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SUMMARY OF WORKSHOP ON TARGETS.

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Gary Russell and I were co-chairmen of the session on Targets and Moderators held on Tuesday. I thank Tim Broome and Dave Picton for recording summaries of the discussion and the contents of the talks. We divided our discussions into two halves; target systems and moderator systems. For the target session we had three reports, one on the LANSCE target system by Gary Russell, another on the ISIS target cooling water activation by Tim Broome and a third by Fritz Takeda on the SINQ target thermofluid dynamics.

I just want to summarise some of the points that came up and invite you all to comment further. I have actually a second motive in making this summary: to get people to commit themselves to collaborate on several of these matters - that is another function of these ICANS meetings. I shall start by relating some of the issues raised.

One of them has to do with the method of cooling decouplers, particularly boron and lithium which capture neutrons by (n, α) reactions and therefore deposit heat locally at the site of the neutron interaction (compared to the broader distribution for gamma capture). We need to have better information on the relationship of heating (say) to proton beam power on the target, and on that basis to devise methods for cooling these decouplers (which we can at least imagine will get rather warm). I don't think that we have much data on this. The burden may lie on the Target/Moderator/Reflector designers simply to work it out by a combination of calculation and measurement. I hope that by the time we meet again, we will have more information on decoupling materials and their cooling in particular.

Gary Russell showed some calculated results that more or less confirm a trend to decouple at lower and lower energies. One of the first thoughts which Günter Bauer has disabused us of is to decouple moderators from their surroundings at some very high energy, like a keV, just to guarantee that the pulses will be unbroadened due to interaction with the reflector. As time goes on, we decouple at lower and lower energies (we abort fewer neutrons), but we still have not fully gone over to Günter's point, namely, "To use all the neutrons, don't throw any away".

There was another set of issues raised, namely - What are appropriate coolants for high power (fixed) targets? Water (H_2O) is a convenient coolant but in booster targets this extends the response time, so that we are led to think in terms of D_2O. Both have similar heat transfer properties, although D_2O is about 10% worse than H_2O. It's actually in some way contrary to what we think, that D_2O is a good moderator; it's only good when we have huge volumes used as a coolant. Where volumes are tiny (the ISIS target is cooled with D_2O) it is an inferior moderator and leads to shorter response times. I don't think that this issue is so important in a non-multiplying target.

A further question arises: If we push harder and harder on power densities, when is it necessary to go away from water as coolant and adopt, for example, liquid sodium or sodium/potassium eutectic? Günter Bauer has done some analysis of this kind and in the IPNS-II design (pursued long ago) the target was intended to be cooled by Na-K eutectic. The next stage questions that apply to even higher power densities are: When is it necessary to move the target material? When can we no longer get away with keeping it one piece standing still? The SNQ wheel-design is one method of moving the target material and spreading out the heat, the SINQ naturally-circulating Pb/Bi eutectic is another. So there are the questions: at what level of beam power must you go from one technology to another (or when must one cooling method be given over in favour of a more difficult one) and when must fixed targets be replaced by moving targets? Eventually, I think, we will have a more general understanding of this, but just now it's an issue that lies before us.

Further, when we address sources of higher power, comes the question in solid target materials of the levels at which thermal elastic shock becomes an important problem. We are capable of delivering pulses that are much shorter than the sound transit time across a typical dimension of a target. When a significant amount of heat is delivered (either uniformly or non-uniformly) to a part (say a disk in a fixed target), it will want to expand, but it cannot in the time that all the energy is delivered, so that after the heating impulse a sound wave will propogate and ring within the part. This gives rise to a number of stress cycles. The question is, when does this thermal elastic stress wave become significant in determining the performance of target parts, not only fuel parts but also windows? There have not been many analyses which relate to our particular technology.

> *A point raised in discussion concerned the problem due to stress cycling from irregularities in accelerator operation or from heat-up and cool-down. This was not considered to be a problem and anyway can be estimated by standard methods. It was suggested that it is relatively benign. We were reminded that the thermal stress is at its maximum in the steady state.*

We surfaced the issue of radiation damage in target parts and windows. A window test has been made in a collaboration between SIN and LANL. There is the question, what does a good window look like and how long will it last? Another problem we are now addressing in the IPNS booster target design is how to characterise the radiation and thermal cycling growth in uranium targets. The IPNS booster target uses the (anisotropic) α-uranium. Might it be better to go over to the cubic gamma stabilised phase? In practical materials with some preferred orientation among the grains - α-uranium grows in a preferred direction and shrinks in another - if the grains are not randomly orientated there will be large scale growth in a particular direction. A related point is that non-uniform irradiation will lead to localised growth and hence stress. This leads to - How do radiation and thermal cycling growth for our temperatures and our alloys depend on stress? This is a highly qualified question, and I think beyond us to answer: somehow we need to learn something about this.

Gary Russell has developed a very innovative split-target design. The basic motive is to provide a region that one can look into through the neutron beam holes, behind which there is no target (source of very high energy neutrons). An early question was whether the presence of this void between the moderators causes significant pulse broadening. This issue has been resolved by the results of various studies that Gary Russell has shown. The void does not significantly broaden the pulse.

In connection with this split target design, we need better understanding of how the moderator couples to the primary neutron source. To get neutron beam currents in absolute terms in relation to proton beam currents maybe not so difficult but you cannot just do measurements cavalierly. I tried them at Argonne and we were left with an uncertainty of a factor of 2; the lower limit says the calculation is good, the upper limit says the calculation is a factor of two too low. We found problems due to collimation elements in the beam lines which were misaligned (makes the measured neutron intensity lower), but somewhere there are still errors in our measurements which have not yet been traced. We should remember that both measurement and calculation are very difficult; the success rate for Monte-Carlo is very low.

The following points arose in a general discussion:

- *No real problem (excepting blunders in the representation!) come from the Monte-Carlo modelling. Mostly a reasonable approximation to the geometry will do.*
- *Proton beam measurements should be able to be made to 10% or better.*
- *It was suggested that maybe the neutron part is perhaps the easier of the two. Care has to be taken to ensure that the proton beam current measurement determines what actually strikes the target. In the early days at ISIS, 50% changes were found in the number of neutrons per proton. This was cleared up with a better monitor of what goes onto the target.*
- *Gary Russell said that (despite several difficulties) they were planning a measurement of the LANSCE system in the very near future.*

One of the motives for the split-target is to produce a lower background of the very high energy neutrons. Somehow one needs to know the extent to which this design really works. It's rather difficult to make the comparison between this target and a solid target, nevertheless it would still be interesting to know if the flux trap geometry does work to produce a low background of very high energy neutrons. A secondary question: here is this hole - should we leave it empty or put something in there? If you put in tungsten or uranium it's no longer a split target! But what about D_2O or beryllium, is this good or harmful? There are questions of how to cool it. That would be an interesting study! Another question for split targets is, should the pieces be of the same material? (eg the front half [where the power density is the highest] of tungsten and then use a high gain material [like Uranium] at the back). These are the questions which it would be hoped would be answered by the time of the next ICANS.

There are some resolved issues. We know that the split targets are feasible: maybe there were no doubts, but now there has been one run, we know it works! The question of the pulse broadening (I say) has been resolved by Gary's calculations. In a separate report, we found that codes now exist to calculate satisfactorily the activity in water coolant and in fact in complex loops of cooling systems; that has been checked out on ISIS and the procedure seems to work. Fritz Takeda showed great progress on the understanding of the systematics of naturally circulating liquid metal target systems. There have been new methods developed which are being employed to refine the design, so that is a resolved issue, but there are still many unresolved design questions.