ROUND TABLE ON UNCONVENTIONAL APPROACHES TO FUSION ERICE-1981

An impromptu discussion transcribed directly from the recorded tapes

Chairman:
Francis F. CHEN (UCLA)

Members of the Round Table:
Charles W. HARTMAN (LLNL)
Rulon K. LINFORD (LANL)
Albert E. ROBSON (NRL)
Ravi SUDAN (Cornell University)
George C. VLASES (University of Washington)

Participants in the audience:
B. BRANDT (FOM — Instituut voor Plasmafysica)
B. BRUNELLI (EURATOM-CNEN)
R. CARRUTHERS (Abingdon, Oxon., UK)
B. COPPI (Massachusetts Institute of Technology)
B. LEHNERT (Royal Institute of Technology)
M. OKABAYASHI (Princeton University)
A. SCHLÜTER (IPP — Max-Planck Institut für Plasmaphysik)

TABLE I - RATING SCHEME FOR UNCONVENTIONAL APPROACHES TO FUSION*

CONCEPT	Time scale	Advantages	Disadvantages	Status of Physics				Reactor Designs	
				Stability		Transp. scaling	Other	Degree of Detail	Severity of Problems
				Primary	Secondary				
Field-Reversed Theta-Pinch TRACT CTOR									
Compact Torus Spheromak									
Multipoles Surmak Intrap									
Field-Reversed Z-Pinch Extrap									
Liners									
Advanced-Fuel Tokamaks									
Particle Rings									
Dense Z-Pinch									
Plasma Focus									
Long, linear Systems									

A: well understood, well worked out, or short time scale
B: needs work, or medium time scale
C: almost unknown, or long time scale

* Though the details of TABLE I were never filled in, it served as a good basis for the discussion. TABLE II shows R. Sudan's tentative scheme.

The foot-notes refer mainly to lectures in the present proceedings.

Editor's note: As the abbreviation of "Reversed Field" (RF) also stands for "Radio Frequency", the term "Field Reversed" (FR) has been used throughout this Round Table.

ROUND TABLE ON UNCONVENTIONAL APPROACHES TO FUSION ERICE-1981

1. INTRODUCTION

CHEN

I have here a form for a rating scheme, whose purpose is to start our discussion, so that we can all leave this meeting with some idea of the status of the various concepts.

It is not intented that this sheet (see TABLE I) should be completed with something in every square. It is simply a scheme to organize our thoughts so that we have a matrix within which to start the discussion. On the left, I have listed the various concepts and grouped them in a way which I thought was appropriate, while across the top, I have written some criteria for discussing these concepts. For instance, the time scale means whether the idea is to be implemented in competition with present-day tokamaks or whether it should be something to be considered for the future.

Something should be said about the "advantages" and "disavantages" of each concept. As far as the status of physics is concerned, there is "primary stability", which is the status of our understanding of the hydrodynamic stability of the plasma, and "secondary stability" which, usually, is less well known, but which has to do with the micro-instabilities and rotational instabilities and other finer effects.

There is our understanding of "transport scaling"; and then, depending on the concept, there are other important aspects of physics which should be discussed. For instance, in the space for liners, one would have questions which concern the liner. In the last two columns, we should discuss the "status of reactor designs", i.e. how much detail is available as far as thinking goes for making the concept into a real reactor.

Now, if you wish, you can, after our discussions, rate these things as A, B and C. However, let me say that as concerns the various concepts an A grade is no better than a C grade, because all that it means when you mark something C is that there is very little information known about that aspect, and therefore you can hope; also you have an excuse for getting more money to do more work on the concept. There is no stigma attached to marking something A, B or C. It is just a matter of understanding the status of our knowledge.

2. AUDIENCE PARTICIPATION

BRANDT

Would you be brave enough to grade the standard tokamaks in the same way with an internal reference?

CHEN

I would give them all A because it would be the standard by which we judge. But do not misunderstand me because these are relative terms. When something is well understood, it just simply means well understood or not when compared with something that you have worked on for a very long time — like the tokamak, for example.

SCHLÜTER

So you imply that nothing is better understood than the tokamak?

CHEN

I would say so simply because of the amount of work that has gone into it and not because it is deserving of any special consideration.

ROBSON

Do you think that transport is one of them? I know there are many others.

CHEN

I think you are right, especially about electron transport. It is not very well understood.

ROBSON

So we may give that a B rating.

CHEN

That may be a B under transport scaling of tokamak. It doesn't matter. This is not a vote. Everyone can give his own rating — he doesn't have to show it to anyone else — it's a matter of co-ordinating our thinking.

CARRUTHERS

There is no way of marking something well understood, well worked out if there is no time scale.

CHEN

Right. I think that we are on the wrong track. It was only meant to be a means of organizing the discussion.

SUDAN

On the means of organization, I feel that you have listed these items as individual concepts, yet within these concepts there are groupings that should be brought together. I feel, for instance, that the field-reversed theta-pinch, the compact spheromak and particle rings are very close to each other. Differences are only in the matter of how you heat and sustain these devices and I think in that sense they could be called group A. The liner as presently conceived demands something like a field-reversed, TRACT[1] or compact torus or particle ring really inside it.

You cannot, therefore, just discuss "liner" on its own because the liner is coupled to what is inside it. The dense Z-pinch and plasma focus are again sisters. So I think, in that sense, one should classify these concepts into broad groups. To my mind, that is a better classification.

CHEN

The way I chose the members of this round table originally was to do that. For instance, Charles HARTMAN could have covered all the field-reversed configurations, but it didn't work out that way and now someone else will have to speak about them.

Let me just give a quick review of the multipole situation before going on to something else. For instance, the time scale for multipoles is probably a long time scale, because it is a concept which is not very well developed and it has to do with advanced fuels, which may not be burnable — except for what Bruno COPPI[2] has said — for quite some time. The advantages of multipoles are purported to be the lack of synchrotron radiation and the stability because of minimum-average-B. There are many disadvantages. One of them is the wall loading. Because the bridge region of the multipole is so small, in order to have enough room for the conductor, you must make the size of the entire machine very large, which means that you have a very large wall loading. And to make matters even worse, the wall load is almost entirely on the surface. The wall loading is not so much due to neutrons as due to X-rays from bremsstrahlung. The difficulty of floating rings could be considered a disadvantage although calculations, so far, show that this is perfectly feasible.

As far as our understanding of the stability is concerned, minimum-average-B is fairly well established. The calculations, even for *finite* β, or the *critical* β for multipoles have been done. The value of β turns out to be rather low, and this may also be one of its disadvantages. Secondary instabilities are trapped particle instabilities or instabilities associated with a toroidal field added to a multipole. I would say that this subject has not been sufficiently studied, although the primary stability has been well explored.

Transport scaling has been explored both experimentally and theoretically for a long time. In my talk[3], I listed three mechanisms: classical diffusion, convective

[1] - *Triggered Reconnection Adiabatically Compressed Toroid (TRACT).* See report *EPRI AP-1752 April 1981,* and also:
- *Long Linear Fusion Systems* by G.C. VLASES, in present proceedings.

[2] *Ignition Experiments with Advanced Fusion Fuels* by S. ATZENI, B. COPPI and G. RUBINACCI, in present proceedings.

[3] *Multipoles and Surmak, I Physics* by F.F. CHEN, in present proceedings.

vortex diffusion and BOHM diffusion. This is understood in the sense that these mechanisms have been seen in the laboratory; they are not completely understood in terms of the theoretical explanation; and certainly, in order to extrapolate to a reactor, one would have to do more work on the transport scaling.

Another important subject is the reduction in synchrotron radiation, which is a very difficult calculation. Until that is done, one does not know whether a multipole has an advantage over a tokamak or any other device in regard to operating at high temperatures.

In the reactor design, I showed you a conceptual design for a D-He^3 multipole reactor[4]. The existence of this design makes the situation much clearer than it used to be. There have been no insuperable difficulties discovered in that design, but the parameters were not optimized; for instance, the shape of the relative currents and the conductors of an octopole were not optimized, and there would have to be many reiterations of different designs before one knew exactly how a reactor would look. I think that the first step has been taken in looking at the reactor aspect of a multipole.

ROBSON

I would like to ask something here, Frank. Something that was new to me on the subject of multipoles, which I hadn't heard before, and which, I think, is relatively recent, was the point that you made about synchrotron radiation. I had always understood that one of the great advantages of multipoles was that, because the plasma was mainly in a field-free region, synchrotron radiation would be small or negligible, and so one could go to higher temperatures and to advanced fuels. However, do I understand that your latest calculations show that this advantage is not so great as it was thought to be, and synchrotron radiation is still a very serious problem in high temperature multipoles?

CHEN

That statement did not come from any calculation but from a very elementary consideration that, since you have a surface field, you must increase the magnetic field to keep the diffusion the same, because the gradients are sharper. And that increase in the magnetic field makes the synchrotron radiation large from the surface of the multipole; and in fact what was not realized before was that the radiation from the surface could be almost as large as the radiation from a non-surface field configuration with a slightly weaker field. The answer will depend on a very detailed calculation of the exact geometry, the exact field shape inside a multipole.

ROBSON

Would you say then that it is crucial for the future of multipoles to do this calculation as soon as possible?

---

4 - *Multipoles and Surmak, II Engineering* by F.F. CHEN, in present proceedings.

CHEN

The calculation is being done at TRW at the moment, and I think the answer is critical.

LEHNERT

If a sufficiently good plasma confinement can be achieved for multipole devices in presence of magnetically shielded supports, it would put these devices in a better position from the technical point of view. Then the internal rings could both be suspended mechanically and be fed by a cooling medium, without the need of a repeated cooling process.

CHEN

Yes, there are two scenarios that can occur when you run a floating ring. One is to have one set of rings being cooled in one vacuum chamber and another set of rings being used in a reactor, and I showed the other day[5] that you can maintain these rings alive for the order of five days. Now, if you had shielded supports which could circulate liquid helium, you would have the second scenario in which you would not have to cool another set of rings and replace them periodically every few days. I also think that you would make the reactor run much more smoothly.

LEHNERT

Let me stress that the shielded supports are a main feature of the Intrap concept. In this respect the number of internal rings is a secondary question. I have taken a single ring as an example. Moreover, the plasma stability problem in this particular case comes close to that of the plasma in Ravi SUDAN's particle ring system. Thus, concerning plasma stability, there is a connection between the spheromak, FRC and Intrap concepts.

CHEN

The support problem is certainly one of the big problems with multipoles, and that is the reason why I group them in the same category.

LEHNERT

I agree.

SUDAN

The magnetic field design of any reactor is probably the most critical component. Several years ago, an accelerator called Isabelle with superconducting 5 tesla magnets, was authorized by the Department of Energy. This project is presently in severe trouble because, whereas the prototypes operated at 5 tesla, the units manufactured by Westinghouse have not cleared 4.2 tesla, although they were

[5] Ibid. [4].

built to the exact specifications of the Brookhaven design. So, in between Brookhaven and industry, there was a loss of 1 tesla.

We shall have to keep in mind that when we are going to very large magnets with big bores, even if the expertise exists inside an individual laboratory, it has not yet diffused into the industrial organization. Secondly, on top of these extreme limits that you are thinking about, if you now want floating levitated poles, that means adding one order of magnitude more difficulty to an already very tough problem. Thus, unless it can be assured that these designs are supported in some way I, in fact, whole-heartedly agree with Bo LEHNERT. Unless you have rigid solid supports for these magnets, I cannot conceive superconducting and levitated conductors operating in a reactor environment. As somebody pointed out, in the case of any little thing going wrong, the inside would be a disaster of some magnitude. I think, therefore, that the problem you raise of supports is an extremely important one for multipoles.

CHEN

I don't think that you fully realize what we mean by supports. The supports do not support the weight of the conductor nor do they support the magnetic forces. They are only leads to bring in the coolant, because you will never be able to make a support that will take the magnetic stresses in a multipole. And, in fact, floating rings have been made in at least 4 different experiments; the technology of floating a ring already exists. I don't think that is the problem.

SUDAN

I think the stability of four rings working under all possible perturbations is not yet established.

CHEN

That's true. The subject has been studied, but it has not been demonstrated experimentally.

LEHNERT

Let me add that, if the shielded supports would really work as is hoped, it would become possible to support each internal ring by several support pairs. But I understand that, if you have such heavy rings as in your system study, it might not become technically feasible to provide mechanical support in this way. Still I wish to back up what Professor SUDAN said some minutes ago. At least we all seem to be of the opinion that successful support shielding would enhance the potential of multipoles.

OKABAYASHI

Based on my experience with multipole device experiments, I'd like to make some comments on the scaling laws which were mentioned in the discussion of multipole ring reactors. These measured transport coefficients were obtained in the 1eV - 10eV range. There are no bases to extrapolate these scaling laws to the plasma in the several hundred keV range. Even in tokamak, it is hard to scale up to the reactor regime, although the extrapolating factor is 3-4. These observed scaling laws should not be considered in a serious manner when discussing plasma performance in 100 keV plasma.

CHEN

I certainly agree with that. There was a design for a proof-of-principle octopole experiment (which was not funded), whose purpose was exactly to extend to higher temperatures our experience with these transport scaling laws. It is clear that one cannot proceed with this concept until some experiment of that type is done. That is what I mean by the fact that a grade of B or C (see TABLE I) is not necessarily bad. It just means that you must take another step.

UNIDENTIFIED

A lot of the things that have been mentioned about multipoles are problems and questions. In any device there are going to be problems and questions. It's worthwhile asking the question: Have there been any failures?

For example, when Scyllac was unable to be stabilized did we call that a failure? Has the multipole programme run up against any failures or roadblocks of that sort?

CHEN

I can't think of any. Maybe someone else can.

COPPI

Years ago, when an official report had been written in which tokamaks were considered useless machines that could not be understood — the Russian work was doubtful — we embarked on the multipole programme at Princeton and I, too, did theoretical work on it. I do not remember that there was any failure that would cripple the concept but unfortunately the programme was stopped because of a change of fashion. Nevertheless, Frank, between the present state of multipoles and what you propose to do, there is an enormous distance. In my opinion, in devising a reactor concept, it is always better not to place too much emphasis, at this stage, on the optimal magnetic confinement configuration that can be adopted but to take any magnetic configuration which can create the desired thermonuclear plasma regimes. I remember L.A. ARTSIMOVICH used to tell me there are many people who like to play the game of installation physics rather than that of plasma physics! I personally would prefer to use, at first, a more proven configuration for the reactor regimes while sponsoring a vigorous multipole programmme just to complete our understanding of the relevant plasma physics in the absence of fusion reactions.

CHEN

The multipole programmes that you mentioned that we used to do at Princeton served their purpose because those experiments showed the existence of classical diffusion and the existence of ballooning modes outside β_{crit} I think that those are very important confirmations of the theory.

COPPI

When we read our papers, I think that Ravi talks about our ballooning modes for multipoles.

BRUNELLI

Up to recent times, I think that the general opinion was that the multipole system could not compete as a reactor, due to the fact that it was almost inconceivable to keep cool the superconductors, screen from irradiation, etc., and so I was very happy to hear that this may be possible without strange cryostatic solutions. When describing the surmaks with exotic fuels, Francis CHEN mentioned a field of 9 tesla. I think that we probably need a higher field — not available with superconductors now.

LEHNERT

Few experiments have so far been performed on multipoles. In particular, I want to draw attention to the results obtained several years ago by Dr. J.E. HAMMEL[6] and his collaborators at Los Alamos with a minimum-average-B quadrupole device having shielded supports. It was somewhat of a tragedy that this experiment was not given the chance of being continued, because it provided very positive results on plasma confinement, even at modest linear dimensions. To judge from these results, it would not become impossible to reach β values which are substantially higher than those so far recorded in tokamaks. As one of the consequences of this, β values of the order of 30% or more may even make water-cooled internal coils feasible in a reactor system.

CHEN

I have done some power balance calculations, and I think that the resistive losses of a normal coil would be much too large. But, if you were to develop multipoles, there is a logical programme that you would do — which has been worked out — you would first make a larger experiment to test the scaling laws

[6] - J.E. HAMMEL, J. HENINS, R.W. KEWISH Jr., J. MARSHALL and A.R. SHERWOOD, in Proc. 4th Intern. Conf. *Plasma Physics and Controlled Nuclear Fusion Research*, Madison, June 1971, Vol. 1, IAEA-CN-28/Vienna 97. See also:
- J.E. HAMMEL, J. MARSHALL and A.R. SHERWOOD, Los Alamos Sci. Lab., Progress Report LA-4888-PR, UC-20, February 1972, 80.
- J.E. HAMMEL, Los Alamos Sci. Lab., Report LA-6203-MS (1976).
- R.L. HAGENSON, A.S. TAI, R.A. KRAKOWSKI and R.W. MOSES, Los Alamos Sci. Lab., Report LA-8186-MS (1980).

to higher temperatures and then you would have a ring experiment in which you would test the technology of superconducting rings and their stabilization. After that, you would go to a proof-of-principle experiment, but you see that you cannot embark upon this series of experiments unless the funding is there, and right now I do not see in the community a spirit of adventure that will allow this to happen.

OKABAYASHI

It may not be such a bad approach to use one "floating ring", as was the case in the Spherator (Levitron) at Princeton. It is quite possible to obtain a plasma parameter range similar to that with a multipole arrangement. In the past, experimental results with octopole, quadrupole and a single internal ring produced the same type of physics results in the same parameter range. With a one-ring arrangement it will be possible to obtain essentially the same basic picture of plasma behaviour in the higher temperature range. Optimization towards a multiple arrangement reactor should be carried out as a second step.

LINFORD

The problem is that with one ring the field in the centre must not be reduced to lower the cyclotron radiation. That was the original argument for having more than one ring. But why should we have one ring when we can make a tokamak with no ring.

OKABAYASHI

The cyclotron radiation is one aspect of the required physics. There are other important issues such as MHD activity, micro-instabilities, etc., which we will be able to study with a single coil arrangement.

CHEN

I see — just to study the physics of the instabilities. That is certainly true. Now let us move on. Perhaps the next most interesting topic to discuss is the field-reversed configurations.

LINFORD

I would like to talk about the field-reversed configurations (FRC), the spheromak configuration and some of the similarities and differences between them. Afterwards I want to come back to the particle rings — about which Ravi will talk — and summarize what things can be done with particle rings that could enhance the behaviour of the compact tori without large orbits. As far as the time scales are concerned, the gross MHD stability has, to a large extent, been worked out, but there are still many complexities to be resolved. However, we are at a very low part of the learning curve as regards the transport scaling and, because of these two factors, I should say that the time scale should not be accelerated very rapidly.

I tend to favour smaller experiments so as *to learn to understand the physics*, before we invest in bigger machines and commit ourselves to spending large amounts of money. But in terms of "advantages", I see that these two configurations have the highest β that it is possible to obtain. For the spheromak, the situation is not fully understood but values of the order of 10% or maybe 20% β are possible. In the field-reversed configurations, from the equilibrium point of view, the range of β is between 50 and 100%. That is quite a high β configuration. The "disadvantage" of both of these systems is that we do not have access to induce currents easily in them to heat them. If we want to maintain one of these configurations for a long period of time, we have to talk about such things as RF current drive or beams or something of this nature in order to sustain the flux in the system. And that is, of course, where particle rings could come in.

One advantage for the field-reversed configuration is that we can translate the rings and move them down a system so that we may talk about a moving-ring reactor. This means that we can move them rapidly enough to uncover the new conducting wall as a function of time so that feed-back stabilization is not required on the diffusion time through the wall.

Under this field-reversed theta-pinch block there is the TRACT reactor, which George VLASES[7] previously talked about. This is a stationary reactor concept. The advantage of this reactor is its very small size. In the case of the moving-ring system, the diameter is about the same, but we are moving it down a system. In reactor studies that have been done, the system is about 40 metres long. It is not extremely long but it is physically bigger than in the case of the TRACT.

The "advantage" of the moving-ring system is that there is no need to worry about sustainment of the ring in any way in terms of refluxing or refuelling or feed-back stabilization. Both the field-reversed configuration and the spheromak have divertor action because of the separatrix. The spheromak tends to have an oblate shape for gross stability reasons, and so therefore it is more difficult to talk about a translating system. It may tend to be a stationary system. This is not an absolute statement because we are still learning about the limits of gross stability of these objects.The possibility of adding particle rings for additional stability to the "tilting mode" would perhaps make these rings stable to the tiltings so that you could translate them in a cylindrical tube.

In the same way we could fuel the field-reversed configuration. The transport scaling in the field-reversed configuration, is very different from that in other toroidal systems such as the reversed-field pinch[8], spheromak and tokamak. I say this because there is no shear in that system at all. Some instabilities are

7 - See corresponding lecture in present proceedings.

8 - *Reversed-field pinch (RFP)*, e.g., the BODIN HBTX-1A, the ROSTAGNI ETA BETA II devices (or the Los Alamos ZT-40).
- See also a well-documented article on the physical properties and engineering concepts of *The RFP Reactor* by R. HANCOX, R.A. KRAKOWSKI and W.R. SPEARS, in Nucl. Engin. and Design **63** (1981) 251.

allowed to exist at fairly strong amplitudes, such as the lower hybrid drift, which are normally not much of a problem, and so the transport issue there is really not understood at the present time. We are working on it. We have learned some things from the experiment and we have found agreement with certain calculations using the lower hybrid drift, but it is, I would say, at a very low level of understanding at the present time.

In the case of the spheromak, these studies are also at a relatively low level, but I think there is more background information coming from the reversed-field pinch, for example, on what one might expect for transport and what kind of microstability might exist there.

It is not clear how close one has to keep to the conducting wall of a spheromak in order to suppress the kind of turbulence which was observed in the ZT-40 machine when it had a ceramic vacuum vessel. Now it has a metallic vacuum vessel and there is a very substantial reduction in the fluctuation level.

A fairly complete first order reactor design has been done for the field-reversed configuration both for the TRACT and the moving-ring system CTOR, but it is very low level in comparison with the tokamak reactor designs. I am not so familiar with the spheromak reactor designs. I don't know whether anyone else can comment on them. This is all I have to say about those two configurations. It is perhaps time to open up for comments.

OKABAYASHI

The first comment is a question. Your comment was that spheromak is oblate, and so we cannot translate the plasma. Why not?

LINFORD

It is more difficult because one has to have a mirror field to maintain this oblate shape while the plasma is translated.

OKABAYASHI

Actually, we do not need to have an oblate shape before a plasma ignites. It might be possible to translate the plasma from a cooking area to a dump section or any other place with a shape stable in the given geometry. The plasma is then to be deformed to an oblate shape when it ignites.

LINFORD

In the burn region itself where you have the blanket, is the plasma moving as a function of time or is it stationary during that phase? I agree that in the guns, for example, we form it and translate it through an entrance region and pop it into the flux conserver. I thus understand what you are saying in terms of forming it and then being able to move it into an oblate flux conserver. That certainly can be done. But what I mean is that in a reactor scheme itself, are you conceiving something which is continually moving past the first wall so that you do not have to worry about feed-back stabilization? On a diffusion time through the wall, one has to worry about feed-back.

OKABAYASHI

Once the plasma is separated from the cooking area, we can overload the first wall in the burning area. We probably do not need to keep the plasma moving in order to minimize the loading on the first wall.

LINFORD

In reactor designs, I don't think that the reason for moving the plasma was to protect the first wall. The reason for moving the plasma is that there is no need to worry about diffusion of the field through the surface of the conducting wall. You see that, on the time scale of the field diffusion through the conducting wall, the stabilizing nature of the wall disappears, and if it is moved fast enough so that you are continually moving the object into a new region of wall faster than the field diffusion time through the wall, then you never have to worry about the complexity of feed-back stabilization for modes like the up/down instability, side-ways slip or whatever.

CHEN

As far as I am concerned the field-reversed configurations are novel in two respects. First the difficulty of making such a configuration and maintaining such a correct distribution is something new and it is an exploratory phase, on which we do not have as much certainty as on the other concepts, such as the standard tokamaks, but the other point is that these are wall-stabilized systems and a question arises as to the engineering of a wall-stabilized system. Does it scare any of the reactor designers to have to make a wall which is in close contact with the plasma and which is a good conductor? Does that put a constraint on these systems which do not exist with the standard tokamak? Isn't that an important question to ask?

OKABAYASHI

According to Princeton's theoretical studies, we do not need to put a conducting wall close to the plasma to stabilize slide-mode or tilt-mode. As I showed in my lecture slides[9], the wall can be away from the plasma surface in 30-50 % of the minor radius. If we want to have about a 1 m or 1.5 m minor radius plasma, the separation can be 50 cm. This may not be good enough. However, you can overload the first wall after the plasma is moved to the cooking area by assuming that the replacement of the first wall does not give too much trouble.

LINFORD

I don't really see that the first wall problem for this case is significantly different

[9] – *The Spheromak* by M. OKABAYASHI and Spheromak Group Study, in present proceedings.

from that of a tokamak, in the sense that in both cases you can back the wall away. There is a separatrix between the plasma and the wall where you can conduct the plasma that is being transported across the separatrix out in a divertor-like action in the same way as is done in a tokamak. So I don't foresee that having a conducting wall within a few centimetres — or whatever is required — of the plasma is necessarily a strong disadvantage.

The point is that since this wall has to exist, normally in a number of reactors, between the plasma and the blanket, we cannot make it very thick or we destory the neutron energy. I mean we reduce it before it gets into the blanket region. The moving-ring system actually has a thin wall at that particular point, which is not highly conducting; the distance between the separatrix there and the wall can be 30% of the plasma radius, which is a similar ratio to that mentioned by OKABAYASHI for stabilization of gross modes for spheromak.

VLASES

I would like to make just one comment on these systems. When he started talking about compact toroids yesterday, Rulon LINFORD[10] defined one as a system in which there is no penetration of the mechanical part of the vacuum vessel into the centre. That is something that I think people should really keep in mind, particularly in the light of some of the problems brought out by Professor SCHLÜTER and Robert CARRUTHERS at this meeting. Certainly, I think that all of us have a more real awareness than we did before — at least that is true for me — of the engineering difficulties that a conventional toroidal system may have. And in particular, it seems to me that the "simply connected" nature of the containment vessel for these systems may lend itself to solving some of the problems that were brought up. For example, in the TRACT reactor that I talked about, the design philosophy was to make the reactor so simple that you could change the first wall very quickly, and not to say that it does not have to be changed.

Another approach which may work is the moving-ring system where you move the plasma away from the generation areas, as has been mentioned. I would suspect that such an approach might again ease some of the engineering problems, although I don't see any direct solutions. It seems to me, however, that it must contribute to solving some of these problems that we have talked about. And then you could even imagine perhaps translating a ring — one of these compact toroids — into a liner which might not even have to implode, i.e., a passive liner. So my general point is that when you have a simply connected system the maintenance should be easier and the engineering design should also be easier and I think that one direction that research should take is to try to exploit this advantage.

CHEN

Some people object to the pulsed nature of the heat load, when you translate

---

10 - *Los Alamos Compact Toroid, Fast Liner, and High-Density Z-Pinch Programs* by R.K. LINFORD, A.R. SHERWOOD and J.E. HAMMEL, in present proceedings.

these things down a tube. Some engineers in the audience should be speaking up.

CARRUTHERS

I think our discussions are being confused by the way in which the word "reactor" is used. We don't all mean the same thing. K.H. SCHMITTER and I have been discussing the end product of the fusion programme — a reactor which will make power in an economic manner. Many of you use the term "reactor" to describe a device in which there is a burning fusion plasma and it is the "reactor evaluation" of this goal which you are discussing. This is a very different animal from the economic power reactor and by overlooking this important, ultimate objective we ignore many problems; problems in areas at least as important as plasma physics in assessing whether or not the economic objective is achievable. This is why I remarked that another option was needed for marking up the rating scheme (see TABLE I). A marking D : adequately understood, inadequately worked out, infinite time scale.

This situation worries me. We have all this good work going into the study of many clever containment concepts — yet, if you consider the economic objectives you will find that most of them have little chance of reaching it, and for very similar reasons. Further work on these various concepts will give a better understanding of the physics and could allow you to build a plasma burning device, but in a way of no relevance to the requirements of an economical fusion reactor.

COPPI

If you were able to study plasmas where nuclear reactions go on, you would be doing a great service to astrophysics, which is wonderful. Maybe something useful will come out of that. I think the usefulness of doing the fusion work lies in learning what we don't know. Now the trouble is that somehow the fusion community has deceived the governments, in letting the governments expect practical results in a short time. We should have gone about our lack of funding, as the "elementary particle" or "astrophysics" people do. In fact, they have proved that it is possible to get money and make a decent living without deceiving.

CARRUTHERS

It is all very well to raise this question of the "usefulness" of plasma physics research, but I doubt whether most fusion laboratories are in a position to unilaterally change their objectives. The statement of objectives when the Culham Laboratory was set up 20 years ago and Euratom policy statements make it clear that we are undertaking a programme to establish whether or not it is possible to obtain useful energy from nuclear fusion. You may say that this is stupid and that there are lots of other interesting things that can be done. However, for the time being we are charged with looking at the reactor question — not at astrophysics. I have been trying to answer the question — can nuclear fusion be usefully exploited as a future energy source ? I find the answer depends upon much more than future work in plasma physics and for this reason I am disappointed that so much of the discussion here is only about "nice devices" which, as you say, will be useful — but not for fusion power.

CHEN

I would now like to call upon Dr. A. ROBSON at this point.

ROBSON

I am not going to talk about liners again, because I had my innings yesterday[11], but I want to add something to what Mr. R. CARRUTHERS has been saying, and I am going to take the liberty of reminiscing a bit, because I suddenly realize that it is 25 years, almost to the day, since I joined the British fusion programme when it was at Harwell. I remember that the atmosphere at the time was one of immense enthusiasm for fusion. It was a more naive atmosphere, to be sure, but nevertheless it was very genuine. The idea at that time was that we should build a very large torus, and pass 40 MA through it. On the basis of simple calculations, that should have made a fusion reactor. Now my job as a new entrant was to look into what to make it out of — in particular, first-wall materials. So I started doing experiments exposing metals to plasmas, and as well as encountering some interesting problems of plasma physics, I also came up against problems which were then unfamiliar to me, but well known to mechanical engineers. One of these was thermal stress fatigue. If you look in the dusty volumes of the 1958 Geneva Conference, you will find a paper on materials, of which I was an author. In it there is a picture of a piece of aluminium which had been exposed to a pulsed plasma for a few thousand cycles, and it looks rather like a dried-up river bed as a result of thermal cycling. For me, this was the first realization that a fusion reactor will have not only plasma physics problems and electrical engineering problems, but problems of mechanical engineering as well.

Now those who can remember what happened in the 1950's may recall that, after the initial excitement of declassification, the popular expectations for the early development of fusion were thoroughly dampened when it became clear that there were enormous problems of physics to be solved before one could even contemplate building a fusion reactor. In England (I cannot speak for elsewhere), there was an interesting reaction: it became positively unfashionable even to discuss fusion reactors. Whereas before 1958 people were willing to discuss practical details of reactors, after 1958, if you brought up such ideas you would be told: "We can't talk about those things yet; we've so much physics to do first". And even now, 23 years later, I still hear people saying: "It is premature to consider what a reactor based on this-or-that concept will look like".

What have we been doing all this time? Well, we have certainly been having a very good time trying to understand the intricacies of plasma behaviour. It's a marvellous subject, with something for every interest; there are lots of small satisfactions

---

[11] *The Linus concept* by A.E. ROBSON, in present proceedings.

to be obtained from learning a bit more about each aspect. Some beautiful technology has been developed. But the time has come, I think, for us to be called to account for all the good times that we have had.

It wasn't until about 1970 that voices began to be heard again discussing what a reactor would actually look like. Bob CURRUTHERS in Europe and David ROSE in the United States were the pioneers in this, and began to draw pictures of systems with D-T fuel going in and power coming out. But it has been a hard uphill road for them to bring together the two segments of the fusion community: the segment which is still totally absorbed in the plasma physics, and which is receiving most of the funding for trying to understand plasmas, and the other segment which is trying to design practical reactors. Now I find myself merely reiterating what Mr. R. CARRUTHERS has already said.

I, for one, believe that the fact that tokamaks are now pre-eminent in the physics field is probably an historical accident. It so happened that it became possible to heat a plasma effectively by ohmic heating because L.A. ARTSIMOVITCH put a large enough magnetic field on the torus to suppress the MHD instabilities, and this allowed significant currents to be passed through the plasma. If we had developed neutral injection earlier to significant powers, it might have been the stellerator which would now occupy the position of the tokamak. The most important consequence of all the advances in plasma physics which appeared in the early 1970's — and there is no doubt that they were great advances — was that they kindled a new enthusiasm in the field, which resulted in vastly increased funding. But almost all of this enthusiasm has been channelled into the devices which just happened to be the first to show good physics results. There has been remarkably little consideration of the fact that they may not be leading to where we eventually want to go, which is to an economic fusion reactor. Even on the alternative concepts most of the discussions we have are on problems of basic physics and we talk of "scientific feasibility" as though it were an end in itself, but unless in each case someone has at least tried to draw a reactor based on that concept and asked the simple questions: "Could it be engineered? Could it be taken apart? How much is it likely to cost?, only then is it reasonable for this concept to qualify as an approach which deserves discussion and further research.

I think that we should be aware of this now. We have before us this list of the many interesting things we have discussed this week, and we are still arguing mainly about points of physics, but the real issue in each case is: "Could you design a device which could actually be built, which could be relied on, and which would last long enough to pay back, in its output, for the investment in money (or to use better units, the energy) that has been put into making it?"

I'm afraid I have digressed from my topic, which was supposed to be "linus". The philosophy of linus has been determined entirely by the end-product: the aim is to try and design a simple, compact reactor that might actually work in the engineering sense, and then examine the plasma conditions that will be necessary. I do not think we should expect to get all the physics done first, and then worry about the engineering.

CHEN

I think the spirit of all the presentations we have had at this meeting has been along that same line. None of these concepts was brought up simply because of the physics. They were concepts that were purported to have advantages in the engineering, and so I think that we all agree with you. Radi SUDAN would like to talk now.

SUDAN

I agree with ROBSON but as in everything I agree almost with him — not all the way. The issue of why we do physics in a particular decade, why we get concerned with technology really is because the level of funding required to move in technology is always an order of magnitude larger than the level of funding we need to do physics. And if, prematurely, *on the basis of insufficient physics*, you move into technology, you run the chance of a greater debacle than if you were to spend the same amount of money in physics. You would then, at most, be charged with the fact that you have explored more alternatives but you haven't yet reached the level where you can decide which one really can become a reactor.
So I think it was not because we wanted to have a good time that that decade was spent in physics. I think the times demanded that we knew more about what we were talking about.

I now come to add a few words to what R. LINFORD summarized on the role of particle rings in a compact torus. Particle rings — this has been one scheme, of which the community at large has been the most skeptical. Of all the schemes put forward there has been least support for particle rings, ASTRON[12]. In the early days there was only one laboratory — Livermore — one group, that of N.C. CHRISTOFILOS, which developed the ASTRON concept. Mirrors and toroidal machines, on the other hand, started in many places, and flourished. The ASTRON idea came somewhat before its time. Many good ideas are still-born unless there is a technology that is able to deliver them. And in the case of ASTRON, it was not until the arrival of pulsed power technology that even this configuration was feasible. So, had pulsed power technology been available in the early 1950s, particle rings and ASTRON would have had a better start. After seven or eight years of long effort by N.C. CHRISTOFILOS and expenditure of money there were no real results, and that is why this idea faded out of the general community. To resuscitate it now has been very difficult in the United States. But, fortunately, we have reached a point where these configurations exist[13], are stable, can be translated, but we have not reached the stage where there is any data on plasma confinement. Therefore, at this level, despite the fact that we would like to design a reactor, I would hesitate at this point to extrapolate the particle ring all by itself to a fully-fledged reactor design, because there are a few gaps in our scientific know-

[12] N.C. CHRISTOFILOS, in 2nd U.N. Conf. on *Peaceful Uses of Atomic Energy*, United Nations, Geneva, 32/279 (1958). See also Phys. Fluids, Vol. 9 (1966) 1425.

[13] *Particle Ring Fusion* by R.N. SUDAN, in present proceedings.

ledge. It would be like asking someone to design a suit of armour for a young child to go to war 18 years from now. There is no idea how this child will develop.

There are other sister concepts like the field-reversed theta-pinch and the compact torus which have some deficiencies with respect to stability and heating where these particle rings can first find application. In other words, let us take the design offered by George VLASES about the TRACT system. It would be quite conceivable to have that burning theta-pinch being fed by particle rings which feed both flux and energy to a quasi-steady burning plasma. That scenario would be possible and because some fraction of the azimuthal current will be carried by axis encircling particles, stability against tilting etc. would be improved. So in that sense I wanted to couple particle rings with compact tori and the field-reversed theta-pinch rather than call it a separate path that takes us all the way to the final reactor.

LINFORD

I have a couple of comments to make on Dr. R. CARRUTHERS' statements. In his talk this morning[14], he indicated that there were certain goals that we should be shooting for. For example, we should be looking for a $\beta B^2 = 5\ T^2$ (where T means tesla). I think that this goal can be easily achieved with the field-reversed configurations that we are discussing. The aspect of the integrated life — the 50 MW years per m^2 problem — can be largely solved by the objects proposed in George VLASES's talk, viz. the fact that the geometry is so simple that the first wall can be removed and changed very quickly in comparison with that of a tokamak. I don't think we need the seven-year life-time of the wall that was mentioned. Maybe someone would like to comment on this.

The need for 15 to 25 MW per m^2 on the first wall is a more difficult problem. The figure depends to some degree on the physical size of this system and it was correctly pointed out that if we have a toroidal system, then because of the size of the blanket and the shield, we end up with a certain volume.

In the case of the linear system that can be used in a field-reversed configuration, that physical volume can be reduced significantly, which lowers this particular figure. The total amount of energy that the plasma would produce is also reduced by the fact that you can reduce the physical size of the entity.

One last point. I would like to re-emphasize what Bertie ROBSON indicated in the case of a liner — a liquid lithium wall — where the wall loading can go up significantly over the 1-2 MW per m^2.

CHEN

To a large extent, the alternate concepts have been driven by the engineering considerations (and field-reversed configurations, for instance, satisfy these requirements a lot better than others) at the expense of physics; and I think the community have come around to where they are willing to give up some difficul-

[14] *Criteria for the Assessment of Reactor Potential* by R. CARRUTHERS, in present proceedings.

ties in physics, or rather they accept some difficulties in physics, in order to provide an object which is more suitable for the engineering. Now the one that seems to me to be the most simple — the easiest concept to make into a reactor — is the "dense Z-pinch". And yet that's the one that has the least work on it and practically nobody ever talks about it, which is why I would like to ask Charles HARTMAN[15] why people do not take that seriously.

HARTMAN

There is one fairly obvious criterion that has been established for alternative approaches here: many unconventional approaches are not sufficiently unconventional to avoid properly the first wall. One can ask also to extend it to other criteria and for example say that any unconventional approach which is significantly better than tokamak should be able to overtake, in a rather rapid time-scale, conventional approaches. I think that differences do arise in that category and on both counts one should look a little more carefully at them. The gas-imbedded Z-pinch at LASNL we heard discussed by R. LINFORD[16] earlier in this week is very interesting in this context. Experiments have been done and calculations suggest in the next experiment one could conceivably have a 6 keV plasma with density ions of the order of 10^{20} per cm^3 lasting of the order of 10-100 nanoseconds.

It really would be an enormously significant accomplishment if this could be done. It is illustrative of what was recognized very early in the fusion program — the potency of dense Z-pinches — they come in small sizes and they are very concentrated points of energy which can reach quite large plasma parameters with very modest facilities. The other aspect, particularly of the gas-imbedded Z-pinch, is when working at perhaps 10-100 atmospheres, you are working with pressure vessels and not vacuum vessels. You are working with systems that can almost not be contaminated because of the high density of gas surrounding them and which therefore can exist close to liquid lithium or whatever you want, so that it seems almost a natural to avoid the vacuum-interface, the first wall problem.

Of course, the obvious, and one great problem with the Z-pinch is that it is violently unstable, at least in some circumstances. The plasma focus seems to exhibit some rather prolonged stability compared with what one would naively calculate from MHD stability. In other configurations this stability might be extended still further. I am thinking in particular of experiments done by John MARSHALL and Allen NEWTON at Los Alamos — where they crowbarred a conventional Marshall gun thereby creating a pinch hanging off the central electrode which lasted some hundred microseconds. It tended to jiggle a little

[15] *Summary of U.S. Compact Torus Experiments* and *Fusion Reactor Aspects of the Compact Torus* by C.W. HARTMAN, in present proceedings.

[16] Ibid. [10].

but generally remained stable — it was really remarkable — here again a different type of Z-pinch. So I think that the question of stability, which everyone can read about in a standard textbook, is really an oversimplification of the great possibilities even this one basic and fundamental plasma confinement configuration can exhibit.

One must certainly realize that the potential for strong instability exists in the dense Z-MHD-pinch, but also that there are possibilities of overcoming it. Two aspects of the gas-imbedded Z-pinch emerge, if I am not going into too much detail; the surrounding gas can be made to diminish the growth rate of the ordinary kink and this has been observed in the gas imbedded Z-pinches. In spite of kinking, the pinch tends to retain its integrity as a tightly-pinched column.

Further, it is observed that kinking tends to form a helix, and the evolution of this helix appears to be progressing towards a sort of force-free configuration which one might expect to slow down further kinking and so forth — something that only experiments can really answer. As I pointed out, it is not really terribly difficult to investigate these questions. An advantage as far as transport scaling is concerned is that a dense Z-pinch tends to operate at such a high density that anomalous transport is almost inconsequential — at least, conventional transport, in the sense of BOHM scaling. If you can actually succeed in releasing the fusion energy in the order of say hundreds of sound transits, then BOHM diffusion is small.

For reactor designs, as I mentioned before, there does exist the possibility of encapsulating or creating this pinch within some sort of a blast confinement system, because typically the energy release is going to be rather high. A scheme like that was discussed in the context of laser fusion quite some time ago (BLASCON), and has been applied to the Z-pinch reactor. It leads, at least in the very preliminary sense, to an extremely simple pressure vessel, just a big tank with a vortex of lithium in it, and possibly helium bubbles to damp shock waves. It also leads to a rather high average power density system, tens of MW per m^2 wall power and tens of W per cm^3 of the reactor vessel volume. Probably a disadvantage in the sense of a reactor is that a very high density Z-pinch tends to be small, so that you have to worry about raising the repetition rate or perhaps, if the vessel is so cheap, modularizing it.

CHEN

It is about time that we finished this session. Professor SUDAN thinks that there is a better way to illustrate these concepts than we have done, and I would like to ask him to write his grouping on the board and explain it.

SUDAN

There is nothing really to explain in the list presented (see TABLE II). I just put together those concepts that appeared naturally related. The Z-pinches and high

TABLE II — SUDAN'S CLASSIFICATION SCHEME OF UNCONVENTIONAL APPROACHES TO FUSION

1. Field-Reversed Z-Pinch
   Extrap
2. Dense Z-Pinch
   Plasma Focus
3. Multipoles
   Surmak
   Intrap
4. Field-Reversed Theta-Pinch
   Compact Toroids
   Spheromak
   Particle Rings
   Linus
   TRACT
5. Advanced-Fuel Tokamaks
6. Long, linear Systems

density plasma focus have probably similar sorts of problems when it comes to reactors. In the theta-pinch, spheromak and particle rings, the magnetic configuration is the same. It is simply the nature of particle orbits and whether you have a toroidal field or not that differentiate them. Bertie ROBSON needs some magnetic configuration like this inside his liners. This could lead to a TRACT system or a liner system or what have you, provided the plasma physics is optimized among these three configurations. Finally the advanced-fuel tokamaks stand on their own.

The transcription of this Round Table from the recorded tapes and the resulting editing work have been performed by J.P. CARNIHAN and G.G. LEOTTA. The texts of the discussions contributed by B. BRANDT, B. BRUNELLI, R. CARRUTHERS, F. CHEN, B. COPPI, CH. HARTMAN, B. LEHNERT, R. LINFORD, M. OKABAYASHI, A. ROBSON, R. SUDAN and G.C. VLASES have been revised by their respective authors. It is obvious that these talks cannot be on the same level as the written lectures, to which the reader is referred for a deeper knowledge of each subject.