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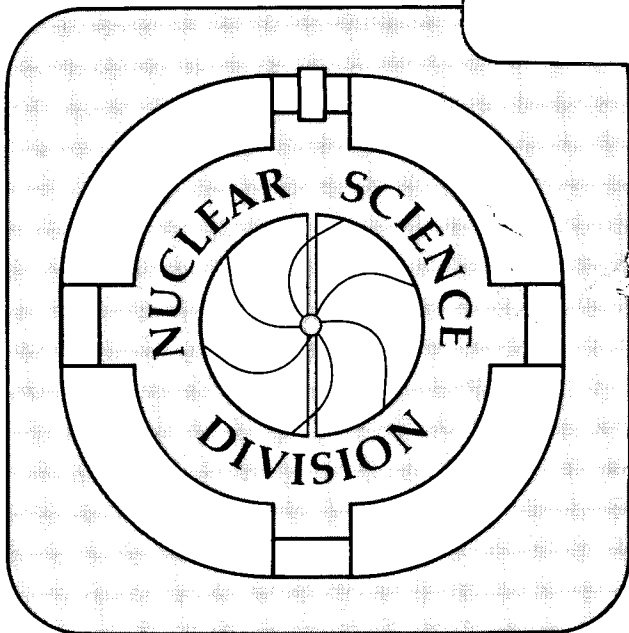
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## Heavy Ion Physics Challenges at Bevalac/SIS Energies

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Heavy Ion Physics Challenges at Bevalac/SIS Energies

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## Heavy Ion Physics Challenges at Bevalac/SIS Energies\*

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At the end of the 8<sup>th</sup> High Energy Heavy Ion Study, it is appropriate to try to put the future physics challenges in this field into perspective. What important milestones have we passed? What experiments and theoretical developments are needed in the next few years to optimize convergence toward our long term objectives? Finally what frontier problems can future high intensity machines (SIS-18/ESR or the Upgraded Bevalac after 1990) address?

First, we recall that the fundamental physics goals of this subfield of nuclear physics can be broadly categorized as follows:

1. Establish constraints on the *thermodynamic* ( $P, S, W$ ) and *transport* ( $\eta, \kappa, \xi$ ) properties of nuclear matter over as large a domain as possible of densities, temperatures, neutron/proton ratios, strangeness concentrations, etc.
2. Understand quantitatively the elements of the nuclear reaction mechanism (clustering, fermi motion, mean fields, effective transport cross sections, nuclear disassembly, multifragmentation, etc.) and their effects on specific observables (double and triple differential cross sections, pion and kaon excitation functions, global flow variables, etc.).
3. Produce nuclei near limits of stability ( $Z/A \rightarrow 1/3, L \rightarrow 100\hbar$ , Strangeness  $\rightarrow -N$ ) and search for novel states of nuclear matter (density isomers, condensates, etc.).

The first ten years of exploration in this field, up to the previous heavy ion study[1] in 1984, resulted in major progress in the second area and extensive (though negative) searches for exotic and anomalous nuclear excitations. During that period only "light" ions ( $A < 100$ ) were available as projectiles. While such light ion experiments were absolutely essential for helping to sort and clarify the complex reaction mechanisms[2], tangible progress on the primary goal had to await experiments with truly heavy ions ( $A > 100$ ) and the simultaneous development of detailed non-equilibrium nuclear transport theories including mean fields. With the development of  $4\pi$  detectors[1], especially the Ball/Wall and streamer chamber, global event analysis revealed unambiguous evidence for collective nuclear flow for the first time[3] in 1984. However, since nature made nuclei only a few mean free paths thick with diffuse nuclear surfaces, it was found that the nuclear flow was considerably weaker than first predicted by ideal non-viscous hydrodynamics[4]. The difficult task of extracting constraints on the nuclear equation of state from such data on nuclear flow thus had to await the development of Vlasov - (Pauli blocked) Boltzmann (the so called VUU, BUU, or BN) transport codes[5,6] that could address realistically the important non-equilibrium aspects of the problem.

At the last meeting[1] the first tentative attempts to deduce the stiffness of the nuclear energy function,  $W(\rho, T = 0)$ , were discussed on the basis of the above major developments in both theory and experiment. In the meantime, there has been an impressive series of further developments that were reported in this meeting. The main results pertaining to the primary goal were as follows:

1. Stroebele showed new streamer chamber results[7] based on an improved Danielewicz flow analysis restricted to deuteron fragments. They found the largest in plane flow momentum yet observed,  $p_x \approx 150$  MeV/c, in the asymmetric system Ar+Pb at 800 AMeV. For this system, the Cugnon cascade predictions do not even reach 50 MeV/c giving further indications that mean field dynamics are important for understanding the magnitude of the flow momenta. They also confirmed the more modest flow momenta in La+La of  $p_x \approx 80$  MeV/c for more peripheral collisions at the same energy. For such peripheral collisions the Cugnon cascade results are much closer ( $\sim 65$  MeV/c) to observation. This indicates that while the "corona" physics is adequately described by simple cascade without mean field effects, central collisions leading to high densities are not.
2. Harris[10] showed new Ball/Wall results on the dependence of flow on fragment mass,  $A$ . These new data show that heavier fragments are correlated in azimuthal angle closer to the reaction plane than lighter fragments are. Furthermore, heavier fragments  $Z \geq 6$  reveal systematically larger in-plane flow momenta per baryon. A quantitative analysis based on QMD[8,9] by Peilert[11] was able to account for the fragment  $A$  dependence of the flow only with an assumed "stiff" ( $K=400$  MeV) equation of state. The momentum dependent "soft" equation of state with in-medium reduced cross section ( $\sigma_{eff}/\sigma_{NN} = 0.7$ ) could only achieve about a third of the observed in-plane momenta.
3. Kampert[12] showed new Ball/Wall data on transverse radial flow in Au+Au. He observed that p,d and t kinetic energy spectra at 90 degrees in the center of mass were virtually identical, in contrast to what was expected if radial flow were present.  $He^3$  and  $He^4$  on the other hand exhibited higher average transverse energies, but the most peculiar result was that  $He^3$  had larger transverse energy than  $He^4$ . These results indicate that radial flow, at least in the simple form first suggested by Siemens and Rasmussen, is not achieved in nuclear collisions. The absence of radial flow can be directly attributed to the importance of viscous effects in nuclei. At the previous meeting[1] Kapusta predicted the absence of radial flow through calculations based on the Navier-Stokes equation. This result together with the relative smallness of directed ( $p_x$ ) flow confirm that the nuclear fluid is very viscous, as expected[13].
4. Keane[14] showed a detailed flow analysis of U+U at 900 AMeV. He observed  $p_x \approx 80$  MeV/c as did Stroebele and showed detailed calculations based on the Frankfurt VUU confirming that cascade leads in this case to only  $\sim 60$  MeV/c. However, the VUU results filtered with the streamer chamber acceptance best fit the observations with an assumed "soft" ( $K=200$  MeV) equation of state. The "stiff" equation of state, on the other hand, produced systematically larger flow momenta than observed in this case. This analysis is thus inconsistent with that of Peilert[11], who found that fragment flow[10] required a stiff EOS.

While the above data significantly extend the flow data base, it is clear that consensus on the form of the nuclear equation of state has not yet been reached. At present, the effective compressibility of dense matter remains uncertain to a factor of two ( $K = 200 - 400$  MeV).

It is easy to identify several obstacles that hinder the convergence rate toward narrower constraints on the nuclear equation of state.

1. The momentum dependence of the mean field.

2. The uncertainties associated with  $4\pi$  experimental filters.
3. The uncertain density and temperature dependence of effective transport cross sections.
4. The absence of self-consistent calculations of the equation of state with present nuclear transport models.
5. The uncertain  $\Delta_{33}$  dynamics and pion absorption mechanisms at high densities.

Last year G. Brown[15] suggested that the apparent stiffness of the equation of state needed in VUU calculations to fit flow data was due to the neglect of the momentum dependence of the nuclear forces. As Brown pointed out at this meeting also, the nuclear mean field involves a cancellation between an attractive scalar field,  $\sigma$ , and a repulsive vector field,  $\omega^0$ . In the initial phase of a nuclear reaction, momentum space consists of two separated Fermi spheres and thus the vector field is enhanced by a Lorentz boost factor,  $\omega^0 \rightarrow 2\gamma_{cm}\omega_F^0$ , where  $\omega_F^0$  is the vector field value in the ground state. The scalar field is of course invariant to boosts and thus the cancelation between the vector and scalar is reduced in favor of the repulsive vector. This could lead to an apparent hardening of the equation of state. Indeed, detailed calculations[6,9] revealed that the directed flow momenta,  $p_x$  differed by only  $\sim 10\%$  between soft, momentum dependent forces (SM) and hard, momentum independent forces (H).

However, new calculations[11,17] discussed by Stöcker now indicate that the above effect may be too small to explain quantitatively the stiffness needed to fit flow observations. The point is that the momentum distribution of the nucleons is thermalized rapidly due to the large nuclear stopping power at these energies. Thus, the initial free streaming momentum distribution changes rapidly into an approximately thermal one for which no extra gamma factors appear. The calculations of Rosenhauer[17] for Au+Au 800 AMeV showed that up until  $\sim 15$  fm/c the momentum dependence of the force indeed causes the in plane  $p_x$  to increase much more rapidly than that caused by a momentum independent stiff force. However, the final value of  $p_x$  is reached only at later time  $\sim 30$  fm/c long after free streaming is over. The  $p_x$  reached at 15 fm/c was found to be less than 1/4 of its final value. Most of the final  $p_x$  is apparently generated after equilibration. The results of the new study indicate for this system that  $p_x \approx 75, 87, 115$  for S, SM, and H respectively. It was not clear why the difference between the SM and H calculations are twice as large as in previous calculations[9], but if these results hold up to future scrutiny, then the apparent contradiction would remain between the softness of the equation of state needed to blow up supernovas and the stiffness needed to explain nuclear flow.

As emphasized by Glendenning[16], in addition to unresolved problems connected with nuclear transport analyses, the problem could lie on the astrophysical side because supernovas provide constraints on the nuclear equation of state only if the prompt mechanism for the bounce is assumed. If Supernova87a liberated less energy than the 1.8 FOE assumed, then neutrino transport could explain the observations. In that case no constraint on the nuclear equation of state would be provided by supernovas and the contradiction with flow data would also disappear. In any case, it is important to keep in mind that the astrophysical constraints are not free of ambiguity either.

The second obstacle in the way of convergence to the equation of state is less basic but practically perhaps more formidable. A generic problem with trying to compare calculations with data taken using complex  $4\pi$  detectors is that the trigger conditions defining

a particular class of events are often difficult to simulate. Therefore, the theoretical uncertainties associated with using a particular “filter” to simulate experimental biases and acceptances are difficult to assess. Calculations typically produce exclusive events at fixed impact parameters. Experimentally on the other hand only the multiplicity and the momentum distribution in a limited region of phase space can be measured. In the literature this has led to learned theoretical debates over which acceptance filter most closely resembles the actual experimental situation. Since the “filtered” values of  $p_x$  differ typically reduced by a factor of two from their values assuming perfect acceptance while the variations of  $p_x$  resulting from large variations of the equation of state is typically less than 50%, this problem is clearly very important to resolve. So what can be done?

In the future, further progress can be made only by simplifying the trigger conditions necessary to constrain the range of impact parameters and the fluctuations of the reaction plane azimuth. At this meeting, Fai discussed several techniques[18] that could simplify the definition and analysis of triple differential cross sections. Next year Madey et. al. will test one of the proposed ideas involving a time-of-flight wall to measure neutron triple differentials. It is important to remember what a triple differential cross section,  $\sigma(E, \theta, \phi - \phi_R, M)$ , really is. It is a *one* body momentum distribution in three dimensions where the azimuthal angle is measured relative to an estimator,  $\phi_R$ , of the true reaction plane and an estimator,  $M$ , e.g. multiplicity, of the magnitude of the impact parameter. Double differentials are simply those one body distributions without a  $\phi_R$  estimator. Of course both  $\phi_R$  and  $M$  require a multiparticle detector. However, it is most important that those estimators are well defined and easily simulated. The precision of the estimators as an impact parameter meter is of secondary importance. In the long run, a new  $4\pi$  device such as the electronic streamer chamber (TPC) proposed by H. Wieman will be required to carry out a full program of one and two particle triple differential measurements. However, in the near future a careful optimization of the  $\phi_R$  and  $M$  meters is very much called for.

The third obstacle listed above is the most challenging at present. In the past several years there has been substantial progress on implementing realistic mean field dynamics in transport codes[5,6,8,9]. We note also that a self-consistent treatment of the momentum dependent Vlasov equation including quantum corrections has been formulated in Ref.[19] based on Walecka’s Quantum Hadrodynamic Theory (see also contribution by Ko). However, a satisfactory self-consistent treatment of the medium modified Boltzmann part of the transport equations has not yet been formulated. In the past year Malfiet and co-workers have begun to address this problem and have shown[20] that Pauli blocking of *intermediate* states could reduce the effective cross sections in equilibrated systems by 30%. At this meeting, Brown pointed out that density dependent polarization effects can on the other hand even enhance the effective cross sections. Recall the pre-critical scattering[21] phenomena that could arise in dense matter. In extreme cases the NN cross section could be enhanced by a factor of two or more. In actual dynamical situations, it is conceivable that the effective cross sections could start out larger and end up smaller than the free space ones. Thus the transport properties of the system could depend dramatically on time and may have to be calculated self-consistently! What makes the problem of determining the effective cross sections particularly difficult is that unlike the mean fields, which can depend only on the  $(\sigma, \omega^\mu)$  fields (in spin-isospin symmetric matter), the cross sections involve the exchange of fields with all possible quantum numbers. In particular, while the mean pion field is zero, pion exchange is the dominant contribution to higher partial waves. Thus, a complete theory of the collision term will require as starting point a QHD theory including pions and rho mesons.



The importance of developing a self-consistent theory of effective cross sections was strongly emphasized during this meeting. New calculations[23] reported by Stöcker for directed flow ( $p_x$ ) using Navier-Stokes with the realistic[13] transport coefficients confirmed the expectation that viscous effects are very large. In fact,  $p_x$  was reduced by a factor of two relative to the ideal (Euler) hydrodynamic case. That reduction is absolutely essential to account for the observed magnitude of directed flow. Furthermore, as mentioned above, the absence of radial flow[12] also points to the importance of transport effects. It was thus made very clear at this meeting that no useful constraint on the equilibrium nuclear equation of state could be inferred from nuclear collisions without a simultaneous constraint on the nuclear transport coefficients!

Fortunately, as work proceeds on the theoretical framework needed to handle medium modified collision terms, there are phenomenological steps that could help reduce the height of the third obstacle. As shown by Keane[14] in comparing observed rapidity distributions of baryons to VUU calculations, the U+U data already rules out a constant reduction of the effective cross section by 30%. Also, K. Frankel showed that the double differential cross section in La+La is sensitive to final state Pauli blocking factors, by comparing the old and most recent Cugnon cascade calculations. Thus, single inclusive cross sections are sensitive to the transport cross sections and could be used to constrain at least possible extreme variations of them. There is, however, very little data on inclusive cross sections on heavy systems at this time to study systematically constraints on effective cross sections. The groups, which prior to 1984 measured systematically cross sections for light ion collisions, concentrated on global analysis and left the basic bread and butter double differential cross sections unmeasured. At this meeting we heard repeated calls for return to such basics, i.e. p,d,t, ... spectra (untriggered) for Nb+Nb through U+U in the 100 -1000 AMeV range. Such data are essential if at least phenomenological progress is to be made on the third obstacle.

The fourth obstacle is least difficult in principle but requires an extensive set of new calculations to overcome. For each transport code, as characterized with a definite set of parameters specifying the mean fields and effective cross sections and a set a prescriptions to handle Pauli blocking and two body scattering style, there exists a definite equation of state. Thusfar, the equations of state have not determined by the transport code themselves, but rather inferred indirectly from other work, e.g. Hartree-Fock, using similar forces. It has been known for a long time though[24] that variations of the scattering prescription alone lead to non-ideal equations of state. Thus, simple intranuclear cascade does not correspond to an ideal equation of state. Clearly, calculations of the pressure, entropy, and energy functionals using the transport codes are needed. The main difference from previous calculations is that the initial conditions must be changed from two incoming nuclei to a uniform nuclear matter at given temperature and density using periodic boundary conditions. The expectation of  $p^\mu p^\nu$  would then measure the energy momentum tensor.

The fifth obstacle, though not discussed at this meeting, was clearly revealed in recent calculations[25] of delta abundances in dense nuclear matter. At the last meeting there was considerable excitement[26] about the possibility of using the pion excitation function to constrain the nuclear equation of state. Recall that Cugnon and Fraenkel cascade calculations systematically overpredicted the measured pion yields. This led to early estimates that a rather stiff equation of state would be needed to understand the data. The suprising results of Feldmeier and coworkers[25] was that in a self-consistent treatment of deltas and nucleons in QHD, a *softer* EOS could result in *less* deltas than in a system with a rather stiff EOS! This seemingly contradictory result is due to the rapid decrease of the effective

masses of both deltas and nucleons in the model. While definitive conclusions could not be drawn because oversimplified one-dimensional shocks were assumed and multinucleon pion absorption[27] neglected, the results underline that pion yields may eventually teach us more about the unknown properties of deltas in dense matter than about the cold nuclear equation of state.

To advance the understanding of pion dynamics in nuclear collisions, it will be important first to get a better handle on pion absorption mechanisms. Overprediction of the pion yield by cascade already occurs for C+C systems, where certainly no high density equilibrium effects are relevant. There is a need to remeasure directly the pion excitation function in such very light systems since in these systems it will be easiest to isolate the pion absorption physics from the density dependent modifications of the delta and pion dispersion relations. Once the pion excitation function in C+C is understood, then it is worthwhile to return to the heavier systems that will teach us about the interesting delta and pion optical effects in dense media. Secondly, as reported by Odyniec[28], the concave shape of the  $p_{\perp}$  spectra of pions still needs to be understood. Hahn and Glendenning[29] suggested that this observation is due to a complex interplay between effects due to cooling of the source, Bose effects, and collective flow boosts of the spectra. Untangling the reaction mechanism will probably require analysis of *triple* differential pion spectra. Again, there is insufficient data to answer such basic questions.

Part of the present ambiguities on the EOS can also be traced to an uncontrolled proliferation of transport codes. There are at least several versions of the Cugnon cascade code and many versions of VUU codes for example. Different versions differ in details and parameters that are not well documented in the literature. There is a well known and time tested cure for this problem, namely, requiring version numbers and systematic documentation of changes from version to version. A very good example is the series of LUND Monte Carlo programs, JETSET6.3, PYTHIA4.8, etc., used in high energy physics for multiparticle jet fragmentation codes. These programs are found in the CERN program library and a long writeup of each program is updated as necessary. The writeup includes latest Lund and DESY preprints by the authors of the programs and clearly describes all subroutines and common block parameters and includes examples of use. In this field the only example of a well documented code is FREESCO[30]. I suggest that a nuclear code library be established, e.g. on the Lawrence Livermore Lab Cray system, modelled after the CERN program library. Ideally, nuclear dynamics codes ranging from TDHF, Fireball, to Hydrodynamic, Cascade, VUU, QMD, to LUND, DPM, etc. would be included with specific version numbers and long writeups. In addition it would be most useful to have a library of experimental filters, e.g. BallWall84.1, StreamerCham84.1, WA80.1, NA35.1, etc., that are provided by the experimentalists as the best estimate of the acceptance of each particular device. That there is rapid development and modification of transport codes is a good sign that progress is being made. However, I believe that establishing such a library is necessary to ensure a more controlled and disciplined growth of this increasingly complex field.

The proliferation of nuclear transport codes is of course linked to the rapid progress that has been made in understanding many elements of the reaction mechanism. At this meeting there was a great deal of discussion on a new aspect of the reaction mechanism, namely multifragmentation, which poses even greater challenges for both theory and experiment in the future. Broadly speaking, multifragmentation, is the study of the propagation of A body correlations through the process of nuclear disassembly into many often large fragments. This is obviously a very complex problem on which only the few steps have been

taken. A long range hope is that this phenomenon may shed light on the nuclear gas-liquid phase transition. At this meeting Randrup[31] emphasized the many challenging problems confronting this topic, while Aichelin[11] showed numerical results obtained using QMD. Randrup emphasized the sensitivity of results to inclusion of the density of excited nuclear states and to interactions between the fragments in the expansion phase. Aichelin showed that the characteristic power law for mass yields may have nothing to do with interesting critical exponents but may be simply the accidental form resulting from averaging over impact parameters. These later results again emphasize the necessity of studying reactions with  $\phi_R$  and  $M$  impact parameter meters. Inclusive yields are likely to teach very little about such complex phenomena. To make progress experimentally it will be of course necessary to build  $4\pi$  detectors capable of measuring simultaneously many fragments of large mass over a large kinematical domain. Present detectors and even proposed extensions[32] do not seem adequate to make a dent on this topic. Essentially a more sophisticated Ball-Wall is needed. Theoretically, it is still unclear exactly which multiparticle correlations are most useful to investigate and which are most sensitive to novel dynamical effects. Stöcker suggested that the excitation of topological cross sections, e.g. the cross section for producing at least four  $Z > 4$  fragments, may be important to look at. But my general impression at this time is that a proper focus in this area of reaction mechanism studies is still lacking and that such a focus must be found to ensure that the ongoing and scheduled experiments on this topic have long term impact.

As we look toward the physics challenges that new high intensity heavy ion beams and cooler rings will offer in the 1990's, several topics look particularly promising:

1. Exploiting dilepton and photon probes.
2. Using subthreshold  $K^+$  production as a novel probe of dense matter.
3. Using radioactive secondary beams to solve astrophysics problems.

Mosel discussed hard photon yields as a probe of the collective flow and microscopic collision mechanisms. High energy photons,  $E \gtrsim 40$  MeV, are mainly sensitive to the rate of neutron-proton scattering in the medium. Hence, these photons may provide an alternate tool to constrain transport cross section. However, high energy photons are also contaminated by  $\pi^0$  decay. Thus a full exploitation of this probe involves a much better understanding of pion production. Lower energy photons on the other hand become sensitive to coherent radiation of the nuclei, and hence may provide new information on the collective nuclear currents associated with the viscous nuclear flow.

Gale[33] discussed how dilepton yields in the mass range 300 - 800 MeV could shed light on the unknown dispersion relation of pions in dense media. The pion dispersion at high nuclear densities may soften considerably due to P-wave coupling to delta-hole and nucleon-hole excitations. The annihilation of a " $\pi^+\pi^-$ " phonon pairs could then yield a dilepton mass distribution that could differ dramatically from the expectation with free space dispersions. Furthermore, since the annihilation occurs inside the matter, the signal will not be so effected by unknown pion absorption processes as the final pion spectra themselves. Thus, dileptons may be unique probe of pion dynamics at high baryon densities and temperatures. On the experimental side, Roche[34] reported the first successful measurements of dilepton pairs at Bevalac energies in p+Be and Ca+Ca at 2 AGeV. Unfortunately, present detectors will not be useful with truly heavy ions and present intensities are too low to permit measurements at lower energies, where more complete nuclear stopping occurs.

Therefore, full exploitation of this probe will require the next generation of detectors at high intensity machines.

Schürmann[35] discussed how subthreshold  $K^+$  production may provide an independent probe of the nuclear equation of state. His calculations showed that while the absolute kaon yields are subject to large uncertainties due to the lack of data on the elementary  $pp \rightarrow K+X$  production cross sections, the ratios of the yields  $R(A/B) = \sigma(A+A \rightarrow K)/\sigma(B+B \rightarrow K)$  is insensitive to those uncertainties. Furthermore, he found that at 700 AMeV  $R(Nb/Ne) = 23$  for a soft EOS while it was 13 for a hard EOS. Thus, this ratio may be more sensitive to the stiffness of the equation of state than  $p_x$  flow. A specific advantage of  $K^+$  as a probe is that it has a much larger mean free path than  $K^-$ , pions or nucleons, and thus suffers less final state interaction distortion effects. The disadvantage is that experimentally it is more difficult to identify a rare  $K^+$  in a large proton background. Thus, a much more sophisticated detector system is required to exploit this probe. An important open theoretical problem that must be looked into is the sensitivity of  $K^+$  production to the  $\Lambda$  dispersion in dense media. Recall the discussion on pion production. There new calculations[25] revealed that pion production is rather sensitive to the unknown  $\Delta$  dispersion[25]. I suspect that unambiguous information on the nuclear EOS using kaons will require similarly the simultaneous understanding of  $\Lambda$  dynamics in dense nuclei. Maybe, we can turn this problem around by using kaons mainly as a consistency check on the EOS as deduced from  $p_x$  etc. with a primary goal of providing unique information on the properties of hyperons in nuclear matter.

Finally, I want to mention a long term goal of using radioactive secondary beams to address problems of astrophysical interest. As discussed in ref.[36] nucleosynthesis involves many reaction steps where radioactive nuclei participate. For example, to break out of the CNO cycle in order to produce elements up to Fe involves reactions such as  $^{15}\text{O}(\alpha, \gamma)^{19}\text{Ne}(p, \gamma)^{20}\text{Na}$  followed by a complicated chain of  $(p, \gamma)$  reactions and weak decays. Very few of the actual reaction rates and decay rates along the chain are known at present. Such reactions could be studied when high intensity primary beams make it possible to produce radioactive secondaries at high rates and when cooler rings will make it possible to store such beams for eventual deceleration to the very low energies of astrophysical interest. For other more conventional applications of secondary beams to the study of nuclear structure see contributions by Shimoura and Matsuta at this meeting and ref.[36].

In closing, this 8<sup>th</sup> High Energy Heavy Ion Study clearly demonstrated that since the last meeting, there has been substantial progress on the main objectives in this field. That progress was made possible by an impressive series of experiments utilizing truly heavy ion ( $A > 100$ ) beams for the first time and the simultaneous development of detailed nuclear transport codes. As summarized here and as emphasized in many of the talks, confronting the future challenges will necessitate a great deal more experimental and theoretical work. At this time, when there is a vast expansion of the field of heavy ion physics into the new realm of ultrarelativistic energies at BNL and CERN, the lure of the quark-gluon plasma poses a new sociological challenge that must also be addressed. As both experiments and theoretical work increase vastly in scope, complexity, and commitment of time there is a danger of spreading the approximately conserved number of physicists out too thinly on too many fronts. It is imperative that experiments be chosen and prioritized very carefully and that the development of phenomenological nuclear transport models be brought under stricter control. A concentrated and vigorous effort will ensure that the next meeting, celebrating the opening of the new SIS machine at GSI in 1989, will be as exciting and stimulating as this one.

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