

NUCLEAR FORCES and the FEW-NUCLEON PROBLEM

A report by Michael J. Moravcsik

WHEN I arrived at the other side of the ocean I was asked whether I came over on a Fulbright or whether I came for the "conference season". A glance at the calendar of international scientific gatherings is enough to ascertain that summer in Europe has indeed grown into a "conference season", the popularity and success of which can be well demonstrated by the story of the London conference on which I am reporting.

The conference was born out of a letter from Cranberg (Los Alamos) to Massey (University College, London) in which it was suggested that it might be useful to have a small gathering of perhaps 30-40 people to discuss problems related to systems consisting of just a few nucleons. Soon it was thought appropriate to include also some discussion on the basic two-nucleon interaction and from there on things began to get out of hand. Interest in the conference grew by leaps and bounds, and after changing the location of the meeting to a larger and larger auditorium, the limit was finally set by the size of the largest hall at University College, holding 250. As a result some people had to be turned away due to lack of space. It might be mentioned, however, that there are other good reasons why a conference should not grow much beyond 200 participants. The great success of the London conference, I think, was partly due to the opportunity of formal and informal discussion, which is very difficult in a huge auditorium with many hundreds of people.

The conference was organized by University College, London, under Professor Massey's direction, with T. C. Griffith as conference secretary, heading a large and very efficient staff of organizers. It lasted for four days from Wednesday to Saturday, and was topped off by a visit to Windsor Castle on Sunday, followed by a boat trip on the Thames. Each day of the conference featured four sessions, which were separated from each other by morning coffee, lunch, and afternoon tea, respectively. There were no parallel sessions so that everyone could attend all of the conference. In addition, there was also an evening session on Wednesday. Friday evening was reserved for the conference banquet, and for Thursday night a sightseeing bus tour of

London was on the schedule so that one could hardly complain about lack of activities during the five days. The composition of the conference included delegates from 14 countries, although the majority of the participants were British or American.

There are essentially two approaches to the formulation of the program of a conference. The program could be composed of short individual contributions (like APS meetings are), or it can feature a few rapporteurs who give a survey talk of recent developments, with discussion to follow. The rapporteur system was used at last year's High-Energy Conference in Geneva, and is continued this year at the Kiev conference. This is not the place to discuss in detail the pros and cons of the two systems, especially since such a discussion would have to take into account, in addition to rational arguments, certain human factors, personality conflicts, and other nonscientific topics. The London conference, confronted with some of these intangible but very real problems, ended up as a compromise between the two systems. There were several longer review papers, but they were supplemented also by some short individual contributions. In my opinion, the longer papers were by far the most valuable, since the short contributions often lacked perspective and sometimes deteriorated into the exhibition of small experimental details or the showing of slides covered with formidable formulas of the calculational minutiae. The most important aspect of the conference, however, was the ample time allowed for discussion. This was perhaps the most educational function of the conference, and at the same time also the most proper one, since the personal and direct exchange of opinion is that aspect of scientific work which is the most difficult to carry out through publications and correspondence. The discussion which took place at the conference, together with the text of the papers delivered, will be published in book form by Pergamon Press.

In the rest of this report I will try to give a summary of the scientific work of the meeting. In doing so I will have to discuss some controversial subjects, and although I will try to give a balanced view, I am sure I will be guilty of a certain partiality. In order to avoid getting into personal conflicts, I will omit all references to authors of the various contributions. I

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Conference delegates leave University College, where sessions were held.

hope that my account will simply serve as an incentive to consult the printed proceedings of the conference which will supply the solid details on which my account is based.

The conference naturally divided into two parts: the two-nucleon interaction and the few-nucleon problem. The first topic was discussed on Wednesday, while the remaining three days were devoted to the few-nucleon problem.

The interaction between two nucleons is, needless to say, one of the most basic problems in present-day physics. It is now over twenty years old, and its history clearly shows ups and downs. It seems that, after a comparative lull, the past three years again represent an active period. There are several reasons for this. Firstly, experimental techniques are steadily improving and machines of higher intensities are being built (in this field, at least at this stage of its development, there is no premium on high energy, but high-intensity beams are of extreme importance). These developments permit certain triple-scattering experiments to be carried out which give basically new information on the interaction. The most important experimental tool for studying the two-nucleon problem is nucleon-nucleon scattering. By now we are beginning to have a fair amount of experimental information on proton-proton scattering, and certain progress is being made also in gathering data on neutron-proton scattering, although this latter field still leaves plenty of room for improvement. Finally, there is an additional incentive to carry out these experiments because of some new developments in the theory of the nucleon-nucleon interaction.

Until recently the theoretical approach to this problem has been either purely phenomenological (effective range theory, phase shift analysis), or purely abstract (meson-theoretical). Some recent approaches have tried to combine these two aspects of the theory. Such combinations at least help us to understand where we stand with the solution of the problem, what part of the problem is in fact explained by theory, and where the region of our ignorance lies.

At the London conference all three theoretical methods earned a thorough discussion. In the realm of pure phenomenology, the Gammel-Thaler potential was discussed in many connections. It is undoubtedly the most

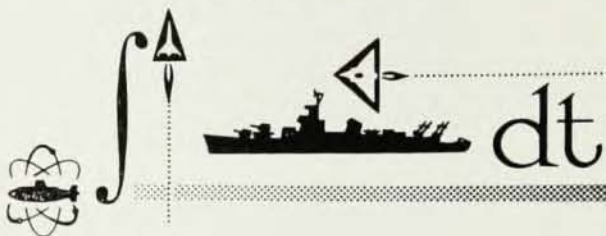


Top to bottom: conference participants R. E. Marshak of Rochester; H. S. W. Massey of University College (delivering the opening address), and H. P. Noyes of Livermore.

reliable description of the nucleon-nucleon interaction we possess at the present time, although its quantitative agreement with experiments, where they exist, is far from being perfect. In addition, its validity is wholly dependent on the correctness of the so-called Solution 1 of proton-proton scattering at 310 Mev (as opposed to the Solution 2). It seems that if Solution 2 turns out to be the correct description of proton-proton scattering at that energy, the picture of a local potential might have to be abandoned altogether.

The purely theoretical attacks on the two-nucleon problem have been, until recently, potential calculations from meson theory. One can subdivide the region of interaction between two nucleons into three regions. The outermost region, it is thought, is dominated by one-pion effects, that is, by the exchange of one pion between the two nucleons. The middle region is characterized, in addition to the one-pion effects, by contributions from the exchange of two pions. Finally in the central region the one- and two-pion effects are sup-

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plemented by contributions from the exchange of more than two pions and by effects due to heavy mesons.

The outermost region can be easily described. This is the region where the interaction is known beyond any doubt, and where all reasonable theories agree. The interaction, in fact, in this region is simply given by the lowest order perturbation calculation, independently of the *validity* of a perturbation expansion.

The central region can be just as easily disposed of, but for a different reason: at the present time, it is quite hopeless. For the time being we have no reasonable theoretical framework which could treat many-pion effects, and our understanding of the physics of heavy mesons is in its infancy.

Most of the effort, therefore, has gone into the understanding of the middle region, that is, into the calculation of the two-pion effects. Scores of calculations have been carried out in the past decade or so to obtain a potential in this region. The result to date is that we still know very little about this region. Pessimists would claim that in fact nothing is known, since the various calculations differ violently. The more optimistic observers claim that, on the whole, the *sign* of the potential in the various states is fairly uniquely determined by these calculations. There is, nevertheless, no clear prescription for doing such calculations in a reliable way within a finite amount of time. It seems as if a basically new idea is needed to reformulate these calculations in a way which promises some practical results.

There are some who believe that such a reformulation has been brought about by the dispersion relation approach. In particular, much faith is placed by some in the so-called double dispersions, which describe processes simultaneously as a function of energy and momentum transfer. This approach has the advantage of giving, at least in principle, an unambiguous prescription of how multipionic calculations should be treated. Also, it exhibits a natural connection between processes like nucleon-nucleon scattering, nucleon-antinucleon scattering, and pion-pion scattering, so that several problems can be solved together. So far, the only practical application of this approach has been in the outermost region where, like all other theories, it gives the one-pion contribution correctly. It is to be shown whether the dispersion relation calculations can produce practical results also in the middle region, where other theories have, on the whole, failed so far.

A fusion between meson theory and phenomenology is represented by the Signell-Marshak potential, whose main virtue is in the observation that while meson-theoretical potentials are in disagreement with experiments, this disagreement can be disposed of by the addition of a simple spin-orbit term. The necessity of this spin-orbit term as well as the degree of experimental agreement with or without it received much attention at the conference, although no very definite conclusions were reached. It is my feeling that one can get considerably better agreement with experiments if

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one does include this spin-orbit term than if one does not, especially in the energy region beyond 100 Mev, but that the quantitative agreement is in both cases poor. Here, like in all other comparisons, one considers mostly the isotopic spin triplet part of the interaction, since the neutron-proton scattering experiments yield, at the present time, fairly meager information.

Another attempt to combine the theory with phenomenology is the so-called modified phase shift analysis, which uses phase shifts only to describe those regions of the interaction which are not known from meson theory. In particular, the outermost region is given exactly from theory, while the two inner regions are treated phenomenologically. This method has yielded some practical results (such as a reduction in the ambiguity of phase shift solutions), but it has not furnished basically new information either. It might be useful, however, if and when a two-pion exchange theory is constructed, because it offers a scheme for a *stepwise* check of the theory against experimental results.

The uncertainty of the inner regions has also been described as a boundary condition in some theoretical work. Its results more or less parallel those of the other methods.

In summary, therefore, we can say that the basic understanding of the two-nucleon interaction is still very far from completion. More experimental data as well as fresh theoretical ideas are greatly needed. One can only hope that the latest vogue of multi-Bev machines and theories of strange particles will not completely distract efforts from studying "low energy" (below $\frac{1}{2}$ Bev) nucleon-nucleon interaction, which is still one of the basic unsolved problems.

LET us turn now to the few-nucleon problem. One can study few-nucleon systems with two aims in mind. One might hope that at least in certain limited regions and under restricted circumstances, the specific pluri-body effects will be negligible and hence one can get additional information about the two-nucleon problem. Alternatively, one might hope that if one knew the two-body effects very well one might learn about the special characteristics of a system which consists of more than two nucleons. Clearly, the second task is the more ambitious one, and, at the same time, the more interesting one. In both cases, however, the connection between the two-nucleon problem and the few-nucleon problem is utilized. I would like, therefore, to discuss the results of the conference concerning the few-nucleon systems from the point of view of whether, at this stage of development, it is really justified to hold a conference which discusses both the two- and the few-nucleon problems in the hope that they are closely related.

The answer to this question is both yes and no. One should clearly try to establish such a connection as closely as possible, and hence a joint consideration of these problems is very useful. At the same time, at present the connection between the two problems is



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Shown at top is R. Wilson of Harvard University at the speaker's table. Bottom, at left, G. Breit of Yale comments from floor following talk by J. Iwadare of Tokyo, at right.

somewhat tenuous, perhaps more so than it should be. As far as the learning about the two-nucleon system from few-nucleon experiments goes, this seems to be discouraging. It appears that in general whatever we can learn from the few-nucleon experiments concerning the two-nucleon interaction we can find out better from direct two-nucleon work, and often even less information is available from a few-nucleon system than from nucleon-nucleon scattering. As an example, the original five phase shift solutions for p - p scattering at 310 Mev have been reduced to only two by two-nucleon work, while similar attempts using nucleon-nucleus scattering have remained fruitless. In addition, there is always the extra uncertainty in the few-nucleon work of whether the approximation taking into account the pluri-nucleon character of the system is good or not.

The reverse process of trying to learn something about the few-body system *assuming* the two-nucleon interaction to be known is in principle more promising, but is at the present time at a quite elementary stage. There are various difficulties. Firstly, there is really not enough confidence in our knowledge of the two-nucleon interaction. Perhaps the best suited form of the two-nucleon interaction for this work is the Gammel-Thaler potential, which, however, is only approximate, especially in its isotopic singlet part, and is in addition quite complicated in form. It is best at higher energies, while at lower energies, where experiments are scarce, it is less reliable. In fact, it gives no binding at all for the deuteron. Secondly, the theoretical treatment of the three- or four-body problems is difficult in itself. In a large fraction of the calculations the impulse approximation has to be used, with some estimates of form factor, distortion, multiple

scattering, and shielding effects. Another basic difficulty is that in the few-body problem the two-body interaction must be also known off the energy shell, which is information not directly available from two-nucleon experiments.

As a result, most of the theoretical few-nucleon work has been more or less an approximate consistency check between two-nucleon interaction and few-nucleon experiments, using forms of the two-nucleon interaction which were in use in the early '50's such as the Serber force, sometimes supplemented by some simple tensor force. To be sure, it might be that in some specific problems the more complicated aspects of the two-nucleon interaction will not play a role. In fact, some of these calculations do reproduce some of the experimental findings. Nevertheless an outside observer is struck by the disparity between the oversimplicity of the approach and the amount of effort going into the calculations. It turns out, however, that calculations using any more complicated two-nucleon interaction would be almost prohibitively complex.

Such a sad state of the theory is even more regrettable since some of the experimental information available in this field is very good indeed. Thus certain features like charge symmetry or time reversal invariance can be checked to a high accuracy, and it is clear that with a good theory much more information could be extracted from those experiments.

There are some more ambitious calculations under way, however. Several groups are planning to undertake a calculation of the ground state of triton, using some fairly reasonable form for the two-nucleon interaction. These calculations promise to take many dozens of hours even on the fastest computers, which may give some indication of the complexity involved in such problems.

The conference also discussed briefly the many-body problem to the extent that it can be applied to light nuclei. The general consensus seemed to be that one could hardly expect to treat in this way nuclei lighter than oxygen, although a calculation treating the alpha particle in this manner was presented and even managed to get agreement with some of the experimental information.

Lest the perhaps overly pessimistic tone of this report be construed as a criticism of the conference itself, let me repeat in conclusion that the meeting was excellently organized and was very instructive. In fact one might argue that it is precisely at the time when a field is in need of substantial development, when the main problems have to be defined and when the possible approaches have to be evaluated, that a conference of this kind is the most useful. Some expressed hope that in something like two years it will be repeated, and this suggestion was greeted with universal enthusiasm. All we have to do now is to make sure that a "breakthrough" occurs in this field within the next two years so that the reporter of the second conference can list more progress in his account than I have been able to report here.