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REPORT OF THE KAON SUBCOMMITTEE OF NUCLEAR SCIENCE ADVISORY COMMITTEE

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I. INTRODUCTION AND SUMMARY

The Kaon Subcommittee was formed in response to the proposal by Canada that the U.S. enter into an international collaboration in support of the construction of a kaon "factory" in Canada. The U.S. contribution is proposed to be $75 M in $15 M installments for five years. The Subcommittee has been asked by NSAC to assist it in responding to the DOE/NSF request for advice on the following issues:

1. "The importance of the proposed physics research and its relevance to the nuclear physics program.
2. The adequacy and appropriateness of the proposed KAON facility to provide the needed experimental capability in this area of hadronic physics.
3. The impact of the KAON project on the U.S. physics program, within the context of the Long Range Plan for Nuclear Science formulated by NSAC and as adopted by the NSF and the DOE in its Program Plan of 1985.

In addition [a fourth issue], the Subcommittee should inform the full committee of its views on the scientific capabilities of potential improvements of U.S. facilities, particularly those at the Brookhaven and Los Alamos National Laboratories as well as the costs of the likely extent of U.S. participation at these facilities or at KAON.

At its discretion, the Subcommittee may wish to provide the NSAC with other information they believe to be pertinent to the charge to that committee."

The Subcommittee held three meetings, each at an interested laboratory: On October 16 and 17 at the Los Alamos National Laboratory, on December 9 at the Brookhaven National Laboratory and on January 7 and 8 at TRIUMF. These were attended by representatives of the Department of Energy and the National Science Foundation. In addition, oral presentations were made to a Subcommittee of HEPAP on January 18 and to NSAC on January 27. As indicated by the appended agenda (Appendix III) each meeting involved testimony by each of the laboratories and by user groups. The Subcommittee formulated a set of questions (See Appendix II) which were transmitted to the laboratories in time for the second, December 9 meeting. These were responded to, in the course of the December and January meeting, orally and in written form.

The Subcommittee wishes to thank the Laboratories for their warm hospitality and efficient arrangements.

A. INITIATIVES:

The Subcommittee was asked to consider three construction initiatives:

1. Los Alamos National Laboratory:

A proton accelerator complex delivering a proton current of 25 μa, *proton energy 60 GeV, duty cycle 50%*. This accelerator consists of the present LAMPF accelerator plus two new linacs delivering protons with energies 1.6 GeV and 2.2 GeV, respectively. This beam is injected into a synchrotron which accelerates the beam to 60 GeV. Compression rings take 98% of the beam at 1.6 GeV and deliver it in short pulses to a neutron and/or a neutrino facility. *Construction Cost*: \$560 M; cost to the nuclear program $\sim$ \$425 M; the remaining support from the condensed matter physics program. R&D *Costs:* \$63 *M*. *Operating Costs:* after completion \$64 M. *Completion Date;* 1998 if started by 1993. Typical experimental *detectors*: Kaon EPICS, neutrino detector, Drell–Yan multi-particle spectrometer, others. Cost $\sim$ \$80 M.

The Los Alamos National Laboratory has had a long experience in operating a world class experimental facility, using intense proton beams. The laboratory itself forms an important resource because of its many capabilities. Recently it has had considerable experience with the Proton Storage Ring.

2. TRIUMF:

A proton accelerator complex delivering a proton current of 100 μa, *proton energy 30 GeV, duty cycle 100%*. This accelerator consists of the present TRIUMF accelerator whose beam is injected into an *accumulator* which prepares the beam for injection into the *booster* which accelerates the protons to an energy of 3 GeV. That beam is prepared by the *collector* for injection into the *driver*. The latter accelerates the protons to 30 GeV. Finally, the beam enters the *extender* which permits slow extraction. *Construction costs*: \$571 M (Canadian) $\sim$ \$448 M (U.S.). R&D *costs*: \$33.2 M (Canadian) \$28.4 *M* (U.S.). A "project definition study" costing \$11 M (Canadian) is now underway. *Operating Costs:* \$90 M (Canadian) $\rightarrow$ \$77 M (U.S.) to be provided by Canada. *Completion data*: 1995. Typical experimental *detectors*: neutrino facility, polarized beam, multi-particle spectrometer, two kaon decay experiments, high resolution spectrometer for K^+-nucleus scattering, hypernucleus experiment. *Detector Costs* $\sim$ \$100 M, U.S. share $\sim$ \$30 M.

The TRIUMF laboratory has also had a long experience in operating a world class accelerator, albeit at an energy and current less than the energy and current at LAMPF. It does not have as backup a large multi-purpose laboratory but will need to go (and is going) to the physics community (*e.g.* Los Alamos, Fermilab and DESY) for additional support. KAON would be sited in a university environment with the attendant advantages for graduate students doing their research there.

The experiments done at KAON would be selected on the basis of scientific merit by an international program advisory committee. Moreover, during the construction period technical reviews by a U.S. agency would be welcome.

3. Brookhaven National Laboratory:

At the present time the "booster," an upgrade of the AGS, is under construction. It will provide a $4\mu a$ proton current at the AGS *energy* (28 GeV) with a *duty cycle of 35%*. BNL proposes the construction of a "stretcher" which would provide an $8\,\mu a$ proton current at 28 GeV and a duty cycle of 100%. The effective current gain is more than a factor of two because of the improved beam quality. It would *cost* \$50 M with a construction time of four years so that the completion data would be 1994 – 5. The "stretcher" and RHIC, the first priority Brookhaven project are mutually compatible; that is, the operation of the stretcher will not affect the RHIC operation and vice versa. Brookhaven has for several years been concerned with rare K decays, with hypernuclear spectroscopy, and is mounting a $g-2$ measurement of the muon. It is a major multi-purpose laboratory with many resources. It is claimed that it can readily manage the construction overlap in time of both the stretcher and RHIC.

B. PROPOSED PHYSICS RESEARCH

The reports issued by TRIUMF, LANL and BNL contain complete descriptions of the research which could be performed at the proposed accelerators. In this report we shall highlight only those experimental programs of special importance. It is apparent that a broad range of phenomena of fundamental importance would become accessible to experimental study upon construction of the facilities under consideration. The results of these studies would present significant and informative challenges to the standard model, reveal symmetries which survive in the multi-GeV range, and lead to a better understanding of the baryon–baryon interaction and the structure of nucleons. Experiments with nuclei would provide information on the effect of the nuclear medium on composite quark-gluon systems. In particular one will be able to explore the circumstances under which deconfinement takes place.

1. The Strong Interactions:

The strong interactions are not well-understood once one leaves the sector in which asymptotic freedom is valid and perturbation methods can be applied. Experimentally multi-particle final states and/or a high density of final states may make it difficult to extract the values of the quantities of interest from the data. However, there are many experiments in which this problem is not present if a sufficiently intense primary beam were available. And these experiments should not only be informative but should provide leads to the theorists.

The study of hyperon–nucleon scattering could reveal the extent to which $SU(3)$ symmetry is present in that interaction and the manner in which it is broken. A quantitative representation is essential for the theoretical study of Λ hypernuclei, Σ hypernuclei if they exist and doubly strange Λ hypernuclei. These nuclei will yield information on the badly known nucleon-hyperon and hyperon-hyperon interaction. Importantly the spectra of Λ hypernuclei could provide evidence regarding the importance of the Pauli principle and the deconfinement of the Λ in nuclear matter. As a corrollary, the Λ acts as a baryonic probe of the nucleus affecting the various macroscopic nuclear parameters providing thereby new ways to test nuclear models.

The H particle and glueballs are examples of particles "predicted" by QCD but not yet observed. Each of these reflect different aspects of QCD, the H particle the hyperon-hyperon and quark-quark interaction, and glueballs, the gluon-gluon interaction. Production of the H (or H nuclei) has not yet been achieved. Higher beam intensities would increase the sensitivity to H production. On the other hand distinguishing quarkless glueballs from other multi-quark–gluon states will not be easy. Higher energy will certainly help. It is proposed to use the Drell–Yan process to probe the quark distribution functions in nuclei and hadrons. At this time, theory is not able to provide a definitive method for interpreting this data (see Appendix I). Relative values of lepton pair production in different nuclei would reflect medium effects but the analysis would have to be phenomenological in nature.

The weaker interaction of the K^+ with nucleons should make it possible to use the K^+ as a probe of nuclear matter with the potential for improving the determination of neutron densities within nuclei.

Finally the investigation of the $\bar{p}$-nucleon system should reveal the relation of the $\bar{p}$-nucleon interaction to the nucleon-nucleon interaction. In particular one can study the extent to which G-parity is conserved. Proton-anti-proton annihilation offers several opportunities for studies of importance. These include, lepton pair production and the determination of nucleon electromagnetic form factors, meson production and the production of exotic states, hyperon and H particle production, charm production and the study of charmonium. The production of charmonium in $\bar{p}-p$ collisions offers the possibility of directly producing excited states of charmonium not directly available through e^+e^- collisions.

2. The Weak Interactions

Studies of rare K decays which are allowed by the standard model are among the most fruitful experiments that can be performed at the proposed facilities. Systematic studies of these phenomena not presently possible would provide greater insight into the details of the standard model and might reveal new physics outside of this framework. Such experiments are now intensity limited. Among the reactions of interest we include

$$K_L \to \pi^0 + e^+ + e^-$$
$$K^+ \to \pi^+ + \nu + \bar{\nu}$$

and

$$K_L \to e^+ + e^- \quad , \quad \text{or} \quad \mu^+ + \mu^- \ .$$

The proposed facilities would permit more sensitive searches for decays forbidden by the standard model. The existence of such decays would indicate new physics at the 100 TeV mass scale. Examples of these are decays in which muon number is not conserved.

$$K_L \to \mu + e$$
$$K^+ \to \pi + \mu + e$$

A third type of experiment is one which is presently limited by systematics rather than statistics. New higher flux facilities can lead to reduction in these effects and allow deeper examination of the physics responsible for these phenomena. Among these experiments are transverse polarization of the muon in $K^+ \rightarrow \pi^0 + \mu^+ + \nu$ and the Dalitz plot and rate asymmetries in $K^\pm \rightarrow \pi^+ + \pi^- + \pi^\pm$.

Weak baryonic interactions can be investigated, extending some of the results currently available, or made feasible because of the increased primary beam intensity. The decay of Λ hypernuclei via the reaction

$$\Lambda + n \rightarrow n + n$$

is one example.

The availability of the anti-proton projectile suggests studies of CPT and CP symmetries. Precise measurement of the $\bar{p}$ mass and magnetic moment would test CPT. Measurements of decay asymmetries and final state polarizations in hyperon–anti-hyperon systems such as $\Lambda\bar{\Lambda}$ and $\Xi\bar{\Xi}$ formed by a $p + \bar{p}$ collision would test CP.

The proposed facilities could also provide more intense neutrino beams. These can be used for improved neutrino oscillation experiments and experiments to test the Standard model through $\nu - e$ and $\bar{\nu} - e$ elastic scattering experiments.

C. SUMMARY OF SUBCOMMITTEE RESPONSE TO NSAC CHARGE

Issue 1:

The proposed facilities would make a broad range of phenomena of fundamental importance accessible to experimental study. In hadronic systems, investigation of the nature of confinement, the existence of the H particle, double strange hypernuclei, charmonium spectroscopy, the behavior of the s and $\bar{s}$ quark in a nuclear environment, the weak decay of the Λ inside a nucleus, the origins of the $\Delta I = 1/2$ rule, as well as the quark-gluon distributions in a nucleon compared to these distributions in a nucleus, are examples of issues which would benefit dramatically from the existence of these facilities. In addition, these measurements would lead to a better understanding and representation of baryon–baryon forces including nucleon–nucleon, nucleon–anti-nucleon, nucleon–hyperon, and hyperon–hyperon interactions. In the electro–weak sector, specific measurements could mount significant and informative challenges to the standard model. They would test speculative generalizations of the standard model which postulate new particles and processes and could reveal those symmetries implicit in the standard model which survive in the multi-GeV range.

Issue 2:

The design of the KAON facility was judged by the Subcommittee to be conservative. There appear to be no major design problems which would seriously impede construction of this facility. The necessary R&D is underway. Technical reviews by the U.S. Department of Energy would be welcomed. The facility would certainly "provide the needed experimental capability in this area of hadronic physics." The TRIUMF laboratory will need to augment its staff and advisory apparatus. They plan to do this.

Issue 3:

The KAON facility would provide a capability which would complement those of the electron accelerator, CEBAF, and the projected construction of the relativistic heavy ion collider, RHIC. The construction would be completed in a timely fashion (1995). The projected users based on the number now using the Brookhaven, LAMPF and TRIUMF facilities total about 800, of which one-third to one-half are from U.S. institutions. The final number may be somewhat larger because members of the high energy community may find the facility attractive. Priorities for experimental programs at KAON would be set by an international "Program Advisory Committee," on the basis of their scientific merit. U.S. scientists would be able to participate provided their proposals satisfy scientific criteria. It is anticipated that the U.S. will make use of approximately one-third of the operating time available at KAON. Operating costs will be furnished by the KAON facility. The construction of KAON would make a unique new facility available to a broad range of U.S. physicists. The total U.S. contribution to the construction would be $75 M over a period of five years. The Subcommittee considers such an investment to be cost effective.

Issue 4a:

The booster facility presently under construction at Brookhaven will enhance the investigations of the rare K decays and hypernuclear physics presently underway. Increased running time before and after completion of this upgrade would have a significant impact on the success of these activities. A stretcher would further facilitate these studies by providing a greater time integrated flux of protons without increasing instantaneous rates. In the pre-AHF/KAON era, the AGS will continue to be the most intense kaon facility available to U.S. physicists.

Issue 4b:

The scientific capability of the AHF as projected is comparable to the KAON facility but with some increased emphasis on higher energies, neutrino physics, and condensed matter physics. However, the plans for the advanced hadron facility (AHF) proposed by the Los Alamos Laboratory are in a preliminary stage. Additional design studies and R&D are needed to resolve major questions associated with the higher energies of this facility.

II. THE PROPOSED ACCELERATORS

A. TRIUMF

The proposed KAON facility at TRIUMF would produce a $100\,\mu a$, $\sim$ 100% duty cycle proton beam at 30 GeV. The underlying approach for obtaining this high current and duty cycle is the use of a chain of rapid cycling synchrotrons and DC rings. The elements of the chain are:

1) the existing TRIUMF cyclotron,

2) the accumulator (A) — a ring that accumulates the cyclotron beam and prepares it for injection into

3) the booster (B) — a synchrotron that accelerates the beam to 3 GeV,

4) the collector (C) — a ring that accumulates the Booster beam and prepares it for injection into

5) the driver (D) — a synchrotron that accelerates the beam to 30 GeV,

(6) the extender (E) — a ring that stores the beam for slow extraction to the experiments.

This system involves a large number of accelerators because of performance limits associated with high currents, cost optimization, the existing injector, and the high duty cycle needed by experiments. Some important parameters are in Table I.

TABLE I: Parameters of Accelerators in the KAON Proposal.

	TRIUMF	**A**	**B**	**C**	**D**	**E**
Energy range (GeV)	-.44	.44	.44-3	3	3-30	30
Circumference (m)	47.6	214	214	1070	1070	1070
Repetition rate (Hz)	DC	DC	50	DC	10	DC
Protons/cycle (10^{13})	—	1.25	1.25	6.25	6.25	6.25
Dipole B Field (T)	—	1.4	1.05	1.5	1.3	1.8
FR frequency (MHz)	23	46	46-61	61.1	61.1-62.9	62.9
Energy gain/turn (keV)	—	—	210	—	2000	—
RF voltage (kV)		354	576	636	2400	600

The TRIUMF cyclotron routinely produces H^- beams with currents up to $140\,\mu A$ at 520 MeV. The current is above the KAON design goal, but the time structure is not appropriate for injection into a cycling synchrotron. On a microscopic time scale the beam has bunches separated by the 23 MHz RF period of the acceleration system, and on a macroscopic scale the current is DC. The microstructure does not need to be changed, but the macrostructure must be converted into a pulse with length equal to the Booster synchrotron revolution period. That is the function of the Accumulator.

The H^- beam is extracted from the cyclotron at 440 MeV, and injected into the Accumulator by stripping ($H^- \rightarrow p$) in a thin foil at the injection point. The extraction procedure and equipment are under development, and 440 MeV was chosen to prevent stripping in the cyclotron magnetic field. Accumulation is possible with such charge-exchange injection, and it was selected for that reason. Details of the injection process are being developed with particular attention to minimizing irradiation of the stripping foil.

The Accumulator would accumulate for 20 msec; the resulting charge is $2\,\mu C$ (1.25×10^{13} protons). The space charge tune shift is 0.15 which is conservative. The RF structure of the beam is converted to 46 MHz by making the Accumulator circumference 4.5 times longer than the cyclotron circumference. After the accumulation period the beam is transferred with a bucket-to-bucket transfer to the Booster where is it accelerated to 3 GeV.

The Booster is a rapid cycling synchrotron with a 50 Hz repetition rate. The high rate of acceleration has substantial implications for the vacuum and RF systems. The vacuum chamber must be thin to reduce eddy current effects but still must provide a high conductivity path for beam image currents. Various designs of ceramic chamber with RF shields are being considered. The RF system must be capable of a large energy gain per turn. That requirement is partially eased by an asymmetric magnet cycle that has a risetime three times longer than the falltime.

The accelerator RF systems are among the dominant factors that led to the KAON design approach. A $100\,\mu A$ beam at 30 GeV has 3 MW of beam power; that power must be delivered by the RF systems. In addition, the energy increase from 440 MeV to 30 GeV means an RF frequency change by a factor of 1.37. By breaking up the acceleration into two steps, 440 MeV to 3 GeV in the Booster and 3 GeV to 30 GeV in the Driver, most of the frequency swing takes place in the Booster and most of the power must be supplied by the Driver. This substantially separates two major RF requirements.

The Collector adapts the Booster beam for injection into the Driver. The beam is extracted from the Booster and injected into the Collector with a bucket-to-bucket transfer. The Collector circumference is five times that of the Booster, and it is filled by five successive Booster cycles to a total of 6.3×10^{13} protons. When this charge has been accumulated it is transferred, with another bucket-to-bucket transfer, to the Driver synchrotron where the energy is raised to 30 GeV.

The Driver synchrotron is also rapid cycling; the repetition rate is 10 Hz. As with the Booster, this calls for a shielded ceramic vacuum chamber and a large energy gain per turn. Even with an asymmetric magnet cycle the RF requirement is substantial, 2.4 MV of accelerating voltage. Both transient and steady-state beam loading of the RF are significant, and sophisticated feedback systems to handle these effects and control longitudinal coupled bunch instabilities will be required.

When KAON is compared to the present performance of the Brookhaven AGS most of the increased current comes from rapid cycling. The number of protons per pulse is a factor of four higher. This factor together with the thin vacuum chamber and large RF system needed for rapid cycling make coupled bunch instabilities a serious concern. Instability growth rates are large, and a number of solutions have been proposed and are being explored. These include lowering the Q's of parasitic modes, active tuning of the frequencies of parasitic modes, and feedback. A complete solution remains to be developed.

The 30 GeV beam is extracted from the Driver and injected into the Extender with a fast bucket-to-bucket transfer. This is followed by a longitudinal phase space rotation to remove substantially the RF microstructure and slow resonant extraction to deliver high duty cycle beams to the experiments. With this approach the Driver is optimized for rapid cycling operation without constraints on the aperture or magnet circuit from slow extraction, and the Extender is optimized for slow extraction without the constraints of cycling. Preliminary studies show that it is feasible to extract the beam from the Extender with 99% efficiency, and a three-stage extraction system with losses at the 0.1% to 0.2% level is being simulated.

The KAON design is a sound approach to building a high intensity hadron machine. The use of multiple rings with specialized functions simplifies substantially a number of accelerator subsystems and will provide flexibility for solving problems that will arise during early commissioning and operation. The design is conservative: magnetic fields, RF voltage and frequency swings are within present practice and the intensities per bunch and per cycle are not significantly above that achieved in the AGS.

There are technically challenging elements (RF, kickers, vacuum chamber) which are being developed and prototyped as part of an \$11 M, fifteen-month project definition study. That study is to be completed at the end of 1989. When it is finished the accelerator design and cost estimate should be developed sufficiently for a construction approval decision. The research and development costs are listed in the table following on the next page.

B. BROOKHAVEN NATIONAL LABORATORY

Brookhaven National Laboratory (BNL) has an operating hadron facility, the AGS (Alternating Gradient Synchrotron) complex, a substantial upgrade underway, and plans for additional enhancements. The achieved performance and future goals are summarized in Table II.

The existing complex consists of i) the AGS itself, ii) injectors for high intensity unpolarized protons, polarized protons, and ions, and iii) secondary beams including neutral beams, and separated and unseparated charged beams. In FY89 the AGS will operate twenty weeks with high intensity proton beams and seven weeks with heavy ions. The high intensity running is of primary interest for addressing the charge of this subcommittee. Typical performance at the present time is: 1.5×10^{13} protons/pulse, 2.5 sec cycle time, 35% macroscopic duty cycle and $1.0\,\mu A$ average current. The effective duty cycle is somewhat lower due to effects such as magnet power supply ripple.

The immediate upgrade has the goal of raising the intensity to 6×10^{13} protons/pulse. At the present time the intensity limit is the space charge tune shift in the AGS at injection. This tune shift ($\Delta\nu$) depends on geometrical factors such as

KAON RESEARCH AND DEVELOPMENT COSTS

(In Canadian dollars)

	Past Expenditures K$	PDS Budget K$	Total Estimated R&D K$
Accelerator Design	1,230	632	2,464
RF Prototypes	480	1,225	3,750
Magnet Prototypes		760	3,040
Magnet Power Supplies	190	925	2,200
Beampipe and Vacuum		857	1,714
Kickers		380	1,900
Cyclotron Beam Extraction	1,250	770	3,030
Shielding and Safety		170	510
Target Areas and Remote Handling		180	900
Controls and Diagnostics		358	1,432
Systems Integration		320	480
Experimental Areas		300	1,800
Buildings, Tunnels, Services		2,030	2,030
Detectors, Spectrometers		—	5,000
Contingency		733	2,932
	4,200	9,640	33,182

PDS Materials and Supplies =	4.35	M$
TRIUMF Manpower =	2.71	
Visitors =	.68	
Consultants =	1.90	
	9.64	
Total R&D		
Materials and Supplies =	14.97	
Manpower =	18.21	
	33.18	

the radius of the ring and the dimensions of the vacuum chamber, the number of particles (N), and the beam energy (γ) and velocity (β). The latter dependences are

$$\Delta\nu \propto -\frac{N}{\gamma^3\beta^2} \quad .$$

Based on experience with a number of machines, the tune shift limit is between 0.3 and 0.6. The AGS operates at the upper end of this range with $\Delta\nu = 0.58$.

The AGS Booster will raise the limit in the AGS by about a factor of twenty-five by raising the (kinetic) energy for injection from 200 MeV to 1.5 GeV. This full factor will not be realized because of the space charge tune shift in the Booster. Using a design value of $\Delta\nu = 0.35$ for the Booster, the factor of four intensity increase is realized. The Booster is an approved construction project that is well underway; the Total Estimated Cost is \$31.7 M. Of this \$26.5 M has been appropriated through FY89, and the remaining funds are anticipated in FY90.

In addition to the Booster there are a number of projects required for the AGS to be able to handle the increased intensity. These include modifications to the vacuum system, RF system, main power supply, instrumentation and controls, and correction systems. The total estimated cost of these projects is \$30 M. They are being funded out of AIP (Accelerator Improvement) funds at a rate of approximately \$3 M/year and GPP funds (General Plant Projects) at a rate of \$1 M/year starting in FY 86; this rate would have to be increased to take advantage of the Booster completion in a timely manner.

Longer term plans are for a Stretcher to increase the flux and duty cycle. The Stretcher would be a storage ring with an energy equal to the peak AGS energy. Accelerated beams would be fast extracted from the AGS and injected into the Stretcher from which they would be slowly extracted over the time between AGS cycles. The AGS would cycle at twice the present rate giving a factor of two increase in intensity, and the effective duty cycle would be close to 100%.

There are conceptual designs for two versions of the Stretcher; one uses superconducting magnets and the other warm magnets. Development of both designs is continuing with the goal of making a choice for validation in the Spring of 1989 and a start of construction in FY91.

TABLE II: AGS Hadron Facility

Time frame	**Protons per pulse**	**Cycle Time**	**Duty Factor**	**Current**
Present	1.5×10^{13}	2.5 sec	35%	$1.0\,\mu A$
After Booster completion (early 1990's)	6.0×10^{13}	2.5 sec	35%	$4.0\,\mu A$
After Stretcher completion (mid- to late-1990's)	6.0×10^{13}	1.3 sec	100%	$8.0\,\mu A$

C. LANL ADVANCED HADRON FACILITY

The Los Alamos National Laboratory (LANL) is developing a plan for an Advanced Hadron Facility (AHF) that would have a high energy synchrotron and two low energy compressor rings. While there are important differences in parameters, the synchrotron would address much of the same physics as KAON and the Brookhaven Stretcher. The compressor rings would accumulate current from a new 0.8 – 1.6 GeV linac and deliver it in short pulses to either a neutrino detector or a pulsed neutron facility for condensed matter physics. There is nothing comparable in the proposals from the other two laboratories.

The 800 MeV beam from the LAMPF linac will be accelerated to 1.6 GeV in a new linac to be constructed as part of the AHF. The 1.6 GeV beam will either be injected into one of the compressor rings or accelerated to 2.2 GeV in another new linac for injection into the synchrotron. Improvements to the present LAMPF linac and the 0.8 to 1.6 GeV linac are the only parts of the AHF shared by the high energy synchrotron and the compressor rings, and to a substantial extent, the two parts of the LANL proposal are independent.

The synchrotron is to have an energy of 60 GeV, an average current of 25 μA, and a 50% duty cycle. There are four designs under consideration to meet these goals. One of these was presented to the committee, and the text that follows is a description of that design. The synchrotron injection will be a direct bucket-to-bucket transfer from the 2.2 GeV linac to avoid beam loss. The beam will be accelerated to 60 GeV with an acceleration time of 63 msec, and then the magnetic field will be "flat-topped" for 83 msec. The beam will be slow extracted during the flat-top for use by experiments. At the end of extraction the magnet will be cycled back to 2.2 GeV and new beam injected. The overall cycle rate is 6 Hz.

There are a number of critical areas that are to be addressed as part of the AHF R&D. These are:

1. Beam losses and resultant activation must be controlled, a common feature of all high intensity projects. The present design calls for a three-stage slow extraction procedure and remotely handled collimators to define the beam tails in both the longitudinal and transverse dimensions.

2. The synchrotron size is limited by geographical features of the LANL site, and the dipole magnets must operate at 2.2 T to reach 60 GeV. These magnets would be heavily saturated, and this has substantial impact on field quality, power supplies, and power consumption. As a result, the magnet and power supply are considered to be technically difficult.

3. The beam is to be accelerated from 2.2 GeV to 60 GeV in 63 msec. The RF frequency swing is a factor of 1.05, and the maximum RF voltage must be 7.6 MV/turn to accelerate the beam while keeping the RF bucket large enough. In the past there has been a substantial R&D program at LANL in the area of ferrite tuned cavities, and this work will be continued.

4. There is closely related R&D in the areas of instabilities, impedances, and beam feedback. This includes damping higher modes of the RF cavity, designing a

vacuum chamber with a small beam impedance and low eddy currents in the rapid cycling magnetic field, and designing feedback loops and dampers.

As compared to the TRIUMF-KAON design, the AHF does not have intermediate energy machines or a final energy duty cycle stretcher ring. This minimizes the number of steps in the accelerator chain at a cost of increased complexity for some of the accelerator subsystems. Examples are the increase in accelerating voltage that is a consequence of not having a stretcher ring, and the need to optimize the lattice for both acceleration and slow extraction. The AHF approach could have higher reliability than a multiple ring design, but that is not clear at this state of development.

TABLE III: AHF Main Synchrotron Parameters.

Kinetic Energy Range	2.2 to 60 GeV
Average Current	25 μA
Circumference	1300 m
Repetition Range	6 Hz
Duty Cycle	50%
Protons/cycle	2.6×10^{13}
Dipole Magnetic Field	2.2 T
RF Frequency	50.3 to 52.7 MHz
Maximum RF Voltage	7.6 MV

The compressor rings would be a second generation of the Proton Storage Ring (PSR) at LAMPF. The goal is to deliver about 1 MW of beam power to both a neutrino target and a spallation neutron target. At the present time the PSR performance is limited by accumulation losses. The AHF compressor ring design is based on experience with the PSR, and it includes a number of features that would minimize these losses. Some of these are related to the H^- stripping process and some to apertures and collimation. A lattice has been designed around the injection and extraction processes, and a first round of injection simulations and collimation designs are complete.

The PSR is near a coherent instability limit at 4×10^{13} protons per pulse, and each of the compressor rings would run at roughly one and one-half times that intensity. Initial instability estimates show that the compressor rings would be stable because of higher energy. These estimates will be developed further including relating them to the PSR performance.

TABLE IV: PSR and AHF Compressor Ring Design Parameters.

Parameter	**PSR**	**AHF Compressor Ring** (per ring)
Beam Energy	800 MeV	1.6 GeV
Average Current	100 μA	600 μA
Repetition Range	12 Hz	48 Hz
Protons per Pulse	5.2×10^{13}	7.5×10^{13}

There are rough cost estimates for the AHF R&D and for the AHF itself. The latter is based on scaling the LAMPF II costs. These are summarized in the two tables below. It is estimated that \$130 M of the \$560 M construction cost would come from material science funding for the neutron area and part of the compressor ring costs.

TABLE V: AHF Pre-Construction R&D Plan

Modifications to the LAMPF Linac	\$6.0 M
.8 to 1.6 GeV Linac	2.5 M
1.6 GeV switchyard	0.2 M
Compressor Rings	4.3 M
1.6 to 2.2 GeV Linac	0.1 M
Main Synchrotron (2.2 to 60 GeV)	11.5 M
Experimental Areas	7.7 M
Primary Beam Area	0.9 M
Utilities	0.6 M
Control Systems	1.0 M
Operational Considerations	0.4 M
Safety and Environment	5.6 M
Architectural	1.8 M
Construction Schedule	0.3 M
Contingency (35%)	15.0 M
TOTAL	\$57.9 M

TABLE VI: AHF Cost Estimate

Linac 0.8 to 2.2 GeV	100 M
Compressor Rings	100 M
Spallation Neutron Area	50 M
60 GeV Synchrotron and Experimental Areas	310 M
TOTAL	\$560 M

III. ANCILLARY COSTS AT KAON

The KAON operating budget of \$90 M per year will cover all accelerator operations including primary and secondary beams. With the possible exception of a limited amount of utility equipment such as large magnets, the instrumentation of the experimental program will be the responsibility of the participating institutions. Adequate planning and funding for the detectors, spectrometers and other ancillary experimental apparatus will be essential to full exploitation of the capabilities of the KAON. Because of the diversity of the program and the increased level of performance which will be required, substantial resources will be essential to the design and construction of the instrumentation for a productive program. A possible scenario for funding an initial complement of KAON experiments was developed and discussed. Instrumentation for a neutrino facility, a polarized beam experiment, a multi-particle spectrometer for meson spectroscopy, two kaon decay experiments, a high resolution spectrometer for K^+ nucleus scattering, and a hypernuclear physics experiment were major items of the initial equipment budget. The total cost for this phase, \$100 M, represents an initial estimate whose precision is uncertain, but whose magnitude provides an important indication of the resources which will be essential to a productive program. In several areas, particularly in the study of rare decay modes, progress in detector performance has been dramatic and the state-of-the-art at the AGS provides a good base for extrapolating costs to performance levels which will be required at KAON.

In discussing possible funding profiles, TRIUMF has assumed that half of the experimental costs will be borne by external, *i.e.* non-Canadian sources. We anticipate on the basis of the projected level of U.S. participation that about \$30 M would be an appropriate contribution of the U.S. physics program. Note that the lower costs of electric power in British Columbia would result in a more effective use of operating funds.

IV. MANAGEMENT

The three laboratories, BNL, LANL and TRIUMF, have successfully operated major facilities in nuclear and elementary particle physics. Both BNL and LANL are multi-purpose laboratories with extensive scientific and engineering resources which can be called upon in support of their projects. The directors at each of these laboratories have promised strong support of the Stretcher and AHF, respectively. The BNL management has stated that they would have the manpower resources to simultaneously build RHIC and the Stretcher. The in-house resources of TRIUMF are more limited since it is by-and-large a single purpose laboratory. However, in connection with KAON, TRIUMF has been actively consulting with appropriate individuals at LANL and FNAL with respect to various elements of the KAON facility. It is also clear that the TRIUMF staff would need to expand to meet the accelerator construction needs, the planning of and construction of detectors, *etc.*

The three laboratories have been "user friendly." The support of users, their housing and their experiments have been successfully implemented by each laboratory. It is anticipated that the users on-site per year would number about 800.

Sections V – VII report on the physics which becomes possible if these facilities were to be built. Only items of special interest are noted. The full list of possibilities are contained in the reports submitted by each of the laboratories.

V. STRONG INTERACTION PHYSICS AT AHF/KAON

Investigation of hadronic structure and quark-gluon degrees of freedom in nuclei are the core subjects of the strong interaction program at these proposed facilities. In spite of the great advances made in recent years by asymptotic QCD, our understanding of the confinement mechanism and non-perturbative process is still very sketchy and the prospect of adding to our knowledge in this field of fundamental importance for nuclear physics is certainly one of the major justifications for such facilities. Since our present ideas rely strongly on "QCD-inspired" models rather than reliable calculations, it is clear that the proposed experimental program must be much more qualitative and/or phenomenological than rare decay Kaon experiments where the results based on the standard model can be accurately calculated. Nevertheless there seems to be good reason to believe that some of the most exciting new physics results will come from these "qualitative experiments."

A. ELEMENTARY SYSTEMS

The most obvious manifestation of QCD effects would be exotic states whose existence is due to the fundamental interaction *e.g.* glueballs, hybrids, or dibaryons. Unfortunately, in many cases isolating such exotic states may essentially require identifying all the non-exotic states to ensure the new state cannot be explained in a more standard fashion. This is surely a formidable task. An exotic state that may be easier to identify (if it exists) is the H particle of Jaffe. This $S = -2$ dibaryon consists of $2u$, $2d$, $2s$ quarks in a totally symmetric S state and is predicted by most model calculations to be bound with respect to strong decay into two lambdas. Finding the H and determining its properties is clearly an important test of such calculations. Two reactions have been proposed for its creation: the single-step process $^3\mathrm{He}(K^-, K^+, n)H$, and the two-step process $^1H(K^-, K^+)\Xi^-$ with the subsequent atomic capture of the Ξ^- on deuterium followed by the reaction $\Xi^-(^2H, n)H$. A search for these reactions will be made at the AGS but since the predicted production rate with present K^+ beam intensities is low and quite uncertain, the much higher K^+ flux available from AHF/KAON may well be required to either find the H or reduce the upper limit to the point where its existence can be safely ruled out. If the H exists its predicted weak binding and hence large size should make it especially susceptible to nuclear medium effects and hence its production rate versus A should be extremely interesting to investigate.

A state whose characteristics seem to imply that it might well be a weakly bound $2(q\bar{q})$ state is the S^* ($J^{\pi C} = 0^{++}$, $I^G = 0^+$); it has a large branching ratio into $K\bar{K}$ even though its central energy is below the $K\bar{K}$ threshold. Thus one interpretation is of a widely spaced $K\bar{K}$ cluster weakly bound by QCD exchange forces. By studying the production and decay of the S^* versus A one might be able to establish the validity of this picture.[3,4]

Another very important field for such facilities is the determination of hyperon-nucleon scattering parameters. Very little data exists in this field and it is clear that before hyperon beams can be used to investigate nuclei we must know the fundamental interaction. Although the present limitation in obtaining this information is often not the primary beam intensity, it is clear that the high intensity secondary beams obtainable from such facilities will certainly make life somewhat easier.

Another candidate for an interesting nuclear probe is the η' since there are strong indications that it contains a large fraction of non-uds components and at least strong theoretical arguments that this component is longitudinal gluons. If this is the case one might naively expect that the gluon field in a nucleus could alter the η' interaction so that again one might see an unusual A dependence.[3]

An interesting test of the applicability of perturbative QCD would be to look for the onset of the high p_T relations proposed by Farrar[4] such as:

$$\frac{1}{\sqrt{3}}\left(\pi^- + p \rightarrow \pi^+ + \Delta^-\right) = \left(\pi^- + p \rightarrow K^+ + Y^-\right) = \left(K^- + p \rightarrow \pi^+ + Y^-\right) \quad .$$

The kaon-induced reactions and their products can provide vital insights into quantum chromodynamics. The exploitation of these opportunities has been severely limited by the absence of K beams of sufficient intensity and purity. Still, the results obtained so far are sufficient to demonstrate the possible great value of such studies.

B. NUCLEAR SYSTEMS

The nature of confinement, symmetry breaking, the existence of dibaryons and double hypernuclei, the behavior of the s and $\bar{s}$ quark in a nuclear environment, the weak decay of the Λ inside a nucleus, the quark-gluon and sea quark distributions, the nature of the baryon-hyperon potential and finally the use of the bound Λ to probe properties of the host nucleus are examples of areas which kaon experiments will illuminate.

For instance, consider what can be learned from the study of Λ hypernuclei.[16] These are formed by the reactions:

$$K^- + (Z,N) \longrightarrow (Z,N-1,\Lambda) + \pi^- \tag{1}$$

and

$$\pi^+ + (Z,N) \longrightarrow (Z,N-1,\Lambda) + K^+ \quad . \tag{2}$$

The ground states, the low-lying spectrum and the radiative transitions can be and in some cases have been studied. The first of these reactions has proved useful up through the p-shell hypernuclei, but has not been successfully used for heavier nuclei. Reaction (2) has recently been applied to obtain heavier hypernuclei.[6] These experiments are intensity limited. Of immediate interest is the nature of hyperon-nucleus potential and its relation to the hyperon-nucleon interaction. General techniques which have been developed to relate nucleon-nucleon forces with nuclear properties are very useable in this area. One has already learned that the $\Lambda - N$ spin-orbit force is relatively small. But there is much more. There is the question of the impact of the Pauli exclusion principle which the u and d quarks in the Λ and those in the host nucleus must satisfy. In the limiting case when there is no effect, *i.e.* the Λ is treated as an elementary particle like the nucleon, it becomes possible for the Λ to occupy low-lying orbitals which would not be allowed if it were a nucleon. Thus the low-lying spectrum will reflect the degree to which the Pauli principle is effective. The degree of deconfinement would clearly play a role and the dependence of the effect on mass number would relate the deconfinement to the nature of the nuclear medium in which the Λ finds itself embedded. A third phenomenon of interest is the weak decay of the Λ inside a hypernucleus through the processes

$$\Lambda + N \longrightarrow N + N \tag{3}$$

$$\Lambda \longrightarrow N + \pi \ . \tag{4}$$

The later can be observed in free space, while the former can only occur within hypernuclei. The strangeness conserving component of the weak interaction in the baryon system has also been observed in the parity non-conserving scattering of two nucleons. The reaction, Eq. (3), will add complementary information on the strange component of the weak interaction. And again the influence of the nuclear medium will be of great interest as one varies the mass number of the hypernucleus.

More problematic is the existence of Σ hypernuclei. It is not known whether their existence is widespread over a broad range of host nuclei. These experiments require higher intensity K beams. Their widespread occurrence would point to a general symmetry (*e.g.* $SU(3)$ symmetry) acting which inhibits the decay of the Σ hypernucleus into a Λ hypernucleus, the width being determined by the symmetry breaking interaction.

An experiment which is now limited by intensity results in the formation of doubly strange nuclei. Ξ hypernuclei can be formed in a $K^- \rightarrow K^+$ reaction. It should be observable and one may expect also to see narrow states as is the case for Λ hypernuclei. Hypernuclei containing two Λ's can be produced from Ξ^- exotic atoms, or from the two-step process $K^- + N \rightarrow K + \Xi$, $\Xi + N \rightarrow \Lambda\Lambda$. A limiting case is the H dibaryon discussed above whose wave function in the baryon-baryon basis is

$$H = \frac{1}{\sqrt{8}} \left(\Lambda\Lambda + \Sigma^0\Sigma^0 + \Sigma^+\Sigma^- + \Sigma^-\Sigma^+ + \Xi^0 n + n\Xi^0 + \Xi^- p + p\Xi^- \right) \ .$$

And if the H exists, the formation of H hypernuclei may become possible.

Much of the discussion above has been concerned with the influence of the nuclear medium on the properties of the hyperons. But the hyperon can also act as a unique probe of nuclear matter. The presence of the Λ in hypernuclei will modify the macroscopic parameters describing the host nucleus. Such properties as the radius, moment of inertia, superconductivity gap, electromagnetic moments, *e.g.* magnetic moments and quadrupole moments, might be modified by the presence of the Λ. These effects are small and require the higher kaon intensity for their measurement.

A particle which should be extremely useful in investigating nuclei is the K^+ since its $\bar{s}u$ structure prohibits the formation of low-lying resonances and results in an extremely weak interaction with nuclei. Thus, it would appear to be an ideal strong interaction probe for studying nuclei. The present meager data indicate however that something strange seems to be going on since the preliminary results from the recent AGS total cross section experiment[7] appears to confirm the old Bugg[8] result that the total cross section of K^+ on ^{12}C is almost exactly six times that of ^2H indicating that either multiple scattering corrections are anomalously small or the interaction in nuclei differs from that on the nucleon. This is also what is indicated in the very meager elastic scattering data.[9] Thus even in this supposedly simply system we may have indications of interesting new effects.[10]

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VI. ANTIPROTON PHYSICS AT AHF/KAON

INTRODUCTION

The number of problems accessible through antiproton induced reactions has increased significantly in recent years. These include investigations of antiproton static properties, CP nonconservation, nucleon electromagnetic form factors, antiproton proton annihilation, hyperon antihyperon production, and charmonium production, to name a few. The enhanced antiproton production rates at the new high current proton synchrotrons offer an excellent opportunity to explore these topics.

The technology for producing antiproton beams has changed significantly in the past ten years and is having a dramatic impact on our knowledge of antiproton physics. The new accelerator facilities under discussion offer opportunities for continued developments in this area. We review briefly the revolution that new accelerator technology is generating for antiproton measurements in the four MeV to seven GeV energy range and then discuss some selected measurements which exploit this new technology.

The traditional approach to antiproton beams is the construction of magnetic beam channels down stream of production targets which capture the antiprotons, make a mass separation from lighter particles, and measure the antiproton momenta. With a typical 30 GeV $1\mu A$ proton synchrotron this gives a peak in the antiproton yield at about 4 GeV/c. At lower antiproton momentum the production yield is considerably smaller. For example in in the 800 MeV/c region, $\bar{p}$ beam intensities of 10^4/sec (per 10^{12} incident protons) in an area of 2 cm^2, a pion to anti-proton ratio of 5–10:1 and momentum resolution at the 1% level are achieved. All of these parameters can be dramatically improved.

High beam purity can be achieved with a Time Separated Beam[1] in which a long magnetic channel is used to allow the contaminate pions to decay away relative to the stable antiprotons. Other beam parameters such as the antiproton beam profile and current are essentially unchanged for comparable proton beam currents.

To achieve enhanced antiproton rates at momentum off the $\bar{p}$ production peak, there are essentially two issues: a) increase the overall antiproton production rate using the new 10 to 100 μA proton synchrotron facilities and b) capture the high yield of antiprotons near 4 GeV/c and decelerate/accelerate them with minimum losses. Dramatic improvements have been achieved at the LEAR facility at CERN and recently at FNAL using the latter technique with a pair of rings for antiproton capture and storage. Stochastic cooling of both the transverse and longitudinal phase space has given significant improvement to the beam parameters. For example, in the extracted beams at LEAR, antiproton currents in excess of 10^6/sec are being used with an antiproton beam profile of less than 1 mm^2 and a momentum resolution of less than one part in 10^4.

The facilities discussed in this report offer opportunities to exploit these techniques with even greater benefits due to the enhanced proton currents in the main ring. Time Separated Antiproton beams offer high purity at moderate cost. Storage and phase space cooling of antiprotons in separate rings is more expensive but offers excellent opportunities for internal target and colliding beam experiments as well as extracted beam measurements. Since these activities can be conducted in parallel with the Kaon Physics activities, this makes excellent use of the high proton intensity in the main ring. Hydrogen jet target measurements now being developed at LEAR and in progress at FNAL offer luminosities up to $10^{31}\text{cm}^{-2}\text{sec}^{-1}$.

The possibilities for generating antineutron beams, polarized antiproton beams, as well as polarized internal gas targets are actively being explored. See for example a recent review article by W. Haeberli.[2]

A. ANTIPROTON PROPERTIES

Comparison of the properties of antiprotons to protons is of general interest particularly in the context of conservation of CPT and supergravity theories. In addition to investigation of the mass splitting of the $K\bar{K}$ system, the $p\bar{p}$ system offers the best hope of a precision measurement of CPT nonconservation. Measurements of the antiproton mass and magnetic moment have come at low energies from investigations of the x-ray spectra in the decay of antiprotonic atoms and at higher energies, from precession of the polarization produced by an applied magnetic field.

The same cooling techniques that permit high luminosity experiments at high energy also provide an opportunity for experiments at ultra low energy. Through a series of deceleration stages, antiprotons collected and cooled at the peak momentum for production (about 4 GeV/c) can be made available at thermal or subthermal energies. For example, techniques are now being developed[3] to use both radio-frequency mass spectrometer measurements and $\bar{p}$ cyclotron frequency measurements in a Penning trap, to improve measurements of the proton-antiproton inertial mass difference to < 1 ppb. This would increase the precision of CPT mass measurements for baryons by 10^4 to 10^5.

Furthermore, plans are now in progress to develop an RFQ-Pulsed ion trap beam with energies in the range 0.001 – 1000 eV for a precision gravitational mass measurement.[4] This will test certain supergravity models in which specific particles have gravitational masses different from their associated antiparticles. These experiments are long term efforts which require precision measurement techniques and which depend on high initial antiproton fluxes in that they make severe cuts on the emittance of the antiproton beam. Work is currently in progress at LEAR. The next generation measurements will benefit from the higher current proton facilities being discussed.

B. ANTIPROTON ANNIHILATION

The investigation and exploration of proton-antiproton annihilation has a long and interesting history but always has been limited by low yield. Several experiments which could exploit the high antiproton yields at a high current proton facility are: (1) Electron Positron Production and the proton EM form factor in the timelike region; (2) Meson Production and the production of exotic states; (3) Hyperon and H particle Production; (4) Charm Production and the study of Charmonium and charmed exotic states. We select the latter case of heavy quark production for discussion here.

The spectroscopy of charmonium ($\bar{c}c$) and bottomium ($\bar{b}b$) systems can provide considerable insight into the strong forces between quarks and antiquarks *i.e.* QCD. The ψ and Υ spectroscopies of the c and b quarks are well described in terms of a simple non-relativistic phenomenological potential model motivated by QCD in which the spacing of the energy levels and the number of bound states are reproduced. Nevertheless the extraction of α_s from the measured transition rates of the radiative decays of charmonium indicates the presence of important second order processes. This is not well understood. Both the formulation of the theory and the available data are inadequate.

Our information about the level positions, total widths, and branching ratios into electromagnetic, $p\bar{p}$, and hadronic channels comes chiefly from interactions in electron positron storage rings in which the virtual photon couples only to resonances with $J^{PC} = 1^{--}$. Other states can only be seen through decay of the vector states for example the n^3S_1 states. Therefore properties of the pseudoscalar, scalar, axial vector, and tensor states are much less well known. It is of great interest to find additional states of this system and to measure the widths for emission of photons and hadrons.

Production of charmonium states in $p\bar{p}$ collisions offers many attractive features. This channel couples directly to many additional states. The state is observed through its decay either into an inclusive J/ψ with a subsequent decay into e^+e^- or an exclusive two photon decay. Detection of other decay channels such as $\gamma\eta_c$, $\eta\eta$, and $\eta\eta'$ are also feasible.

Initial work in this area was started at the ISR[5] and continues at FNAL.[6] The cross sections for the final states of interest are as small as 10 pb. Thus it requires an integrated luminosity of about 10^{+37} cm^{-2} in order to obtain 100 events. For example, a gas jet of 10^{14} atoms/cm^2 and a circulating antiproton current of 15 ma (about 1.5×10^{11} circulating antiprotons at FNAL) corresponds to a luminosity of 10^{+31} cm^{-2} sec^{-1} (10 events/day/10 μb).[7] The LEAR facility is too low in momentum to produce charmonium. This could be a fertile area for investigation at a new high intensity proton facility if coupled with a state of the art large acceptance detector.

Additional studies, including searches for heavy quark exotics (*e.g.* $c\bar{c}$ gluon) as predicted by Ono,[8] have been discussed by Poulet[9] and Dalpiez.[10]

D. CP VIOLATION

The motivation for studying violations of CP symmetry remains strong. The main problem in the field at present is to find new signals of CP violation which might distinguish among the competing theories. Until recently there was only one non-zero observable, ϵ, provided by experiment. Recently, a non-zero value of ϵ'/ϵ has been suggested by CERN experiment, NA31 and an experiment at FNAL.[11] Both of these are accommodated by a variety of models and it remains to establish a deep insight into the physical origin of CP non-conservation.

Among the proposed searches for new signals of CP violation the investigation of decay asymmetries in hyperon-antihyperon systems such as $\Lambda\bar{\Lambda}$ or $\Xi\bar{\Xi}$ is very attractive. The reactions $p\bar{p} \rightarrow Y\bar{Y}$ can provide a particularly clean laboratory for

CP violation studies. To the extent that the hadronic production process is charge conjugation invariant, Y and $\bar{Y}$ are produced with equal polarizations. Parity conservation requires that their polarizations be transverse to the production plane. Since the $p\bar{p}$ initial state, and as well, the $Y\bar{Y}$ final state, have definite CP properties, final state interactions cannot generate a misleading signal. Owing to Baryon number conservation, there is no $Y\bar{Y}$ mixing, and therefore any signal constitutes a measure of direct $\Delta S = 1$ CP violation.

For $\Lambda\bar{\Lambda}$ production and decay by $\Lambda \rightarrow p\pi^-$ ($\bar{\Lambda} \rightarrow \bar{p}\pi^+$), current measurements of the decay asymmetry parameters, α and $\bar{\alpha}$, give a normalized difference of

$$A = (\alpha - \bar{\alpha})/(\alpha + \bar{\alpha}) = -0.023 \pm 0.057$$

based on about 16,000 events.[12] CP violation in several models can be expected at the level of 10^{-4} for either the $\Lambda\bar{\Lambda}$ or the $\Xi\bar{\Xi}$ systems.[13] For example, a $\Lambda\bar{\Lambda}$ study would require about 10^{+9} events for a 3 σ measurement.

One can achieve an order of magnitude better sensitivity through a measurement of both the decay asymmetry and the final state polarization. This could be achieved through a study of the decay sequence $\Xi^- \rightarrow \Lambda\pi^- \rightarrow (p\pi^-)\pi^-$ where the first decay gives a decay asymmetry and the second decay gives a measure of the final state polarization. The $\Xi\bar{\Xi}$ cross section at $\sqrt{s} = 3$ GeV is about 2 μ barns. This measurement seems practical if a peak luminosity of $10^{+32}\text{cm}^{-2}\text{sec}^{-1}$ were available. This experiment is not feasible at LEAR since the maximum $\bar{p}$ momentum (2 GeV/c) is below the Ξ production threshold and thus is an ideal candidate for a new high flux antiproton facility.

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VII. ELECTROWEAK PHYSICS ISSUES

There are a number of electroweak physics issues to be addressed in connection with the proposals from KAON and AHF.

A. FRONTIER ISSUES

Elementary particle physics is in the satisfying yet frustrating situation of having a theory which works too well. The so-called Standard Model (SM) of weak, electromagnetic, and strong interactions is consistent with all known experimental data. In spite of that, there is hardly a theorist to be found who believes this is not just some sort of low energy effective field theory resulting from some more fundamental theory. Indeed, there are persuasive arguments† that the theory will be modified significantly by an energy scale of 1 – 2 TeV, even though there is not general agreement on what new degrees of freedom will appear.

In order to probe the physics at shorter distances or higher energy scales than investigated heretofore, there are two largely complementary approaches available: (1) The high-energy route makes a direct assault through the construction of new, more energetic accelerators in the hope of observing new phenomena or extremely massive particles. (2) The low-energy route attempts more sensitive searches or more precise measurements. Some searches are intended to find a light, very weakly interacting particle, such as an axion, a new (possible massive) species of neutrino, a very light Higgs, or some other weakly interacting massive particle (WIMP) that frequently forms the debris of theories involving very high energy scales. Other experiments search for neutrino oscillations, in the hope of establishing the existence of a non-zero neutrino mass, while others search for decays prohibited by present theories, such as $K_L \to \mu e$ or $K^+ \to \pi^+ \mu e$. Yet other experiments seek to observe suppressed but not prohibited decay modes, such as $K_L \to \pi^0 e^- e^+$ or $K^+ \to \pi^+ \nu\bar{\nu}$, or to measure some parameter precisely, often in order to elucidate the nature of CP violation as in a determination of ϵ'/ϵ in $K_L \to 2\pi$ or the transverse muon polarization in $K_{\mu 3}$. Frequently, these searches probe for the effects of new forces or particles beyond the SM, such as alternatives to the Kobayashi–Maskawa (KM) mechanism for CP-violation in the SM, or seek to determine the nature of the generational transitions.

It is the latter, low-energy path which is of concern to us here. A comprehensive discussion of the panoply of opportunities presented in this approach is beyond the scope of this report and has, in fact, already been given in publicly available documents directly related to our deliberations.[2–4] Further information may be found in other recent reviews.[5–12] The greatest opportunities for fundamental breakthroughs would seem to be associated with the variety of experiments possible in the study of rare K-decays, and most of our discussion will deal with these. These sorts of

† These arguments all stem from various perceived inadequacies of the scalar "Higgs sector," required in order to give particles mass without breaking gauge invariance and renormalizability. They result from problems of self-consistency associated with naturalness, triviality, or possible new strong interactions associated with a breakdown of perturbation theory.[1)]

experiments and the theoretical ideas on which they may bear are summarized in Fig. 1, taken from an overview by L. Littenberg. Following some generic remarks on this approach, we shall illustrate the physics issues and arguments which bear most directly on the utility of a kaon-factory, such as the one proposed by TRIUMF, by focusing on those experiments which would appear to benefit most from such a facility. The reader requiring greater detail or more inclusive discussion will find it in the aforementioned references.

B. RARE K DECAYS

The sensitivity of a search for a forbidden decay frequently quoted in the context of a hypothetical Born approximation amplitude involving the exchange of some new type of particle between quark and leptons, although other possible mechanisms could of course be imagined. The amplitude for this is of order $g^2/M^2 \equiv \Lambda^{-2}$, where M is the mass of the exchanged particle and g represents the coupling strength. Thus a given experimental limit may be interpreted as a limit on Λ or M, the latter being of course dependent on what is assumed for g.[‡] In any case, for a given experimental limit on the decay rate Γ, $\Lambda \propto \Gamma^{-1/4}$. So one general observation is that an order of magnitude improvement in the experimental sensitivity results in only a small improvement in the momentum scale being probed ($\lambda \rightarrow 10^{1/4}\Lambda \approx 1.8\,\Lambda$). Unless one has strong theoretical reasons to anticipate an observable signal from the improvement, or unless the increased sensitivity is relatively inexpensive, one may not wish to invest money or effort in such a proposal. In this context, we might comment on the great interest and enthusiasm for the BNL program exploring rare K-decays. This field had been rather dormant for nearly a decade, and experimenters recognized that, because of technological advances in both detectors and data-processing, it was now feasible to explore many decay channels with a sensitivity down to branching ratios of order $10^{-11} - 10^{-12}$, exceeding previous limits in most cases by four to six orders of magnitude. In previous terms for forbidden decays, this corresponds to mass scales Λ on the order of 10's or 100's of TeV. While BNL has the most comprehensive program in this area, important experiments of this variety are being carrying out at LAMPF, KEK, FNAL, and CERN as well.

It appears that proposals for high-intensity kaon facilities are unlikely to improve upon the BNL limits on forbidden decays by nearly so much, at best by factors of 10 to 100, so in this sense, proposed kaon-factories will have a more difficult time. However, as we shall discuss further below, there are other reasons for wanting to go

‡ Recently, a limit is quoted on M with the qualification that g be of order of one or the other electroweak gauge couplings, in which case $M < \Lambda$. However, if the process occurs at a scale at which the particles are strongly interacting, as in technicolor or other constituent or bound-state models, then one might imagine $g^2 \approx 4\pi = \mathcal{O}(10)$ so that $M > \Lambda$. Both conventions have been used in the literature, so clearly the mass scale being probed depends to some degree on theoretical prejudices.

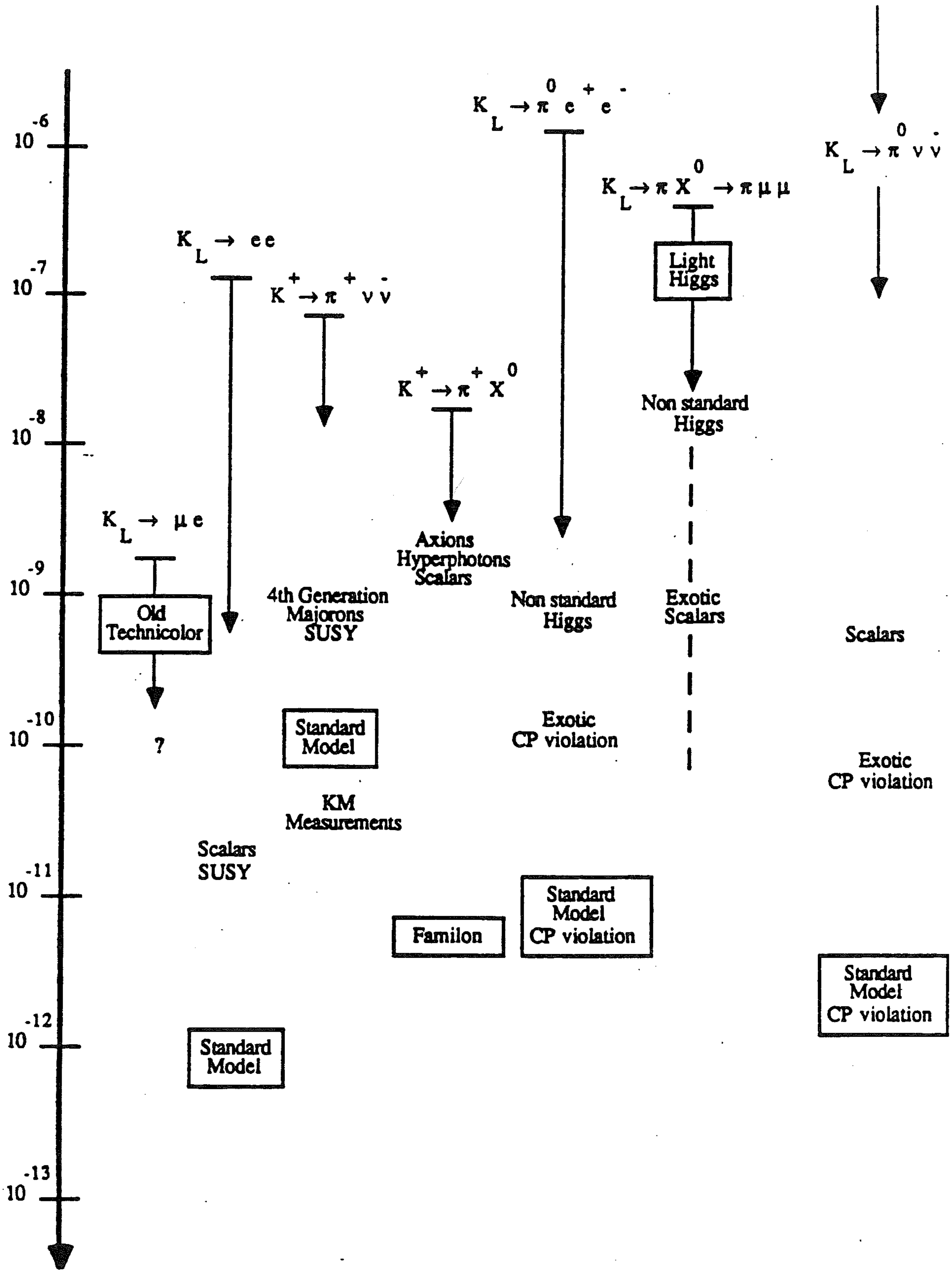

Figure 1: Rare and Forbidden Decays

one or two orders of magnitude beyond th anticipated BNL sensitivity. * The point is that, unless they discover a signal when none is anticipated, searches for forbidden, unexpected decay modes may profit only marginally by an order of magnitude increase in sensitivity. In contrast, those models in which a signal is expected or recorded may profit enormously by an increase in statistics by a factor of 10 – 100. In the time-honored tradition, this may permit ever more sensitive tests through studies of differential decay distributions and measurements of polarization or interference effects rather than just the relatively few events needed to quote a total rate.

Decays forbidden in tree approximation in the SM but allowed at the one-loop level frequently have branching ratios of the order of $10^{-10} - 10^{-11}$, so the existing or anticipated experimental sensitivity is well-matched to the theoretical expectations, providing an important way of probing whether radiative corrections behave as expected by the SM. Assuming however that no rates are discovered above the level expected by the SM, this "window-of-opportunity" for the entry of new physics in certain channels will be closed by the time the presently planned BNL program is concluded. The predictions of the SM are frequently somewhat uncertain because they depend on things like unknown Kobayashi–Maskawa mixing angles, the mass of the top quark, and matrix elements that are not precisely calculable due to non-perturbative "long-distance" effects (such as the hadronic wave functions). Of these, the first two will be gradually improved by further experimentation and by the eventual discovery of the top quark.

This third uncertainty, which goes beyond perturbation theory, presents a formidable theoretical challenge which is likely to improve gradually by continual efforts at lattice gauge simulations of weak matrix elements,[13] but it is hard to predict at this time just how soon these effects will be calculated reliably. This does not necessarily mean that this class of observations becomes uninteresting for several reasons:

(1) In principle, new physics now may interfere with SM amplitudes, so the sensitivity to non-SM contributions may behave as Λ^{-2} rather than Λ^{-4}. How useful this observation is will depend in detail on the degree of uncertainty associated with the theoretical predictions; this must be discussed on a case-by-case basis. A good example is $K^+ \to \pi^+ \nu\bar{\nu}$, for which the SM prediction is thought to be quite reliable.[14]

(2) While the absolute magnitude of these decay rates may be uncertain, certain features associated with polarizations or angular distributions are unambiguous signatures of new physics. Determining the polarization or binning data to test angular distributions may profit enormously from an increase in statistics by one or two orders of magnitude beyond the few events which may be used to establish the level of the branching ratio.

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* Here we have in mind the anticipated sensitivity of BNL with the booster facility now under construction. A "stretcher" facility which would provide experiments with additional intensity, yield additional improvements in sensitivity by a factor of 2.5 or more.

As another, experimentally more difficult, example, the longitudinal polarization of the muons in $K_L \to \mu^-\mu^+$, could be much larger than the SM expectation of 10^{-3}.[15] Limits on these quantities, as with forbidden decays, place limits on the strength and kind of permissible non-SM physics.

Another example which has received a great deal of theoretical[16] and experimental[17] attention recently is the reaction $K_L \to \pi^0 e^- e^+$. The current experimental limit[17] is about 4×10^{-8} whereas the expected SM branching ratio for this is on the order of 10^{-11}–10^{-12}. While there remains a window for new physics, the theoretical situation in the SM is presently unsettled. It seems that the CP-conserving amplitude and the CP-violating amplitudes may be comparable and that the CP-violating amplitude receives approximately equal contributions from a "direct" decay amplitude and an "indirect" amplitude due to the usual mixing ($\propto \epsilon$) between K_1 and K_2 CP-eigenstates. In principle, an examination of the Dalitz plot and a study of the $K_S - K_L$ interference can distinguish CP-conserving (2γ or Z^0) modes.

The search for $K_L \to e^- e^+$, with an expected branching ratio of 10^{-11}, provides another sensitive test of the strangeness-changing neutral current.

C. PRESENT STATUS AND FUTURE OUTLOOK

At the present time investigations of the rare decay modes of K mesons are vigorously proceeding, at BNL and KEK. This round of experiments which began in the early 1980's was undertaken to study physics beyond the Standard Model, and will be completed in about two years. We estimate the following branching ratio sensitivities (90% CL) will be achieved:

$K^+ \to \pi^+ \mu^+ e^-$	1.5×10^{-10}
$K_L^0 \to \mu e$	5×10^{-11}
$K_L^0 \to e^+ e^-$	2×10^{-11}
$K^+ \to \pi^+ \nu \bar{\nu}$	10^{-9}
$K_L^0 \to \pi^0 e^+ e^-$	10^{-9} .

While all these experiments will fall short of their design goals by about an order of magnitude, they will have achieved significant improvements over previously published limits. More important, perhaps, is the knowledge gained in the course of these undertakings which will allow more sensitive experiments to be done in the next round.

Among the factors which have limited the sensitivity of the present experiments are high rates through the detectors resulting in difficulty to trigger efficiently and spurious tracks through the apparatus, insufficient beam intensity, limited detector acceptance, and higher instantaneous rates than expected due to microstructure in the beam spill. In all cases the experimenters have indicated confidence that these problems can be overcome with modifications to their detectors and beams, and since the AGS booster will be coming online, beam intensity problems will also be alleviated. Thus, it is not unreasonable to anticipate that sensitivity to these decay modes could be increased by an order of magnitude in the next round. If a stretcher is completed, another factor of at least three will be realized. With experiments performed in this optimistic scenario we might expect to see the following sensitivities by the late 1990's:

$K^+ \to \pi^+\mu^+e^-$	10^{-12}
$K_L^0 \to \mu e,\ e^+e^-$	10^{-12}
$K^+ \to \pi^+\nu\bar{\nu}$	5×10^{-11}
$K_L^0 \to \pi^0 e^+e^-$	10^{-11} .

With attention focused on rare K decays, the potential to re-examine other K decay phenomena has been increased. By-product measurements acquired during the above experiments have already, or will soon be made, *e.g.* studies of $K^+ \to \pi^+e^+e^-$ with significant statistics, and searches for the decays $K^+ \to \pi^+\mu^+\mu^-$ and $K_s^0 \to \pi^0e^+e^-$. There is also a program beginning at Fermilab to search for $K_L^0 \to \pi^0e^+e^-$ which should be competitive with the BNL and KEK experiments, and which will improve the measurements of K_L^0 and K_s^0 to $\pi^0\gamma\gamma$.

With an upgraded K^+ beam which is presently on the drawing board at BNL, two orders of magnitude improvement in statistics could be achieved in a measurement of the CP violating component of polarization in $K^+ \to \pi^0\mu^+\nu$. If systematic effects can be held to an adequate level, this would result in a limit on $\mathrm{Im}\,(\xi) < 10^{-4}$. Also two order of magnitude greater statistics could be realized for K_{e4}, which would allow a significantly improved examination of the CP violating distributions in this decay. We do not know of proposals to make these measurements, but the potential will exist.

In the above description we see a significant increase in sensitivity to rare K decays, and a potential for new measurements to be made by the middle 1990's with existing facilities. What then is the outlook for the future? At first glance one might think that all windows of opportunity in this area would be closed, a view held by some who spoke with our committee. We, however, have come to a different conclusion, and feel that opportunities will still exist.

To organize our thinking we have grouped these experiments into three types. The first is those which will surely profit from an improved, higher flux facility where another order of magnitude would be realized; in particular, experiments where a signal is expected in the Standard Model. Even in our optimistic scenario the search for $K^+ \to \pi^+\nu\bar{\nu}$ will only obtain tens of events; having hundreds will allow a real study of the decay mechanism. Since the Standard Model component of this decay is so suppressed, interference effects with non-Standard Model physics can become significant, but will be unobservable with insufficient statistics.

Likewise, only a few $K_L^0 \to \pi^0e^+e^-$ events will be observed with existing facilities. Since there are theoretical uncertainties in the relative size of CP violating and CP conserving amplitudes for this decay, studies of kinematic distributions and of the CP conserving (but rare) decay $K_s^0 \to \pi^0e^+e^-$ will have to be made before understanding is achieved. Also, collecting a larger sample of $K \to e^+e^-$ events will provide new tests of $\mu - e$ universality, as well as further tests of the Standard Model.

A second type of experiment is one which searches for decays beyond the Standard Model. Here one can set new limits, but the theoretical interest will not be great unless these limits are significantly better than the old ones. A factor of 10 – 20 should be achievable with the new facility, but is it worth it? It certainly would be if something were discovered. This type of experiment is sort of a crap shoot, and the profitability is hard to assess. We include in this class $K_L^0 \to \mu e$ and $K^+ \to \pi^+ \mu e$.

The third type is those experiments where systematic effects are or will be the dominant factor at existing facilities. A benefit of a higher flux facility is that one can develop beams of higher quality but with intensities comparable to those of present beams. This, and other ideas which will evolve with new technology and a dedicated community should provide the capability to yield improved results. For example, we include in this type $K^+ \to \pi^+ \mu\nu$ polarization and studies of CP violations in $K^\pm \to 3\pi$ ratios and distributions.

D. NEUTRINO STUDIES

Neutrino beams can also provide windows to new physics, and generally these experiments could benefit from the increased flux available at a high-intensity hadron facility like KAON. Searches for neutrino oscillations test for neutrino masses and mixings. Precise studies of the ratio of elastic scattering $\nu_\mu e^- / \bar{\nu}_\mu e^-$ provides a sensitive measure of $\sin^2\theta_w$. This reaction is free of the complications associated with deep inelastic scattering from nuclear targets and is a natural proposal for the high intensity hadron facility. High precision experiments, together with a comparison with the anticipated precise measurements of M_Z from SLC, LEP-I, and the TeVatron and future measurements of M_W from at the TeVatron and LEP-II, can provide sensitive tests of weak radiative corrections in the SM.[18] Depending on the results, such tests may either verify SM predictions or provide insight into extensions of the SM.

For these examinations of electroweak phenomena, indeed for the entire experimental program proposed for a new facility, similar situations to those in K physics exist. For example, there is a new program proposed to search for ν_μ disappearance at BNL. It is a 10 km baseline, double detector that will use the full intensity of the AGS with booster. It will have the sensitivity of eliminating about 1.5 – 2 BJ's from the $\sin^2(2\theta) - \delta M^2$ plot. (A BJ is an area of the plot equal to one order of magnitude in both dimensions and was suggested by Bjorken as the minimum area to justify a new experiment.) Thus, a new facility will have a reduced window from that which is presently available, but will still be able to further the search.

For a new ν_μ to ν_e oscillation experiment, however, while the BNL effort will explore this, sensitivity is already being limited by systematic effects. This experiment falls into our third type.

The Committee was also told about possibilities of neutrino experiments with low energy, intense neutrino sources. These are proceeding at LAMPF and include $\nu_\mu e$ elastic scattering, measurements of electromagnetic properties of neutrinos, and oscillation searches. A continued program in this area is complementary to studies with higher energy facilities and will surely increase our understanding of neutrino physics in a significant way.

E. SUMMARY

To sum up, studies of the predictions of the SM in these ways would be very desirable supplements to the high energy studies underway and anticipated for the SSC. There are clearly interesting opportunities for elucidation of the SM at a kaon-factory, even taking into account the great strides which are being made in BNL, FNAL, KEK, LAMPF and CERN experiments. We are only just beginning to discriminate the mechanism underlying CP-violation, 25 years after its initial discovery. New experimental information about its nature, (there is potential here for several different kinds), would be especially welcome, if only to rule out viable alternatives to the SM explanation. It is important to recognize the desirability of exploratory searches as well. These kinds of high-risk (no signal), potentially high-reward (revolutionary consequences if found) experiments will always attract a number of experimenters who are challenged by their difficulty and potential importance. More stringent upper limits constrain theoretical ideas, while any definitive observation of a deviation from the SM would propel this field of experimentation into the "hottest" topic around.

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APPENDIX I

THE DRELL–YAN PROCESS

The Advanced Hadron Facility (AHF) envisioned by Los Alamos would have twice the energy, one-fourth the beam current, and half the duty cycle of the proposed KAON facility. One of the primary reasons, it has been argued, for preferring a 60 GeV facility over a 30 GeV facility is the capability to perform studies of the Drell-Yan process $h + A \to \mu^- \mu^+ + X$ on a variety of targets A with hadron beams h consisting either of a primary proton beam or secondary π and K beams (around 40 GeV.) For this reason, we have attempted to evaluate both the interest in these kinds of investigations and the theoretical basis underlying their interpretation. The purpose of this Appendix is to indicate the physics reasons for wanting to study this process and to discuss the theoretical situation somewhat further than was dealt with in the documents provided our Subpanel. Our conclusion is that, while Drell-Yan experiments may be interesting to carry out, the basis for their theoretical interpretation is murkier than is generally appreciated.

There are several reasons why these reactions offer attractive opportunities to study the quark structure of hadrons and nuclear matter: (1) This process provides the only experimental method of inferring the structure functions for mesons, whose properties could provide new insights into the nature of quark confinement. In addition, studies of the ratio of the kaon to pion structure functions at relatively large quark momentum fractions may provide further inofrmation on the nature of the breaking of SU_3-flavor symmetry. (2) For a given beam, one would like to investigate how the nucleon's structure function varies from one nucleus to another, following on the celebrated EMC effect for deeply inelastic muon scattering,[1] showing that nuclear effects indeed make important changes in the quark distributions. (3) Utilizing a K^+ beam, one preferentially selects the strange quark and up-antiquark content of the target, possibly providing a more sensitive measure of the strange quark distribution than otherwise achievable. This is of particular interest in this in view of recent EMC results on the spin distributions in the proton.

For a variety of experimental and theoretical reasons, there seems to be no disagreement that a 30 GeV primary beam has too low an energy at which to perform or to interpret such Drell-Yan experiments, so we will not review that issue here. We are satisfied that, as argued in the presentations by Los Alamos, a primary beam energy of 60 GeV is approximately the minimum energy at which meaningful experiments of this type may be carried out. However, there are some reasons to think that that even higher energies would be more desirable. In any event, these experiments should be performed. But one must bear in mind that their theoretical interpretation is at this time not clear so that a semi-empirical phenomenology would need to be developed. As we stated in the body of the report, it is our judgment that the case for enabling Drell-Yan studies is not so compelling as to cause us to want to reject a lower energy facility which can support a great many other interesting and important physics experiments.

For brevity and simplicity, we will intially summarize the theory here in terms of a single nucleon target, following subequently with some remarks about nuclear targets and Item 3 above. In Feynman's parton model, the Drell-Yan process is described by the simple formula

$$\frac{d\sigma}{dM_{\mu\mu}^2 dx_F} = \frac{4\pi\alpha^2}{9M_{\mu\mu}^4} \sum_i \frac{e_i^2}{x_1 + x_2} \left[q_i^h(x_1)\bar{q}_i^N(x_2) + \bar{q}_i^h(x_1) q_i^N(x_2) \right] . \tag{1}$$

The probability of finding a quark (antiquark) of flavor i, charge e_i (in units of electron's charge e) in the hadron beam with longitudinal momentum fraction x_1 is denoted by $q_i^h(x_1)(\bar{q}_i^h(x_1).)$ Similarly, $q_i^N(x_2)(\bar{q}_i^N(x_2))$ represent the corresponding probabilities for a target nucleon N. The kinematic variables are the energy-squared, $s \equiv E_{CM}^2 \approx 2M_N E_L$, the invariant mass of the lepton pair, $M_{\mu\mu}$, the pair longitudinal momentum fraction, $x_F \equiv x_1 - x_2$, and $\tau \equiv {M_{\mu\mu}}^2/s = x_1\, x_2$.

For a nuclear target, one must rescale various kinematic relations, but the experimental goal remains to infer the quark structure functions of a nucleus or the ratios of structure functions for different nuclei. In principle, the structure functions for nucleons can be determined in deeply inelastic neutrino, electron, and muon scattering so that, to some degree, one can actually predict the Drell-Yan process for proton or antiproton beams. Being of order α^2, the rate for the Drell-Yan process is inherently small and, because the quark distributions fall off for large x, it becomes progressively smaller as one presses toward larger values of τ. Certain general experimental considerations restrict the desirable beam energy. To avoid muon-pair contamination from decays of ψ-resonances, it is necessary to require $M_{\mu\mu} \gtrsim 4\ GeV$. For a given beam energy, this then places a lower limit on the value of τ which may be studied. For example, to be able to explore values of τ down to 0.1 would require $E_L \gtrsim 80\ GeV$. A competing consideration is that, the higher the energy, the longer it takes to accumulate events, especially at large values of τ. This latter point motivates one to choose an energy no larger than the minimum necessary. It is not clear to us what the optimal energy results from these competing considerations, but it is clear that this process *is* a natural candidate for exploration with a high intensity hadron facility.

Measurements[2,3] of the Drell-Yan process have been made in fixed target experiments from lab energies as low as 40 GeV ($E_{CM} \approx 8.7\ GeV$) for π, K, p and $\bar{p}$ beams to as high as 800 GeV for proton beams at the Fermilab TeVatron. Although there are not so many events, some data has been collected at the CERN S$\bar{p}p$S collider as well.[4] It should be noted that an analogous mechanism is responsible for $W^\pm$ and Z^0 production at the S$\bar{p}p$S with $E_{CM} = 540$ and $630\ GeV$ and at the TeVatron having $E_{CM} \approx 1800\ GeV$, providing important alternative opportunites to check the theory. The results of all these measurements is that the observed cross sections are much larger than would be anticipated by the naive formula, based on knowledge about the nucleon structure functions or models of the pion structure function. The observed cross sections exceed the predictions based on Eq. (1) by about a factor 2-3 in fixed target experiments and a factor of about 1.5-2.0 or so at collider energies. (It is difficult to be too precise as yet because of large uncertainties in the antiquark and sea quark distributions.) For simplicity, this ratio, an *ad hoc* experimental "K-factor," has been frequently *presumed* to be an overall multiplicative factor independent of τ and x_F, a hypothesis which is not inconsistent with data, given the substantial uncertainties of the distribution functions or the limited experimental range of τ.[5]

There are two kinds of corrections to the simple picture embodied in Eq. 1. First, there are "higher-twist" effects associated with the masses and transverse momenta of the constituents of the beam and target. Secondly, there are the modifications introduced by the nature of the strong interactions as embodied in quantum chromodynamics (QCD).

First, there is a need to go to high energy to minimize "higher twist" effects, i.e., corrections due to finite mass or transverse momentum that vanish like a power of $1/\sqrt{s}$. For example, Feynman argued that one needs to avoid a "wee" region having $x \lesssim 2/\sqrt{s}$ (in GeV,) a conjecture supported by more sophisticated theoretical analyses in QCD as well.[6] For a 60 GeV primary beam, this would restrict one to $x \gtrsim 0.19$, while for 40 GeV secondaries, this requires $x \gtrsim 0.23$. If one wishes to be able to compare with Drell-Yan or structure function data acquired at higher energies, it would be desirable to determine structure functions down to smaller values of x. To accomplish this, one might prefer somewhat higher beam energies, since these "higher twist" complications are expected to fall off at least as fast as $1/\sqrt{E_L}$. This may force certain choices between a desire to determine the nucleon's antiquark and strange quark distributions over a large kinematical range and the desire to probe meson structure functions at larger values of x.

Secondly, QCD leads to significant modifications of the preceding "naive" parton model embodied by Eq. 1. The quark distribution functions, here as in deeply inelastic scattering, are not scaling distributions but in fact vary slowly (but predictably) with the resolution scale $M_{\mu\mu} \equiv Q$. This first effect is well-established in deeply inelastic scattering experiments but, while quite important for comparing experiments performed at widely different Q^2, does not account for the K-factor. Furthermore, in QCD, there are other "radiative" corrections due to the nature of the gluon interactions which modify not only the hadronic structure functions but also the primary process by which muon pairs are formed. These include quark vertex corrections, gluon bremsstrahlung, and gluon-quark interactions, whose magnitude is determined by the size of the QCD coupling constant $\alpha_s(Q^2)$ on the relevant scale as well as certain kinematical factors. Indeed, one of the successes of QCD has been the demonstration that the basic Drell-Yan picture is correct[7] while, at the same time, accommodating the much larger cross sections (i.e., K-factors) observed. The $O(\alpha_s)$ corrections have been determined by a number of groups,[8] and the contributions of $O(\alpha_s^2)$ which are thought to be most important have been computed as well.[9] *

These corrections turn out to be very much larger than for many other processes calculable in perturbative QCD, in part because of differences between effects at

* The $O(\alpha_s^2)$ corrections so far include only virtual and soft gluon emision and do not include hard bremsstrahlung for which collinear "singularites" may also lead to enhancements. While these are believed to provide the largest effects, second-order contributions stemming from qg, qq, and gg subprocesses have not yet been calculated.

spacelike and timelike momenta. As an example,[9] for a 200 GeV beam ($E_{CM} = 19.1\ GeV$) and $M_{\mu\mu} = 10\ GeV$ ($\tau \approx 0.3$), the $O(\alpha_s)$ corrections are 83%, and the $O(\alpha_s^2)$ corrections about 62%. Thus the sum of these two terms yields a theoretical K-factor of about 2.5. Earlier indications[10] that first-order effects were sufficient to account for the observed yields may have been fortuitous and, while QCD is successful in understanding that there can be large corrections to Eq. (1), it "is not excluded that this will jeopardize the predictive power of perturbative QCD at low energies."[9] Having a valid theoretical understanding is *not* a prerequisite to carrying out interesting experiments. Indeed, progress in experiment and theory often go hand-in-hand, so it does not follow from these observations that Drell-Yan studies should not be carried out in fixed-target experiments. It does mean however that the standard QCD perturbation theory is not under control and the ability to make inferences about hadronic structure from Drell-Yan data acquired in fixed target experiments is at this time subject to question.

Because $\alpha_s(Q^2)$ diminishes very slowly with increasing Q^2, this is a theoretical challenge which cannot be avoided by modest increases in energy. For example, in the analogous process of Z^0 production by quark-antiquark annihilation at the CERN collider, where $E_{CM} = 540\ GeV$ and $Q \approx 92\ GeV$, the $O(\alpha_s)$ corrections are 43%, and the $O(\alpha_s^2)$ corrections about 18%. Within errors, this accounts for the observed cross section. To remedy this theoretical problem in fixed target Drell-Yan experiments will probably require learning to sum up the most important higher order corrections, much as has been done via the renormalization group in other applications. Certain theorems have been established concerning "exponentiation" certain virtual and leading *log(1-x)* contributions in an effort to represent the dominant effects coming from higher orders.[6] It would seem to require a great many difficult calculations to determine whether these singular contributions which dominate as $\tau \to 1$ will account for the actual variation with τ in the moderate range of τ at which experiments will be carried out, and the numerical work of Ref. [7] suggests caution in assuming this is the case. For example, experiments even at the proposed AHF would generally be limited to $\tau \lesssim 0.6$, and higher energy facilities would be even more restricted.

If, for example, radiative corrections could be shown to approximately independent of τ and x_F, that is if the theoretical K-factor were constant within a determinable error, perhaps structure functions or at least *ratios* of structure functions could be extracted anyway. A kind of lore exists that the QCD corrections, even though larger than the leading term, justifies a constant K-factor because it was found that over the relevant kinematical range, the first-order correction was essentially constant. The logical conclusion, in our opinion, is that the theoretical situation is one in which the naive perturbation theory has broken down, and one cannot be sure what will happen in higher order. While the theoretical results thus far[8,9] do seem to support the notion of a weak dependence on x_F, they do not support independence of τ. For the 200 GeV case, between $\tau = 0.2$ and $\tau = 0.6$, the theoretical K-factor varies quite a bit. While the $O(\alpha_s)$ correction varies by only about 20%, the addition of the second-order corrections magnify this to nearly 40%. In short, while the first-order correction tends to support the notion of a moderately varying K-factor, the second-order correction tends to suggest a much greater variation. Intuitively, one would expect the addition of collinear bremsstrahlung and even higher order corrections to convey additional τ-dependence, as the relation between the momenta of the annihilating $q\bar{q}$-pair and the momenta of the partons in the hadrons becomes ever more convoluted and indirect. In any case, until a better theory exists, it is not clear how reliably structure functions may be extracted.

There is some hope that, in ratios of doubly-differential cross sections with the same beam but different targets, many of the radiative corrections to the K-factor might cancel out. This is a very interesting question and is, of course, the relevant issue for the interpretation of Drell-Yan data on different nuclei directly in terms of nuclear structure functions. At present, there is insufficient theoretical understanding to allow one to state the extent to which this hope may be realized, although some first-order calculations tend to be supportive.[12] It is certainly the case that factorization can be experimentally tested by testing whether ratios of $d\sigma/dx_1dx_2$ for different targets are independent of x_1. Over the range where this has been tested, the data is rather encouraging.[11] Moreover, the dependence on x_2 seems to reflect the sort of nuclear effects on quark distributions first observed in deeply inelastic scattering,[1] This is the point-of-view taken in the Los Alamos proposal. Further support for this may be found from the fact that the observed effects cannot be ascribed to QCD radiative corrections,[9] to the extent that they are now known.

It is clear from the above that their overall magnitude is such that one can have little confidence in the validity of QCD perturbation theory as currently practiced. The theoretical results so far discourage one from applying perturbative QCD below collider energies or from simply assuming that the K-factor is independent of τ. Given the magnitude of the QCD corrections, it is not so clear whether the subtle changes due to nuclear effects can be interpreted directly in terms of quark distributions. Until the theory is improved, it is going to be exceedingly difficult to disentangle complications coming from radiative corrections and modifications to structure functions themselves.[12] Some of these effects are probably more cleanly addressed in deeply inelastic scattering experiments.[13]

In summary, to determine the optimal energy at which the Drell-Yan process may be exploited for the study of the quark and gluon structure of hadrons and nuclei is not an easy task and requires a combination of systematic data accumulation together with theoretical progress and analysis. This effort will benefit from gathering additional dimuon data at existing facilities at FNAL, CERN (and hopefully also from RHIC a few years hence) together with structure function measurements in deeply inelastic scattering. At least until the Drell-Yan theory is substantially improved, comparisons of the structure functions for different nuclei will probably be most reliably done in deeply inelastic scattering experiments with electron, muon, and neutrino beams. Nevertheless, for the unique properties outlined previously, the Drell-Yan process is likely to remain of interest, for it is certainly more directly sensitive to the the quark properties of hadronic structure than most other processes initiated with hadron beams. Although significant theoretical progress is required, the issues are at least well understood, and there is the potential that, in time, the theory will improve and enable more precise inferences to be drawn. One needs to be able, given the structure functions determined through deeply inelastic scattering, to predict the Drell-Yan cross section accurately. Only then will we be able to judge whether the reverse can also be done, viz., whether given the Drell-Yan data, structure functions may be reliably inferred. In conclusion, an experimental program to study the Drell-Yan process is likely to prove interesting and to provide incentives to the development of a better theory, but it seems that further work would be desirable to determine the optimal parameters of energy and intensity to obtain the most useful and easily interpretable data.

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APPENDIX II

QUESTIONS ASKED BY THE KAON SUBCOMMITTEE FOLLOWING MEETING ON OCTOBER 16

I. THE PHYSICS POTENTIAL OF THE PROPOSED KAON FACILITY — THE PHYSICS REASONS FOR HIGH INTENSITY AND ENERGY

A. Weak Interactions

BNL:

1. What can BNL achieve with the present facility plus the booster under construction?

2. With the stretcher?

LANL and TRIUMF:

3. What improvement can be achieved with the increased intensity? Can all the intensity be used in view of the detector and target problems?

   Analyze specific weak decays: $K_L^0 \to \mu e$, $K^+ \to \pi^+\mu^+e^-$, and $K^+ \to \pi^+\nu\bar{\nu}$.

4. What are the advantages of high intensity and energy for the study of neutrino properties?

ALL:

5. What features of the standard model will be tested with your facility?

6. In what way will these results complement those to be obtained at CERN and Fermi Laboratory.

B. Strong Interactions

LANL and TRIUMF:

7. What properties of QCD will be studied with this facility? The Drell–Yan process is suggested as a central element of the hadron physics to be studied with these new facilities. What beam energy and intensity would be optimal?

8. What are the experiments which would be especially attractive to the nuclear physics community?

ALL:

9. Are there plans to develop detailed designs and simulations of benchmark experiments in the kaon/AHF research programs in order to provide more accurate assessments of problems, costs and resources required? Is so, could these be described and at what phase of the planning and construction would they be performed?

II. CONSTRUCTION

BNL:

1. When will be booster be completed? What is the time schedule for the stretcher? How much R&D is needed? Cost of R&D?

LANL and TRIUMF:

2. R&D time required? Schedule for construction?

ALL:

3. Manpower requirements for construction including R&D. Break down into accelerator physicists, research physicists, staff, technical and non-technical. How big an addition to present staff?

4. What mechanism is there for a technical review? When would it occur?

5. Costs: R&D? Construction? Time scenario for these expenditures?

III. DESIGN PROBLEMS

TRIUMF:

1. The TRIUMF cyclotron appears not to be the optimum choice as injector for an advance hadron facility. Extraction of the H^- beam is difficult, beam emittance and extraction efficiency are not the best and an accumulator ring must be interposed between the injector and the first booster ring. A clean technical solution would be a dedicated short pulse proton linac operating at 50 Hertz. What savings are realized by using the TRIUMF cyclotron? Over the long term would the operating simplicity of a linac-based design result in substantial savings?

TRIUMF and LANL:

2. An update on progress on RF and instability accelerator problems as well as the stability of the slow extraction process. Substantial disagreement exists over what constitutes an acceptable level of beam loss in slow extraction of the primary beam from the stretcher ring. What is the justification for the quoted numbers?

BNL:

3. In a booster plus stretcher scenario of $10\,\mu a$, what is the proton intensity per pulse in the machine and how near is this to the space charge limit on injection from the booster? What are the losses in the AGS during injection and acceleration and upon injection into the stretcher? What are the losses in the stretcher during injection and extraction?

IV. EXPERIMENTAL AREAS

ALL:

1. How many beam lines will be needed to exploit the facility? What is their function? With respect to K-beam lines what is the expected K/π ratio? What spectrometers and detector facilities will be needed? What are the floor area requirements? Costs (beam lines, spectrometers and detectors)?
2. Problems associated with high intensity: Have designs for targets and detectors which can handle the high intensity and high energy been considered? What plans to you have for developing remote handling equipment?
3. What are your estimates of the costs for detectors special targets and remote handling equipment? Have these been included on your proposal estimate of project cost?

BNL:

3. What are the costs of detectors with the present facility?

V. FUNDING

ALL:

1. Construction funds: Current status and expectations.
2. Operating funds: What increase in operating funds will be required?
3. After the facility is in full operation, how many hours per year would the facility be available for experiments?

BNL:

4. What are the operating costs and the corresponding number of accelerator hours devoted to kaon experiments with the present facility?

TRIUMF:

5. What is the relative proportion of operating costs, detector and spectrometer and target costs to come from Canadian sources?

VI. POLICY ISSUES

BNL:

1. With RHIC operational, what would be the running schedule for the proton users? How often and for what period would one need to fill the RHIC? Would there be any restriction on proton energies? What would be the cost differential between running RHIC alone and running the AGS for protons along with RHIC?

VII. USER COMMUNITY

ALL:

1. What is the estimated size of the user community which would be interested in performing experiments at your proposed facility? Please break down this group by country of origin. How many from universities? What components of this group are currently using your facility?

APPENDIX III

AGENDA — KAON SUBCOMMITTE MEETINGS

October 16, LAMPF Auditorium

9:30	Executive session with D. Hendrie and J. P. Schiffer
10:30 – 12:00	Presentation of Canadian kaon proposal
12:00	Lunch
1:00	Canadian proposal continued
2:30 – 4:40	Presentation of BNL proposal

October 17, Orange Box Auditorium

9:00 – 10:30	Executive session to discuss two proposals
10:30 – 1:00	Open
1:00 – 3:30	Presentation of LANL proposal
3:30 – 4:00	S. Hanna, LAMPF users
4:00	Further discussion

December 9, Brookhaven National Laboratory

9:00	Committee meets in executive session
9:30 – 11:00	BNL's response to committee questions
11:00 – 12:30	LANL's response to committee questions
12:30 – 1:30	Committee — Working Lunch
1:30 – 2:00	BNL users — A. J. S. Smith
2:00 – 2:45	Tour — AGS experiments
2:45 – 4:15	TRIUMF's response to committee questions
4:15	Committee meets in executive session

January 7, TRIUMF

8:30 – 12:00	TRIUMF presentation User Issues — S. Page
12:00 – 12:30	BNL
1:45 – 3:15	LANL
3:15 – 6:00	Executive session of Kaon Committee: Questions for agencies and NSAC
7:00	Dinner at Faculty Club

January 8, TRIUMF

All day	Executive session of Committee