CONCLUDING REMARKS

A Short Anecdotal History of the Cyclotron

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Introduction

Since it is now almost exactly 50 years since my first association with a cyclotron and 25 years since the first real cyclotron conference at Sea Island, I thought it might not be taken amiss if I recorded some thoughts and anecdotal remarks on this history. The first 25 years is less well known to the present members of the Conference, so I will tend to place an emphasis on that period. I would also like to record some guesses as to what the future might hold.

Obviously, this is a personal view of the history of the cyclotron and of our conferences, and I make no claim to an unreasonable amount of objectivity.

The Early Period 1930-1945

The early development of the cyclotron was described in a series of papers by E.O. Lawrence and his students[1] (Lawrence and Edlefson, 1930) (Lawrence and Livingston, 1931), but the definitive paper describing what might be considered to be a "proof of principle" was published in 1932 by Lawrence and Livingston.[2] A cyclotron of 11-inch diameter had been constructed and with a dee voltage of 4 kV had accelerated protons to 1.2 MeV. This was an extensive paper and it states clearly the requirement of a magnetic field decreasing with radius in order to maintain axial focusing of the ions. A drawing shows the focusing force due to the curvature of the magnetic field lines necessarily accompanying the radial fall-off of the axial field. However, near the end of the article, the following statement refers to the future:

"In the higher range of speeds" (up to 25 MeV protons are mentioned) "the variation of mass with velocity begins to be appreciable, but presents no difficulty as it can be allowed for by suitable alteration of the magnetic field (in the same empirical manner as is done to correct its otherwise lack of uniformity)". Of course, the cyclotron resonance frequency $\omega = qB/mc$ is given and so the two contradictory requirements on the magnetic field are clearly stated in separate sections of the 1932 article but no conclusions relating the two are drawn. In 1934 as a second year graduate student at Berkeley I took an excellent course from Lawrence in classical electricity and magnetism, including a thorough discussion of Maxwell's equations. I feel sure that Lawrence understood there was a problem here, in the relativistic mass increase, but believed that somehow it could be solved, and in the meantime the thing to do was to build bigger cyclotrons with larger RF voltage on the dees. However, I shall call this type an NR cyclotron (non-relativistic).

It is interesting to note that the paper of Harold Urey and collaborators on the identification and separation of the heavy isotope of hydrogen is in the same issue of the Physical Review as the "proof of principle" cyclotron paper. And so, although the first nuclear physics results reported from the cyclotron[3] were an excitation function of the reaction $^{7}Li(p,2\alpha)$, attention soon turned to ^{2}H (the "deuton") as a bombarding particle. Some initial mistakes were made due to lack of experience in the field, but the "deuton" turned out to be a prolific producer both of neutrons and of artificial radioactivity. One of my first recollections of Lawrence (1934) concerns his talking to an impromptu group on the eve of his departure to attend an important physics conference in Europe. I was impressed by this young professor from Berkeley (Where?) going to the sources of knowledge and scientific power in Europe to describe the work of the Radiation Laboratory. One of the points he mentioned was the resolution of the name for the ^{2}H nucleus. Berkeley had been calling it the "deuton" while the Cavendish Laboratory under Ernest Rutherford (really the father of nuclear physics) had been calling it the "diplon". In amusing vein Lawrence told us on his return that a compromise had been reached with the incorporation of Rutherford's initials - hence the deuteron.

Some idea of the interest and excitement in the cyclotron nuclear physics field can be gained from a list of the cyclotrons in operation 1937, five years after the publication of the proof of principle:

NR Cyclotrons in Operation - 1937

Berkeley	5.5 MeV d, 11 MeV α
Cornell	2 MeV p
Michigan (Ann Arbor)	6.5 MeV d
Princeton	9 MeV α
Rochester	3.8 MeV p
Swarthmore (Bartol Foundation)	
Urbana	1 MeV d

Cyclotrons were under construction at Paris, Copenhagen, Cambridge (UK), Columbia and Chicago - others were in the planning stage.

Technological Change in Five Years

	1934	1939
Magnetic field	Whatever could be afforded or scrounged	Better understanding of focusing requirements and tolerances
RF accelerating field	tuning coil home-made triodes	resonant lines home-made triodes
Source of ions	bare filament	hooded arc source
Vacuum	sealing wax glyptal (!) oil pumps	rubber gaskets "Wilson" seals fractionating oil pumps
Controls	rheostats and meters	some electronic control particularly for magnetic field

I was personally confronted in a dramatic fashion by the relativity problem on a cold Thanksgiving afternoon in 1937 in Ann Arbor, Michigan. As a rather lonely National Research Fellow of two months, I was enjoying an excellent dinner at the home of one of the senior professors in the Physics Department, and we were just starting dessert when the phone rang. It was a member of the cyclotron group, asking me to come over immediately to talk to Professor Hans Bethe about a message of great importance. Greatly embarrassed, I apologized to my gracious host and hostess and hurried over. In essence, Bethe[4] told us that because of the conflict

*also, Professor of Physics, Emeritus, UCLA between the axial focusing requirement and the relativistic mass increase, no cyclotron could ever accelerate particles above 8 MeV! Needless to say, we were concerned but felt fortunate that our improvement program envisaged a top energy of only 7.5 MeV. However, 39 years later I got my revenge for the loss of a peaceful Thanksgiving afternoon. As Director of TRIUMF I persuaded Hans Bethe to give the keynote speech at the scientific dedication of our 520 MeV cyclotron. Of course, I took pleasure in reminding him of his 8 MeV upper limit!

The relativity problem was explored in detail in two papers by Rose[5] and Wilson[6] in vol. 53 (1938) of the Physical Review, but in practice the only alleviation of the problem that they proposed made use of very large dee voltages to reduce the total number of turns. In fact, the first step in the solution of the relativity problem was taken by the well-known theorist L.H. Thomas in two papers[7] in the following volume 54 (1938), but these papers provoked very little discussion. Two years later R.R. Wilson[8] wrote an excellent 40-page review article on the "Theory of the Cyclotron" but the sole mention of the Thomas solution is the following: "L.H. Thomas has shown that variation of B with polar angle can produce focusing and also preserve resonance and stability." Later we will explore reasons for the neglect of the Thomas papers.

The FM Cyclotron 1945-1950

The solution to the relativity problem in the cyclotron which first came into actual use was that of modulating the accelerating frequency in accordance with the mass change in the relation $\omega = qB/mc$ where $m = T/c^2 + m_0$ and T is the kinetic energy of the ion. Although several people had considered this possibility, the tolerances on the energy gain $qV_D \cos\phi$ required to keep the applied frequency in consonance with the changing mass appeared to be completely impractical. (ϕ is the phase angle of the ion with respect to the peak in the RF wave.) The discovery of the principle of phase stability by Vexler (1944-45) and independently by McMillan (1945) changed the situation completely. If the peak energy gain per turn qV_D is made larger (e.g. by a factor of two) than that required to keep ω in consonance with the mass, this principle shows that there will be an equilibrium phase ϕ_0 (60° in this case) about which the phase will oscillate. As a result the energy gain per turn will oscilate about a mean value of $qV_D \cos \phi_0$ and the total energy will eventually reach the desired maximum value. It was generally believed that a cyclotron of this type would require a moderately high injection energy. However, some ion path work I had done during the war, combined with further calculations, convinced me that ions could be picked up directly from an ion source at the centre of the machine. At the optimum value of $\phi_0 = 60°$ I estimated that an efficiency of 3-5% could be achieved. And so in the fall of 1945 I proposed to Lawrence (at Berkeley) that the 184-inch cyclotron be finished using frequency modulation and that as a proof of principle we use the old 37-inch cyclotron. In the relation $\omega = qB/mc$ I suggested we simulate the acceleration of deuterons to 200 MeV (11% mass change) by an 11% decrease of magnetic field with radius.

I am convinced that there has never been a major accelerator that is quite as forgiving as the FM cyclotron or synchrocyclotron. Within a period of three months we had changed the magnet, installed one dee, a non-hooded ion source, probes, set a rotating capacitor whirling around, and had brought a beam out to full radius. (Richardson, MacKenzie, Lofgren and Wright, 1946) The yield as a function of $d\omega/dt$ verified my calculations on the ion pick-up process.

Lawrence had been following our efforts closely because he was concerned about the feasibility of the design now under resumed construction for the 184-inch magnet. This involved 1 MV on the dees and 25 turns to give 100 MeV deuterons! After I demonstrated the full radius beam to him, Lawrence became very excited and rushed out of the laboratory to drive up the hill to the engineering office. I understand he passed a truck carrying one of the huge dee stem tanks necessary for 1 MV on the dees. Stopping the truck, he told the driver to turn around and take the tank back to storage - or maybe the dump! (The FM cyclotron required only a few kV on one dee.) This occurrence illustrates the eagerness with which Lawrence adopted new ideas and included them in the program of the laboratory. It is amusing to note that the first nuclear physics research with an FM cyclotron was at 15 MeV on "The New Reaction (p,pn)", Richardson and Wright.[10]

Four years after our "proof of principle" there were 10 FM cyclotrons in operation and four were under construction - a testimony to the hunger of physicists to get back to fundamental research and also to funds made available by the governments of various countries.

List of Operating FM Cyclotrons in 1950
(four years after "Proof of Principle")

x	UCLA	Jan 1946	20 MeV p originally at Berkeley	
x	Berkeley	Nov 1946	350 MeV α (pions)	190 MeV d
x	Rochester	Jan 1949		240 MeV p
x	Princeton	1949		18 MeV p
	Dubna	1949	?	later 700 MeV p
x	Amsterdam	1949		28 MeV d
✓	Harvard	1949		140 MeV p
x	Harwell	1949		180 MeV p
✓	McGill	1949		100 MeV p
x	Columbia	1950		385 MeV p

The following FM cyclotrons were under construction:

✓	Uppsala	180 MeV p (modified to SF)
x	Liverpool	380 MeV p
x	Chicago	450 MeV p
x	Carnegie Inst. Tech.	440 MeV p

✓ still operating x terminated

The number of FM cyclotrons started after 1950 and still in operation is about 8. Those at CERN and Dubna are the only ones over 200 MeV.

The SF Cyclotron 1950-

In 1950, when the construction of meson-producing synchrocyclotrons was at its height, there appeared a demand for a large flux of neutrons. Experiments on the 184 had demonstrated the high neutron multiplicity obtained from the spallation of heavy nuclei by nuclear projectiles of several hundreds of MeV. And so it was suggested that high currents of 250 MeV deuterons, for example, could easily produce a gram of neutrons per day. The prime candidate for an accelerator for this purpose was the linac, but the L.H. Thomas modification of the cyclotron was also considered seriously for the first time. Convinced of superiority of the cyclotron, I suggested to Lawrence that we could get a "proof of principle" for the Thomas cyclotron by modeling 250 MeV deuterons ($\beta = v/c = 0.47$) with 70 keV electrons. The electrons at their final energy would have the same relativistic velocity as the deuterons, but a momentum smaller by a factor of 3700. As anticipated, I found that stray electric and magnetic fields were more serious for electrons than for deuterons but that in practice they could be either eliminated or compensated. This program was born classified, so for several months I spent 4-day week-ends at Berkeley (after 3 days teaching nuclear physics at UCLA) in a locked shack at the top end of the hill, tuning the 54 trim coils which had been installed on the contoured poles of the magnet. Initially, it was rather lonesome, with Lawrence being the only frequent visitor. However, after I had demonstrated the correctness of the Thomas principle and in addition had found the beam could be extracted with 80% efficiency using the $\nu_R = 3/2$ and/or $\nu_R = 1$ resonances, I received the help of a number of excellent colleagues. Nevertheless, the clammy hand of classification continued to be fastened on the development until 1955 when Lawrence was allowed to describe it in his lecture at the first Atoms for Peace Conference at Geneva. Publication[11] followed in 1956 and included the description of the second electron model at somewhat higher energies. I should remark that several times during the early 1950's I made informal proposals to the U.S. Atomic Energy Commission that a high current Thomas-type cyclotron be built at UCLA with an energy sufficient to produce large quantities of mesons. Continued classification made this impossible.

Why did it take so long for the Thomas idea of combining the focusing possible with an azimuthally varying magnetic field with the radial increase of field required by relativity? The following reasons certainly contributed to the delay:

1. The analysis of Thomas was expressed in terms of a sinusoidally varying field of the form

$$B = B_c \left[1 + a\left(\frac{\omega r}{c}\right) \cos m\theta + b\left(\frac{\omega r}{c}\right)^2\right] ,$$

where B_c = central field, r = orbit radius, $m = 3$, $b = \frac{1}{2} - \frac{a^2}{m^2-1}$.

Schiff pointed out later that $m = 4$ could also be used. Experimentalists received the impression that the field must be of the sinusoidal form. This was beyond the technical abilities of the 1930's.

2. The development of the FM cyclotron in 1945-46 satisfied the immediate need for relativistic energies and for the investigation of meson physics. This was reinforced by the fact that the synchrocyclotron was so easy to build and to commission.

3. Thus it was only when the usual currents ~1 μA from the FM cyclotron proved inadequate that the Thomas suggestion was revived. Unfortunately the "proof of principle" achieved in 1950 was classified for 5 years.

The next important contribution to the solution of the relativity problem was made by the MURA group[12] (Kerst, 1955) in their development of the FFAG (fixed field, alternating gradient) accelerator. They pointed out that shaping the Thomas hills and valleys in a spiral (azimuth changing with radius) would increase the axial focusing force of the magnetic field. The importance of this effect is shown by the simplified expression proportional to the focusing force $1 + 2\tan^2\varepsilon$, where ε is the spiral angle. Thus the contribution of the spiral (second term) can be many times greater than the simple Thomas effect. In TRIUMF at high energies, for example, the second term is 15 times as large as the first. The idea of the spiral completed the magnetic field of the SF (sector focusing) cyclotron. The highest velocity particles that have been produced in the SF cyclotron were those of the electron model of the Mc^2 proposal at Oak Ridge (R. Livingston, J. Martin et al., 1961) where the final β is $\sqrt{3}/2$, but the first cyclotron accelerating nuclear particles to velocities higher than that achievable by the NR cyclotron was that of UCLA (Richardson, Wright, Clark et al., 1960) at 50 MeV protons.

The other components of the SF cyclotron also required major development. Most of the lessons learned and improvements made for the FM cyclotron were not applicable to the SF cyclotron. This was particularly true of the RF system, where higher voltages were required but the rotating capacitor could be (thankfully) thrown away. At this point I should mention the name of Ken MacKenzie of UCLA, whose seminal contributions to the RF were important to both types of cyclotrons. Other attractive oportunities opened up with the new cyclotron type: high efficiency extraction, external ion sources both polarized and unpolarized, better beam quality, heavy ion acceleration, etc. On the other hand, as one who has brought several of both types of cyclotron into initial operation, I can comment that the SF cyclotron is much less forgiving of sloppy tolerances than is the FM cyclotron.

By the time of the 2nd Conference (1962 at UCLA) there were six SF cyclotrons in operation with $\beta = v/c > 0.25$

UCLA	1960	50 MeV p
Berkeley	1961	60 MeV p
Karlsruhe	1962	55 MeV d
Oak Ridge	1962	65 MeV p
Colorado	1962	30 MeV p
Ann Arbor, MI	1962	35 Mev p

It should be mentioned that one of the last NR cyclotrons to be built was that at Dubna (Flerov) for the acceleration of heavy ions. It has a diameter of 3.1 m and energy/nucleon = $K(Q/A)^2$ with $K = 250$ and a nominal upper limit of 10 MeV/nucleon $\beta = 0.15$. This cyclotron, and its predecessor at Stockholm, were both very successful in using energetic heavy ions to produce new transuranic isotopes.

Since 1962 the proliferation of designs and objectives has been spectacular. Perhaps the best way of summarizing the developments in the last 20 years is to list the major objectives that seem to have activated the field and to give an example or two.

Towards Higher Energies and Intensities

The efforts of Oak Ridge in this direction with the concept Mc^2 have already been mentioned.

The effort at UCLA (1958-64) was primarily designed to achieve the highest possible proton energy (50 MeV) with the limited funds available. It was hoped that this would lead to the construction of a meson factory. The general features of the design later formed the basis for TRIUMF (1968-1974) at 520 MeV with variable energy, high extraction efficiency, simultaneous beams.

The other cyclotron meson factory is SIN (J.P. Blaser, Hans Willax et al., 1961-1974), also based on design features unique to high energies. SIN has constant 590 MeV energy, and extremely high extraction efficiency.

Towards Excellent Beam Quality

The original MSU cyclotron design and the care employed in its construction (H. Blosser, M. Gordon et al., 1959-1965) resulted in a beam of high quality of variable energies and particles well suited to precise experiments in nuclear physics.

The objective of high beam quality was carried to higher energies at Indiana (M. Rickey, R. Pollock et al., 1966-1975). In fact 200 MeV protons and equivalent energies of other light ions were obtained in that separated sector facility.

The latest step in the direction of higher beam quality bids fare to be a giant one indeed. This is the concept of electron cooling, developed for very high energy accelerators but now being applied in a storage ring at IUCF, Indiana.

Towards Versatility

Obvious examples of this objective in the initial group of SF cyclotrons are the Berkeley 88 inch and ORIC. The large amount of effort on ion sources, both polarized and unpolarized, is oriented to this purpose. In Europe, Jülich, Groningen, Grenoble and GANIL are obvious examples of facilities where versatility is important.

Heavy Ions - Superconducting Cyclotrons

The ability to accelerate heavy ions is an obvious attribute of the versatility of the previous section but when this ability grows to paramount importance it seems to lead to the concept of a superconducting cyclotron - if only for economic reasons. Here Chalk River, MSU, and Milan have taken the lead and it will be a fascinating task to try to evaluate the relative excellence of two cyclotrons, vs. a cyclotron and a van de Graaff, vs. one cyclotron and some possible source of heavily stripped ions. Presumably time will give us the answer.

Highlights of the Cyclotron Conferences

Perhaps the best way of summarizing the development of the cyclotron since 1958 is to present the highlights of the 10 international conferences which have been held since then, usually on a triennial basis. Again, what is a highlight reflects my personal judgement.

1st Conf. 1959 - Sea Island, GA - 85 participants

1. There is great excitement as the conviction grows that the concept of sector focusing will open up a whole new field for cyclotrons with full intensity, unlimited by the relativity problem.

2. Several SF cyclotrons are reported in operation but all have $\beta = v/c < 0.12$, which is achievable with an NR cyclotron.

3. Several proposed projects are described which will achieve velocities with $\beta > 0.25$.

2nd Conf. 1962 - UCLA - 139 participants

1. The first successful acceleration of H^- ions in a cyclotron (Colorado) is reported.

2. Detailed investigation of the beam of the first truly relativistic SF cyclotron (UCLA 50 MeV protons since 1960) is discussed.

3. Preliminary reports on the operation of the Berkeley 88 inch and the Oak Ridge ORIC are given.

4. Reports on the construction of design studies of 6 other SF cyclotrons are presented.

5. Much development work on orbit theory and computation, on magnetic field measurements, RF systems and extraction is reported.

6. 11 papers on meson factories are on the program.

3rd Conf. 1963 - CERN - 146 participants

1. "Meson Factories" are included in the title of the conference, and 29 papers concern meson factories either directly or indirectly, including the concepts which later became SIN and TRIUMF.

2. Reports on the newly operating cyclotrons at Michigan (Ann Arbor), Karlsruhe and Philips are given.

3. The first publication on the separated orbit cyclotron (SOC) is presented by F.M. Russell.

4th Conf. 1966 - Gatlinburg - 224 participants

1. The SIN Meson Factory takes its final form

2. The H^- Meson Factory moves from UCLA to Vancouver.

3. There are 5 papers on the conversion of FM cyclotrons to SF operation (one is, hopefully, successful).

4. Three papers are presented on the SOC.

5. Some 25 papers are presented on beam diagnostics, extraction and ion sources (including polarized sources).

5th Conf. 1969 - Oxford - 202 participants

1. The Cyclo-Graaff at TUNL starts operation. A 15 MeV cyclotron injects negative ions into a 2 MeV van de Graaff giving 20 keV energy resolution.

2. IUCF is under constructions at Indiana.

3. Heavy ion acceleration elicits 13 papers, including several proposals for new machines.

4. A complete session is devoted to extraction, with 10 papers.

6th Conf. 1972 - Vancouver - 195 participants

1. Ten papers are presented on cyclotrons as particularly effective accelerators of heavy ions. It is clear that this property of cyclotrons is becoming increasingly important as a justification for their support.

2. 13 papers are presented on the applications of cyclotrons (primarily medical).

3. Computer control of cyclotrons is becoming an important topic.

7th Conf. 1975 - Zürich - 231 participants

1. "Applications" becomes part of the title of the conference and 26 papers are presented on the topic.

2. The initial operation of the two cyclotron meson factories SIN and TRIUMF is reported.

3. Indiana, IUCF, reports its first beam.

4. The GANIL Heavy-Ion Laboratory is proposed.

5. The demand for heavy ions has spawned a new breed of cyclotrons. Both MSU and Chalk River are well along in the design of superconducting cyclotrons of K = 500.

8th Conf. 1978 - Bloomington - 205 participants

1. Chalk River and MSU are joined by Milan in the design and construction of superconducting cyclotrons for heavy ions. 16 papers are presented, primarily from the three institutions.

2. The separated sector concept continues to attract adherents with new proposals.

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3. With the continued emphasis on heavy ions several new or improved concepts for the sources of those ions are discussed: in particular the electron cyclotron resonance (ECR) source, the electron beam ion source (EBIS) and improved PIG sources are discussed.

9th Conf. 1981 - Caen - 225 participants + GANIL staff

1. Eleven papers are presented on superconducting cyclotron projects from nine institutions with Texas A&M joining construction phase with Chalk River, MSU and Milan. Five design studies concerning heavy ions at other institutions are under way and one (TRIUMF) concerns the acceleration of protons to multi-GeV energies.

2. Twenty-five papers are reported on room temperature SF cyclotrons, some compact and some of the separated sector type. Two reports are given on projects in the People's Republic of China (Lanzhou and Shanghai).

3. There are 102 cyclotrons in operation, 11 FM and 91 others, almost all of the sector focusing type. At least 45 of these are for isotope production or medical use.

4. The ion sources for cyclotrons continue to receive a great deal of attention, particularly those sources prolific in heavy ions.

The Future

Superconducting Cyclotrons

It now seems clear that the successful operation on the MSU 500 has assured the future of the superconducting cyclotron, particularly where the acceleration of heavy ions is of prime importance. This is because of the economy of achieving the required bending power with superconductivity. Some improvements in the design may be possible - that is usually the case. Also, should one go higher in energy than the K = 1400 of Munich, or are there clever ways of getting ions more heavily stripped that might make that path unnecessary? At what energies should synchrotron rings take over (if at all)?

Regarding high energy protons, TRIUMF has spent some 6-8 man years and many computer hours on a design of superconducting cyclotrons for protons up to 15 GeV. The project was dropped because the physics requirements for high energy kaons and antiprotons raised the desirable proton energy to 30 GeV, and it was felt that a design including a superconducting cyclotron ring at 30 GeV was impractical at the present time.

There is the possibility that in the future there will be a demand for a neutrino factory or an intense neutron source which might best be satisfied by a superconducting cyclotron in the 5 GeV range of energies. Our studies have gone far enough to show that a machine of this sort is probably quite feasible. The following table lists the parameters of the two-cyclotron design,[13] using TRIUMF as the injector:

	I	II
Injection energy	430 MeV (TRIUMF)	3.5 GeV
Extraction energy	3.5 GeV	15 GeV
No. of sectors	15	42
Radius (max)	10.1 m	42.4 m
Radius (min)	7.5 m	40.6 m
No. of RF cavities (1 MV)	9	54
RF frequency	46 MHz	115 MHz
Magnet excitation	2.1×10^6 At	2.5×10^6 At
Coil dimensions	8×60 cm^2	8×60 cm^2
Sector field	4 T	5 T
Gap width	7 cm	7 cm

A number of useful computer codes were developed for the design of the magnets.[14] The contour of the magnetic field for the 3.5 GeV stage is shown in Fig. 1 while the sector design and orbits are shown in Fig. 2. A group of five of the 42 sectors and the beam orbit of the 15 GeV stage are shown in Fig. 3. Tolerances due to the necessary passage of integral and half integral resonances were investigated and also the requirements of passing an intrinsic non-linear resonance. Extraction using an integer resonance was investigated. No unsolvable problem was found but the difficulties increased as the energy increased. I would suggest that if a 5 GeV superconducting cyclotron is considered, it should be preceded by a 1 GeV superconducting injector, probably using separated sectors.

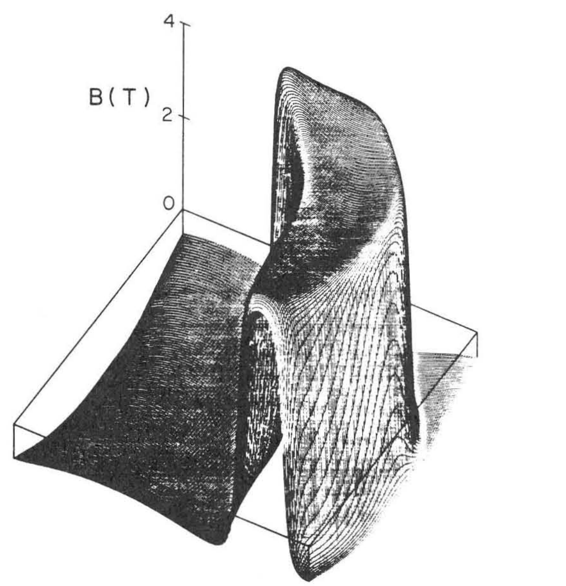

Fig. 1

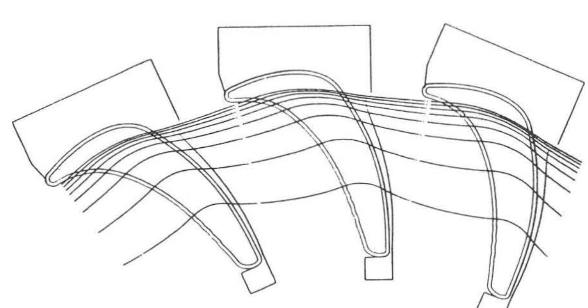

Fig. 2

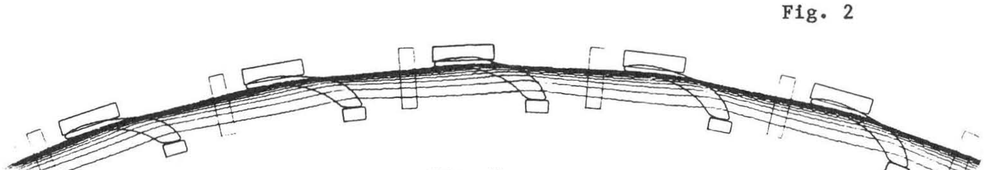

Fig. 3

Some Highlights of the 10th Conference

Initial Operation of Several Cyclotrons

1. GANIL reported the acceleration of a beam of $4x10^{11}$ particles per second of Ar^{16+} with energy of 60 MeV/nucleon.

2. MSU reported the operation of the K500 superconducting cyclotron with a sample beam of $3x10^{11}$ pps of N^{5+} with energy of 35 MeV/nucleon.

3. Grenoble reported $8x10^{9}$ pps of Ar^{17+} at 30 MeV/nucleon.

Meson Factories

SIN is planning on intensities up to 2 MA with the new injector while TRIUMF is planning on using RF booster cavities to facilitate continuous extraction of the H^- beam and to reduce the beam lost by electromagnetic stripping.

Under Construction

SC Cyclotrons: Chalk River, Milan, Texas A&M, MSU.

SS Cyclotrons: Japan, RIKEN, Dubna, South Africa.

Modification: Uppsala

Munich Status

The Munich group reported on the magnetic characteristics and manufacturing problems with their very large (2.5 m radius) prototype superconducting sector. As an intermediary between the van de Graaf and SUSE, they are now designing and building TRITRON, a superconducting version of Russell's Separated Orbit Cyclotron (3rd Conference, 1963).

Small Cyclotrons

G. Wolber of Heidelberg discussed the present status and probable future for the use of small cyclotrons in fast neutron therapy. Although the effects of the world wide recession have taken their toll, he believes that the future for cyclotrons in the 40-60 MeV range looks quite good. The desirability of making the cyclotron as flexible as possible for use with patients cannot be over emphasized. The MSU proposal for a midget superconducting cyclotron looks particularly interesting in this regard.

Cooling Rings

The INS group at Tokyo has performed some very important tests at the TARN ring. They used stochastic cooling with longitudinal pick-up and booster delay lines connected across the ring to get the following results.

Beam	7 MeV p β = 0.12
Ion frequency	1.14 MHz
Intensity	12μA. 6.6 x 10^7 protons
Δp/p (full width)	1.4% initial 0.5% final
Cooling time	~20 s

R. Pollock of Indiana discussed the cooling in storage rings. In contrast to the stochastic cooling above, electron cooling times are of the order 0.1 to 10 s. He also concludes that for E/A > 2 A MeV/nucleon it is possible to obtain higher luminosity by recirculating the beam than by the use of a single pass (for equal mass particles). Electron cooling is definitely more expensive than stochastic cooling and requires long straight sections. The IUCF cooling ring will hopefully be available for testing at the time of the next Conference (October, 1986). Another interesting storage ring reported by Indiana was at 300 keV and was used to store pulses for acceleration in the SS cyclotron.

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