XXIV MAZURIAN LAKES SCHOOL OF NUCLEAR PHYSICS — SUMMARY

M. Blann

We are here at the conclusion of the 24-th Mazurian Lakes School of Nuclear Physics. This is the longest running and most successful school of its kind. Some of the conference organizers have worked on these conferences for 24 years or more, and the success and longevity of this school owes very much to them, and to all the organizers along the way. Many of us have benefited very much over the years, not only because of friendships we have made, but because through the contacts made at these meetings we were able to have Polish scientists join our labs as visiting faculty and post-docs, enriching our home institutions with both their intellectual abilities, and special Polish spirit. For the benefit of us all, I wish long continued life to these schools.

But let us turn to the topic of nuclear physics. Perhaps our research goals are to understand the properties of nuclear matter — structure and reactions in a quantitative way — for systems which are neither two body nor infinite body — but which lie in an awkward stage in between. We would like to be able to understand the limits of stability in A, Z, I, and shape; modes of decay, and how nuclei interact with each other and with other fundamental particles. While the development of theories and models, and performance of experiments to accomplish these goals is a great intellectual challenge, the knowledge is important in many applied areas which affect our lives. These include applications as diverse as improving the success of radiation oncology — affecting one of six of us here — to designing more reliable chips for the electronics industry. We have an exciting intellectual challenge which also has relevance to society. It is important to continue this work, so it is particularly pleasing to see the many students here who are the present and future of the field.

In making comments of a more specific nature, I would point out that I found all talks fascinating and of excellent quality; however I shall not try to comment on all, but rather to look at the broader areas discussed at this school. Thinking of the high spin workshop, appropriately honoring Zdzisław Szymański on his 70th birthday, I think of three classic works which have provided a bedrock for understanding and interpretation of deformed nuclei and quite a bit of inspiration for their study.

The first of these is the rotating liquid drop model of Cohen, Plasil and Swiatecki dating from 1974 , when heavy ion reaction studies were still at a fairly early stage. With beautiful clarity it illustrated the expected behavior of nuclei under rotation — changing from spherical to oblate to prolate to triaxial, until finally the equilibrium and saddle point energies merge and the nuclei become unstable to fission at a critical angular momentum.

The next work is that of Sven-Gusta Nilsson, from the 50's, showing what happens to shell model levels in a deformed (H.O.) potential, and the third, Strutinski's work relating density of single particle levels to energy shifts in the mass. Together these works provided a framework within which we could understand many of the observations of nuclear spectroscopy at high angular momentum — at least to first order — and to see the importance of the single particle levels on the mass surface — a theme which enters many of the talks presented here in some form, as this conference has highlighted the importance of being able to predict and extrapolate the nuclear mass surface to nuclei far from stability.

We have heard of superdeformed nuclei being identified with 2:1 axis ratios — the RLD model predicted ratios up to 2.2:1 if memory serves. We have also heard of possible hyper-deformed nuclei with axis ratios of 3:1. They remain to be confirmed; if they are, the liquid drop model will need a somewhat closer look. We heard talks by Swiatecki and by Bartel on rotating Thomas Fermi models for nuclear masses. We saw that already Swiatecki is getting vastly improved $I = 0$ fission barriers with his approach, and probably will for barriers at high I as well. From this work, and Bartel's work, we expect to get information on the temperature dependence of fission barriers, which will improve our modeling capability in this area. And this approach may also be relevant to predicting the existence of hyperdeformation or to further questioning its existence.

Overall, Szymański reviewed the achievements of the high angular momentum workshop, and I shall not duplicate. The presentation of Haensel on neutron star observations was however provocative in that it suggested that observation of rotational periods may put limits on the nuclear equation of state, a goal which has evaded accelerator based studies for over 20 years!

We turn from the axis of "the limits of nuclear matter due to angular momentum" to limits due to neutron number at each Z-proton rich with too few, and neutron rich with too many. A large number of talks dealt with this topic which is a very timely one indeed. We had a review of the challenges in physics, by Nazarewicz, and of both work in progress and planned new facilities by Casten and Villari. Additonally several very interesting papers of work in progress in this area were presented.

Several speakers showed figures showing predictions of the neutron drip line according to different mass formulas. These differed by up to 20 neutrons in the tin region, again emphasizing a challenge to theory to get it right, and a greater challenge to experimental measurement to let them know when they have! This exchange will be essential, and fruitful — and will illustrate the idiom "one hand washes the other".

The lovely presentation of Kratz and Thielemann on the astrophysical$R$-process is very relevant to this discussion. By using known nuclear data where available, and theory elsewhere, they set up master equations for the R-process to compare calculated and known abundances of isotopes. By putting in reasonable values and analytically studying the results, they could identify nuclides for which they were very suspicious that the theoretical data — such as neutron binding energies, b decay lifetimes, shell gaps, neutron decay branches — were in error. This activity pin-points particularly interesting nuclides to study with radioactive ion beams, and with theory. Indeed it was pointed out that the shell structure around the Sn region N drip line is probably much reduced and that such a prediction came from the Hartree–Fock–Bogoliubov calculation of Dobaczewski.

It was pointed out that nuclear density distributions towards the neutron and proton drip lines are an open question — and we were reminded of the neutron halo results in ^{11}Li. We were posed the question as to what magic numbers, if any, will exist in drip line nuclei. There are many delayed p and $2p$ emitters to be identified — not just as a curiosity, but because they will help pin down the nuclear mass surface far from stability. An extremely rich area for investigation, indeed.

Relevant to the questions of the mass surface far from stability, the talks of Kluge, Beck and of Bollen on the capabilities of ion traps were fascinating, and it is exciting that such a powerful new tool is in use to enhance our experimental capability to measure masses far from stability very accurately. And we heard cyclotron mass spectrometry results from Ganil given by Chartier, as another tool in use for mass measurement.

The pace of study of exotic isotopes made by deep inelastic collisions. (DIC) is surely quickening. We heard results from Rykaczewski, Persson, Blank and Czajkowski. Some of those planning RIB facilities would like to be able to predict the yields of secondary beams available from DIC sources. So the experimental results and modeling of the Cracow group, as reported by Królas is a very encouraging start. They analyzed using the minimum potential energy model of R.D. Present with a separation parameter (Present's work was to explain the charge dispersion in fission), and got quite encouraging results. It will be nice to see how universally this will work and how far it might be improved.

We enjoyed the review of present and future RIB facilities by Casten and Villari, covering both ISOLDE type thick target sources, enhanced by laser ionization, and of sources using DIC beams. We heard of use of these as the source of unstable products to study, and as secondary beams to be accelerated and used in nuclear reactions. The SPIRAL facility at GANIL is expected to be on line in 1998. For me, an exciting possibility will be to use neutron rich secondary beam projectiles to reach the predicted doubly magic $Z = 114$ nuclides which had been predicted to have quite long lifetimes — but which we have thus far been unable to populate as we could get the right proton number but not enough neutrons. Of course, we should be able to produce many nuclei nearer the neutron drip line, and we may see something different in the reaction dynamics.

There are many interesting questions to be asked and answered with respect to nuclear structure for these exotic nuclei. Clearly this is, and will remain an important, exciting growth area in nuclear physics for the present and for many years to come. Experimental results will pose a challenge to theory, and new theoretical understanding will help steer the next generation of experiments. This is the type of experimental program I get enthusiastic about, because the goals, and what needs to be measured are clear, as is the value of the results. We know figuratively speaking where to set our detectors, and what to look for!

Now we have said a little about the I and N/Z dimensions of the nuclear physics coordinate system. There remains the energy dimension — the nuclear reactions dynamics part (and of course the interesting atomic physics). We want to be able to understand what happens when we collide two nuclei at ever increasing energy.

What may we learn about the nuclear EOS? What about the interactions and resonance behavior in dense nuclear matter versus for N-N interactions? What about collective flow of nuclear matter? And what physics mechanisms determine the formation, multiplicities and spectra for cluster emission? To what extent are heavy ion collisions just Fermi boosted N-N collisions, and to what degree will high nucleon densities mediate this aspect via multibody processes supplementing two body processes? How does the incident energy divide between thermal and compressional energy?

One viewgraph correctly stated, I am sure "Heavy Ion Collisions provide a dynamical exploration of the nuclear matter phase diagram".

The corollary is the statement, "The devil is in the details".

One observation might be made at this point. That is that the interpretation of so much of the physics of heavy ion reactions depends on model codes, such as BUU, VUU, QMD, RQMD, INC, *etc.* This tries to force ultimately quantal phenomena into semi-classical formulations. There are problems that arise, and we must always be diligent as to how much of the interpretation is model dependent — an artifact of assumptions of the model code rather than consequences of nature. The phenomena we are trying to unfold are extremely complex, so this cautionary note is easier said than done. Once again, "the devil is in the details".

There are many excellent double differential spectra for (nucleon in, $\times$ nucleons out) reactions for which a single target (*e.g.* Zr, Pb) has been used for measurement for incident energies of 18 to 800 MeV in fairly small steps. Such data were used in the recent NEA/OECD benchmarking exercise to test intermediate energy reaction codes. I would like to suggest that any code used to treat heavy ion reactions should be benchmarked versus these relatively simple data — and perhaps for nucleon in, cluster out data as well. It would be valuable, perhaps at the next school, to have some critical presentations on the status of some of the models upon which our interpretation of experimental results is so very dependent!

These questions are being probed for answers with sophisticated new detectors, of nearly 4π geometry, measuring many parameters of the reactions with the ability to make cuts which we believe are representative of different centralities of collision, and to deduce information on the velocity of the emitting source. Much of the experimental data requires a simulation model — such as BUU or QMD, to fully interpret the results. This is a great — and difficult — challenge for reaction models. It is again the problem of having neither a two body nor an infinite body system, and of trying to represent quantal systems semi-classically in model codes. Many of the microscopic channels for these models are unknown. Experiment is certainly driving theory in this case. We might note that probably most events under discussion have maximum multiplicities of 400 or fewer, and the main microscopic channels are known. I often wonder how we will analyze RHIC results with multiplicities of 20,000 per event for which most channels are unknown. That will really be a challenge!

We have had reports on extremely powerful detectors for studying these heavy ion reactions — INDRA as described by Eric Plagnol, FOPI by Klaus Hildenbrand, and TAPS by R. Holzmann. We also heard about high energy photon measurements with TAPS by Y. Schutz, supplementing the discussion of neutral meson probes by Holzmann. A powerful array of detectors, and many outstanding experimental results probing the energy axis of our nuclear physics coordinate system!

The "first results" from INDRA are very impressive indeed. For protons and heavier, one can associate spectra of emitted particles with the velocity of the emitting zone. Using a gate which is thought to be representative of impact parameters, one can see emissions from target, projectile, and from the center of mass, which would be interpreted as a neck or hot zone between centers. Emission of heavy clusters from this region was an interesting result — perhaps consistent with the fluctuating low density region pointed out to us by Randrup from his V–L (Vlasov–Langevin) calculation. These data should provide excellent tests of simulation codes such as BUU and QMD, or more likely will provide the benchmark results with which to develop those codes from broad brush stroke barn painters to fine brush stroke, fine art painters - if this is possible; we are after all trying to force semi-classical codes to reproduce quantal systems. Partly for this reason — and only partly — the codes are better at high E/A than low. But our detectors measure fragments which are at or near ground state. We have codes which have been benchmarked to deexcite fragments below 200 MeV quite well, and which are surely superior to BUU and QMD in this energy regime.

I would advocate using these codes for final de-excitation corresponding to Randrup's "hybrid animals" and I think that this is widely done. If an ^{16}O nucleus is observed from a zone at 10–12 MeV/A excitation, the primary fragments may have been ^{40}Ca, and the momentum of the ^{16}O will have been perturbed by the random walk of all the light particles emitted going from primary to final fragment. So interpretations of final yields are not trivial. But in any case, these data provide an extremely rich base for stimulating improvement in, and for testing models.

We heard many interesting ideas from Klaus Hildenbrand representing the FOPI group, and many conclusions: at energies below 1 GeV · A, mass flow is dominated by clusters. In the 1–2 GeV A regime, for π^+, $K^{0,+}$ the flow apparently vanished, while it still followed the protons for the Λ. We heard how the flow measurements may be used to deduce the conversion of incident energy between compressional energy and thermal energy, and the relationship of thermal excitation to cluster yields. The second phase of FOPI is just about ready to come into operation, yet it has already provided a wealth of information in its use as a "forward" detector.

R. Holzmann described the TAPS photon spectrometer for studies involving neutral meson production. Very many interesting results were reported. In the course of the work the group has measured a π^0 absorption cross section, and extracted the cross section for Δ capture on a nucleon (Δ + N) $\rightarrow$ N + N. Their result confirmed prior estimation based on detaile d balance, and shows that experiments of the type described may be used to deduce resonance cross sections in nuclear matter.

Large π^0 multiplicities with strong Δ excitation were found in the heavy ion reactions investigated, and η production was found and measured, showing excitation of higher lying baryon resonances. There is a hope of understanding various resonances in heavy ion reactions by these studies — when they are coupled with BUU or other transport codes for interpretation.

Y. Schultz reported on "Return of the Hard Photon". The photons, unlike the mesons, have no final state interaction with nuclear matter. They should therefore give unperturbed information on the source, and perhaps by Doppler shift on the source velocity. The hard photons come largely from first chance N-N bremsstrahlung processes, but the TAPS spectrometer has also shown a second, longer time scale softer source at lower incident energies, below 50·A MeV, where the BUU simulation suggests a longer oscillation period of the interacting nuclei, and this source decreases with increasing energy above 50 · A MeV. The first chance N-N bremsstrahlung spectra are sufficiently sensitive to require a Thomas–Fermi treatment of the skin density/potential. Fermi coupling is necessary to get the energetic spectra, and high momentum components beyond the semi-classical limit must be assumed. Even then, the BUU does not get the full high-energy limit — but then, neither has the theoretical NNγ spectrum been experimentally verified. Once more we find the microscopic input to the simulation code lacking experimental verification.

The work of Marta Kicinska–Habior is extremely timely in gaining probes of hot nuclear matter. She and her collaborators have been able to understand the GDR parameters and deduce their dependence on A, Z, I, shape and excitation. They have shown that excited nuclei do indeed sample all shapes, and they are very far along in making the oft assumed Brink–Axel hypothesis the "Warsaw–Seattle–Copenhagen fact". Their results will be important in interpreting many experiments in which energetic photons are measured to separate different γ ray contributions, and perhaps as an aid in calculating reaction time scales.

N. Cindro gave a most scholarly presentation of the Hanbury–Brown–Twiss method of deducing the size of a small source by interferometry. I found it a very welcome review of an effect often quoted, but seldom so clearly described.

Madeleine Soyeur suggests that the study of dileptons from heavy ion reactions would be a fertile new area to gain information on the behavior of hadronic matter at very high baryon density and temperature. She pointed out that the HADES collaboration should be able to get excellent data. To interpret these data will require much work to understand in-medium properties of vector mesons. But results from CERN indicate enhanced dilepton production for relativistic heavy ion reactions, suggesting that studies of this type have a strong likelihood of getting data on in-medium effects in dilepton production in heavy ion collisions. The HADES detector was described earlier this morning by P. Salabura.

This takes us rather high up in energy, to the ultrarelativistic regime. I would remind that at the other extreme for nuclear physics, H. Rebel told us how he can deduce important stellar burn cross sections which are too low to measure in the lab by using Coulomb induced dissociation with a high Z target to give the inverse reaction, then applying detailed balance to get the desired cross section.

This gets us to lower energies, closer to the atomic physics processes we have heard about, which provided the topics the past few days. We had the talk by R. Schuch, illustrating the extremely high precision measurements which can be made for atomic and plasma physics by cooling in Cryring. This makes it possible to perform reaction studies with dilute beams which have excellent energy resolution, and with the ability to control the charge of the interacting ion.

We heard how the use of adiabatic expansion of the electron beam optimizes resolution in the e-ion collisions. Schuch illustrated these capabilities by examples of study of the structure of the electron continuum via laser assisted capture of free electrons, measurement of cross sections for dielectric recombination, relevant to plasma physics and accelerator beam loses, in the dissociation of molecular ions — which has chemical applications, and of course the ability to confirm the spectral lines of charged ions which has applications in astrophysics and plasma diagnostics.

P. Mokler gave us additional insights into the modern frontiers of atomic physics using storage rings. We saw the possibilities of studying transitions in H or He like ions, even for U, and even of preparing U^{92+} for investigation. I should add that there was a comparison of the 10^8 U^{92+} at ESR versus the 10' at the LLNL EBIT facility. There the comparison is done differently — they show a drawing of EBIT on a tabletop vs. GSI. It is presented as a case of David *vs.* Goliath, with David waiting to get a little bo(u)lder.

Mokler gave illustrations of many measurements which could be made with this storage ring technique, with emphasis on QED effects and contributions to the Lamb shift. Applications to measuring binding energies in highly charged ions, and of determining $l - s$ coupling schemes were discussed as well. It was a fine tour of the possibilities of a relatively new tool, and a welcome education for me — and I am sure others.

We heard more of the application of these techniques in the presentation of Th. Stoelker, reporting on the atomic structure of He like ions. GSI has a very rich program in atomic physics, indeed!

We finally heard a theoretical review and results of experimental programs relevant to the physics of extremely high E- fields which result when two heavy nuclei pass close by. Most of us became aware of this area from the work of the Frankfurt theory group, and it was summarized very clearly in the presentation of Prof. G.Soff. We look at the conclusion on dynamic positron formation supported by the two experimental presentations by R. Betts, with the APEX detection at ANL, and by H. Bokemeyer from GSI. Both groups now get continuous positron spectra which are consistent with theoretical predictions in magnitude and shape. I think that we were impressed by the difficulty of the measurements, and the great attention to detail which was necessary to complete these measurements or to get them to their present state. The discrete line structure superposed on the continuous curves reported in some of the earlier work probably resulted as artifacts from extremely difficult measurements, and were not observed in the Argonne group work nor I believe in the last rounds of studies at GSI.

These are my impressions of an overview of this school. We have heard outstanding lecturers cover some of the very exciting front-line areas in nuclear and atomic physics. And we have seen that these fields are very much alive now and for the foreseeable future. I thank the organizing committee for giving me the opportunity to be once more in Mazuria, among the very special people here.