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Transcript of the Workshop to Discuss Plans for a National High Intensity Radioactive Nuclear Beam Facility

J.M. Nitschke

October 1989

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Transcript of the Workshop to Discuss Plans for a National High Intensity Radioactive Nuclear Beam Facility

Lawrence Berkeley Laboratory
Berkeley, California 94720, USA

October 19, 1989

Edited by J. Michael Nitschke

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RADIOACTIVE NUCLEAR BEAMS

THE FIRST INTERNATIONAL CONFERENCE

October 16–18, 1989, Berkeley, California, U.S.A.

WORKSHOP TO DISCUSS PLANS FOR A NATIONAL HIGH INTENSITY RADIOACTIVE BEAM FACILITY

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Lawrence Berkeley Laboratory

Auditorium (Bldg. 50)

19 October 1989

AGENDA

1. Purpose of the Workshop (JMN)
2. Scope of the project (JMN)
3. Comments from other laboratories
4. Discussion:
   a) Physics questions relevant to machine performance/parameters (Support from Theorists)
   b) RNB production accelerator for protons and ^{3}He beams
   c) RNB production methods (i.e., ISOL, He-jet, Fragmentation)
   d) "Direct" use of RNBs with ~100 KeV energy
   e) Post-accelerator options (i.e., Cyclotrons vs. Linacs)
   f) Post-accelerator parameters:
      - Energy range
      - Energy resolution
      - Mass range
      - Beam quality
      - Pulse structure
      - Duty factor
      - Beam purity
      - Intensities, etc.
   g) Test sites for development of RNB Facility components (i.e., LAMPF, LBL, ANL, TRIUMF, CERN/ISOLDE, others?)
5. How do we proceed from here? (i.e., Workshops, Users group, other?)

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Lawrence Berkeley Laboratory
October 19, 1989

M. Nitschke (LBL):

Good Morning. I would like to welcome you to the Workshop on Plans for a National High Intensity Radioactive Beam Facility. Before we get started I have some organizational announcements:

...we want to discuss until about 10:30, then have a coffee break, and continue until lunch at about 12:30. So we have a net total of about three hours to discuss what we want to do. I would like to ask your permission to record this session. We will later have the recording transcribed, edited slightly, and have it sent to those that signed the workshop registration and mailing list.

Also, I would like to announce that if you would like to visit the HERA facilities at the 88-inch Cyclotron, Dick Diamond told me that he will be there and is willing to show it to you. The same applies to the OASIS separator at the SuperHILAC, this afternoon if you would like to come up, Phil Wilmarth and myself will be there to show you the SuperHILAC and the on-line isotope separator. If anyone is interested in touring the Bevalac, including the Japanese radioactive beam facility please come down to the ARC office. With this we can actually get started with the subject matter.

To begin, I would like to say a few words about the purpose of the workshop, and about the scope of the project. It is clear that there is no unanimous agreement about what a high intensity radioactive beam facility should actually look like in detail. Already in the title there is an assumption, namely that it is a high intensity radioactive beam facility. Now, for some people high intensity is something else, than for others. Some people can do experiments with beams of 10^6 or so ions per second and others need 10^{12} ions per second. So, this is actually a big subject in the entire topic: the many orders of magnitude change in beam intensity that we will have to expect from a facility regardless of what method we are going to use to produce these beams. We all know that on the extremely neutron-rich and neutron deficient side beams may get down to, let's say, 1 ion per second or even less. Also, the definition of radioactive beams, of course, is

not entirely clear. Do we want to call beams that have 100 keV total energy already radioactive beams? Do we want to call the astrophysical beams of 1 MeV/u radioactive beams? Are only beams above the Coulomb barriers radioactive beams? So all this is still in flux and will define itself, I think, as time goes on. We have for many years here in Berkeley been looking at different ways of producing radioactive beams. As you know, projectile fragmentation is working quite routinely, but at some point we were asking ourselves what would it take to get a state-of-the-art high intensity facility that would really make use of the limits of what is technologically and physically possible in terms of primary beams intensities; that is, of primary beam intensities from which then the secondary beam intensities could be derived. This lead for example to ideas about a fast cycling syncrotron that would have beam energies of hundreds of MeV/u where, through projectile fragmentation, we would get secondary beams of relatively high intensity similar to ideas now being designed, constructed and realized at GSI, except that we were thinking of even higher intensities. We made a few "back of the envelope" type calculations and realized that, if we wanted the highest intensities this was not necessarily the best method to do it. If we wanted to have beams at high energies it was the right thing but for low energy beams one would have to start with a high intensity proton beam and use the ISOL technique.

Those of you who have been at NSAC meetings have seen this view graph and I will just show it briefly to make a comparison between producing radioactive beams with protons vs. heavy ion projectile fragmentation. One can write down a generic experiment: say we want to make a radioactive beam using the projectile fragmentation of ^{139}La. To make a beam of an isotope around mass 100 that may be a good way of doing it, and one would get a production intensity of 2×10^{11} with a relatively light target of, for instance, carbon with a target thickness (in order to reduce the momentum spread to a reasonable value) of 1 gm/cm^2 and a generic cross section of 10 millibran. With these numbers projectile fragmentation would give a primary beam intensity of about 10^8 per second at an energy slightly below the primary beam. Now, if one wanted to produce the same isotope with an ISOL technique one would start with protons and use reverse kinematics, in a way. The projectile in this case is typically a 600 MeV proton beam with an intensity of 100 microamperes and the target is molten ^{139}La; a target that has been extensively explored at ISOLDE. The target thickness is perhaps 200 gm/cm^2, and again the cross section is about 10 millibran. In this case one would get a primary radioactive beam yield of about 5×10^{12}. You see that in the primary yields there is a difference of about 4-5 orders of magnitude. Of course that means very little at this point. Because if

your don't want to use the high energy beam you have to decelerate it, and if this beam is too low in energy, which it obviously is, it's just thermal, then you have to post accelerate it. So from here on all kinds of factors are entering the subsequent yields and I put in just some typical numbers. You may, for instance, in the storage and cooling of the radioactive beam obtained through projectile fragmentation loose quite a factor due the half-life of the isotope, the deceleration process, halting and cooling in between and so forth; so by the time you are down at the Coulomb barrier you may have lost about 2 orders of magnitude. The same is true if your want to post-accelerate this beam here. You will have losses due to ionization; there will also be losses due to stripping (there have to be at least one or two stripping processes), there is an acceleration loss, transmission loss, and so forth. So one will have perhaps the same order of magnitude loss that was incurred in the projectile fragmentation case, and we may get down to beams on the order of 10^{11} per second. But again, the enormous differences in target thickness and primary beam intensity make this method by several orders of magnitude superior to the method of projectile fragmentation. At least that is my view of it. It may be possible to come up with an accelerator of superior design that would increase the projectile fragmentation yield somewhat. Now my intention is not to present a proposal for an actual facility somewhere in the country or at LBL but, rather to open up the discussion with a more or less generic facility which has the components that we need to discuss today.

One would start out with a high intensity proton accelerator, that doesn't have to be necessarily a circular accelerator like a cyclotron (TRIUMF, PSI) it can be a linear accelerator as long as one gets protons with energies on the order of 600 MeV to 1 GeV with sufficient intensity (100-300 microamperes). One would not want to start out with anything like 300 microamperes, but it would be good to be able to do it in the future. Now, I said protons, but as you will see or as you already know ^{3}He in certain cases can provide quite an advantage over protons in producing very proton rich isotopes and deuterium is also in some cases preferable to protons. Then such a beam would interact most likely with a target of about ~100 gm/cm^2·thickness. We will hear from a representative of LAMPF today that one can go an alternate route; we heard already during the RNB conference that one could go maybe to much higher beam intensities (by a factor 10) and a thinner target, but I will leave that out for the time being. I will just consider the "low" proton intensity high target thickness approach, and liquid or a molten targets from which the radioactive species would emanate. Then there is a very crucial step after this: because of the enormous activity that is being generated at the target (alpha

emitters of significant amounts in the actinide region or in the lead region would be produced depending on the target, there may be some formation of plutonium and so forth) there is a chemical separation step of some sort necessary. Most likely it will be a combination of steps like using very simple thermal effects, a cooled transfer line, condensing certain species, up to refined molecular chemistry that one can get by adding halogens (fluorine and chlorine). New chemical methods would have to be developed in the future depending on what kind of species one wants to accelerate. Then this chemical separation, which hopefully would get rid of the bulk of the unwanted elements would be followed by an ion source. We have already heard some ideas on ion sources. ECR sources look very promising; surface ion sources are very specific as far as the chemistry goes; there are some ideas (and research is being done) on laser ion sources. They are very intriguing because they are very specific and one could even think of producing isomeric beams with them. Professor W. Fowler when he responded to our invitation to come to the conference said that isomeric beams would be what he would like to discuss. Yesterday from Dr. Kutschera, we heard some interesting ideas related to laser ions sources. By the way, Prof. Fowler is becoming a member of the Legion d'Honeur in Paris this week, which is why he is not at this conference; otherwise he is always very interested in the astrophysical applications of RNBs. Coming back to the ion sources, depending on the post-accelerator it may also be advantageous to have pulsed ion sources. We know of the very successful developments by Reiner Kirchner at GSI making a pulsed ion source that can discriminate between different elements and enhance certain elements and suppress others depending on the vapor pressure, the diffusion properties, the half-lifes and so on. I don't remember exactly what the enhancements are, but they are at least an order of magnitude, sometimes a factor of 40. Of course laser sources are naturally pulsed sources because you cannot get CW lasers that have the power and probably the equipment can't stand it anyway. Laser sources would be matched to pulsed post-accelerators. All this is extremely difficult technologically because, as you realize the radiation level near the target under which this has to operate are tremendous; tens of kilorads. Just think how large an ECR source for instance is with all its power supplies, magnets and pumps; even if you miniaturize it, it is as least as large as this table here and has very delicate equipment.

The ion source would have to be followed by an isotope separator. The isotope separator could also be used to produce low energy RNBs as is done at ISOLDE for low energy radioactive nuclear beam studies like colinear laser spectroscopy, solid state physics, production of isotopes and so forth. By the way, production of isotopes of

course could also be incorporated in a future project; this is something we may want to talk about to broaden the spectrum of such a facility. Then, in order not to contaminate the post accelerator and to get the necessary beam purity (we heard about this yesterday), we need isobar separation (close to stability this will be a difficult task but as one gets further away from stability a resolution of about 1 part in 30,000 will give the separation of adjacent isobars and the purity needed for the post-acceleration process. Now, the post-acceleration process is broken down into two parts. This is probably necessary because almost nobody to my knowledge is prepared for the low energy per nucleon low-charge-to-mass-ratio ions that have to be accelerated to energies above the Coulomb barrier. We have pre-accelerators that can accept beams that usually originate from PIG sources or ECR sources that have been optimized for high charged states. But we will have to think carefully of how to design low beta structures as RNB pre-accelerators. Another generic problem that is probably unavoidable, regardless of what people are planning in different parts of the country or internationally, is that we unfortunately will have to strip, at least once if not twice; perhaps once at about 100 KeV/u and then a second time, like is done at the Unilac at the moment and at the SuperHilac, at around 1-1.5 MeV/nucleon. Otherwise these accelerators become a mile long and the cost goes through the roof. Now, I have drawn here a post accelerator in a linear fashion again, but that is not necessarily so. We should discuss what kind of post accelerator people think would be best at this point. We already have some ideas from Dave Clark on cyclotrons and yesterday we also heard about the coupling of GANIL's two cyclotrons to produce radioactive beams. This again is an open point for discussion. Finally, the post-accelerator is supposed to give us energies above the Coulomb barrier, perhaps a generic number would be 10 MeV/u; but we have heard from the astrophysics people that they are interested maybe down to 0.2 MeV/u.

I don't have much more to say and I would like to open up the discussion except to just remind you that we have a tremendous sources of information available from the research done at ISOLDE at CERN. You look at their user's handbook and you get all the intensities because, once you have the numbers at this point here the calculation of the subsequent steps is really relatively straightforward: we now have from Hermann Wollnick's group initial calculations of high transmission, isotope/isobar separators for the Japanese Hadron Project, for ISOLDE III, and others; we know how to build RFQ's (we have an expert here, John Staples) and also LAMPF and others have a lot of experience in linacs, and of course we know how to build post accelerators either of the ordinary kind or superconducting. So having a number here, that is the primary number

of radioactive ions per microampere of protons, is an extremely valuable fact for which we have to thank the ISOLDE people, and they will play a very crucial role in everything we do in the future. I have just one more fairly straight forward slide: a cut through the valley of stability at A=125, just to show you what kind of resolution is necessary for the isobar separator. Close to stability, in order to separate isobars, you might need resolutions as high as 50,000 if you really are interested in separating let's say ^{125}Sn from ^{125}Sb. If you go further out for the more exotic beams, fortunately Nature has provided us with a nice coincidence, namely that the valley gets steeper as we go further out and we need less and less resolution to separate isobars. So, that's very nice because these beams are probably the most critical ones and the most interesting ones, and those hopefully we can provide with the highest purity. This is an example of an isotope/isobar separator that was developed for the Japanese Hadron project. There is a first part that consists of all magnetic elements. It is a relatively complicated geometry to get the resolution and purity that is needed for this purpose. You also see it is not a minor component in the entire complex of accelerators and other items just from the length of it; such a device would be something like 20 meters long. Let me stop at this point talking about the generic features of an RNB facility and ask perhaps representatives from other laboratories to make some comments. What their thoughts and their plans are.

G. Wozniak (LBL):

I think the concept of a radioactive beam accelerator is a very exciting one, but I am a little concerned about so much emphasis on a particular concept or way of delivering the beam. I think we should really be discussing now, the physics that can be done and how one would justify such a facility. The specifications you would need to do the science, and then leave it to some working group or to accelerator physicists to come up with the best design to produce those kinds of beams. I think it is a mistake to concentrate too early on one particular kind of concept.

M. Nitschke:

I appreciate your comment, Gordon. If you look at the agenda, we actually have this in mind. From the agenda, you can see that we wanted to discuss physics questions relative to machine performance, but we I didn't want to make this an extension of the conference. Because the physics was actually supposed to be discussed at the conference. I think we did it for three days; there were posters, and there will be proceedings. If we go more into physics we will not get through today. So perhaps we could stick to the

agenda as it is outlined. This agenda was arrived at in collaboration with the local organizing committee and as you see we will come to some rather relevant questions, we will have to discuss all the things you just mentioned. We will have to discuss energy range, resolution, mass range, pulse structure, intensities and so forth which are some of the parameters of a facility like this. At this point perhaps somebody from another laboratory would like to say something.

H. Haas (CERN):

I think it would be interesting to find out an optimal solution for every one of the components what one would want. My first comment would be on the first accelerator. You had mentioned other particles. Of those as you know we have ^{3}He at CERN. And we have quite some experience, not only the positive one that you had mentioned, but also problems of taking the high power. What appears to be interesting are actually deuterons. They give you a factor of two more particles per current and current is what you have to supply. The other point is that that the 600 MeV or maybe even the 1 GeV might not be the optimal energy. You may want to go a little bit higher for the target thicknesses to get the optimal intensities.

L. Buchmann (TRIUMF):

I mean you can think about primary accelerators but if you want to build a new one that is about 90% of the cause. You can do that but you have to have a very good reason to build another good high intensity proton or even deuteron accelerator in the world. I don't think a radioactive beam facility would justify it.

M. Huyse (Leuven):

One of the possibilities that is not mentioned here is to take a high intensity low energy proton accelerator and then dedicate it more to the nuclei close to stability where I guess if you take your yields we arrive at the same number of such a high intensity also. So in the cases of astrophysical studies where the most interesting cases are close to stability I think this start scheme is also something to think about.

M. Nitschke:

John or Dave would you like to say something at this point about the thoughts at Los Alamos of how to go about this?

J. McClelland (LAMPF):

At Los Alamos we have begun to look at what role might play in research and development activities towards a proposal for a U.S. radioactive nuclear beam facility. I probably am not a person well known to this community but there are certainly people that were involved in this that you do know. In fact they are here at this conference now. In particular Will Talbert, Dave Vieira and Hermann Wollnick were involved in this. There are a number of people from other divisions within Los Alamos besides the medium energy physics group, including the theoretical physics and accelerator technologies division. We also have the same slide for what the elements might be and try to address what the issues are that need to be explored, for each one of these components. The list becomes very extensive. TRIUMF has certainly looked at many of these and of course the Isolde group at CERN. I think the primary difference is that the kinds of intensities that are being discussed -- up to 100 microamps -- put a different perspective on this problem especially in terms of the high radiations involved. The LAMPF management is certainly aware of the growing interest in such a facility and this was evidenced during the NSAC deliberations as well as the DNP town meetings. We are also in the process of evaluating future projects at LAMPF, so this is a very good time to have that discussion. Some of the things that this community may want to consider when thinking about R&D projects would be that we in fact do have existing high intensity proton beams. I'll give you a breakdown as to the types of beams that are available. There's is considerable remote handling experience because of the activities we routinely deal with in our target cells. Experience in design and operation of large accelerators and extensive experimental support. It would seem that these components would be of some use in the kinds of activities that people are considering.

Let me give you a brief overview of what the capabilities of LAMPF are in this respect. There are a number of beam lines available at LAMPF. We took a look at several of our existing facilities to see what might be available for this project. In terms of the main LINAC, of course you know this is a 800 MeV, LINAC in its production mode. We do run it at lower energies for specific applications. We run three different types of beams. The primary one milliamp beam is an H^+ beam . We deliver H^- beams to the proton storage ring and also to the nuclear physics area. So this is the high resolution spectrometer and some of our newer nuclear physics facilities. In the main

beam line the intensity is fixed at one milliamp. There is very little opportunity to change that because of the production which requires it for the meson facilities. In the what's called line D operation, here in addition to feeding PSR there are lines available where one can run variable intensities up to 100 micro amps. And by variable, that can be down to fractions of a microamp. That is accomplished by chopping the beam at the front end of the LINAC. The beam delivered in this area here is typically of lower intensities, the limit there is less than 10 microamps. That's primarily due to our ability to stop the beam. So the available beam stops. So what you see is that there are basically three types of beams: one milliamp, up to 100, and up to 10 microamps are available. The structure of this beam is 800 microseconds at 120 Hertz, 20 of those pulses are dedicated to the proton storage ring so you're left with 100 pulses per second available for all these other facilities. It is possible to piggyback these beams. The accelerator can actually handle a higher peak current than what's being run for the 1 milliamp beam so it's possible to stack H^- on top of an H^+. So that's the way we could accomplish these average intensities up here.

Since we don't have too much time, the one point I would like to make now is that we did look at two types of production targets. We heard quite a bit about the ISOLDE type where the fixed target is in very close proximity to the ion source: 10's of centimeters. There is another approach which is a thin target which is typically 10 milligrams, thin enough to that the recoil particles can come out. I'm sure that most of you are aware of this technique. It allows one to capture the radioactive nuclides in an aerosol loaded Helium jet and transport them away from the high radiation area. So this is the point that you might want to consider. If you are talking about very complex sources matching into other post accelerators the component viability may be a very crucial issue. This idea of transporting the activity outside into a lower radiation field may be very advantageous. Of course you are giving up a lot in target thickness so you require the very high beam intensities to make up some of that. I think what is required here would be a demonstration of its performance. This type of system has not been put into a 1 milliamp beam to see what the yields are. So we've looked at some of the components, one of them is a target cell that can withstand the 1 milliamp of beam loading that you would have. It is a very simple design of two concentric cylinders with water cooling in the jacket. For a 1 milliamp beam near our beam stop -- the size of which is about 4x4 cm -- there is 17 kW of power which can be dissipated with this technique. So that the maximum temperature rise at the center of the beam is not in excess of 220 degrees F. This type of system has been modeled and apparently would

work. There are places to insert such a device. This is in the beam stop of the primary line, the 1 milliamp line. These inserts come in and out of the beam. One would put this target cell on the end of one of these inserts which is now available for use, so it could be done rather quickly. Hermann Wollnick has looked at some of the magnets that are available at LAMPF that we could refurbish with some new vacuum jackets to produce a prototype mass separator to diagnose some of these tests that one could think about doing with the various systems. This is a very simple dual quad triplet with a 45 degree bender that gives a mass resolution in excess of a 1000. This is something that can be put together rather quickly for diagnostic purposes.

We also believe the thick target needs to be pursued. We are not suggesting that only the helium jet or thin target be the only approach to be investigated but, because there is so little experience at present, that is something that one may want to consider and could be done quickly. In terms of thick targets we do have the beams that are available. We did come to the conclusion, however, that new facilities would have to be constructed, mostly, because of the remote handling requirements that are necessary to get up to the kinds of currents being discussed. I'll just show you some of the remote handling facilities we have at LAMPF. This is a remote manipulator, typically when we service one of our target cells, we are dealing with radiation fields from 5 to 10 krads per hour. Personal exposure must be kept below 1 millirem per hour. So the technique that's been developed at Los Alamos is a robotic arm that is controlled remotely from the site where the work is taking place by a feedback system. So anything you can do with your hands you can do with this device here. Typically there are three people working on this for two shifts a day at two different target stations. So there are 13 people involved in that operation. The training is quite substantial. It takes a couple of years before someone is quite good at this. It's not something that you get into quickly.

So I didn't want to go on too long but I certainly agree with the statement that was made that the physics justification is certainly an important ingredient in all of this. It would probably dictate the types of technology that would be employed. We strongly endorse the formation of a radioactive nuclear beam user's group and that that group consider Los Alamos for appropriate R&D activity. And to promote that idea, we propose hosting a workshop in Los Alamos as early as the end of March to pursue some of these goals of specifying what needs to be done and what laboratories and university groups would do, and what part of the work. There is a time slot available for one of our meeting facilities at Los Alamos on March 28 and 29th, and we could think about touring

LAMPF on the 30th, say. If this time were good, and we could get a reaction soon from this group, we'd be willing to try to put that together.

J. M. Nitschke:

Thank you, John. Perhaps, at this time, could you entertain some questions?

J. McClelland:

Yes. I'd be happy too. There are experts that could answer detailed questions here.

Q:

What is actually feasible for the target thickness at an ISOLDE-type facility?

J. McClelland:

There are experts here, but I understand that target thicknesses up to 200 g/cm^2 have been looked at and there have been feasibility tests to see which of those targets would survive at the high currents and there are a few of them that do.

H. Ravn (CERN):

I could comment on the thickness. Our targets are actually ranging in thickness depending on the speed we want to make, how short lived the isotope is. Because it all depends on the inorganic chemical properties of the element. They range between 13 grams to over 300 grams per square centimeter and obviously here we're speaking mostly about the thick ones. It's mainly a matter of delay time. If you want to produce millisecond isotope you are often going into very thin or dispersed fiber or powder matrices that give thin targets. I would also like to comment a little bit on the ion source you showed of ISOLDE. Many people fear to have a delicate device as an ion source close to a high intensity proton beam. Actually that is not at present our major problem. First of all, the 10 cm were put there to enhance short, very short-lived isotopes. We have in the past had the ion source 1 meter away from the target. This is not a great technological problem to do that. The major breakdowns are usually not in the ion source or in the target. It's trivial problems like cooling water, vacuum, O-rings and gasket,

things that are most likely to be also in the Helium jet gas cell. So there are a number of new developments like we have a running laser ion source with 20 percent ionization efficiency -- and we have demonstrated that for only one element, yterbium -- which is pulsed and there all the complexity is as far as you want it form the target.

J. McClelland:

As I said, I was not proposing that this was the only alternative. In fact one might find that a combination of both sources in proximity to one another might be able to tune in to what type of species you are interested in. I think that this idea of very neutron rich radioactive species is probably going to push you to very short half lives. And than that might be the issue then, as to how fast you can transport these things out of the target. The helium capillary of course is running at a very high flow so it may have an advantage that it could access the short lived activities more efficiently. But it will probably suffer with respect to the thick target for the longer half- lifes. So I think some combination may be the appropriate way to go.

R. Stokstad (LBL):

This aerosol chemistry I can imagine is somewhat complicated. Are there any limitations in what sticks to these aerosols and is carried along with them.

J. McClelland:

Why don't we have Will Talbert, the expert, comment on that.

W. Talbert (LAMPF):

The limitation is mainly that the volatile elements will not stick to the aerosols, but all the non-volatiles which are difficult in the complementary thick target approaches, stick very well and are transported very efficiently. We have made tests of the transport efficiency through a helium jet capillary at LAMPF and found out that the transport efficiency is almost independent of elements so long as you're not talking about the gaseous elements. And we do see that in the halogens the transport is perhaps half as efficient as in all the rest.

K. Sümmerer (GSI):

I should repeat one comment that we had already in a private discussion. I think the target thickness for the thin target approach of 10 milligrams/cm^2 refers to a product where you can gain from the recoil velocity of the fission. When you talk about a pure spallation, you are probably down by another factor of 100 due to the slower recoil velocity of a pure spallation product.

J. McClelland:

Well that's absolutely true. I think the system that I discussed here is optimized for the fission yields. However, one can think about a very thin foil that is in a spiral form inserted into the same concentric tube. Now a very thin target that would be able to access the spallation products in an equal efficiency. So what I showed you was for fission but there is natural extensions of this idea for spallation. And also one can think about taking that outer volume now the water cooling and inserting many cylinders to make up even a factor of 10 in target thickness using the same type of technique. So any individual cell would be a 10 mg coating but you would have perhaps 10 of these cells intercepting a beam.

Q:

Would you still be able to cool the inner part then?

J. McClelland:

Yes, you would think of having the jacket containing the water flow just as this one did, but now instead of cooling a single one it would cool many of them so you have these little water flow diverters even in this design that I showed that you can accommodate that type of design. Again these have not been built. What I showed was a finite element calculation which was used for a system similar to that where we had two concentric spheres in the main beam line. That system was in for many years and worked quite well. So we feel that works. Any extensions from that we would have to work with people on designs and tests.

W. Kutschera (Argonne):

I would like to know whether there is any experience in going to these very high intensities and extracting secondary particles? As far as I understand at CERN, ISOLDE the beam intensities are in the microamp range so far. We are talking about milliamps. Please correct me if I'm wrong. I would just like to know or hear some comments of experts whether that's just a scaling of a factor 100 or whether there are quite some problems.

J. McClelland:

The 1 milliamp was for this thin target which we felt confident can work. I think that making the jump from 2 or 3 microamps which is done now up to 100, in my opening remarks, I think that's where the challenge is. We would think of doing this in steps, perhaps building up a facility that could accommodate up to 10 where you're really trying to make that transition of 1 to 10 and understanding the problems and perhaps the feasibility of 100, but I think the problems become so enormous that I don't believe that a simple scaling is going to get you to where you want to be.

W. Kutschera:

Is the sketch you showed of the thin target something you're planning to do?

J. McClelland:

No, this is something that is straight forward to do, in the near term. I don't believe that LAMPF is taking the position that they are going to go forward with this independent of this community. If people want to do it they would come to Los Alamos, and actually do the work. I mean there are services that can be provided at LAMPF but there is no flexibility in our operation that would let us take on a major new project for ourselves. It wouldn't be our intention to do so anyhow.

R. Stokstad:

What's the cross section for a 800 MeV proton to induce a fission and what is the cross section for a 30 MeV proton to induce a fission?

J. McClelland:

I don't know the answer to that. Will, do you know? or Dave?

W. Talbert:

About 10 barns total cross section for fission, at 800 MeV; that means for a particular species on the order of a few millibars at the peak.

Q:

How different is it at 30 MeV?

A:

I don't know.

M. Nitschke:

I think one of the main things is that when you go to high energy fission you fill in the valleys in the fission product distribution. So that you can extend this entire range.

E. Roeckl (GSI):

I think we are going into too much detail, but let me still make a comment to thin target versus thick target, because there seems to be one -- I think important -- point which is not on your list, namely, your thin targets are solid targets and the helium transports all elements. Hence you have to do the chemistry somewhere else. So you can not use the very successful ISOLDE chemistry which is done inside the thick target, such as liquid targets and so on.

J. McClelland:

I don't believe that it is so linked to the chemistry. The aerosol and the helium is transporting these materials to a skimmer and the ionization then takes place at that point.

E. Roeckl:

Yes, it takes place for all elements. Unless you do laser, or pulsed or whatever, so you do have to do the chemistry somewhere else. Whereas ISOLDE is doing it in most of the cases, sort of, in the thick target.

J. McClelland:

I guess, it's not something that bothers me. There is mass separation at some point.

E. Roeckl:

Well , you can make isobar separation but that's quite demanding.

G. Wozniak:

I am still having a little problem with this discussion. And I guess from the standpoint of being a scientist, what I would like to know is can the machine people deliver me a nanoamp of ^{11}Li or of ^{26}Ne, and I don't care how you make it.

J. McClelland:

Well, we had the same problem. It's difficult to construct a figure of merit for what it is you're trying to do if you don't know what the physics driving that device is. But you won't get a request and it would be nice if the community could come forward with a ...

M. Nitschke:

We cannot deliver a nanoamp of ^{11}Li. I think we can agree on that. Are there some other comments? I would like to ask Cary, whether you want to make some comments regarding Argonne National Laboratory's plans and ideas.

C. Davids (ANL):

That's about where it is right now. At Argonne we have the ATLAS facility which many of you may be familiar with, and the we have had a few discussion in our group regarding what we could do in terms of radioactive nuclear beams. I might remind you, that we have a superconducting linear accelerator that is used as a booster accelerator from an FN Tandem. From this facility we are able to obtain heavy ion beams. Right now we run with the tandem but we know are in the process of installing a superconducting booster which will take the output form an ECR source; and we have done a lot of resonator development with this. The positive ion source shown schematically here, is an ECR source mounted on a platform run at 300 kv, which is then put into an injection line into a series of three positive ion injector linacs made up of low beta resonators which we have developed and tested at our facility. And then the tandem which is sitting here will be replaced by beams from this facility. The important point for this discussion is that we have the lowest beta resonator which is set for about 0.009 v/c value. Now, our ATLAS facility delivers beams up to 17 MeV per nucleon at the low mass range and will deliver Uranium beams at 7 MeV per nucleon. We will be able to cover the whole range from about 17 to 7 MeV per nucleon with lower energies at the higher masses. So we consider this facility to be an ideal type of post accelerator for such a radioactive beams facility as has been discussed up to now. The front end of the device of course, is the problem. Now our ideas have gone no further than ideas, and a few "back of the envelope" calculations. We are considering how to do a fairly quick and dirty activity in radioactive nuclear beams. This is a diagram of the ATLAS facility, where we have the booster linac which takes beams form the tandem which is in this vault right here, then injects into a second stage; the ATLAS linac addition. So we have a booster linac which goes up to 6 or 7 MeV per nucleon and then we get the higher energies here and we have experimental areas set up for both sections. Here is the positive ion source.

Our current ideas, that we have been discussing right now, relate to the installation of a small 30 MeV cyclotron which would accelerate protons, deuterons, ^{3}He and alphas -- a high intensity cyclotron. Which we would use for production of radioactive isotopes in the A<21 area, that is the ones that have been mentioned in talks in the past few days for astrophysical studies. We have done a few estimates of intensities that one could obtain. And using a facility here -- I've just drawn in the target and ion source and a

magnet in here schematically -- with the present injector linac, as constituted, we could consider using an ECR with charge 2 ions. This will give you about 30 percent of the full charge 1 ions, if the ECR is run as we run this one at 300 KeV. Now, if you go to lower energies, of 60 KeV then you have to have fully stripped ions. So that would be a possibility without developing a whole new series of lower beta resonators. With the current ATLAS injector one could use plus 2 ions if the source were run at 300 KeV. So that's really the extent of our thinking right now. That the ATLAS superconducting technology is ideally suited to such a facility and we think the low energy part is solvable. Although one could conceive of using the time between now and when such a facility were developed to develop a lower beta resonator which would allow you to use the conventional 60 KeV type ion sources. That's all I have to say.

D. Vieira (LAMPF):

What type of target and ion source coupling schemes have you thought about?

C. Davids:

Well we haven't really done any discussions of that. Since the elements that we're talking about are mostly volatile like nitrogen, oxygen, fluorine and neon, those are the elements that we're talking about. You would have ion source which would take advantage of the volatility.

J. McClelland:

Do you know what the approximate cost of ATLAS is totaled to be?

C. Davids:

The capital cost of the last three resonators plus a new building was 7 million dollars. I don't know what people consider the total cost of the ATLAS facility. It's not 50 million or 100 million dollars, it's some number I think, less than that. But you'd have to get that directly from Lowell Bollinger, say, at ATLAS. It's a modestly priced facility. The technology was developed inhouse and the resonators are all fabricated in our shops.

D. Horen (ORNL):

I have a point, I think we're losing the focus. What I would like to see I think is sort of what was pointed out before. Some simple expression of a physics range -- and you can define it any way you want to because you're talking about a national facility here -- and then I would like to know what is the manpower that is going to be available for that particular physics that you've already discussed you're going to design this machine. Because there are a lot of other physicists out there and from one of the workshops at one time there was a grouping of the radioactive beams, and the equation-of-state people, and some of us reaction people. It seems like you're throwing away two-thirds of the manpower from your design. I gather, because you keep talking about these very low beams and I think if that's what you're talking about your national facility then I think you ought to clarify that right now so that when some of us leave we know exactly what the object of this workshop was and these goals. I don't care about the details of all the design of the elements. I agree all that can come later. Because it doesn't make any difference anyway. If you can't get the money for it to talk about it.

R. Stokstad:

If Dan's interested in heavy radioactive beams he can go down to the 88-Inch Cyclotron this afternoon and see an experiment being done by John Alexander with a radioactive beam on a radioactive target. The species is ^{235}U. And since Dan and the rest of us also went through that earthquake let me add that only a half hour after the earthquake hit the beam was back out at the cyclotron. We're very fortunate.

G. Wozniak:

I would like to second Dan's comment. I think we need some statement, are people primarily interested in astrophysics and very low energies, are they interested in direct reaction studies with what I could call a radioactive beam with some sizeable intensity (which is probably 10^7 or greater particles per second), or are people primarily interested in characterizing exotic nuclei, where one can get by with particles per second or less. I think we need to know what people expect from a facility so then one could

take that into consideration when one tries to come up with a design that will allow that physics to happen. We haven't been discussing that.

L. Buchmann:

Last Monday, I had to talk about that a bit. I mean the whole game of measuring nuclear cross section with radioactive beams is an intensity game. You have to get intensity otherwise you can forget it. The demand for astrophysics is close to stability. You have to be in the 10^{10} range which is about a nanoamp, otherwise you won't be able to measure anything. You go to nuclear cross section above the barrier so then you go up in cross section, so you may get away with 10^5 or 10^6, but ^{11}Li not since you are interested far away, normally. So you have to aim for intensities. And if you can't get them or they are very marginal, better forget it.

M. Nitschke:

Well, we are still at point number 2 and we are rapidly approaching the coffee break. So are there other comments from other laboratories. I know that Rick Casten had planned to be here but apparently he is not. I talked to him briefly over the phone and they have had some thoughts of using the AGS booster perhaps. There is additional unused beam available there to do some studies, but I don't know any details so I can't comment any further. I'm not at all opposed if we don't stick to the agenda. If the feeling in general is that we should discuss more physics than by all means let us do this. I think there are only a few items that we have to get to, namely the last point: How do we proceed from here. We should not leave too little time to discuss this. Perhaps come up with some ideas about requirements. We don't have to pin down exactly energy ranges and so on, but if people think there are one or two or several parameters that are crucial, then they should say so. At the moment I have the feeling that for instance the intensity is a real issue. And I said in the beginning that this is a difficult issue because we are talking about a facility like I discussed earlier with beam intensities as high as 10^{11} per second but then falling off very rapidly perhaps -- sort of as a rule of thumb--an order of magnitude per neutron removed from stability. But we know it is not a linear effect at all, it is more like a parabolic distribution in mass. So there is a very large range and it is a difficult question to answer. How much beam intensity do we need for certain detector sensitivities to still extract a reasonable amount of physics? This is further

complicated by the problem that the detector development is progressing very rapidly. If you think for instance that in the past we have done gamma ray measurements with detector efficiencies of let's say 10^{-4} or 10^{-3}. Now we are talking about "Gammasphere" or "Euroball" where we have gamma ray detection efficiencies of perhaps 50 %. And certainly radioactive beam facilities of the future will have to rely very heavily on the development of efficient new detectors specifically for these weak beams and with low background. Also consider for instance that there is a beam dump that has an enormous amount of radiation accumulating, the Faraday cup of your experiment has to be much further away. Things like this will all have to all be considered.

So, if there are no more comments regarding point no. 3 then we can indeed go on to 4A where we intend to discuss physics questions.

W. Kutschera:

I think you last comment is very important but in a way it also sets the stage here for where one could actually build such a facility. Because if it relys on very sophisticated experimental equipment being developed right now and being put at certain facilities, it would mean that a radioactive beam addition should be done at one of those facilities which actually has all of this sophisticated experimental equipment available.

E. Roeckl:

I have the impression that those of us who have been at the workshop during the past three days, have got a good impression what the physics would be. But those who have not are sort of missing a compilation of what one would do in terms of Coulomb excitation, the reaction studies at low energies of astrophysics interest and so on. To me it's clear that you will start soon with these astrophysical studies at Louvain, for ^{13}N (p, γ) . And to me it's also clear that the astrophysical interests are best met by post accelerating ISOLDE beams. I just have a quick question to your Mike. If we think about this other scheme which was called "coming-from-above," that is deceleration, of high energy beams from fragmentation. You have the number of 0.02 efficiency including cooling in there? Why is that number so low? You said something about short half-life ..... decay during cooling and so on. To me this number is sort of low. I agree that the grand total in terms of beam intensity atoms per second is lower for this

"coming-from-above" option compared to acceleration of ISOLDE type beams. But why is the 2 percent efficiency given for cooling?

M. Nitschke:

This number is of course very much dependent on the specific case. That is, how much stored intensity you want, what kind of ion you are dealing with, how hot the ions are that needs to be cooled down, whether they need to be cooled down in a single step only to modest energies let's say around 100 MeV down from 500 MeV/u or whether one has to do a two step cooling process from a few hundred to maybe 50 and then down to 10 MeV/u or so, whether these two step processes can be done, whether it is ever possible to dynamically cool and decelerate at the same time ... all of these numbers get in there and it becomes very much dependent on the case. But one thing is clear that you have a duty factor problem because you have to load the cooler, the ESR, then you have to wait for the cooling down, then you use the beam with slow or fast extraction, depending, and then you load it up again and cool down again and so on. So depending on the speed of the cooling you loose a large amount in duty factor. So that is where part of the problem comes in and of course there is also a half-life dependent effect. For instance people are talking about ^{11}Li, the half-life of ^{11}Li is 9 milliseconds. You may have trouble cooling that fast.

G. Münzenberg (GSI):

I must say, I agree with your numbers but not how you got them. There are some principle limitation and this principle limitation is space charge because once you start cooling you can not cool a beam which is too high. That means you are limited automatically to 10^6 or to even lower intensities when the nuclear charge of the cooled species becomes high. On the other hand once you want to use stochastic cooling then you can't exceed 10^7 atoms in the ring because the Schottky pickups have a certain band width, and this limits the intensity. So your number is correct and will never be higher as far as we now know even if we could produce more in the fragmentation process. So this is out of the business in this case.

M. Nitschke:

Just to get it back in focus somewhat. I think there was a remark about the range of physics and the number of people that would be interested in such a facility. If I could

just briefly point out, some of the ranges and topics that could be addressed. I think I said earlier already that basically in this general area one would have the possibility of isotope production let's say for medical purposes. I know that this is a project that is going on at LAMPF at all times. One would have a low energy facility available as part of this entire complex: for collinear laser studies, for all the atomic physics studies that go on at ISOLDE for instance, and for nuclear orientation, mass measurements and so forth, off of exotic nuclei. Because we have to remember that what ISOLDE is doing now would be enhanced by several neutrons out further from stability and one could certainly consider discussing more exotic nuclei than can be obtained anywhere at the moment, except perhaps at heavy ion accelerators on the proton rich side. Then another possibility of course is to use the low energy part as was already explained for astrophysical purposes. And you know at GSI, for many years there was a very successful program at 1.5 MeV/u in what they called the "Stripper Hall." I think, there were 12 beam lines (it is now occupied by an accelerator) and that program was very productive not only in solid state physics but a lot of fundamental measurements were made in this "Stripper Hall." Besides astrophysics then one can consider solid state physics, implantation of radioactive beams, and so forth.

J. Vervier (Louvain):

There is one possible application of the radioactive nuclear beam which has not been dealt with neither at the conference nor in this workshop, it is condensed matter physics. There are some possibilities which are suggested in the CERN report proposing their facility and I wonder if anybody here could elaborate any more about that.

H. Haas:

Since our program in low energy radioactive beams at CERN has about 25 up to 30% of nuclear solid state physics of one sort or another, or applications including a small medical program, we have clearly thought about solid state application of accelerated radioactive beams. The people who have obtained the draft version of what we want to submit -- the proposal -- have certainly recognized that the amount of solid state physics that is in there is rather limited. I think one has to find for accelerated radioactive beams rather unique and specific applications but it will by far not be as broad as the keV beams use.

C. Davids:

This is a three hour workshop and I don't think we can hope to accomplish a complete survey of all the physics to be done with radioactive beams. In fact I would be quite pleased if the main accomplishment of this workshop were to get a user group organized and get people working on future plans. We have to have several days where people come who are interested -- and I think if we can get something, an organization, put in place at this meeting it will be considered to be a success.

D. Horen:

I agree with you one hundred percent. The only point I was trying to make is that once you start talking about your facility, than you ought to define what your physics is. Once you talk about the facility, if you are limiting it to that particular physics, well let's just summarize it in a very short thing, so that you go away and you know what you are designing your thing for. But I agree with you one hundred percent.

R. Firestone:

I would like to make a comment about beam intensities. People seem to think that you have to have the same beam intensities perhaps as we're used to in other experiments. But let me point out that for instance if you want to make a superheavy element you only have to make one of them in order to study it. If you want to make a nucleus far from stability if you can make a hundred of them you can measure a Q-value. If you want to study in-beam gamma ray spectroscopy to get more nuclear structure information, with a gamma sphere probably 10^5 particles per second or maybe even more (less?) is going to give you a pretty good experiment there; certainly almost equivalent if not even better perhaps with the modern facilities, compared to what we were used to 10 , 15 years ago. So I think there are an awful lot of areas in nuclear structure, and now we are talking about nuclei as there are 5000 or so nuclei that we can make that we haven't even looked at before with extremes in neutron number or even extremes in proton number that we'll never get to any other way. We're going to have to do it this way, one way or another. If you want to do your experiments you're used to doing now with radioactive beams, that's the next generation.

(Coffee Break)

M. Nitschke:

We need to finish this workshop on time at 12:30, so we have an hour and a half to wrap things up. Perhaps the most important point is that people feel we should talk about where to go from here, what kind of organizational structure we are thinking about to continue; and perhaps the most concrete thing at the moment is that we have a proposal from Los Alamos to hold a workshop in the spring. I would like to give people a chance to comment on this, what they think about the idea, and how they wish to contribute to organizing it, and what we need to make it a successful follow-up to this meeting. Are there some comments and some ideas on this?

W. Talbert (Los Alamos):

My own personal viewpoint of such a workshop: I think it should be steered toward addressing the question of what physics exists, if certain capabilities in radioactive nuclear beams were available somewhere. In our deliberations we found very quickly that the only things we really could address were those of the facility nature and not particularly of the scientific nature. It's my personal view that we really need to develop a compelling case for approaching the physics that would be new with such a facility. And so I would like to see this workshop assume such a character.

M. Nitschke:

Would you or John like to say a little bit more on how you plan to organize the workshop, in the sense that, should we have for instance an organizing committee consisting perhaps of a chairman from Los Alamos, who pulls together some people from the community, who gets people to think about the subjects they would like to discuss, physics questions, and invite perhaps people from other disciplines that have not been involved in our meeting here. The one thing that came to mind, for example, is that certainly with radioactive beams you could make interesting contributions to high spin physics, in my view. And I discussed it some time ago with Frank Stephens, but we haven't heard much about this during this meeting. And there are certainly other subjects we have missed. Paul Kienle sent me a letter saying don't forget stored radioactive beams. That was also not covered, in detail at least, at the conference that we just had. So we definitely have gaps and such a committee would have to worry about filling those gaps.

W. Talbert:

I don't know if I speak for the entire Los Alamos delegation on this but, we just sort of philosophically felt that a follow-up would be appropriate; but a lot of the appropriateness of that depends on the viewpoints expressed here, whether there is a strong interest in such a thing. I don't think we would be willing to pursue a workshop if nobody really were interested in it. It's not for our own sake. There is some possibility of good physics out there, we just can't put our finger on it yet. And we'd like to explore this, I think.

M. Nitschke:

You were asking for contributions from other laboratories to help you to organize this and to continue from here. I think that what we would be willing to do at LBL is to help in the formation of a users group, let's say. This is a very fancy word for essentially a mailing list, and perhaps a newsletter from time to time. We have a large mailing list of initially 800 people that were invited to contribute to this conference. That narrowed down to about 500 that responded and expressed interest. And then finally we had about 135 registrants for the conference itself. So we have some kind of graded mailing list available that we can use as a starting point. We can for instance send these out and ask people for feedback whether they want to be on the mailing list for a users community; and also you have those names for invitations to the workshop.

J. Symons (LBL):

I was going to say I think you would get a good response; so why don't you ask for a show of hands on who would support that.

(Show of Hands)

M. Nitschke:

Okay. This is of course an assembly of interested people: you have come from the USSR, and from Belgium, France, Canada, and Japan; it's a lot of effort.... (I'm not going to mention all 15 countries, just the extremes.) So you are definitely interested, and

it's easy to talk among ourselves; but of course we need to convince many more people, because such a facility -- as we have already seen from the details that have been discussed -- is going to be expensive. It's going to be a major national effort to get this going. To deal with the radiation problems, with the primary accelerator, etc. After all, we are talking about coupling two accelerators and there is a lot of technology there.

C. Davids:

In connection with the U.S. national long range plan, the exercise that we're going through now: We had a workshop in April of this year at Argonne at which time a lot of this material on radioactive nuclear beams was presented. There was a large group of people at that meeting who expressed interest in such a facility. So I think that there's a community in the United States and elsewhere too, but I think there is a community in the United States that is quite interested, including the high spin people. They were heavily involved with that and they have plenty of ideas on that. I don't think we going to be lacking in numbers, just because we don't have a lot of people here at this particular gathering.

W. Kutschera:

I would just like to add to Cary's comments, that if we are going to get those people he was talking about in a mixed meeting, the physics issues have to be addressed for sure because the users by definition are interested in the beam on target as it was expressed earlier here too. They don't care how it's being produced and we cannot avoid this discussion at the early stage in order to get those users interested in the subject.

M. Nitschke:

But one has to see of course that there is a sort of "chicken and egg" problem, which you don't have in an ordinary heavy ion facility nowadays. You can walk up to the SuperHILAC and say: I need a beam of ^{176}Yb. And, after a shift or two of tuning, they will deliver the ^{176}Yb to you, with variable intensity and variable energy. This will not be as simple for a radioactive beam facility. The user, in the very beginning, will be very much involved in the production of the beam. He has to be careful what element he chooses. He has to know what intensity to expect and what element to choose. For

instance whether he wants to trade intensity for neutrons or isospin. I give you a very simple example to make the point. Please excuse, it's trivial. Say, for instance you produce a beam with x neutrons from stability to make a compound nucleus reaction. There is a trade off: if you get a beam with (x-1) neutrons, a more neutron deficient beam; you get less intensity. But in a compound nucleus reaction you would have to evaporate one neutron less from your compound nucleus. So you have to optimize: is it worth going more neutron rich with more intensity or is it better to go more neutron deficient and lose intensity. If you want to make an isotope like ^{100}Sn for example, it is an important consideration whether you do a compound-nucleus-2n reaction or a 3n reaction. It may make the difference as to whether you can observe that nucleus or not. We have given some thought to things like this. I give you another example. Sometimes it doesn't make much difference whether in a nuclear physics experiment you go one Z higher or lower. You evaporate a proton -- in let's say a compound nucleus reaction again -- or you don't evaporate the proton, you do an xn or pxn. Sometimes the cross sections are similar. Or you have the choice between pxn and αxn. Sometimes these are quite similar. But in a radioactive beam facility whether you are one Z higher or lower can make an enormous difference. It may actually make the difference of getting a beam or not getting it at all. For example in the case of an alkali beam which may be two orders of magnitude higher in intensity than the next beam one Z up or down from the alkali beam. That's just an example. So lithium, sodium, or potassium may be the initial beams in the facility that will have very high intensities, because of the high ionization efficiencies. All that has to go into the thinking of the experimentalist who plans the experiment. So I feel one has to have this combination between physics and the way these beams are produced, to arrive at a realistic planning process.

J. McClelland:

I agree with what you say. When we were thinking about this workshop, we considered one day of physics discussion and one day of some of the technology that would be involved. If it is to be held in Los Alamos, I think one of the things I personally would like to hear is: that we have had three days of physics discussion now, and it seems that people are not quite satisfied in the focus that has been derived from that . So I would really like to hear what changes such an agenda might entail. Whether formal presentations may not be the most appropriate format and it's more of a general discussion and brainstorming on some ideas. I think that would be an important thing to determine because no matter who holds this workshop, I think these are the people that

have just been through something like this and would like to make some suggestions on what the next step is.

M. Nitschke:

Yes, I think it would be very productive to get feedback from the audience while we're here.

D. Horen:

I think, my guess is that the way you're going to proceed in the future is going to depend upon three things. Number one is: are you going to try, is this going to have to go through NSAC? And if it is I would guess that you would have to have a pretty good solid physics justification. The second is whether you can do this without NSAC then I guess you can go any way you want to. And the third is can you carry on development work without NSAC. Can you just get money to do the development work? And so there seems to be these three roads down the future. If you're going to have to go through NSAC, then I think you are going to have to get some pretty solid physics justification to push it.

J. Symons:

Just to comment on that. You are going to have to go through somebody who is going to pay for it. DOE is going to want good scientific justification whether or not it goes to NSAC. And they do their own reviews in various different ways, and I think that question of NSAC is a question of cost. And if it's a project of $10 million or more it's certainly going to go to NSAC. If it's something less than that you can maybe do things in a more informal way with the agencies. So I think that NSAC is certainly going to be following with interest whatever happens but formal NSAC reviews will be, I think, determined by the price of the project. And in general R&D can be negotiated with the agency but again it's a question of what the final cost is going to be. If they see it's R&D toward a very large facility they will want to get NSAC involved very early on to review it.

L. Buchmann:

First, let me say something about cost. It's more than $10 million. I mean, even our simple facility was in the sort of $15 million range only going to 1.5 MeV/u, and if you really build a full scale accelerator going to 5 or 7 MeV/u, and want to have 100 of microamps through the primary target then you are probably talking at least $30 million without any primary accelerator.

G. Wozniak:

I would just like to comment. I think one wants to have workshops but I think we've heard some comments from different communities here people who are interested in astrophysics, and mention of high spin physics, and I think what one really needs to ask of these different people in exotic nuclei studies is: what kind of beams, and specifications on the machine do you need to pursue that kind of physics. Then one can feed this to the machine designers, to say: can you produce a machine that's going to deliver these kind of things. I mean, you need a feedback process. You're not maybe going to be able to handle all of them but right now we're still talking very vaguely. A radioactive beam is a radioactive beam which some people say is 10^2 particles per second and someone down here said 10^{10} for astrophysics. We have to be a little more specific so that one can proceed on considering design concepts.

M. Nitschke:

I would like to bring up a question that I mentioned earlier responding to John McClelland. In my feeling there has to be a close collaboration and feedback between the people that will produce the beams and the people that want to use them. Is that thought in general to be a necessary condition at this point in time? Because if you look historically for example at the development of the cyclotron let's say at this laboratory by Lawrence. The people that developed the cyclotron were also the people that did the experiments. Al Ghiorso is here who can definitely testify to this, not only regarding the cyclotron but then also the Alvarez accelerators: for example the SuperHILAC type machines, they were developed by physicists who then did the first experiments with it; and that actually was the secret to the success of many of these schemes. The people that developed the machines knew what kinds of experiments they wanted to do. Nowadays, things have become so specialized, that somebody who, say, is an RFQ specialist

probably is not simultaneously a high spin physicist or vice versa. But this being a fact of life, and having such a variety of accelerator concepts available fortunately, means also that we have to keep that connection going. The post acceleration of these very low beta beams is going to be a challenge. We have to work with the accelerator physicists to obtain high transmissions. We have to discuss very carefully where, when and whether to do the stripping. To give you another example, thinking about the stripping process: let's say you have a lithium beam, since lithium has been a very popular subject, lithium is actually a very favorable radioactive ion. Because it is so light it comes already in a charged state that gives you a charge to mass ratio sufficiently large for the post acceleration. So lithium is perhaps the one element that you need to strip only once or maybe not at all during the acceleration process. But as you go higher in Z you will have to strip and at some point again the experimenter will have to decide: do I go to a beam that needs to be stripped and therefore will give me a factor of 3 to 4 less intensity, or maybe even a factor 10 if you strip at very low energies like 100 keV/u, or do I do an experiment rather with a low Z beam that needs not to be stripped through the entire facility? All of these things have to be understood by the experimenters, so they have to be involved with the accelerator concept and the design of the machine from the beginning.

J. Vervier:

I agree with what you say and I can just witness that our experience in Louvain-la-Neuve is that the same team which is working on the production of the beam is also working on the first experiment. This is an illustration in practice of what you say.

L. Buchmann:

My experience is about to get something going is that you don't diffuse it over a wide variety of people but get sort of 5 to 10 people together travelling the world and producing a 20-50 page paper which is sent out to other people where they can chew on.

M. Nitschke:

That was just one point I wanted to take up. Another one would be, whether there are specific test sites for the developments of components, for example are there laboratories that can contribute uniquely to the testing of certain parts of the total structure. I have mentioned before on occasion that we have an offer from the Canadians,

from TRIUMF, since they have the TISOL facility. They have an ECR source, as you heard during the meeting, that is running now. We have an invitation from them that they would like to collaborate with us. The future of their facility is closely linked to the decision on KAON. It is not quite clear even what the sign is, if KAON goes whether they also go with it because their facility will be in the noise of KAON, as I have heard mentioned, or whether it goes the other way that if KAON does get built there will not be enough money to do the astrophysics facility. This is all not clear. John D'Auria and others have been very active in the United States coming to workshops, for instance to the NSAC meeting at Argonne and have contributed quite a bit to this field already with planning workshops for astrophysics in the past at Mt. Gabriel and so on. Los Alamos we have heard will definitely be willing and able to provide test facilities for intermediate intensity or low intensity proton beams for testing sources and targets and radiation hardened devices that one may need for such a facility. Are there others? And of course there is Argonne National Laboratory having a superconducting linac and the associated technology. So those are just some items that come to mind automatically. It is obvious that those would be natural contributions to the RNB facility. Are there other things that we may want to talk about and that people want to contribute? Are there apart from what is on the agenda and what we have planned here so far--are there other subjects that people would like to take up and discuss here. It was difficult to put this together because we didn't know everybody's feelings on the subject.

G. Münzenberg:

I just maybe want to pick up a topic from Kienle. I'm not quite sure whether you really thought about the possibility of using such a storage ring for low energy ions. I don't know how they compete with the traps for laser physics or whether you could just use it as a storage ring to increase the signal to noise ratio, just as an accumulator. I don't know whether one should really think about this also, for instance many of our experiments at GSI will occur at 10 MeV per nucleon. I don't know whether somebody thought about scattering or charge exchange in internal target at these energies and so on.

D. Vieira:

In some private discussions it was mentioned that the European community has kicked this around quite a bit, about going to a much higher intensity facility than ISOLDE and others that are planned. I was wondering if someone could maybe just

briefly comment on how that is proceeding and it could certainly stimulate us here in the United States.

H. Haas:

I think I can summarize the discussions that have been going on and are still going on in Europe. It is absolutely clear that for a radioactive beam facility for nuclear energies, that is 5 MeV per nucleon and above or maybe 3, there you would also want a high intensity injector. The question is then only, is that the next step or perhaps is the next step using the presently existing facilities and do some first stage in radioactive post acceleration? The answer to this has not fully come up yet, when we will decide to write proposals, but it is clear that the answer has to come up in the next few months in Europe because of time conditions at CERN and at Rutherford Lab.

Q:

Do you have some workshop planned or conjectured about this?

A:

I don't think workshops are actually the proper thing to discuss such political issues.

M. Nitschke:

Are there some comments regarding the organizational structure. How should we proceed forming a users group? To put in very concretely then: At the point right now Los Alamos is planning a workshop in March. John, do you want a show of hands of people who are personally interested, or know of somebody in their immediate group who would be interested in this. (Show of hands.)Okay, about 23 people. Now those who would not attend personally but know of people that they represent, or that are in their group or laboratory that would come to such a workshop. One from Japan. Isao, you would have people actually coming from Japan to such a workshop or yourself? Okay, a total of three. That's not a bad start. We are a very small group. Because of the earthquake problems we are probably only about half the number of those who originally signed up.

W. Myers, (LBL):

I feel that I'm sort of on the fringe of these considerations since I'm not a accelerator designer nor an experimentalist but I really want to encourage the Los Alamos people to rethink this workshop of theirs. Two days of physics would be a great idea and a couple hours of accelerator considerations would be fine as far as I'm concerned. I mean , I don't know whether you guys have got the message from this group of not, but to me, it seems like a lot of the machine and institutional considerations are pretty well in hand or people understand them and what needs to be done. The physics is the soft spot and it's really important to my mind to look at experiments that are currently being done in a marginal way that would benefit from these developments. That's maybe the first place to start. And developments beyond that. I mean I'm really interested in some of the developments in astrophysics and at the neutron drip line and I think we are a long ways away from focusing on those and getting the people involved that should be involved. To me there's more energy going in places where we already know the answers than there ought to be.

M. Nitschke:

This will be a difficult task for the organizers to balance this. The necessary information to the experimenters and perhaps even to the theorists., on one hand, and not to over-emphasize the technology; and on the other hand to emphasize the physics that can realistically be done. We are willing to help you with this and I hope that other laboratories like Brookhaven, Oak Ridge and Argonne will contribute. I think we all have to support the next organizers, whether it is the organizers of this workshop or the next radioactive or exotic beam conference in the future, to balance the program and to delineate it from other conferences and workshops that are going on, like: Nuclei far from Stability, AMCO, and EMIS. We had a meeting last night with the International Advisory Committee, the session chairmen, and the local organizing committee discussing the long-range future and what kind of meetings one may want to have. There is a proposal from the Louvain-la-Neuve group to host another conference of this sort around 1991. We have to see how this fits in with all kinds of other constraints. There is the EMIS-12 conference, as we have heard from Dr. Fujioka in September in Sendai. So there are possibilities of combining efforts, but also possibilities of emphasizing the technology in one meeting and concentrating on the physics in another meeting. All these things have to be worked out in the future dynamically. But at the moment then we have

perhaps arrived at some kind of feeling that a workshop would be most welcome at Los Alamos; put together by a local organizing committee with some advice from the community.

J. McClelland:

Dave and I were just quickly discussing how we might accomplish this, fitting in an agenda for two or perhaps three days. Perhaps one way we could proceed is that people that are interested in participating in the organization of that meeting either sign up or meet briefly after this meeting so that we can, from an interested group of people, have those people do the organization. There will be come local representation obviously to handle the logistics of a meeting at Los Alamos. But what I think I'm hearing is that there are there some very strong feelings as to what the content of that meeting needs to be, and we would like to involve all those interested people from the very start. It would be very nice if there was already a steering committee for this users groups in place, but that would have to happen soon, to meet a meeting deadline by the end of March. So I would suggest then that perhaps after this meeting interested people would meet very briefly down at the podium and we'll try to take some names and and contact people in the very near future. Let me explain that date. It certainly isn't absolutely necessary. There are some very nice facilities though at the main Lab which are available for this meeting that would allow both parallel and plenary sessions to take place, and they're very heavily subscribed to. So between now and the end of March there are really only four days that they were available. We can hold it at LAMPF in the auditorium and we have some conference rooms at basically any date. But if people like the accommodations of nice meeting facilities, the end of March would be the most suitable from our point of view.

L. Buchmann:

I think it would be good to have a steering committee or whatever, not because having done some real work but at least produce some sort of paper, have a very good concept about physics and technics in place.

J. McClelland:

You see, I view the workshops as part of that process.

L. Buchmann:

But if there's nothing produced at this state you essentially get a repeat of this conference.

C. Davids:

Our experience with that workshop in April was that you have to have a document in place. You can't write a document by committee. The way we did it there was, we requested a one or two page document from everyone by Bitnet. And from this Rick Casten sat down and digested it and produced a draft document which in two days, the 60 or 70 people were able to go through and produce a coherent consensus. If you do not have a draft document or some framework at least to work from it's extremely difficult, as the the town meetings showed. A lot of work had to be done on the outside and it's less satisfactory because the people who put the input were not always around to put their final stamp on it.

L. Buchmann:

You need something to fight about.

D. Vieira:

Just to expand a little on Cary's idea. I thought that did work very well. People came forward with their one page summary of what they were interested in in the future, and I think, we could do something similar to that here, request people to summarize their interests, their views on the science that they would like to address, and to a lesser extent, perhaps, the way they would like to do it. That could be solicited and provide a very good discussion start. I would like to know if this would be something that people here would like to contribute to or not?

M. Nitschke:

Are you specially referring to let's say a Bitnet contribution of a page or so , the way Cary did it?

D. Vieira:

For example, or written.

M. Nitschke:

I think that worked out well.

E. Roeckl:

If you considered a physics program. I am referring to Bill Myers comment, interest in drip line nuclei. There one's point which I think wasn't really considered during the past three days, and this is nuclear structure far from stability. This is partly systematic studies like masses and moments which I think are important to improve our knowledge of nuclear structure. And the second point is approaching new shell closures and searching for new decay modes. And I think a radioactive beam facility with post accelerated beams say, this way number one, "coming from below," that may give us new tools to approach these goals. But just the target and ion source development with increased beam intensities far from stability can already yield new tools approaching this game. I think you should not forget about these sort of experiments.

M. Nitschke:

Could you explain that a little more. What do you mean that those ion source target combinations will give us already new tools?

E. Roeckl:

Well if you consider the shell closure at ^{100}Sn which we have not reached yet. We are sort of one to two nucleons away from it. If we improve on target ion source systems say, higher intensities, faster release, that is higher production rates. We get just one or two nucleons further away from stability That may sound like a small step but I think in this particular case, I would personally consider it to be very important.

M. Nitschke:

Yes, that's a good example.

D. Clark, (LBL):

I was thinking that in order to help to focus on the physics that would be available, it would be useful to put together a short summary of the whole range of A and Z nuclei and their intensities that might be available from some typical scenarios that we might imagine, like a beam at 600 MeV and 100 microamps, using the data included in the CERN user's handbook for example. But some kind of a short summary that we could give to other people in astrophysics, as well as nuclear physics and solid state physics. So that people know what is available, and then they can think of what kinds of experiments they might want to do.

R. Firestone:

And conversely to what Dave Clark just said, I think it would be very useful to have some experimentalists put in actual proposals for experiments they might want to do. With some estimate of what beam intensities they would need, and the conditions they would need to work under, so that would drive the experimental design as well. I think maybe having an idea of what beams are available is one side of it and the other side is what do you really need, and perhaps both of those could be collected simultaneously.

M. Nitschke:

You may remember that we put forward an upgrade proposal for the Bevalac. And in this proposal, we took the approach of working out some very distinct examples for each one of the subjects we wanted to address: equation of state, exotic nuclei, whatever it was. We had one page where the entire experiment was worked out: the physics goal to be achieved, the beam intensities to be obtained, and the countrates in the detectors, going through the efficiencies; and coming out with the number of counts per second. I think we need some of these specific examples to make it clear why radioactive beams are useful, and why it is the only way to reach certain goals like ^{100}Sn, or whatever the example may be. Also to give people that are outside the field, and that would be competing with us for the funds and the attention of all kinds of agencies, to give them examples that they can understand and work with and that can be discussed. I think that is a very important; that is the kind of homework we will have to do.

H. Haas:

I would have one very specific wish for R&D. One of the decisions one has to make if one builds an accelerator -- if it is not the superconducting one -- is whether one has or can accept pulsed beams or must accept CW beams, continuous beams. And there are possibilities nowadays of trapping beams, storing ion beams. This is a completely independent development that could go on, and it's clear that there are ideas in this field but they haven't been developed and in our group we clearly don't have the power for that. It's a big and important step in deciding which accelerator you build, whether you have to accept the continuous one-plus beams or whether you can bunch before that, and on which time scale you can bunch before that? Can you bunch in one millisecond or 10 milliseconds or a tenth of a second?

M. Nitschke:

That gets us somewhat into a subject that is on the agenda, namely some of the major parameters that one may want from such a facility. I'm not quite sure that people are ready to discuss this, i.e. the basic idea of having a complementary arrangement to the one where the projectile fragmentation people are able to study beams down from let's say 500 MeV/u. I don't know exactly what the dynamic range of the ESR is. I always thought that maybe a factor of 100 is a little bit much, that is you would have trouble getting perhaps down to 5 MeV/u. The cooling dynamic range is what I was always a little uncertain about. Let's say if the projectile fragmentation process together with the cooler/decelerator could get down to ≥10 MeV/u then such a facility would be logically joining this energy range, "coming from below" I would therefore like to ask: Is it in generally acceptable to plan this for energies around the Coulomb barrier? That is, have energies around 10MeV/u. Would that be something that is highly objectionable, i.e. would people right away want 30, 40, or 50 MeV/u. I know that Gordon Wozniak is here who has studied this intermediate energy range and would probably want higher energies. I can on the other hand answer the question myself, saying that, of course, whenever you have a beam of 10 MeV/u nothing prevents you from adding a storage ring or a post-accelerator. We did it here at LBL from 8.5 MeV/u to 1 GeV/u several years ago. This is always possible, and I think it therefore makes sense to start around beam energies near the Coulomb barrier. I feel that 6.5 MeV/u which the Japanese have chosen for the Large Hadron Project is perhaps too low, because you often have to go through windows, or you would like to study some higher excitation energies, or deep inelastic reactions. In those

cases 6.5 MeV/u may be somewhat marginal, 10 MeV/u seems like a more reasonable energy to shoot for.

G. Wozniak:

I think one should not finalize design parameters like that now, that would keep people from coming to your workshop. You want to make it as broad based as possible, and argue if there is enough physics in energies above the Coulomb barrier to justify additional costs -- if it does cost more -- and not try to preclude that from the beginning.

M. Nitschke:

What I meant to say is, that experiments that require energies above 10 MeV/u, if they are interesting enough, they will probably be done at GSI before we can hope to have a facility here at in the United States. Conversely, astrophysics and related Coulomb barrier experiments may benefit from the higher intensities of an ISOL based facility. I see it not as an either-or, proposition but as a complementary proposition. We also shouldn't forget that we have MSU which will have these intermediate energies available. They use the projectile fragmentation process and have a separator to go with it, and that certainly will be available for the intermediate energies. What I am trying to get a feel for is: Do people have any strong feeling about any of the properties of such a facility? What is the opinion about using the low energy part of the facility for doing ISOLDE type physics? Would there be interest in making use of the very high intense satellite beams that are available "free of charge," so to speak? It was just pointed out by Ernst that a factor of 100 in intensity may only mean that you can get two neutrons richer or two neutrons more deficient but in a critical case like ^{100}Sn that's all it may take to get there. On the other hand of course, the intensity increase of 100 fold for a given isotope may shorten your experiment by a corresponding factor. One has to see this very clearly: the gain in neutrons is often not as spectacular as the savings in beam time.

H. Ravn:

I think that there is one more aspect of the high intensity which, I think, you have forgotten to mention. Today about 50% of the elements are either not available at ISOLDE or they are available in so low intensity that they are below threshhold for making a number of experiments. By increasing the intensity by one to two orders of magnitude you would not only have <u>one</u> isotope, you would have a whole range of

elements coming above detection level and starting new physics of a large range of elements.

M. Nitschke:

If there are no other comments or topics to discuss, we can conclude the workshop. To summarize, I think we have a fairly good understanding about the Los Alamos workshop in the spring. We finished a little early, which leaves people that are interested time to come to the podium and sign up for help to organize the workshop. We also have volunteered here at LBL to put together a mailing list to start a users group and perhaps to put out a one page newsletter or Bitnet message from time to time for this user's group, and we'll see how it develops from there. We have had some feedback from the community that it is very important to discuss more physics, that probably these three days were not enough, that we have missed certain subjects that were not represented but that are never-the-less important. We have also seen that considering the difficulties of connecting two or three accelerators together will require a lot of technological development, and it will require a lot of money. And money always means a lot of fights and lots of competition from other people, and we will have to work on that aspect simultaneously with the physics and with the technology. I thank you all for attending the workshop and wish you a safe trip home.