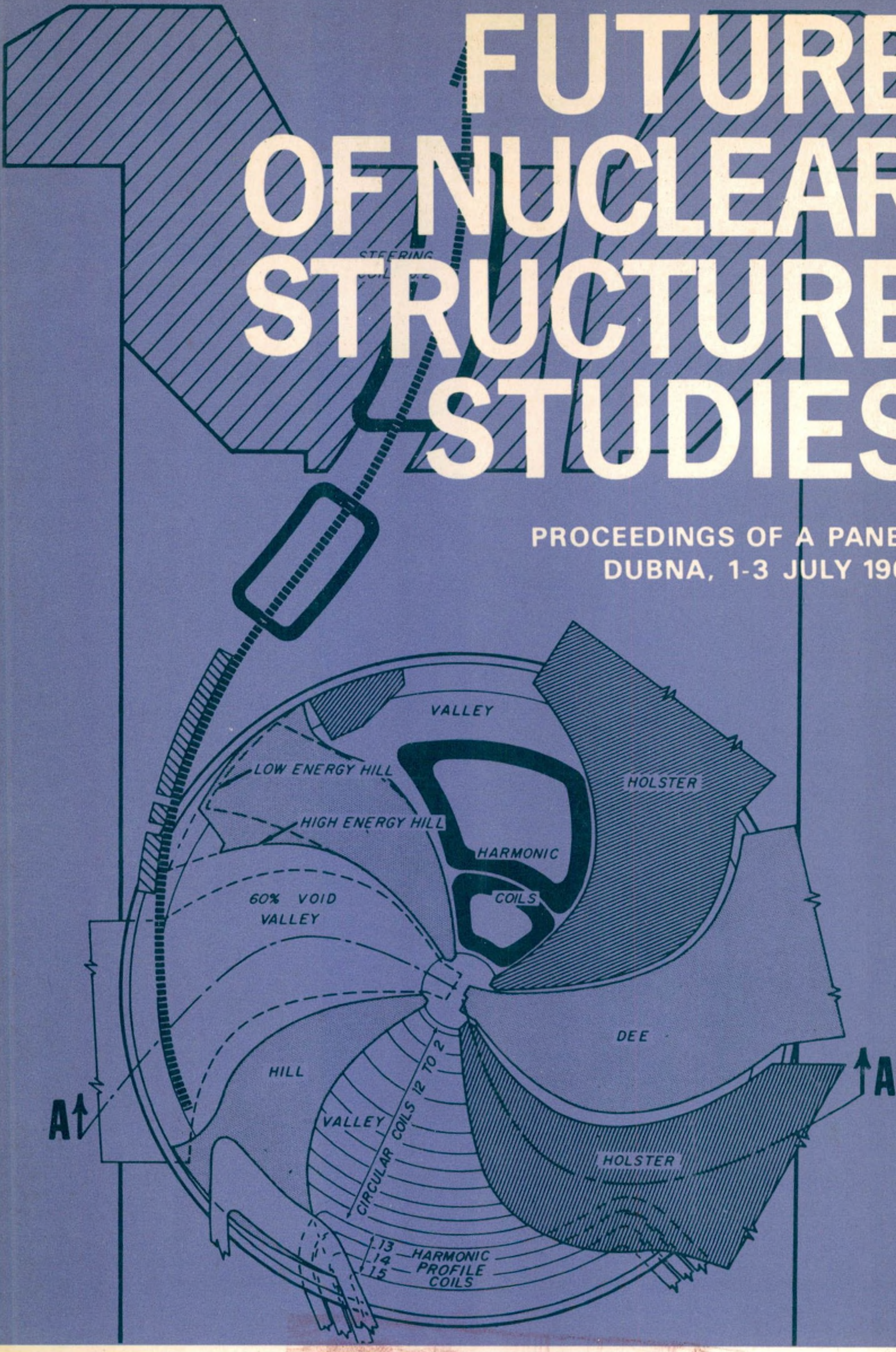
FUTURE OF NUCLEAR STRUCTURE STUDIES

PROCEEDINGS OF A PANEL,
DUBNA, 1-3 JULY 1968



INTERNATIONAL ATOMIC ENERGY AGENCY, VIENNA, 1969

FUTURE OF
NUCLEAR STRUCTURE STUDIES

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PANEL PROCEEDINGS SERIES

FUTURE OF NUCLEAR STRUCTURE STUDIES

PROCEEDINGS OF A PANEL ON
THE FUTURE OF NUCLEAR STRUCTURE STUDIES
HELD IN DUBNA, 1 - 3 JULY 1968

INTERNATIONAL ATOMIC ENERGY AGENCY
VIENNA, 1969

FUTURE OF NUCLEAR STRUCTURE STUDIES
(Panel Proceedings Series)

ABSTRACT. Proceedings of a panel convened by the IAEA and held in Dubna, 1-3 July 1968. The meeting was attended by 34 experts from 10 countries.

The content includes papers by leading nuclear physicists on such subjects as: prospects for the synthesis of new isotopes and elements; future developments in nuclear structure studies; new research tools; reaction electron- and gamma-spectroscopy; and mass spectroscopy. Open problems and future developments are stressed both in the papers and in the discussions. Papers are also included on organizational aspects of nuclear physics in small and developing countries, on the role of international scientific centres and on the possible development of regional centres.

All papers are in English, with the exception of one which is in Russian. The abstracts, discussions and recommendations of the panel are in English.

(163 pp., 16 × 24 cm, paper-bound, 62 figures; 1969) Price: US $5.00; £2.1.8

FUTURE OF NUCLEAR STRUCTURE STUDIES
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FOREWORD

As the world enters an age in which nuclear electric power is a reality and man increasingly benefits from nuclear applications in medicine, agriculture and industry, it is useful to re-examine the basic sciences that underlie these important developments. In many ways modern nuclear technology is built on, or derived from, basic research on the atomic nucleus. Today, nuclear physics studies are a fundamental part of the national atomic energy programs in many of the Member States, now more than 100, of the International Atomic Energy Agency.

The purpose of the IAEA Panel on the Future of Nuclear Structure Studies, held at Dubna on 1-3 July 1968, was to provide an appraisal of the present status of our knowledge of nuclear structure, to point out the open problems, and to suggest the most promising directions for future research. To reflect the needs of many of the IAEA member nations, special consideration was given to the problems of smaller institutes and developing countries.

The major recommendation of the panel concerned the question of regional centres for low- and medium-energy nuclear physics research. The panel supported the organization of regional centres formed by three or more developing countries and equipped with apparatus of intermediate cost and sophistication. The text of the resolution appears at the end of this book, which contains the papers presented to the panel and a record of the discussions.

The panel was held in conjunction with the Dubna-sponsored International Symposium on Nuclear Structure, which took place immediately after the panel, on 4-11 July 1968. The invited papers from this symposium, all in English, were published by the Agency in 1968 under the title "Nuclear Structure: Dubna Symposium 1968".

Special thanks are due to the co-chairmen of the panel, Professor V. F. Weisskopf, of the Massachusetts Institute of Technology, USA, and Professor V. G. Soloviev, of the Joint Institute for Nuclear Research, Dubna, who guided the work of the panel.

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THE RELEVANCE OF NUCLEAR PHYSICS

THE RELEVANCE OF NUCLEAR PHYSICS

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I am asked what nuclear physics is about, that is, nuclear physics as distinct from particle physics and other parts of physics. I see three trends in this science. One is the discovery of new phenomena, phenomena of nature which we have not seen or observed, of which we did not know anything before. The second trend, I would say, is towards the solution of fundamental problems, the answers to certain basic questions in physics; I shall give some details later on. The third is the construction of new concepts in physics necessary to deal with the problems not only in nuclear physics but also in the rest of physics.

The order of these three items is unimportant. Let me start with the first. The first topical paper in this Panel is Dr. Flerov's talk on supernuclei. This is a field in which we explore new phenomena, like discovering a new continent. We do have some theoretical ideas of how these nuclei may behave and what properties they may have; I am sure, and you are probably too, that these ideas are all wrong. I would go so far as to say that I would consider the attempts of Dr. Flerov and his colleagues a failure, if it turns out that the theories are correct. His discoveries would be much more interesting if the theories turn out not to be correct. The discovery of transuranic nuclei is a typical example of nuclear physics going into a world of new phenomena never seen before, and I think the best example within this field of science. But there are other examples. The researches described by Dr. Wilkinson are of a similar character. The reactions of nuclei with the new particles obtained by high-energy accelerators are much the same. Here we are also looking at new phenomena which probably cannot be predicted but can only be guessed at; this work goes under the name of surface phenomena.

Since we do not know very much about the nuclear surface, I am convinced that, if we get a deeper understanding of the nuclear surface by those strongly absorbed particles like pions and kaons, we will find phenomena that are new, that are unexpected. The whole world of hypernuclei also belongs in this category. Here very little research has been done yet and again I am sure that, if and when we have accelerators which provide high-intensity pion and kaon beams, the nuclear physics of hypernuclei will be a new science and a broad science which, just because it is a variation of our ordinary nuclear physics, will give us some unexpected and most interesting information about nuclear structure. In some ways one could consider that a nucleus is only the lowest level of a kind of molecular spectrum and that there exists a higher internal excitation, over which there is again a substructure of levels. This higher step is the hypernucleus or the double hypernucleus, and so on. So this is a new world which is yet to be discovered. There are more, but I would like only to give these three examples.

The second aspect, to my mind, which illustrates the importance of our subject is what I call the fundamental problems. The obvious fundamental problem at the centre of nuclear physics is the nature of the force between elementary particles, between the nucleons.

In fact, one should really formulate it a little more generally, one should not talk about the force between nucleons because that already presupposes the existence of such a force; this means that one can find a potential, a 2-body, 3-body or 4-body potential. Very probably, this is only an approximation. We know now, on the basis of meson physics, that it must be an approximation. There is a fundamental problem here, a problem which intrigues me very much: Why is it that we can describe nuclear phenomena so well by assuming ordinary or at least velocity-dependent forces or exchange forces between nucleons?

When you compare the situation, for example, with atomic, molecular and solid-state physics, obviously you cannot describe phenomena in the same way; you cannot explain the cohesive energy of a metal and problems of this kind by potentials between atoms — we know that the electrons play an important role. Yet if you really look at the distance of atoms in a solid and of nucleons in nuclear matter, the ratio of these distances to the actual size of the object, namely of the atom in one case and the nucleon in the other, is almost the same. Why is it then possible in one case and not in the other to introduce a force potential? Perhaps some of our more informed colleagues can give a better answer to this than I am able to. Why can we explain nuclear structure on the relatively simple basis of a nuclear force between nucleons? Perhaps we won't in the future, perhaps in 10 years or so nobody will express the structure of the nucleus by an N-N force, but at present it seems we can.

Let me compare nuclear physics and elementary particle physics with molecular chemistry and atomic physics. There is a certain parallel here. At present it seems as if chemists, namely the nuclear structure people, can work very independently of the physicists, namely the high-energy people. This was not so in the history of chemistry. In fact, chemistry could only really develop when the structure of the atom was completely understood. Perhaps this had something to do with the fact that the binding energies of molecules are much closer to the excitation energies of the atom than the binding energy of nuclei are to the excitation energy of the nucleons. What I wanted to indicate is that we do have here a fundamental problem which we have to solve.

Now I shall move on to the third aspect, the construction of new concepts. This, I think, is a very important part of nuclear physics. In dealing with the problems of nuclear structure, new ways of thinking have been introduced that were not only useful for nuclear physics but have their own use in many other fields. The problems of many-body systems are concerned here.

Somehow, the nucleus is an extremely good many-body problem, good in the sense of a good object to study. It is not as big as the solid state, where the microscopic element is almost hidden; it is better than the atom because it is a more democratic system; the atom is an authoritarian system with one nucleus in the centre which is the source of the main power. So in many ways a nucleus is a good object for studying what a many-body problem is. And in fact the results are also visible. Nuclear physics has produced a number of new ideas or has improved older concepts, such as collective modes and quasi-particles. The quasi-particle concept was introduced by Frenkel and Landau and originated in solid-state physics. At that time, there was no difference between solid-state, nuclear and elementary particle physics. The new concepts in many-body systems, such as quasi-particles and collective modes, have been developed further and have taken on new aspects in nuclear physics which they did not have in solid-state physics. A large number of new concepts have been developed in nuclear physics, such as the optical model, the analogue state, the compound nucleus, and many more, and these are important indications of the value of the subject.

I have just sketched three aspects of the scientific value of nuclear physics. Let me now, with this as a background, say a few words on the direction in which this field may or should develop. After all, this Panel is devoted to the future of nuclear physics.

Nuclear physics is in an interesting and strange position between elementary particle physics and the other parts of physics. I once used in one of my articles the expression 'extensive and intensive science'. An extensive science is one which has a lot of applications like atomic physics today; here the degree of extensivity is very large because atomic physics is used from biology to astrophysics, apart from its industrial applications.

An example of an intensive science is elementary particle physics. It is at present a spearhead of research and therefore has not yet much application in other fields of research and human activity. The longer a science exists the more extensive it becomes; new intensive fields are discovered all the time. Fifty years ago atomic physics was as intensive, in this definition, as high energy physics is today. Nuclear physics today is in the middle. It definitely has some intensive and some extensive characteristics, and this makes it so interesting. It also makes it somewhat difficult to plan ahead and to say what will be the most important and the most interesting aspect because there is increasing pressure from the side of application, and I mean here not only technical applications but applications in other sciences. There is pressure also coming from the yet unsolved fundamental problems. So one must be careful to shift the emphasis of research from one to the other so that we get maximum benefit from the field. Let me say therefore a few words about the extensive side or, if you wish, the application side of nuclear physics.

Again I say I am using the word 'application' in the most general way and emphasize application to other fields of knowledge rather than technology. (I say this not because I despise technology but because the technological applications are rather obvious and do not need to be talked about so much). Well, the application of nuclear physics in this sense becomes wider and wider every day.

Let me first mention astrophysics. As Dr. Teillac's paper shows, one gets directly into quite fundamental questions of the development of the universe and astrophysics with relatively small means. It seems to me that astrophysics is the most direct and most impressive application of nuclear physics, because the cosmos is the place where nuclear physics is at home.

If we do not look at the cosmos, nuclear physics plays a strange role: we spend a lot of money to ask a question and later on we spend even more money to answer the question. Nuclear physics, with a few notable exceptions, does not exist on earth except when we create it. But we know now that nuclear processes do exist and in fact are quite essential in the stars. This is why the application of nuclear physics to astrophysics seems to me one of the most exciting and most relevant applications of nuclear physics. I emphasize this strongly because I have the feeling that this application is to some extent neglected, neglected by theorists and by experimentalists. It is perhaps due to a kind of conservatism in the transition of nuclear physics from an intensive into an extensive science.

More effort should be spent in thinking about those experimental and theoretical studies that are of importance in astrophysics. There are quite a number. I do not wish to go into detail, but certainly experimentally there are a number of processes which should be investigated which are of obvious importance for the energy production in stars, such as alpha capture processes, then the oxygen-oxygen, carbon-carbon, or the oxygen-proton cross-sections; these are reactions which can be investigated with relatively small means since the energies in stars correspond to the energies of our small apparatus and not to the bigger ones, although the intensities are so weak because of the Coulomb barriers. A lot of theory is needed here also to extrapolate the experiments to those values which are relevant to astrophysical processes. Dr. Flerov mentions the cosmic importance of super-nuclei in his paper. This is only a very short sketch of a field that is extremely exciting, fundamental and relevant for nuclear physics and, as I said, I believe that we should spend more effort on these problems. This effort should be taken away from studying nuclear problems for their own sake.

There are many things in nuclear physics which we do not understand and where more research is needed. But on the whole we are a little further already in nuclear physics than we were 20 years ago and it is therefore, I think, now time to get away from a too narrow view of nuclear physics, towards a view which every scientist should take, namely to a general search for understanding of the world in which we live. This is why astrophysics should have an essential place.

So much about fundamental applications. There are, of course, many applications of nuclear physics today which make this science more and more extensive. For example, the applications in solid state become more important every day: the investigations of interatomic fields, magnetic field distributions within the solid, the investigation of crystal structure, the structure of liquids, neutron spectroscopy. In other words, the nucleus is an extremely fine and practical tool for the investigation of atomic structures.

But perhaps equally important, and since I am a theorist, in my own mind perhaps even more important, are the applications of the concepts which are created and developed in nuclear physics. I consider the knowledge about many-body problems which we have assembled in nuclear physics one of the most important sources of application. If we understand the many-body problem in the nucleus, it will give us help and training to understand similar problems in solid state or in atoms. Let me be critical, since this is the purpose of the Panel. Our theorists as well as our experimentalists are too specialized. I believe that a theoretical physicist who works on the many-body problem of the nucleus ought to be equally interested in the many-body problem of the atom and solid state. It is the same problem and, let me add, it is the same Nature. I believe that, if one only looks at the many-body problem in the nucleus, there is a danger of specialization which not only prevents people from coming to the right conclusions but also makes the subject much less interesting.

There are questions in this connection which I do not understand much about: I look at them from the point of view of the amateur. I would like to ask the question why it is that atoms behave so differently from nuclei, that they have fewer manifestations of collective modes. I was told yesterday by Gerald Brown that one has discovered collective modes also in atoms, but certainly they are less pronounced and that must have a reason. So it seems to me that questions of comparison between different manifestations of the many-body problem are very pedagogical. I mean pedagogical in the sense that we learn a lot about nuclear and solid state phenomena by looking at the many-body problem as a whole and not from one side only. Of course, this wider view is also important for the experimental physicist. In planning experiments and in planning machines, one ought to keep in mind for which development the innovation will be most fruitful and will give us the most information, not about specific nuclei but about nuclear physics as a whole, about physics as a whole. Somebody has said recently that one should not investigate the spectrum of the rare earths just in order to understand the rare earths. We should investigate it in order to understand nuclear structure in general or the many-body problem in general. This is easy to say and harder to do because detailed investigation is always of importance whatever it is and if you do not understand the rare earth spectrum you will not understand the many-body problem. But there are limits to it. We must find the right way between these limits. This is difficult, in particular in the next decade when the financial support for physics is going to be less than it was. I hope I am wrong, but I fear I am right. Under these conditions, the correct choice, not only of what experiment every single individual should undertake, but of what machines one should build and how one should organize research, becomes much more critical than it was in the past.

Let me not go much further in my discussions. There are many questions I have not tried to answer. I have attempted just to sketch some questions, such as what kind of nuclear physics should be supported, should one go to high-energy machines, should one build more tandems, more Van de Graaff's, what can one do with low energy and with high energy. These are very difficult questions and they also have a lot to do with the problems to be discussed at this Panel, such as what can different nations do, not only what should they do, but what can they do, and how can we see that it is done. I just wanted to emphasize the problems and outline the situation of nuclear physics within the framework of physics as a whole.

This meeting should be concerned not only with the factual questions of science, but also with the, let me say, philosophic and practical questions of nuclear physics. Why do we do nuclear physics, what is the sense of it, what is the meaning of it and, most importantly, how can we defend the support of nuclear physics, how can we convince the governments to spend money on such a thing, which to a certain extent is our pleasure? And so we will have to be quite clear among ourselves that this is a very important matter. I hope we all believe this.

SCIENTIFIC ASPECTS

ПЕРСПЕКТИВЫ СИНТЕЗА НОВЫХ ИЗОТОПОВ И ЭЛЕМЕНТОВ

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Abstract — Аннотация

PROSPECTS IN THE SYNTHESIS OF NEW ISOTOPES AND ELEMENTS. The methods of new element production: neutron reactions, high energy proton reactions, heavy ion reactions, fission from excited states, are considered. The possibility of existence of the region of stable super-heavy nuclei and methods of synthesizing various isotopes of elements with Z from 114 up to 126 are discussed.

ПЕРСПЕКТИВЫ СИНТЕЗА НОВЫХ ИЗОТОПОВ И ЭЛЕМЕНТОВ. Рассматриваются способы получения новых элементов: реакции на нейтронах, на протонах высокой энергии, реакции под действием тяжелых ионов, деление из возбужденных состояний. Обсуждается возможность существования области стабильных сверхтяжелых ядер и методы синтезирования различных изотопов элементов с Z от 114 до 126.

В своем докладе я хотел бы остановиться на некоторых вопросах, связанных с перспективами синтеза и изучения свойств новых изотопов и элементов, в областях далеких от полосы стабильности. При этом, естественно, будет сделан упор на многочисленные результаты, полученные благодаря тщательному изучению свойств радиоактивных изотопов, изучению основных закономерностей, действующих в ядре. Следует отметить, что все достижения в ядерной физике были возможны только благодаря самоотверженному труду огромного отряда физиков, которым приходилось исследовать как можно более широкий круг явлений, входить во множество деталей, делать порой черновую работу, которая в дальнейшем, однако, оказала огромное влияние на развитие науки и техники.

СИНТЕЗ НОВЫХ ИЗОТОПОВ

После открытия Беккереллем, Марией Склодовской-Кюри и Пьером Кюри естественной радиоактивности и открытия Жолио Кюри искусственной радиоактивности начало экспериментальных исследований не существующих в природе изотопов и элементов можно отнести к 1934 году, когда группа итальянских физиков во главе с Э.Ферми проводила опыты по изучению захвата нейтронов различными элементами. Аналогичные исследования в те годы проводились в Советском Союзе, в Лаборатории И.В.Курчатова.

При изучении структур стабильных и радиоактивных ядер ядерная физика, с накоплением экспериментального материала, раскрывает картину строения ядерной материи. Естественно, поэтому хочется проанализировать достижения в этом направлении. Сколько уже синтезировано и изучено изотопов и сколько еще предстоит получить и изучить в будущем? Ответ на этот вопрос можно получить, взглянув на рис.1.

На этом рисунке изображены как уже синтезированные изотопы, так и изотопы, которые будут синтезированы в дальнейшем [1]. По осям отложены Z (число протонов в ядре) и N = A - Z (N — число нейтронов в ядре). Стабильные изотопы изображены темными квадратами. Внешний контур, представленный сплошной линией, обозначает границы области стабильности, полученные на основании теоретических оценок. Оценка числа возможных изотопов в этой области дает величину ~4000 – 5000. До настоящего времени было синтезировано 1500 изотопов.

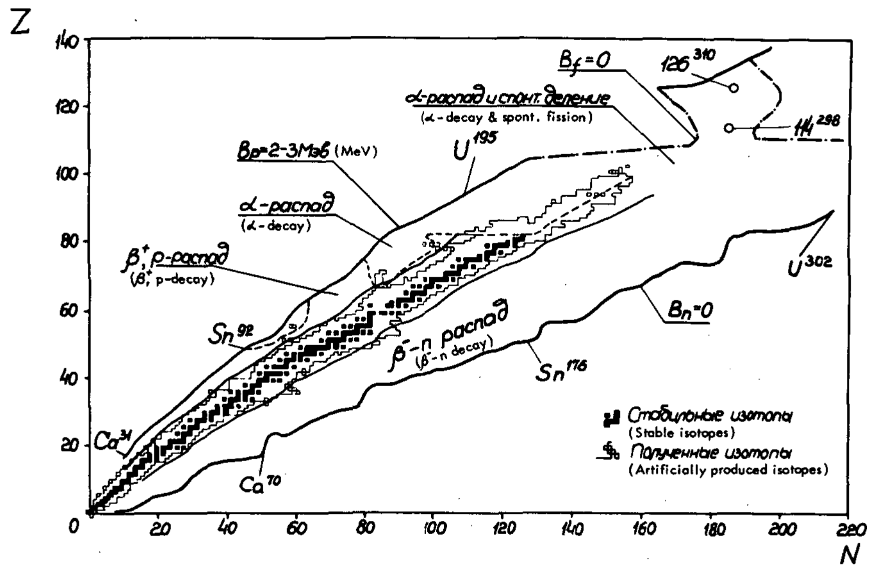

Рис.1. Карта известных изотопов и тех изотопов, которые могут быть синтезированы в будущем.

FIG.1. Chart of the known isotopes and the isotopes which may be produced in the future.

Что нового может дать синтез и исследование еще неизвестных изотопов? Я думаю, что именно здесь природа может предоставить различного рода неожиданные находки: новые островки стабильности, новые области деформации, изомерные состояния различных типов и многое другое. При синтезе таких изотопов, как кальций – 31 или кальций – 70, получаются ядра с очень необычным соотношением протонов и нейтронов, кулоновских и ядерных сил. В связи с этим становится также понятным особый интерес к изучению трансурановых элементов, где кулоновские силы очень велики, и поэтому почти не существует обычного барьера деления, а различные оболочечные эффекты могут быть выражены чрезвычайно сильно. Именно в этой области элементов открыт новый вид ядерной изомерии: изомерия формы [2]. Здесь же возможен ряд других интересных явлений, связанных, например, с наличием второго минимума в энергии деформации ядра [3], предсказанного Струтинским, и т.д.

МЕТОДЫ ПОЛУЧЕНИЯ НОВЫХ ИЗОТОПОВ

Для синтеза новых изотопов представляют интерес несколько типов реакций:

а) Реакции, вызванные заряженными частицами средней энергии (p,xn)(α, xn), а также реакция с He^3 дают нейтронно-дефицитные изотопы. Нейтронно-избыточные изотопы можно получить в реакциях (n,p)(n,α)(p, 2p)(d, p).

б) Использование нейтронных потоков большой плотности и низкоэнергетическое деление приводят к образованию нейтронно-избыточных ядер.

в) В реакциях, вызванных протонами высокой энергии (скалывание, деление после быстрого каскада), образуются и нейтронно-дефицитные и нейтронно-избыточные ядра.

г) В реакциях под действием тяжелых ионов (полное и неполное слияние, деление при высоких энергиях возбуждения) также образуются и нейтронно-дефицитные и нейтронно-избыточные ядра.

При сравнении различных методов получения изотопов необходимо знать сечения этих реакций. Эти значения либо известны, либо их можно получать экстраполяцией из известных сечений подобного типа реакций. Учитывая полученные значения сечений и имеющиеся интенсивности пучков на ускорителях или реакторах, можно оценивать эффективность различных методов получения изотопов в широкой области Z и A. При сравнении целесообразно разбить все методы синтеза на две группы:

1. Направленные реакции, в результате которых образуются один-два представляющих интерес изотопа.

2. Реакции, в которых одновременно образуются изотопы в широкой области Z и A. Преимущество первого типа - в простоте идентификации полученных изотопов (по кинематике реакции, по функции возбуждения и так далее).

Реакции первого типа удобны, но они не дают возможности далеко отойти от полосы стабильности. Кроме того, не всегда возможно подобрать необходимую комбинацию мишень-частица. Реакции второго типа сразу дают большой выбор изотопов в широкой области Z. Однако при этом возникают проблемы экспрессного разделения изотопов и их идентификации. Особенно большие трудности возникают при прецезионных спектрометрических исследованиях короткоживущих изотопов. Вместе с тем в настоящее время существуют анализирующие системы, работающие непосредственно "на пучке" ускоренных частиц ("on-line"системы).

В частности, в Лаборатории ядерных проблем (Дубна) имеется ряд установок, работающих в режиме "on- line": газонаполненный сепаратор для быстрого выделения (10^{-6} сек) радиоактивных продуктов ядерных реакций с разрешением по массе $\Delta A \simeq \pm 0{,}03\,A$, электромагнитный масс-сепаратор ядер отдачи с разрешением по массе $\Delta A = \pm 0{,}002\,A$ с временем разделения для газообразных продуктов $\sim 5 \cdot 10^{-3}$ сек, установка для экспрессного непрерывного химического разделения продуктов и др. Поэтому в докладе не будут рассматриваться вопросы, связанные с выделением и идентификацией изотопов.

Преимущества и недостатки различных способов синтеза подробно рассмотрены в работах Рудстама [4]. Из этого анализа вытекает, что большинство методов не дает возможности далеко отойти от полосы стабильности. Исключение составляют реакции, вызванные протонами высокой энергии. Схема этих реакций следующая:

После взаимодействия протона высокой энергии с ядром мишени происходит либо процесс быстрого нуклонного каскада, либо фрагментация, в результате чего ядро приобретает значительную энергию возбуждения. В дальнейшем осуществляется испарение нейтронов или деление. Вероятность образования набора ядер с различными энергиями возбуждения, после быстрых процессов, рассчитывается по методу Монте-Карло с набором констант взаимодействия σ_{pp}, σ_{nn}, σ_{pn}, $\sigma_{p\alpha}$, $\sigma_{n\alpha}$ и т.д. Используя подобные расчеты, а также опираясь на экспериментальные данные, Рудстам вывел формулу для расчета сечения образования изотопов после быстрых процессов, зависящую от четырех параметров. Медленные процессы рассчитываются по обычным формулам ядерной физики. Экспериментально было показано, что относительный ход этих сечений меняется слабо при переходе от энергии протонов 500 – 800 Мэв к энергии до 30 Гэв, что видно на рис.2, где представлены кривые выхода изотопов I(Z = 53) при делении урана протонами 590 Мэв и 18 Гэв [5].

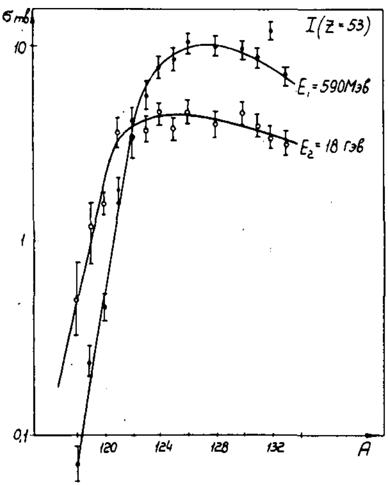

Рис.2. Кривые выхода изотопов I (Z = 53) при делении урана протонами с энергией E = 590 Мэв и E = 18 Гэв.

FIG.2. Isotope yield curves of I (Z = 53) in the fissioning of uranium with protons of energy E = 590 MeV and E = 18 GeV.

Из этих данных также можно заключить, что использование протонов высокой энергии, чрезвычайно эффективно для синтеза изотопов с $Z \leq 75$. Следует отметить, что этот метод в течение последних десяти лет с успехом использовался в Лаборатории ядерных проблем для синтеза и изучения свойств изотопов и изомеров в широкой области Z и A.

Для синтеза изотопов трансурановых элементов могут быть использованы либо нейтронные потоки большой плотности, либо тяжелые ионы. Синтез большинства известных изотопов, до фермия включительно, осуществляется путем облучения урана или плутония нейтронами в ядерных реакторах или при взрывах ядерных устройств. Суть этого метода состоит в том, что в нейтронных потоках большой плотности ядра последовательно захватывают несколько нейтронов до того, как они испытывают β-распад. Это дает возможность получить из исходного ядра ^{238}U очень тяжелые изотопы урана, которые в дальнейшем после нескольких β-распадов превращаются в изотопы более далеких элементов. Оптимистические оценки показывали, что таким образом можно получить элементы с большим Z (Z = 110, 112 и т.д.). Проделана грандиозная работа, получена большая плотность нейтронов, разработаны методы экспрессного химического выделения трансурановой фракции, но попытка получить ядра с $Z > 100$ не увенчалась успехом [6].

В то же время, с 1954 года, в Советском Союзе и в США параллельно развивались методы синтеза в реакциях с тяжелыми ионами, что позволо синтезировать элементы до 105 включительно. В настоящее время число синтезированных изотопов в трансурановой области близко к 100.

СИНТЕЗ ИЗОТОПОВ В РЕАКЦИЯХ С ТЯЖЕЛЫМИ ИОНАМИ

При облучении тяжелыми ионами синтез изотопов можно осуществлять в реакциях следующего типа:

А. Реакции неполного слияния

Реакции неполного слияния имеют много разновидностей, каждая из которых имеет сложную зависимость от большого числа параметров (энергия, заряд и масса частицы и ядра мишени, структурные свойства взаимодействующих ядер и т.д.). Из всего набора реакций для синтеза изотопов наибольший интерес представляют реакции многонуклонных передач, когда в передаваемом комплексе отношение числа протонов к числу нейтронов может сильно отличаться от единицы. Эти процессы могут быть интересны с точки зрения получения как нейтронно-дефицитных, так и нейтронно-избыточных ядер. В Дубне, например, наблюдалась с заметным сечением передача 5 – 8 нейтронов [7]. Однако систематического изучения многонуклонных передач не проводилось, и поэтому трудно предсказать, насколько перспективным окажется этот метод для синтеза изотопов, сильно удаленных от полосы стабильности. Необходимо в ближайшее время провести систематическое исследование реакций многонуклонных передач.

Б. Реакции полного слияния с испарением нейтронов

При взаимодействии тяжелых ионов ($A \leq 40$) с ядром мишени одним из основных процессов является образование составного ядра с большой энергией возбуждения. Переход возбужденных ядер в основное состояние сопровождается, в основном, испарением нейтронов, что приводит к образованию нейтронно-дефицитных изотопов. Это дает широкие возможности для исследования ядер, лежащих вне полосы стабильности, при изучении α-распада, протонного распада и т.д. Сечение таких реакций можно записать в общем виде

$$\sigma(A_c - xn) = \sigma_c P_{xn}(E^*) \prod_{i=1}^{x} \frac{\Gamma_{ni}}{\Gamma_{полн}}\ ,$$

где σ_c – сечение образования составного ядра, $P_{xn}(E^*)$ – вероятность того, что ядро с энергией возбуждения E^* испустит x нейтронов. Такие реакции, в частности, чрезвычайно удобны для синтеза нейтронно-дефицитных изотопов – излучателей протонов. Так, в Дубне, Карнаухову с сотрудниками [8], а затем канадским и американским физикам [9] удалось синтезировать около двух десятков протонных излучателей, хотя в своих работах они лишь немного отошли от полосы стабильности. При дальнейшем продвижении в область нейтронно-дефицитных ядер в реакциях с испарением нейтронов сечение падает, так как резко возрастает вероятность конкурирующих процессов.

Аналогичная ситуация возникает при синтезе изотопов трансурановых элементов. Реакции с испарением нейтронов в течение последних десяти лет интенсивно использовались для синтеза новых трансурановых элементов [10]. Были разработаны экспрессные физические и химические методы выделения и идентификации. Все это позволило в последние годы в Дубне синтезировать и изучить физические и химические свойства изотопов 102, 103 и 104 элементов [11].

Недавно в Дубне были синтезированы два изотопа 105 элемента [12]. Их предварительные характеристики следующие:

$$^{261}105 - E = 9{,}4 \pm 0{,}1 \text{ Мэв}, \quad 0{,}1 \text{ сек} \leq T_{1/2} \leq 3 \text{ сек};$$

$$^{260}105 - E = 9{,}7 \pm 0{,}1 \text{ Мэв}, \qquad T_{1/2} > 0{,}01 \text{ сек}.$$

В настоящее время проводятся дальнейшие эксперименты по уточнению свойств 105 элемента на основе усовершенствованной методики. Всего за последние годы в Дубне были синтезированы (большинство из них впервые) и изучены следующие изотопы:

$$^{245,\,246,\,247}_{99}\mathrm{Es},\quad ^{246,\,247,\,248,\,249}_{100}\mathrm{Fm},\quad ^{252}_{101}\mathrm{Mv},$$

$$^{251,\,252,\,253,\,254,\,255,\,256}102,\quad ^{256,\,257}103,\quad ^{260}_{104}\mathrm{Ku},\quad ^{260,\,261}105.$$

Однако с увеличением Z исследуемых изотопов (в связи с тем, что в реакциях с испарением нейтронов образуются нейтронно-дефицитные ядра) резко возрастает вероятность деления. Это означает, что вместо ожидаемого, например, изотопа 100 элемента, в результате деления образуются два ядра с $Z = 50$. Возрастающая нестабильность ядер, при увеличении Z, привела к тому, что при решении проблемы синтеза трансурановых элементов приходится иметь дело с чрезвычайно малыми сечениями. Опыты, проведенные в Дубне, показали, что, если сечение образования 102 элемента составляет 10^{-8} барн, то для 104 элемента эта величина составит $\sim 2 \cdot 10^{-10}$ барн, а для 105 элемента она будет еще меньше. Так как падение сечения экспоненциально, то увеличение времени экспозиции не спасет дело. Выход здесь виден в получении более тяжелых изотопов данного элемента. Однако в реакциях с испарением нейтронов, при использовании определенной комбинации мишень – тяжелый ион, удается получить лишь 1 – 2 нейтронно-дефицитных изотопа представляющего интерес элемента, т.е. необходимо иметь возможность изменять Z и A мишени и иона в широких пределах, что не всегда возможно.

Более перспективным методом синтеза тяжелых изотопов трансурановых элементов может оказаться деление под действием тяжелых ионов. В Дубне Оганесяном с сотрудниками было проведено систематическое изучение механизма деления [13]. Имея в распоряжении ускоренные ионы от ^{11}B до ^{40}Ar включительно можно было исследовать область ядер до $Z^2/A = 44$. Облучались мишени Ta ($Z = 73$), Au ($Z = 79$), Bi ($Z = 83$), U ($Z = 92$). Радиохимическим методом выделялись Te ($Z = 52$), Ba ($Z = 56$), редкоземельная группа, а также в ряде случаев – тяжелые осколки от Au ($Z = 79$) до At ($Z = 85$). В дальнейшем с помощью Ge(Li) – детектора измерялась γ – радиоактивность осколков, затем по полученному спектру производилась идентификация изотопов и определение их выхода. Распределение осколков по массе хорошо описывается функцией Гаусса:

$$P(A_f) = \frac{1}{\sqrt{\pi\sigma^2}} \exp\left[-\frac{(A_f - A_c/2)^2}{\sigma^2}\right]$$

где A_f и A_c — массы осколка и составного ядра, σ^2 — параметр, *определяющий* ширину массовой кривой. Были получены следующие результаты:

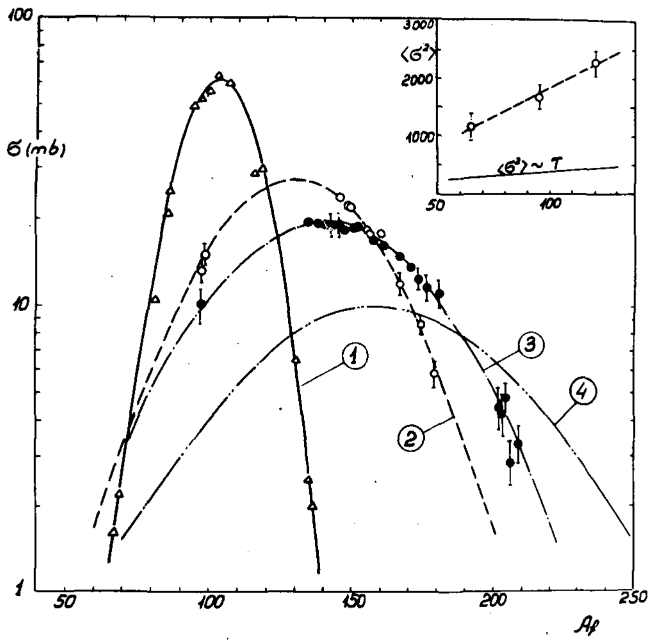

Рис.3. Массовые распределения осколков деления в реакциях:
1. $^{197}_{79}Au\ (^{12}_{6}C, f)$ 3. $^{238}_{92}U\ (^{40}_{18}Ar, f)$
2. $^{238}_{92}U\ (^{20}_{10}Ne, f)$ 4. Расчетное массовое распределение осколков деления в реакции. $^{238}_{92}U\ (^{84}_{36}Kr, f)$.

FIG.3. Mass fragment distributions for the reactions:

1. $^{197}_{79}Au\ (^{12}C, f)$ 3. $^{238}_{92}U\ (^{40}_{18}Ar, f)$

2. $^{238}_{92}U\ (^{20}_{10}Ne, f)$ 4. Estimated for: $^{238}_{92}U\ (^{84}_{36}Kr, f)$

1. Ширина массового распределения резко возрастает с ростом составного ядра, и в случае $^{238}U\ (^{40}Ar, f)$ наблюдается большой выход изотопов Au (Z = 79), Tl (Z = 81), Pb (Z = 82), Bi (Z = 83), Po (Z = 84) и At (Z = 85). Ширина изотопных распределений также резко возрастает с ростом Z^2/A (рис.3 и 4).

2. Осколки деления с большой вероятностью образуются в изомерных состояниях. Так, например, в реакции $^{238}_{92}U\ (^{20}_{10}Ne, f)$ были получены в одном облучении все известные изомеры Te с большим выходом (рис.5).

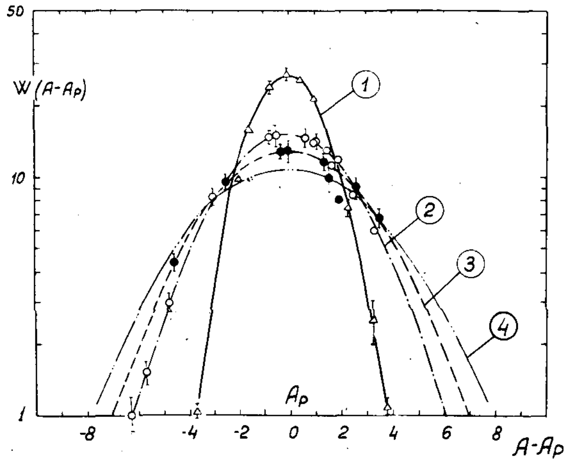

Рис.4. Изотопные распределения осколков деления в области ядер с $57 \leq Z \leq 64$. Нумерация кривых дана на рис.3.

FIG.4. Isotope fragment distributions in the vicinity of the nuclei with $57 \leq Z \leq 64$. For numbering of curves see Fig.3.

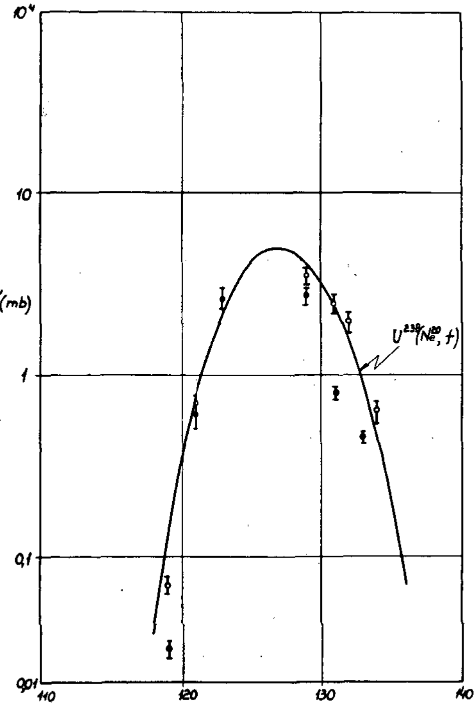

Рис.5. Выходы изотопов Te (Z = 52) в реакции $^{238}_{92}U\,(^{20}_{10}Ne, f)$: открытые кружки – основное состояние; черные точки – изомерное состояние.

FIG.5. Isotope yields of Te (Z = 52) from the reaction $^{238}_{92}U\,(^{20}_{10}Ne, f)$. Open circles – ground state; black dots – isomeric state.

3. Ранее уже отмечалось, что в реакциях с испарением нейтронов образуются короткоживущие изотопы трансурановых элементов. Так, в реакции $^{242}_{94}Pu$ (^{22}Ne, 4n) $^{260}104$ образуется изотоп $^{260}104$. При делении сверхтяжелых составных ядер масса наиболее вероятного изотопа значительно больше. Для примера можно указать, что наиболее вероятная масса 104 элемента в реакции $^{238}_{92}U$ ($^{84}_{36}Kr$, f) и $^{238}_{92}U$ ($^{132}_{54}Xe$, f) равна $^{274-xn}104$, что соответствовало бы образованию составного ядра при облучении $^{242}_{94}Pu$ несуществующим изотопом ^{32}Ne. При облучении урана ураном наиболее вероятная масса 104 элемента еще больше. Выход любого изотопа данного элемента в делении можно оценить по следующей формуле:

$$f(A'_f\, Z) = \sigma_c\, P(A_f)\, f(Z - Z_p(A_f))\, P_{xn}(E^*) \prod_{i=1}^{x} \frac{\Gamma_{ni}}{\Gamma_{\text{полн}}}$$

где $A'_f = A_f - xn$. $P(A_f)$ – вероятность образования массы в делении, а функция $f(Z - Z_p(A_f))$ определяет вероятность выхода различных изобар с массой A_f.

Из этой формулы видно, что, хотя сечение образования определенного изотопа данного элемента невелико (для максимума массового распределения не превышает $5 \cdot 10^{-27}$ см^2), однако отход от наиболее вероятной массы данного элемента на величину ~ 8 нейтронов уменьшает выход всего на один порядок. Поэтому даже при делении урана неоном этот метод получения изотопов более эффективен, чем деление урана тепловыми нейтронами. Кроме того, можно надеяться, что при переходе к более тяжелым ионам, как Kr ($Z = 36$), Xe ($Z = 54$), W ($Z = 74$), в осколках деления можно будет синтезировать изотопы трансурановых элементов вплоть до 105 элемента, со значительно большими сечениями, чем в реакциях с образованием составного ядра. Можно предполагать, что деление окажется перспективным методом для систематического изучения "изомерии формы", в различных областях периодической системы. Это могут быть как спонтанно-делящиеся изомеры, так и изомеры, распадающиеся по другим каналам, путем аномальных γ-переходов, необычных видов α-распада и т.д.

ПЕРСПЕКТИВЫ СИНТЕЗА СВЕРХТЯЖЕЛЫХ ЭЛЕМЕНТОВ

До недавнего времени считалось, что граница периодической системы определяется конкуренцией поверхностных сил притяжения и кулоновских сил отталкивания [14]. Однако в дальнейшем было показано, что при рассмотрении стабильности тяжелых ядер необходимо учитывать неоднородности в энергетическом распределении нуклонов – так называемые "оболочечные эффекты" [3]. Первоначально предполагалось, что по аналогии с известным магическим числом для нейтронов $N = 126$, следующее магическое число для протонов будет также $Z = 126$. Кроме того, из расчетов получалось, что для нейтронов магическим числом будет $N = 184$, т.е. ядро $^{310}126$ должно быть дважды магическим [15]. Однако расчеты схемы одночастичных уровней протонов в потенциале с размытым краем показали, что магическим числом для протонов будет $Z = 114$, и таким образом дважды магическим должно явиться ядро $^{298}114$ [16]. В это же время в Лаборатории ядерных проблем Музычкой и Струтинским был проведен более тщательный анализ стабильности ядер для $104 \leq Z \leq 130$ и $170 \leq N \leq 190$. По методу Струтинского, кроме оценки щели в одночастичном спектре, рассчитывалась зависимость энергии ядра от его деформации. Было показано, что существует целая область сферических ядер вокруг $^{298}114$, устойчивость которых по отношению к спонтанному деле-

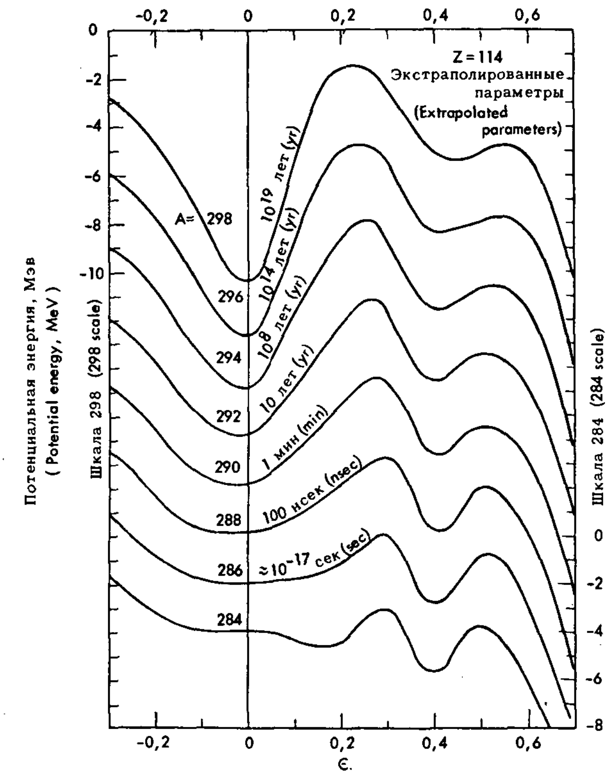

Рис.6. Ожидаемые времена жизни по спонтанному делению для изотопов 114 элемента.
FIG.6. Expected spontaneous fission decay lives for the isotopes of element 114.

нию по мере удаления от центра области монотонно уменьшается [17]. В дальнейшем подобные результаты были приведены в работе Святецкого [18]. Кроме того, в ряде работ оцениваются времена жизни различных изотопов этой области по отношению к спонтанному делению, α-распаду, β-распаду и электронному захвату. Эти результаты, конечно, далеки по точности от того, что хотели бы иметь экспериментаторы, так как небольшая ошибка в расчетах может дать сильное отклонение от ожидаемой величины времени жизни (для примера можно указать, что изменение $\Delta Q_\alpha \simeq 0{,}5$ Мэв приводит к изменению в $\Delta\tau_\alpha = 10^3$), но сам факт существования области сверхтяжелых ядер в настоящее время не вызывает сомнения. Так, из рис.6 [18] видно, что для наиболее стабильных изотопов 114 элемента время жизни относительно спонтанного деления $\tau_{сп} \geq 10^{16}$ лет (оценка полного времени жизни, которая включает α-распад, β-распад и электронный захват), дает значительно меньшую величину $\tau \approx 10^2 - 10^7$ лет).

Подобные расчеты существует и для элементов Z = 122 – 126. Однако их времена жизни по α - и β-распадам не должны превышать 10^{-3} сек. Возникает вопрос: как добраться до острова стабильности? Попасть в эту область можно, совершив прыжок через область нестабильных элементов (100 – 110), а это может быть сделано в лабораторных условиях только в реакциях с тяжелыми ионами.

Начиная с 1967 года, группа американских физиков (Святецкий, Томпсон и др.) попытались синтезировать 114 элемент. Самая тяжелая частица, ускоряемая в настоящее время на линейном ускорителе тяжелых ионов - ^{40}Ar, поэтому они использовали реакцию:

$$^{40}_{18}Ar + ^{248}_{96}Cm = ^{288}114_{174} \rightarrow ^{284}114_{170} + 4n.$$

Изотоп $^{284}114$ находится далеко от дважды магического ядра $^{298}114$, поэтому надежда получить 114 элемент в этой реакции была невелика. Действительно эффект не был обнаружен, и авторы дают верхнюю границу для сечения образования $\sim 10^{-30} - 10^{-31}$ см^2 [18]. Значительно более перспективной выглядит реакция:

$$^{48}_{20}Ca + ^{244}_{94}Pu = ^{292}114 \rightarrow ^{288}114 + 4n$$

или

$$^{48}_{20}Ca + ^{248}_{96}Cm = ^{296}116 \rightarrow ^{292}116 + 4n.$$

Это видно из рис. 7. В настоящее время в Лаборатории ядерных проблем ^{48}Ca ускорен и проводятся фоновые опыты.

Однако ситуация с синтезом 114 элемента может значительно улучшиться, как только появится возможность ускорять ионы, тяжелее криптона. Так, при делении урана $^{132}_{54}Xe$ наиболее вероятным будет изотоп $^{305}114$, а энергия возбуждения осколка будет соответствовать испарению 4 – 6 нейтронов. Чтобы получить этот изотоп в реакции, идущей через составное ядро, необходимо было бы облучать $^{244}_{94}Pu$ не существующим в природе сверхтяжелым изотопом $^{61}_{20}Ca$.

При переходе к более тяжелым элементам (Z = 120 – 126) легче подобрать необходимую комбинацию мишень-ион для синтеза изотопов с N = 184. Для примера можно указать:

$$^{48}_{20}Ca + ^{252}_{98}Cf = ^{300}118 \rightarrow ^{296}118 + 4n$$

$$^{64}_{28}Ni + ^{244}_{94}Pu = ^{308}122 \rightarrow ^{304}122 + 4n$$

$$^{68}_{30}Zn + ^{244}_{94}Pu = ^{312}124 \rightarrow ^{308}124 + 4n$$

$$^{84}_{36}Kr + ^{232}_{90}Th = ^{316}126 \rightarrow ^{312}126 + 4n$$

и так далее. В случае этих реакций составные ядра могут получаться с очень небольшой энергией возбуждения, поэтому, если не появятся дополнительные факторы, повышающие энергию возбуждения, сечения образования этих изотопов могут быть велики $\sim 10^{-26} - 10^{-28}$ см^2.

Здесь я также хочу отметить, что при появлении более тяжелых ионов осколки деления заполнят большую часть периодической системы, поэтому появится возможность синтеза большого количества новых нейтронно-избыточных ядер.

Теперь можно остановиться на очень интересных результатах, полученных группой профессора Фаулера, которые недавно были сообщены профессором Пауэллом [19]. Эта группа использует фотографические эмульсии для изучения элементного состава космических лучей. Недавно ими были обнаружены следы частиц, ионизирующая способность которых указывает на то, что их заряд Z ~ 106. Эти результаты, если они будут подтверждены, явятся безусловным доказательством существования долгоживущих сверхтяжелых элементов.

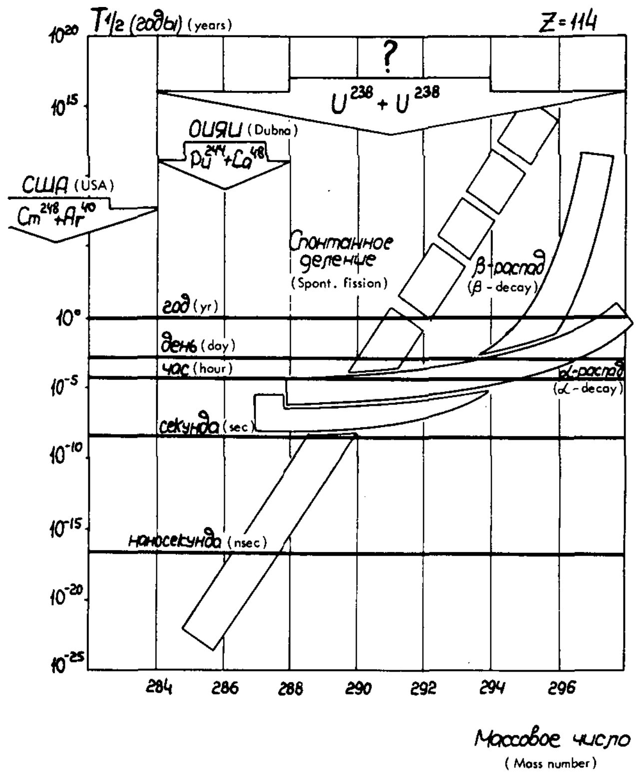

Рис.7. Возможные методы синтеза изотопов 114 элемента.

FIG.7. Possible fusion methods for the isotopes of element 114.

Однако, как сейчас кажется, оценка величины Z в настоящее время не может быть сделана достаточно надежно. Это связано с тем, что хорошо изучены в космических лучах пока только следы релятивистских ядер железа (Z = 26), а заряд более тяжелых элементов поэтому определяется экстраполяцией от этих значений. Следует уточнить поэтому калибровочные характеристики для формы следов тяжелых ядер в фотоэмульсиях. В частности, можно использовать тот факт, что в интервале Z от 83 до 90 все изотопы являются радиоактивными с малым временем жизни. Поэтому, если надежно изучить ионизирующую способность частиц с Z = 83 и 90, то в дальнейшем можно будет использовать эти данные для определения заряда более тяжелых частиц. Опираясь на экспериментальные данные, полученные Лабораторией ядерных проблем при синтезе элементов в области Z от 100 до 105, можно показать, что время жизни самых долгоживущих изотопов этих элементов не превышает нескольких дней. С другой стороны, многочисленные теоретические оценки указывают, что наиболее стабильными по всем видам ядерного распада должны быть изотопы элементов с Z, близким к $Z = 114$. Поэтому можно считать, что,если будет подтвержден факт наблюдения в космических лучах тяжелых ядер с $Z > 100$, то это могут быть только изотопы элементов с $110 \leq Z \leq 116$.

Учитывая скорости частиц в космических лучах, сечение деления при столкновении с атомами водорода и ряд других факторов, можно ориентировачно оценить возможные времена жизни частиц с $Z > 100$ в космических лучах. Эти оценки дают времена $\sim 10^6 - 10^8$ лет.

Теперь я хотел бы коснуться геохимического аспекта данной проблемы. Химические исследования курчатовия, проведенные в Лаборатории ядерных проблем показали, что, начиная с $Z = 104$, застраивается внешняя электронная оболочка, т.е. курчатовий оказался уже не актинидом, а химическим аналогом гафния. Поэтому можно предполагать, что изотопы 110, 112, 114 элементов должны являться аналогами платины, ртути и свинца. Тогда наиболее долгоживущие среди этих тяжелых ядер ($T_{1/2} \geq 10^8$ лет) можно попытаться обнаружить в природных минералах. Но так как поиск каждого из них должен осуществляться в различных природных соединениях, то наиболее удобными среди них, в смысле химической обособленности, являются соединения свинца. Если опираться на данные, полученные Фаулером и др. [19], то возможная примесь изотопов "экасвинца" в первозданном свинце, по оценкам, составляет $\sim 10^{-14}$. Так как "экасвинец", по предположениям, должен испытывать спонтанное деление, то не лишены смысла поиски спонтанного деления в свинцовых минералах.

Учитывая ожидаемую примесь "экасвинца" $\sim 10^{-14}$ при $\tau_{1/2} \approx 10^6 - 10^8$ лет, наблюдаемый период распада свинца должен соответствовать величине $\sim 10^{22} - 10^{24}$ лет. Поэтому для регистрации актов спонтанного деления в природном свинце необходимо разработать метод, который позволил бы использовать большие количества свинца и был бы нечувствительным к фону осколков деления, обусловленных космическими лучами или другими причинами. В дальнейшем, если будет получен положительный эффект, после разработки соответствующих химических методов выделения "экасвинца" из свинца или при использовании масс-сепаратора можно будет провести идентификацию этих изотопов и изучение их свойств. Аналогичная методика может быть применена и при поисках изотопов "экаплатины", "экартути" и т.д.

С другой стороны, необходимо отметить, что, если в космических лучах или при изучении различных минералов будут обнаружены долгоживущие ($T_{1/2} \geq 10^6$ лет) изотопы сверхтяжелых элементов, то должна существовать целая плеяда изотопов и элементов, обладающих меньшими временами жизни, которые могут быть синтезированы и изучены на ускорителях тяжелых ионов.

В заключение доклада я хочу отметить, что в настоящее время определилось несколько интересных направлений, связанных с вопросами поиска и синтеза изотопов сверхтяжелых элементов. Причем каждое из этих направлений содержит в себе много интересных и неизученных вопросов, и прогресс в каждом из них может оказать огромное влияние на дальнейшее развитие ядерной физики, позволит по новому подойти к проблеме происхождения элементов, развить исследования химических и физических свойств далеких элементов.

Поэтому позвольте мне выразить уверенность, что в ближайшее время вопрос о возможности существования сверхтяжелых ядер будет проверен экспериментально.

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SOME DEVELOPMENTS IN OUR APPROACHES TO NUCLEAR STRUCTURE PHYSICS

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Abstract

SOME DEVELOPMENTS IN OUR APPROACHES TO NUCLEAR STRUCTURE STUDIES. In the first part the author discusses the evolution of traditional approaches, including accelerators and on-line techniques. In the second part he evaluates new approaches involving elementary particles and high-energy methods.

I shall divide my paper into two parts. Part I concerns the evolution of traditional approaches supplemented by new techniques; Part II treats the evolution – still really only evaluation – of new approaches involving elementary particles and high-energy methods.

PART I

ACCELERATORS

A continuous evolution is, of course, taking place in all our conventional nuclear structure accelerators.

The new isochronous cyclotrons offer improved energy resolution, energy variability, duty cycle and range of ions. These machines are now bidding to challenge the electrostatic generator but that challenge cannot yet be said fully to have materialized and the electrostatic generator currently remains the scarcely-disputed king of the nuclear barnyard. Perhaps the most interesting recent news about electrostatic generators is the decision of our Romanian colleagues to acquire a large American tandem Van de Graaff. These tremendously powerful machines will add a new style to nuclear structure research in what I might call the JINR countries.

There is a theorem about electrostatic generators and that is that by the time you have acquired your machine you have also acquired a case for having 1.5 times as many volts on the terminal. Sometimes the case advances even a little more quickly than that and although machines with 20 MV on the terminal have not yet left the drawing board a good case can already be made for a tandem with a 30 MW terminal. This case is based on precisely the considerations given by Flerov and others relating to the superheavy elements, namely, our need to enter in a very general way the region of possible islands of stability in the neighbourhood of Z = 114 and 126. For this task very heavy ions of very high energy are needed and it seems as though terminal energies well in excess of 20 MW will be demanded. The alternative electrostatic generator approach of using two coupled machines will be pursued at Brookhaven (2 MP tandems – 12 MV terminals) and, hopefully, in the HVEC laboratories at Burlington through the even more powerful MP-TU (16 MV terminal) combination. Such machines may well open up a new and important area of nuclear structure studies. As Weisskopf has emphasized, we should rather hope that what we find there is not what we expect rather than that the superheavy elements simply conform to our present ideas about the behaviour of nuclear matter.

Our pursuit of very high energies through very heavy ions involves us also in the search for high initial ionization states of those ions. This search takes us into one of the many interfaces between nuclear physics and solid state or atomic physics. Rasmussen has emphasized this and has suggested that it may be possible to find such high charge states by starting from isomeric or other processes that create abundant K-vacancies and that so lead to high charge states at the termination of the subsequent cascade.

Current developments in electron linear accelerators are also of importance. Such machines having high currents and high duty cycles will be very important in a large range of ways including the measurements of transition moments as a function of momentum transfer. The related matters of separating out the components of mixed multipole transitions and, in general, the search for high-multipole excitation that can be done differentially by varying the momentum transfer are also important. Electron linacs should also be surprisingly good sources of slow pions and muons for the sort of studies that I shall be dealing with in Part II of my talk.

ON-LINE TECHNIQUES

A major revolution is taking place in the way in which we use accelerators through the use of complex apparatuses directly coupled to them. Such on-line coupling of mass spectrometers as emphasized by Teillac and the β-ray spectrometers as emphasized by Sakai provide tremendously powerful facilities that we have scarcely, as yet, begun to exploit.

Of course, the most dramatic advent in recent years is the on-line use of computers and this is a sufficiently vast topic to merit a conference devoted entirely to it. Suffice it to remark that tremendous problems of data handling are involved. We are already carrying out experiments in which four detectors each with a resolution of the order of 1 part in 10^3 operate in coincidence, demanding storage of information in more than 10^{12} quadrupole co-ordinate elements and its subsequent inspection and digestion. So tremendous are the problems of data handling that the prospect is opened up of carrying out accelerator experiments remote from the accelerators both in space and time. The information from complex coincidence counter arrays will be stored on magnetic tape in multi-dimensional form and this magnetic tape will, in its turn, become the object of subsequent research, as often as not yielding information of a kind not contemplated when the raw data collection took place. Experimental libraries of magnetic tape will be built up and research will take place in those libraries with the aid of computers in very much the same way as historical research takes place among collections of documents rather than by participation in the events that give rise to them. This tendency is already apparent in high energy physics where, for example, a bubble chamber run, while being aimed at specific problems, generates data that, perhaps years later, are used for the solution of problems undreamt of at the time when the run was carried out. Magnetic tape libraries of experiments available on a worldwide basis would enable someone in a remote country, provided he had access to an appropriate computer, to carry out a nuclear structure experiment on a powerful accelerator thousands of miles away as readily and as effectively as someone on the spot.

PART II

WHY HIGH ENERGIES?

The justification for our use of high energy or elementary particle methods for investigating nuclear structure must be that by those methods we can acquire information of a kind that cannot be obtained by conventional methods or that complements information obtained by conventional methods. We must not ask what nuclei can do for high energy accelerators but rather what high energy accelerators can do for nuclei.

There are two chief reasons for using high energy methods:

(a) Granularity

The nucleus is composed of neutrons and protons. This statement would be accepted by most people and yet it conceals a great deal of ignorance and is indeed untrue. It is untrue because the nucleons are held together in fairly intimate connection by pion and other particle exchange and so are certainly not identical with the particles that we know in the free state – but how different are they? To what degree is it proper to picture the nucleus as a collection of neutrons and protons? To what degree are essential changes introduced by the background field in which the nucleons are immersed? These are questions of intense importance that we shall certainly not answer without going inside the nucleus and looking at nucleons as individuals – if such they are – and this obviously involves us in using as probes particles capable of imparting momentum transfers corresponding to distance scales of less than the nucleonic separation. This means particles of hundreds or thousands of MeV or, alternatively, the absorption, with the release of all or some of the rest mass energy, of particles such as pions, kaons, anti-protons etc.

The ignorance concealed within the simple statement that nuclei are made of neutrons and protons is that, even granted that the statement may be true, it tells us nothing of the important matter of the spatial arrangement between the nucleons, particularly of the question of their short-range correlations which, because of the short range of the nuclear force, are all-important for the energy content of the nucleus and for the higher momentum components. It is again evident that only high energy methods can directly examine small-scale spatial structure.

Our models of nuclear structure have led us to certain pictures of the nucleus of a highly detailed character but this knowledge, if such it can yet be called, is highly inferential and demands the 'photographic' information that can be obtained only through the equation $kR \gg 1$ for its confirmation.

(b) Spatial relationships.

When we probe a nucleus by any method we rarely probe it uniformly because our particles come into it from outside and if there is any absorption at all the interaction will not be proportional to the density of nuclear matter or to any simple function of that density. Even if the ingoing particle is weakly absorbed – a neutrino or a photon for example – the outgoing particle that brings news of the interaction is usually strongly interacting and so biasses our information towards certain regions. If the outgoing particle is weakly interacting then we indeed learn something about the nucleus as a whole in an unbiassed way – unless the interaction itself biasses us towards neutrons or protons as in the case of electron scattering, for example, where we learn about the overall charge distribution but essentially nothing about the neutrons. But we should dearly like to gain information about the nucleus biassed in a clearly-defined way towards its various regions – we should like to be able to examine its centre without reference to its edge, we should like to be able to examine its edge without reference to its centre. We may to some degree achieve these objectives if we have adequate faith in the optical model and DWBA but the optical model is at best a very clouded crystal ball and it must always be remembered that it deals only with averages and takes no account of the fluctuations that, in fact, exist within the nucleus and that may dominate certain rare phenomena. One should, therefore, be particularly sceptical in relying on the optical model where, for example, nuclear matter is very tenuous or high momentum components are in question.

The contribution that high energies make here is, firstly, the trivial one concerned with short wavelengths but, more specifically, the fact that the probing particles can be negatively charged: μ^-, π^-, K^-, Σ^-, p^- etc. Such negatively-charged particles when 'stopped' in matter fall into atomic orbits. Under certain circumstances we may know what these orbits are and so what is, in some detail, the spatial relationship between the probe and the nucleus. Thus, for example, we know that the μ^--meson will gain its atomic 1s-state before decay or absorption by the nucleus and we can recognize the X-rays it emits on its way there, passing through other orbits of well-known spectroscopic character. If the particle is strongly interacting then the orbital(s) from which it is absorbed can be similarly ascertained from X-ray studies and so the products of the absorption come initially from a region of the nucleus about which something is known. Of course, structures of the kind just made about the optical model will again apply to some degree but the identification of the orbital from which interaction takes place can be an immeasurable benefit.

In addition to these two chief advantages of high energy methods there are many subsidiary reasons for our interest. The following is a very abbreviated list:

(i) (Trivial) We must obviously use energies considerably greater than the depth of the effective nuclear potential if we are to probe the full energy/momentum structure of the nuclear Fermi sea. (Recall that we have no evidence for the reality of deep shell structure in nuclei beyond about Ca: is there any literal sense in which there exist 1s-nucleons in Pb?)

(ii) Probes of J = 0 will be useful in limiting the number of scattering amplitudes in, for example, a multiple scattering analysis directed at the short-range correlation problem.
(iii) Probes of T = 1 may be able to show up new forms of collective motion of $\Delta T = 2$ and have benefits associated with their ability to change the nuclear charge by 2 units, with or without change of the nuclear wave function.
(iv) Special couplings available through elementary particles may show up certain types of nuclear state more readily than conventional probes, e.g. in muon absorption.
(v) By inelastic scattering we can study transition moments as a function of momentum transfer and so add another dimension to the testing of nuclear models.
(vi) Our ignorance of the possible importance of 3-body forces in nuclear structure is great. It may be very informative to search for these effects first in hypernuclei since the λ-hyperon cannot enjoy the relatively long-range single-pion exchange with nucleons so that double-pion exchange which could give rise to a 3-body λNN force may be an important ingredient of the interaction between the λ-hyperon and nucleons.

MUONIC X-RAYS AND ELECTRON SCATTERING

The use of high energy electrons to probe the nuclear charge distribution has been practised for so long that it may almost be regarded as a part of conventional nuclear structure physics rather than as part of the newer high energy methods. However, it is one of the clearest examples of the need to use short wavelengths to reveal details of spatial structure and so properly belongs in the present discussion. Recent developments by Hofstadter el al., where energies as high as 750 MeV have been used in elastic scattering studies, have shown the need for the introduction of at least a third shape parameter into our description of the charge distribution. They have also raised the important question as to whether the better approach is the continual introduction of more parameters into a basically 'Fermi-type' charge distribution or whether we should not perhaps rather start from a charge distribution that we believe may have some more 'fundamental' justification such as one synthesized from single-particle wavefunctions generated within a Saxon-Woods or other potential that correctly reflects the binding energies of the various proton shells. The latter approach has been advocated particularly by Elton; it has had the rather impressive success of adequately accounting for the 750-MeV electron scattering from Ca isotopes using a charge distribution generated to account for the 250-MeV scattering. The 3-parameter Fermi-type distribution that was adequate at 250 MeV failed significantly at 750 MeV. This suggests that simple single-particle wavefunctions of the Saxon-Woods type may indeed be a good starting point for our discussion of the nuclear charge distribution. Of course, it would be exceedingly interesting if such wavefunctions failed to describe the electron scattering since that would reflect directly on our detailed account of nuclear structure. It would reflect on such matters as configuration mixing, correlations, and upon the question raised earlier, namely, the degree to which a shell pattern of the single-particle type deeply underlies the structure of heavy nuclei at all.

There are, as yet, no serious signs, from electron scattering done, that single-particle distributions are inadequate but it must now be remarked that muonic X-rays, also essentially a probe just of the charge distribution and vigorously studied by Backenstoss, Anderson, Telegdi, Wu, Devons et al., must simultaneously be accounted for by the charge distribution put forward to fit the electron scattering data. Indeed there are, according to Elton, signs that in Pb the Saxon-Woods type wavefunctions, although they give a satisfactory account of electron scattering, do not adequately fit the muonic X-ray data, having rather too much charge at the centre. A more satisfactory simultaneous account of electron scattering and muonic X-rays is given by the wavefunctions deriving from the more-nearly-realistic approach of the Bethe-Brueckner theory which give a hollow at the centre of the corresponding charge distribution rather than a hump there. This success of the Bethe-Brueckner theory is very promising, particularly since the approach is not an extremely elaborate one, being essentially a type of Thomas-Fermi model. It is interesting and significant that the inclusion of the Coulomb force is necessary to reconcile the electron scattering and muonic X-ray data within the Bethe-Brueckner treatment.

The promise of finer detail about the radial charge distribution coming from the combination of higher energy electron scattering and very precise muonic X-ray measurements is of considerable importance. We may note that 'realistic force' calculations are now yielding good results for absolute binding energies. However, these results are relatively insensitive to the radial wavefunctions, three to four terms of a harmonic oscillator expansion being apparently adequate. On the other hand, the approach to the nucleon wavefunctions themselves, using Hartree-Fock methods, demands eight or more oscillator terms for an adequate representation. This clearly shows that we need quite fine experimental detail on the radial charge distribution to check the predictions of detailed models insofar as they concern the wavefunctions themselves rather than just the energies.

It is, of course, possible to use the energies of the muonic X-rays by themselves for determining the parameters of the nuclear charge distribution. From such measurements we get significant, say 10%, information about a third parameter by carrying them out with an accuracy of the order of 0.1 keV such as is now becoming possible. We must remember, however, that there are factors at work that affect the X-ray energies much more than 0.1 keV and that some of them are not yet under complete control theoretically. One of these factors is the polarization effect – the virtual involvement of excited nuclear states through the muon-nucleus coupling – which shifts the overall energy levels in ordinary second-order perturbation theory. The magnitude of this effect in a heavy nucleus is estimated to be several keV but the uncertainty in the estimate still considerably exceeds the accuracy with which energy measurements can and must be made. Another effect of uncertain magnitude concerns the electronic shielding of the nuclear charge from the orbiting muon. This could certainly be calculated with adequate accuracy if we knew how many inner electrons were present, but since the muon arrives at its low orbits following Auger cascades it is not completely clear what the inner electronic configuration might be at the time that the muonic transitions are actually made.

It seems most likely that best progress will, in fact, not be made by using muonic and electron scattering information separately (the latter is itself, of course, also subject to uncertainties associated with virtual nuclear excitations) but that we should rather put the two types of information together in the manner already indicated. Since the two sets of data are sensitive to different aspects of the nuclear charge distribution, to seek distributions consistent with both will significantly limit the allowable range of those distributions. By doing this, we are, of course, begging the question of the possible charge form factors of the electron and muon themselves, but these are problems better approached by the methods of quantum electrodynamics proper and such work will be carried out in parallel with the studies that we are now discussing. The simultaneous use of electron scattering and muonic X-ray information does not relax the strictures on our need fully to understand the involvement of nuclear and atomic processes such as polarization and electron shielding, and it is not clear at this stage how these difficult problems are satisfactorily to be resolved.

In addition to the high importance of the overall charge distribution for testing general nuclear models, there is of course the very interesting and more specific question of isotope effects. Studies in electron scattering have already been made in Ca isotopes and muons are already being used extensively for similar purposes. Although one may not be able to give a 'dead reckoning' account of the charge distribution itself from the basis of nuclear theory, changes in the distribution from one isotope to the next may be theoretically analysable and give information of importance for nuclear models.

In addition to the important isotope shifts, there are also, in muonic X-ray spectra, isomer shifts to be measured. As is well known, as a result of the coupling between the electromagnetic field of the muon and the nuclear charge distribution, it is possible, in the course of the muonic cascade, to excite the nucleus into various excited states, particularly the rotational states which have strong electric quadrupole moments to the ground state and whose excitations in the heavier nuclei are comparable with the magnetic fine structure of the muons in their 2p and higher orbits. Accordingly, when the muon eventually arrives in its 1s-state the nucleus will quite frequently be in its first excited state from which it subsequently decays, the muon remaining in its atomic 1s-state. The nuclear transition therefore takes place in the environment of a muon of well-known wave-function. The energy of this transition will therefore depend, among other things, on the difference of the radial charge distribution of the nucleus in its excited and ground states. This effect is probably the most important one operating to make the gamma-transition energy different in the muonic environment from what it is without a muon present, and enables us to make a statement about the dependence of the nuclear charge size on the state of excitation. (Similar information is occasionally available from Mössbauer studies but the muonic approach is probably somewhat more general and is of at least comparable accuracy although, as has recently been emphasized by Grodgins et al., it is subject to some uncertainty owing to effects of the magnetic hyperfine structure in the excited nuclear state).

The electric quadrupole hyperfine structure of muonic X-ray spectra is now a widely studied phenomenon. In contrast to the situation in ordinary electronic spectra, the muonic magnetic hyperfine structure is, owing to the much smaller magnetic moment of the muon, relatively unimportant compared with the electric quadrupole hyperfine structure and so the latter can be studied relatively uncomplicated by the former, although the former must always be borne in mind.

A most important point is that the deep penetration of the muonic 1s- and 2p-orbitals in the heavier nuclei makes the quadrupole hyperfine structure sensitive to the form factor of the quadrupole moment. This in turn gives us hope that we might be able to distinguish between different angular dependences of the radial charge distributions of deformed nuclei. In such nuclei, as already mentioned, the probability of exciting a nuclear rotational state is quite high and so the X-ray pattern is rather a complicated one and its quantitative interpretation involves the off-diagonal as well as the diagonal nuclear electric quadrupole matrix elements. Before unambiguous analysis can be made in terms of the angular dependence of the radial distribution of charge, we must know the relationship between the various electric quadrupole matrix elements. In principle, all this might be derived from the muonic data alone by a sufficiently detailed study of the double cascade $3d \rightarrow 2p \rightarrow 1s$, although better progress is likely to be made by folding the muonic information in with that deriving from other methods that yields Q_0, B(E2) etc. Very detailed studies of quadrupole hyperfine structure are likely to be of great importance for this question of the angular dependence of the charge distribution.

Quadrupole hyperfine structure is a good tool for the determination of quite small electric quadrupole moments and already competes favourably with other methods. It is also worth remembering that the hyperfine structure is dependent on, and so may determine, the sign as well as the magnitude of the quadrupole moment.

It is finally to be remarked that an important use of muonic X-ray data may come in normalizing information that can be obtained more readily by ordinary optical methods but whose interpretation is there complicated by factors that do not enter in the muonic case. An example is the form factor of the nuclear magnetic moment. This is obtained from the magnetic hyperfine structure which is very small but measurable for muons. Optical spectra contain this information but the muonic data are free of the very formidable uncertainties that attend the interpretation of the ordinary optical data on account of our ignorance of the electronic wavefunctions and other associated matters. A few judiciously chosen measurements with muons would be very valuable in normalizing the optical data. A similar remark applies to the isotope shift which again can be measured with great precision optically but whose interpretation there is also complicated by a number of factors to do with the electronic wavefunctions. Spot normalization through muons would be most valuable.

NEUTRON AND MATTER DISTRIBUTIONS

Electron scattering and muonic X-rays are giving us an increasingly detailed picture of the charge distribution in nuclei. Such measurements are, however, essentially insensitive to the location of the neutrons which is just as important a problem. The location of the neutrons relative to the protons is obviously a matter of great importance and something that must be satisfactorily described by any acceptable nuclear model. It is therefore most desirable that we should devise methods that are either specifically sensitive to the location of the neutrons or that, alternatively, respond to neutrons and protons indifferently so as to give a mapping of the overall nucleon density.

Firstly, some remarks on low-energy approaches.

If, as Rost has done, one constructs a Saxon-Woods potential for heavy elements that correctly reproduces the known neutron and proton binding energies and that also accounts for electron scattering, then that potential generates a neutron distribution that, at the edge of the nucleus, sticks out several tenths of a fermi beyond the proton distribution. The nucleon distributions generated by Bethe-Brueckner theory show the same effect.

As Greenlees has recently shown, if one constructs the optical model potential for low-energy nucleon-nucleus scattering simply by a convolution of the matter distribution with the nucleon-nucleon potential, then in order to fit the experimental elastic scattering data one requires matter distributions that are considerably bigger than the proton distributions and that, by comparison with the proton distributions, imply a neutron skin of the order of half a fermi in thickness.

Now that analogue states are being identified in the heavy nuclei we may hope to determine the size of the last neutron orbital by the Coulomb energy displacement that results when it is transformed into a proton yielding the appropriate analogue state. Information on the neutron distribution gained in this way implies, according to Schiffer, that the neutron distribution has a larger radius than the proton distribution although the effect does not seem to be as large a one as that deriving from the analysis of elastic nucleon scattering and, indeed, corrections of as yet to be determined magnitude may even reverse the sign of the effect.

On the other hand, information from stripping and pick-up reactions in the heavy nuclei appears to show that the neutron and the proton tails are quite comparable and that there is no obvious neutron skin.

A decision on this most important point from conventional nuclear structure methods is therefore not clear and we should like some more direct approach.

A high energy approach that has already been used with positive results concerns the absorption in matter of stopped K^--mesons. As is now well known, this process is likely to be one of peripheral absorption; this conclusion is almost certain if the K^--mesons are absorbed from 'circular' orbits as they probably are to a first approximation. It is computed that the bulk of the absorption takes place beyond the region where the nuclear matter density has fallen to one half of its central value and the products of the absorption therefore bring us news of the composition of the nuclear surface. The absorption products are, of course, different for absorption on neutrons and protons; in particular the former process yields negative Σ-hyperons plus neutral pions, while the latter may yield either positive or negative Σ-hyperons together with pions of the appropriate opposite charge. As shown by Burhop et al., it is possible, by studying this process in nuclear emulsion and by comparing events associated with Auger electrons (and that therefore take place chiefly on heavy elements – Ag and Br) and those associated with a visible recoil prong (and that therefore take place chiefly on light elements – C, N, and O) to draw conclusions about the neutron-proton ratio in the surface of the heavy elements in terms of that in the surface of the light elements. Since any reasonable model will give very similar distributions for neutrons and protons in light elements, one may use this hypothesis as a calibration to enable one to deduce the neutron-to-proton ratio in the surface of the heavy elements. The answer is that in the heavy elements the effective neutron-to-proton ratio in the region where absorption of the K^--mesons take place is 5 ± 1. If we seek to interpret this number in terms of nucleon distributions of conventional radial form, we find that the neutron distribution may have a radius of approximately 0.7 fm greater than the proton distribution. (The results could also of course be explained if the neutron distribution, while having the same half density point as the proton distribution, had a greater diffuseness.)

Another high energy approach to the problem of the nucleons in the nuclear surface comes from the quasi-elastic scattering of particles of very high energy. Experiments carried out by Cocconi et al., using protons of 19 GeV show a dependence of the near-elastic scattering cross-section (energy loss less than about 100 MeV) on momentum transfer that breaks into two clearly defined components. The first component falls extremely rapidly with increasing momentum transfer while the second falls still rapidly but much less so than the first. The first component is due to the coherent elastic scattering by all the nucleons in the nucleus acting together while the second component, which shows just the same momentum-transfer dependence as free nucleon-nucleon scattering, is due to the quasi-elastic scattering of the incident protons by the individual neutrons and protons of the nuclear matter distribution acting incoherently. The magnitude of this second, quasi-free, scattering component obviously depends on the structure of the nuclear surface, since not only must a quasi-elastic scattering event take place but the scattered nucleon must escape from the nucleus without absorption. The degree of sensitivity to the surface thickness is, according to Glauber, that the cross-section increases by a factor of approximately two on going from a uniform sphere to one having a standard Fermi-type surface. These experiments therefore essentially count the number of nucleons effective for such quasi-free scattering in the nuclear surface, the overall extension of which is thereby determined. To the degree to which the experiments have received analysis so far there is no sign of a neutron skin. The extension and refinement of these experiments, and of their analysis, is an important matter that deserves considerable attention.

The different interactions of positive and negative pions with neutrons and protons can also be made the basis of experiments for probing the neutron versus the proton distribution. The experiments carried out more than ten years ago by Abashian et al. and analysed by Elton suggested that, in heavy nuclei, the neutron skin, if any, did not have a thickness of greater than about 0.2 fm. These experiments and their analysis now demand refinement.

It is also likely that pion production can yield important information on the neutron versus proton question. If the pion production is by protons with an energy of about 600 MeV, then the positive pions are produced chiefly by interaction of the incident protons with protons in the nucleus (this is the prediction of the isobar model which, according to Margolis, appears to work quite well in practice at these energies), while negative pions are produced only by interaction of the ingoing proton with neutrons in the nucleus. A comparison of production of the pions of the two charges therefore reflects directly on the neutron versus proton distribution in the nucleus, although close attention must be given to problems of final state interactions, charge exchange and so on.

Another high energy method that has already been used is the coherent photoproduction of various particles. (γ, π^0) production, for example, may take place almost indifferently on neutrons and on protons and if, therefore, one studies coherent production the angular distribution of the product photopions gives one direct information about the spatial distribution of nucleons irrespective of whether they are neutrons or protons. This work has not yet been carried out significantly in heavy nuclei; it can be extended to other product particles.

A further high energy method that is potentially quite promising is the study of the charge exchange of charged into neutral kaons for small momentum transfer. The small momentum transfer will favour the periphery of the nucleus as the site of the interaction while the choice of sign of the incident kaon will determine whether the exchange takes place on neutrons or on protons.

The absorption of stopped π^--mesons is more complicated than that of K^--mesons since it proceeds predominantly on nucleon pairs while that of K^--mesons is due chiefly to single nucleons. On the other hand, the π absorption process is experimentally much easier to study and is becoming the subject of increasingly-refined theoretical analysis; we may hope that significant information on this matter distribution will be forthcoming from it.

The absorption of stopped π^--mesons will refer to somewhat denser regions of the nucleus than are operative for the absorption of stopped K^--mesons. Regions even sparser than those probed by stopped K^--mesons may be operative for the absorption of stopped anti-protons and, probably, Σ^--hyperons. A comparison of data from all such negatively-charged particles should be most valuable in synthesizing a detailed picture of the structure and composition of the nuclear surface.

We may perhaps finally note, that, in addition to the differences between neutrons and protons in respect of their gross radial distribution, it would also be very interesting to gain information on the differences, if any, between them in respect of their deformations. We know quite accurately the deformation of the protons in, for example, the regions of the rare earths and of the actinides but we know essentially nothing about the deformation of the neutron distribution. It is clearly very difficult to get information on this point but a possible approach comes from the competition between absorption of negative kaons and their X-ray transitions. Absorption is critically determined by the effective radial distribution of matter which itself is determined by, among other things, the overall deformation of the matter distribution. The absorption rates, which may be determined directly by their competition with the X-ray transitions, are sensitive functions of the deformation: for example in a heavy nucleus in which the deformation has a magnitude of about $\beta = 0.1$, a change $\Delta\beta = 0.005$ gives a 5% change in kaon absorption probability. This should easily be measurable as between, for example, one isotope and the next and so quite fine changes in the matter distribution may be determined. (One must also, of course, bear in mind that changes in the absorption probability are also brought about by changes in the radius and diffuseness of the matter distribution. The same 5% change in absorption probability could, for example, be produced by a change in radius of about 0.04 fm or a change in the diffuseness constant of about

0.01 fm.) Experiments of the 'neutron versus proton' type in deformed nuclei would also be of great interest.

CORRELATIONS AND HIGH MOMENTUM STATES

We know that short-range correlations and, correspondingly, high momentum states must exist within the nucleus if the residual nucleon-nucleon interaction there even remotely resembles that obtaining in the free state. It is, indeed, the basis of the current and quite successful 'realistic force' calculations of absolute nuclear binding energies and nuclear spectra that the interaction between two nucleons encountering one another within the nucleus is essentially the same as that which they would experience in the free state. It is, however, extremely difficult to get direct or even indirect information about short-range nucleon correlations and our present experimental ignorance on this point is effectively total. We must, hopefully, look towards high energy methods for the resolution of this problem since, as mentioned earlier, it is only by the use of particles of short wavelength that one can determine spatial structures on the scale of the mean nucleon-nucleon separation or less.

The short-range nucleon-nucleon correlations will affect the details of any potential that we calculate on the basis of our knowledge of the interaction between the particle experiencing the potential and free nucleons. It has been emphasized, particularly by Ericson, that the potential operating between a nucleus and a pion of low energy can be computed in terms of the pion-nucleon and pion-nucleon-nucleon interactions, as empirically determined, provided that one takes into account the short-range correlation structure within the nucleus. Points at which an experimental confrontation with theory can most significantly be made are the level shifts in pionic atoms and the level widths, the latter being due chiefly to the πNN interaction. In principle, a detailed analysis of this confrontation will tell us something about the short-range nucleon correlations but matters have not yet progressed to the point where unambiguous statements can be made. It does appear, however, that the data may be better accounted for by a correlated nuclear wavefunction than by an uncorrelated one.

High energy elastic scattering gives a clear approach to the problem of short-range correlations. It has been known for a long time, as a result of the work of Glauber et al., that in heavy nuclei a description of high energy particle-nucleus scattering in terms of a multiple scattering formalism is directly equivalent to the optical model. In light nuclei, however, the multiple scattering formalism differs from the optical model which is itself not there directly related to physically significant parameters of the nucleon distribution. The work of Palevsky et al. has shown that for nuclei heavier than about C the scattering of 1 GeV protons essentially reveals only the Fermi density distribution known from other methods (neutron and proton distributions being assumed the same) and tells us nothing new. For lighter nuclei, however, we may experimentally separate out the elastic scattering into components due to single scattering, double scattering and so on. These various components are coherent and in the angular range where the one is taking over from the other, interesting interference effects will be found (which will, incidentally, tell us things about the interaction between nucleons that we cannot determine from NN scattering). The multiple scattering components are sensitive to the 2-nucleon correlation function and it is the hope that an analysis of them will reveal that correlation function. The analysis will proceed most powerfully by comparing high energy electron scattering (which is not directly sensitive to correlations) with the high energy scattering of strongly-interacting particles. One must choose charge distributions that satisfactorily account for the elastic electron scattering and then ask, for each such choice, what correlation function, within the underlying nucleon distributions that reproduce that charge distribution, is demanded to explain the, say, double scattering of the strongly interacting particle. It will be the hope that nucleon correlations are demanded by such analysis in the sense that only correlated wavefunctions will be able simultaneously to explain the electron and strongly-interacting-particle scattering. This situation has not yet been reached and, for example, the scattering of 1 GeV protons by helium can be explained in terms of an uncorrelated nucleon wavefunction, although one containing correlations may appear even now to give a slightly better fit. Such experiments must be greatly extended and refined and should also be made with pions as well as protons, since with pions the complications arising from the spin dependence of the primitive scattering amplitudes are much less.

We might, at this point, remark that such experiments can also be carried out with the production of unstable particles such as nucleon isobars and that the components of their angular distribution corresponding to multiple scattering will then contain information about, in this example, the isobar-nucleon scattering amplitude. It is clearly impossible to obtain this information other than through the offices of complex nuclei. This is only one point at which the interaction between nuclear structure and elementary particle studies, through these high energy methods, may bring great advantage to both.

It is well known that inelastic scattering is sensitive to nucleon correlations. Inelastic scattering for high values of the momentum transfer is essentially determined by the quasi-free scattering on individual nucleons in the nucleus acting incoherently. Inelastic scattering for zero momentum transfer is zero and the way in which the scattering cross-section falls down from its quasi-free incoherent value to zero is one in which the coherence between the individual scattering nucleons plays an increasing role. If we exclude meson production and sum over all inelastic channels then the inelastic scattering in this coherent region where the cross-section is falling towards zero depends directly on the Fourier transform of the pair correlation function. This interesting region, that gives us information about the pair correlations, is associated with the coherent action of the nucleons in the nucleus in just the same way as is the forward diffraction elastic scattering peak, and as is hidden beneath that peak; very good energy resolution is therefore needed to separate out the elastic from the inelastic scattering in the region sensitive to the pair correlations. It has been suggested by Ericson that a possible way around the need for high energy resolution is to study the charge exchange of pions, for example (π^+, π^0), a process which is automatically inelastic (with appropriate choice of target nucleus). Good energy resolution would therefore not be needed. Of course, we would not get directly an unbiassed picture of the nucleon pair correlation function but would gain information rather about the difference between the pair correlation functions for unlike and for like nucleons.

When nuclei are bombarded by high energy protons quite copious yields of high energy deuterons are found in a manner that suggests that the process is an elastic one in which 'deuterons' are knocked out of the nucleus under the impact of the ingoing protons or, alternatively, in which the ingoing proton picks up a neutron from a correlated neutron-proton pair within the nucleus. In either event nucleon-nucleon correlations within the nucleus are demanded and so this process gives us information, at least in principle, about such correlations. The interpretation of these experiments is so far not very quantitative but it is interesting to note that from the kinematical relationship between the emerging proton and deuteron one may infer the effective momentum distribution within the nucleus of the struck 'deuteron'. This corresponds to motions of the struck correlated pair with energies of the order of 10-20 MeV showing that the strongly correlated nucleon pair is itself in a state of some tranquility relative to the rest of the nucleus.

This last conclusion is in agreement with data on the (π^+, 2p) reaction in which low energy pions irradiate nuclei and are absorbed on correlated neutron-proton pairs with the production of two protons of high energy. Again the kinematical relationships in the final state allow us to make a statement about the momentum distribution of the absorbing neutron-proton pair and again the statement is that that pair is not in violent motion relative to the rest of the nucleus but in a state of motion such as would correspond quite closely to the lowest quantum state of an object of deuteron mass within a container of nuclear dimensions (Grashin and Shalamov).

The two latter consistent observations are in agreement with the picture of the nucleus that is used as the basis of calculations of nuclear binding energies and level structure, namely that, when a close interaction between two nucleons is taking place, the rest of the nucleus is 'unaware' of it.

A further approach to the question of correlations of high momentum states inside nuclei is from the absorption of slow pions with the production of single fast nucleons, e.g. ^{12}C (π^-, n) ^{11}B, or alternatively, the inverse process in which single pions are produced under the bombardment of energetic protons such as to leave the residual nucleus in a ground or low-lying state, e.g. ^{15}N (p, π^0) ^{16}O. Such experiments are rather hazardous to interpret since the optical model, which is used as an aid in the case of heavier nuclei, deals only with averages and gives no account of the fluctuations that may be responsible for the occasional production of a nucleon in a high momentum state. Such fluctuations can falsify the conclusions that we might draw about the reality of high momentum states in the actual nucleon distribution. It is probably most profitable to study very light nuclei and to make an analysis using the Glauber multiple scattering formalism rather than the optical model. Important information has already been obtained from the absorption of negative pions in ^3He and ^4He (Eckstein and Divakaran) and a programme of work designed to study processes inverse to these, e.g. ^3He (p, π^+)^4He; d(p, π^+)t, over a range of incident nucleon momenta would be very valuable.

PION ABSORPTION

Owing to the high energy release in the process of pion absorption this will tend to take place on closely correlated nucleon pairs. It is difficult to use the process as a quantitative measure of the strength of the pair correlations, although, as has just been remarked, such analysis may become more meaningful in the very light nuclei. The process can, however, be studied with profit in order to reveal the parentage structure of such closely correlated and so strongly interacting nucleon-nucleon pairs. The chief contribution to the binding energy of a complex nucleus comes from the close interaction between nucleon pairs and it is therefore very important to establish that the environment in which such close interaction comes about is that assumed in the models that are used to calculate the energy balance. Data so far available, chiefly from Charpak et al., indeed reveal parentage structures in close agreement with those deriving from our conventional methods of nuclear structure computation. In particular the parentage structure of ^{10}B for ^{12}C peaks rather near the ^{10}B ground state whereas that of ^{12}C for ^{14}N peaks very far from the ground state. This striking difference between neighbouring nuclei is just what is expected from the independent particle model in intermediate coupling (Kopaleishvili). The extension of these experiments and in particular, an improvement in their resolution (at present about 5 MeV) to the point where discrete parent states may be detected would be extremely valuable.

PION SCATTERING

Charge exchange scattering of charged pions depends on overlap integrals somewhat like β-decay matrix elements and so a study of them is somewhat like a study of β-decay but with variable momentum transfer. Such work would be very valuable in adding a new dimension to our testing of nuclear models just as inelastic electron scattering, because of its variable momentum transfer, adds another dimension to our testing of the predictions made by nuclear models as to radiative transition rates. It is already clear from the work of Tanner et al. that cross-sections are large for such pion charge exchange scattering where mirror states are involved and are small where the overlap integrals are known to be small from the corresponding β-decay. Such work is still in its infancy.

Another form of pion scattering is the $(\pi, \pi N)$ reaction whose energy dependence appears to reveal the familiar 3,3 resonance of free pion-nucleon scattering and which therefore appears superficially to be a straightforward quasi-elastic knock-out process. However, in that case, we should naively expect the cross-section for $X(\pi^-, \pi^- n)Y$ where X is self-conjugate to be greater than the $X(\pi^+, {}^{\pi^+ n}_{\pi^0 p})Y$ cross-section by a factor of 3 near the peak of the 3,3 resonance. In practice, as shown by Tanner el al., for targets of ^{12}C, ^{14}N, and ^{16}O, the ratio is 1.0 ± 0.1. While it is clear, as emphasized by Shapiro and Kolybasov, that the cross-section ratio should come down from its naive value of 3 on account of the Fermi motion of the struck nucleons, it is not clear that it should come down as far as unity and these observations continue to pose an interesting and unresolved problem. Detailed studies of the kinematical relationships and energy distributions of the outgoing particles are obviously needed to make progress here.

Pion-nucleus elastic scattering is relatively unstudied under conditions of good energy and angular resolution. An interesting observation already made by Stroot et al. is that π-^{12}C elastic scattering at 180 MeV shows a very deep minimum at a scattering angle of about 50°. This is the 3,3 energy for which, for free πN scattering, the real part of the 3,3 amplitude vanishes. The only way, as Ericson has pointed out, of understanding the very deep minimum at just this energy would appear to be that the real part of the scattering amplitude also vanishes when the nucleons are immersed in nuclear matter. This implies that nucleons within the nucleus indeed look very much like nucleons in the free state which, as emphasized in the Introduction to these remarks, is not a trivial point. The picture of the nucleus as a collection of neutrons and protons, such as we normally understand those terms, begins to gain some direct credibility.

K^- ABSORPTION

The probably peripheral nature of the nuclear absorption of stopped K^--mesons has already been extensively discussed and one or two points at which it can bring us important information about the nucleus have already been mentioned. We may here recall that the study of the non-mesonic absorption process, $K^- + NN \rightarrow Y + N$, promises to bring us information about the degree of nucleon-nucleon correlation in the nuclear surface. The fact that non-mesonic K^- absorption appears to be about 20 times stronger in complex nuclei than in deuterium, even though the mean density of nuclear matter in the periphery of the complex nuclei where such absorption is taking place is approximately the same as that in the deuteron, naively indicates that the degree of nucleon-nucleon correlation in the nuclear surface is very much higher than one would expect from simple shell model wavefunctions. This conclusion has been heavily disputed by Wycech but the situation is not clear and, in any case, good experimental data relating to definite nuclei are badly needed.

MUON ABSORPTION

A very fruitful study on nuclear structure physics over the last many years has been that of the so-called giant dipole resonance of nuclear photodisintegration. This, for self-conjugate nuclei, is a $T = 1$, $J^{\pi} = 1^-$, $L = 1$, $S = 0$ collective isospin resonance pictured in the Goldhaber-Teller model as an oscillation of all the protons in anti-correlation with all the neutrons. This is one of the simplest modes of nuclear collective excitation. There are to be expected, however, other collective modes that are not excited, or are only weakly excited, by photons. One such is the spin-isospin collective oscillation which may be similarly described as $T = 1$, $J^{\pi} = 0^-, 1^-, 2^-$, $L = 1$, $S = 1$. Such collective oscillations are just as important as the familiar dipole state and information on them would be eagerly welcomed. The spin-isospin resonances can, in fact, if they exist, be excited by muon absorption through the axial vector component of that interaction just as the giant isospin resonance can be similarly excited through the vector component; they may, in fact, be thought of as giant inverse Gamow-Teller beta-decay resonances. It indeed appears that one cannot satisfactorily account for the absolute absorption rate of negative muons without invoking the existence of the spin-isospin resonances at energies approximately equal to the familiar isospin resonance (Foldy, Walecka et al.). Such absorption is, in fact, dominated by the spin-isospin resonances on account of their greater number (and the approximate equality of the vector and axial vector coupling constants).

Direct study of the spin-isospin resonances is not easy since their excitation is revealed by the energy spectrum of the product neutrinos which clearly cannot be determined. Ericson et al. have, however, pointed out that the radiative capture of negative pions proceeds via a Hamiltonian that closely resembles that for the axial vector absorption of muons. Accurate determination of the high energy gamma-ray spectra following negative pion capture should, therefore, directly reveal the excitation of the spin-isospin collective oscillations. This type of study should be an extensive and exciting chapter of nuclear structure work.

CONCLUSIONS

It is clear that a book is already needed to do justice to the accomplishments and, in particular, to the promise of the applications of high energy and elementary particle methods to the study of nuclear structure. I have left untouched great areas of work and have not mentioned a large number of matters where the information to be expected is of a kind that duplicates that obtained by conventional methods rather than contains elements of essential novelty. It will be clear that much of the work that I have touched upon is still in an extremely primitive state and must in no way be judged only from the standpoint of what we have already learned from it. Enormous advances will be made when the pion factories come into operation and yet more spectacular advances would be forthcoming were very intense beams of kaons also to come onto the market. It should also be emphasized that, with rare exceptions, the great accelerating machines have so far not been made available for nuclear structure studies and that much exciting work could be done, even with present facilities, should they be freely opened to this kind of work.

It must not be supposed that nuclear structure experiments at high energies are any easier or cheaper than elementary particle experiments at those same energies. Indeed, it may be expected that they will be more expensive and more difficult because of the frequent need for relatively high resolution which is usually not demanded in elementary particle studies. However, it will quite frequently arise that a prospecting experiment could be carried out quite easily using facilities already established for a high energy experiment by continuing that experiment for a short time but replacing the hydrogen target by one made out of complex nuclei. There is a lot to be said for the suggestion made by Zupancic that one should deliberately foster mixed teams which principally carry out high energy experiments but which contain one or two nuclear structure physicists who are able to spot points at which a slight extension of the work such as I have just mentioned might bring big profits for nuclear structure physics. And it is perhaps worthwhile finally re-emphasizing the point already made, namely that the important study of the interaction between very short-lived 'elementary particle' states with nucleons can essentially only be done through the offices of complex nuclei in which the production nucleon and the scattering nucleon are in the same nucleus.

The application of high energy methods to nuclear structure studies will often not be easy and will often be rather expensive but will bring information essential for completing our account of nuclear structure physics.

DISCUSSION

G. E. BROWN: I should like to ask what has happened to α-particle clusters in the surface of nuclei and whether one can investigate them by strange particles. Your discussion about the K^- - absorption and the ratio of 5 for neutron to proton densities would seem to exclude this. I think on rather general theoretical grounds one might expect, at least in heavy nuclei, that when the density became low you would predominantly have α-particle clusters.

D. H. WILKINSON: I should like, very briefly, to recapitulate the evidence. The negative kaon absorption, by theory at least, is a surface phenomenon. It is very difficult to understand that ratio of 5. Indirectly one can take the data that I have briefly mentioned as confirming the surface absorption hypothesis. Of course the kaon absorption can go by two different sorts of channel: a hyperon-pion channel and by absorption on a nucleon-pair and a hyperon-nucleon channel which can be experimentally separated. The ratio of these two channels in deuterium is about 1%. In heavy nuclei, it is about 20%, even though the absorption is taking place chiefly in a region where the mean density of nuclear matter is thought to be about the same as it is in the deuteron. Of course, this we do not know. The X-ray studies in particular will be very valuable for telling us something about the density distribution of matter where the mean density is very low. But taking a Fermi type parametrization one would expect the average nucleon density to be about the same as in the deuteron. So it seems that although the absorption takes place in a region of low mean density, the correlations there are very much stronger than they are in the deuteron. The other figure of interest is the absorption in helium-4, where the probability again is about 20%. So, extremely crudely and without any pretence at being literal, the correspondence of these two figures might suggest that there are clusters in the nuclear stratosphere of approximately the same seriousness as in the α-particle. This has been criticized recently by Vitzek, who has shown in a very complicated calculation that indeed, in heavy nuclei, one might expect a significantly greater proportion of non-mesonic absorption than in the deuteron even though there is no stronger clustering. Of course there are other experiments that bear on the question of α-particles in the surface, particularly the old alpha-to-alpha experiments at 1 GeV, which were certainly repeated and extended, but also seemed to indicate in the very tenuous regions of the surface of heavy nuclei that there was a strong abundance of clusters with an α-particle character. Now as for reconciling one to one with five to one, we don't know how to do it. It could be that both statements are wrong; it is 50-50 α-particles and uncorrelated nucleons..... But if you take both at their face value it is difficult to avoid the conclusion that there is a considerable neutron excess, and also difficult to avoid the conclusion that there is stronger nucleon clustering that one gets from uncorrelated shell-model wave-functioning. This is a very unsatisfactory situation — just one more point which high-energy methods can make a contribution to.

G. E. BROWN: My statement is based on very general theoretical considerations. Namely, when one makes shell-model calculations of the excitation of low-lying vibrations, say in nickel and zirconium isotopes, one finds, using different projectiles, that the excitation of these low-lying vibrations is as if they were completely T = 0 type excitations. A shell-model calculation would predict a rather large admixture of T = 1. These calculations were made in Copenhagen by Veje, and also later, once by Petersson and Veje. It seems that in the surface of the nucleus neutrons are anchored much more tightly to protons than the shell-model would indicate. Namely, you simply cannot pull them apart to make them vibrate against each other nearly as easily. A particularly striking indication of this is the giant dipole-resonance. Following suggestions of Bohr, Mottelson, Petersson and Veje in Copenhagen made a calculation of a giant dipole resonance in a simplified model, taking dipole-dipole force. They found that if one wanted to obtain the position and the basis of the shell-model they needed a component which was four times larger than that which is found in the optical model central well — a really extremely striking effect. Now the one model which does get the dipole resonance at the correct high position is the Jensen-Steinwedl model. The Jensen-Steinwedl model operates directly from the symmetry energy. In other words, there are very few assumptions, but the main assumption is that at the edge of the nucleus the proton fluid does not oscillate relative to the neutron fluid, $\Delta\rho = 0$ at the edge of the nucleus. But this is completely different from any shell-model calculation which would allow $\Delta\rho$ to fluctuate rather widely at the edge of the nucleus. And it seems to me suggestive that the Jensen-Steinwedl model does get the energy right, whereas shell-model calculations do not unless one simply mechanically cranks up a particular component of the force.

I. S. SHAPIRO: I should like to complete the list of new possibilities which are opened for nuclear physics in connection with the use of high energy particles. Especially of great interest are the experiments investigating the interaction of particles-resonances with nuclei. As an example one could consider the reaction $^{12}C\ (\pi^-, \pi^- n)\ ^{11}C$ in the energy region close to the baryon resonance $\Delta_{3/2,\ 3/2}$. The application of data obtained from this reaction has allowed the estimation of the order of magnitude of collision length of a nucleon's isobar on the carbon nucleus.

The experiments on the double charge exchange of pions are also very interesting. In principle these experiments allow one to come to a decision about the existence of many-nucleon resonances (i. e. nuclei, in which one nucleon is displaced by a baryon resonance).

These experiments are, however, very difficult. International collaboration and co-operation in this part of nuclear physics is therefore essential. Because of complexity and laboriousness of these experiments, close contact between the experimentalists and theoreticians when planning the experiment is necessary. No less essential is the purely theoretical problem — the development of an adequate theoretical method that will permit indication of the characteristic features of these phenomena.

D. H. WILKINSON: I should particularly like to thank Professor Shapiro for his last remark. In the United Kingdom, we have recently reached a decision in principle to open 7 GeV accelerator Nimrod to nuclear structure work. We would very much welcome people from anywhere to join the group that are going to use Nimrod for nuclear structure work. I think I could almost make this a formal invitation particularly to the Dubna group to come and join in if they would like to. I should also like to emphasize the significance of what Professor Shapiro said: these measurements are difficult. Typically a high-energy nuclear structure experiment mounted at a high-energy accelerator is more demanding than a high-energy experiment, and also more expensive. This we have seen in our detailed planning for using Nimrod for nuclear structure work.

THE CHALLENGE AND PROMISE OF THE NEW RESEARCH TOOLS: SOLID-STATE DETECTORS, COMPUTERS AND ACCELERATORS

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Abstract

THE CHALLENGE AND PROMISE OF THE NEW RESEARCH TOOLS: SOLID-STATE DETECTORS, COMPUTERS AND ACCELERATORS. Nuclear structure research has been the beneficiary of many recent technical advances and in turn directly stimulated some advances. The exciting possibilities of the lithium-drifted germanium detector in gamma spectroscopy have only begun to be realized. The large amounts of high resolution data have put a premium on developing more automation of data handling through more sophisticated electronics and computers. Trends in accelerators are discussed, and reference tables listing isochronous cyclotrons and Tandem Van de Graafs around the world are given. Attention is directed to new frontiers of research in heavier ion accelerators, and tables of characteristics of existing and proposed heavy ion accelerators are given. The difficulties of obtaining multiply charged ions from known types of ion sources are considered, and the high charges resulting from Auger cascades of a K-vacancy are noted. It is suggested that intensive research on decay schemes and charge states of recoil products of nuclear reactions could lead to a practical accelerator of very heavy ions. As an example is discussed a possible arrangement in a Tandem Van de Graaf, where a deuteron negative ion beam strikes a source foil in the positive terminal, with recoil products or fission products accelerated to ground.

Studies on noble gas and halogen fission products by gas transport systems and isotope separators are noted. Also reviewed are germanium gamma studies on unseparated ^{252}Cf spontaneous fission products using tape-transport methods and K X-ray coincidence.

Next are reviewed studies on gamma and conversion-electron spectra of recoils and fission products in flight. The use of solenoidal or fringing-field magnets for conversion electron studies is discussed. Some of the qualitatively new aspects of nuclear studies with very heavy ion beams are mentioned.

Finally, it is stressed that the research here called for on gamma cascades and charge states of nuclear reaction products is most extensive, may in part be done with modest equipment, and will need the co-operative efforts of many workers.

Nuclear structure research has been beneficiary of many recent technical advances and the direct stimulus for some of them. The lithium-drifted silicon and germanium detectors were developed in direct response to needs of nuclear structure research for higher resolution in multi-channel energy measuring devices. We have only begun to realize the tremendous potential of this order-of-magnitude improvement in resolution over the scintillation spectrometers. There is literally a data explosion making available for the first time much high quality data on nuclear energy levels and properties. Most studies of gamma spectra using the lithium-drifted germanium detectors have uncovered much structure in multiple peaks below the old, unresolved peaks from scintillation spectroscopy. Another development that has affected and is greatly affecting nuclear science, both in the theoretical and in the experimental area, has come in the revolutionary im-

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provements of computers, large and small. This development has come in response to broader demands of the technological society outside our nuclear science, but our work has been very much affected and has fed back into the development in many significant ways. The past ten years have seen, within the United States, a substantially increased concern over scientific excellence in education and research. This interest has been tangibly reflected in the funding that has permitted two new classes of modern accelerators to be widely constructed throughout the country. It is only now that a serious reaction is setting in, a deep questioning in government of the priorities for support of the various demands on government. This year is seeing a sharp curtailment of the expenditures in basic science. These two principal new classes of accelerators serving nuclear structure science are the Tandem Van de Graafs and the isochronous cyclotrons.

By restricting the listings here to these two newer classes of accelerators, I don't wish to imply that the older conventional cyclotrons, Van de Graafs, and reactors are not useful for research. In fact, there is very much new that can be done and is being done on the very old types of accelerators, using the new instrumentation – the high resolution, solid-state detectors. One new frontier in accelerator development has appeared in the proposals for new accelerators for heavier ions than are now available, such acceleration to reach energies above the Coulomb barrier for all possible targets.

Table I lists the characteristics of some existing heavy ion accelerators. The Yale and Berkeley heavy ion accelerators give the greatest energy per nucleon and the Dubna 300 cm cyclotron gives only slightly less energy with considerably more intensity. This table is taken from a review panel report [1] on the Berkeley Omnitron proposal.

Table II, from the same panel report, lists the calculated capabilities of some proposed heavy ion accelerators. One of the big problems in designing efficient accelerators for ions as heavy as those in the middle of

TABLE I. BEAMS FROM SOME EXISTING HEAVY-ION ACCELERATORS

Accelerator	Projectile	Maximum energy (MeV/nucleon)	Intensity (particles/s)
Berkeley HILAC	^{12}C	10	2.5×10^{13}
	^{20}Ne	10.5	3.8×10^{12}
	^{40}Ar	10.5	10^{11}
	^{84}Kr	10	10^{8}
Yale HILAC	^{6}Li	10.5	1×10^{11}
	^{12}C	10.5	4×10^{11}
	^{20}Ne	10.5	8×10^{10}
	^{40}Ar	10.5	5×10^{8}
Dubna 300 cm cyclotron	^{20}Ne	9	3×10^{14}
	^{42}Ar	9	3×10^{12}
Dubna 150 cm cyclotron	^{10}B-^{14}N	6-7	

TABLE II. COMPARISON OF SOME PROPOSED HEAVY-ION ACCELERATORS

Accelerator:	Omnitron		Unilac (Germany)		TU Tandem (ORNL)		XTU-MP Coupled Tandems (MIT-High Voltage Eng.)		HIVEC (ANL)	
Cost (millions of $):	9.1		5.1		7.7		12		7.6	
Accelerated Ion	E_{max} (MeV)	I	E_{max} (MeV)	I	E_{max} (MeV)	I	E_{max} (MeV)	I	E_{max} (MeV)	I
^{1}H	1430	8×10^{12}	33.7	1×10^{16}	40	1×10^{14}	36	3×10^{13}	120	6×10^{14}
^{4}He	600		18.3	6×10^{15}	15	6×10^{12}			60	3×10^{14}
^{12}C	500	3×10^{11}							40	2×10^{13}
	6.5	8×10^{12}								
^{16}O	500				11	5×10^{13}				
^{20}Ne	500	2×10^{12}	15.4	2×10^{15}					17	2×10^{13}
	6.5	6×10^{12}								
^{35}Cl					8	2×10^{13}				
^{40}Ar	405	5×10^{11}	13	6×10^{14}			16	5×10^{11}	17	2×10^{12}
	6.5	3×10^{12}								
^{79}Br					5.2	8×10^{11}	11.4	3×10^{12}		
					5.6	1×10^{11}				
^{84}Kr	370	2×10^{11}	10.4	6×10^{13}					6	5×10^{11}
	6.6	1×10^{12}								
^{127}I					3.6	1×10^{11}	8.9	3×10^{12}		
					4.0	1×10^{10}				
^{132}Xe	336	1×10^{11}	9	4×10^{13}						
	7	8×10^{12}								
^{238}U	298	5×10^{9}	7	8×10^{12}			5.9	3×10^{11}		
	6.6	5×10^{11}								

the periodic table is the difficulty of designing an ion source that will produce very much of highly charged ions. We get an idea of the source difficulties for heavy ions by looking at Fig. 1. This shows the actual relative abundance of various multiply charged species from one of the best types of heavy ion sources, a so-called 'PIG discharge' in a magnetic field with 2000 V on the anode [2]. One sees that the +10 Xe ions constitute only ~3% of the abundance of singly charged ions. Thus, the proposed designs for new heavy ion accelerators must accommodate to initial acceleration at these rather low charge-to-mass ratios and make use later of gas or foil stripping after some initial energy has been gained. If by some magic we could devise a way to achieve more highly charged ions at the source, the design problems of the heavy ion accelerator would become correspondingly simpler.

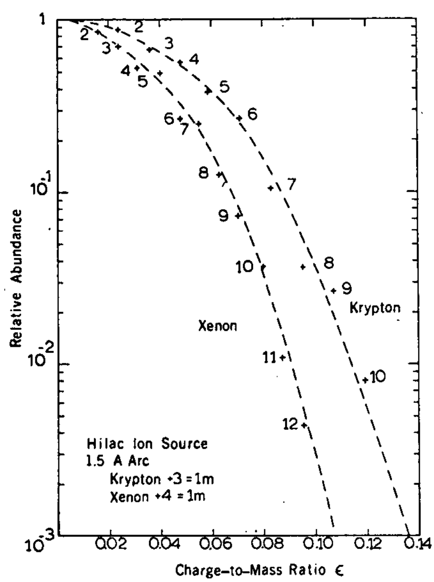

FIG. 1. Relative abundance of various charge states of krypton and xenon from a 'PIG' discharge ion source of the Berkeley heavy ion linear accelerator.

Now let me remind you of the great effectiveness of a K-vacancy in shaking off outer-shell orbital electrons through an Auger cascade. Snell at Oak Ridge has made careful studies on the charge distribution resulting after beta-decay or internally converted isomeric transitions among radioactive xenon isotopes. Figure 2 taken from his review article [3] shows the distribution of charges he experimentally observed. Focus attention on the dotted bars for the isomeric transition, which often creates a K-vacancy. Note that the most probable charge is +9, and down only two orders of magnitude from this is the charge of +17. Gunter, Asaro, and Helmholz [4] have made similar studies on the recoil daughters following the alpha-decay of ^{226}Th, and Fig. 3 shows their results. The curve on the left is for alpha-decay to ground, and it illustrates the rather low charges associated with the alpha-decay process itself. The curve on the right is for alpha-decay

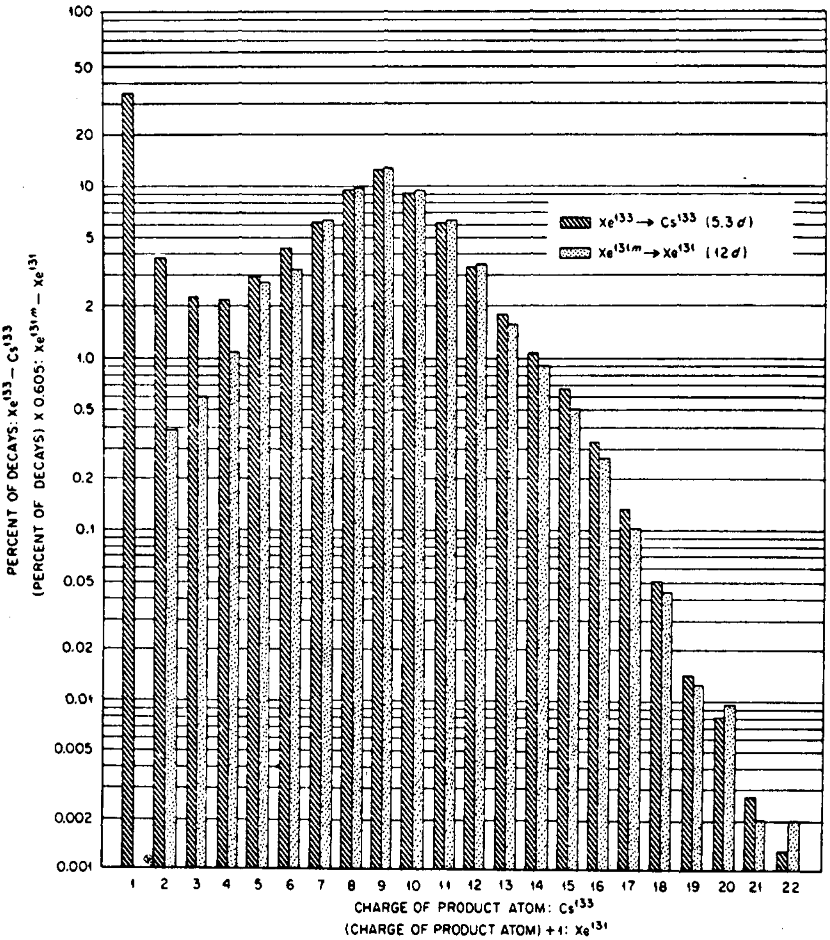

FIG. 2. The atomic charge spectra resulting from internal conversion in ^{131m}Xe - ^{131}Xe (light histogram) and the β^--internal conversion decay of ^{133}Xe - ^{133}Cs (heavy), plotted together for comparison. The ^{131}Xe spectrum has been shifted by +1 unit of charge to compensate for its lack of a β^--particle, and its intensities have been normalized so as to match the total number of internal conversions represented in the spectrum of ^{133}Xe.

to the 111-keV excited state. The de-excitation of this state occurs in part by K-electron conversion and the resulting Auger cascade following that produces the curve on the right with the much higher charges on the ions. There is a rather flat peak that only drops off at charge 13 and by charge +20 we are only a factor of 30 down from the peak.

Next let us look briefly at studies of the ionic charge distribution on thermal-neutron-induced fission fragments from a thin uranium source. These studies have been made at a reactor near Munich, where a large mass spectrograph looks directly at the thin ^{235}U source. The work was performed by Ewald [5] and by Konecny and Siegert [6]. Figure 4 shows the charge distribution for various masses from 132 to 137 in the heavy fragment group at 78 MeV kinetic energy. The most probable charge is around 23 and no data are shown for charges higher than 24. We note that the different mass chains exhibit considerable differences – the fall-off towards high charges for mass 137 seems definitely less than for the other masses, and we might suppose that there is a higher amount of K-conversion to make higher charges more probable. It would be indeed interesting to follow these curves out to considerably higher charges.

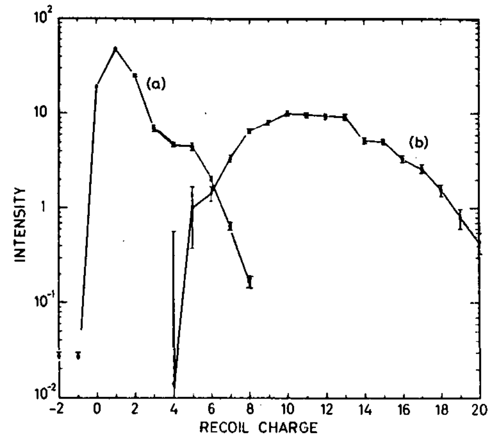

FIG. 3. Charge distributions of recoils following ^{226}Th alpha-decay; (a) charge distribution of R_0 recoils; (b) charge distribution of $R_{111}e$ recoils (R_{111} recoils which decay by conversion electrons).

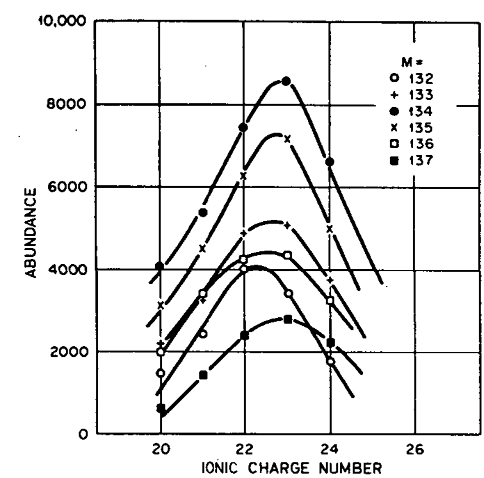

FIG. 4. Ionic charge distribution of individual fission products for the heavy group ($132 \leq A \leq 137$) at fixed kinetic energy (77.7 MeV).

The lithium-drifted solid-state detectors have made it possible to resolve the K X-rays of adjacent elements throughout the whole fission-product region, and hence studies of K X-rays in coincidence with fission fragments have become more valuable. From such studies, we might, among other things, hope to gain a better understanding of the differences between ionic charge distributions of fission fragments as seen in Fig. 4. The studies are obviously easier with spontaneous fissioning sources than with neutron-induced fission because of lesser difficulty from general radiation background. We know of at least three laboratories where such studies [7] of K X-rays in coincidence with spontaneous fission of ^{252}Cf have been carried out, namely the Argonne National Laboratory, the Forrestal Laboratory at Princeton and the Lawrence Radiation Laboratory in Berkeley. Figure 5 represents recent results from the work of Bowman and others at Berkeley [8]. We see plotted the number of K X-rays per fragment versus atomic number, with most probable mass number scale along the top. These results were taken from triple coincidence measurements where solid-state detectors measured the pulse heights of both fission fragments, and the lithium-drifted silicon detector measured the X-ray energy. The three pulse heights associated with an event went onto magnetic tape event-by-event and were sorted in the large computer. The timing was set such that these represent X-rays

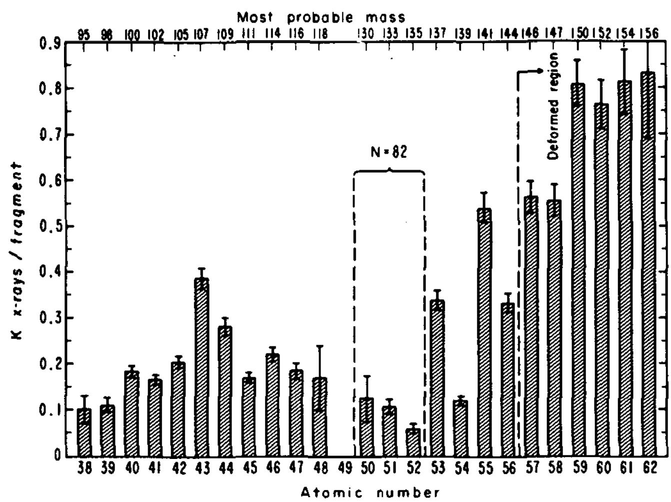

FIG. 5. *Estimates of the yields of K X-rays per fragment arising from primary ^{252}Cf fission products.* The indicated errors reflect only the uncertainty in the X-ray intensity measurements. Atomic numbers for which N = 82 closed-shell fission products are expected to occur are denoted, and the approximate boundary of the deformed region is shown. The top scale indicates the calculated most probable fragment mass associated with each atomic number.

emitted within 93 nanoseconds after fission. First of all it is obvious that there is a substantial amount of K-conversion in the prompt cascade de-exciting primary fission fragments. Remember that the fluorescence yield for K-vacancies is substantially less than unity for the light elements, so that for many light as well as heavy elements there seems to be an average of one K-vacancy per fragment. The magic region around 82 neutrons seems to be a region of rather low yield; perhaps the gamma cascade involves higher energy transitions that do not internally convert so well. The very heaviest elements detected are probably deformed nuclei cascading through rotational sequences, and they naturally show a high yield of K X-rays per fragment. One notes also that there is a good deal of fluctuation from element to element. Perhaps technetium fragments (Z = 43) are good prospects for developing high charge in vacuum, since the high K X-ray yield and lower fluorescence yield means many K-Auger events.

Directly complementary to the study of K X-rays in prompt coincidence with fission fragments are the studies of conversion electron spectra in prompt coincidence. I will not take time here to show any of the hundreds of spectra that have been obtained by Rand Watson and collaborators in Berkeley [9]. Similar studies have also been made at Princeton by Atneosen and Thomas [10]. In the Berkeley work a great aid to obtaining sharp spectra was found to be the use of the 5 kG fringing field of a large C-magnet. The fringing field served as a high geometry electron transporter, the electrons precessing in trochoidal orbits around the perimeter and behind lead shielding, so that the cooled lithium-drifted silicon detector for the electrons could not see the source directly and was thus not bombarded with fission fragments or with electromagnetic radiation. In the geometry of this fringing field magnetic arrangement it was easier to get sharp spectra for fission fragments giving off conversion electrons in flight, 1 cm or 2 cm along the flight path, for then the electron source was truly massless. Four-fold coincidence spectra have been measured by Watson and others but are not yet completely analysed. These four-fold coincidences involve two fission fragment pulse heights, a K X-ray pulse height and a conversion electron pulse height. The four-fold coincidences should allow the separation of K-conversion lines from all other electron radiation. These three- and four-fold pulse-height analysis coincidence experiments may well employ 1024-channel analysis or more in each dimension. Thus we may have 10^{12} different kinds of events, and we are necessarily in need of the most sophisticated computer usage. We are learning that the programming and data processing part of the experiments may often be the hardest and the longest part of the work. Multi-dimensional pulse-height analysis studies are not to be entered into lightly.

Now let us return to some modern experimentation using the high resolution inherent in the lithium-drifted germanium detectors but using them in singles measurements with ordinary pulse-height analysers. Morinaga and Gugelot [11] working at the Amsterdam cyclotron with sodium iodide detectors first showed the possibilities of resolving discrete gamma-rays from rotational bands following (α, xn) reactions. Studies of conversion electrons and gamma-rays following nuclear reactions for the medium heavy elements are now strongly pursued in a number of laboratories, too numerous to mention. In line with one theme of this paper, the hope of finding reaction products or fission products that create many K-vacancies in the course of de-excitation, I show an example of the gamma-ray singles spectra from (α, xn) reactions on tantalum. This work was done by Hjorth, Ryde and Skånberg in Stockholm [12]. Figure 6 shows the spectra at different alpha-ray energies which maximize the different final nuclei in rhenium. The spectra are complex but rather well resolved. Figure 7, also from their report, shows the level scheme of the nucleus ^{183}Re, the principal product of the 30-MeV bombardment. The gamma-rays of the lowest figure of Fig. 6 have been sorted into the cascade down two well-developed rotational bands. One might well guess that ^{183}Re reaction products, including the 1 ms isomer, if they decay in vacuum, might have a reasonable probability of developing multiple K-vacancies and quite high charges.

Well, let us pause now from considering these specific studies and carry out a little far-out speculation as to whether there are possibilities

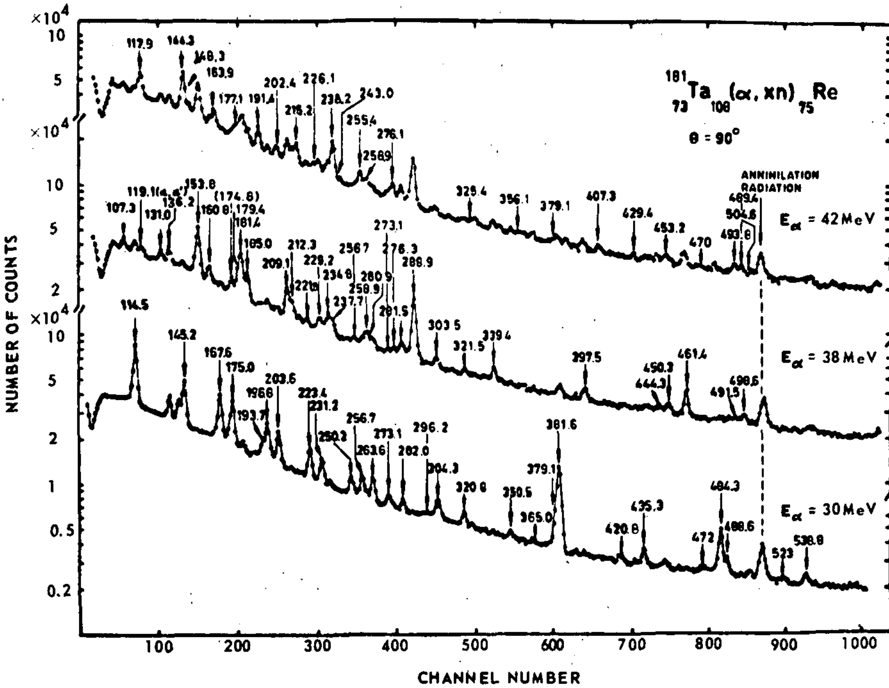

FIG. 6. Gamma-ray spectrum from (α, xn) reactions measured with three different bombarding energies.

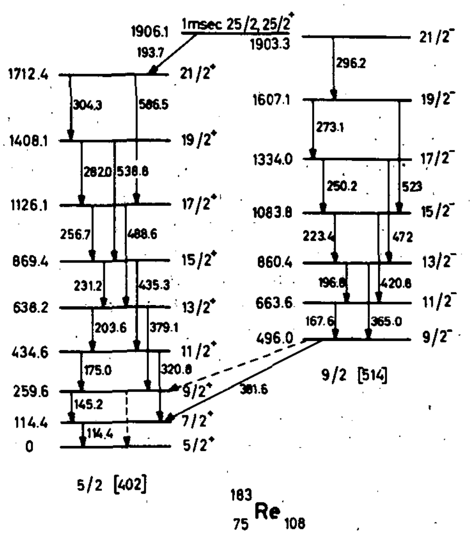

FIG. 7. Rotational bands observed in ^{183}Re following ^{181}Ta(α, 2n)reactions.

for practical acceleration of the highly charged fission fragments or recoil ions. When we consider the challenge and difficulties of such accelerator schemes as the one at Novosibirsk, envisaging clashing beams of anti-protons and protons, I think we are justified in doing a little bold thinking of our own in terms of accelerating fission fragments or nuclear reaction recoils. Figure 4 gives the charges of fission fragments with 78 MeV of kinetic energy. Let us envision an arrangement in a standard MP Tandem Van de Graaf, assuming 10 MV positive on the high-voltage terminal. Let us form at ground negative deuterium ions and accelerate them up to the terminal to 10 MeV.

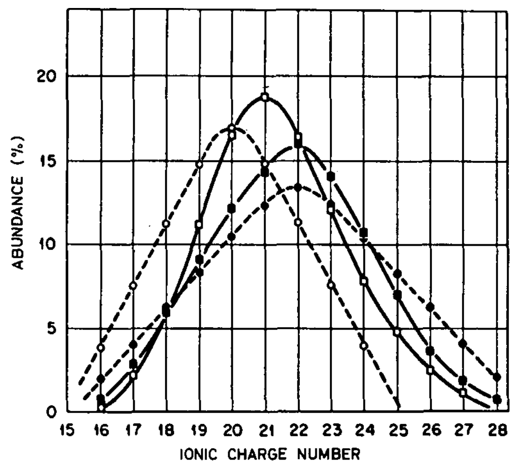

FIG. 8. The ionic charge distribution of fission products integrated over all particles of the light (□) group and of the heavy (■) group (solid lines). The dashed lines are for comparison of the corresponding values of Lassen (O light group, ● heavy group).

Then instead of the conventional arrangement of stripping with acceleration down to ground as a positive ion, let us have the deuterons impinge on a foil of uranium, causing fission. Consider the fission fragments of Fig. 4 and the most probable charge state of 23. The fission fragments will gain an extra 230 MV of energy in accelerating down to ground, and that will be added to the approximately 80 MW kinetic energy they have in the first place to give us a total of 310 MeV. Unfortunately, that is not quite enough energy to get over the Coulomb barrier on targets of comparable mass. The Coulomb barrier around Z = 50 will be about 250 MeV, and to surpass that barrier will require about 500 MeV of energy in the laboratory system, so we don't have enough. The situation is more promising for the fragment in the light region of fission. Reference [6] gives an overall average charge distribution for light fragments. The distribution peaks at a charge of 21, as we see in Fig. 8, and it has fallen to about 10% of its central value at charge 26. If we consider a light fragment, say a krypton, with about 100 MeV of kinetic energy from fission and most probable charge of +21 accelerating down to ground from the 10 MeV terminal we get something over 300 MeV of kinetic energy. But at the +26 tail of the charge distribution we would get another 60 MeV more of energy. Now this is enough kinetic energy to get over the Coulomb barrier of targets in the same region of Z. That is, for krypton on krypton there is a Coulomb barrier of roughly 150 MeV, and this means an energy in the laboratory system of around 300 MeV to surpass. We might well worry that there would be too little discrimination and that we would have all kinds of particles coming down the pipe. There could be some magnetic discrimination at the high voltage terminal, so that we selected out only those fragments that had attained an unusually high charge through Auger cascades. There could also be magnetic analysis at the end of the machine. Of course, one could envisage other types of accelerators, too, but we limit ourselves here to the above example with an existing type of accelerator.

There is much complementary information yet to be learned from high resolution spectroscopy on the beta-decay of fission products. Promising studies are in progress, some involving isotope separators, some quick chemical separations especially of noble gases, and some no chemical separation at all but K X-ray coincidence to identify the element. With solid-state detectors there is real promise in these studies, despite the high decay energies and consequent complexity of decay schemes.

Before closing let me note a few of the qualitatively new phenomena that could be studied with heavier ion accelerators. Transfer of neutron pairs between grazing heavy nuclei could be an important probe of pairing phenomena, but both target and projectile need to lie away from the light nuclei, where $Z \approx N$, since for light nuclei pairing of unlike nucleons complicates and competes with simple pairing of the like nucleons. Experiments on the nuclear Josephson effect, first proposed [13] by Goldanskii, can best be done with nuclei for which neutrons and protons are filling different shells. The bombardment of deformed target nuclei by deformed projectile nuclei hold the possibility of reaction below the calculated barrier for spheres, since there is some probability of collisions with the long axes touching. Such reactions might most nearly correspond to inverse fission. The fact that negative Q-values of magnitude comparable to the Coulomb barrier are the rule for very heavy ion reactions may make for qualitatively new results from scattering or compound nucleus reactions. Scattering of deformed nuclei near the angle at which the cross-section drops below the Rutherford value should exhibit large tensor polarization effects. Also the population patterns of rotational states in scattering or simple transfer reactions will contain much fundamental information about the shape of the nuclear optical potential, the Nilsson functions of the transferred nucleons or clusters and so on. The large centre-of-mass motion in these very heavy ion reactions will mean large Doppler shifts in the gamma-rays emitted by fragments, and these effects can be used in many ways for lifetime measurements down to the picosecond region and for efficient studies of angular distributions of reaction products.

The studying of fission fragments in flight is useful for the fundamental information it may give, not only on nuclear properties but on atomic properties of highly charged species. Watson and I [14] have measured shifts of about 1 kV in K-binding energies of fission fragments in flight, due to the high charges. Our Hartree-Fock atomic calculations also show a tremendous shrinkage in ionic size in going up to +20 ions. Certainly internal conversion coefficients, Auger coefficients, collision radii and so on are greatly modified at these high charges. Scattering and charge exchange cross-sections are important to design of heavy ion accelerators and probably also in aspects of astrophysics.

There is good work for many minds and hands here. If we are to move ahead rapidly and efficiently, we must learn better to pool our resources and efforts. None among us will have laboratories so richly endowed that we can conceive the tasks in narrowly competitive terms. Let us see more centres like Copenhagen and Trieste where scientists from the entire world are welcome to come, to learn, and to work together.

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DISCUSSION

V.F. WEISSKOPF: In trying to understand a little better the connection between the deformation of the nucleus and ionization, one is obviously dealing with an interesting mixture between atomic physics and nuclear physics. I don't know where one physics begins and the other ends.

J.O. RASMUSSEN: The atomic and nuclear physics are very much mixed up in the experiment but there is a close inner play between them in this class of experimentation on fission fragments. The reason one sees such large probability of K-vacancy for the deformed fragments, I think follows from the fact that most of these fragments decay through a cascade of the rotational steps. The rotational transitions in this region are of slightly larger energy than the K-binding, they have rather high K-conversion causations and thus lead to many K-vacancies. If nature had been a little bit different and the rotational spacings were below the K-binding energy then one would not have had as many K-vacancies.

V.F. WEISSKOPF: Are the life-time rotational states longer than the lifetimes of the X-ray?

J.O. RASMUSSEN: The X-rays emit seven orders of magnitude faster than a nuclear transition.

REACTION ELECTRON- AND GAMMA-SPECTROSCOPY

Present status and prospects of its future development

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Abstract

REACTION ELECTRON- AND GAMMA-SPECTROSCOPY: PRESENT STATUS AND PROSPECTS OF ITS FUTURE DEVELOPMENT. Experimental procedures and the theoretical background in reaction electron- and gamma-spectroscopy are reviewed and some examples are presented. As an application of the technique in rather low energy reactions, the gamma-ray spectroscopy of proton capture where the nuclear reaction proceeds via an isobaric analogue state is described and its physical meaning is illustrated. Timing methods and several topics are mentioned as new research fields of this technique.

1. INTRODUCTION

Beta- and gamma-spectroscopy has played an important role in the understanding of the atomic nucleus. In fact, the shell model and the collective model were founded mainly on the basis of the information on nuclear properties of low-lying excited levels provided by this experimental procedure. The levels here are excited by beta decay from the parent radioactive nucleus produced by various types of nuclear reactions, and the de-exciting radiations are studied by using appropriate experimental techniques. Though this procedure has proved very useful, it has several obvious shortcomings. The Q_β value, that is, the energy difference between the parent and the daughter nucleus, hinders us from investigating levels with excitation energies above this Q_β value. The very strict selection rules of beta decay allow transitions only to the levels with restricted spin values. Radioactive isotopes with lifetimes shorter than about one hour have not been studied in detail owing to the technical difficulties. Thus the excited levels which have been investigated so far are limited to those with energy under several MeV in the neighbourhood of the beta stability line.

Instead of this procedure, we can make use of gamma rays and internal conversion electrons from nuclear reaction. Measurements of these radiations are here called reaction electron- and gamma-spectroscopy. It is this experimental technique which I am now going to discuss. The lifetime measurements and particle-gamma and gamma-gamma angular correlation experiments used in the experiments are very similar to those in classical radioactive beta-, electron- and gamma-spectroscopy. (Hereafter we call this method radioisotope electron- and gamma-spectroscopy). Up to the present, experiments have mostly been done with gamma-rays from direct reactions on light and medium nuclei [1].

It has recently been found that reaction electron- and gamma spectroscopy with gamma rays from compound reactions provides very fruitful information on nuclear structure, to the same extent as the radioisotope electron- and gamma-spectroscopy provided in the past. The processes involved in this recently developed technique consist of three steps: the first is the formation of a compound nucleus produced by bombardment of a target nucleus with an accelerated particle: the second is the successive emission of some kind of particle from the compound nucleus; and the third is the emission of gamma rays from the residual excited nucleus. The gamma rays and conversion electrons are the object of the studies. The first successful use of this reaction electron- and gamma-spectroscopy was made by Morinaga and Gugelot with (α, xn) reactions [2]. As 52-MeV alpha-particles bring the target nucleus a high orbital angular momentum, the compound nucleus has a statistical population favourable for high spin states. The neutrons subsequently being evaporated with low angular momentum, the residual nucleus of high excitation energy will have the same trend of high spin population as the parent compound nucleus. Thus the gamma-rays de-exciting from this residual nucleus flow mainly along a series of levels which have the lowest excitation energy for a given spin value. By this principle, workers have found members of the ground band up to spin of 10^+ in many deformed nuclei. Many workers have investigated excited level systems in nuclei not only in the deformed region [3-8] but also in the vibrational region [9-11] by using gamma-ray spectroscopy and internal conversion-electron spectroscopy.

In early 1965, Sakai, Yamazaki and Ejiri recognized the nuclear alignment of the compound nucleus on the plane perpendicular to the incident beam direction, because the incident particle transfers the angular

TABLE I. CORRESPONDENCE RELATIONS BETWEEN REACTION AND RADIOISOTOPE ELECTRON- AND GAMMA-SPECTROSCOPY

	Radioisotope electron- and gamma-spectroscopy	Reaction electron- and gamma-spectroscopy
Parent nucleus	Radioactive nuclide	Compound nucleus (intermediate nucleus)
Decay mode of parent nucleus	Beta decay and alpha decay	Particle emission (in general, neutron)
Decay mode of residual nucleus	Gamma decay	Gamma decay
Limitation for excitation	Q_β and selection rules of beta decay and alpha decay	Rather weak limit
Angular distribution of gamma rays	Isotropy	Anisotropy
Use of accelerator in measurement	No	Yes

momentum with $m_z = 0$ to the target nucleus [9]. As this alignment is scarcely disturbed by the evaporation of slow neutrons with low orbital angular momenta, the residual nucleus is aligned so that the gamma ray de-exciting from it has an anisotropic angular distribution. Already, in Ref. [9], the following statement appeared: "The anomalous K/L ratio observed in certain transitions may be associated with the aligned compound nucleus [see Ref. [12]]. The investigation of the electron angular distribution might open a new field of nuclear spectroscopy and provide valuable information on the nuclear structure and on the reaction mechanism". In fact, measurements of gamma-ray and conversion-electron angular distributions in reaction electron- and gamma-spectroscopy are just the counterpart of those of gamma-gamma and gamma-electron angular correlations in the radioisotope electron- and gamma-spectroscopy, because, in the latter case, the detection of the first gamma-ray plays only the role of selecting an aligned state. The correspondence relations between the reaction and the radioisotope electron- and gamma-spectroscopy are given in Table I.

In the following, we are going to discuss successively the experimental techniques (section 2), the theoretical considerations (section 3), typical examples of reaction electron- and gamma-spectroscopy (section 4), proton capture gamma experiments (section 5) and the prospects of future development of this method (section 6).

2. EXPERIMENTAL TECHNIQUES

Reaction electron- and gamma-spectroscopy has been performed in many institutions throughout the world. As an example, I shall describe the experimental arrangement at INS. The special feature of our experiments at the Institute lies in the use of a cyclotron. Beams of alpha particles and protons from the Tokyo cyclotron are used to bombard the target. Experiments using a cyclotron have been considered to have disadvantages such as instability of beam condition and high background in comparison with those obtained using a tandem accelerator. These inconveniences are avoided by the following simple technique. Two beam systems used at INS are shown in Figs 1 and 2. The first is the one used

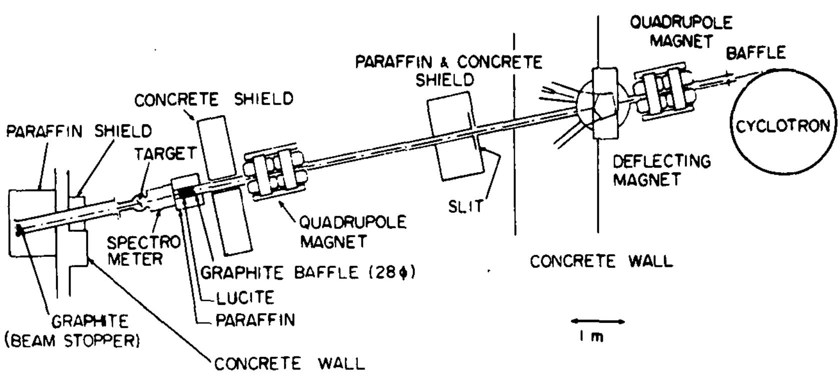

FIG. 1. Schematic diagram of the beam system with the S-RACE spectrometer.

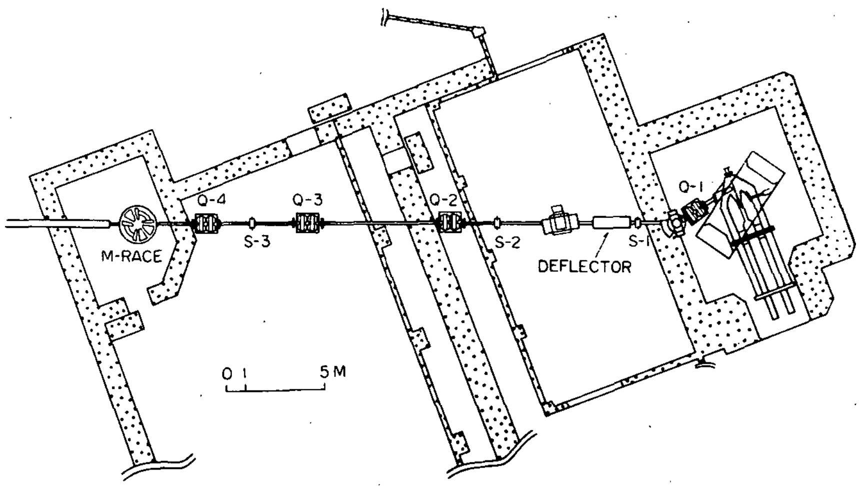

FIG. 2. Schematic diagram of the beam system with the M-RACE spectrometer.

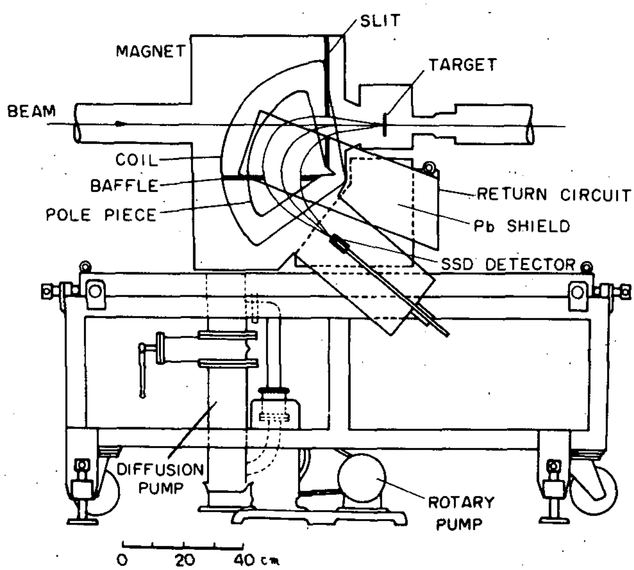

FIG. 3. Side view of the S-RACE spectrometer.

in 1964 where a single-gap reaction conversion-electron spectrometer (S-RACE) was installed for measuring internal conversion electrons [13]. The second is the one used in 1966 in which the duct was extended to a gamma cave situated 30m from the cyclotron and a multi-gap reaction conversion-electron spectrometer (M-RACE) was attached [14]. The experimental procedure overcoming the drawbacks mentioned above is to cut off the off-axis beam intensity at the first slit by 10% and to have this slit serve as a virtual source for an independent optical system composed of

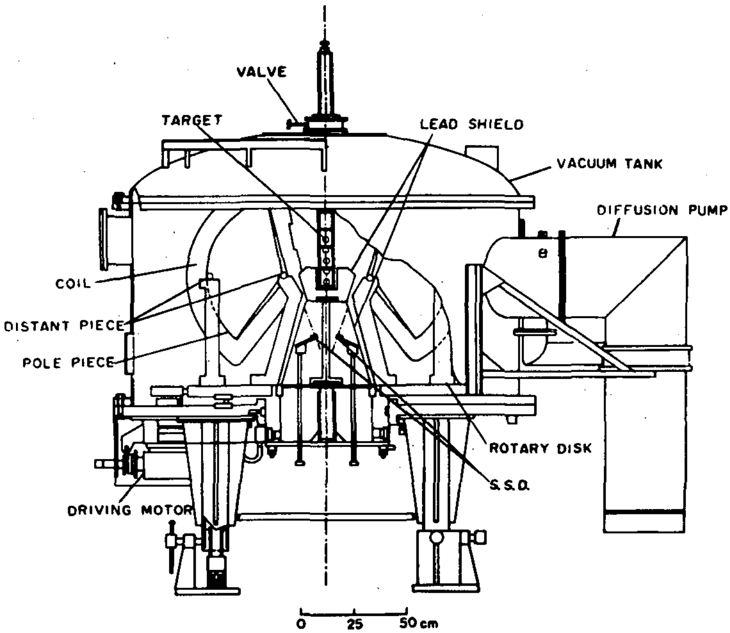

FIG. 4. Side view of the M-RACE spectrometer.

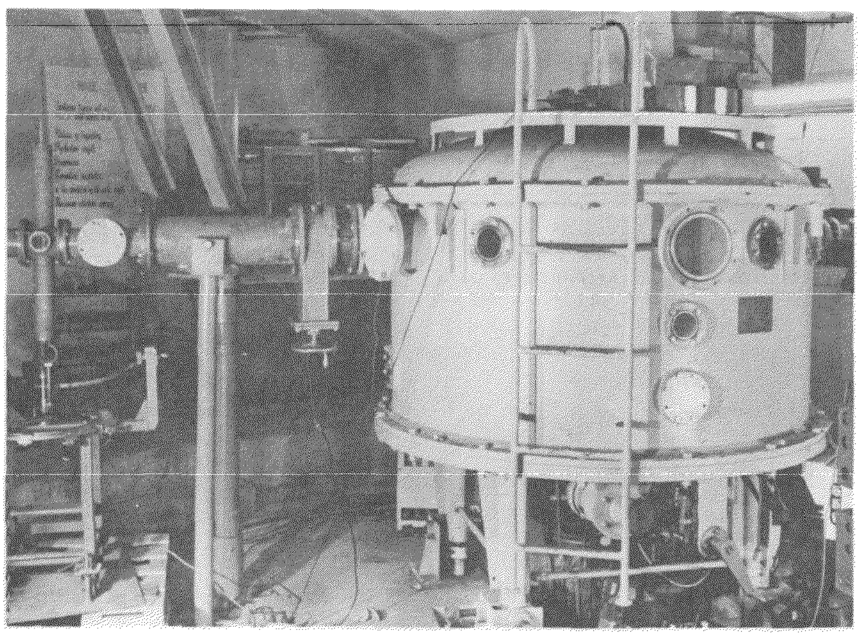

FIG. 5. Photograph of the M-RACE spectrometer.

one pair (in Fig.1) or two pairs (in Fig.2) of quadrupole magnets. With this technique, we have a fixed beam spot on the target position which is the image of the optical system and independent of fluctuation of primary beam from the cyclotron.

The RACE spectrometer is a sectortype double-focusing spectrometer and the M-RACE enables us to measure the electron intensities in five directions. Schematic side views of S-RACE and M-RACE are presented in Figs 3 and 4. A photograph of the M-RACE is shown in Fig.5. As is well known, a double-focusing spectrometer focuses electrons with momentum spread of 8% on a focal plane, so that we can have a spectrometer with multi-gap and multi-detector array very well suited for reaction electron- and gamma-spectroscopy. In this respect this spectrometer is advantageous over the orange-type spectrometer. However, we have so far used one Si(Li) detector for each gap. The characteristics of the spectrometer are 0.4% momentum resolution with a transmission of 0.3% for each sector. The targets were prepared with deposits of powder on mylar membranes of 6 μm thickness. Sometimes, self-supporting metal targets were made by an electro-plating or evaporation method. The thickness of the targets was in general 1 - 4 mg/cm^2.

3. THEORETICAL CONSIDERATIONS [15-19]

In this section, I shall discuss the theoretical considerations for (p, 2nγ) reactions in terms of the statistical model.[1] The process of the reactions is schematically illustrated in Fig.6 [10].

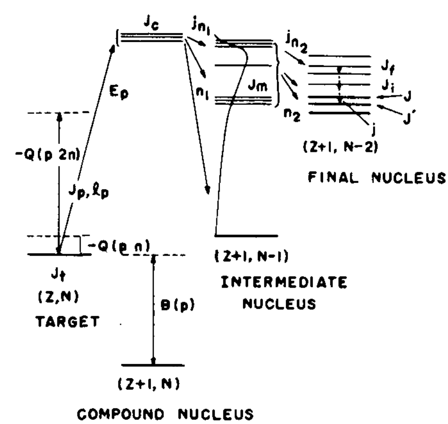

FIG.6. Scheme of the (p, 2n γ) reaction.

3.1. Relative yield of gamma-rays [9]

The mechanism of (p, 2n) reactions can be divided into the following four processes: (i) formation of compound states by proton bombardment, (ii) formation of intermediate states after the evaporation of one neutron

[1] I shall discuss only the case where the residual nucleus is an even-even nucleus, which we are interested in.

from the compound states, (iii) evaporation of another neutron from the intermediate states, producing excited states in the final nucleus, (iv) gamma emission from the latter states. The cross-section for process (i) is written as follows:

$$\sigma(J_c, \pi_c, J_t, \pi_t) = \pi\lambda\!\!\!^{-2} \sum_{\ell=|J_c-S|}^{J_c+S} \sum_{S=|J_t-\frac{1}{2}|}^{J_t+\frac{1}{2}} \frac{2J_c+1}{2(2J_t+1)} T_\ell(E_p)\, \tfrac{1}{2}\{1+(-)^\ell \pi_c\pi_t\} \quad (1)$$

where $\lambda\!\!\!^{-}$ is the de Broglie wave-length of the incident particle and J_c, π_c and J_t, π_t are the spin and parity of the compound state and those of the

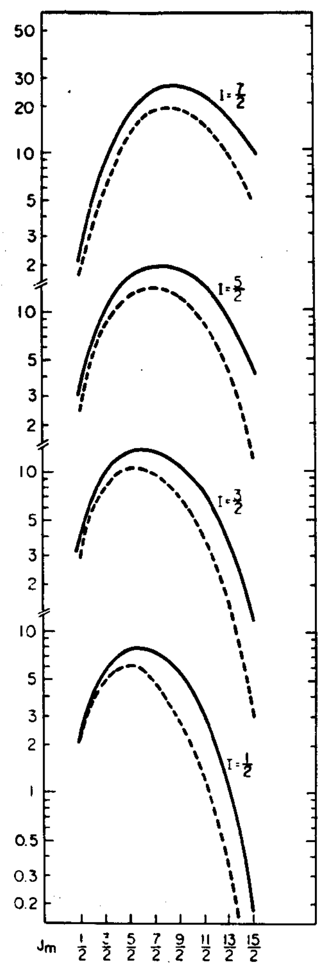

FIG. 7. Relative spin distributions of the intermediate states for the target of atomic number Z = 50 and spins I = 1/2, 3/2, 5/2 and 7/2. Solid and broken lines stand for the distributions at incident energies of 14 and 12 MeV, respectively.

target nucleus, respectively. The quantity $T_\ell(E_p)$ stands for the transmission coefficient of incident protons with orbital angular momentum ℓ. The cross-section for process (ii) is given by

$$\sigma(J_m,\pi_m,J_t,\pi_t)=\sum_{J_c\pi_c}\sigma(J_c,\pi_c,J_t,\pi_t)K(J_m,\pi_m,J_c,\pi_c)\left\{\sum_{J_m\pi_m}K(J_m,\pi_m,J_c,\pi_c)\right\}^{-1} \qquad (2)$$

where

$$K(J_m,\pi_m,J_c,\pi_c)=\rho(J_m)\sum_{S|J_m-\frac{1}{2}|}^{J_m+\frac{1}{2}}\ \sum_{\ell=|J_c-S|}^{J_c+S} T_\ell(E_{n_1})\tfrac{1}{2}\{1+(-)^\ell\pi_c\pi_m\} \qquad (3)$$

and

$$\rho(J_m)=k_1(2J_m+1)\exp\{-J_m(J_m+1)/2\sigma^2\} \qquad (4)$$

The level density in the intermediate nucleus is designated as $\rho(J_m)$ and the parameter σ is proportional to the moment of inertia and the temperature of the nucleus. Fig.7 shows the relative spin distributions of the inter-

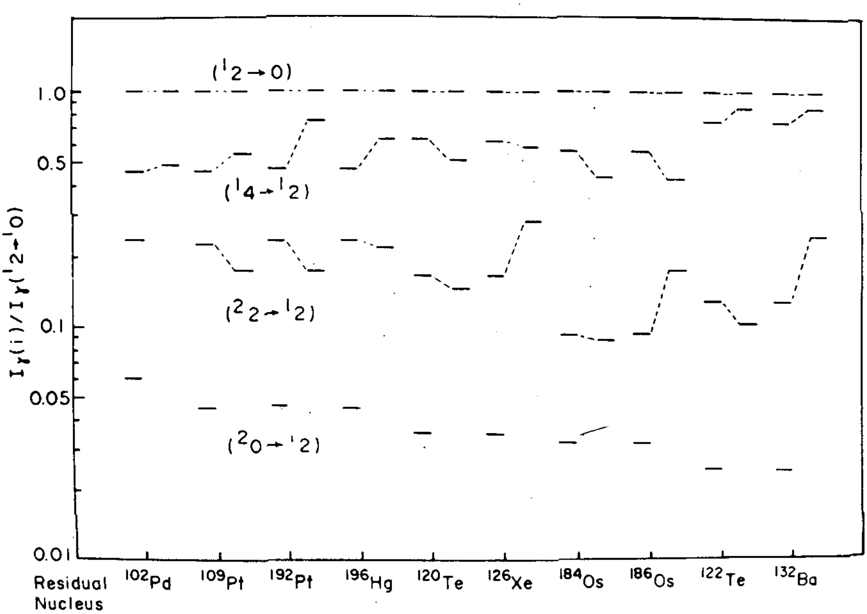

FIG.8. Intensities of gamma transitions in the final nuclei for $E_p \simeq 14$ MeV. Intensities are normalized with those of the gamma $^12^+ \to {}^10^+$. For each nucleus, the theoretical values, illustrated in the left column, are compared with the experimental ones illustrated in the right column. The dotted lines connect the calculated intensities of $^12^+\to{}^10^+$, $^14^+\to{}^12^+$ and $^22^+\to{}^12^+$ transitions with the observed ones.

mediate states for $Z = 50$, where the transmission coefficient $T_\ell(E_{n_1})$ was calculated for neutrons of 1 MeV. For process (iii), the procedure used in the calculation of process (ii) can be repeated with $\sigma(J_c, \pi_c, J_t, \pi_t)$ replaced by $\sigma(J_m, \pi_m, J_t, \pi_t)$, which in turn is replaced by $\sigma(J_f, \pi_f, J_t, \pi_t)$. The level density $\rho(J_m)$ should then be replaced by $\rho(J_f)$ which is the level density in the residual nucleus. However, the states populated in the residual nucleus have low excitation energies so that the expression $\rho(J_f)$ may not be valid. We must therefore make assumptions about the levels in question. The results of the calculation are shown in Figs 8 and 9 for vibrational nuclei [9] and rotational nuclei [3], respectively, with appropriate assumptions. The predicted values agree well with the observed gamma intensities. This implies that the reaction mechanism can be fairly well interpreted by means of the statistical model. The consideration of the relative yield of gamma rays has proved to be a powerful tool for assignment of phonon transitions in the vibrational region and of ground-band transitions in the deformed region. This fact in turn enables us to locate the phonon states and the members of the ground band. Other important information on the spin value of the states can be obtained from the excitation curves of the relative yield on the associated gamma-rays. The excitation of high spin states is more favourable with higher bombarding energy than that for low spin states. With this information, we can determine the 4^+ member of the two-phonon triplet [9]. A similar theoretical framework can be made for alpha-induced reactions.

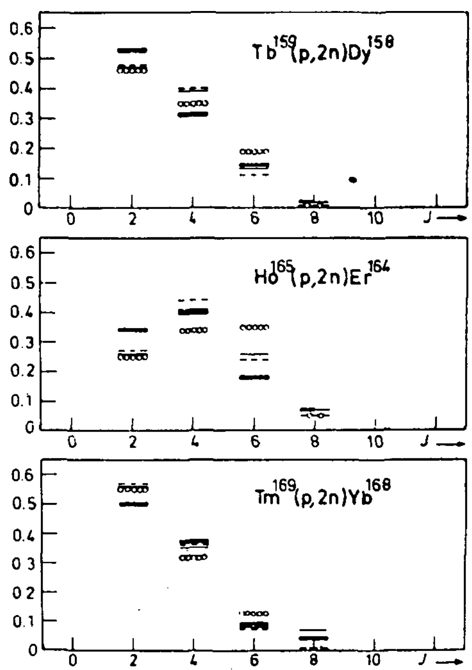

FIG.9. Relative populations of rotational states in the ground bands of ^{158}Dy, ^{164}Er and ^{168}Yb (thick bar). Cascades from the higher rotational levels have in each case been subtracted. The marks ----, —— and oooo are calculated relative populations with various assumptions (see Ref. [3]).

3.2. Angular distribution [10]

Angular distribution functions of gamma-rays from reactions illustrated in Fig.6 can be expressed as

$$W(\theta) \propto \sum_{\nu} a_{\nu} P_{\nu}(\cos\theta) \tag{5}$$

where P_{ν} is the Legendre polynomial of order ν. The angular distribution of the gamma ray $J \rightarrow J'$ can be calculated with the following a_{ν} value:

$$\begin{aligned} a_{\nu}(J \rightarrow J') = k_2 \sum_{J_c j_p} & (2J_c + 1)\, \eta_{\nu}(j_p J_t, J_c) T_{j_p}(E_p) \\ \times & \sum_{J_m j_{n_1}} I_{\nu}(j_{n_1} J_c J_m) \rho(J_m) T_{j_{n_1}}(E_{n_1}) \Big/ \sum_{J_m j_{n_1}} \rho(J_m) T_{j_{n_1}}(E_{n_1}) \\ \times & \sum_{J_f j_{n_2}} I_{\nu}(j_{n_2} J_m J_f) T_{j_{n_2}}(E_{n_2}) \Big/ \sum_{J_f j_{n_2}} T_{j_{n_2}}(E_{n_2}) \\ \times & B_{\nu}(J_f \rightarrow J) F_{\nu}(j J', J) \end{aligned} \tag{6}$$

where

$$\eta_{\nu}(j J, J') = (2j+1)(2J'+1)^{\frac{1}{2}} (-)^{J'-J+\frac{1}{2}} C(j j \nu; \tfrac{1}{2} -\tfrac{1}{2}) \overline{W}(j j J' J'; \nu J) \tag{7}$$

$$F_{\nu}(j J', J) = (2j+1)(2J+1)^{\frac{1}{2}} (-)^{J-J'+1} C(j j \nu; 1 -1) \overline{W}(j j J J; \nu J') \tag{8}$$

$$I_{\nu}(j J J') = [(2j+1)(2J'+1)]^{\frac{1}{2}} (-)^{J+J'-j-\nu} \overline{W}(J J J' J'; \nu j) \tag{9}$$

and

$$B_{\nu}(J_f \rightarrow J) = \sum_{J_f \rightarrow J} \prod_{i=0}^{i=n} I_{\nu}(j_i\, J_i\, J_{i+1}) \tag{10}$$

The coefficient $B_{\nu}(J_f \rightarrow J)$ for the effect of the cascade contributions is summed over all the de-excitation processes from the state J_f to the state J through the transitions $J_f = J_0 \rightarrow J_1 \rightarrow J_2 \rightarrow \ldots J_i \rightarrow J_{i+1} \rightarrow \ldots J_n \rightarrow J_{n+1} = J$, and j_i is the multipole order of the gamma transition from the state J_i to the state J_{i+1}. Here, as is allowed in the statistical continuum theory, the number of compound states and intermediate states is assumed to be so large that all the interference terms from the states of different spin and parity, and from different partial waves of the incident protons and the emitted neutrons, cancel.

In the intra ground band transitions, namely, $J_i \to J_{i+1} = J_i \to j = J_i \to 2$, the last term $B_\nu F_\nu$ reduces to

$$B_\nu(J_f \to J)F_\nu(j\,J',\,J) = \prod_{i=0}^{i=n-1} I_\nu(j\,J_i\;J_i\;-j)F_\nu(j\,J,\,J_n) = F_\nu(j\,J_1\,,J_f) \tag{11}$$

where we use the relation between the Racah coefficients

$$\overline{W}(j\,j\,J\,J;\;\nu\,J-j)\,\overline{W}(J+j\;J+j\;J\,J;\;\nu j) = (2J+1)^{-1}\;\overline{W}(j\,j\,J+j\;J+j;\;\nu\,J) \tag{12}$$

Therefore, we obtain

$$a_\nu(J \to J') = \sum_{J_f} a'_\nu(J_f - J_1), \tag{13}$$

where the quantity $a'_\nu\;(J_f \to J)$ is the coefficient of the angular distribution of the transition $J_f \to J$ without including the effect of the cascade contribution from the higher state, and is obtained by setting $B_\nu = 1$. Finally, we obtain the angular distribution function $W(\theta)$ for intra ground band transitions $J \to J-2$ as follows:

$$W(\theta) = 1 + A_2P_2(\cos\theta) + A_4P_4(\cos\theta) \tag{14}$$

with

$$A_\nu(J \to J-2) = a_\nu(J \to J-2)/a_0(J \to J-2) = \sum_{I=J}^{N} \rho'_I\,A'_\nu(I \to I-2) \tag{15}$$

where we consider the series of the ground band 0^+, 2^+, 4^+, ..., N^+ and $\rho'_I = a'_0\,(I \to I-2)/a_0(J \to J-2)$. For conversion electrons, $W(\theta)$ is given by

$$W(\theta) = 1 + b_2A_2P_2(\cos\theta) + b_4A_4P_4(\cos\theta) \tag{16}$$

where the quantity b_ν is the particle parameter. A typical example of the calculation for the ground band up to $N = 8$ spin with excitation energies $E_I = 20I(I+1)$keV is reproduced in Fig.10 [10].

In the vibrational region, we can easily verify the following equations:

$$a_\nu({}^12^+ \to {}^10^+) = a_\nu({}^14^+ \to {}^12^+) + a_\nu({}^22^+ \to {}^12^+) + a'_\nu({}^12^+ \to {}^10^+) \tag{17}$$

and

$$A_\nu({}^12^+ \to {}^10^+) = \rho_{{}^14^+}A_\nu({}^14^+ \to {}^12^+) + \rho_{{}^22^+}A_\nu({}^22^+ \to {}^12^+) + \rho'_{{}^12^+}A'_\nu({}^12^+ \to {}^10^+), \tag{18}$$

where $\rho_I(I \to I') = a_0(I \to I')/a_0({}^12^+ \to {}^10^+)$ and the ${}^22^+ \to {}^12^+$ transition is assumed to be of pure E2 character and the relation for the special case $J_n = J = j$:

$$(2J+1)\overline{W}(j\,j\,J\,J;\,\nu\,J')\overline{W}(J_n\;J_n\;J\,J;\;\nu\,J)$$
$$= \overline{W}(j\,j\,J_n\;J_n;\;\nu\,J) \tag{19}$$

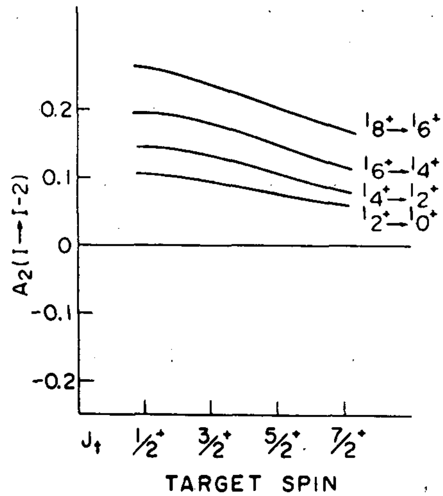

FIG.10. Coefficient A_2 of the angular distribution of gamma rays from rotational levels excited by the (p, 2n) reaction including the contribution of the cascade transitions from the higher excited state.

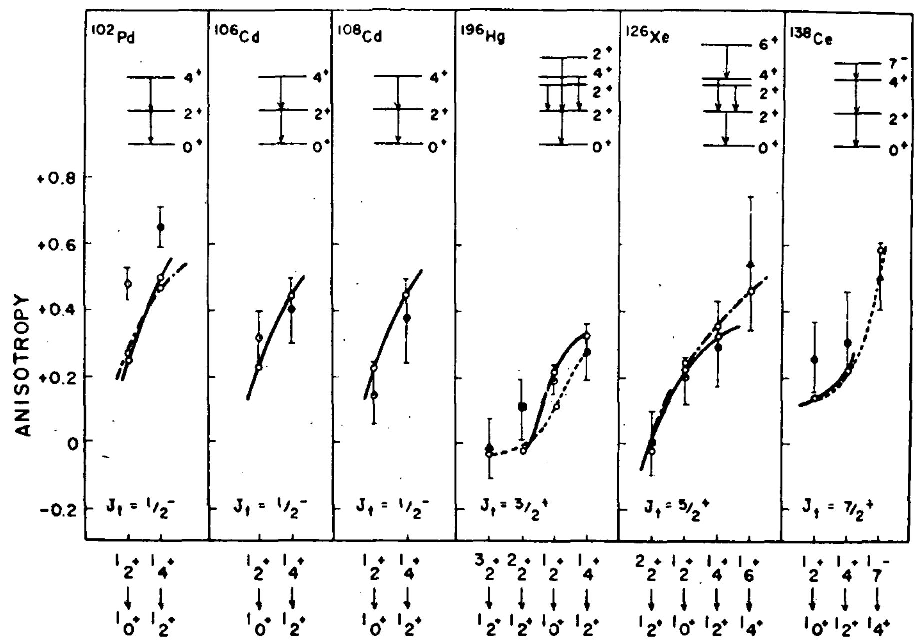

FIG.11. Anisotropy of the conversion electrons from vibrational levels produced in the (p, 2n) reactions on vibrational nuclei. Black circles, double circles, black squares and black triangles refer to the experimental points for the transitions $^{1}4^{+}\rightarrow{}^{1}2^{+}$, $^{1}2^{+}\rightarrow{}^{1}0^{+}$, $^{2}2^{+}\rightarrow{}^{1}2^{+}$ and others, respectively. The inserts show the decay schemes of the observed conversion electrons. Solid lines, dot- and dash-lines and broken lines are the calculated anisotropies (white circles) for the residual excited states of $^{1}0^{+}$, $^{1}2^{+}$, $^{2}2^{+}$, $^{1}4^{+}$; $^{1}0^{+}$, $^{1}2^{+}$, $^{2}2^{+}$, $^{1}4^{+}$, $^{1}6^{+}$ and those presented in the inserts, respectively.

is used. Here we take into account the cascade contributions to the $^{1}2^{+}$ state only from the two-phonon $^{1}4^{+}$ and $^{2}2^{+}$ states. The results calculated on the assumption of the residual states $^{1}0^{+}$, $^{1}2^{+}$, $^{2}2^{+}$ and $^{1}4^{+}$ are plotted as a function of the target spin in Fig.11 [10] and compared with the experimental one. The agreement is satisfactory.

4. TYPICAL EXAMPLES

In this section I describe several typical examples of reaction electron- and gamma-spectroscopy in connection with the experimental trend of excited level systems in even-even nuclei.

4.1. Deformed region

Many workers [3-8] have studied the excited level systems of even-even deformed nuclei with reaction electron- and gamma-spectroscopy since the work of Morinaga and Gugelot [2]. Especially, Stephens et al.[7] used heavy-ion nuclear reactions to produce high spin states in a compound nucleus, taking advantage of the fact that a heavy ion brings large orbital angular momentum into the target. They observed de-excitation of the ground band up to spin 20 with a single wedge-gap electron spectrometer. The highest observed spin value in the band will increase with use of a heavier ion. A test of the presence of the top of the ground band suggested by Mottelson and Valatin [20] will be performed in the very near future. The rotational spacings of high spin states provide important information about the nuclear shape in highly excited states. This type of spectroscopy also has proved a good method for obtaining the beta and gamma bands in deformed nuclei. As typical examples, level systems of $^{152}_{62}$Sm and $^{164}_{66}$Er investigated by the Copenhagen group are shown in Fig.12 [4] and Fig.13 [21]. Recently, level systems in Gd isotopes were investigated with (α, xn) reactions on Sm isotopes [22]. The results are presented in Fig.14.

4.2. Transition region

This method was also applied to nuclei in the transition region. It turned out that ^{150}Sm and ^{152}Gd [6] and Os and Pt [5] have a level sequence just like the ground band in the deformed region. Recently, we measured conversion electrons from ^{148}Nd (α, 2n)^{150}Sm, ^{150}Sm(α, 2n) ^{152}Gd and ^{153}Eu(p, 2n)^{152}Gd reactions and the low-lying levels in ^{150}Sm and ^{152}Gd were obtained as shown in Fig.15 [23]. A beta-like band (quasi-beta band) corresponding to the beta band in the deformed nuclei appears in these nuclei and the interband transitions between the levels with the same spin value have a large E0 component just as in the case of deformed nuclei [21]. In Pt isotopes we have a level sequence 2^{+}, 3^{+}, 4^{+} which may correspond to the gamma band in the deformed nuclei. The gamma-like band was termed a quasi-gamma band.

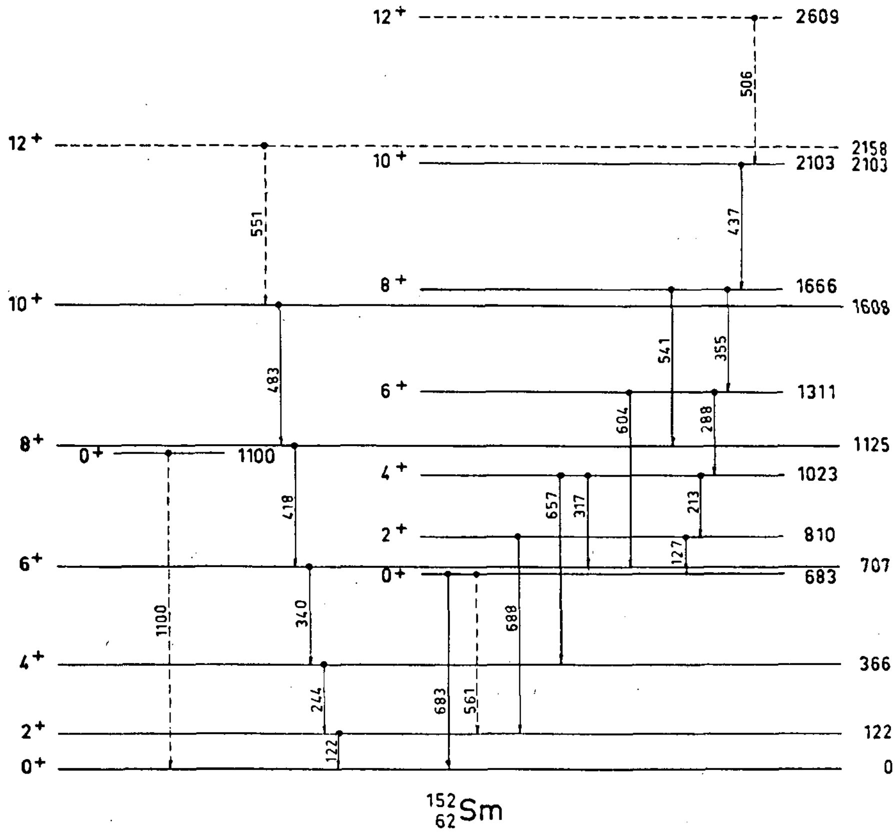

FIG. 12. Energy levels in ^{152}Sm.

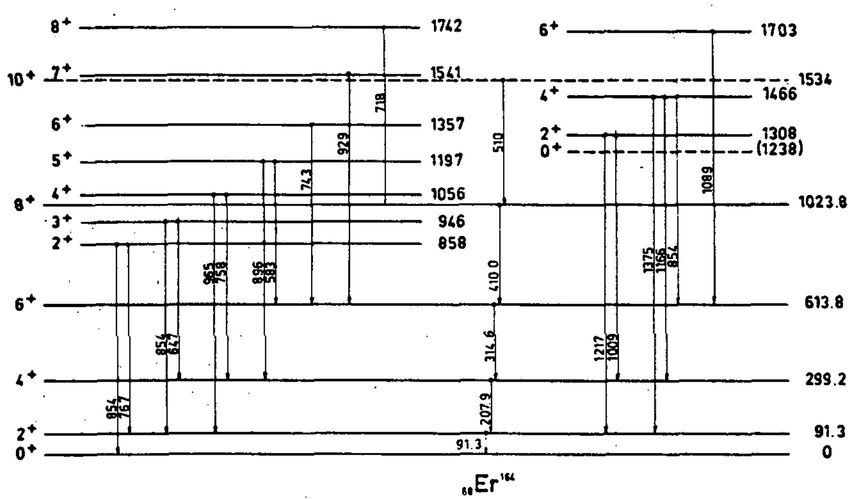

FIG. 13. Energy levels in ^{164}Er.

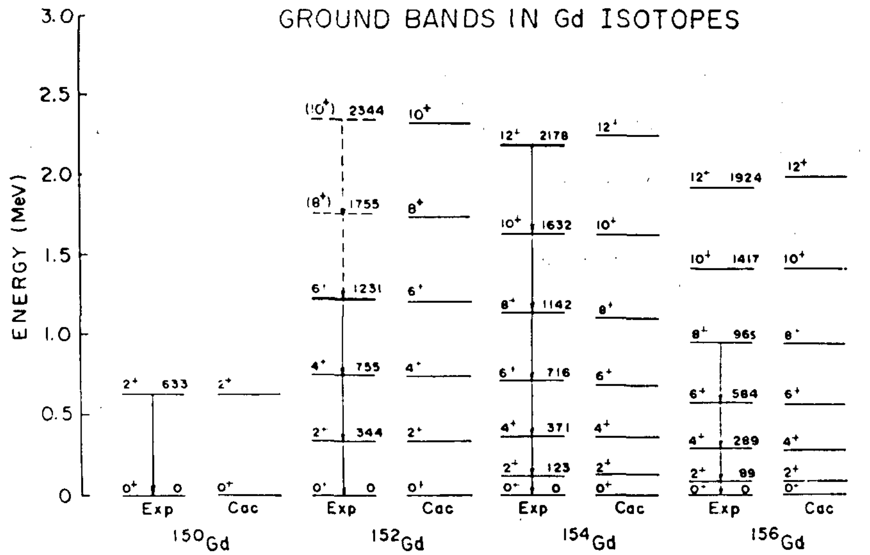

FIG.14. Energy levels in Gd isotopes.

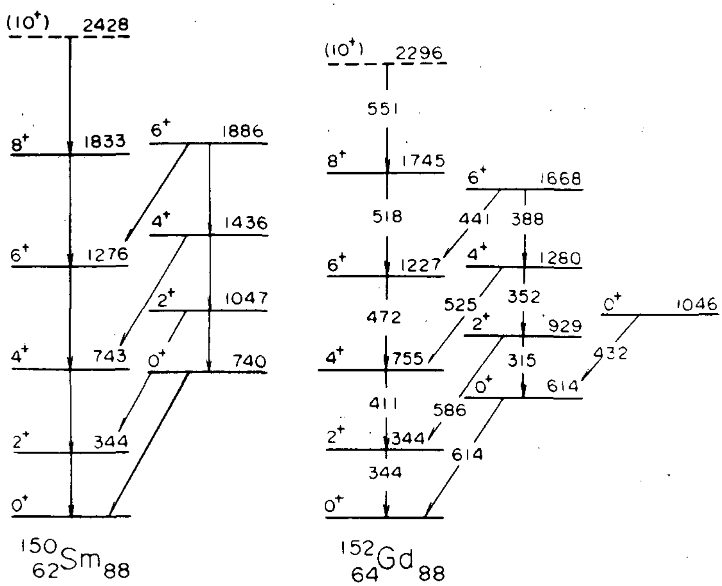

FIG.15. Energy levels in ^{150}Sm and ^{152}Gd.

4.3. Vibrational nuclei

Low-lying excited states in vibrational nuclei have been investigated mainly at INS by means of reaction electron- and gamma-spectroscopy [9-10]. A number of 2^+ and 4^+ doublets of the two-phonon states have been observed. The spin was determined by the excitation curve and the electron angular distribution. Betigeri and Morinaga [11] showed a presence of a band system similar to the ground state rotational band in Te and Xe isotopes, as presented in Fig.16. Recently we performed reaction electron spectroscopy in case of the $^{139}La(p, 2n)$ and $^{141}Pr(p, 2n)$ reactions [24]. The preliminarily proposed level systems for both nuclides are presented in Fig.17. They closely resemble each other and the relative yields and the angular distributions for the corresponding transitions for both nuclei are also similar, as shown in Figs 18 and 19. Though the level systems are very tentative, a beta-like band and a gamma-like band seem to exist in vibrational nuclei.

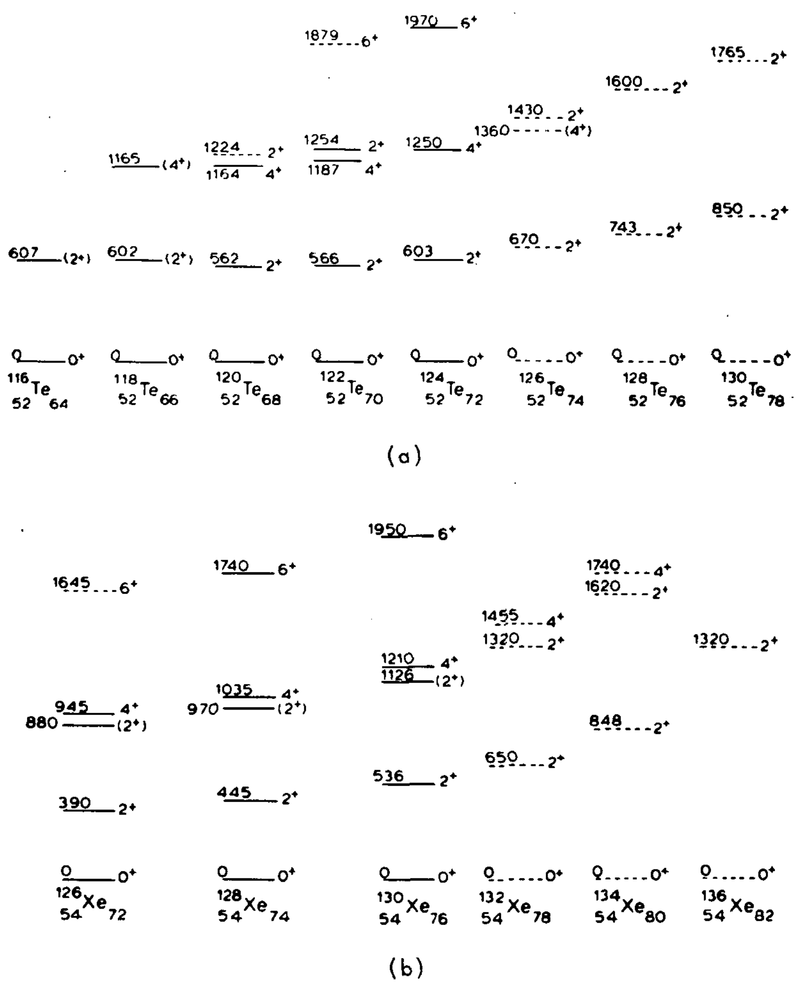

FIG.16. Observed energy levels in (a) even Te and (b) even Xe isotopes. Levels indicated by lines are the ones observed in the experiments of Ref. [11].

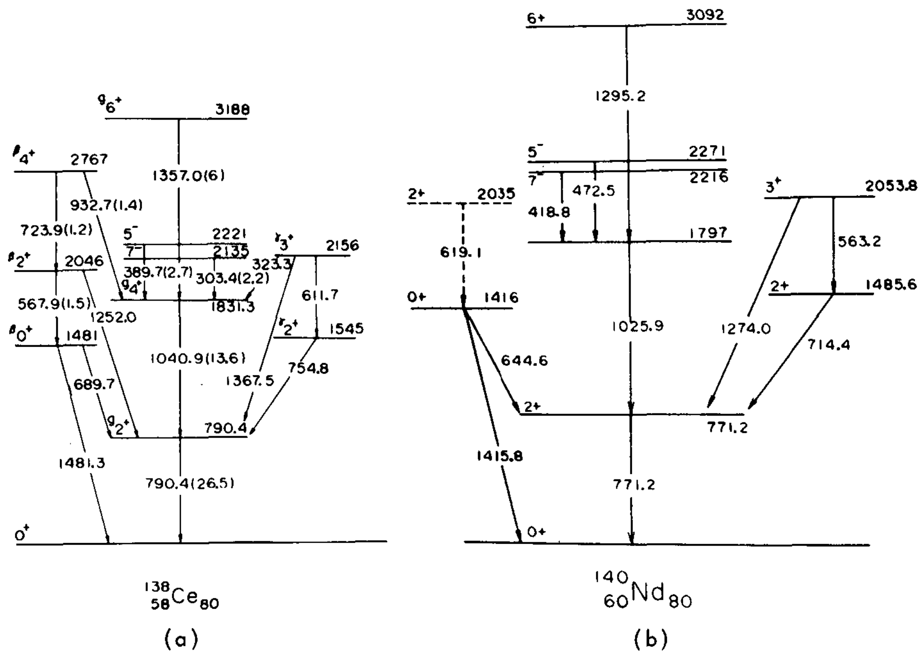

FIG.17. Tentative low-lying excited level systems of (a) ^{138}Ce and (b) ^{140}Nd.

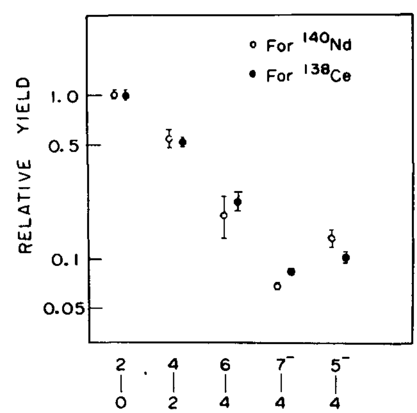

FIG.18. Relative yields of intra quasi-ground band transitions and $7^- \to 4^+$ and $5^- \to 4^+$ transitions.

4.4. Concluding remark

The results described above prove that reaction electron- and gamma-spectroscopy is a powerful tool for studying the low-lying excited levels of nuclei in all nuclear regions. The remarkable feature that the ground band, the beta and the gamma band appearing in deformed nuclei persist in nuclei in the transition and vibrational regions as quasi-ground, quasi-beta and quasi-gamma bands, has been discussed in detail by the present author [25].

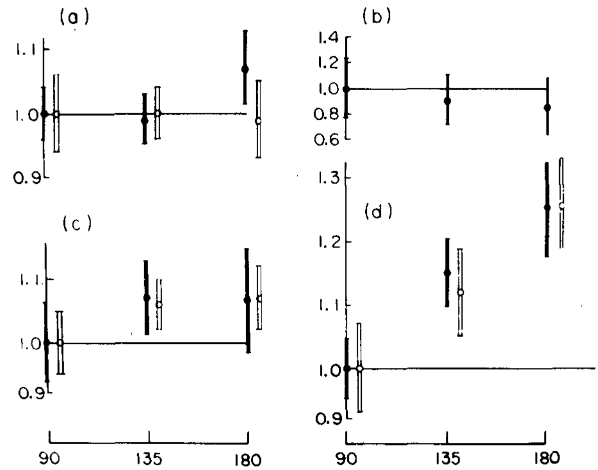

FIG.19. Angular distributions of K conversion electrons.
(a) Black: 303.4-keV $7^- \rightarrow 4^+$ transition in ^{138}Ce
White: 418.8-keV $7^- \rightarrow 4^+$ transition in ^{148}Nd
(b) 323.3-keV transition in ^{138}Ce
(c) Black: 790.4-keV $2^+ \rightarrow 0^+$ transition in ^{138}Ce
White: 771.2-keV $2^+ \rightarrow 0^+$ transition in ^{140}Nd
(d) Black: 1040.9-keV $4^+ \rightarrow 2^+$ transition in ^{158}Ce
White: 1025.9-keV $4^+ \rightarrow 2^+$ transition in ^{140}Nd

5. PROTON CAPTURE GAMMA EXPERIMENT

This type of experiment with nuclei in the s-d shell was extensively performed by the Utrecht group [26]. Important physical meanings of a special case were recently stressed by the author [27]. They are associated with the observation of de-exciting gamma rays from isobaric analogue states produced by proton capture reactions. The main advantage of this experiment lies in the fact that the analogue states are of very simple nature easily amenable to theoretical interpretation. Another advantage is that the information on the low-lying states can be derived from measurements of gamma-rays only in the highest energy region, and clear data can be obtained free from the contributions due to other gamma-rays, even in the case in which the reaction has a small cross-section. Though the gamma-rays in question have, in general, a rather high energy, of the order of 10 MeV, the advent of large-size lithium-drifted germanium detectors makes it feasible to carry out this kind of experiment. It is worth mentioning that the angular distribution of gamma-rays can provide information on the radiation character. With this procedure, we can deduce the relative B(E2) and B(M1) values of the gamma components of the isobaric analogue state. The fact that a small Van de Graaf machine with several MeV is sufficient to do this type of experiment due to large $Q(p, \gamma)$ values is an attractive feature for investigators in developing countries. Among various applications of this procedure to the study of nuclear structure, two simple cases will be discussed.

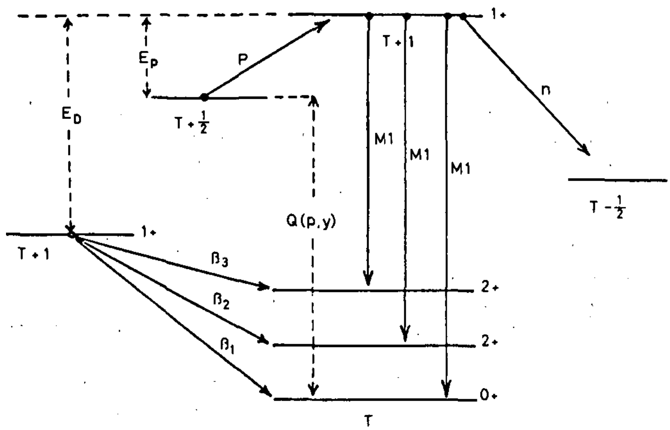

FIG. 20. Energy scheme of proton capture reactions where reaction proceeds via an isobaric analogue state. E_D stands for the Coulomb displacement energy. Relationship between allowed beta transitions of the G-T type and M1 gamma transitions are illustrated.

5.1. Allowed beta transition of G-T type

Studies of nuclear beta decay have provided a large amount of information about the low-lying excited levels because the transition probability depends sensitively on the wave-functions of the levels in question. On the other hand, it is known that a proton with an appropriate energy can produce in the compound nucleus an isobaric analogue state which can correspond to the parent nucleus of beta transition. Since allowed beta transitions of the G-T type from 1^+ states to the ground, to the one-phonon state and to the two-phonon states in vibrational nuclei are successively hindered [28], it is very interesting to know about B(M1) of M1 transitions from the analogue states to the corresponding excited states, because the transition matrix elements of beta and magnetic transitions closely resemble each other. The physical situation is schematically illustrated in Fig.20. Hindrance phenomena correlate closely to the phonon character of the relevant levels. You can see the merit of this kind of experiment from the following example. The spin and parity of ^{56}Mn are 3^+ and the 1^+ state is 111 keV above this ground level so that we cannot study the relative matrix element of G-T type beta decay to the ground state and to the first 2^+ state of the ^{56}Fe daughter nucleus. Then, we may excite the 1^+ state isobaric analogue to the 1^+ state in question by the $^{55}Mn(p, \gamma)^{56}Fe$ reaction with 1.415 MeV protons [29] and observe M1 transitions from this state to relevant states in ^{56}Fe.

It is also recently observed that the beta transition to the first excited 0^+ state is not hindered. Analogous reasoning leads us to expect that the M1 transition to the excited 0^+ state might be as fast as that to the ground

0^+ state. Then such a gamma ray can be observed easily, because, in this case, the intensity reduction caused by the transition energy is very small, in contrast to the enormous reduction factor in beta decay. Thus such an experimental method might be a powerful technique for finding the so-called 'missing' 0^+ member of the two-phonon triplet in the vibrational region.

5.2. Core coupling

The idea of core coupling is one of the most important models in recent nuclear structure theory as Mottelson discussed at the Tokyo Conference on Nuclear Structure [30]. It may present a sensitive test for the anharmonicity of nuclear vibration. The core coupling of a 2^+ phonon with a particle in the j orbit produces $2j+1$ ($j<2$) or 5 ($j>2$) members. Such studies have been done with Coulomb excitation experiments, inelastic scattering experiments, etc. These experiments require stable isotopes as targets. Consequently, a systematic trend over a wide range of isotopes cannot be obtained. The excitation level systems of copper isotopes are considered as examples of this model, but they have only two stable isotopes, namely ^{63}Cu and ^{65}Cu. However, if we excite an isobaric analogue state and study the gamma-rays from this state, we can extend the systematics to unstable isotopes. For example, the low-lying excited state in ^{61}Cu can be studied by ^{60}Ni(p, γ)^{61}Cu reaction. The experimental situation can be seen in Fig.21.

6. PROSPECTS OF FUTURE DEVELOPMENT OF REACTION ELECTRON- AND GAMMA-SPECTROSCOPY

In this section I shall comment on several points of future development of this type of experimental technique.

6.1. Direct reactions

Reaction electron- and gamma-spectroscopy can be applied to direct reactions with medium and heavy nuclei. Though the cross-section for this type of reaction is one order smaller than that for the compound reaction, the electron lines from the levels populated in ^{114}Cd(p, p'γ) have been observed [9] (see Fig.22). This type of experiment will provide a hopeful research field in the near future. Measurements of gamma rays and of inelastically scattered particles are complements each of the other.

6.2. Heavy ion reactions

Heavy ion reactions with heavier and more energetic projectiles can be used to give considerably greater linear and angular momentum to the compound system. The former effect may produce a Doppler effect of gamma rays large enough to enable us to measure the lifetimes of the associated levels, and the latter effect would make it possible to observe de-exciting transitions from very high spin members of the ground band.

Another interest in heavier ions lies in the accessibility to regions of the periodic table that cannot easily be reached with lighter ions. Recently, $^{124-120}Sn(^{40}Ar, 4n)^{160-156}Er$ and $^{128-122}Te(^{40}Ar, 4n)^{164-158}Yb$ were reported, as studies of level systems in the 88-, 90- and 92-neutron number isotopes of Er and Yb [31].

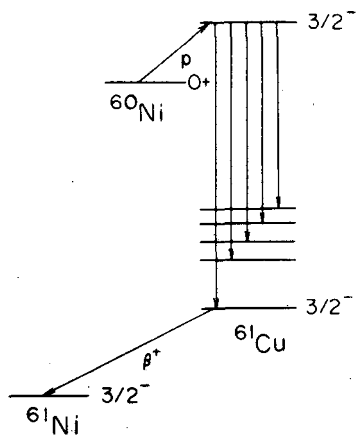

FIG.21. Energy scheme for $^{60}Ni(p, \gamma)^{61}Cu$ reaction. Gamma rays excite core-coupling states in ^{61}Cu.

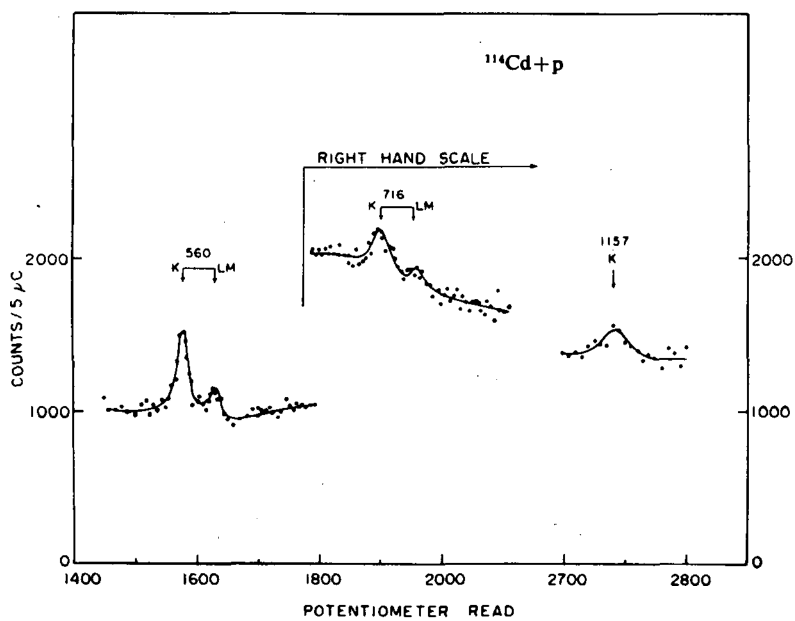

FIG.22. Spectrum of conversion electrons from inelastic scattering on an enriched ^{114}Cd target with 14 MeV protons.

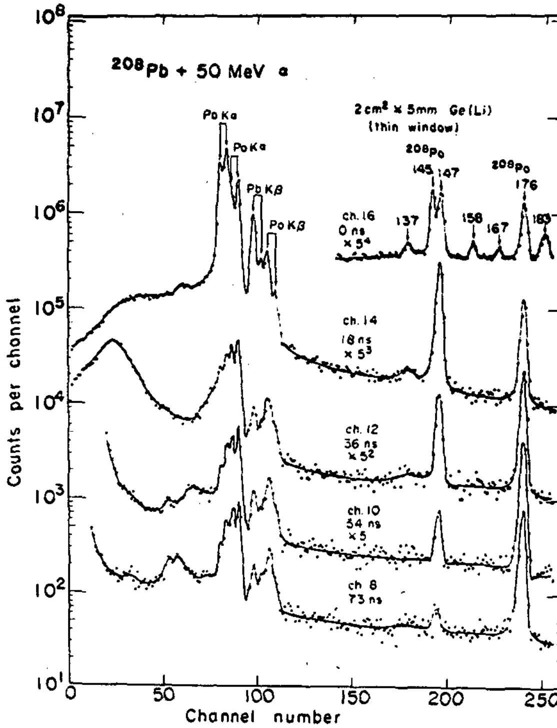

FIG. 23. Time spectra observed in the $^{208}Pb(\alpha, 4n)^{208}Po$ reaction at 50 MeV with a 2 $cm^2 \times 5$ mm thick thin-window Ge(Li) detector. The 147-keV and 176-keV γ-rays have delayed components with $T_{\frac{1}{2}} = 8.3 \pm 0.5$ ns and $T_{\frac{1}{2}} \approx 40$ ns, respectively.

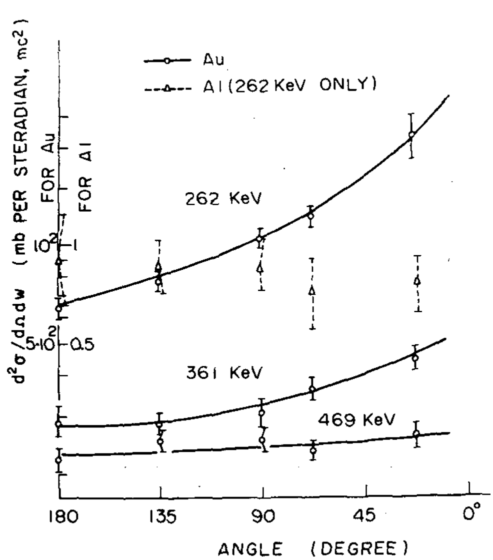

FIG. 24. Differential cross-section of continuum electrons.

6.3. Timing method

Isomeric states populated by nuclear reactions have been studied by beam-chopping techniques [32]. Recently, Yamazaki and Ewan succeeded in observing nanosecond isomeric states produced in (particle, xn) reactions, with the use of natural beam bunches from the cyclotron which give a zero point signal for the time scale [33]. The time distribution of the gamma-rays was studied with Ge(Li) gamma-ray detectors. Two dimensional time spectra are shown in Fig.23. They observed many isomeric states with this method [34]. The investigations proceeded for studying time-differential angular distribution of gamma-rays by making use of nuclear alignment of the levels populated in (particle, xn) reactions. This will provide information on the nuclear moments of high spin states. Preliminary experiments were performed in the Pb(α, xn)Po reactions [35]. This type of experiment will afford a powerful tool for investigating nuclear structure in the near future.

6.4. Stopping electron

Finally, I would like to mention a special feature of reaction electron spectroscopy. We noticed an electron continuum coming from the target in the study of (p, xn) reactions with 55-MeV protons from the Tokyo Cyclotron. These electrons contributed to the background of the electron spectra together with the continuum gamma-ray background. The contribution of electrons was separated from that of gamma-rays by analysing the energy of pulses from a Si(Li) detector with a multichannel analyser. The detectors were placed at the focal points in the M-RACE spectrometer. The response curves of each detector were calibrated with various radioactive sources placed at the target position. The differential cross-section is shown in Fig.24. A surprisingly large anisotropy was observed [36]. Though the phenomenon must be associated with an atomic origin, such as a type of stopping electron [37], further experiments are in progress to see to what extent it contains interesting physical meanings.

Finally, I shall conclude by saying that this sort of spectroscopy will be refined by means of larger Ge(Li) gamma detectors and coincidence techniques, together with on-line data processing systems with electronic computers, and will provide precise information on the genetic relations and branching ratio of the transitions. We can therefore expect that a great deal of information on levels in the 1 - 3 MeV excitation energy region will be accumulated to the extent that we shall be enabled to make a more crucial test for various nuclear models.

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DISCUSSION

G. ALAGA: I should like to ask: (1) whether you think there is one single nucleus which you would consider as an example of a nice vibrational state in the tin region and whether one can say that the vibration pattern is established; (2) whether in different regions considered one finds certain similarities of the vibration structure and whether one finds also some differences which might be certainly attributed to different configurations.

M. SAKAI: I don't think there exists a good vibrational nucleus in the tin isotopes, because the tin isotopes are single closed shell nuclei.

Certainly, there are certain groups of nuclei which seem to have the vibrational structure in different regions. For instance, the Se-Kr region, Pd-Cd region, Te-Xe region and Pt-Hg region. But if you carefully examine the level structure, you may notice some difference. The fact that 0^+ states seem to appear at a rather high excitation energy in the Pt-Hg region is a typical one. I think you are right that the difference might be attributed to different configurations.

G. ALAGA: I didn't mean the single closed shell; I just wanted to ask you which of the nuclei you would say is closest to the vibration pattern.

M. SAKAI: I am sorry I can't answer your question. Several Cd isotopes might be closest to the vibrational pattern, but I am not sure at all.

G. ALAGA: I was asking these questions because I would like to hear an unbiased view from an experimental physicist. To develop theories of vibrational states it is probably very hard to start from the beginning so one certainly has to have some kind of leaning on the experiments in order to find the proper approximations or correct some of the descriptions one already has. That's why I was asking which would seem to you the most reliable experimental evidence since I wanted to know whether one has enough data for a complete interpretation.

G.E. BROWN: It has seemed to me for some time now, that Bohr and Mottelson have sold us a Bill of Sale but have deluded us for many years because they proposed, with a liquid drop model, on the basis of parametrising, that they have 0, 2 and 4 triplet. One hardly ever sees all three and when one does usually the transition probabilities don't follow very well from the vibrational picture and then if one looks higher up in the spectrum the other levels don't follow very much at all, as they would from the vibrational spectra. With respect to vibrations of deformed nuclei the gamma degree of freedom is present also in SU(3) — no wonder that the gamma degree shows up quite generally. However, referring to the beta vibrations one always returns to a couple of the samarium isotopes which are very special and just at the point where nuclei go from spherical to deformed. Otherwise there are practically no good beta vibrations. Therefore it seems to me that there is probably something wrong with the whole model and that it is really time that we stopped just patching it up and talking in the same old language, but we really have to try to solve the many-body problem here.

J.O. RASMUSSEN: I would agree too that one has to be rather careful these days in talking about nuclear shape vibrations and that one must base one's use of this model on the deeper understanding from microscopic treatments along the lines of random phase approximation and Tamm-Dancoff and such as Soloviev and others have used. It would seem to me that these microscopic calculations would tell us that there are still certain regions where the concept of a beta vibration is a proper one. Indeed in the region of gadolinium and samarium a lot of the collective electric quadrupole strengths gather on the lowest zero-plus state from a microscopic calculation and therefore in that region it may be useful to talk about beta-vibrational states. In the centre of the region around erbium the calculations both of Soloviev and Sheline indicated that the beta-vibrational strength rises high as fractured among many zero-plus groups and that there one must be very guarded about using it. We have the calculations of Kumar and Banerjee which have seemed to be useful especially in the region of osmium and the region of samarium and perhaps that was a careful or a fortuitous choice of nuclei; it is probably at the edges of the region of deformation where this concept of beta and gamma vibration may still apply to single, lowest-lying excited states.

G.E. BROWN: People always come back to the samarium and gadolinium isotopes when they talk about beta vibrations. The whole concept doesn't seem terribly useful to me if it is so special. I think this is an important example of where the hydrodynamical way of looking at things really doesn't work. The particular degree of freedom is fractured through most of the nuclei and probably indicates the usefulness of microscopic calculations.

G. ALAGA: I agree with Brown about quadrupole vibrations or so-called vibrational states, but I am still suspicious that one does not have sufficient experimental evidence to prove or to disprove the slightly distorted vibrational structure, because sometimes the key evidence is lacking and that is what I wanted to hear from Professor Sakai. I would just like to mention another point. I think that the case of weak-coupling of nickel wasn't picked quite properly – probably the weak-coupling was much better in the case of bismuth, because the quadrupole strength usually doesn't have more than 60%, if so much, of the total quadrupole strength. So it is always a case of intermediate coupling and the mixtures in nickel are much higher than for instance in bismuth. In bismuth one has such a weak-coupling situation. Unfortunately you see it's a very unique example – we try to go through the periodic table and find another case where this would apply and it turns out that all the states are very high up and it simply does not work.

R.K. SHELINE: I should like to speak briefly about beta-vibration $K = 0^+$ states in deformed nuclei. As one looks more carefully it turns out that more and more of these states are being observed. Brown has mentioned the neodynium, gadolinium and samarium regions but also there are very low-lying $K = 0^+$ bands in ^{158}Dy, ^{160}Dy and ^{162}Dy. There are very low-lying $K = 0^+$ states in ytterbium 168, in thorium 228, in thorium 230, in ^{230}U, in ^{232}U, and in ^{234}U. In part the difficulty is that experimentally it is very difficult to populate a state with $K = 0^+$ which lies of the order of one MeV above the ground state. As our experimental information gets more complete, I think also the systematics will become more obvious. There is however the difficulty that there are other kinds of $K = 0$ vibrations, namely the pairing vibration which tends to complicate this picture.

M. SAKAI: Today is a crucial time in that we have to revise the traditional nuclear theories because many experimental pieces of evidence are sometimes contradictory to the nuclear model of the Copenhagen School.

This is why I have mentioned in my talk that we have now very good experimental techniques to investigate the levels at rather high excitation energy which might present test cases for the nuclear models. I mean the levels at the energy region of the three-phonon state in the framework of the vibrational model and of the two-phonon state of the gamma band and the beta band in the framework of the vibrational model. The conception of the quasi-bands introduced in my paper can also be examined now by means of this sophisticated experimental method.

J.O. RASMUSSEN: I would follow the comments of Sheline to say that one of the most stimulating examples of a large number of excited 0^+ states is the work of Gromov and his group at Dubna who found, I think, five different excited 0^+ states in erbium 164, the E0/E2 varying quite strongly. This has provided quite a challenge for the theorists; I think there is some theoretical work here at Dubna that has shown the possibilities of bringing the microscopic theory towards an understanding of this work, but I think that the study here in such case as the spin-one or spin-zero nuclei beta-decay is uniquely valuable in discovering these multipole and large numbers of 0^+ excited states.

G.E. BROWN: The real question is whether the gamma-ray transition probabilities follow from the beta vibration model or not. You can find 0^+ states in many nuclei because 0 is as good an angular momentum as any. But I am just afraid that people haven't seen the excited 4^+ state in ^{16}O.

A.B. MIGDAL: Considering the quadrupole-quadrupole in nuclei, of course you are missing some kind of vibration, e.g. a vibration which has the symmetry 0^+. If you consider the local interaction, and this has been done recently, you obtain in all spherical nuclei the 0^+ vibration which is not the two-phonon vibration, but one-phonon 0^+ vibration at the right place. Only near the double magic nuclei have you not got such a vibration. The same thing might also be valid in deformed nuclei.

G.E. BROWN: The question is whether this is a vibration or not. I know that in tin isotopes, for example, which are good examples of spherical nuclei, at least in ground and low excited states, if you take a picture of several quasi-particles and diagonalize it as best you can, you find a 0^+ excited state which, in fact, comes down somewhat in energy. You could call it to some extent collective. The real questions are: Is a band built up on top of this 0^+ state and are the gamma-ray transition probabilities such as would be predicted by shape vibration, which means that they are highly collective and that the band has certain regularities?

A.B. MIGDAL: Of course they are not the shape vibration and the 2^+ states. They are vibrational because of the rearrangement of density which has the symmetry of the second Legendre polynomials. We can calculate directly the change in density which is due to the 2^+ excitations. This change shows that it is only rearrangement of the density inside the nucleus.

V.F. WEISSKOPF: Is the vibrational spectrum built up in that way or not? Is there a vibrational spectrum, one quantum, two quantum, three quantum?

A.B. MIGDAL: When you consider the first state, you can calculate the density matrix which corresponds to this transition; this density matrix vibrates with the same frequency and has a symmetry of second Legendre polynomials and is not on the surface of the nucleus; it is inside the nucleus.

V.F. WEISSKOPF: Then it is not important where the vibration is but that it is a vibration; why does Brown then deny that it is a vibration?

G.E. BROWN: It is a funny situation where theories are assumed to be correct until proved wrong. I thought that people had to establish theories. This is not in answer so much to Professor Migdal but in relation to my earlier statement that I think that Bohr and Mottelson have sold us something because they tell us that there is shape vibration and everybody assumes that it is so and when they see things in experiments they fit them in although it is quite clear that the evidence is pretty sporadic and usually the transition probabilities don't follow this model.

A.B. MIGDAL: If you want to know something you should calculate a density matrix. Calculating the density matrix you see in what place of the nucleus you really have the rearrangement of the density. But of course you can consider only the change in quadrupole moments and not look inside the nucleus and you obtain more or less the same relations.

G.E. BROWN: I am essentially in agreement because if I were to do the same problem I would use very much the same methods. I would talk about it in a different way and tell a different philosophy, but when it came down to doing things it would be very much the same. One simply has to put the particles in and diagonalize the many-particle problem as well as one can. My point is only that if one does this, as Soloviev and collaborators have been trying to do with other types of forces, then the systematics of the beta vibration come out in only a few cases and if that is so, it is a very special thing. Why do we then talk about it as if it is something which we can use to classify excitations in practically all nuclei? So I don't think that we have a real argument; we have philosophical arguments, but not practical ones.

A.B. MIGDAL: Practically, there is a full system of equations from which you can obtain the results, and physically the result is that it is some inside vibration.

M. SAKAI: The experimental results now are still so poor that we could not seriously answer whether the beta band exists at all. We will have to be more patient and wait for new experimental data.

G. ALAGA: A question for Professor Migdal: when one discusses the surface vibrations of surface modes and density modes, how can one tell the difference between the two? Of course the difference exists in calculation, but where can one prove it experimentally? What data should be considered to prove or to show that it is actually density vibrations of the rearrangement as you say and not a surface vibration?

A.B. MIGDAL: When considering transitions in atoms you never ask this question. It is quite obvious that if you consider some kind of collective vibrations, i.e. some bound states built up from the quasiparticle and the quasihole, this is just the same problem as in an atom. For instance, considering the quadrupole moments in excited states you will of course obtain entirely wrong results if you consider such rough pictures as shape vibration. Recently we have shown that from the realistic picture you really obtain the right value of quadrupole moments in excited states. That gives an answer to your question because the quadrupole moment in excited states shows you in which place the rearrangements actually take place.

G.E. BROWN: Don't the monopole moments of excited states show you even more directly?

A.B. MIGDAL: You can show that the monopole moments – I mean the change in r^2 – in excited states have a very small effet. So they are difficult to measure.

G.E. BROWN: Well, there have been measurements with μ-mesic X-rays. These give a direct measurement. Now preliminary measurements certainly in tungsten isotopes seem to show that the excited 2^+ states have a smaller r.m.s. radius than the ground state. This is very hard to understand with a shape vibration.

A.B. MIGDAL: It is easier of course to obtain the monopole moment than quadrupole moments.

THEORETICAL PROBLEMS IN NUCLEAR STRUCTURE

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Abstract

THEORETICAL PROBLEMS IN NUCLEAR STRUCTURE. Problems concerning the nucleon-nucleon interaction, interactions off-the-energy shell, and the use of effective interactions in calculation of nuclear spectra are discussed.

1. INTRODUCTION

I do not propose to give a justification here for doing theoretical nuclear physics. I work in this field because there are many interesting problems. History judges what is useful and what is not; the greatness of influence of a scientist is unfortunately often measured by how long he can hold history back.

Atomic physics was first more or less abandoned for the then frontier – nuclear physics. Only recently have people returned, to find an astonishing richness of new phenomena. Only recently have resonances been seen in electron scattering from hydrogen. Such resonances are nicely described in formalisms invented for the description of nuclear reactions. Absorption of photons, the measurements of which were made possible through greatly improved experimental techniques, show collective, plasma-type effects. To explain these one has to go beyond the Hartree-Fock description, once thought to be the "end all" of atoms.

Many theorists left nuclear physics years ago for particle physics. Most nuclear phenomena thought to be understood at that time appear to us in quite a different light today. Many new and important effects have been discovered. Who could have previewed the Mössbauer effect?

Cross-fertilization of different fields of physics stimulates the overall development. Not only the original meson was predicted from nucleon-nucleon forces, but vector mesons were foreseen as the origin of the short-ranged spin-orbit force, some time before they were found experimentally. The SU(6) theory of particles was a direct extension of the Wigner supermultiplet theory. In GeV proton-proton scattering, people have rediscovered compound-elastic scattering. Nucleons at high energies appear more and more like composite systems, and techniques invented for nuclear problems are often employed, although they are usually given fancy new names.

The world of nature is a unity and its explanation must embody this. I intend, therefore, to cover especially those points which either relate nuclear physics to other parts of physics, or where one can give a more or less firm description of phenomena, starting from definite physical assumptions which can be tested.

2. THE NUCLEON-NUCLEON FORCE

The nucleon-nucleon force is the most basic of nuclear ingredients, although this is generally forgotten, as theorists gravitate more and more towards working with complex spectra and complex situations.

Let's face it. Particle physicists simply are not going to calculate the nucleon-nucleon force from meson theory. It's hard work. They would rather conjecture away in areas where they think that they can produce results by pure thought. On the other hand, couplings of the π, μ and vector mesons to nucleons become better and better known. Schwinger, Weinberg and others are busy manufacturing Lagrangians and sets of rules for using them in perturbation theory, so that the results satisfy the tested requirements of PCAC, chiral invariance, etc. It seems to me that these Lagrangians and these rules could also be used to calculate the nucleon-nucleon forces from meson theory much better than has been calculated in the past.

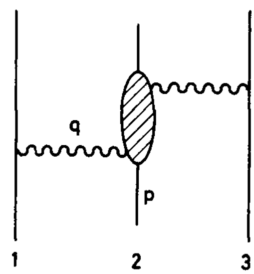


FIG.1. Origin of the three-body force. The cross-hatched area represents an N^*, a nucleon-antinucleon pair, etc.

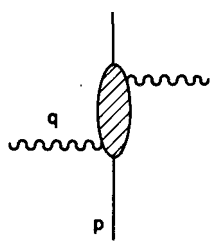


FIG.2. The middle part of the three-body force, which can be viewed as the scattering of a virtual pion on the nucleon.

3. NUCLEI AS SOFT PION FACTORIES

This brings me to my next topic, and that concerns nuclei as a source of soft pions. Pions travelling back and forth in the nucleus are virtual. Since they are emitted by low-energy nucleons, they usually are of low energy, and have a relatively small momentum; that is, they are soft.

Such soft pions enter into the three-body force, for example. As shown in Fig. 1, this can be envisaged as arising from a pion emitted by nucleon 1, which takes nucleon 2 to some excited state, this nucleon de-exciting by a pion carrying on to 3.

In lowest order, the momenta of the pion are $q_0 \cong \mu_\pi^2/M$, $|\vec{q}| \sim \mu$, with $\hbar = c = 1$. For practical purposes, $q_0 = 0$, since it is so small.

The middle part of the three-body force, shown in Fig. 2, can be considered as the scattering of a virtual pion of momentum $q_0 \cong 0$, $|\vec{q}| \sim \mu$ off the nucleon. Atler, Hamilton and Weinberg have shown that at $q_0 \cong 0$, $|\vec{q}| = 0$, the isospin symmetric scattering amplitude must vanish. Extrapolation away from this point must be smooth and the amplitude slowly varying, or it would be impossible to understand the successes of soft-pion physics.

Now, I want to illustrate an important point by this. Soft pion developments following from current algebra, PCAC, etc. make statements about scattering amplitudes off-the-mass shell ($q_0 = 0$, $|\vec{q}| = 0$ implies zero pion mass). Particle physicists then have to make arguments that these predictions extrapolate back smoothly into meaningful predictions for particles on-the-mass shell. In nuclei, the amplitudes are already off-the-mass shell, although in general somewhat further off than the point for which the prediction is made. Still, quantities generally extrapolate more smoothly in this latter direction than in the direction back to the mass shell.

Thus, study of two-and-three-body forces in nuclei is intimately coupled with soft-pion theory, and nuclei are copious sources of soft pions.

4. EFFECTIVE FORCES IN NUCLEI

Knowing the nucleon-nucleon force in free space, the next problem is deriving the effective nucleon-nucleon force in nuclei from it.

Let me say first of all that the nucleon-nucleon force is not really very well known, even empirically. Neutron-proton scattering experiments are simply not accurate enough nor numerous enough to pin it down well. However, we have various potentials which reproduce several hundred data fairly well.

Given a nucleon-nucleon force, empirical or theoretical, then the problem of deriving the effective force in nuclei is one of doing the many-body problem properly for finite systems. I believe that this can be done, as I shall discuss it in the symposium later, but many aspects of this have still to be tested and tied up.

I believe furthermore that one can make a Landau theory of nuclear matter, in which the various parameters are calculated from the nucleon-nucleon force. In fact, Sven Olaf Bäckman is doing that in Copenhagen, and it looks reasonably successful. If this can be done, then it gives important clues as to how to make microscopic theories of other Fermi liquids, such as liquid ^{3}He.

5. SPECTRA

Given effective forces, we can try to calculate spectra. In the s, d-shell, for example, one diagonalizes the interaction in all states that can be formed by distributing n particles among the $1d_{5/2}$, $2s_{1/2}$ and $1d_{3/2}$ orbitals (see Fig. 3). Here there are a very large number of possible states, and Elliot's SU(3) has been a big help.

In order to check models, such as keeping only the SU(3) states of highest weight, that is, the most deformed states, one does need, it seems to me, more or less exact diagonalizations of rather large problems. However, brute force methods, even with the most modern computers, won't take one very far in this respect. One needs, also, rather high-powered group theoretical techniques. Arima and collaborators in Japan, which is hardly foremost in computer availability to academic people, have probably come furthest in this respect, although Edith Halbert and collaborators at Oak Ridge have diagonalized up to 6 particles in the s, d-shell.

If one doesn't have a high-speed computer, one shouldn't despair. Such exact diagonalizations can explain only part of the spectra. One knows that in ^{18}O, for example, highly deformed states obtained by lifting particles out of the p-shell mix in important ways into the low-lying spectra. The first excited state in ^{16}O at 6.06 MeV seems to be mainly composed of four particles and four holes (see Fig. 4).

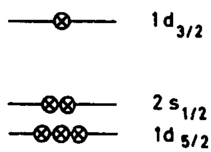


FIG.3. Typical configuration of 6 particles in the s, d-shell.

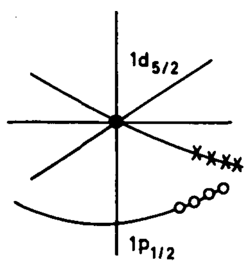


FIG.4. Schematic representation of the excited state in ^{16}O as four particles and four holes in a Nilsson diagram.

6. ISOBARIC ANALOGUE STUDIES

Let me jump now to a topic that seems to be far away from those above, but which hasn't been sufficiently covered thus far. The isobaric analogue states give us compound states of known structure, often at high excitation energies. Utilizing these through their decay, for example, we can determine components of wave-functions rather precisely. Recent work in the ^{208}Pb region has given us rather accurate checks on calculations and on our use of many-body theory in finite systems.

7. DISCUSSION

Where does this leave us? Well, I think it leaves us at a point where it is unlikely that studies of deformed nuclei in the rare-earth region on actinides will tell us much with respect to fundamental laws.

Fission is fascinating, and its practical application overwhelming. With Professor Flerov, I am tickled by the prospect of making superheavy nuclei. But I believe that the study of fission will tell us mainly about fission, and that not much will be more widely applicable.

What I am really saying is that, while it is a legitimate activity of nuclear theorists to understand and describe nuclear phenomena in terms of models, they should not lose sight of fundamentals. I believe that most nuclear theorists have, and that is why they are becoming isolated from other theorists. This is a great pity, because we have much to learn from each other.

I make no apology for not outlining what I consider to be important experiments. Although I consider close contact with experiment essential for any theorist, for ours is by and large an empirical subject, we should also not forget that we are trying to discover the laws of nature, and not only play houseboys for the experimentalists.

DISCUSSION

A. B. MIGDAL: It is very useful to divide the problem of the nucleus into two parts. One is to introduce some constants; you should, however, be sure that these are really constants and not variables. Knowing these constants from experiments, you can solve the second problem - how to calculate these constants from some three-body forces. I should say that this second problem is very important but not so interesting as it seems from the first view; and there are very many difficulties. First of all you know the forces only on the mass shell and in the nucleus you should know them off the mass shell. Secondly, the three-body forces really exist, but you cannot calculate them. In my opinion you should first of all work out a theory for ^{3}He and ^{3}H. After you have produced a good theory for these light nuclei with three-body forces, I shall believe in the application of these forces in more complex problems. To check some principles, you should first of all take the most simple system. Only in one sense is the nucleus a good object for theoretical exercises – the many-body problem – and of course it is interesting to check the applications of many-body theories to the nucleus. But for checking the fundamental principles the nucleus is not a good object. Therefore, in my opinion, the second problem – the calculation of the constants – is a very important but not a principal one.

G. E. BROWN: I would say that we are learning more about behaviour of interactions off the energy shell. This is connected with the developments of current algebra and PCAC, etc.

A. B. MIGDAL: For this you should do some experiments. For instance, consider some problems connected with the interaction of deuteron with light, and obtain some information about interaction off the mass shell. Of course, such experiments can be done.

G. E. BROWN: The current algebra does now make statements about the behaviour of amplitudes off the mass shell and this cannot be directly tested. But other predictions of current algebra can be tested and models consistent with current algebra can be made for extrapolation off the mass shell. The second point is that one has other models for going off the mass shell, namely one has various nucleon-nucleon forces and one can just mechanically take them off the mass shell. In none of these models does going off the mass shell produce very large ambiguity. One would have to think of quite pathological models in order to make large differences and I don't believe that this is a strong objection. The third point that I would make is that I also don't believe that one would find out from nuclear spectra very many fundamental things about the nucleon-nucleon force. The situation is really just too complicated in nuclei and one needs to look in nucleon-nucleon collisions, which is a much simpler situation. But I believe that if we are working as nuclear physicists we ought to be able to derive, in a straightforward and didactic way, the quantities that we are dealing with from more fundamental principles. I would feel uncomfortable in working and fitting parameters all my life.

A. B. MIGDAL: Why not check this approach in the more simple examples?

G. E. BROWN: The example has been checked to some extent through the calculations of Blatt and collaborators. They do not obtain too good a check, because they miss the binding energy by about $1\frac{1}{2}$ MeV but that is only 2% of the potential energy and so it shows that they are able to calculate the potential energy of this system to within 3% or 4%. Presumably, with better nucleon-nucleon forces they may be able to do this.

But let me first of all say that the system of ^{3}He, if you want to do all this, is not very elementary. With more particles it really becomes a many-body problem in which the Pauli principle simplifies many phenomena.

V. F. WEISSKOPF: I believe that the discrepancy between Brown and Migdal is a very positive element because there is no contradiction between the statements. Why shouldn't Migdal go on working in this field, which I think is very good and interesting, and, at the same time, Brown and his collaborators and the Copenhagen group try to calculate these constants which you are using. This is all very good; it would be deplorable if this conference ended with the fact that one approach had been decided to be wrong and the other approach right.

However, I would like to ask Brown a question. I am sort of old-fashioned and I haven't followed the latest developments. Is it really true that we know the nuclear forces so well that we can attempt such calculations? I thought that one still didn't know whether the nuclear forces are velocity-dependent or not, whether they are local or not, whether one can express a nuclear force by a potential or not – these difficulties would actually affect your kind of calculation very thoroughly but of course would not affect the Migdal kind. To my mind this makes the Brown calculation more important because it may be that the further calculations, such as those of Blatt, and, as you say, perhaps the easier calculations in many-body problems, may help to decide how we should represent nuclear forces.

G. E. BROWN: I would not like to call you old-fashioned. But, the point that I was trying to make is that there have been a lot of developments in particle physics, e. g. by Schwinger and Weinberg, who tell you that if you want to calculate meson-nucleon scattering, you should take a Lagrangian and use simply the perturbation theory. If you should do that for pion-nucleon scattering, why shouldn't you do it for nucleon-nucleon scattering?

On the other hand, I wouldn't like the discussion to turn this way between Professor Migdal and myself, because we are much more in agreement than we are in disagreement. The main controversial point that I wanted to express was that we will find out much about fundamental physics from complex spectra and I think we will, by studying the rare earth region, find out much about the rare earths, but I do not think that we'll find out much about physics.

I. N. MIKHAILOV: Some very simple calculations on the properties of the effective force for nuclear structure models has also been done at Dubna.

Our philosophy was that Migdal's theory is not too different from the Hartree-Fock or random-phase approximation. In these approximations it is fairly clear what should be called by an effective force and which other quantities or parameters can be connected with the effective force. We started by trying to connect properties of the effective force with the properties of the nuclear self-consistent potential. We tried to analyse the connection between our estimate of the force and the calculations done by Professor Brown. These kinds of renormalization were taken into account in our first approximations. We feel that not all the important renormalization processes which come through the short range part of the reaction matrix were taken into account.

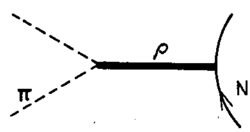
V. G. SOLOVIEV: I believe that the theory of the nucleus should not be reduced to a two-body problem. The interaction of two nucleons as completely as we may describe it is much poorer than the nuclear many-body problem.

Nuclear physics is therefore developing in two directions. To understand the structure of the nucleus we must on the one hand try to study and investigate very complex spectra and to use as high excitation energies as possible. The second process should be the investigation of as many nuclei as possible, the investigation of heavy nuclei, investigations as remote as possible from the stability region, etc. Investigating, for example, the deformed nuclei in the rare earth region, we get the impression that these approximations and models are only suitable for these particular nuclei and give us very few fundamental things. I do not think that this is so. I think that investigations of deformed nuclei increase our knowledge of the nuclear many-body problem, and thus help us to form better theories. To a certain extent the different models that we use have a tendency to become unified at some time. There must be different approaches and there must be different directions in our investigations, and it seems to me that a very interesting direction is the approach that Professor Migdal has proposed. Of course this is not the only correct approach.

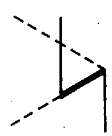
Ya. A. SMORODINSKY: It would be very nice to make the nuclear matter theory form the first principles. We have to know for the interaction of two nucleons not only the scattering amplitude, but the wave function, i.e. the properties of the system at the short distances. This is why we need the potential and why we cannot use only the scattering data in the high-energy region. We know from the high-energy scattering that the potential between two nucleons is very cumbersome. It consists of at least five different terms and nobody until now has succeeded in putting down the potential which described really all kinds of polarization phenomena. If you confine yourself only to the scattering you can write the potential but it is not unique. It is possible to construct a lot of potentials with the same scattering properties. If you include the polarization phenomena nobody can give a real potential which gives a good description of all the known phenomena. From the calculations of ^{3}H and ^{3}He made by Simonov and others it was clear that the behaviour of the potential at the very short distances is not well known, and that the properties are very sensitive to the potential behaviour in the origin of the co-ordinates. That means that we don't have now a good basis for building the nuclear matter theory for the first principles and the only way today is to work out some semi-phenomenological theory.

G.E. BROWN: I would like to comment on Professor Smorodinsky's remarks. Professor Smorodinsky is an expert on the two-body problem, especially proton-proton scattering. It seems to me, if I may say so, that you have a defeatist attitude, namely when one wants to solve a problem, you think of all the difficulties you might encounter rather than going ahead and trying to work at the problem and seeing what you can do with it.

There are many examples where when one has worked at the problem it has turned out to be very simple, or much simpler than it seemed to be at first. Of course, you may say that I am naive and simple-minded. But let me bring up a recent example. One of the most striking successes of the last two years is Weinberg's formula for the scattering of the low-energy pions by nucleons and by nuclei. The conclusions of Weinberg's work on soft pions are, as far as the isospin antisymmetric amplitude is concerned, that the low-energy pion-nucleus scattering is given by the exchange of an S-meson

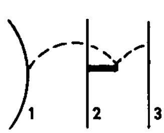


Thus, doing the simplest thing one can imagine, one obtains the pion-nucleon scattering. The soft-pion rules tell one that processes such as



where, for example, a nucleon-antinucleon pair is involved, should not be added in. Part of the time, the ρ-meson is a nucleon-antinucleon pair, and this would involve double counting.

We then have a simple recipe for calculating the isospin-antisymmetric contribution to the three-body force in the triton. Namely, we consider the process



This is why I say that we should use what we are learning from high-energy physics and from particle physics to tell us about how to do things in nuclear physics. In this particular example, we see that all the difficulties you might have thought up to stop people from doing the simplest thing, are not applicable.

Of course, I don't mean to say that the problems of complex spectra are not valid ones to work with and that there's not a great richness of physical phenomena. What bothers me is that nuclear theorists have gravitated almost completely towards these areas and that very few are concerned any more with the connections back with fundamentals and other parts of physics; I don't see that these connections are going to be made by anybody else unless we do them, the reason being that it's just too hard work, and my feeling is that high-energy physicists don't like to do this kind of hard work.

D.H. WILKINSON: I am extremely happy to see this intimate relationship between high-energy physics and nuclear structure. At the same time, I would like to invite Professor Brown to become useful again. He explained that he was no longer prepared to be the houseboy of experimentalists and I think he deserves a sabbatical for a short time, but I would like to invite him to be useful again and tell us how in fact we can reassure ourselves that these sorts of things are really going on.

I have seen a conserved vector current; I have never seen a partially conserved axial vector current. I would like to know whether it is possible to go and find one. This is a fundamental theory, which has model aspects to it, and I would very much like to know what sorts of experiments it is possible to do, that will bear as directly as possible on this theory.

I should now like to ask another question: what sorts of experiments might be useful in getting direct information about these very important off-energy-shell effects, in particular whether nuclear bremsstrahlung experiments are likely to give information of direct relevance in the conditions which are of importance in nuclei. If so, what sorts of energies should we be involved with and, most important of all, with what accuracy would such measurements have to be made? This is moving into another plan of nuclear structure physics where one is having to do experiments of a greater subtlety. Technically they are very difficult and one does not want to do these experiments unless they are going to produce data of as direct relevance as possible to these very important issues that have been discussed here.

G.E. BROWN: I would say it is not strange that you have never seen a partially conserved axial current because this would, of course, imply a pion of zero mass. For the three-body force, the point is that one is very close to the part where the axial current is conserved and that's why the amplitude vanishes, exactly at the point where it is conserved, namely off the energy shell. One can't check things at just that point, one has to extrapolate back to the physical region in order to check such things as this pion scattering.

I think Professor Wilkinson has, however, brought up a very important area that I forgot about in my paper, namely the bremsstrahlung. This gives us, in principle, important information about the nucleon-nucleon force off the energy shell. The problem is that the photon doesn't carry a lot of momentum; at least in the recent experiments rather soft photons have been used so that the information is not very far off the energy shell. And in using this information, once people have learned how to calculate things correctly, practically all of the known nucleon-nucleon potentials give results which are so close to each other that within experimental errors one cannot distinguish between them.

If one can do experiments with photons of higher energy to carry one further off the energy shell this will give very important information, and I believe these experiments are now being attempted. Professor Wilkinson also in his paper mentioned the connection between the radiative capture and non-radiative capture. Now, using the PCAC theory one can make this connection with the proviso that the extrapolation in the pion mass is smooth.

The other thing that one can check is in more complicated phenomena where one has the problems of complex nuclei and would not expect a simple formula to hold such as Weinberg wrote down. One can check just how large the contributions from excited states and from anomalous thresholds are. There are relatively few places in particle physics where one can really check the importance of anomalous thresholds.

D. H. WILKINSON: I don't find Professor Brown's reply very comforting for two reasons. One is that I know that I can't have pions of zero mass but I can deal with simple systems where the predictions of PCAC might be checked rather more directly. I am thinking particularly of problems like the predictions that PCAC must make about the renormalization of the axial vector contribution to beta decay and where one might hope by, as Blin-Stoyle has done, comparing a pion production amplitude in nucleon-nucleon collison with the tritium beta decay, to make an absolute prediction of the renormalization of the axial vector contribution. That I would regard as at least a partial check of PCAC.

I was hoping to have suggestions about simple situations where PCAC might make a verifiable prediction similar to the predictions that were made by the conserved vector current.

However, and this brings me to my second point, one must always be sceptical if the predictions made by a conservation theorem or symmetry property are very similar to the ones that one would have made in any case.

I could refer now to Professor Brown's reference to the correspondence between muon capture and radiative pion capture. It is perfectly true that PCAC makes predictions about the relationship between these two processes but it is also true that they are the same predictions as one makes by conventional means, without regard for PCAC. PCAC merely removes some of the approximations that are made in the conventional predictions. It does not change the predictions themselves. So there again those experiments which are now in progress and seem to be going in the direction that one expects from PCAC do not seem to constitute a very sharp test of the hypothesis.

One presumably wants more accurate experiments. The history of the conserved vector current, of course, is somewhat similar. One does make rather closely similar predictions to those of CVC by other methods and one's confidence in CVC comes about because the predictions that it makes are very close to what we find in experimental facts.

Now, are the predictions of PCAC as exact as the predictions of CVC? I presume they are not, because in problems like renormalization of the axial vector coupling constant one does assume pion dominance and so on. It does not seem to me, as an experimentalist, to be as exact a working theory as CVC and so the predictions that it makes cannot, I presume, themselves be exact in the same sense as those of CVC. And when one remembers that the predictions are not very dissimilar from those that one would have made by conventional methods I merely emphasize my anxiety in using this approach to make predictions about very complicated systems where we cannot possibly hope to verify them.

G.E. BROWN: I think you missed my main point and that is not that PCAC often gives the same predictions as naive approaches, and therefore one can justify naive approaches to such as Professor Smorodinsky. I am not really asking you to check these questions on complex phenomena. I think they should be checked on simple phenomena, probably primarily by particle physicists.

USE OF MASS SPECTROMETRY IN SOME PROBLEMS OF NUCLEAR PHYSICS, AND RELATED APPLICATIONS OF THIS TECHNIQUE

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Abstract

USE OF MASS SPECTROMETRY IN SOME PROBLEMS OF NUCLEAR PHYSICS, AND RELATED APPLICATIONS OF THIS TECHNIQUE. The paper discusses present-day work in the fields of nuclear physics and nuclear astrophysics, and points to future developments.

This paper aims to show how a certain number of regions of interest in nuclear physics can be investigated in laboratories with relatively small budgets, if they work in collaboration with larger centres equipped with accelerators or reactors. The authors would also like to give an example of the application of nuclear physics to astrophysics, since, apart from the interest of such applications in themselves, they open up great prospects for nuclear physics. Mass spectrometry has proved itself to be an important tool in this field, and some of the work undertaken in Orsay is presented here (see, for example, Ref. [1]).

The main objectives of the studies can be summarized briefly in three categories:

A. Study of the mechanism of reactions where energies higher than 100 MeV are deposited into the target nucleus by incoming particles.
B. Study of the properties of nuclei far from stability:
   - their existence compared to predictions of mass formulae;
   - their mass and decay schemes compared to existing nuclear models.
C. Study of specific nuclear reactions which are considered to occur at the surface of stars and in interstellar space or on meteoritic matter.

Information needed to solve these questions includes not only formation cross-section of the reaction products and their identification but also their energy and spatial distribution, the effect of varying energy of incoming particles, etc. Mass spectroscopic investigations have been essentially limited to the first point.

In summarizing some experiments carried out in recent years the authors find three groups in respect to the technique involved:

(1) The mass spectrometer used is identical with those used in other fields but the methods for sample preparation are generally different.

(2) The measurements performed constitute a new application of a recently developed type of mass spectrometer.
(3) The mass spectrometer is designed to operate directly in the beam of the accelerator; isotope production and analysis are simultaneous.

A common feature in all cases is the fact that very minute amounts of nuclear reaction products are formed due to the small reaction cross-section (millibarns), the low fluxes of particles and the limited availability of machine time. Thus, experiments have been generally limited to the two groups of elements for which mass spectroscopy has achieved the highest sensitivity (noble gases and alkalis). One important point, however, is that with mass spectroscopy we can study stable isotopes; and, in this case, the limiting factor is often the contamination by natural elements; thus, very elaborate handling techniques and ultra pure elements must be used.

A. STUDY OF THE MECHANISM OF REACTIONS AT HIGH ENERGY

A.1. Measurement of nuclear reaction cross-sections leading to the production of rare gases

Few experiments have been made in this field since the first experiments in 1954 and 1958, but motivations have still remained the study of nuclear reaction mechanisms and the interpretation of the data collected in meteoritic studies.

In the last case the aim is to compare experimental cross-sections with predictions based on spallation statistics, the object being to compare the isotopic spectra of rare gases found in meteorites that have been bombarded by high-energy cosmic rays with the pure spallation spectra of elements which are considered to be the main cosmic ray targets in these meteorites.

New experiments at Berkeley have recently given the spallation yields of Xe isotopes from barium targets irradiated by 730 MeV protons.

Both the above-mentioned motivations have led Schaeffer and colleagues in Brookhaven [2] to undertake a large program for the measurement of all rare gases at 3 and 29 GeV in targets ranging from copper to uranium. The type of high-sensitivity mass spectrometer employed, as well as the extraction and purification methods of rare gases used, are those utilized in meteoritic studies. The extreme sensitivity of the method appears when considering that a five-minute bombardment (3×10^{13} protons) in the Berkeley synchrocyclotron was sufficient to obtain a xenon spectrum. Experiments similar to these were carried out in 1962 at 540 MeV (CERN).

A.2. Measurements performed with the surface ionization technique (mostly reaction mechanism studies)

The surface ionization technique has been used in a much greater number of experiments. It has evolved from a classical method in which alkali elements were first chemically separated from the target and then introduced into the ion source of the mass spectrometer.

Several methods based on vacuum distillation of the reaction products from a more or less refractory target have allowed measurement of the production cross-sections of various alkali isotopes in high-energy nuclear reactions.

In the first, the oven technique, a small, very carefully cleaned oven contains the irradiated sample; immediately above it is the mass spectrometer filament cooled with liquid nitrogen.

The nuclear reaction products are thus transferred to mass spectrometer filaments during short successive distillation sequences. Isotopic ratios are measured on samples of the order of 10^{-12} g and in some cases smaller than 10^{-14} g, and absolute cross-section is obtained by normalization with respect to some radiochemically determined isotope [3].

Thus Nguyen Long Den studied the production cross-section of ^{22}Na, ^{23}Na, ^{24}Na in Al at 150 MeV and made an extensive study of the (p, 3p xn) reaction on ^{139}La and (p, 5p xn) on ^{141}Pr which led to the production of cesium isotopes. These reactions, in which α-particles are emitted, give some information on the presence of α-particle clusters at the surface of the nuclei studied. Gradsztajn, Epherre, Klapisch, and Yiou succeeded in measuring the production cross-sections of the stable lithium isotopes in ^{12}C in a broad range of energies (50 MeV - 25 GeV) using the oven method and, with a somewhat different method, extended the same study to a ^{16}O target [1,4].

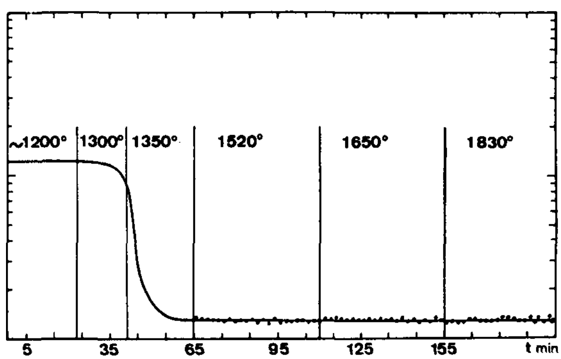

FIG.1. Evolution of the $^7Li/^6Li$ ratio as a function of time and temperature in the filament method.

While still making use of surface ionization, a method specific to high-energy nuclear reactions has been developed, which consists of irradiating the mass spectrometer filaments directly. Although these are very thin compared to a usual target, their bombardment inside the ring by the circulating beam of the accelerator leads to a high production rate. This is because the energy lost by the GeV protons in the filament is so small that the protons traverse the target a great many times before being lost. During the isotopic analysis the surface contamination is evaporated at relatively low temperature while the reaction products, which have first to diffuse out to the surface, appear at higher temperature. The evolution of the isotopic ratio of the lithium emitted from a Ta filament has been studied (Fig.1).

It should be noted that, despite the fact that the sample is practically exhausted at the end of the analysis, there is no evidence for isotopic discrimination: the $^{7}Li/^{6}Li$ ratio is constant. This has been verified in several experiments and can be understood in terms of diffusion by substitution at practically infinite dilution. Relative and absolute cross-sections have been obtained by this method for the production of lithium isotopes in Pt, Ta, Rh at 19 and 30 GeV as well as for ^{22}Na and ^{23}Na production in Pt at 30 GeV.

The very slow rate at which the isotopes of interest diffuse out of the irradiated filaments makes it essential, however, to use an electron multiplier and pulse-counting techniques.

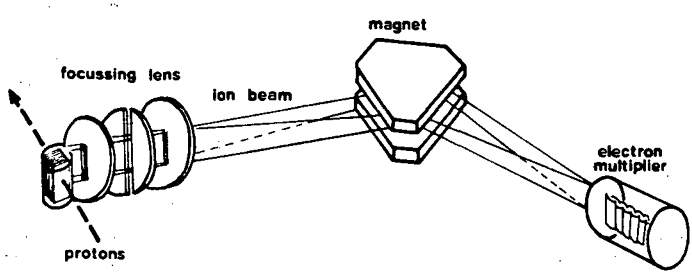

FIG.2. Schematic representation of the mass spectrometer (on-line) operating in the beam of an accelerator.

B. MASS SPECTROMETERS OPERATING DIRECTLY IN THE EXTERNAL BEAM OF AN ACCELERATOR (Nuclear structure and reaction mechanism studies)

The need to study the evolution of the nuclear properties of isotopes when they are further away from the valley of stability has motivated the investigation of new techniques which would be capable of accomplishing in a very short time the chemical and isotopic separations required.

Several projects are being developed at present among electromagnetic isotope separator groups and some experiments have already been performed on base or volatile elements with a target connected to the ion source of the separator through a more or less extended gas line. However, we shall restrict our topic to the experiments performed with mass spectrometers.

In the method developed at Orsay [5] the ion source of the mass spectrometer is directly bombarded by the proton beam. One relies on diffusion in solids at high temperatures to extract rapidly the reaction products from the target, and on surface ionization to perform both a good chemical separation and at the same time a very efficient ionization of the alkalies. Recently the same method has been applied to negative halogen ions.

A schematic representation of the experimental set-up is shown in Fig.2. A high-energy proton beam traverses the ion source which is at a normal potential of 3000 V and simultaneously heated to 1500 to 1800°C; after deflection, the ions are measured by a high-gain electron multiplier used in the pulse-counting mode.

The source principle is as follows: a succession of thin foils of the element under study and of thin graphite slabs are enclosed in a cylindrical metal foil heated by Joule effects. Under high-energy proton bombardment the nuclear reaction products recoil with relatively high energies out of the metal target and are stopped in the graphite, which has been calculated to be thick enough. In the hot graphite (1500 - 1800°C) these products diffuse out rapidly and the work function of graphite is high enough so that only alkali elements have a large probability of leaving as positive ions.

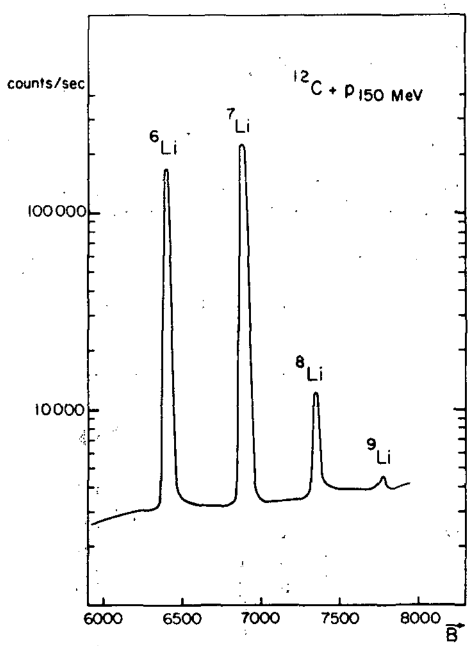

FIG.3. Li mass spectrum obtained from 150 MeV proton bombardment of a carbon target ion source.

The choice of graphite as a diffusing agent has been made both because of its crystallographic structure (large inter-laminar distance) and its refractory properties. Targets are usually composed of 25 such foils and slabs, separated from each other by thin graphite spacers so as to reduce the extraction time of the ions.

At this point, we shall show the Li mass spectrum obtained with the first and simplest of these target ion sources made of pure carbon slabs 0.1 mm thick. One sees clearly not only the stable ^{6}Li and ^{7}Li but also ^{8}Li (0.8 sec) and ^{9}Li (0.17 sec), each isotope being recorded for the same length of time at a proton energy of 150 MeV (Fig.3).

Typical diffusion curves obtained in this way at 1500°C are shown in Fig.4. They correspond to the diffusion in graphite of ^{24}Na recoils from an iridium target, and ^{83}Rb and ^{129}Cs from a thorium target, all bombarded by 10 GeV protons.

The parameters affecting the diffusion time are numerous and it is not possible to examine them here.

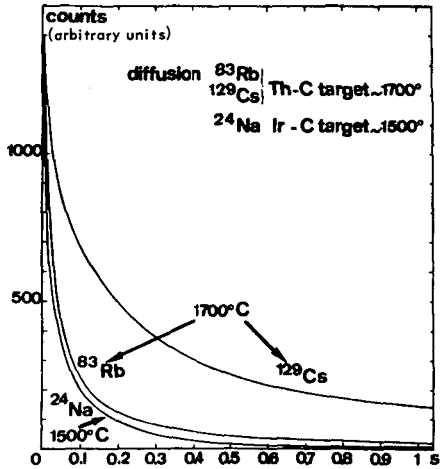

FIG.4. Variation of the number of atoms leaving the ion source of the mass spectrometer as a function of time after interruption of the high-energy proton beam (diffusion curves). The ^{24}Na is produced in Ir and diffusing out of carbon. The ^{83}Rb and ^{129}Cs are produced in Th and diffusing out of carbon at the indicated temperatures.

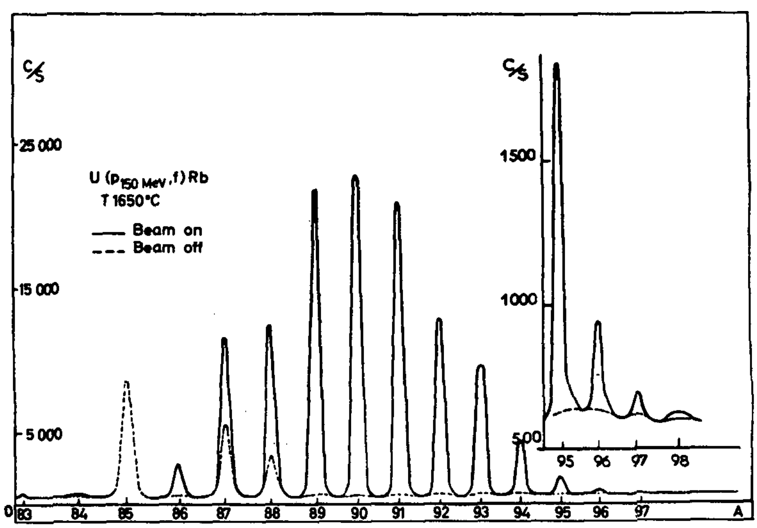

FIG.5. Mass spectrum of the rubidium isotopes formed in 150 MeV fission of uranium.

Cross-section measurements of spallation, fission and fragmentation reactions at high energies

Within the last two years three instruments of the type just described have been built and operated on-line with various accelerators. A great advantage of these small machines (radii of curvature 15, 22 and 30 cm) is that they can be mounted and operated in the home laboratory and then transported easily to the various accelerators where and when high-energy protons are available: the experiments we shall mention have thus been performed at Orsay (150 MeV), Saclay (3 GeV), and CERN (10-25 GeV).

Experiments at 150 MeV

Figure 5 shows a mass spectrum of the isotopes of rubidium obtained by Amarel et al. [6], who bombarded a uranium-carbon target at 150 MeV.

One sees here all the isotopes of rubidium from mass 83 to mass 98. The last three, 96, 97 and 98, were not known before this experiment and have radioactive half-lives shorter than 1 sec. A similar spectrum has been obtained of the Cs isotopes formed in the same experiment.

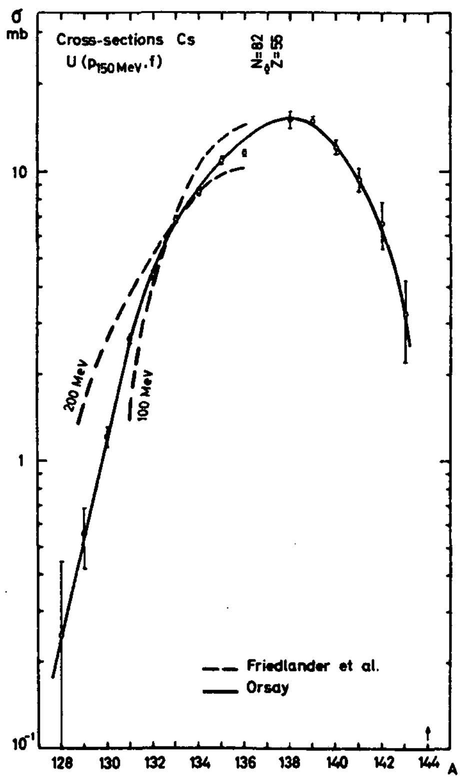

FIG. 6. Production cross-sections of Cs isotopes from high-energy fission of uranium.

A comparison of the cross-sections obtained with this method (150 MeV) and the results of Friedlander et al. obtained at 100 and 200 MeV by chemical separation followed by isotopic analysis is shown in Fig.6 for the isotopes of Cs formed in a uranium target. The agreement is excellent. The order of magnitude of the smallest cross-section which it is possible to measure is about 20 μb.

Experiments at 10 GeV

The extreme pressure existing on the use of very high energy accelerators demands the most efficient possible use of such beams when they become available. Thus in a series of experiments performed at 10 GeV two mass spectrometers were simultaneously put on line with the fast-extracted proton beam of the CERN P.S.

The first is a 30 cm radius instrument used for the heavy fragments and the second (15 cm) for the lighter ones. Ion sources are interchangeable and 17 of them, corresponding to targets ranging from carbon to uranium, were studied in a ten-day run. Besides obtaining many new fragmentation cross-sections, which will be useful in the interpretation of reaction mechanisms, several new isotopes have been found which are close to the predicted limit of particle-stability. The very short diffusion times of the reaction products in the ion source which we have indicated previously are such that the limiting factor for the detection of new isotopes is not their short half-life but their small production cross-section.

The present limitation of this technique to the study of alkali elements is not as serious as it would appear. Indeed the reaction products in heavy targets are spread out over the entire table of the elements and a regular sampling in the region of Li, Na, K, Rb, Cs, as well as Fr, gives a very broad spectrum of information. Moreover, the use of other ionization techniques should enlarge the scope of mass spectrometry quite substantially in this field.

Half-life measurements and study of the radiations emitted by the collected isotopes

In the experiments just described, the total amount of target material was of the order of 20 to 30 mg. Increasing this to 0.5 g, as was done with uranium at 150 MeV, yields separated isotopic beams which are intense enough for it to become possible to study the β- and γ-rays emitted by the radioactive nuclides.

In fact, the very short time which elapses between production and collection (and which includes an effective chemical separation) makes this method superior to the radiochemical ones for short-lived species.

The instrument is essentially the same as the ones mentioned previously (22 cm radius, 3 kV acceleration) except for the collector end. There a simple electrostatic deflector has been set behind the collector slit so that the isotopic beams can be either switched towards an electron multiplier for mass spectrum recording and selection, or left to drift straight on to a thin metal strip behind which can be placed a β- or γ-ray detector or even a neutron counter (Fig.7). In a recent improvement the beam has been post-accelerated to 10 kV and a 5-m-long pipe and a set of electrical quadrupole lenses have been used to move the final collecting point into a well-shielded cave.

From theoretical considerations this region of the heavy Rb and Cs isotopes had been predicted to contain some delayed neutron precursors. Therefore a series of experiments has been carried out in which a neutron counter was placed at the final collector in the shielded cave where neutron background was negligible. Neutron emission has been detected at the rubidium masses 93, 94, 95, 96 and 97, and at the cesium masses 142, 143 and 144, thus doubling the number of identified delayed neutron precursors known.

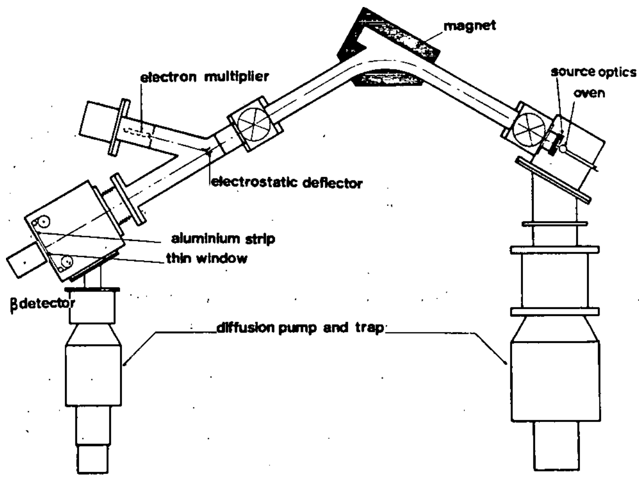

FIG. 7. Diagram of the on-line mass spectrometer used by Amarel et al. for the study of the β- and γ-rays and neutrons emitted by the short-lived isotopes of Rb and Cs.

C. STUDY OF SPECIFIC NUCLEAR REACTIONS IN CONNECTION WITH ASTROPHYSICS

To the nuclear physicist Li, Be or B have no characteristics which should single them out particularly. On the other hand their abundance in nature is very anomalously low compared to their neighbours He, C, N and O (10^{-6} to 10^{-8}) and this problem has long been recognized as meaning that a special process had to be at the origin of the nucleosynthesis of these elements.

In 1955 Fowler et al. [7] suggested that spallation reactions on heavier elements such as C, N and O, could be responsible for the production of Be at the surface of some stars; and in 1962 Fowler, Greenstein and Hoyle [8] published a very complete model of the nucleosynthesis of the light elements in the solar system which was based both on astrophysical and nuclear considerations. Since the latter depended heavily on spallation cross-sections which were not known experimentally at the time, a series of experiments using mass spectrometric techniques were undertaken and the results led Gradsztajn to propose a model of the nucleosynthesis of Li, Be and B which is different from the Fowler, Greenstein and Hoyle (FGH) model in some fundamental aspects.

Thus the problem was to study in the laboratory the nuclear reaction leading to the production of the various isotopes of Li, Be and B both stable and radioactive:

Li 6, 7		
Be 7, 9, 10		on ^{12}C, ^{14}N, ^{16}O
B 10, 11		

and to compare the ratios $^{7}Li/^{6}Li$, $^{11}B/^{10}B$ with the equivalent measured in natural settings such as the earth or the meteorites.

Method

Use was made of a Nier-type mass spectrometer equipped with a high-gain electron multiplier detector for pulse counting of the ions.

The ion source was of special type. Be and B in contrast to Li cannot be efficiently ionized by the mass spectrometer thermionic ion source. Thus a very sensitive sputtering ion source was used, of the Castaing and Slodzian type. This new arrangement allows at the present time analysis of the 10^{-13} to 10^{-12} g of Be and B produced by the spallation of ^{12}C and ^{16}O in a few μA-hour proton bombardment. This represents an improvement of several orders of magnitude in the detection of Be and B. The spallation products are extracted from the irradiated targets and deposited on the metallic sample plate, which is bombarded by the primary ion beam.

Figure 8 represents the apparatus, with its primary ion source yielding a focused beam impinging on the sample plate from which secondary ions are emitted. This secondary ion beam is accelerated to 1000 V, focused by a 3-element lens, and directed towards the magnetic analyser. The isotopic beams at the collector are measured by an electron multiplier. For the sake of simplicity the ion gun is essentially composed of a tantalum furnace, emitting Cs^{+} ions by surface ionization, and of a focusing lens. The cross-section of the 2000 eV ion beam is about 2 mm^2 and its density at the sample plate is 10^{-9} A/mm^2 .

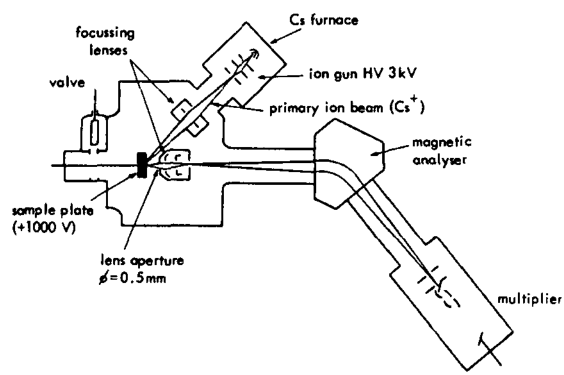

FIG. 8. Ion-sputtering mass spectrometer.

Figure 9 shows the mass spectrum obtained without any deposit on a clean sample plate and Fig.10 the overall sensitivity (mass-spectrum corresponding to a water sample containing 10^{-12} g enriched ^{10}B and 1.3×10^{-12} g enriched ^{6}Li.)

At this point, it must be stressed that the efficiency of the sputtering process varies very significantly from one element to another: under our experimental conditions, a solution containing equal amounts of ^{7}Li,

^{9}Be and ^{11}B will give rise to peaks with relative heights approximately equal to 80 : 1 : 0.1, but this is much more satisfactory than what thermionic emission would lead to.

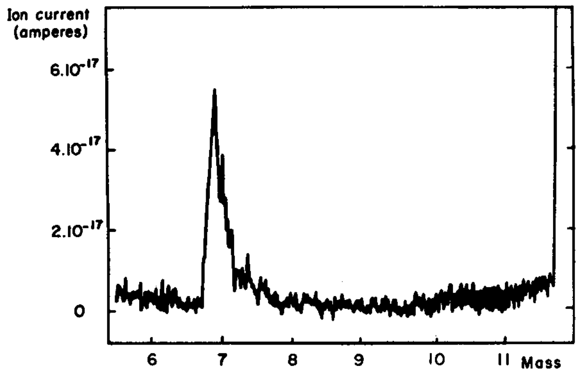

FIG. 9. Mass spectrum obtained without any deposit on a clean sample plate.

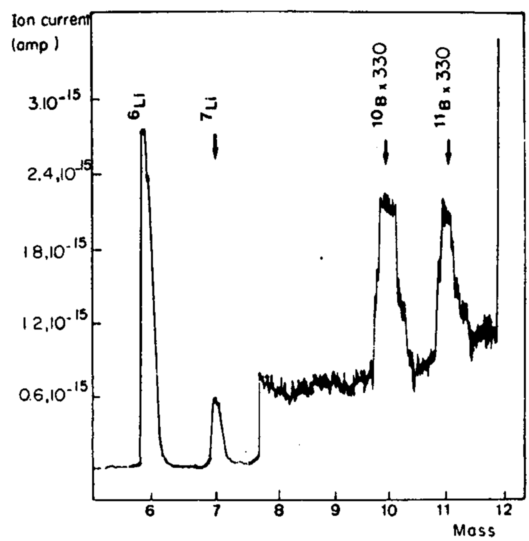

FIG. 10. Sensitivity as determined from a water sample enriched in ^{10}B and ^{6}Li.

The oxygen targets consisted of 30 g of very pure water contained in a platinum vessel 2 cm in diameter and 7 cm long. Proton irradiations were performed in external beams at 155 MeV (Orsay), 600 MeV and 19 GeV (CERN). After bombardment, 10 g fractions were concentrated to 100 mg which were then deposited on the metallic sample plate of the ion source, evaporated to dryness and analysed.

After many unsuccessful attempts involving various methods of preparation, water containing less than 10^{-12} g/g of either lithium or beryllium and less than 10^{-11} g/g of boron was prepared by fractional crystallization.

The water samples were handled in an argon atmosphere. While no contamination with natural beryllium could ever be detected during a complete manipulation, contamination with natural boron usually reached 10^{-10} g/g. Contamination with lithium was of the order of 10^{-12} g/g.

As a consequence, the proton flux in our experiments was always such that the amount of boron produced by spallation would be several times greater than that due to contamination.

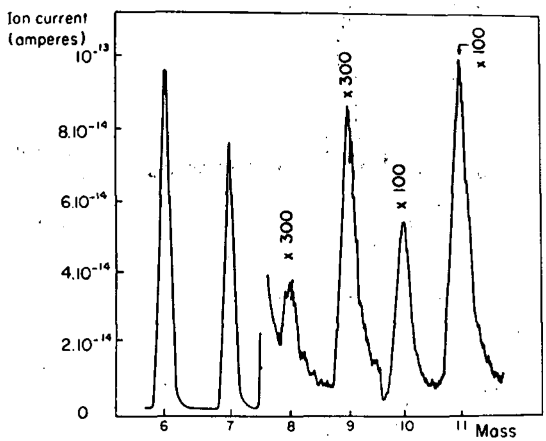

FIG.11. Typical spectrum of irradiated water.

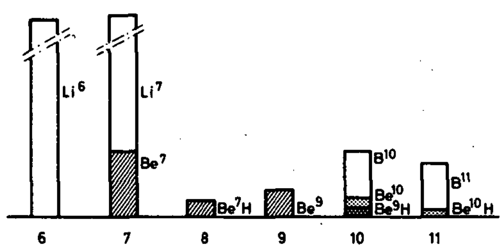

FIG.12. Contribution of various isotopes to the peaks shown in Fig.11.

Isotopic ratio measurements

A typical spectrum obtained after a 20-hour bombardment is shown on Fig.11, while Fig.12 represents schematically the contribution of the isotopes of Li, Be and B to the different peaks. That neither mass 8 nor mass 9 is present in unirradiated water samples containing only lithium and boron was already apparent from Fig.10.

The presence of a mass 8 peak in irradiated water samples is due to a peculiarity of beryllium which we have accounted for.

(1) Determination of the $^{7}Li/^{6}Li$ ratio

As we have pointed out earlier, lithium ions are emitted approximately 80 times more efficiently than beryllium ions from equal amounts of the elements. Hence, the contribution of ^{7}Be to the peak at mass 7 is only about 1% of the contribution of ^{7}Li and we have taken the ratio $^{7}Li/^{6}Li$ to be that of the ion currents at these two mass values except for this small correction of the mass 7 peak which we base on the height of the mass 8 peak.

(2) Determination of the $^{9}Be/^{7}Be$ ratio

Two possibilities exist of obtaining this ratio: one consists in relying on the BeH/Be value to deduce from the mass 8 peak the height of the peak at mass 7 due to ^{7}Be and comparing it to the peak at mass 9 due only to ^{9}Be. The other method is based on the possibility of eliminating selectively the lithium from the deposit made on the sample plate by the water sample. As this last method turned out to be easily performed, we preferred it. Thus after having deposited on the sample plate the 100 mg of irradiated water, and having dried and analysed it, we added a 10 mg drop of very pure water, which easily dissolved the lithium present and only a small fraction of the beryllium, and was then pipetted out.

In this way, the lithium contribution to mass 7 is reduced to less than 5% of the peak height, and this can be checked and corrected by the corresponding residual ^{6}Li peak at mass 6. The ratio $^{9}Be/^{7}Be$ is thus given by the ratio of the ion currents at masses 9 and 7.

(3) Determination of $^{10}Be/^{9}Be$ and $^{11}B/^{10}B$ ratios

We shall not relate here the method but suggest reference to Yiou et al. (see Ref. [1]). The final results at different proton energies are presented in Table I.

The most important results which we shall use are $^{7}Li/^{6}Li$ and $^{11}B/^{10}B$ ratios (which will include the ^{7}Li arising from the decay of ^{7}Be (54 d) and in the case of ^{10}B and ^{11}B include the contribution of ^{10}C (20 sec) and ^{11}C (20 min)).

The values indicated in Table II are derived from those of Table I but include the estimated effects of the presence of ^{12}C and ^{14}N in the "target" as well as a proton energy spectrum such as measured in solar flares.

We note from it that the spallation value for $^{11}B/^{10}B$ includes the natural value of 4 while the Li isotopic ratio is definitely far from it. Hence, we differ from the FGH model in that no flux of neutrons is required to explain the natural isotopic ratio but rather suggest that the nucleosynthesis of Li, Be and B may have occurred at the surface of the contracting sun.

This picture is similar to that suggested for Tauri stars. Li and B would be produced in ratios such that $^{7}Li/^{6}Li = 2.5$, $^{11}B/^{10}B \simeq 4$, but because of convective mixing at the surface the light elements would be brought down near the base of the convective zone where thermonuclear (p,α) reactions occur which burn ^{6}Li at a rate approximately 100 times

TABLE I. RESULTS OBTAINED FROM PROTON BOMBARDMENTS OF AN ^{16}O TARGET
Isotopic ratios, and absolute cross-sections in mb

Energy	$^7Li/^6Li$	$^9Be/^7Be$	$^{10}Be/^9Be$	$^{11}B/^{10}B$	^7Li	^6Li	^9Be	^{10}Be	^{11}B	^{10}B
155 MeV	0.85 ± 0.09	0.32 ± 0.04	0.21 ± 0.05	2.3 ± 0.4	8.5 ± 2.5	10 ± 2	1.7 ± 0.5	0.35 ± 0.2	25 ± 8	11 ± 3
600 MeV		0.37 ± 0.1	0.24 ± 0.05	2.1 ± 0.6	≤25	18 ± 5	2.4 ± 1.2	0.6 ± 0.4	25 ± 12	12 ± 5
15 GeV		0.34 ± 0.08	0.29 ± 0.08	<2.9			2.2 ± 1.1	0.64 ± 0.5		

that of ^7Li while neither Be nor B are affected. Hence the natural isotopic ratios $^7Li/^6Li = 12.3$ and $^{11}B/^{10}B = 4.0$ can be accounted for.

We should like to mention only briefly some other interesting results.

(a) It is well known that if Li, Be and B have a low universal abundance they are quite abundant in cosmic rays. It is generally assumed that the spallation of medium and heavy cosmic-ray nuclei colliding with interstellar matter (which is supposed to be essentially H) is entirely responsible for their production. Beck and Yiou [9] using the new measurement of spallation cross-section have calculated that the amount of matter traversed is 5.5 g/cm^2.

(b) The results have shown the ratio of cross-sections of $^{10}Be/^9Be$ in ^{16}O to be about 0.25 from 150 MeV to 19 GeV. The low value of this ratio will make it quite difficult to use the abundance of ^{10}Be (lifetime 1.7×10^6 years) to determine the cosmic ray's transit time from the source to the earth.

(c) The comparison of the cross-section ratios:
$Li/Be = (^6Li + ^7Li)/(^7Be + ^9Be + ^{10}Be) \simeq 2.5$ (Table I) with the value $Li/Be = 2.2$ measured in the cosmic radiations [9] shows very clearly that ^7Be is stable in high-energy cosmic rays. This, associated with the work of Lawrence and Levinger who calculated that ^7Be at 1 GeV/nucleon will not capture or lose an electron in less than several tens of g/cm^2 of hydrogen, means that the ^7Be nucleus has been produced naked from a mother nucleus itself naked.

CONCLUSION

The experiments performed in the last few years by mass spectrometry indicate clearly that this technique is now adapting quite readily to the problems which arise in the fields of nuclear physics as well as in nuclear astrophysics where data on isotope production at sub-nanogramme levels are required.

TABLE II. VALUES DERIVED FROM TABLE I BUT INCLUDING THE ESTIMATED EFFECTS OF ^{12}C AND ^{14}N IN THE "TARGET" AS WELL AS THE PROTON ENERGY SPECTRUM

	Spallation	Earth and Meteorites
$^7Li/^6Li$	2.5 ± 1	12.5
$^{11}B/^{10}B$	5 ± 2	4
Li/Be	25 ± 10	~50 (M) ~20 (E)
B/Li	2 ± 1	0.2

New interest in cosmic rays, recent discoveries or hypotheses in astrophysics (quasars, neutron stars, etc.), where we think nuclear reactions play an important role, open certainly a field for new developments.

We believe also that several laboratories which have no large equipment in nuclear physics can make definite contributions in such fields.

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DISCUSSION

A. HRYNKIEWICZ: I would like to mention the application of a mass separator for implantation of radioactive nuclei in ferromagnetic foils for magnetic moment measurements in a very short lifetime range. Very high magnetic fields of the order of several hundred kilogauss or even a few megagauss permit measurement of dipole magnetic moment in a picosecond range. The implantation technique with a mass separator has many advantages when compared with alloy formation or with thermo-diffusion. It gives well-defined and reproducible magnetic fields and it can be used for very short-lived isotopes as products from nuclear reaction where a mass separator is used on line with an accelerator. And what is important here is that this method works very well for rare earth isotopes, which are very interesting from the nuclear structure point of view, and for these isotopes the thermo-diffusion method or alloy formation do not work at all. We don't see any internal magnetic field acting on rare earth nuclei when the conventional methods are applied.

V.F. WEISSKOPF: I did not completely understand how far the measurements are in agreement or disagreement with the Hoyle-Fowler theory of spallation production of the light elements.

J. TEILLAC: There is a big disagreement.

J.O. RASMUSSEN: I would like to ask whether you think that the rapid diffusion out of the graphite is a consequence of radiation damage or the recoils going in.

J. TEILLAC: We don't know exactly. But lithium diffuses very rapidly in graphite because the lattice is very large. For the other ion the diffusion is a little longer but I don't really know why there is this difference and why it is so rapid.

INTERMEDIATE STRUCTURE EFFECTS IN NEUTRON-INDUCED FISSION CROSS-SECTIONS

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Abstract

INTERMEDIATE STRUCTURE EFFECTS IN NEUTRON-INDUCED FISSION CROSS-SECTIONS. The author discusses the intermediate structure in the fission cross-sections, in relation to the spontaneously fissioning isomers.

In 1964 Flerov and Polikanov discovered the existence of certain transuranic isotopes which decayed by spontaneous fission with anomalously short half-lives, many orders of magnitude shorter than seemed possible on the basis of nuclear systematics. The anomalously short half-lives were ascribed to the existence of isomeric states, and since then several of these isomers have been identified and their properties studied at Dubna and in Copenhagen by Bjørnholm and others.

These isomeric states were identified as belonging to isotopes of americium; they have half-lives of the order of milliseconds, and their principal mode of decay is spontaneous fission. The energies of the isomeric states have been found to be around 2 or 3 MeV above the ground state so that it is remarkable that they should not decay by gamma emission. Analysis of the shapes of the yield curves for formation of the isomers shows that their spins are not high, $\lesssim 7\hbar$, so that high multipolarity of all electromagnetic transitions cannot explain their stability against radiative decay.

Strutinsky has offered an explanation of the stability of these states. He considered the effect of the shell structure of the deformed nucleus on the liquid drop calculations of the potential energy of deformation of these heavy nuclei. Minima in the shell correction term occur where there are gaps in the single particle level structure near the Fermi energy of the system. For spherical nuclei these minima are most pronounced at the magic numbers, but the important property of the Nilsson diagram emphasized by Strutinsky is that other gaps occur at non-zero deformations, and such gaps recur with increasing deformation for a given nucleon number. Thus the calculations show that more than one minimum can occur in the potential energy of deformation of a nucleus and such a secondary minimum is expected to be strongly pronounced in the transuranic nuclei.

This secondary minimum provides a possible explanation of the spontaneously fissioning isomers. Such an isomer would be the lowest vibrational state in the secondary minimum. Its probability of decay by spontaneous fission would be enhanced, while decay by radiation would be inhibited because of its small amplitude in the normal minimum.

The existence of such a secondary minimum in the potential energy curve has been used by Weigmann, and more completely by Lynn, to explain some very interesting experimental results in the sub-threshold neutron-induced fission cross-sections of several nuclei. Very simply, one can postulate that two sets of levels can be built up for such a heavy nucleus. The first set (class I) are built on the normal ground state – the lowest vibrational state in the normal minimum, while the second set (class II) are built upon the lowest state in the second minimum (Fig. 1).

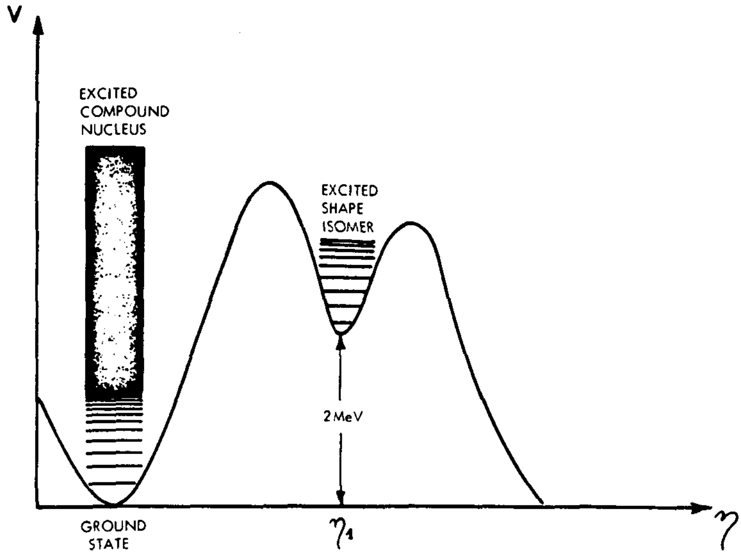

FIG. 1. Curve showing the two sets of levels.

When these states lie below the first maximum of the potential curve, they will be weakly coupled due to tunnelling through the barrier, the coupling being weaker the lower are the states. The class II states will have much larger fission widths than the normal class I compound nuclear states since only the second maximum in the potential will hinder fission in this case. Thus we would expect the compound nucleus (class I) states to exhibit very small fission widths except when they are in the vicinity of class II states with which they can mix. In other words, we expect to see well separated groups of compound states which exhibit relatively strong fission, and gaps between them where little or no fission occurs. This is exactly what has been observed experimentally.

Figure 2 shows the case of ^{237}Np where the phenomenon was first observed by Paya et al. at Saclay. The upper part shows the reduced neutron widths of the resonances over the energy range 0-100 eV while the lower curve shows the fission cross-section. It will be noted that little or no fission occurs above 50 eV or below 25 eV. Several such groups were observed in the cross-section, although only the first was fully resolved.

Figure 3 shows the data of Weigmann and Schmid from Geel, on the cross-section for neutron-induced fission of ^{240}Pu. These were the data which caused Weigmann and Lynn to postulate on the origin of this effect.

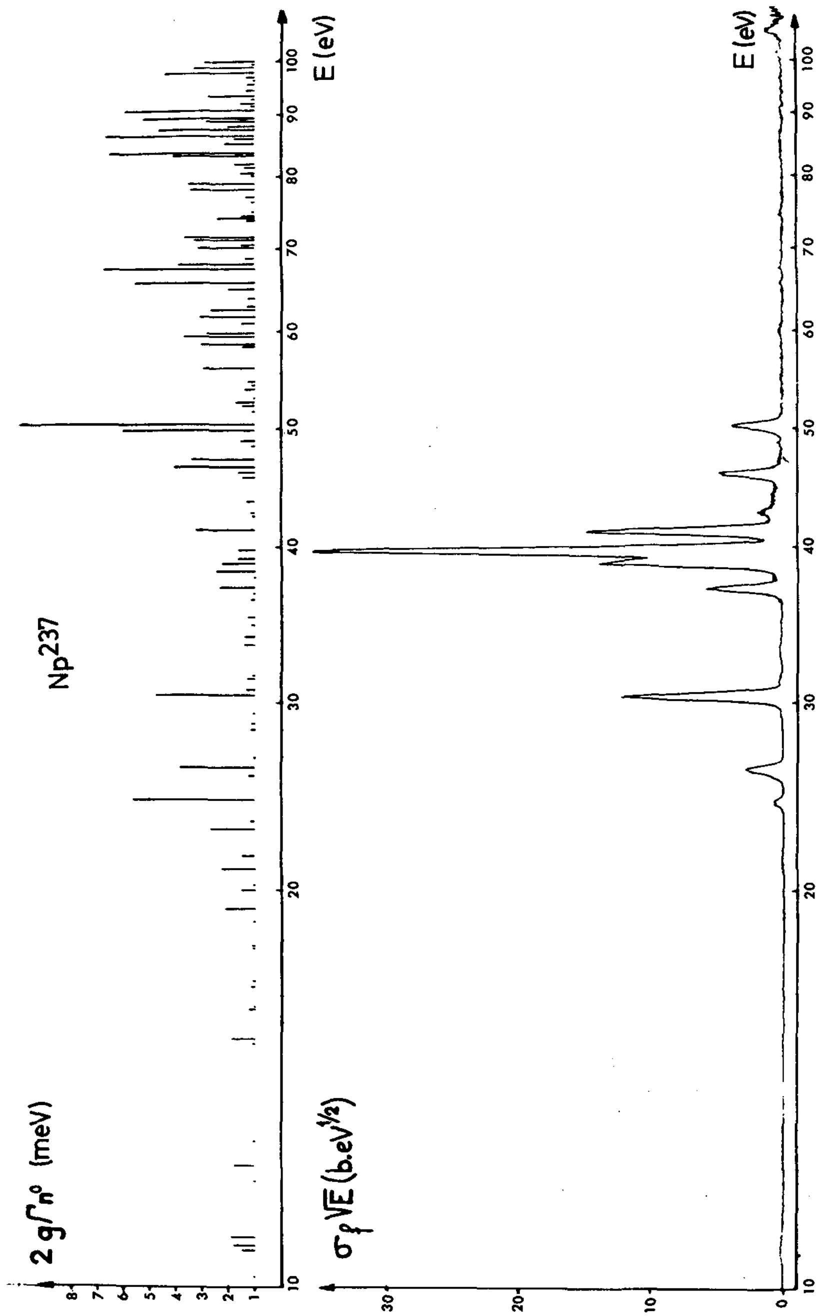

FIG. 2. Results for ^{237}Np

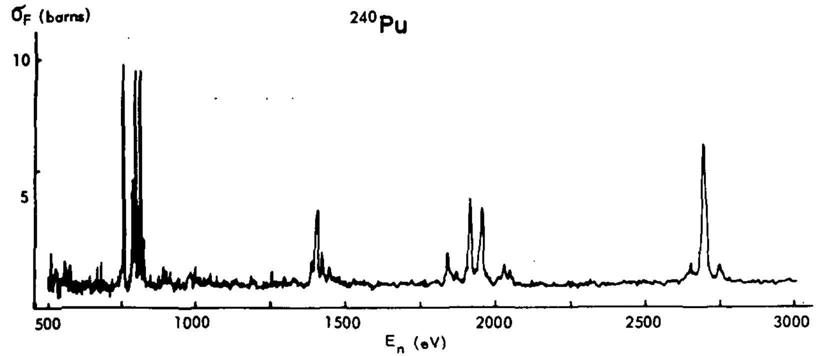

FIG.3. Results for ^{240}Pu

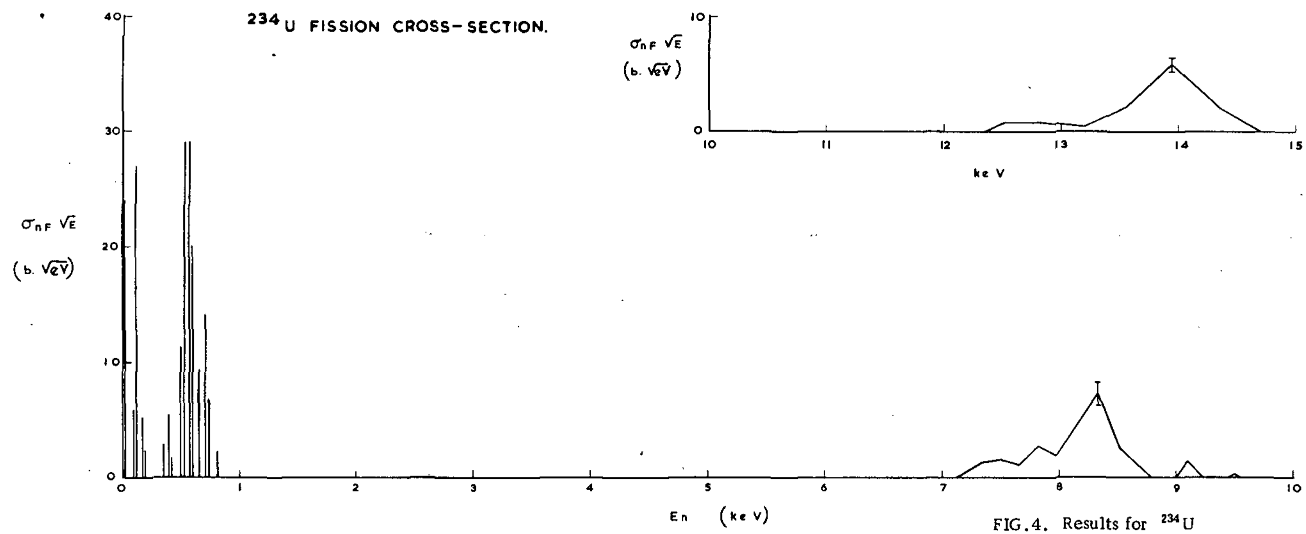

FIG.4. Results for ^{234}U

Here several groups of fissioning resonances are clearly seen, the spacing of the class II levels being ~700 eV compared with 13 eV for the class I resonances.

Figure 4 shows the results of James and Rae at Harwell on the fission cross-section of ^{234}U. Here a strong group is seen below 1 keV which is well resolved, and higher-energy unresolved groups occur at about 8 keV and 14 keV.

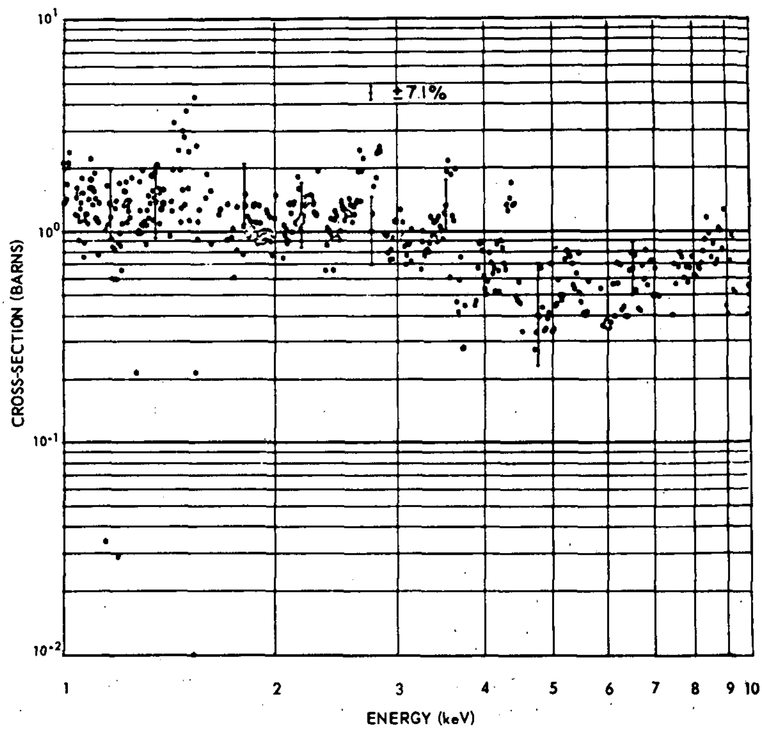

FIG.5. Results for ^{241}Am

An estimate of the energy of the lowest state in the second potential minimum can be made from the ratio of observed spacing of the class II and class I states, using a level density formula such as that of Lang and LeCouteur. This calculation gives for ^{238}Np 2.3 MeV, for ^{241}Pu 1.9 MeV and for ^{235}U 2.4 MeV.

A particularly interesting case is the fission cross-section of ^{241}Am, shown in Fig. 5. These data are taken from the nuclear explosion measurements of Seeger et al. and show a series of peaks in the cross-section with a mean spacing of about 1 keV. If these are assumed to correspond to class II states, then a level density calculation gives an energy for the class II minimum of 2.8 MeV. This agrees remarkably well with the energy estimated by Flerov et al. for the spontaneously fissioning isomeric state in ^{242}Am of 2.9 ± 0.4 MeV.

Thus it seems that the connection between the intermediate structure in the fission cross-sections and the spontaneously fissioning isomers is clearly established, and these phenomena suggest that the study of these and related problems in nuclear fission will be a fruitful field for future research.

DISCUSSION

V.G. SOLOVIEV: I would like to point out that Strutinsky's second minimum met a certain sceptical attitude from the very beginning. We are namely extrapolating the fairly large deformations and this minimum is based on the discharge of single-particle levels. However, a number of calculations, including those by Strutinsky, Pashkevich, and others, have shown that the second minimum is stable enough with respect to the change of the parameter, in the case of the Nillson, Saxon-Woods potentials. Apart from this, one might have feared that this mimimum does not exist. It may be caused by gamma deformation. For strongly deformed nuclei, there is no minimum because through the gamma you have a direct transition to the basic minimum. It turns out that the gamma deformation decreases the barrrier, but the second mimimum remains. On the other hand, it remains as some sort of fluctuation at the level of the average field for a fairly narrow region of the nuclei, such as in the region of uranium, plutonium and curium. Therefore, it would be very interesting to try, in an experimental way, to expand the area observed for these spontaneously fissioning isomers both to the heavier elements and to the area of the lighter elements.

J. TEILLAC: Are there any studies about the connection of the phenomenon of fission of deformed nuclei and fission of oblate nuclei? At what stage are neutron and gamma rays emitted? Further, is the fission of very ionized atoms the same as for atoms not ionized?

V.F. WEISSKOPF: I would say that the ionization should not have any influence on the fission, but I am not sure whether I am right. The second question: I believe that the gamma rays are emitted not by the deformed nucleus but by the fission products, in a state of very high excitation, and they may be polarized because there is a preferred direction in the fission process.

J. TEILLAC: It would be interesting to investigate this field.

R.K. SHELINE: I think Professor Teillac was referring to the possible competition between fission and gamma rays. You have two alternative processes. One is spontaneous fission and the other is a gamma transition back to the first minimum.

V.F. WEISSKOPF: That is something else, of course.

J.O. RASMUSSEN: Does the very nice explanation of Dr. Rae also explain why particularly the odd nuclei exhibit the millisecond isomerism, if the double minimum is a very general phenomenon in this region?

E.R. RAE: The double minimum appears to be quite general because, of the nuclei of which I showed the cross-sections, two were odd and two were even. I think that the reason for not observing the spontaneously fissioning isomers in other cases may be partly because fission may not always be the most probable mode for decay. Lynn has calculated that the lifetime for one of these cases is probably comparable for fission and for radiation, and it might be that in some cases radiation wins and therefore one doesn't see the fission. This is one possible explanation. Another one may just be that the lifetimes are too short to be observed. We have looked actually for uranium isotopes for lifetimes between a few nanoseconds and several seconds and have failed to see any fission but we intend to try to look for gamma-rays with the germanium protector. Also, the effect on the spectra of captured gamma-rays in the resonance region for heavy nuclei may be affected by the presence of the second minimum in that the gamma-ray spectrum would be quite different for decay of Class II states as compared with decay of Class I.

A. B. MIGDAL: I should like to show another example supporting the second minimum. Let us consider the transition to the deformed state in samarium. Adding only two neutrons, we obtain a very deformed nucleus. So we have here a kind of transition of the first order. Landau, analysing the phase transitions, showed that the only possibility of obtaining the first order transition is the curve with two minima. I think that from samarium one could be sure that there are two minima.

I. S. SHAPIRO: Modern nuclear physics is developed by proposing a model and then by testing this model by experiment. The main problem here is how to check the model experimentally. The situation in nuclear physics is such that we have different models, sometimes direct opposites, which try to explain the same experimental situations. What I would like to emphasize is that when we propose a model we must enumerate the crucial experiments which give us the possibility of distinguishing between different models. This is not really easy; it is a theoretical problem. When we say that we have now very many experimental data, we must remember that sometimes we get many experimental data which are not crucial for testing different models. When you ask about the crucial experiments then it is very significant and very important what kind of theoretical formulation you use to express the same theoretical idea. I will give some examples: the first one is rather far from the theme of this session but it is very well known. I mean the fine structure of positronium. If you take into account that the positron and electron can virtually annihilate and then reannihilate, then this virtual process gives rise to an exchange interaction between the positron and electron and this gives a shift of the levels of the positron. When you use quantum electrodynamics you obtain this effect automatically. You can also calculate this effect in the framework of wave-function formalism, introducing a charge variable and antisymmetrizing the wave-function of the system. It is, however, clear that when you use the quantum field formalism you obtain this effect automatically and you also obtain the possibility of predicting large numbers of experiments which suggest the hypothesis that the positron and electron are the same particle differing only by the charge. When you work with the wave-functions you must, in each case, invent new ideas to propose experiments. In this narrow field of fine-structure positronium these approaches give identical results. Let us now take another problem. What is the direct nuclear reaction? The main question in this field is whether the so-called processes are really direct or whether they are produced by a coherent action of many nuclei. It is not clear now, despite the fact that we get many experimental results, whether we can distinguish between such possibilities and we have to ask ourselves what are the crucial experiments for such tests.

The diagram method of nuclear reaction recently proposed gives a formalism which shows what are the crucial experiments to choose between these two possibilities - between coherent production of final particle and the direct approach. Many experiments that have been done are not crucial in this sense. In my opinion this is one of the examples which show that to obtain an adequate theoretical method for the physical features which form the basis of the model is an extremely important question. For these reasons I think that it is very important now to discuss between physicists who work actively in certain fields of nuclear physics, what is the most adequate method. One should remember that the number of theoretical physicists is now enormously large. When I started, for example, our Institute had two or three theoreticians and now we have three theoretical departments.

I want to make one further remark about the general position of nuclear physics. I agree with Professor Weisskopf that nuclear physics is different both from atomic physics and from the study of microscopic bodies. In this sense nuclear studies belong to the fundamental studies of physics.

When asking for money for nuclear physics from a man who occupies a high position in industry, you should not answer his question, "what is the purpose of studying nuclear physics"? by saying "I want to know what nucleons are." The right answer will be that you will study the nucleus and then we can produce the isotopes and the isotopes will help you to produce bicycles or shoes and so on.

The same approach should be used to the physicist who has a high position in elementary particle physics, when asking him to give you some money for studying the high-energy system, the mechanism of high-energy nuclear reaction. If he asks you "for what purposes", the wrong answer will be "I am interested in the mechanism of the nuclear reaction of high energy". The right answer will be "because I will study the interaction of elementary particles with the non-hydrogen targets only".

Ya. A. SMORODINSKY: I would like to make a short remark about direct interaction. In order to make a theory you have to know the amplitude of the scattering of nucleons by nucleons. In the case of the three-particle problem people have done a lot in reconstructing the amplitude from the experimental data; they have to perform a large number of different experiments, called the complete set of experiments, which include the measurement of polarization, correlation, and so on. In the case of two particles inside the nucleus, it is impossible to perform such kinds of experiments. But there is one example in which it is possible, and it must be done. It is a collision of the nucleon with nuclei; for example, the 2p reaction. Here we can prove that it is direct interaction. That is, we have a particle and we have two particles after reaction and we check by known means that it is in fact a direct interaction. But in this case we have to measure the correlation of polarization, the rotation of polarization and only in that case is it possible to reconstruct the amplitude in which 3 n's are free and 1 n is inside the nucleus. That means it is possible to measure the amplitude close to the surface of the nucleus. It will be an intermediate region between the free particle and the Migdal theory. It is very important to learn this amplitude, partly because Migdal says it is possible to build a theory of how the interaction between the two nucleons changes from the free interaction to the interaction inside the nucleus. An intermediate experiment must be done and I would like to ask experimentalists if it is really possible to perform the measurement of the complete set of experiments on existing accelerators.

I. S. SHAPIRO: I would like to stress that one should be very careful in interpreting the data on direct nuclear reactions and by using them for the determination of constants which determine the structure of a nucleus. First of all we should be sure that the assumed mechanism of the direct process virtually takes place. One should, for this purpose, perform a set of experiments, indicated by the diagram theory of direct reactions. Unfortunately at the present time there are only a few such experiments (reaction $^{6}Li\ (\pi^{+},\ 2p)\ ^{4}He$, investigated in CERN, and the process ^{12}C $(\pi,\ \pi p)\ ^{11}B$, investigated at the Institute of Theoretical and Experimental Physics in Moscow). In most cases the conclusions about the mechanism of a direct reaction are made with clearly insufficient data and this may lead, and actually does lead, to rough errors (the examples just mentioned above confirm this).

D.H. WILKINSON: I don't want to answer Professor Smorodinsky's question directly but I would like to make what I hope is a relevant comment, and that is on the general difference between knowledge and influence. I think more and more one has to go to high-energy experiments really to know things.) I think that one can perhaps make an influential answer to Professors Shapiro and Smorodinsky on the question of direction interactions, at least of a particular kind. One is used to thinking of direct interactions as processes in which the particles enter the nucleus, interact immediately and come out again, and as Professor Shapiro has said, it would be difficult to demonstrate this directly, and Professor Smorodinsky has called for a complete set of experiments to do it. However, one can do direct interactions without going into the nucleus and in that case I think one can infer that they are direct, even though one cannot demonstrate it. I am referring now to stripping reactions below the Coulomb barrier. In this case one can infer from the energy dependence of the cross-section that the proton in the (d, p) reaction has not been anywhere near the nucleus. The stripping process has taken place quite a long way from what we normally call the surface of the nucleus so I would say the agreement between the dependence of the cross-section on energy and what we calculate from the reaction mechanism, allows us to infer that indeed it is direct and does not involve a collective type of process.

FORUM ON SELECTED TOPICS IN NUCLEAR STRUCTURE

V.F. WEISSKOPF: We are now coming to the more general questions about the future of nuclear physics. The remarks I made earlier were just to open up the subject, and I shall now ask various members of the Panel to make their remarks.

J.O. RASMUSSEN: I should like to start by commenting on the significance of electron accelerator machines in nuclear physics. Owing to the somewhat unusual sorts of specialization the photon-nuclear and electron-scattering nuclear physicists, at least in the United States, have a rather separate tradition compared with others of us studying nuclear structure. From our side, we have a very great interest in the possibility by electron scattering of obtaining information in heavier nuclei of higher multipole moments or even monopole moments of excitation. It was, I think, from electron scattering at Stanford that one had the first ideas about the collectivity of modes as high as 2^4 or 2^5 pole. The high-energy scattering with wavelengths short compared with nuclear dimensions can give us higher moments of the multipole excitation than would be measured by radiated lifetimes where the proton wavelengths are long compared with nuclear dimensions.

I would like now to move to something rather more general, and then finally address myself to some of the practical questions of how one gets governments or businessmen or high-energy physicists to lend or give us money. Nuclear structure studies and nuclear structure theory in the rare-earth region of the deformed nuclei have generally come in for criticisms as being the sort of thing that is not likely to yield fundamental information. I think it is natural to rise to the defence of some of the theoretical and experimental studies in the region of the deformed nuclei, and perhaps I base my defence on the point that the nucleus is a good object for many-body theory and that something from our studies of many-body nuclear theory in the nucleus can be carried back to solid-state studies and to other fields that also use many-body theory. One of the great advantages of the rare-earth region of the deformed nuclei is that the shell model works better here than anywhere except perhaps one nucleon away from a double-magic nucleus The coupling schemes become relatively simpler when the degeneracies of orbitals in a spherical well are broken up and one has essentially only the two-fold Kramers degeneracy left. The deformed region has been particularly suitable in studying superfluid phenomena for applying the Bogolyubov transformation, and is perhaps most analogous to the metallic state one has essentially in this region - a rather uniform level density. In recent investigations we have found differences between the nucleus and the superconducting situation in a metal. The phase transition between the normal and superfluid state is not at all a short-phase transition.

If we take properly into account the conservation of particle numbers, quite unimportant for the large solid-state system, we find this phase transition to be a very gradual one. Perhaps these studies of the broadening of a phase transition with extremely small systems may be useful in other fields. The rotational branching patterns that are given by various transition processes in the deformed nuclei, contain a wealth of detailed information, details of the nucleon wave functions that are often hard to obtain in other nuclear regions. We are at present looking at the pairing phenomenon for fission saddle shapes; some experiments, some angular distributions and fissions, indicate that perhaps the gap is twice as large for saddle shapes around plutonium-240 and in the region of polonium the gaps may be three or four times as large, and this has raised some very interesting questions. It has made one think deeper about where the origin of the pairing interaction arises and that perhaps there are renormalization effects on the effective pairing interaction.

Finally, let me make a few remarks on this general philosophical question and practical question of why nuclear structure studies should be supported. Professor Weisskopf's analysis pointing out that our field lies between the intensive and the extensive connections is one that we should very much bear in mind. We must guard against pointing out how we can build better bicycles, though the temptation is there with governments. I think an automatic defence is that society should give significant support to creative intellectual activity of which this field is an outstanding example and that we must direct ourselves to the fundamental problems in these undergrounds of creative and intellectual activity. I think also that we have a defence in the important connections with other fields.

We have found our contacts in governments responsive to arguments that we are making greater efforts to co-operate with one another to see that the new or old facilities that we have are available not just to one or two men for their ideas but also to people in universities without nuclear facilities in the region. We must see in a period when not everyone will be able to build his own accelerator that we must look to ways for better co-operation, to temper the narrow competitive spirit that has often pervaded science.

D.H. WILKINSON: I would begin with at least a different narrow horizon, not that I think that what Professor Rasmussen has said is not absolutely right, but rather to present a complementary view. I feel myself that the question of science and what we consider to be worth doing is very much a matter of personal taste and perhaps we don't have the right as nuclear physicists to judge our own field. I think I can illustrate what I am saying in an extreme way by referring to Professor Weisskopf's classification of intensive and extensive sciences, the intensive ones being those that are self-sufficient by definition of creating new ways of thought or finding new attitudes which do not yet connect up with others, and the extensive sciences either feeding into other sciences or coming out of them or feeding into practical applications or perhaps coming out of them. If you ask workers in the intensive sciences for their view - at least the completely dedicated workers - they would say that their science is proper science. It is a science in which they are having to use their feelings; a science in which one is shaping the science through the nature of the human being. We cannot find more out about nature than we are capable of understanding and to a certain degree this perhaps means we are forming nature in our own image. So the workers in the intensive, as opposed to the extensive sciences, would perhaps say that theirs is an instinct science and the others are extinct sciences. That is of course an extreme point of view.

Now I come back to what I have already said, namely that I don't believe that we, in a certain sense, have the right to make a judgment about our own science because even if we decide that we have understood it all and that it no longer is a satisfactory activity for us, then surely there will be others for whom it is a satisfactory activity and for whom it is more an instinct as opposed to an extinct science. There is a serious point in this and that is that I feel that we should as it were make proper preparations recognizing this point that I have just made for handing on in due course. We know that it is very bad if one imperialist nation pulls out from colonial territories without making proper adequate preparations for the smooth transfer of power. I think we should do the same. We are becoming an extensive science. We are already having significant contacts with other sciences and I think this should perhaps be positively encouraged. I am not taking a pessimistic view here - I am not saying that our science is still not satisfying to us; it obviously is or we wouldn't be sitting around discussing it now. But I do feel that the closer the contacts that we can breed with what at the moment appear to be rather separate disciplines, the better. And I would say this goes both ways. I believe that we should, as I have already said, cultivate our connections with the high-energy physicists as well as cultivating connections the other way with such people as chemists and biologists. We can certainly benefit by strengthening our ties with high-energy people and they I feel can benefit too. There should be a serious attempt to introduce nuclear structure physicists into high-energy teams, not with any specific purpose of immediate benefit, but rather as a longer-term investment. The opportunity often has been lost, when a high-energy experiment has been carried out simply replacing the hydrogen target by a gadolinium target or something of that sort, of learning something of great use for nuclear structure physics with a very small percentage increase in the amount of running time. We are, of course, already in close contact with other disciplines that to some degree have come out of our own. We are in close contact with the solid-state disciplines, with biological and medical science, in obvious ways including relatively exotic and long-term ones such as the use of negative-charged elementary particles for local therapy, a possible approach to treatment of cancer, and so on. I think there is another respect in which nuclear physics can be useful. This remark applies also to high-energy physics and that is that occasionally, not very infrequently, we do seem to have a reason for applying a new technology on a large scale. I am thinking of two things in particular now: superconductivity and superfluidity. It is quite likely that in the long run large cryogenic superconducting devices will have industrial application. At the moment, industry has no excuse for putting millions of dollars into very large superconducting magnets. But nuclear physicists do and out of the experience of such work will come a demonstration of the feasibility or otherwise of applying these techniques on a very much bigger industrial scale. In other words, through nuclear physics one does occasionally have a reason for applying a technique on a semi-industrial scale. Superfluidity has not, so far, been applied in this way although there is the sensational application from Stanford University for a superconducting linear accelerator which will get rid of its energy by being totally immersed in superfluid helium, which has a heat transport of the order of 1000 times better than copper.

The final question is whether one should stay in and continue to press for nuclear structure physics. It depends on the size of the country. It may well be right for larger countries to put more of an effort into other sciences, perhaps high-energy physics, if we are sticking to the context of the nucleus, but at the same time it may be right for the smaller, less fortunately endowed countries to put a much bigger proportion of effort into nuclear structure work. That may well be their method of remaining in touch with the frontier of science.

I have tried, in these remarks, to present this complementary point of view to Dr. Rasmussen's, namely that we should deliberately look outside our own field, not because we feel that it is becoming dry and that we might wish to leave it, but because fields do develop (while tastes remain rather constant) and a given type of person with a given type of taste may well move from one field to another. In doing so he should recognize that others very probably will want to come into his own and he should take every opportunity of encouraging them to do so.

G. ALAGA: I should like to say a few words on the problem of nuclear physics as I see it from a developing country. It is probably fair to say that the time of the qualitative work in nuclear physics has gone and as Professor Brown and others have pointed out it seems we are facing a period when actually hard work has to come. If one looks at these qualitative problems one can see that every year a few more minor qualitative problems arise and are solved. This certainly cannot keep busy all the existing nuclear physicists. I think that one has to face up to the more difficult problems and try to find the correlations between certain data and events and then also try to find new approaches in order to get approximate solutions to the many-body problems. Those who are making new approaches should try to state their results in a much simpler and clearer way and then should try to make comparisons with the results of other research workers; one could then see clearly the similarities and differences between two different approaches. In this respect I think the organization of topical conferences might be extremely useful because more detailed discussions could be done at such conferences and one could make more comparisons of the data to find out the differences and the similarities.

A few additional words on experiments. I am not an experimentalist and therefore am not qualified to say much about this, but I have on a few occasions analysed completely wrong experiments and I am probably not the only one. When the theorists do a certain piece of work they should of course state as clearly as possible really what they have shown and what they have proved. The same should apply to the experimentalists; they should try to say what the experiment has really established and analyse the experiment as far as possible and not publish the wrong data many times; they should try to find out also the systematic and other errors. It is probably very useful to have a combined experimental treatment so that one gets evidence from different sides and one can thus establish the behaviour of the individual nuclei, the excited state, and so on, and also the properties as one moves along the number of the changing nucleons. This would probably help greatly in deciding which of the theoretical approaches has to be adopted and which one is the most promising.

In my opinion the theoretical physicists are often convinced of - I wouldn't say wrong - but not quite correct theories, and in this way they discourage the experimental physicists from doing certain investigations, and vice versa, the experimental physicists also sometimes think that some of the data are already established and thus they discourage the pushing on of the theory in a certain direction.

G.N. FLEROV: I should like to dwell on one particular question which we have discussed in the Soviet Union. It is not quite clear yet but I would like to draw your attention to it. I remember that when I was young and worked in the Physical Technical Institute under Academician Ioffe there was a whole school of experimentors studying secondary electron emission. We studied various metals, we had different coefficients from different experimentalists and it turned out later on that in these years, using the oil pumps, we had studied not the secondary emission of metals, but the secondary emission of oil films covering the metals, and the coefficient depends rather on the oil than on the metal surface. Those who were engaged in it know that the work of all those years was not valid at all. It seems to me that we have a similar situation in nuclear physics. I think that more than half the work, and in particular the experimental work, has been incorrect, that sometimes the experimental technique has not been sufficiently developed for the particular experiments and sometimes there were some very incorrect ideas. A young person who wants to acquaint himself with a given problem will have to go back to what has been done before, and nobody, of course, in papers, records that work done was not very successful. It is very good training for young people to find out the mistakes that the older workers have made and this will give them a very solid basis for later on. In Dželepov's tables on spectroscopy the spectra that we have performed since the year 1965 are included. Again I think this is wrong. Sometimes we omit some very important fundamental work. Therefore I think we should think about how we look backwards and make assessments to evaluate the work that has been done in the past. For example, for nuclear tables being established in the United States of America with all the references we should have all the data with a correct assessment; there are qualified people who could do this, and it would help us greatly in our further work. How this should be carried out in practice I do not know, but I think some work ought to be done in this direction.

V.G. SOLOVIEV: I agree with the statements that have been made in the discussion by Professors Weisskopf, Rasmussen and others. I can't quite agree with the statement by Professor Flerov. I hope as a physicist that we are getting objective information on nuclear structure, but perhaps not always very clear or not absolutely accurate. I still hope that the experimentalists are measuring the nuclear properties and not something else. I should like to stress that it appears very important for the development of nuclear physics to have a very wide front of research and investigations. I believe that this is also true for the theory of the nucleus.

I should like to say a few words concerning the problem connected with the fundamental importance of nuclear physics. The importance of nuclear physics investigations would be considerably decreased if it were possible to describe all properties of the nucleus on the basis of nucleon-nucleon interactions. I believe this would be a very effective direction if we wanted to explain the properties of complex nuclear systems from elementary interactions and principles. I think, however, that the nucleon-nucleon interaction gives us only limited information on the forces involved and such a many-body system as the nucleus could give us complementary information. When we analyse the nucleon-nucleon potential then we see that even in the perturbation theory we cannot reduce all graphs to the nucleon-nucleon potential. Dealing with the three-body system we understand that the concept of potential is only relative. Having four bodies it seems that the difficulties are almost insurmountable and it is probably very important in this connection that we investigate a complex nucleus. Essentially we are giving up the idea of the primary nucleon-nucleon interaction. Investigating a complex system we are looking at lowly-excited states and we are limited to a small degree of freedom, and we build a model which will make it possible to understand to some extent the structure of the nucleus. The most important thing is that the fundamental results are the results of the properties of symmetries, spherical symmetries or the axial symmetries in deformed nuclei.

A good example of this is when we have multipole-multipole interaction, and a surface Δ interaction produces exactly the same results, although the radial dependence of the interaction is rather different.

In the future development of nuclear physics a very great role will be played by hypernuclei. Perhaps very shortly nuclear physics will develop into some kind of hypernuclear spectroscopy.

A.H. WAPSTRA: There is one point that I have not yet heard in this discussion about the possibilities of nuclear physics and about the arguments that we can use, let us be honest, to get money. This one point has to do with the education of new physicists. It has been said in the Netherlands by Professor Kasimir, who is Director of the Philips laboratories, that some of the best physicists he got came from nuclear physics. They are no longer working in nuclear physics as such - they are working in branches like solid-state physics or electronics or computer techniques but he was of the opinion that in nuclear physics the high quality of the education was such as would only with difficulty have been obtained in any other branch of physics. This is an example of nuclear physics moving in the direction of an 'extensive' subject, in Professor Weisskopf's terminology.

I have personally also found out that there is the possibility in the other direction. Under my direction there is at the moment a group working in CERN on the scattering of negative kaons on polarized protons, and of this group of four physicists three have come from nuclear physics. We should make these facts known when arguing about the usefulness of nuclear physics.

V.F. WEISSKOPF: It is certainly a very important fact that nuclear physics is probably one of the best training grounds of all fields.

We have, in our discussion, avoided certain dangers. In science one must always be careful not to plan too much and not to plan too little. Some planning is usually necessary and each researcher himself must make a choice between one experiment and another; even the limitation of time forces us to make such a choice, not to speak of the limitation of financial support. On the other hand it must be very dangerous to do too much planning. One of the great strengths of science is the individual initiative - the individual independence of the research workers. Each scientist must decide for himself what he thinks to be important and if such judgment is different in all the groups the better. That makes for scientific progress. In fact I am a little worried about the large number of international meetings which prevents the different nationalities from developing in their own special fashion. It might perhaps be better for science if we had fewer meetings, not only because we lose so much time but also because we would work a little more independently from each other. Probably on the whole the advantage of having a lot of communication outweighs the disadvantages if we are strong enough characters to do what we want to do ourselves and not listen too much to what other people say. I think this discussion has shown that we do have such strong characters, and all the better.

ORGANIZATIONAL ASPECTS

PROBLEMS OF THE DEVELOPMENT OF NUCLEAR PHYSICS IN SMALL COUNTRIES (AS EXEMPLIFIED BY CZECHOSLOVAKIA)

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Abstract

PROBLEMS OF THE DEVELOPMENT OF NUCLEAR PHYSICS IN SMALL COUNTRIES (AS EXEMPLIFIED BY CZECHOSLOVAKIA). The author treats of Czechoslovakia as typical of the smaller European industrial countries, summarizes the country's difficulties in keeping abreast of nuclear physics developments, and suggests ways in which the smaller countries can play their part in the development of nuclear physics.

It is to some extent risky to look for common and general features that would characterize a number of countries in such a narrow sphere as the development of nuclear physics. I shall take it for granted that such features do exist in small European countries. This assumption is based on the fact that small European countries have a common history and similar geographical as well as cultural conditions. As far as nuclear physics itself is concerned, it is, of course, necessary to exclude a few countries, such as Denmark, on account of their great tradition. All I can hope for is that everything that I say will at least concern Czechoslovakia.

It is therefore perhaps of interest for countries of the same size, similar history and the same hopes to mention here the paths that the development of nuclear physics has taken and is still taking and, in passing, to indicate ways in which it may develop in the future.

I shall start by giving some general information about Czechoslovakia, since it is important for the understanding of nuclear physics in the country.

Czechoslovakia has long been an industrial country. At first glance this fact would seem favourable for the development of natural sciences, among which one includes physics. This really was true in some scientific disciplines, namely those on which industrial production was directly dependent. However, people managing industry and working as qualified engineers and technicians often think of physics, especially classical physics, more as some abstract study which is, of course, necessary for the good basic training of a qualified engineer, but which is, for a man concerned with economic problems, too broad and theoretical an activity, and on which it is not worthwhile spending too much time and more money than necessary. Why this is the case can best be explained by taking nuclear physics as an example.

Nuclear physics began to be known to the public towards the end of the Second World War, in connection with the atomic and hydrogen bombs. In spite of, and possibly just because of this, nuclear physics as a new and important scientific branch did not find any response amongst the Czechoslovak public.

Moreover, the high costs mentioned in the world press as necessary for the preparation of nuclear weapons also had a negative influence. The situation began to change when it was gradually realized that the useful exploitation of nuclear energy in the form of electrical energy was possible. This was the period following the first International Conference on the Peaceful Uses of Atomic Energy held in Geneva in 1955, and when the first nuclear power plants were being put into operation. At that time much information became widely accessible, showing the results of a long period of work by large nuclear physics groups in a number of countries. For people thinking of using nuclear energy for industrial purposes this fact was overlooked since their time was fully taken up by organizational, technical and technological problems. Nuclear physics was rather an unpleasant obstacle, diverting attention and exhausting means. Only that part of applied nuclear physics was appreciated which was directly connected with the construction of reactors, i. e. today's reactor physics. Actually, the situation amongst nuclear physicists was just the opposite: they were attracted by basic research in nuclear physics. The primary and literally historical merits of nuclear physics in the field of the use of nuclear energy were lost or unrecognized; only a moral debt was repaid to nuclear physics, and then often on the advice of more experienced and far-seeing friends from abroad. When defending the needs of adequate research in nuclear physics it was necessary to use such arguments as elementary terms, and in fact existing technical realities currently used today in reactor techniques and in the nuclear power industry would not have been possible without precise and exacting research in nuclear physics. These are roughly the reasons why, in certain situations, even the high industrial standard of a country does not act as a stimulus to the development of scientific work in some branches such as nuclear physics, but rather in the opposite sense. It is thus the duty of each civilized country to participate in this field according to its means.

There are a number of other reasons for the unfavourable forces working against the development of some of the exact sciences. Czechoslovak cultural tradition is rich in the sphere of the arts and some of the humanities. It is closely connected with efforts for national revival and independence. This tradition arises more easily as it is less conditioned by material means and perhaps also is not so ambitious as far as material and personal conditions are concerned. At the same time, however, society subconsciously regards culture as concerning only the arts and humanities and this attitude does not create good conditions for the development of other sciences.

Under these circumstances the only possibility of ensuring the development of such a science as nuclear physics is by the existence or creation of scientific institutions supported both materially and morally by the state. Such institutions may manage the majority of all scientific institutes of different types in the country. They must, however, be directed by scientists, as only in this way can an adequate and mutually balanced development of different scientific fields be achieved, naturally under the condition that their management is democratic and that in the sphere of intellectual activity they are independent of the state.

With such an arrangement for controlling the development of science, the basic problem is how to determine the adequate development of individual scientific branches in the given country and how to distribute funds so that the development of science as a whole is balanced. This problem is important not only in small countries but also for the great powers, as is demonstrated in the excellent paper by Weinberg, "Criteria for scientific choice" [1]. The problem for small countries is more difficult in that they must maintain certain contacts with big science and at the same time cultivate some scientific branches which due to their high costs either are or represent for them big science. Low-energy nuclear physics has unfortunately been forced into this position even if – in my opinion – it is in fact not big science at present.

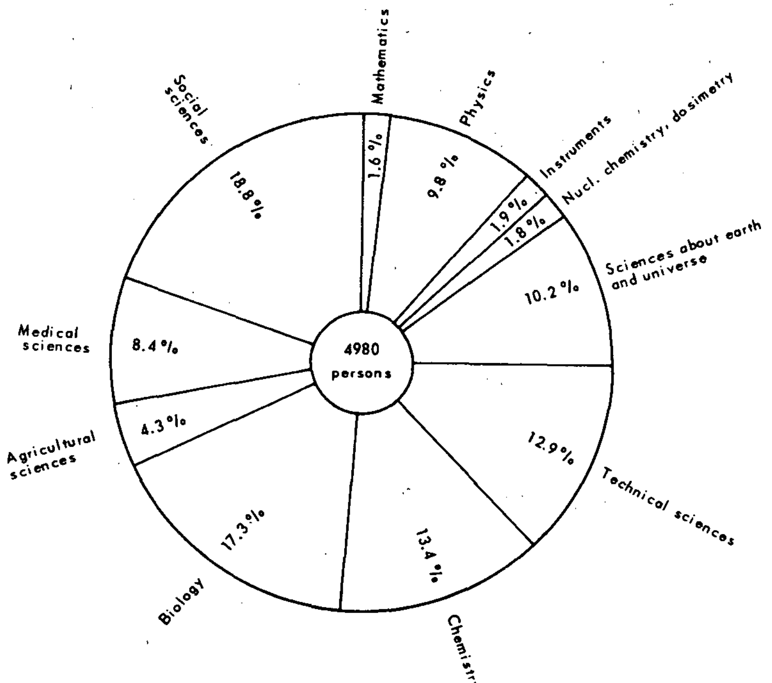

FIG. 1. Distribution of scientific and technical workers of the Czechoslovak Academy of Sciences according to individual scientific disciplines in 1967.

In spite of various good theoretical efforts the problem of an adequate and balanced development of different scientific disciplines in our country has been solved in a simple way. The Czechoslovak Academy of Sciences, established in 1952, was to take charge of this development so that it would correspond to the requirements of modern society. Support was given primarily to those disciplines which had the greatest tradition, or whose representatives held more important positions as experts and socially, or which had groups of young, enterprising and enthusiastic scientific workers. The first two factors had about the same weight or were dependent upon each other, the third was of smaller influence. These initial conditions were followed by a more or less equal development, with some exceptions. Such an exception was the more intensive development of nuclear sciences after 1955 that was instigated by Soviet physicists. The same positive role in high-energy physics was played by the foundation of the Joint Institute for Nuclear Research in Dubna in 1956; after that high-energy physics began to develop more rapidly for a few years. The present situation is shown in Fig. 1, where we see the number of people [2] working in individual scientific disciplines in the Czechoslovak Academy of Sciences in 1967. A similar picture is obtained when we take into consideration all people working in these branches on the basis of the so-called State Plan of Basic Research, not only in the Academy but also in other institutions (universities, industry, etc.). The absolute number of all workers is about twice as high and the share of the last three sciences increases: in agricultural sciences to 7.3%, in medical sciences to 22.5% and in social sciences to 23.6% [3]. The share of physics, unfortunately, thus decreases from 9.8% to 5.7%. The distribution of physical sciences in the spectrum of physical branches is plotted in Fig. 2. Here, there is no perceptible difference when one includes all people working according to the State Plan of Basic Research but the absolute number of workers is increased by about 90 persons only. Considering that the above-mentioned numbers of workers partially include also technical and assistant staff, whose number is higher in physical disciplines, the situation is not very favourable.

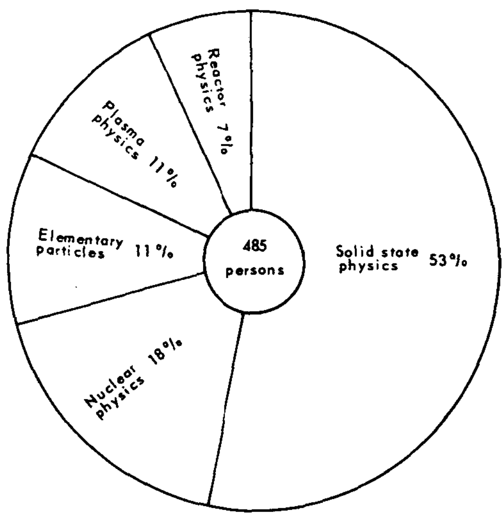

FIG. 2. Distribution of scientific and technical workers of the Czechoslovak Academy of Sciences according to individual physical disciplines in 1967.

The basic equipment employed in Czechoslovakia in the Nuclear Research Institute, which determines the profile of work in nuclear physics, corresponds to the average level of the 1950s. It is as follows: a classical cyclotron, accelerating protons to 6.7 MeV, a reactor having an output of 4 MW, a linear accelerator of the Van de Graaff type designed for 5 MV, and a few magnetic beta spectrometers. In recent years, this equipment has been complemented by a helium liquefier, a multiangular magnetic analyser of nuclear reaction products, solenoids for strong magnetic fields, and a GIER computer. These instruments make it possible to work in some spheres of nuclear beta- and gamma-spectroscopy, oriented nuclei studies, neutron physics and nuclear reactions. For such studies to have an adequately high standard and rate of progress, they require good recording and detection devices, and evaluating facilities such as semiconductor detectors, multichannel analysers, control panel computers; of these, we have some multichannel analysers of acceptable quality and top-level germanium detectors. Unfortunately, these instruments – as is true of electronics as a whole – are being developed more rapidly than the basic equipment of nuclear physics, and because of their complexity they are becoming more and more expensive. Thus it is often questioned whether it is worthwhile for small countries to work in nuclear physics at all. One solution offers itself: to work in theoretical physics only where the main activity is ideas – which cost nothing. This solution cannot be accepted either. Theoretical physics today requires computing equipment of high quality for the interpretation of experimental data and this – like the instruments – is also very expensive. And finally, this solution only increases technical retardation or even technical backwardness and thus makes the small countries unfavourably dependent culturally.

The pessimistic tone of the foregoing reflections concerning the development of nuclear physics in small countries may be dispelled by the conviction, already expressed, that low-energy physics is quite accessible for small countries. As for the future, it is important to find a form of development which will lead or keep it on a high expert level and thus remove the above-mentioned doubts. First, it is necessary to choose a certain trend which would comply with this requirement.

A promising discipline of low-energy physics is the field of nuclear reactions, by means of which it is possible to construct a large number of nuclear states and thus to study the manifold properties of atomic nuclei. Evidence of this is given by the discovery of analogue states of nuclei and the amount of information being obtained by the investigation of the inelastic scattering of particles in nuclei and by reaction with the transfer of particles (stripping, pick-up). Here, one can start out from classical nuclear spectroscopy which, in small countries, has been relatively the most developed. Sources of necessary particles with small energy spread in a beam are offered today by linear tandem accelerators. Unfortunately, these accelerators are not yet available in socialist countries and, moreover, the energy of the accelerated particles is rather limited from above. Therefore, great attention is being paid to the progress of cyclic accelerators with variable energy of accelerated particles. The upper limit of energies of accelerated particles is continually rising, reaching about 100 MeV for protons; at the same time, it is not impossible to construct a cyclic accelerator which would reach the same energy spread in the beam as the tandem accelerator, i.e. $\Delta E/E \approx 10^{-4}$ for protons, with a rather strong intensity of particles in the beam.

A great obstacle in the way of obtaining such an accelerator may again be its price. However, this problem might be most easily solved for Czechoslovakia if the Joint Institute for Nuclear Research, which is also engaged in low-energy physics and of which Czechoslovakia is a member, were to be equipped with such a device. If the interests of individual members of the Institute are respected, it would be best if such an accelerator were located in one of the national centres of nuclear physics and as close as possible to individual national centres. If this suggestion could not be realized for some reason, I am of the opinion that a few small neighbouring countries could collaborate and buy the accelerator and operate it together. After all, this solution seems to have the best prospects for the future. Building up and concentrating all the unique instruments in one place eventually results in an excessively complicated administrative problem in that place, and serves to isolate national centres from the devices and hence, to a certain extent, preserve the countries' backwardness. In low-energy physics it may be perhaps more advantageous to build up national centres with the participation of neighbouring countries so that each would gradually have a certain unique device, thereby complementing each other. Experience from the 1950s speaks in favour of such a procedure since in a number of countries at that time national centres came into being simultaneously with nearly the same equipment. Although the significance of the foundation of such centres for individual countries has for the time being been rather underestimated, on the other hand the centres were obliged for some time to engage in the same physical problems and thus their common effectiveness decreased.

In my comments I could have discussed other branches of nuclear physics, for example I could have talked about the great importance being given to the construction and investigation of oriented systems. However, in treating of the future of nuclear physics in Czechoslovakia, I chose to mention only nuclear reactions, or rather, the accelerator. I did so on purpose. This is because the question of having the necessary basic equipment and instruments is the decisive factor for the future of research in nuclear physics. The number of people working in nuclear physics is roughly sufficient; we count on it increasing by about 3% per annum in the coming years. In the sphere of personnel matters, there is one problem which may be common to small countries and that is the exchange of workers among various institutes and institutions inside the country. With few institutes and few applied nuclear physics posts in industry, the possibilities for interchange of workers in this field is very limited.

Reflections on the development of nuclear physics in small countries can hardly be concluded by definitive statements since nuclear physics must be considered in context with other sciences and with the life of society in general in such countries. In addition, it is closely related to the development of nuclear physics in the large countries. In concluding, I should especially like to draw your attention to this fact and at the same time ask the large countries to be more aware of and to have more understanding for the development problems of nuclear physics in small countries.

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ROLE OF INTERNATIONAL SCIENTIFIC CENTRES IN PROMOTING NUCLEAR RESEARCH IN DEVELOPING COUNTRIES

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Abstract

ROLE OF INTERNATIONAL SCIENTIFIC CENTRES IN PROMOTING NUCLEAR RESEARCH IN DEVELOPING COUNTRIES. Economically underdeveloped countries are primarily interested in research work which will yield an immediate economic effect. However, such work can be successful only if based on a sound theoretical scientific foundation, and this cannot be attained without developing fundamental research. For the developing countries, nuclear physics, with the rich promise attending the use of its results, is one of the most desirable branches of fundamental research. The development of theoretical physics is of importance too since it contributes greatly to raising the level of experimentation and university and secondary school teaching. Scientific research, however, particularly in nuclear physics, comes up against great difficulties in underdeveloped countries, e.g. limited finances, a weak technological infrastructure, shortage of qualified scientists, and a low level of training. The problem of making progress in nuclear physics in developing countries may partially be solved by participation in international research centres. This possibility is discussed on the basis of experience obtained by the Joint Nuclear Research Centre at Dubna.

I should like to deal with certain features demonstrating the important role played by international scientific centres in contributing to the progress of science in different countries, particularly in the so-called developing nations. I shall be dealing mainly with the Joint Institute for Nuclear Research but I think that those examples and situations which I mean to talk about can be related more or less to other international organizations of the same kind.

THE NEED FOR FUNDAMENTAL RESEARCH IN ECONOMICALLY BACKWARD COUNTRIES

Economically backward countries are primarily interested in developing such research work as will yield an immediate economic effect, the fields chosen for applied research often being largely determined by the particular historical development of the state in question. However, such research can only be fruitful if it is based on firm theoretical scientific foundations, and these cannot be attained without developing fundamental research.

The need for fundamental research, even in developing countries, is not doubted by anyone. Professor C.F. Powell has stated in this connection: "A country not engaged in any branch of advanced science runs the risk of remaining outside the mainstream of human development with all the serious consequences for its intellectual life and its productive force which that implies".

For developing countries one of the most desirable fields of fundamental research is nuclear physics, given the rich promise attending the use of its results.

The development of theoretical physics likewise plays an important part and makes a major contribution to raising the level of experimental work and university and secondary school teaching.

DIFFICULTIES IN ECONOMICALLY RETARDED COUNTRIES

Scientific research work, particularly in nuclear physics, runs up against great difficulties in underdeveloped countries. In our time enormous financial resources and the efforts of a large team of scientists, engineers and technicians are needed in order to carry out scientific research, but the limited financial capacity and comparatively weak technical infrastructure of underdeveloped countries prevents them from buying expensive modern scientific equipment or, a fortiori, building it themselves.

The lack of qualified staff and the inadequate level of training is also a serious handicap. Even if a certain number of quite well qualified specialists is available, the absence of an experimental basis and other favourable conditions for scientific work leads to their departure for other fields of science and industry, i.e. their de-qualification and, in certain countries, to a "brain drain". Other difficulties present themselves too, such as the absence of technical skill and experience in research work and in the selection of the problems to be tackled.

Unfortunately it often goes beyond the capacity of the country itself to solve all the problems involved in developing nuclear physics. Therefore it becomes necessary for such countries to discover a way out and to bring about more favourable conditions for research work in as short a time as possible. I would like to refer to the great support given by the International Atomic Energy Agency to developing countries, particularly in training and raising the qualifications of specialists from these countries, in organizing scientific conferences, and in the exchange and supply of scientific information.

The moral duty of the leading developed countries is to give every stimulus to the development of scientific research in technically retarded nations. Furthermore, advanced countries should give both intellectual and material help to developing countries. Such help can take many forms, one of them being multilateral international scientific co-operation, for example in international research centres.

JOINT INSTITUTE FOR NUCLEAR RESEARCH

The Joint Institute for Nuclear Research is one such centre, founded in March 1956 on the basis of an agreement between the governments of a number of socialist countries. The Institute's objectives, as stated in its Charter, are:

"- to provide for theoretical and experimental research in the field of nuclear physics to be carried out jointly by scientists from Member States of the Institute;

- to contribute to the development of nuclear physics in Member States of the Institute by the exchange of experience and the results of theoretical and experimental research;
- to maintain relations with interested national and international research and other organizations with a view to developing nuclear physics and seeking new ways for the peaceful application of atomic energy;
- to contribute to the overall development of the creative abilities of research workers in Member States of the Institute."

The area covered by research carried out at the Joint Institute is quite wide. It comprises many different kinds of investigations undertaken in modern theoretical physics and many different kinds of experimental work involved in studying the atomic nucleus and elementary particles. In this way, the subject matter of the theoretical and experimental research work performed at the JINR meets the needs of a large body of physicists from Member States. While, in the field of high-energy physics, research work is carried out mainly at Dubna, in co-operation with groups of scientists from Member States, in nuclear physics more of the work is done in the individual Member States, though this work in turn is closely linked to the work done at the laboratories of the Joint Institute. The Institute, to a certain degree, co-ordinates nuclear research in Member States, for example by organizing conferences and meetings of scientists from different countries.

These efforts towards co-operation and collaboration arise from the fact that in our time science has to be organized on an international basis and can only be carried through by teamwork; this, I think, is felt everywhere, including the Member States of our JINR.

The participation of physicists from countries such as Mongolia in the JINR gives them access to such installations and facilities as the proton synchrotron, synchrocyclotron, the multiple-charge ion accelerator, the IBR pulsed reactor, and bubble chambers. It is self-evident that this is the only possible way for countries with limited economic potential to join in "big-league" science. Giant modern accelerators or large bubble chambers, computers and automatic instrumentation are difficult to set up, even with the joint efforts of several small countries.

TRAINING OF SPECIALISTS

If the development of science is a basic problem for international organizations like the JINR the training of specialists is no less an important result of their activities. The education and intellectual maturation of scientists is a sine qua non for the further successful progress of science and therefore the appearance of a great and talented scientist is of no less significance than the setting up of an experimental installation.

Along with well-known figures in the scientific world, the Member Countries send to Dubna their young specialists to work alongside more experienced colleagues and acquire the habit of independent work; on returning to their countries after a few years, these younger men show themselves capable of heading scientific teams there.

While posted to Dubna, scientists from Member Countries have written 40 doctoral and 255 M.Sc. theses.

By now more than 700 scientists, about 200 engineers and 150 auxiliary technical workers from Member Countries of the Institute outside the USSR have worked at the JINR and improved their qualifications.

Many examples could be given of how individual scientists gained international prominence by the work of the Institute.

No description of Dubna's importance in the training of specialists would be complete without mentioning the fellowship scheme established by our Institute. Fellowships have been established at the JINR for theoretical physicists, experimental physicists and scientists in other fields, mainly for developing countries not Members of the Institute. A number of physicists have already made use of this opportunity. The governing body of the Institute has placed three fellowships at the disposal of the International Atomic Energy Agency, and at its recommendation physicists have been coming to us from developing countries like Pakistan, the UAR and Yugoslavia.

JOINT PROJECTS

Research projects carried out both at Dubna and in the research laboratories of Member Countries, using the same experimental material, are known as "joint projects". The number of these projects is growing all the time and they occupy a major part of the Institute's scientific program. For terms of comparison two figures can be cited: in 1962, 32 such projects were carried out, but by 1967 this figure had grown to 203. This indicates the widening links of the Institute's laboratories with scientific organizations in Member Countries.

Those who carry out and direct many of the joint projects in the different countries are those very scientists who were trained at Dubna and there acquired the necessary skills and knowledge.

A number of countries owe it to Dubna that they have been able to establish their first high-energy physics laboratories, like the one in Bulgaria at the Institute of Physics of the Bulgarian Academy of Sciences, one at the Institute of Physics of the Mongolian Academy of Sciences, and others in other countries. At the same time joint projects assist in the execution of the JINR's own scientific program and attract more scientists to work on these problems.

SCIENTIFIC CONFERENCES AND MEETINGS

I should like to make some mention of yet another activity of the JINR contributing to the development of science in Member Countries. This is the convening of scientific meetings on topical problems in science and methodology. In an attempt to meet the desire on the part of scientists from Member Countries to develop their links and joint activities further, to exchange information and decide where further work is needed, the JINR holds a large number of scientific conferences and meetings annually. Naturally it is easier to organize such meetings in international centres like Dubna than in any one single country. Twelve to fifteen conferences are organized at Dubna annually. For a country like Mongolia, where science is only in its developing stage, these conferences are very useful. One of the forms taken by them is the international seminars organized by the JINR on a systematic basis. Furthermore, our Institute organizes each year about ten sessions of different committees like the ones on the use of bubble chambers, photo-emulsion, nuclear physics and neutron physics, which enable the efforts of Member Countries in any given branch of scientific research to be co-ordinated.

Experience has shown that short-term visits by scientists from Member Countries to Dubna to discuss results of joint work and visits of JINR scientists to Member Countries for advice and assistance are of very great importance for the development of institutes in the different countries. The number of such visits rises annually. For example, 307 specialists from Member Countries visited Dubna in 1967, whilst 252 JINR scientists visited Member Countries.

CONCLUSIONS

The experience acquired in the more than ten years since JINR was founded is indicative not only as an example of international scientific co-operation but also as a form of effective assistance and co-operation on the part of advanced countries in the interests of scientific research at small institutes and in developing countries.

International scientific centres of the Dubna type make it possible for scientists from developing countries to work fruitfully in the widest possible variety of branches of nuclear physics, as well as contributing to the development of scientific research in such countries. It would be useful here to recall the many statements made by our scientists, paying tribute to the importance of the JINR in the development of physics in their own countries. A number of scientists have even said that without Dubna certain countries might be faced by the problem of the "brain drain".

The scientific conferences organized by the centre permit the exchange of information and the maintenance of co-operative relations between the scientific centres in our different countries, and in addition to this enable different branches of research work to be co-ordinated.

Joint projects undertaken by the JINR and scientific centres in the different countries make it possible for the latter to widen their programs significantly and undertake more sophisticated research, and also contribute to the establishment of new laboratories in the individual countries. All this shows the effectiveness of the JINR and reflects the dynamic character of the principles on which it was established.

CONTRIBUTION TO THE PROPOSAL FOR THE FORMATION OF REGIONAL CENTRES FOR NUCLEAR PHYSICS IN THE DEVELOPING COUNTRIES

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Abstract

CONTRIBUTION TO THE PROPOSAL FOR THE FORMATION OF REGIONAL CENTRES FOR NUCLEAR PHYSICS IN THE DEVELOPING COUNTRIES. The paper discusses the development of nuclear physics in developing countries, pointing out the difficulties of research in the future. A proposal for the formation of regional centres is put forward. Arguments justifying the formation of these regional centres are presented together with the suggestion that UNESCO and IAEA act as sponsors and financial supporters of the regional centres. The institutes in the developing countries could serve as a basis for such centres. Countries which are qualified and interested in having and using the facilities of the regional centres should also participate in the financing of them.

INTRODUCTION

It is well known that the study of nuclear physics in most of the developing countries was initiated with the prospect of peaceful or non-peaceful uses of atomic energy.[1] In this way, nuclear physics occupied the central position in the development of physics, and an urgent need was felt to train physicists and equip laboratories. Atomic energy commissions were created and they directed their efforts towards this end. During a period of about 20 years of relatively intense financing (compared to the possibilities now at hand) and training of nuclear physicists, some equipment, experience, and staff could be gathered in the research centres of the developing countries.

It is well known and has been reported many times[1] that in the course of the development of nuclear physics, the developing countries had to face many difficulties in obtaining funds for equipment and the training of staff. Often, this was made even more difficult, by the lack of tradition and by the rigid rules for creating and operating the new institutions. Most of the time, the new institutions were independent bodies under the auspices of the national atomic energy commissions.

The 'official' interest in the further development of nuclear physics in most of the developing countries has ceased to a large extent. This is reflected in the reduction of the finances made available for research in nuclear physics. The atomic energy commissions have been losing a lot of their prestige. Having in mind the general trend of development, it seems extremely improbable that the situation will be greatly improved in the future.

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[1] See Proceedings of the Fifteenth Pugwash Conference on Science and World Affairs, Addis Ababa, 1965 - 1966.

At this stage, we would like to raise the question whether or not it is worthwile to continue research in nuclear physics in developing countries. We feel that the answer to this question should be positive and would like to give some hints on how to solve the problem. We suggest, as a serious possibility for the developing countries (probably the only one), to consider the old idea of forming 'regional institutes' and thus unifying efforts and financial means in pursuing nuclear physics research.

HISTORICAL DEVELOPMENT AND PRESENT STATE AND ROLE OF NUCLEAR PHYSICS IN DEVELOPING COUNTRIES

As has just been said, in most of the developing countries nuclear physics underwent a somewhat unnatural and forced development. Its significance for the economic situation and the security of the developing countries was overestimated and nuclear physics very often was identified with 'nuclear power'. Early in its development, inadequate and not very promising 'machines' were often built or purchased, partly as a result of lack of funds and lack of judgement. It had often only been realized afterwards that additional equipment and trained staff at all levels would be needed. More or less systematic training programs were adopted, the training mainly being done in countries having the tradition and facilities for research in nuclear physics. Leaving the problem of the brain drain aside for the moment, we turn to the problem of the newly trained physicists who returned to their home countries. Most of the time, these physicists were still junior (according to the standards of the developed countries), and on returning home they often had to play the role of senior nuclear physicists, either fully or in part. They had to transplant or initiate some new problematics under very unfavourable conditions, and at the same time they had to help educate the younger people. Many of these physicists were successful. As a result, we have, in some developing countries nowadays, a situation where the number of nuclear physicists ranges between 25% and 60% of the total number of physicists engaged in research work. Next to research work, most of these nuclear physicists are actively engaged in teaching on the graduate and postgraduate levels, provided either of them exists. Nuclear physics has also influenced and, to some extent, stimulated the development of some new techniques in the developing countries. It has also greatly contributed to the establishment of scientific criteria and the creation of the research atmosphere. It has often provided a nucleus for the development of the neighbouring fields of physics.

What are the existing facilities for research in nuclear physics in the developing countries? Usually we find Cockcroft-Walton accelerators (up to 2 MeV), small cyclotrons (14 MeV), betatrons (30 MeV), small Van de Graaffs (2 MeV), and experimental reactors. And then we are at the end of the list. The instrumentation is usually old-fashioned (non-automatic) requiring many hours of stand-by. The existing spectrometers, coincidence equipment, etc. are generally old-fashioned standard instruments, not combined with other equipment necessary nowadays for more complete and conclusive investigations. The existing computers are usually small and therefore not suited for more elaborate calculations. Summarizing the situation, it is fair to say that the developing countries are duplicating and even triplicating their efforts and are actually in mutual competition with limited means for a very difficult part of nuclear physics research. With technological advance, increasing costs and rising prices, it seems that the continuation of the old 'policy' would eventually lead to a 'die away' of nuclear physics in developing countries! The question arises whether or not to go ahead.

TO DO OR NOT TO DO NUCLEAR PHYSICS RESEARCH IN DEVELOPING COUNTRIES?

The question of discontinuing nuclear physics research in the developing countries brings up several other questions.

What is one to do with nuclear physicists? There are several possible answers. One could suggest to them that they make a change and occupy themselves with other research work, or that they stop research altogether. Both these suggestions are dangerous for the developing countries. The best nuclear physicists would probably not agree to change their field and would seek solutions by themselves in the developed countries. Thus, they would be lost for the developing countries, both as nuclear physicists and as teachers. Moreover, they would take away with them part of the scientific atmosphere which is already so difficult to create. Probably some of the less successful nuclear physicists would be willing to change their field and there is no real hope that they would be able to create the nucleus of a successful new study. It is always best to start with the promising young people who are attracted to that field, or to attract those working in closely related fields.

There are opportunities for nuclear physicists from the developing countries to use the facilities of the large centres for nuclear research. This is, of course, an excellent chance for senior scientists from the developing countries to get acquainted from time to time with the newest achievements and developments in nuclear physics at the highest level. These large centres may also provide excellent opportunities for a few younger nuclear physicists to get some training.

The idea of discontinuing research in nuclear physics in the developing countries would not only have bad consequences for nuclear physics itself, but also for teaching at the universities and for the entire scientific activities of the developing countries where nuclear physics plays a major role. To use only the facilities of the large centres which are few and distant, and not have any scientific research facilities at home would again keep nuclear physicists away from the developing countries. So nuclear physics research cannot be discontinued. The question is, how to organize it.

HOW TO ORGANIZE PHYSICS RESEARCH IN THE DEVELOPING COUNTRIES?

The existence of the 'large centres' is, of course, a condition which seems to be necessary for the development of the entire nuclear physics program, and at the same time entirely or partly solves the problems of the countries where the centres are located. These centres play an extremely important role in the development of nuclear physics in the countries collaborating with them, but they do not solve the problem of nuclear physics research in the developing countries. Their existence is essential for the furtherance of nuclear physics research in developing countries, but alone is not enough.

In trying to solve this problem, we have to look for some additional 'intermediate structure' which we would like to call 'regional centres'. The idea of regional centres is an old one that has been brought up on several occasions, both nationally and internationally, but so far it has not attracted enough attention. The main idea and aim of the formation and location of the regional centres should be the successful conclusion of some particular activity in nuclear physics research in a given place, and should have as a prerequisite the prospect of obtaining new experimental facilities and the promise of efficient collaboration. The regional centres should, as a rule, be located in cities that have universities, so that the local university staff could also contribute to the formation of the critical size of the centre. Thus, the organization of the regional centres would, in some cases, just mean an extension of the existing facilities, and only in rarer cases, the building-up of completely new facilities.

To illustrate the idea of the regional centres, let us discuss a possible model. Such regional centres do not exist, so we cannot take an existing example and discuss it. With the existing equipment none of the research institutes in the developing countries can be transformed into a regional centre. The required condition for transforming a successful institute in one of the developing countries into a regional centre, is the creation of new experimental facilities. Just for the sake of argument, let us consider some of the real possibilities. It it were possible to acquire a 50 MeV cyclotron, a tandem or a similar machine, provide it with the proper instrumentation and locate it so that it could be used efficiently by several centres, this would provide the basis for a regional centre. This regional centre would, in the place where it is located, offer the opportunity for continued research in certain fields of nuclear physics and also encourage co-operation with the other centres on an equal basis. The advantage of the regional centres should be that the idea should lead to the creation of more regional centres and thus we would have the opportunity of doing a given type of research at relatively low cost in the developing countries. In addition, the problems of travelling would be reduced considerably and, in the most favourable cases, travelling would be simply commutation. The regional centres would certainly reduce the brain drain from the developing countries and create opportunities for returning, even for those who have been away for a longer period. Last but not least, the regional centre would contribute considerably to the promotion of mutual understanding.

PROBLEM OF FINANCE

A very rough estimate of the costs for such a regional centre over a period of 10 years would be approximately 10 million US dollars. Who would be the 'patron' of these centres and who would participate in the financing of such projects?

The regional centres in the developing countries, as proposed, are considered to be open international institutions. Therefore, it would be appropriate for them to belong to the specialized agencies of the United Nations, such as UNESCO in Paris and the IAEA in Vienna.

The funds should, in the most part, come from these agencies and contributions should be made by those countries which are interested in participating in and using the facilities of the regional centres.

We are aware that this is partly an appeal to the developed countries to help bring the regional centres into existence, because the former contribute the greater part of the funds of the United Nations. On the other hand, it is at the same time an appeal to the developing countries to find a way to collaborating in creating the regional centres.

The developed countries should be interested in supporting the advance of the developing countries, who in their turn should be interested in furthering research in their own countries. So we hope that the idea of regional centres may attract some attention both in the developing and developed countries.

In presenting these views, I have used data on the developing countries assembled by Messrs. Mladjenovič and Šlaus as well as information gained in conversations with colleagues from both developed and developing countries, to all of whom I should like to express my sincere thanks.

GENERAL DISCUSSION

V.F. WEISSKOPF: In discussing the ideas presented in the three previous papers, I think we should open up the problem in its entirety, and the problem is of course very wide.

First of all, we ought to make it quite clear why nuclear physics is important for developing countries. It is an attractive field for young scientists, but it would be wrong for a developing country to pursue a very specialized field of nuclear physics, and at the same time have no physics of another type. It is important that nuclear physics be interwoven with the rest of physics.

Another point that we should take up is what kind of nuclear physics should be done in a developing country. This is an important subject. The physics which is done should be interesting physics. Repetition of things that are done better somewhere else would be very undesirable. In my opinion, nuclear structure physics is much better in this respect than, for example, high-energy physics. There are still, and will continue to be, a lot of fields in nuclear structure physics which can be done and can be done excellently with relatively modest means and small laboratories. I emphasize the excellence because we all know it makes no sense to do physics which is not excellently done.

Let me mention a few points to stimulate discussion about this subject. The introduction of the lithium-germanium detector has opened a wide field in nuclear structure that has not yet been exploited. For example, there are many beta/gamma-ray coincidence measurements which can be done relatively cheaply with radioactive sources. Professor Sakai is involved in this kind of measurement. Here is a broad field of important, essential studies that lies before us. Nuclear spectroscopy is full of this kind of problem, such as special transitions, which require not much in the way of means but a lot of patience.

Let me emphasize another point. The interface between nuclear physics and solid-state physics is especially important for small laboratories for two reasons. First, certain techniques can be used with relatively modest means. I am thinking here of nuclear techniques to investigate the electromagnetic field in solids, magnetic field distribution, electric multipoles, and other fruitful applications of nuclear physics to the solid state. Second, it establishes just that element which is so necessary in a young country, namely the teaching not of <u>nuclear</u> physics, but of <u>physics</u>. It is especially important for the younger countries that the students do not become specialists, but really physicists in a broader sense, and for this, I believe, these researches are ideal.

Now, we must also consider another type of question: What kind of organization gives the best support to this kind of work? There is the question of central institution versus regional institution, and also of what kind of regional institution.

I believe that whenever a co-operative institute has been started it has been a greater success than expected. We should remember this. You see, when CERN started people were very sceptical and it turned out to be a great success; when Dubna started people were very sceptical and it was a great success. So I do believe that we have been traditionally pessimistic about scientific collaboration. The reason is probably that international collaboration is usually very difficult, but let's not forget that we scientists are more gifted in this direction than the rest of humanity. And this is a gift which is so important not only from the scientific point of view, but from the human point of view, and we should go to the utmost in this respect. I am glad not only that the big international institutes have been mentioned, but also that Professor Alaga has presented new ways of regional collaboration which have not yet really been tried.

Finally, I would like to reply to one remark of Professor Sodnom. I understand very well that conferences are extremely important for the physicists of the developing countries. However, I do believe that here also we must try to look for better forms of conferences. Usually, the younger physicists have very little contact and feel very lost. I think seminars and summer schools are probably much more fruitful, not only for education but also for the exchange of ideas. There is a need, however, to be more systematic in planning summer schools and seminars.

Exchange of scientists is also important. And this exchange should go both ways. Not only should people from, let us say, Mongolia come for some time to CERN, but I think we should also have people from the so-called developed countries spend more than just an hour or so in Ulan Bator, for example.

H. NIEWODNICZAŃSKI: I want to limit myself now to this question of developing countries and how to support research work in physics. In many countries there are young physicists who are getting their training by spending time in, say, such institutes as Dubna or in the United States, England, Sweden, France and so on. But after returning there is no true possibility of continuing their work. So this idea of regional centres is very important.

Of course, it would be unwise to build a new institute. It would be better to start with an existing and developing institute which will be supported by the country where it is, by three or four neighbouring countries, and also by an international organization such as the IAEA or UNESCO. This will create the possibility of establishing a more modern laboratory with, say, a larger accelerator equipped with additional important apparatus such as computers. Probably this would be the easiest and quickest way to help those countries to develop research.

L. AGNEW: I could comment on this question of organization of international projects and centres. First let us distinguish between centres and projects. A centre implies a large capital investment: buildings, equipment, staff, and certainly a long period of activity. An international project can be a single experiment or a continuing series of experiments at some existing institute. For example, scientists from three or four countries could collaborate with equipment and men to conduct research at a major facility such as a research reactor or an accelerator.

Regional centres are very difficult to start. They require an enormous effort on the part of the organizers, who will have to convince the authorities, whose budgets are already stretched, that it is an important undertaking.

The International Atomic Energy Agency has set forth the conditions under which it will help to establish an international centre. First of all, the request should come from a group of governments because the Agency does not normally supply the initiative for such a thing. The proposal must state why the centre is needed and how it is to meet both the training and research needs. The proposal should include the value of the centre for the technical, scientific and economic development of the region, and it also has to include what the needs of the region are for manpower to be trained at the centre. The request would have to specify what support, both financial and 'in kind', the participating governments would propose. The proposal should state the intention of the participating governments concerning their assumption of full responsibility both financial and technical for the centre after the Agency's financial support has been concluded. A centre in which the Agency could take an instrumental role in beginning, and which would have some planned future release of the Agency from support, would be the kind we would be interested in. In addition to funds, the support would be assistance in the form of supplying experts, visiting professors, equipment and fellowships to help young men to come from other countries and work at the centre.

Thus a mechanism does exist for creating a centre with the Agency's help. The Director General of the Agency, with the help of the staff, would evaluate a request from a group of governments and then make a proposal to the IAEA Board of Governors. The Board of Governors would take action about establishing assistance for a centre. The money that would be provided, if such a programme could be implemented, would come ultimately from the United Nations Development Programme, at our request, or from the Agency's own budget.

Mr. U.L. Goswami, IAEA Deputy Director General in charge of Technical Assistance, has authorized the following statement: 'For various reasons I would suggest that you make it very clear to the Panel that the probability of continuing financial support by the Agency for regional centres is not great, and that even if a group of countries in a region were to submit a proposal for a centre which is entirely valid and justified from a technical viewpoint, the task of finding the necessary resources for the Agency's financial support would still present acute difficulties.'

Now, there are additional minor ways in which the Agency helps research in developing countries. We have seminars and study groups from time to time in various regions of the world. We have some research co-ordination projects, in which, for example, we sponsor a meeting between research people who are working on related aspects of the same problem, and who need to talk to each other to exchange ideas on how to proceed. Also the Agency can help with the exchange of equipment, for example the loan of some complicated device from one research reactor to another. Finally, the Agency can help with the procurement of special materials, for example uranium or plutonium targets for specific experiments, which in some countries are very hard to obtain. The Agency has ways of obtaining them from the advanced countries.

J. TEILLAC: I agree with Professor Weisskopf that it is necessary to do excellent physics. Regional centres as envisaged by Professor Alaga would certainly be a good thing not only for developing countries like Yugoslavia, which already has a physics program, but also for countries which have practically no physics. Probably a laboratory should be developed around a particular technique, and I give as examples nuclear astrophysics, nuclear chemistry, nuclear methods in solid state. But it is necessary to have a very good physicist who works well in the particular field.

K.F. ALEXANDER: Modern industry is more and more based on science, big science. All the countries with only a small amount of industrial development have to manage a technical revolution, and that is not possible without big science. And here nuclear physics can play a very essential role. Not only via the direct feed-back as in nuclear energy or solid-state physics, but perhaps even more in the sense of indirect influence. For big science, you simply have to use industrial installations and computers, both on- and off-line. You have to form research teams, and you need international co-operation. Here the role of the centres for nuclear research can be very helpful. This can lead to the extensive connections (in the sense which was defined by Professor Weisskopf), which have to play the leading role in conducting the technical revolution in the smaller developed countries.

G. N. FLEROV: For a number of years, I was the scientific leader of a large institute of nuclear geophysics, and I would like to point out that care must be taken in developing the applications of nuclear physics. After the first chain reactor, people in many countries began to set up centres for nuclear reactors. A great amount of work was also devoted to certain applications, such as how to determine the amount of liquid in a vessel by remote measurements.

With the evolution that has taken place in experimental methods, there are now many worthwhile applications of nuclear physics. In small countries, there are specific applications which are linked, for example, with the geographical situation, the situation of science, or other factors. In such countries one could solve specific tasks: there are countries which suffer from a lack of water, other countries have too much water, some countries suffer from earthquakes, etc. It strikes me that science and the applications of science should aim at particular tasks based on an analysis of the country and its future economic activity.

May I point out a few research aims. Electrical power first of all, which is of course necessary; it is also necessary to find mineral ores and water. Uranium makes it possible to get energy, and this is a very good exchange equivalent. Then there is oil, and then a number of elements which the technologists are using more and more these days.

Now, as regards an international centre, the natural grouping of countries automatically leads to the fact that they have in general the same geological and biological features, and then the tasks could be based on an analysis of the countries' needs. I have sometimes heard statements that one has to start by dealing with pure physics for a problem, and then put the problems of methods. I would say that it's the other way round. Work in the applied field unleashes the sympathy which we are missing at the moment, and in this way we will get the money which also is lacking. So we need both the money and the sympathy; both have to be won. But I would like to end on an optimistic note: my feeling is that we can in fact find ways to proceed.

V. F. WEISSKOPF: Professor Flerov has brought up a very important point. There is always the problem of pure and applied sciences. In the large countries this problem is perhaps not so serious because it adjusts itself almost automatically. For example, in the United States and in Western Europe the ratio of expenditures for applied science to basic science is 10 to 1, and I suppose this will be similar in the Soviet Union. It is of course very difficult to define these numbers because it is difficult to define what is applied and what is pure, but we have a qualitative impression.

Professor Flerov has pointed out that the developing countries face very definite and important technical problems such as water supply, power supply, and agricultural problems. These problems must be solved. If one presses too much for pure nuclear physics, one endangers somehow the supply of money, activity and talent to the applied tasks. On the other hand, if you have too little basic science you run into danger in the training of new scientists and the ability to exploit new ideas, and you sacrifice the future for the present. Of course, the present is perhaps more important for developing countries than for developed countries, and this makes a difficulty. One could for example take the radical view and say that basic science as we understand it here should not be introduced at all into new countries because there are more important and immediate tasks to do. It is a very popular point of view and I don't believe that it is right. On the other hand, one often makes the mistake of introducing very sophisticated sciences of basic nature into a country that doesn't even have the more elementary science which is necessary for its own development technically and intellectually. Since this is an important matter, I would invite other people's opinion on it.

G. ALAGA: According to the conclusions of other meetings that have tackled this problem, the gap between the science and the application in most of the developing countries is so large that the developing country actually has very little use for the science. So it doesn't really matter what kind of science it develops because neither nuclear physics nor solid-state physics nor any other kind of science has immediate impact. Of course this is a hard problem to solve and I don't think it can be solved just by making a jump - one has to start somewhere, and in the developing countries the situation is not everywhere the same.

The main point of my proposal for regional centres is concentrated on those countries where research has already been started. Usually it started on a not very well organized or planned basis; perhaps it started on a broad base, and then, as in biology, the selection principle worked. In some fields progress is quicker and more able people are attracted, while other fields lag behind. And my proposal was to pick those fields where some progress has been made in developing countries and try to locate the regional centres there. It is to be expected that those people who have been successful in achieving some progress in a difficult situation would be able to tackle the new equipment successfully.

Regarding the open problem of what to start and how, in the past the people were often sent abroad and tried to transplant the problematics from those places. But they have very little chance to compete with a greatly superior centre. I think that the scientists in developing countries have to be able to do independent science and not to be tutored from a distance.

F. JANOUCH: We are discussing here perhaps three main problems: where to do the nuclear physics, what to do in the nuclear physics, and how to get money for it. All of us who work in nuclear physics feel the negative attitude of weariness with which our governments and general patrons face our demands which are not always in proportion to our national budgets. At present therefore it is important to have convincing arguments to justify the further development of nuclear physics.

Perhaps it would be worthwhile to collect a wider panel of people in order to discuss what are the most promising and most important trends in nuclear physics, and to create some kind of guide book for young men in nuclear physics. But the arguments which are important for us and for the people who are doing the research are perhaps not entirely convincing for the people who have the power and duty to distribute money among the different scientists. Perhaps not enough stress has been laid on the influence of nuclear physics on the development of other branches of science. Apart from random and fundamental discoveries which can accompany research on the structure of the nucleus, nuclear physics in the broad sense has profoundly influenced other sciences and technology by its methods, the organization of its research and its instruments, and, if by nothing else, it has earned the right to exist.

Apart from the weariness that our patrons feel at our growing financial demands, we also feel a different kind of weariness from information. Thousands of journals, hundreds of thousands of preprints, and dozens of scores of conferences make our information system chaotic. Development is so fast that we are printing now information which is out of date, and therefore pre-prints and personal contacts are becoming important sources of information. It is paradoxical that in the second half of the 20th century we are getting back to the beginning of the circle, to the idyllic times when at the beginning of the development of our science people exchanged their results and scientific news in personal letters. But what I think is bad in this situation is that there is a large number of physicists, especially from small national centres, who are more or less excluded from this circle of information. I think that physicists, as they have many times in the past when organizing international centres and other types of co-operation, should try to do some pioneering work in the creation of some sort of system of preprints. In the same way, as Professor Weisskopf has already mentioned, I think it is proper that physicists should address themselves to the task of better planning for conferences, summer schools, and other meetings. Perhaps the most promising prospect is offered by the European Physical Society, which is trying now to bring this scheme into some order.

J.O. RASMUSSEN: I would like to second the remarks of Professor Weisskopf that a particularly rich area in which to seek unique kinds of new research would be interfaces between nuclear science and other branches of physics or other major disciplines. And as many of us have stressed, the modern solid-state detectors, the uranium and lithium detector and so on, open up all sorts of possibilities for research with very modest investment. For example, we know very well of the excellent research in Professor Trlifaj's laboratory in Prague where low-temperature physics is studied in juxtaposition with nuclear work. I think the competition from the developed nations is surprisingly thin in some of these areas, since there is very little concentrated work in low-temperature nuclear alignment within the United States and within other developed countries.

With our nuclear methods we have the capability of producing very highly ionized and unusual atomic species, and now we have new ways of investigating these interfaces with atomic physics. As touched on by Professor Teillac, the interface with astronomy or cosmology is an important area. I recall a conference a couple of years ago where we heard discussion of some of the first work on X-ray astronomy. It was carried out by sending rockets up a few minutes in the upper atmosphere to observe X-rays. The report was given at a nuclear physics conference, and we were somewhat astounded that with all the expense of rocket flight, the nuclear instrumentation was Geiger counters; much more information could have been gathered with the instrumentation we developed in nuclear structure. The suggestion was made to have some pulse height analysis from solid-state detectors. Such research has since been done, yielding information about the temperatures of some of the unusual X-ray emitting sources. In this field some countries may have a unique advantage: for example, astronomers in the southern hemisphere will be the only ones able to observe certain regions of the heavens. Some of the modern X-ray astronomy rocket soundings have had to have direct wire hook-ups, with astronomers in the southern hemisphere observing the optical intensity fluctuation of the stellar objects at the same time as the rocket flights were carried out.

Turning to another separate discipline, the germanium detector has a tremendously increased possibility of analytical measurement and there are many applications to archaeology. Some of the most underdeveloped countries may possess absolutely unique resources in terms of archaeological objects. The analysis, the interconnection, the establishment of trade patterns in ancient Africa, and so on, can be studied by using the new nuclear detection techniques. But they cannot be done by people without some nuclear sophistication and training - some knowledge of what a decay scheme is. It will be people who develop knowledge in both areas, and I feel often it will be people who received nuclear training, who will move into the interface areas.

In concluding my remarks, I would like to agree with Professor Weisskopf that we should really push for a longer-term exchange of younger people and not confine ourselves to the brief contacts of visits and conferences. We try through our limited private resources, and the all too limited university fellowships, to support the exchange students. The research support is usually no problem, but there are sometimes obstacles in obtaining governmental money for salaries. I think it would benefit us if we had more information about IAEA fellowship policies - what restrictions there are on particular countries, the amount of the stipends, and whether they support only the one-way exchange from a developing country to a developed centre, or vice versa.

R.B. LEACHMAN: I would like to speak about regional centres, based on my own personal experience at the Kansas State University, which is in a region of the United States that is quite far from laboratories and universities that are centres of excellence. We are installing a new laboratory equipped with the type of facilities that Dr. Alaga spoke about, namely, a tandem Van de Graaff and some smaller ones. Under the present economic situation in the United States, there is very limited money and there is a desire for regional participation.

We realize that we should utilize this new facility as a regional facility, and we ask what sort of nuclear physics we should do and how it should be done. We find that it is difficult with the present technology to undertake nuclear physics spectroscopy or nuclear structure. It's difficult in two respects: one of them is that using accelerators for spectroscopy involves rather sustained running periods. Under these conditions, the scientists - professors or students - are away for long periods, and the home institution does not have the benefit of actually having the person doing the work there. Another factor is that, because of financial restrictions, it is difficult to acquire the type of equipment, such as on-line computers and large sophisticated reaction spectrometers, that is needed to be on a competitive scale with the large well-established institutions.

Bearing these things in mind, we are indeed turning to other applications of nuclear techniques in our own establishment which we look on as somewhat of a regional centre. We intend to have a considerable emphasis on implantation work, which has the advantage of initially simpler equipment, and has the further advantage that students and professors can take specimens back to their home institutions where they can engage in very worthwhile studies with rather simple equipment. Another field is that of in-beam spectroscopy. Here, particles from accelerators are put through foils and the optical excitations are studied for their astrophysical importance. Although the advantages for home study are not as great, it is possible of course to take home the exposed films and to study the spectra. Finally I should mention the field of radiation biology, which can be similarly exploited in regional centres in a way that will give the greatest benefit to the home institutions.

D.H. WILKINSON: If nuclear physics is being done in countries which do not have a strong technological, and therefore strong academic, base, then frustration can arise very easily. One obvious way is to get as much outside help as possible. We have heard that the Agency does have an expert consultant service. I understand that the government of a developing country could ask the Agency to send out an expert to teach on solid-state physics, or counter technique, or something of that sort. This is admirable, and perhaps that service should be much more widely known.

But there is another kind of problem, one that I think could perhaps be handled much more on the lines of a 'medical service' for equipment that breaks down. I do know laboratories in the rather remoter and smaller countries that have been literally held up for weeks or months trying themselves to put something right, that someone who is really very familiar with the matter could have done in a day or two or even an hour or two. I wonder whether the Agency could not consider setting up an emergency service in which technical experts would be on call in the same way as a doctor is on call. Perhaps in theory as well as in experiment and technology, although I was of course thinking more of vacuum pumps and electronics and the things that can plague a laboratory. It's obvious that as far as accelerators are concerned, remote countries should only go in for the sort of accelerator which is backed up by this kind of service. But it is not so readily available for smaller matters, such as electronics and vacuum pumps, or integrals and matrix elements. I think it is quite wrong to say that genius is bred on adversity. I think Mozart did a grave disservice to nuclear physics by writing such good music under such bad conditions, but even Mozart did not have to spend half his life tuning his piano. Of course lute players did spend half their lives tuning their lute, and we know what happened to the lute.

G. ALAGA: I would suggest, if it is the feeling of the panel, that a short conclusion should be made about possibilities and prospects of organizing nuclear physics in developing countries: it should be brought through the Agency to the attention of the governments, and also to the attention of the physicists in the developing countries. I would not be scared by the fact that at the moment there is no money available; I would prefer to take Professor Weisskopf's view that always at the beginning it's hard. A few minutes ago I learned that Rumania is acquiring a tandem Van de Graaff. Maybe some other places will be also in the position to do something and to offer a starting point for regional institutes.

RECOMMENDATIONS OF THE PANEL

The Panel stressed the importance of nuclear physics research not only as a viable fundamental field of science, but also as providing a base for nuclear technology, for gaining experience of a wide range of other modern technologies, and for training scientists.

Recognizing the difficulties faced by smaller institutes and developing countries, the Panel recommends that the Agency communicate with the Member States and appropriate international organizations, urging them to make efforts to ensure the continued strength of nuclear physics research programs.

In particular the Panel recommends that the Agency give full support, and, if possible, initiate action towards the establishment of Regional Centres providing the means for the pursuit of research and training in the field of low-energy nuclear physics.

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