Pramāṇa – J. Phys., Vol. 32, No. 4, April 1989, pp. 599–603. © Printed in India.

Future directions

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The origin of nuclear physics can be dated to 1896 when the first specific nuclear phenomenon (radioactivity) was seen in the laboratory, or to 1911 when the nucleus itself was first sighted by Rutherford, or even 1932 when the discovery of the neutron made possible the first systematic beginning of nuclear theory. Nevertheless, the nucleus remains exotic and enigmatic, uncovering every now and then a new facet to sustain our curiosity and excitement.

Take the basic question of nuclear constituents. We have come a long way from the proton-electron model to the quarks and gluons. We study nuclei with a variety of probes; with ever-increasing beam energies, probing deeper and with better resolution, seeing more and more complex processes with increasingly more sophisticated measuring devices, we continue the spiral of answers begetting more questions. Initial efforts to understand nuclear phenomena in terms of nucleons only, then the discovery of mesons, and a variety of efforts to decipher the role of mesons in nuclei, and to trace their finger prints, and finally during the last two decades we have seen enormous activity to detect and pin down the presence of gluons and quarks in nuclear processes. One cannot really say that at any one stage a complete and satisfactory understanding was achieved, before moving on to the next stage. A great deal of nuclear phenomena remain to be understood, and can be understood, in terms of nucleons alone, and similarly a large body of nuclear processes can be (and remain to be) clearly understood only in terms of nucleons, mesons, isobars etc. Such activities will continue in future, as I shall try to describe later. But the glamour and visibility have clearly shifted to the nuclear physics of quarks and gluons. High energy nuclear physics has become the new frontier, with the quark-gluon plasma the holy grail. With heavy ions accelerated to high relativistic energies, colliding to heat and compress nuclear matter, the crucial question is the determination of parameters at which the phase transition to quark-gluon plasma will take place, and the signature that would reveal explicitly the occurrence of this phase transition. Special high energy accelerators (such as CEBAF) are being built just for doing nuclear physics. Availability of high quality secondary beams (μ, π, K etc.) with energies of many GeV is assured in the near future with accelerators such as LAMPF II, TRIUMPH II etc. Thus, high energy nuclear physics will remain an expanding frontier for many years.

Another basic problem of nuclear physics is that of the nucleon-nucleon (NN) interaction. The evolution of a research field frequently follows a scenario such as this: a new discovery is made or a new idea is propounded heralding a major breakthrough, a great deal of excitement is generated with many scientists joining the gold-rush, then gradually the quick and easy problems are done and the exhaustion of interest begins. The remaining problems become more complex, sometimes intractable, filling up finer details requires a lot of hard work, so that when somewhere else a new frontier opens up, the exodus to the new promised land begins. The *NN* interaction problem has a similar history, and in detail still remains obscure and unsolved. It began with potential models, phenomenology and data-fitting. Meson theories such as OBEP and various refinements strengthened and provided reliable inputs for the phenomenology. Now the interest has shifted to quantum chromodynamics and quark interactions. Of course, the level at which one tries to understand *NN* interaction depends very much on what one wishes to do with it. It is already known in sufficient detail for low energy nuclear structure problems. On the other hand, in terms of the gluon exchanges between quarks, the QCD theory only barely gives a qualitative understanding of the features of *NN* interaction revealed by phenomenology such as the repulsive core, weak and long attractive tail, spin orbit and tensor components etc. The problem remains open and, I hope, will be in the forefront of nuclear physics research.

The study of nuclear infinite matter and calculations of its saturation properties (of density and binding energy per nucleon) is another area where interest has waxed and waned. The current situation seems to be that all the calculations lie on a Coester band which stubbornly avoids the tiny island where experimental points appear to cluster. Recent developments of relativistic nuclear mean field theories appear to suggest that this discrepancy may soon be resolved. More work is obviously needed, and obviously will be done. Or alternatively, the resolution of the problem may be coming from the work of L Satpathy and his colleagues on a new mass formula reported at this conference. His mass formula has enjoyed a great deal of success and predicts numbers for saturation density and binding energy per nucleon in nuclear matter that are unconventional, but closely agree with the recent calculations of B Day. We shall wait and see. The relativistic field theory is now being applied to several areas of nuclear physics, and promises to reveal new insights into many nuclear processes.

Let me now turn to a topic that has held my interest for quite a long time, and which, I believe, promises to be of great interest for many years to come. The handful of nuclei provided to us by nature are remnants of cosmic fires lit in distant past. There are many more that we can cook up in our laboratories. The mass formulas tell us that the number of observable nuclei, constrained by spontaneous fission at one end, and bounded by neutron and proton drip lines, are about 6000. About 265 stable and about 60 radioactive nuclei occur naturally. However, since 1934, when the first artificial radioactive nucleus was produced by Joliot and Curie, about 2000 radioactive nuclei have been synthesized by nuclear scientists. It is rather unlikely that we will be able to produce and study all the theoretically possible 6000 nuclei in laboratory. However, the present state-of-the-art indicates that it should be possible to compose another 1000 or so nuclei with the available technology and instruments. The present rate of producing new nuclei appears to be about 40–50 per year. Thus, there should be enough new nuclear species to play with for the next 20 years. And, of course, studying detailed properties of these nuclei—mass, radius, electromagnetic moments, decay schemes, spectrum etc. will take many more years. Behind this effort is the development of awe-inspiring technologies—producing and studying few atoms at a time, fantastic background elimination, extremely short lifetime measurements—

that are being pushed to limits. Techniques of producing radioactive beams and their interactions are being developed. One can say in this field—you ain't seen anything yet!

What does one learn from such efforts? Anything new? Surprising? Fundamental? I will try to give you some flavour of what one learns.

Studies of light nuclei close to neutron drip line have given several surprising results. Measurements of radius show that the isotope ^{11}Li is almost as large as ^{28}Si, the root mean square radius being 3·27 fm. This has been explained in terms of a large neutron halo, perhaps, involving di-neutron-like structures in the surface region. Recent measurements of binding energies of He isotopes of mass 5, 7 and 9 show that they are unbound by 0·89, 0·45 and 1·13 MeV only, whereas standard mass formulas such as Garvey-Kelson predict them to be unbound by as much as 1·2, 2·3 and 3·5–2·5 MeV respectively. This observation throws serious doubt on our understanding of pairing energy and its dependence on neutron excess and raises an interesting question to search, if ^{10}He would be stable.

Our understanding of mean fields in nuclei has greatly expanded with the discovery of new regions of deformations. In recent years, it has been shown that while N or $Z = 40$ seems to be a good magic number if the corresponding Z or N is near 28 or 50, nuclei with both N and Z near 38–40 show very large ground state deformations with $\beta \sim 0{\cdot}4$!

Till about a decade ago, the nuclei in the mass region 60–90 were assumed to be quasi-vibrational and rather uninteresting, largely due to insufficient experimental data and our simplistic concepts about the properties of nuclear mean fields on which the shell model is based. Studies of Hamilton, Ramayya and others on neutron-deficient isotopes of Se (and later many other nuclei) showed that the spectra of nuclei such as ^{70}Se, ^{72}Se, ^{74}Se etc are quite complex and show coexistence in energy of bands of states of quite different deformations. Extensive theoretical and experimental studies in the last decade have showed in this region sharp changes in nuclear structure with small changes in neutron or proton number, largest ground state deformations for nuclei with N and Z near 38–40, coexistence of bands of several different shapes and deformations in the same nucleus and so on. The group at PRL has carried out several studies of such nuclei, and also have developed an overall understanding of the nuclear mean field in this region and its variation with N, Z and excitation energy. Experimental work in this region—particularly on nuclei close to $N = Z$ line—is likely to continue at a vigorous pace for many years to come, and it will be an interesting theoretical challenge to obtain a microscopic understanding of the observed properties. The heaviest $N = Z$ nucleus ^{80}Zr and its couple of excited states have been observed only recently and efforts to create and study other odd-even and odd-odd nuclei in this region are being made.

Theoretical studies of shell structure at large deformations have shown existence of new magic numbers. In the region mentioned above, it is clear that the shell gap expected at N, $Z = 40$ exists only when the corresponding Z or N are near 28 or 50, but disappears completely when both N, Z are near 40. On the contrary, new stable prolate and oblate shapes appear for different combinations of N, Z values of 34, 36, 38 etc. Many such theoretical studies have been in the framework of potential energy surface calculations. It would be interesting to explore the mean field properties microscopically (in terms of Hartree-Fock theories) and see their dependence on the nuclear interaction.

In still lighter nuclei (around $A = 40$), many new proton-deficient, neutron-rich isotopes have been studied. Here again, the questions of deformations, validity of magic numbers, need to consider across-the-major-shell excitations for nuclear structure etc. arise. We have studied nuclei such as ^{31}Na, ^{33}Mg etc. Is $N = 20$ a good magic number here? Obviously not. Personally, I look forward to studies of nuclei such as ^{44}Cl and ^{46}K and their low-lying spectra. This pair would have a relationship as seen earlier for ^{38}Cl and ^{40}K, only in this case the $f_{7/2}$ particle would be replaced by an $f_{7/2}$ hole. Will simple shell model be still valid? For particle-hole-type odd-odd nuclei, the ground-state spins follow a simple shell model rule, $J = j_1 + j_2 - 1, j_1$ and j_2 being the shell model orbits for the two odd nucleons, e.g. the ground-state spin of ^{8}Li turns out to be 2, and that for ^{40}K, ground state is $J = 4$. It would be interesting to see ground state spins for ^{90}Nb or ^{88}Y—one should expect them to be as large as $J = 8$! Nuclei in the doubly magic region $Z = 28$, $N = 50$ are also now being explored (e.g. ^{78}Ni) and should give new insight into shell structures. One can obtain a fair idea of the state-of-the-art situation and of future developments to be expected in this field from the Proceedings of the Fifth International Conference on Nuclei Far From Stability, 1987 (AIP Conference Proceedings No. 164).

In the last 3–4 years, the discovery of superdeformations ($\beta \sim 0{\cdot}6$) at very high spins ($J \geqslant 30$) and high excitation energies in complex nuclei such as Nd, Gd, Dy, Ce etc. in mass region 130–150 has created quite a sensation. This topic has not been discussed here and I shall not dwell for long on it. Nevertheless, it is well to realize that although qualitatively and in principle one understands (and can even claim to have predicted) such superdeformed bands, there are many details regarding the rapid cooling of the compound nucleus to the superdeformed band, the sudden end of the band and transition of the nucleus to a much less deformed band, the occurrence of such bands in some nuclei and not others etc. are not fully understood, and need a lot of detailed theoretical work. Studies of nuclei at high angular momenta and energies, their collective and particle-type excitations etc. will be a rich field for experimentalists and theoreticians alike for decades to come.

In passing, I would also like to mention the newly emerging field of beta-delayed multi-nucleon decays. Coupled with the interesting availability of new exotic isotopes, this will be a very exciting topic for future explorations of nuclear physicists.

This has been a wonderful conference. For the last four days, we have heard about new developments in many areas of nuclear physics. We heard about the shell model, collective excitations of nuclei, role of symmetries and statistical theories in understanding deep problems of nuclear structure, mass formula and properties of nuclear matter, relativistic field theory in nuclear physics, interactions of heavy ions, quarks and gluons and even far flung topics such as tachyons, Gribov anomaly and applications of nuclear physics to tea; and it has been a highly educational experience.

The message is clear. Nuclear Physics is alive and kicking. Its problems change, approaches and methods and tools change, details change, levels of understanding change, but the activity of unravelling the mysteries of nuclear structure continues ceaselessly. As I mentioned earlier, many of the fundamental problems remain evergreen.

Why then do we hear now and then whispers of dying nuclear physics, that nuclear physics holds nothing exciting? Partly, this is the doing of physicists themselves who, as they shift from one area to another, declare rather extravagantly that anything other than their own immediate interest is unexciting and expendable. But primarily, the brutal fact is that science expands its frontiers very rapidly, and the resources to support it remain limited. It is then necessary to decide the priorities to determine the future directions in which the thrust needs to be concentrated. This whole process is awfully complex, and not at all free of operation of human weaknesses. Be that as it may, the fact remains that for nuclear physics, new regimes of interest are opening up continuously. There is much that remains to be explored on a fundamental level. This conference has certainly established that.