Beam Quality for Nuclear Scattering and Reaction Studies

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I want to direct my attention to one way of keeping one group of physicists happy with an accelerator for the specific purpose of doing nuclear scattering and reaction experiments, and to describe what kind of particle beams this particular group of people has been using for about the past two and one-half years, just to give us some judgment for this particular small area of research that one can do with cyclotrons.

For orientation, let me remind you that at the University of Washington we have a 60-inch fixed-frequency machine. The beam is brought out of the machine and passes through an external focusing system indicated schematically in Figure 211. The beam passes first through a focusing magnet, a large shielding wall (water), and through an analyzing magnet into either a small scattering chamber or a large scattering chamber. The latter will be ready for use in about a month. Our beam system will be described in more detail this afternoon.

For several years work has gone on in the small scattering chamber, and it is pertinent to inquire how happy the numerous graduate students, staff members, and visitors have been who have used it. The first question one asks is, what kind of beam does the experimenter want? What are its characteristics? One can write down about eight quality factors for a beam. I will discuss only a few of them. The first specific question one asks is, how much beam is wanted? The numbers are rather surprising. They range from 0.1 mμa -- remember this is the beam size desired from the machine to make the experiment most feasible in terms of counting rate and rapidity of gathering the data, consistent with the accuracy desired, the background considerations, and so on -- up to a maximum of 1 microampere. So we see that this is a range of 10^4 in beam magnitude.

Well, one might then ask these same experimenters whether they have experienced an instance in which they would have liked to have had more beam. The answers were generally quite negative, although in one or two isolated cases they pointed out they could have done the experiments somewhat faster with larger beams. But then there are worries about accidental coincidences which cast doubt on the desirability of larger beams.

Then we tried to ask, how much more beam could we use? Can we conceive of experiments that would be feasible if we had more beam? Within the immediate imaginations of the group there were one or two proposals for as much as 5 microamperes. In one case, a sort of radiochemical kind of scattering experiment, a proposal was made for utilizing up to 100 microamperes.

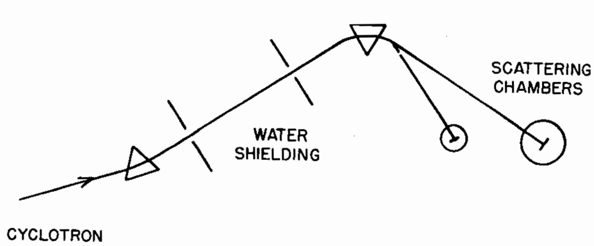

Fig. 211. Schematic diagram of external beam system, University of Washington cyclotron.

Next, one might ask about the size of beams that people use on targets, and one learns the sizes range in diameter from 1/32 in. up to about 1/4 inch. This depends, of course, on the size of the scattering chamber and on the angular resolution desired.

The next question, what is tolerable in terms of angular divergence of the beam at the target? We looked to see how rapidly some cross sections changed, and, for example, a case was cited in which the change in cross section with angle is a factor of 10 in 5 degrees. That immediately shows that one must have a very small pencil of beam. Again, on looking at $dE/d\theta$ one finds cited the case for C^{12} of 260 kv per degree. So one needs angular divergences in general well below 1^o, and for the most part our people work with $1/4$ to $1/2^o$ angular spreads. For some of the planned future work in the new scattering chamber smaller angular resolutions will be used.

I will skip over questions of beam purity and long-term stability. On the other hand, the type of experiment you can conduct in a scattering chamber will depend on the general room background, the particular counting arrangements, and so on. Everyone at Washington is in general agreement that one would like a beam which passes gently through the entire system without touching anything except the target in question, and then goes an infinite distance away without hitting anything else. This is clearly not achievable.

Now we come to another consideration, beam time distribution. We can divide the beam time distribution into two general categories, short and long. I shall talk only about short time distribution, and here again I think we can divide the short time distribution into three classes: short short, medium short, and long short. By the short-short time distribution I mean that due to the r-f pulsing. How long are these pulses? And what would experimenters like them to be? Until very recently we have not worried very much about the length of the r-f pulses; nor have we made any effort to utilize the beam bunching as a means of selecting desired coincident events.

Now with our larger scattering chamber nearly ready for use we are beginning to think about utilizing this very feature of cyclotrons to do time-of-flight experiments. Time-of-flight can also be utilized to select the desired events from the background events. For example, an alpha particle, say of 40 Mev, moves at about a fifth the velocity of light and, therefore, if one has large enough trajectories in the scattering chamber, one can arrange the counters such that the desired alpha particle registers long before any undesirable scattered radiation comes from other scattered alpha particles which strike the outside of the scattering chamber. In fact, it was in part for these reasons that we chose to build the large scattering chamber which I will describe briefly in a later session.

We have made a rough measurement of the r-f beam pulses, and we find them to be about 4 mμs long. That is, this leaves about 85 mμs dead time which can be utilized in the manner just described. We do not know much about the time distribution of these pulses; that is, whether some of them are large and some are small. We do know, however, that our beam is severely modulated.

We have only begun to worry about other kinds of beam variations. We know, for example, that the beam modulation patterns at Oak Ridge shown by Jones are also present in our machine. Recently, we have come to believe that the other kinds of variations, the medium-short term and the long-short term, are due principally to the 5% voltage ripple in our oscillator power supply.

We have looked at the modulation of the beam as a whole, and we find that it has a 360-cycle ripple, as expected. Next, we placed three probes in the beam "front" as it emerges from the deflector channel. We find that there are phase differences between the 360-cycle modulation which appears on each of the three probes, and therefore, we believe that there is a correlation between the angle at which the beam comes out and the r-f dee voltage. This is not too surprising for the reason that with rising dee voltage the total number of ion turns in the cyclotron becomes fewer and fewer. Thus, with a decreased number of turns, the phase at which the ion enters the deflector channel shifts. Since we have an r-f deflector that means that the deflecting voltage is a function of the r-f voltage, and hence a function of the 360-cycle ripple voltage. So we believe that our beam would not "spray" as badly as it now does if we were to regulate the dee voltage.

In a similar manner one can make an argument that the axial spread of the beam will also be influenced by the 360-cycle ripple; as the ions pass through the $n = 0.2$ value in our magnetic field (which occurs just before the exit radius) the "dwell time" in that region is a time when energy can transfer from radial to axial oscillations. This time depends upon the dee voltage. Hence, the magnitude of the dee voltage will influence the axial beam spread. So, we believe once again we will benefit if we eliminate the 360-cycle ripple.

Now I would also like to note that we see, in addition to the 360-cycle ripple, a beam intensity fine structure that seems to be associated with the magnitude of the dee voltage. We would like to attribute this fine structure to the slowly expanding and contracting orbits, so that we are seeing individual turns entering the exit channel as the dee voltage varies. The result is a modulation of about 10,000 cycles per second. Again, it appears that stability would be improved considerably by very closely regulating our high voltage. In summary, both the slow (360 cps) and the medium ($\sim$10,000 cps) beam modulation has a common basic cause.

Let me just reemphasize the desirability of having every r-f pulse the same size. In coincidence experiments this becomes very important. If you can rely on this fact, then you can make accidental coincidence corrections from a single measurement. But if you make any changes in your machine between the accidental coincidence determination and the genuine experimental measurement itself and hope to make a reliable correction you are really in a bad situation unless you know that the machine puts out uniform r-f pulses.

The reason for the uncertainty is you can achieve the same total beam by a variety of ways, as we already know. If you cannot rely on this kind of beam stability, then it means your accidental coincidence rate during the time your experiment is in progress is an unknown, since the accidental coincidence rate increases as the square of the size of each of these pulses. Therefore, one should monitor the detailed r-f pulses extremely carefully.

EISBERG: At Minnesota we have a linear accelerator with which we do experiments in the energy range the cyclotron will be working. In a certain very interesting class of experiments, one can use this fine structure to advantage. But, at least for the experiments that we have been doing, which I think are typical of those that will be done with counters, one would like really to smear this out. It would really be to one's advantage if one could put some serious thought into what could be done to destroy the phase bunching to increase the duty cycle. This was certainly brought out by Schmidt in his remarks.

Of course, the reason these people run with such very low beams is because their duty cycle is limited. This happens in all sorts of experiments, even ones that are not obviously coincidence experiments. Upon detailed analysis most of these things always turn out to be duty-cycle limited.

SCHMIDT: I disagree with you in part and I agree with you in large measure. My disagreement comes when you consider the time constant with which you can do counting.

EISBERG: Five millimicroseconds is easily obtainable now.

SCHMIDT: It depends upon what you are doing. If you wish to measure the pulse height accurately you can certainly take the leading pulse fast enough, but it is the decay time that you must worry about, and you must have a relatively long-time collection constant and decay time, to make certain that you are counting your pulse height. In addition, what one would really like to have is a machine that you could adjust the r-f pulse length to any desired value.

HAVENS: The problem is different from your duty-cycle because I know of no counting system that can count two pulses in 5 mμs unless you have some intermediate storage; so that here you have a high repetition rate with the r-f pulses, whereas your machine, I think, is a 60-cycle repetition rate.

EISBERG: We have a duty-cycle that is on for 200 microseconds. I am not talking about that. I am talking about this kind of duty-cycle and I am making possibly a slight extrapolation to what electronics will be three years hence when these machines are available. I think you have to make that extrapolation. You cannot look at what kinds of experiments you are doing right now and have been doing; you have to look at what the electronic techniques will allow them. It is quite clear that there is no fundamental limitation to counting and doing everything you can do now, only on a millimicrosecond time range. When you do that you will find that you will be seeing this r-f fine structure, and in many circumstances it will be very desirable to smear out the bunching to increase your duty-cycle.