PARTICLE PHYSICS AND NUCLEAR PHYSICS CONVERGENCE AND PERSPECTIVES

Closing talk 'Quark matter 90'

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FOREWORD

In giving this closing talk, I have the difficult task of replacing Roberto Salmeron, who should have summarized this Conference according to the original programme. When asked to take his place, I said that rather than actually summarizing the Conference, I would instead talk along more general lines, stressing the important convergence of nuclear and particle physics, which we have witnessed throughout the eighties. Relativistic heavy-ion collisions—the research beyond our 'Quark Matter' series—is actually but one facet of a much wider trend, with far-reaching consequences for the organization of research in nuclear physics.

Quark Matter is a topical domain at the interface between nuclear and particle physics, where one finds many joint teams issuing from both fields. It is only one facet of a more general trend that brings nuclear physics closer to particle physics as the former experiences a big change in its style of work.

I have been closely associated with this, throughout '89, as a member of a survey of basic nuclear physics research in France¹, which naturally considered this research in both the European and the world-wide context. This survey followed, in effect, a rather similar one that took place a year earlier in Germany².

There is a good convergence of views between the conclusions drawn in these two reports. They were both well received by the high authorities who commissioned them. Their reading may thus provide a reliable guide-line for the foreseen evolution of nuclear physics in Europe. Indeed, other studies carried out in other countries seem to have reached similar conclusions³. Such surveys have, of course been carried out in the United States⁴ for quite some time.

The phenomenon under study is particularly clear at CERN where, as we know, a large number of nuclear physicists have always been engaged in the relativistic heavy-ion programme. At present, there are altogether over 500 physicists working at CERN in that field, and half of their number come from the domain of traditional nuclear physics. Most collaborations include mixed teams of both nuclear and particle physicists. This is, however, but one aspect of a more general trend. The deep-inelastic muon scattering programme has

now been 'taken up' by nuclear physics, and the EMC Collaboration, which made famous the EMC effect, is now replaced by the NMC (the New Muon Collaboration). Physics at the Low-Energy Antiproton Ring (LEAR) is also mostly addressed by mixed teams. As well as this community of about 500 nuclear physicists who now use the CERN installations for their research, there is also the still typically nuclear community, of about equal strength, long—and still—involved in research at ISOLDE, now in its third phase of development.

In the United States, two major new experimental facilities are CEBAF, which will start research in '94, and RHIC, which we all hope will now be rapidly constructed. They are at present the key elements of the Nuclear Research Programme, and would have been considered as *bona fide* high-energy facilities not too long ago. Nuclear and particle physics are partly merging. We also see a similar trend shaping up at KEK in Japan.

These ideas are new and important, and I shall focus this closing talk on these recent and far-reaching developments.

Before continuing, I should, however, mention the two points which I know Roberto Salmeron would have liked to stress in his talk. The results presented at this Conference, and the discussions that took place, have underlined their relevance. The first point is that we need more extensive results on nucleon-nucleus collisions in order to better understand the new features that are met with in nucleus-nucleus collisions. The second one is that we need data at different energies in order to better 'corner' any threshold effect. These two points are particularly relevant when we consider the state of the art with respect to the J/ψ suppression effect, long observed by NA38.

As I have said, I shall not attempt to summarize the Conference. Many clear and thorough talks have already beautifully summarized the different key facets of our quark matter research. We have all in mind the lively and sometimes passionate discussions that have followed some of them. There is no need to come back to them with my own prejudiced views. Nevertheless, I wish to stress the new aspect which has been imposing itself since Quark Matter 88—the fact that strangeness enhancement has come of age.

At the meeting in Lennox, we already had some important indications from BNL, and also from CERN, that there was an enhancement of strangeness production in reactions with large transverse energy or large multiplicities. We now have very strong and well-documented evidence for this⁵.

Here there have been impassioned discussions about what it actually means in terms of quark-gluon plasma formation. The same still applies to J/ψ suppression, and will certainly continue for a long while⁶. We are indeed dealing with a complicated physics while exploring new grounds, namely matter at very high quark and energy densities. I think that we can but be encouraged by all that has already emerged from a few short runs, with projectiles

that are still of medium size!

Another new fact that came up at this Conference was ϕ enhancement, as reported by NA38. The effect is clear and convincing. Yet the very high p_T cut-off ($p_T > 1.3 \text{ GeV}/c$), which is needed to escape from background, does not make the significance of that result so clear⁷.

1. NUCLEAR AND PARTICLE PHYSICS: THE SCHISM AND THE REUNION

The two fields of research have a common origin in the study of the deep structure of matter, although they split asunder in the late fifties. Nuclear physics then specialized in the study of nuclear structure, the nucleus being seen as an assembly of nucleons which were treated as elementary particles. On the other hand, particle physics focused on the study of the structure of the nucleon.

In the fifties, there were indeed already clear signs that the nucleon was a composite object. It had a measured size, and it showed many excited states. The proton and the neutron were already appearing as only two members of a large family of baryons, whilst the pion was but the lightest of a family of mesons. By the end of the seventies, the quark structure of the nucleon had been ascertained.

The formation of excited states of the proton and the production of higher-mass baryons and mesons did require collision energies (in the centre of mass) in clear excess of 100 MeV, whereas nuclear excitations were typically involving much smaller energies ($< 10 \text{ MeV}$). There was therefore a clear separation in the means of investigation of the two fields.

The separation was complete in the early sixties. Nuclear and particle physicists had their different problems and their own series of conferences.

The last great common venture was the analysis of the structure of the weak interactions, with the discovery of the universal V-A coupling in the late fifties. Key results still came from both nuclear and particle physics.

One had to wait for the eighties in order to witness a marked convergence, after the profound rift which first developed. At present we are feeling a strong ecumenical wind as the two fields of research join forces over a number of topical endeavours.

- i) Nuclear physics has pushed nuclear structure up to the point where the quark structure of the nucleon became relevant. The EMC effect has opened up a promising approach to the study of nuclear matter at close range. Detailed coincidence studies of nuclei with high-energy lepton probes are attracting much attention (NMC, CEBAF, etc.)
- ii) The complexity of the hadrons in terms of quarks has the appeal which the complexity of the nucleus in terms of nucleons, and now also of quarks, once had, catching naturally

the attention and encouraging the efforts of the nuclear physicists. One therefore sees more and more nuclear physicists turning to the analysis of hadron structure, developing with new tools a domain of research left half-explored by particle physics in the sixties. This is particularly the case with LEAR and medium-energy hadronic probes.

- iii) The complexity of nuclear structure has actually long been analysed in terms of mesons in nuclei and in terms of the excitation of nucleons into deltas. Electrons and hadronic probes at the GeV level (LAL Saclay, BATES MIT, Saturne II,...) have been particularly efficient tools. This new important aspect of nuclear physics, with better-performing accelerators (100% duty cycle) becoming available (MAMIB, NIKHEF, CEBAF, ...), is differentiating itself less and less from traditional particle physics.
- iv) The new state of nuclear matter, long searched for in conditions where the repulsive core of nuclear forces could be overcome, is now predicted to be a quark-gluon plasma with a critical temperature of the order of 200 MeV for the deconfinement of colour. Proper experimental conditions call for high-energy collisions and the use of high-energy facilities (the CERN SPS, the BNL AGS). As we know well, this is our own joint venture.
- v) One also sees more and more nuclear physicists working on questions such as double β decay, rare (or 'forbidden') decay modes (LAMPF, SIN,...), neutrino oscillations (Le Bugey, ...), dark-matter searches—where the questions come from traditional particle physics, but for which the experimental skills and techniques originating from nuclear physics are particularly well suited.

With this long list one thus sees that nuclear physics has to some extent lost its once specific character. The understanding of nuclear structure has reached such a status that the open questions lead directly to particle physics. More specifically, physics at the quark level, and in particular quantum chromodynamics, is the framework to which a fair fraction of nuclear physics research has now shifted.

Sure enough there is still a strong breed of nuclear physicists who are busy with more traditional nuclear matter studies at lower energies, working mainly with tandems—sophisticated cyclotrons or synchrotrons accelerating heavy ions. At stake is the study of exotic or highly deformed nuclei, or the analysis of the properties of nuclear matter in a broader sense, finding out its equation of state (GANIL, SIS, RIKEN,...). In this case however, one sees that the new questions addressed come from astrophysics, at least as well as from nuclear physics proper. Even along these more traditional lines, nuclear physics is also losing its specific character.

This is the physics aspect. There is also an important aspect of research organization.

Particle physics has been after resolution and therefore after higher and higher energies. Since the fifties, it has gone from 100 MeV (in c.m. collision energy) to 100 GeV, with

the compact and efficient understanding of structures and interactions now provided by the Standard Model. One hopes to reach 1000 GeV (1 TeV), or more, before the year 2000!

This research called for high energy and therefore for large and costly machines, such facilities becoming fewer and fewer but each one serving a larger and larger number of external users. In Europe this implied, at an early stage, an international collaboration, which has centred around CERN and, to a lesser extent, DESY.

The organization of this research the world over has been characterized by a concentration of means on a very few major laboratories use by a widely spread constituency of physicists. This has been the case everywhere—in the United States, in Western Europe, in the USSR, and in Japan.

A prominent new feature has been the emergence of colliders, which have now left but a secondary role to the fixed-target operations. With colliders, highly sophisticated detectors have to be available as soon as the machine starts up, so that it can be exploited to the full. There is nothing resembling the first-generation, second-generation, ... sequence of experiments that were once common with fixed-target machines. This is the hard lesson that was learned with the ISR, but well in time for its highly visible success at the CERN $p\bar{p}$ Collider. This means that the detectors, which have to be built together with the machine, make up a large fraction of the overall cost of the machine proper (of the order of 0.4 for LEP). This then brings up the long-considered concept of a 'World machine' in an adiabatic way, without the many obvious difficulties associated with the full operation of a laboratory on a world-wide basis. Although the construction and operation of the laboratory may remain a regional affair, the cost and use of the detectors (and also of specific machines) are more widely spread over a larger users' community, together with their respective funding agencies. The HERA model, also originating from the LEP experiments, is certain to be at the basis of many future ventures. This is already the case in particle physics, and will also soon become the case in nuclear physics.

Research in nuclear physics could long remain dispersed between many machines, each machine constructed and operated at the level of one institute and each providing conditions for front-line research.

As studies have developed, more powerful instruments have become necessary, and nuclear physics has had to concentrate more and more on larger facilities with research carried out, to an increasing extent, by external users. In Europe national facilities (GANIL, Saturne II, GSI,...) eventually took up the larger fraction of the research effort, as many tandems or cyclotrons operated at the institute level closed down or were transferred to other fields. This concentration was imposed by the cost of the required instrumentation, which could be considered only at the national level.

However, this brings up another important point. The number of users needed to exploit such facilities to the full has now reached the several hundreds mark in most cases, which is too many people for any single European country. The existing national facilities have indeed to rely on an increasing number of foreign users (50% for GANIL and Saturne II, for instance). The constituency on which the use of any such machines is based has therefore to be international in character—and this is especially true for the larger machines now being proposed.

This fact—even more than the cost of such instruments—requires a well-organized European collaboration in nuclear physics. Such a collaboration has started to emerge, but so far only on a limited empirical and case-to-case basis. It has now to find its proper form so that new projects can be discussed and planned on a broad enough basis. The organization of research in nuclear physics has to follow the same pattern as that for particle physics, developed long since. This is already the case in the United States, NSAC advising DOE on matters of nuclear physics, as HEPAP does for particle physics. It is not yet the case in Europe, where methodical planning is now urgently needed. I shall come back to this, but first—next to stressing this strong convergence of nuclear and particle physics—I have to mention a separation that remains deep-seated since it is more of a cultural than of a technical nature.

Particle physicists are after new and basic things. They look for simple yet non-trivial systems where hitherto unknown effects can manifest themselves most clearly. They search for unity and simplicity in a world of seemingly high diversity and complexity. They look for new grounds.

Nuclear physicists, on the contrary, seem to like complicated situations. They seek to have a better understanding of that which is only half understood. They chafe under the yearning to make sense out of complicated systems, and to test their understanding, looking at intricate situations. At first sight, some of their favourite problems may seem to be too complicated and of secondary importance to the typical particle physicists. Yet a second look would quickly show that they are important problems indeed. There are many examples, of which the detailed knowledge of the nucleon-nucleon scattering amplitudes up to 1 GeV and the detailed knowledge of the neutron form factor are but two.

Realizing the existence of, and overcoming, this cultural difference was one of my greatest experiences during the 1989 survey^{1,8}. This is a very positive element—although something of a problem—with the joint teams, which come from both nuclear and particle physics and which are quite numerous by now.

There is a complexity that the particle physicists cannot escape, however. It is that of the vacuum. The Standard Model is particularly simple and attractive; yet it is at work on

a complicated vacuum that hides much of the symmetry that is present, and needed, in the theory. The vacuum is far from nothingness. It can have—and seems to use the full—all the complexity of scalar fields. It is because of the complexity of the vacuum that the W and the Z acquire their high masses and that colour is confined within hadrons. This is also the way quarks and leptons get their mass. The modern vacuum has actually much in common with a superconductor. In particular, the Glashow–Salam–Weinberg electroweak theory can be considered as a more involved, relativistic, and non-Abelian version of the Landau–Ginzburg theory of superconductivity. The critical temperature beyond which the mass of the W and Z should disappear is of the order of 200 GeV. The critical temperature beyond which colour should no longer be confined is of the order of 200 MeV, and therefore—as we all here hope—well within reach in the laboratory. We even already have some good evidence that this is the case!

Understanding the deep nature of the vacuum is one of our present great challenges in physics. We may experiment much beyond the critical temperature in order to better understand the actors whose role is to create such particular conditions at low temperatures. This is the typical particle physics approach, with its present drive towards interactions at the TeV level.

One may also study an extended and complicated system going through a phase transition, something which should also happen to the vacuum since it behaves as a medium. This is a typical nuclear physics attitude. It is an ideal meeting ground, as we all know, for nuclear and particle physics.

The vacuum has often appeared complicated in the past, but this was because something fundamental had not yet been properly realized. Indeed, it looked as if it were a semiconductor before the negative energy states appearing in the Dirac particle theory were correctly eliminated in the framework of quantum field theory and antimatter was accurately understood. Its present behaviour, which in two very different respects mimics that of a superconductor, should herald much new physics, to be discovered once the phenomena are properly understood.

2. TOWARDS A SIMILAR STYLE OF RESEARCH

Frontier research in nuclear physics calls for accelerators that have higher and higher energies: for instance, MAMIB now and CEBAF in 1994 for electrons; GANIL now and soon SIS, at GSI, for heavy-ion research; with later, RHIC at very high energy indeed. Some nuclear physicists show a reluctance to being pushed to such high energies, sometimes under the rather pressing encouragement of the particle physicists with whom they collaborate.

Indeed for the former, the high-intensity, high duty cycles, and the possibility to use detectors looking simultaneously at many facets of the same reaction, are more important than sheer energy and the increased resolution that are sought by the latter. The nuclear physicists are even more reluctant because for them it means a big change in their style of work and not a continuation of it, even if this change is to a hitherto unknown domain of physics—as is usually the case for the particle physicists⁹.

I think that our domain of research has shown how fruitful such joint ventures can be, and how early reluctances or fears on both sides can be overcome. Nuclear physicists have learned not to be frightened by a big jump ahead in energy, thus running the risk of losing their proven technical abilities. Particle physicists have learned not to be frightened by very complicated events, with the risk of getting muddled up in a mess of secondaries.

Yet what is largely new with nuclear physics is the size (and cost) of each of the new ambitious projects and the large constituency of physicists which their proper use requires. New projects have now to be discussed, planned, and realized on a wide basis, which in Europe is automatically international. The structures that exist in the United States are still lacking in Europe.

There is no nuclear physics panel equivalent to ECFA, and this lack has recently been strongly felt owing to the difficulties of carrying out quick and efficient negotiations for the realization of a lead source for the CERN SPS, which clearly involved a large enough constituency at the European level. Whilst ECFA can be seen as the equivalent of HEPAP in the United States, there is no equivalent of NSAC. Such a forum, where new projects could be discussed and constituencies assessed, and where a good merging could be achieved between machine designs and physics studies, should be urgently set up. Recently, NuPECC came into being. At first a panel of laboratory directors, NuPECC was organized to provide some much-needed co-ordination. It has now been enlarged into a wider body, but it would still help if NuPECC could convene something equivalent to ECFA, thus offering to all potential users a forum where they could talk about new proposals at the full European level.

As previously said, the constituency needed for a large new facility in Europe can be found only at the European level. This is especially the case for the high duty cycle electron machine which many nuclear physicists would now like to see built.

The main purpose of such research is to come back to the famous SLAC results on the quark structure of hadrons (10–15 GeV) with a continuous high current, so that coincidence experiments could be done with proton and nucleus targets. The EMC effect first observed with pulsed muon beams, with its striking result that the quark distribution in a nucleus is different from that found with free protons, calls for a more profound understanding, which implies looking at what takes place during deep-inelastic scattering, studying the hadrons

recoiling from the scattered lepton.

Such a machine, if it were to take priority over a medium-energy, high-intensity proton machine of the KAONS type, would require a large number of users (three to four hundred), in much the same way as CEBAF already does. The constituency has to be accepted as the much-needed complement to the physics case. This also applies to the use of smaller machines, and in particular to the new tandems (for instance, the Vivitron). In this case it is the detectors needed for extracting new information from such machines that here require the backing of a European collaboration. One even sees appearing the concept of ambulatory detectors, making proper use of the—perhaps too many—lower-energy machines that were built when co-ordination was not yet effective enough. This is especially the case for the projects Eurogamma and Euroball, which are similar to the project Gammasphere in the United States. The same applies also to medium-energy heavy-ion physics ($\sim \text{GeV}/A$) where, in Europe, efforts should eventually be transferred to the SIS or GSI for maximum efficiency.

In all domains of nuclear physics—and not only those which now have a strong interface with particle physics—one perceives the need for a stronger collaboration. Not only funding but also constituency, which is more and more a prerequisite for funding, can only be achieved on a wide basis which, in Europe, requires a tightly knit international collaboration.

3. THE COSMIC CONNECTION

Nuclear and particle physics have also an exciting meeting ground in their common connection with cosmology. In the Standard Cosmological Model, the temperature of our expanding Universe drops first as the inverse square root of time. The closer one tries to approach to the Big Bang, the higher is the energy per particle (1 MeV at 1 s, 100 GeV at 10^{-10} s). When trying to understand what happened in the distant past—something which has direct relevance to many questions raised by the study of the cosmos at large—one has to use the results of traditional nuclear and particle physics and of the physics only recently learned with the $p\bar{p}$ colliders and LEP.

In between, when the Universe was 10^{-6} to 10^{-5} s old, is the physics of the transition from quark matter to hadronic matter, which we are looking for. The Universe was first a quark-gluon plasma, and was hadronized as the temperature fell below 200 MeV.

The early Universe has not been discussed explicitly at this Conference, but the subject remains an important aspect of our research. Hadronization occurs when the temperature is already much below the nucleon mass, and this may lead to large fluctuations between baryon-rich and baryon-poor regions. This has much relevance to the limit on the baryonic density derived from the relative abundance of the light elements, and therefore to the possible role

of hitherto unseen baryonic matter, which the standard approach has limited to 0.2 of the critical density. Our new views about hadronization proceeding from a quark-gluon plasma have indeed reopened the question of the baryon density in the Universe and with it that of the primordial lithium density, which is still at the origin of lively astrophysical debates.

4. CONCLUSION

Nuclear and particle physics, which separated clearly from each other in the late fifties, are now developing more and more interfaces. This is associated with an important mutation in the style of research in nuclear physics, which brings it in line with the research which so far has been typical of particle physics and astronomy. In several domains of topical research, we also find mixed teams coming from both nuclear and particle physics. Quark matter, with its present momentum and success, is but one facet of a more general trend associated with an important change of nuclear physics. Our domain of research bears witness to the success and the potential of such ventures, which benefit from the skills and techniques borrowed from different fields of physics. It shows, at present, a very good complementarity of the different facilities in the world. As the backbone of our present and future research, we will indeed see CERN following BNL with the heaviest projectile at larger energies as the lead-source SPS injector comes into operation in early '94. We will next see RHIC, a few years later, carrying this research to much higher energy and under very different initial conditions. This collider research should pave the way for the exploitation of the LHC with lead-lead collisions at 3.6 TeV/A beam energy. The exploratory runs of the late eighties have been very encouraging, and we can anticipate a bright future.

Quark matter shows how nuclear and particle physicists, despite their initial cultural differences, can work together fruitfully, pooling their resources to make progress along very exciting lines of research, but this does not take place without difficulties in domains that fall in between the proper realms of funding agencies. So, as Benjamin Franklin once said at a critical period of American history: 'Let us hang together or we shall hang separately'.

REFERENCES AND FOOTNOTES

- [1] The members of the survey organized by the Académie des Sciences, and commissioned by the Minister for Research and Technology, were G. Charpak, P. Darriulat, M. Goldman and M. Jacob. The report has now been made public as: Rapport 24 de l'Académie des Sciences 'La physique nucléaire fondamentale' (1989).
- [2] Künftige Entwicklung der Grundlagenforschung in der Mittelenergie und Kernphysik. Bericht des ad-hoc-Ausschusses des Bundesministers für Forschung und Technologie 1988. The members of the German audit were A. Faessler, P. Kienle, K. Kleinknecht, A. Richter, H. Specht and J. Wess.
- [3] For an authoritative review, see I. Sick, Europhysics News, June 1990.
- [4] See, for instance, Proc. Boulder Workshop on Long-Range Plan for Nuclear Science '89.
- [5] See, in particular, the recent results of NA35 and WA85, reported at this Conference.
- [6] Remember that it took 10 years from the first observation of anomalous large p_T effects at the ISR (1972) to the unambiguous production of spectacular jets at the $p\bar{p}$ collider (1982). In 1981 there was still 'evidence' for the absence of jets in hadronic collisions.
- [7] I personally think that this ϕ enhancement is related to jet fragmentation: the larger the E_T , the larger the probability of having a 'jetty' event. At this energy, jets cannot be seen clearly (see Ref. [6]). Nevertheless, we now have no doubt that they are there and are often at the origin of large- p_T secondaries. As is well known for K's and π 's, the leading fragmentation of a jet into a ϕ is not much reduced compared with its leading fragmentation into a ρ or an ω . Experiment NA38 may thus see the jets that NA5 failed to see a decade ago!

It is also clear that both the formation of a QGP and the abundant production of minijets ($p\bar{p}$ collider collisions) are expected to yield an enhancement of strangeness production.

- [8] This is not a personal bias. M. Tannenbaum, private communication at this Conference.
- [9] This can be rephrased in an allegorical way with the story of the chicken and the pig. The chicken and the pig are walking together down the road in the early morning, when the pig says:
'What about some breakfast?'
'Yes,' answers the chicken, 'what about ham and eggs?'
'Well, I am somewhat reluctant', says the pig. 'For you it is only a sacrifice, whereas for me it is total commitment!'