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# Concluding Remarks*

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We work in a difficult field. The energy range to which this workshop is devoted is not usually considered a glamorous one. What we would like to regard as adequate funding, the means of engaging the excited attention of our colleagues who have other interests, is often missing. But the hard experimental and theoretical work reported here has rewarded all of us. I have heard on all sides that this has been an exciting conference.

Each of you will have his own choice as to what was most exciting. I must make my own selection. Since I like controversy, the difficulties brought out by Levin, Sawada and Kawai caught my imagination.

Levin gave evidence that the physical intuition we develop in two particle problems can be completely misleading when it comes to understanding where the three particle wave function becomes asymptotic—in some cases, he had to go out to several thousand fermis in a situation that apparently had nothing to do with the Efimov effect. Sawada hit on another aspect of the problem with his claim that the continuum coupled channel model when discretized cannot give the correct asymptotic behavior—in fact, must predict no breakup cross section. But the CDCC method claims a great deal of empirical success. Kawai claims that this fact comes about because the fomulae as used rely on wave functions bounded by other than asymptotic considerations; in other words, the physics guides the practice, and makes sense.

The issues raised here as to the relation between mathematics, physical models, physical intuiton and the practice of physics are, in my opinion, not easy to meet. In a sense that physicists like to avoid, they are technical philosophical issues. If so, I suspect this problem will be with us for a long time. Obviously, I could be wrong. For instance, the theory revived by Glöckle that constrains the region of interest in the three particle wave function to an infinite strip whose width is bounded by the range of forces could meet the problem in a satisfactory way. So might Chandler's "New Dynamical Equation," if he can attract practitioners. We will see.

It has long been thought that the peculiar behavior of the nucleon deuteron effective range function near threshold can only be understood by means of three body dynamics—specifically by the single nucleon exchange—and not by means of a potential model. Yet Adhikari showed us that by making a potential model using the long range $1/r^2$ Efimov behavior and an appropriate cutoff outside a short range potential, the low energy behavior is readily simulated. This also gives us a simple explanation of the correlation between the three nucleon ground state binding energy and the doublet scattering length known as the Phillips plot. If so, we now have a "shape independent" approximation for the low energy three nucleon parameters. This means there is no likelihood of being able to distinguish 2 body off shell from three body force effects until we move well away from threshold.

Of course, some observables might still be sensitive to one effect rather than the other. The Osaka–Graz work reported here explores offshell sensitivity in n–d polarization parameters in an interesting way. Or the new type of low energy parameterization presented by Hasegawa which he claims represents a cancellation between two body force effects and hence leaves a sensitivity to three body forces—might give us a tool. But systematic investigation is needed before the case can be made.

Some experts are dubious. Adhikari has not derived his model from three body theory, and Sandhas says he has tried to do something similar and failed to justify it. The case is questioned even more strongly when it comes to the p–d Phillips plot. Alt has told us that no known approximation to Coulomb effects in the p–d system works. Only an exact three body calculation can be believed. In general, I agree, as does Zankel. He is now doing exact Coulomb calculations and does get a Phillips plot, also low energy parameters in the four nucleon and five nucelon systems. But lacking low energy precision p–d doublet phase shifts—which are currently beyond experimental reach—the question will remain open.

Generallly speaking, there were several places at this workshop where the question of how to distinguish three body forces from two body offshell effects was raised. So far as I am concerned, no clear answers emerged.

On the experimental side, we had considerable good news. The neutron–proton (isopin zero) phase parameters $^1P_1$ and $\epsilon_1$ have long looked peculiar from a potential model point of view. A systematic program undertaken at Kahlsruhe to settle this issue (reported by Klages) has paid off and achieved believable results; further work will clarify the issue still more precisely. One unexpected result is that the D phases now disagree with older potential models but agree with a new Bonn potential. This is very interesting since according to private communication from Sasakawa, this new potential binds the triton almost correctly *without* any three body force. Amusingly enough, new 12 MeV p–p polarization experimenets from Erlangen (Kretschmer) favor the older models rather than the new Bonn potential. The ball is back in the theorists' court.

Other places where experiment is ahead of theory are the beautiful investigation of charge symmetry breaking reported by Grüebler, the n – $^3$He experiments reported by Klages, the n–n scattering length results reported by Koori, and Ohsawa's investiation of the deuteron-polarized deuteron 4 body breakup reaction mechanism at 56 MeV.

One interesting aspect of this conference was several investigations of reactions with the lithium isotopes (Motobayashi, Kretschmer, Koori and Lehman). One reason for the interest is the solar neutrino problem, but the connection to the fusion production of tritium is of long range technical interest. Lehman shows that the 3 body character is clearly evidenced in the electron (e, e$'$d) correlation. The intriguing possibility of neutron-free fusion using polarized plasmas was investigated by Hoffman—unfortunately with negative results. We can expect to see more studies related to fusion in the future, as well as studies of low energy $N\bar{N}$ systems such as that reported by Plessas.

As to theoretical methods, we heard this morning from Fonseca that it is possible to replace the unsatisfactory Born–Oppenheimer approximation by a rotationally invariant formalism firmly grounded in three particle theory. Vanzani has show us how to get from the full N–particle theory to small cluster formalisms of various types. Sandhas has actually tackled the extremely difficult task of computing four nucleon breakup reactions.

The resonating group method is coming into our discussions in an interesting way. It can be used as the basis for constructing locally equivalent potentials, for instance by the techniques presented by Sofianos and Fiedeldey. These potentials or others based on them can then be used in few body calculations such as Oryu's study of the $3\alpha$ or $4\alpha$ systems. You have just heard from Schmid that the method needs to be refined by orthogonalization, and that we owe those who practice DWBA calculations on heavy nuclei help along these lines.

Finally, Redish has discussed the difficulties with using optical models at intermediate energies. He has raised a fundamental issue with regard to the proper relativistic treatment of spin and the relativistic scalar-vector model that has no obvious nonrelativistic limit. We can expect very interesting discussions on this issue in the future.

Clearly this workshop has proved to be both exciting and rewarding. I am sure I speak for all of us when I ask you to thank the organizers and their staff for the hard work that made this possible.