. Further proton decay searches (to which I add: with improved sensitivity to kaoncontaining modes as well as pion-containing modes) and further searches for various possible lepton-flavor-symmetry violating effects (large compared to those arising from neutrino masses and mixings) will provide tests of GUT ideas.

Next we heard from Aoki a nice review of simulations to study the projected sensitivities to neutrino mass effects, and in particular to CP violation for JHF. The sensitivity depends on the size of the third neutrino mixing angle. She reported that the long-term reach (Phase II of JHF with very intense beams) could possibly see CP violating effects for θ_{13} as small as 0.04. This is a tough business, requiring, as in the B decay sector, a set of "redundant" measurements to provide the necessary cross-checks and pin down the effect. She identified some possible "smoking gun" signatures including direct CP violations in flavor-violating decays and anomalous energy-dependence. A talk later in the day by T. Kobayashi gave us further details of the plans for this experimental program, and explained the two phases. The first phase will use the (rebuilt) SuperK detector while the ambitious second phase requires "HyperK", which is a compartmented detector with approximately twenty times larger total volume than SuperK. This detector would give a better reach for seeing CP violation effects, and would also give improved reach for proton decay over its expected 20 year running life. Aoki's talk was followed by a discussion by Sato of long-baseline μ^- decay to μ^+ appearance searches as a way to test for new physics.

Turning to atmospheric neutrinos, Miura reviewed the SuperK observations and added a discussion of initial K2K results. The ratio of μ -type to *e*-type neutrinos observed at SuperK shows clear deviations from the no-mixing pattern, with upward going μ -type neutrinos depleted during their transit through the earth, but no corre-

sponding enhancement of the electron-type fluxes. This suggests a μ to τ oscillation. The early K2K comparison of near and far detector fluxes sees 44 events at the far detector where the expected number, based on the near-detector flux, is 63.9 (+6.1 or -6.6). A little more running to improve the statistical significance of this result would be very good, but now unfortunately will be delayed until SuperK is rebuilt, albeit with a smaller number of phototubes. The hope is that this can be accomplished in about one year.

Our day on neutrino experiments was rounded out by two talks on future acceleratorto-remote-detector neutrino experiments planned or proposed. Saakyan told us of plans for Minos which will look for ν_e appearance in the Soudan mine starting with a ν_{μ} beam at Fermilab, and also use the measured neutral current spectrum to put limits on the mixing to sterile neutrinos. Nakamura told us about the status of OPERA and ICARUS, two proposed experiments for the Gran Sasso underground laboratory both aimed at seeing ν_{τ} appearance from a CERN-based ν_{μ} beam. Each of these experiments has challenges, their eventual sensitivity goals are similar, about a dozen events over a five year period if the large mixing angle fit to the atmospheric neutrino results is correct. OPERA is an approved experiment, using an emulsion scanning technique similar to that of DONUT, but its schedule is uncertain. Scanning rates must be improved to achieve interesting sensitivity; new equipment now arriving in Nagoya should give a factor 20 improvement in scanning rate. Meanwhile the ICARUS experiment must pass a number of tests, both of safety and of efficacy, before it can be approved. A test module has been built and is operating in a surface laboratory in Pavia.

4 OTHER FLAVOR PHYSICS

In the final day of this meeting we visited a number of topics. A major theme of the day was tests of lepton-flavor symmetry breaking. In the Standard Model such effects are small even with neutrino mixing effects included, so these experiments provide a window for new physics effects which one can hope is quite clean. We also heard a number of talks on flavor physics in the charm and strange sectors, where very rare modes or small effects could give windows for new physics effects, provided we can control the hadronic uncertainties in the Standard Model predictions. As recent developments in the saga of the muon g-2 show, this proviso also applies in the lepton flavor sector once we reach high enough precision to be sensitive to hadron-loop effects.

We heard from Hisano how SUSY models are constrained by the combination of $\mu \rightarrow e\gamma$ searches and the muon g-2 measurement. This talk reminded us that already model builders have to consider strong constraints on additional flavor-symmetry-violating effects, both in the quark and the lepton sectors. However their ingenuity is quite up to the task, and there are many models that are still viable.

We then heard about proposed lepton flavor violation experiments, Molzon told us the status of MECO, the muon conversion experiment at Brookhaven and Mori reviewed a μ to $e\gamma$ experiment under construction at PSI. MECO, while approved, is not yet funded, but work on magnet design is proceeding. Its sensitivity will be comparable to current μ to $e\gamma$ limits, and greater for cases where the exchange particle is not a photon. The PSI experiment aims for three orders of magnitude improvement over the current limit, which will give sensitivity to some of SUSY parameter space in the next 2–5 years.

Debevec the reviewed the beautiful BNL muon g-2 experiment and the improved

precision that will be achieved when all data is analyzed. Here the uncertainties in the theory prediction are comparable to those of the current experimental result and are unlikely to get much smaller. The details of this subject are changed by the recent (post-conference) recognition of a sign error in one term of old theory estimate for g-2 [12]. With the corrected sign there is no statistically significant discrepancy between the theory and the experiment. The conclusion that the experiment constrains but does not eliminate SUSY models remains true, but there is no indication that a SUSY addition to the Standard Model is required.

Moving on to the quark sector we heard from Link about the FOCUS experiment. FOCUS has improved our knowledge of rare charm decays and performed an interesting search for $D - \overline{D}$ mixing and mass-difference effects. (BaBar and Belle are now also beginning to enter this arena.) It is a difficult game, the effects sought are small and doubly Cabibbo-suppressed decays of the D mesons complicate the analyses. However there is more to come in this arena, eventually it may yet provide a window to new physics.

Di Domenico then reported on Kloe at $Da\phi ne$. The luminosity there is a factor of 10 too low for the originally-intended CP violation tests, but studies of rare ϕ decays are proceeding and yielding some initial results. Meanwhile, as Wingerter-Seez (for NA48), Hsiung (for KTeV and future FNAL experiments), and Kettel (for BNL) and informed us, kaon physics continues, with searches for rare decays giving ever more sensitive limits (down to 10^{-12} in some cases), and even some observations. Among these the Brookhaven group has now observed a second $K^+ \to \pi^+ \nu \overline{\nu}$ event, with little background contamination. This gives a rate in the ballpark of the Standard Model, but with large enough uncertainty that there is still room for some significant new physics. The next generation searches, planned at BNL and FNAL will have sensitivity to pin this down much better, with 5 to 10 events in the next few years expected at the Standard Model rates for the Brookhaven experiment, while the proposed CKM experiment at FNAL could eventually see of order 100 events. Imazato reported on the K-physics program at KEK, with a new result on T-violation effects in $K^+ \to \pi^0 \mu^+ \nu$ and again plans for more sensitive experiments eventually using the intense JHF proton beam to produce large numbers of kaons.

The final talk of the day (aside from this summary) was from Kuno, who gave us a very rapid overview of the status and plans for JHF, or the Japan Hadron Facility. This talk outlined a long term and ambitious program of flavor physics, starting from an intense 50 GeV proton beam and going on to a possible muon collider some 30 years in the future. The initial phase of this facility is approved and moving towards construction, but the funding is limited so there will be a step-by-step approach to the desired facility.

I want to conclude by thanking our hosts for an excellent meeting and for their hospitality here. We have seen an impressive range of flavor physics, and the Japanese laboratories and physicists are active in almost all of it.

References

[1] I. I. Bigi and A. I. Sanda, Nucl. Phys. B **193**, 85 (1981).

[2] E. Fernandez *et al.*, Phys. Rev. Lett. **51**, 1022 (1983).

- [3] B. Aubert *et al.* [BaBar Collaboration], Phys. Rev. Lett. 87, 091801 (2001) [hep-ex/0107013]. K. Abe *et al.* [Belle Collaboration], Phys. Rev. Lett. 87, 091802 (2001) [hep-ex/0107061].
- [4] For a current list of results see the experiment's websites at: bsunsrv1.kek.jp/ and www.slac.stanford.edu/BFROOT/www/ Public/index.html
- [5] D. Cronin-Hennessy *et al.* [CLEO Collaboration], arXiv:hep-ex/0108033.
- [6] M. Beneke, G. Buchalla, M. Neubert and C. T. Sachrajda, Phys. Rev. Lett.
 83, 1914 (1999) [hep-ph/9905312]. M. Beneke, G. Buchalla, M. Neubert and C. T. Sachrajda, Nucl. Phys. B 606, 245 (2001) [hep-ph/0104110].
- [7] Y. Y. Keum, H. Li and A. I. Sanda, Phys. Rev. D 63, 054008 (2001) [hepph/0004173].
- [8] See for example M. Ciuchini, E. Franco, G. Martinelli, M. Pierini and L. Silvestrini, Phys. Lett. B 515, 33 (2001) [arXiv:hep-ph/0104126].
- [9] See for example J. N. Bahcall and M. H. Pinsonneault, Rev. Mod. Phys. **64**, 885 (1992) and references to earlier work contained therein.
- [10] The earliest results are in R. J. Davis, D. S. Harmer and K. C. Hoffman, Phys. Rev. Lett. 20, 1205 (1968). For a review of the history see J. N. Bahcall and R. Davis, arXiv:astro-ph/9911486.
- [11] C. Athanassopoulos et al. [LSND Collaboration], Phys. Rev. C 54, 2685 (1996) [arXiv:nucl-ex/9605001].
- [12] M. Hayakawa and T. Kinoshita, arXiv:hep-ph/0112102.