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SCIENTIFIC AND TECHNOLOGICAL OPTIONS ASSESSMENT · EUROPEAN PARLIAMENT

CRITERIA FOR THE ASSESSMENT OF EUROPEAN FUSION RESEARCH

(STOA FUSION PROJECT)

VOL. I

MAY 1988

EP–STOA–F1

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EUROPEAN PARLIAMENT SCIENTIFIC

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P R O J E C T

(S T O A)

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The STOA Supervisory Panel authorises publication of this report. The views expressed in this report are not necessarily those of the European Parliament, or the Panel or individual members thereof.

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* F = France, D = Federal Republic of Germany, UK = United Kingdom, LDR = Liberal and Democratic Reformist Group, PPE = Group of the European Peoples Party, S = Socialist Group, ED = European Democratic Group

STOA

S T O A FUSION

PROJECT

VOL I AN APPRAISAL OF THE

EUROPEAN THERMONUCLEAR

FUSION PROGRAME

Reference European Parliament STOA Project: STOA Fusion Project,Vol. I

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STOA Fusion Workshop

JET Joint Undertaking, 12/13 November 1987

List of persons present	Affiliation
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J McMahon	JET
M L Watkins	JET
P Taylor	PERG, Oxford
C Degenhardt	Futures Magazine

CONTENTS OF VOL. 1

INTRODUCTION

Background to the STOA Investigation

On 26 March 1987 the STOA Project (Scientific and Technological Options Assessment) was formally launched at a meeting of the European Parliament's Committee on Energy, Research and Technology in Brussels.

One of the three subjects chosen for investigation during this pilot phase of the project was entitled: 'Criteria for the Assessment of European Fusion Research'. The reasons for this choice, and the major lines of inquiry to be developed, were outlined in the STOA Monthly Newsletter Launch Issue of March 1987:

> "Background and objective
>
> The importance of fusion research within the overall European Community energy research effort is indicated by the fact that JET and the General Programme together consume about 50 per cent of the total EC energy research budget. If NET (the Next European Torus) is approved this figure will surely rise. Clearly the NET decision, when it comes, will be a major one in budgetary and political terms. It is an issue on which the European Parliament will undoubtedly be asked to give its opinion within the next few years. Therefore there is a need now to start preparing for this debate by attempting to identify salient criteria for the assessment of European research into controlled thermonuclear fusion."

It was noted that reviews of the European Fusion Programme had tended to concentrate on the scientific success or otherwise of the programme - "not surprisingly, given that the current phase is designed to establish the scientific feasibility of controlled thermonuclear fusion". It was decided to broaden the STOA Fusion Project to include other important features:

> "Of equal importance, however, will be a call for evidence on the technological and commercial parameters of the programme, with particular reference to the opportunity cost of the research programme in terms of other possible energy research investment. It is not too early to ask for expert opinions on the technological, commercial and environmental safety aspects of thermonuclear fusion: it is a characteristic of modern complex technologies that effective Parliamentary control is often rendered extremely difficult because it comes too late in the day. Commitments of time, money, infrastructure and expertise can build up an irresistible momentum. The most

fundamental question to be explored by the STOA study will be: "What criteria shall be used to judge the success of the European fusion research programme?" "

The Sweet Study

STOA, recognising that it did not have the resources to conduct a full appraisal of the European Fusion Research Programme, commissioned a study aimed at identifying the "salient criteria" for the future assessment of the Programme. This study has been carried out by Mr Colin SWEET (et al) acting as Consultant to the Centre for Energy Studies, Southbank Polytechnic, London. During the course of this study, the STOA Fusion Project organised the STOA Fusion Workshop on 12/13 November 1987 at the JET Joint Undertaking in Oxfordshire in the UK, so that the actors involved in promoting the Fusion Programme could exchange ideas with independent experts and critics in the presence of Members of the European Parliament. Chapter Five of the Sweet Study was commissioned by STOA from Earth Resources Research.

At the Workshop Mr SWEET presented an interim version of his study. This has now been extensively revised in the light of the Workshop discussions. The study is therefore now in its final form. It must be made clear that the study does not represent the opinion of the European Parliament or of its STOA Project. The study has been commissioned and drafted as a contribution to public reflection and debate on this important matter, and comments upon it will be welcomed.

Part of the purpose of the STOA investigation into fusion research was to experiment in the methodology of parliamentary technology assessment.

Methodological Considerations

In an article written in 1970[1], before the establishment of the US Congress Office of Technology Assessment, Harold P. Green noted that:

"Most public discussion upon technology assessment to date has ignored a fundamental point. There is never any lack of articulation of the benefits of a technology. Every technology has powerful vested interests - private and frequently governmental and political - who can be relied upon to press the benefits to the technology assessors. The problem is that the negative factors and the risks are never fully or even adequately articulated. (....)

---

1 Harold P. Green: "The Adversary Process in Technology Assessment", Technology and Society March 1970.

These consideration lead me to the conclusion that what is needed for the technology assessment function is an agency which would act as a responsible devil's advocate or technological ombudsman and play for the role of adversary in the Congressional and public forums..."

Green believed that this kind of perspective was particularly important when dealing with government funded or sponsored technological developments:

"These technologies develop with government investment which is in no way related to the forces of the market place (....) government supports technology development merely because desirable benefits are foreseen even though there are no market incentives ... none of the restraints and deterrents which are present with respect to privately developed technologies are operative in this case."

Whilst not necessarily endorsing such an adversarial perspective, STOA does support the idea of critical, open debate on such issues.

Format of the Present Document (Vols. 1 and 2)

The present document is in two volumes. Volume 1 is the Sweet study. In Volume 2 readers will find the European Parliament's Resolution of 10 March 1988 on the 1987-91 fusion research programme[2] and the report for the Committee on Energy, Research and Technology by Alman Metten, MEP, on which the resolution was based.[3]. This isfollowed by a revised version of the Background Briefing first compiled in March 1988 and circulated to Members of the European Parliament at the time of the debate on the Metten Report in Strasbourg. This contains additional material arising mainly from the Workshop proceedings and studies requested by, or submitted to the STOA Project.

It is intended to publish at a later date a summary volume of the STOA Fusion Project in the nine official languages of the European Community.

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[2] OJ No. C 94, 11.4.88

[3] Doc. No. A2-320/87

AN APPRAISAL OF THE THERMONUCLEAR FUSION PROGRAMME

Prepared for the Scientific and Technological Options Assessment of the European Parliament

by Colin Sweet, Timothy Jackson and James Sweet

Centre for Energy Studies

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EXECUTIVE SUMMARY.

1. Introduction.

Thermonuclear fusion is a technology that offers the prospect of making an important contribution to future energy supplies either directly, or through supplying plutonium to a fast reactor programme. Since the early 1950's, fusion research has sought to secure an energy 'breakeven' (strictly speaking, 'breakeven equivalent') from fusion reactions in Deuterium. The progress made in the JET stage has been such that this is expected to be achieved in 1991. Achieving reactor conditions in a fusion reactor means improving the decisive parameters by a further factor of 5 and that will require the building of another larger reactor - the NET stage, which will become operational around the turn of the century.

Fusion is therefore a science research programme, wholly funded from the public sector. We have sought to assess it as such - a task not helped by its institutional separation from other Community Energy R&D progammes with which, in a full benefit cost study, it has to be compared. Market related tests are closely linked to technical feasibility, and both share the problem of uncertainty.

Within the EEC, fusion is the only fully integrated programme, engaging all of the member states of the Community (and two non-member states). Internationally it has achieved a high level of co-operation and a design study for an internationally funded R&D programme involving the USA, Japan, the USSR and the EEC is currently taking place.

A consensus of current thinking is that fusion power is not likely to enter the commercial phase before the middle of the next century. Bringing it to the market involves institutional and political problems which it would premature to speculate upon. However, the STOA project has commissioned this research study with a view to clarifying the criteria that are appropriate for appraising the fusion project. We have accordingly used analytical methods which are generally applied to public sector funding, and which are consistent with Community policies of deepening the internal market, transparent pricing, etc (set out in "The European energy policy, Jan 1987").

Specifically we have addressed ourselves to the Commission's own appraisal of the fusion programme and the Proposals it has made to the Council of Ministers and the European Parliament embodied in the articles and documentation in COM(87) 302.

2. Fusion as a Resource.

While fusion is frequently described as a large resource in quantitative terms, the resource requirements for the fusion technology itself do not appear to have been researched adequately. References to the estimated reserves for Deuterium and Lithium, are seen to be quantitatively impressive. However, the data is subject to large margins of error, and not enough research has been done on the logistics or the cost of potential reserves. Similarly, expressions that state that one gram of D-T mixture equals 10,000 litres of oil (CEC 1987C) may be arithmetically correct but they bear no relation to delivered energy values. As such they are inaccurate and misleading. THESE QUANTITATIVE EXPRESSIONS MAY BE INTENDED AS A RATIONALE FOR FUSION POWER, BUT THEY ARE NOT.

If fusion is to be placed in an energy policy context, two sets of qualitative statements are required. Firstly, in terms of the benefits and costs of alternative energy technologies how does fusion rank, ie. what is the opportunity cost of proceeding with fusion research? Secondly, what are the constraints on a fusion programme a), in material terms (minerals, metals, etc) b), social terms (environmental regulations, siting, safety, risk, etc) c), systems requirements d), capital requirements and e), technology transfer? In this short report we can do little more than explore such matters, but what we say points to the need for a full feasibility study.

3. Facing the Future.

Figures S.1 & S.2 give an illustrative view of the possible time/cost requirements, assuming that the results of the scientific research phase justify moving to the Development phase. The transition between these phases is signified in figures S.1 & S.2. The realisation of a breakeven in the plasma will be to initiate further work aiming at system breakeven. This would be the transition phase when it would be appropriate make decisions about the development stage.

4. Framework for Evaluation.

The perceptions that have characterised the science research phase are ceasing to provide an adequate context for the appraisal of fusion power. The expression that fusion should be proceeded with because it "opens a new way to power generation, having a moderate impact on the environment and using a practically inexhaustible fuel justifies...its development" (CEC 1987b) is a normative statement that expresses institutional interests rather than reality. IT IS DESIRABLE TO CREATE A FRAMEWORK WHICH ALLOWS FOR A MORE OBJECTIVE EVALUATION OF FUSION POWER AND WHICH IS CONSISTENT WITH MARKET RELATED ECONOMIC ASSESSMENT AND CHANGING PERCEPTIONS ON ENVIRONMENTAL CRITERIA.

Figure S.1 European Fusion RD&D Costs (1950-2050).

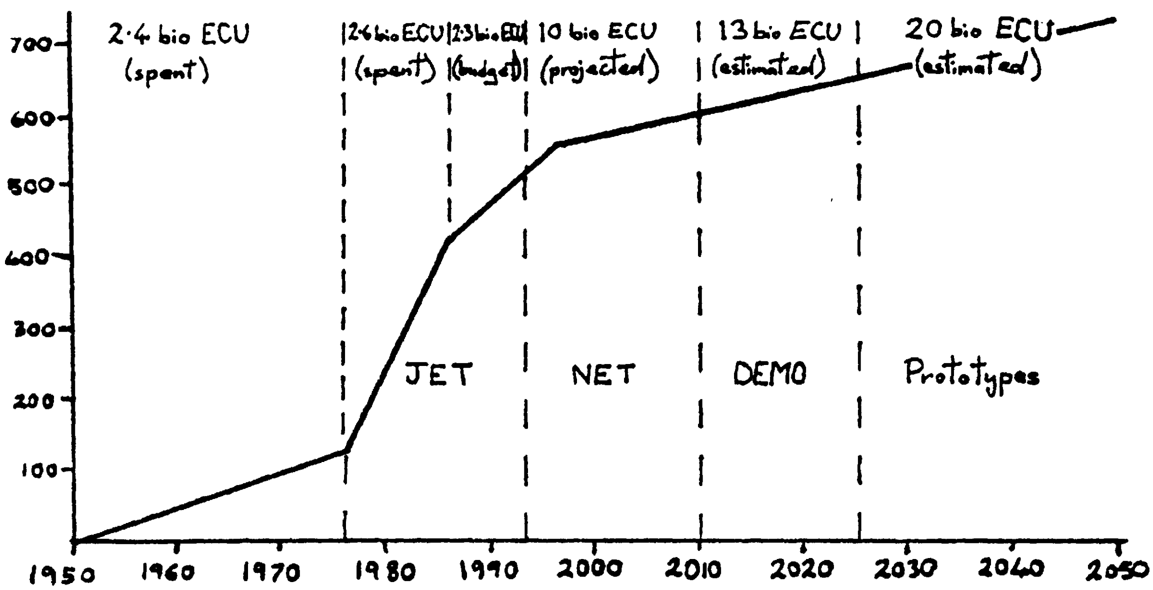
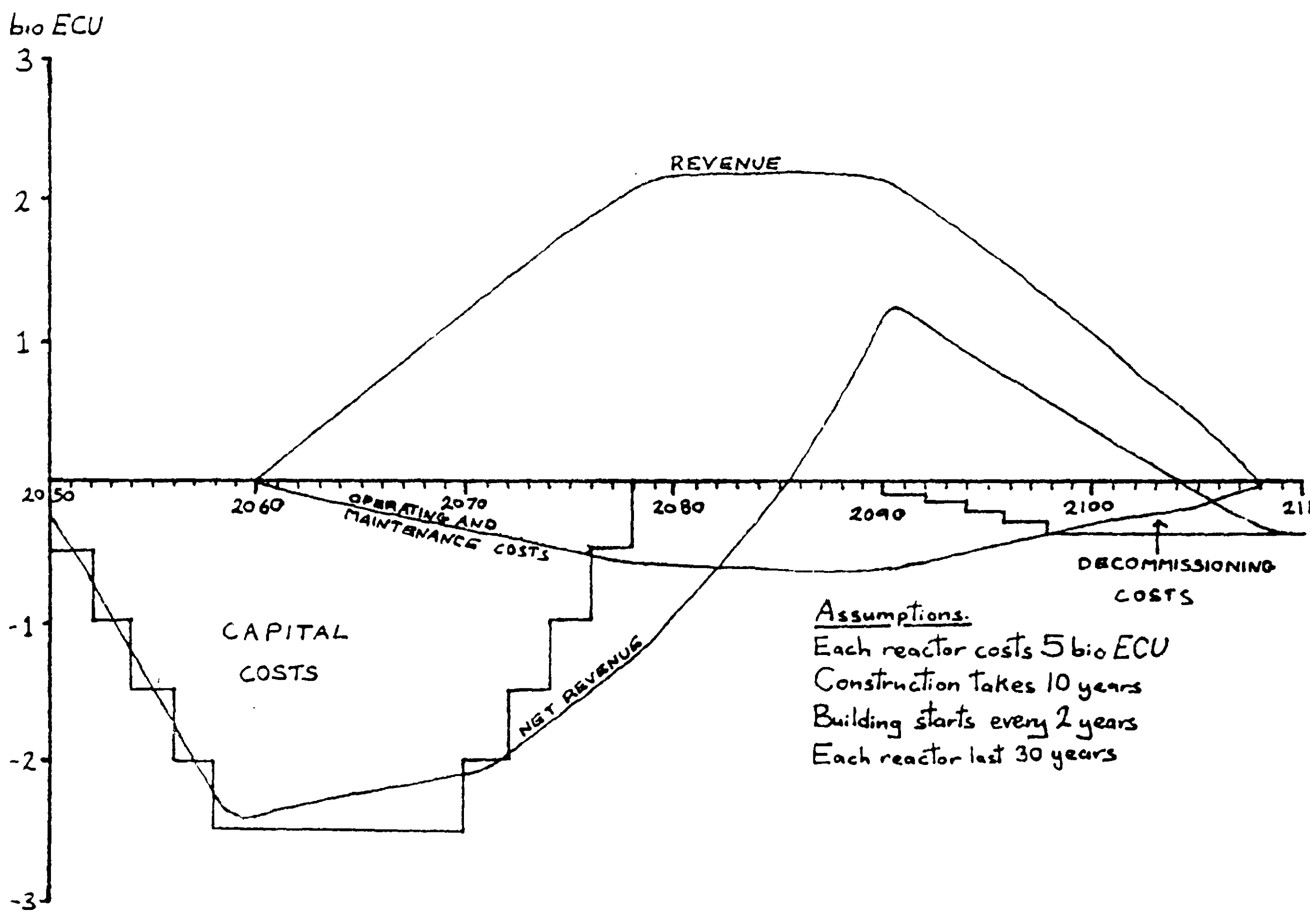


Figure S.2 Commercial Programme Cost Scenario (2050-2110).



5. Economic Assessment.

While it is recognised that no certainty can be attached to any evaluation of fusion power within competitive electricity supply markets of the future, there is nevertheless a need to assess fusion in terms of mainstrean economics. The expert report on fusion economics of (COM(87) 302. Annex) has concentrated on making judgments on the commercial viability of fusion in the mid-21st century. We have found this defective for two reasons. (a) It focusses on the long term but neglects the present. In particular, it does not seek to assess fusion in terms of the Community's R&D priorities. (b) The methods used to arrive at the conclusions are not consistent with those used in public sector analysis. They do not use statistical techniques necessary in handling data characterised by a high degree of uncertainty, and their use of theory and method can only be described as idiosyncratic. WE BELIEVE THAT THE STUDY OF THE EXPERT GROUP ON THE ECONOMICS OF FUSION SHOULD BE PUT TO ONE SIDE, OR SUBMITTED FOR A SECOND OPINION.

WE DECIDED, DESPITE OUR RESRVATIONS, AS THE ONLY SOURCE OF DATA WAS FROM THOSE WORKING IN FUSION RESEARCH, THAT WE SHOULD TEST THE DATA IN A SENSITIVITY TEST. WE CONCLUDED THAT FUSION COMPARES POORLY WITH OTHER FORMS OF ELECTRICITY GENERATION. ON PRESENT EVIDENCE FUSION HAS LITTLE PROSPECT OF BEING COMPETITIVE.

WE DO HOWEVER EMPHASISE THAT SUCH JUDGEMENTS CAN BE NO BETTER THAN THE DATA THEY USE AND THE SOUNDNESS OF THE ASSUMPTIONS THAT ARE MADE. A FULL AND PROPER ECONOMIC EVALUATION IS OVERDUE. WE RECOMMEND THAT THE REQUIREMENT TO DO THIS IS ATTACHED TO THE NEXT FUNDING PROVISION.

6. Is Fusion Feasible?

We have sought to understand the present stage of technical progress. The use of the term 'Break-even' as defining the present programme to achieve an energy balance in the Hydrogen-Deuterium plasma reaction is open to misunderstanding. IN OUR VIEW 'BREAK-EVEN' SHOULD BE USED AS DESCRIPTIVE OF THE STAGE WHEN THERE IS AN ENERGY BREAKEVEN IN THE SYSTEM AS A WHOLE. IT IS THIS ACHIEVEMENT WHICH WILL OPEN THE WAY FOR FUSION POWER TO BE USED FOR ELECTRICITY GENERATION. Scientific feasibility, as that term is currently used falls substantially short of 'break-even' in the sense that we have used it.

7. Engineering.

The developmental stage that lies ahead involves engineering problems of great complexity. We seek to understand what are the likely costs, trade-offs and time scales. We do not share the view that is to be found in COM(87) 302, that there will be massive fall in costs with the first series in the post demonstration programme. The evidence, if anything, points the other way. We look at the experience of other industries including nuclear fission. If anything, the fusion reactor is less well placed. Its great mass, its low power density, and the difficulty of achieving a high availability are serious disadvantages.

8. The Environment.

The study of the environmental aspects suggests that the uncertainties here are greater than have been recognised. The recent research of the UK National Radiological Protection Board has thrown serious doubt on the wisdom of shallow burial for the large amounts of radioactive waste from a fusion programme. The economic, legal and social problems involved in deep disposal are, from present standpoints, problematical. We agree that the hazards of fusion are not be measured in the same terms as those of fission reactors. However, there are considerable concerns about safety and health, and in the light of the changing criteria being used (which are complicated because they vary between states), and the legal aspects of plant siting, we believe that there is undue complaceny about the environmental aspects of fusion. Experience has shown waste disposal to be one of the most intractable problems of the nuclear fuel cycle. WE BELIEVE THAT THE ENVIRONMENTAL ASPECTS SHOULD RECEIVE MORE ATTENTION, AND THAT AN INDEPENDENTLTY BASED STUDY IS REQUIRED. WE SUGGEST THAT THIS SHOULD BE UNDERTAKEN IN TIME FOR THE NEXT PROGRESS REVIEW.

9. Management.

We suggest that the time has come to look at the management strategy and structure. We are principally concerned with the need to take a look at the broad concepts which drive the management process. We make two proposals. One is a conceptual matter, the other a strategic one. They are intended to be seen as closely connected. Firstly, the use of what has been described as the sequential approach should be subjected to thorough discussion and questioning as a basis for management control and programming. The Commission, under the heading "Objectives of the 1987-91 Fusion Programme", say:

> "The way towards fusion reactors for energy generation can be schematically and somewhat arbitrarily divided into three stages: demonstration of scientific feasibility, of technical feasibility and eventually of economic feasibility." (CEC 1987)

This expresses the sequential approach as a management concept (see figure S.3 below). First, solve the science, then the engineering and then the economics will come right. Schmitter and Carruthers, two senior engineers in the fusion programmes disputed this logic in 1976. They described it as too simplified:

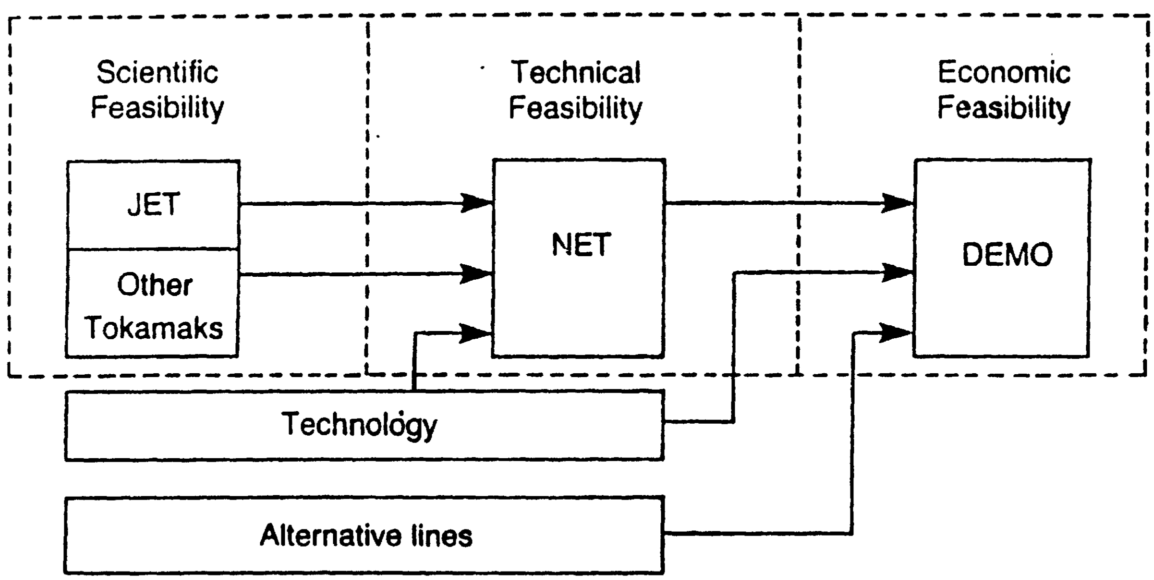
> "It neglects the inevitable interaction of the

three components and the historical fact that the widespread application of new technologies has usually preceded a complete scientific understanding. A second approach is to consider a more interactive approach but still somewhat open ended... A third approach considers the possible role of fusion power in a long term energy strategy."

(Carruthers et al 1976)

WE ENDORSE THE VIEW THAT THE PROJECT MANAGEMENT SHOULD BE BASED ON AN INTEGRATED ASSESSMENT, AND NOT BE OVER-DEPENDENT ON ANY ONE DISCIPLINE.

Figure S.3 **European Fusion Programme Strategy**



presented to the STOA Fusion Workshop by C Maisonnier, Director, Fusion Programme.

The central problem we suggest for project management is that of uncertainty. The sequential approach is not useful in this respect. It has no way of dealing with uncertainty. Modern management methods require an integrated approach which subject all areas of work to critical appraisal, making use of risk analysis, probability assessment, and relating each aspect to the central objective - ie. in this case to the production of a socially useful product (electricity) consistent with the best use of resources. **AS FUSION RESEARCH WILL BE SEEKING FUNDING FOR THE DEVELOPMENTAL STAGE IN THE EARLY 1990's RE-APPRAISAL OF MANAGEMENT SHOULD BE A CENTRAL OBJECTIVE.**

Before the next stage is embarked upon it has been said that there will be a full review. We discuss the method of monitoring the fusion programme as we understand it to have operated in the last decade. It has been mostly concerned with technical matters, and has been characterised by the narrowness of approach that we have referred to in the previous paragraph. While that may have served a useful purpose in the earlier stage, **WE ARE STRONGLY OF THE OPINION THAT THE NEXT REVIEW SHOULD TAKE THE FORM OF A FULL FEASIBILITY STUDY, USING INDEPENDENT EXPERTISE.**

With respect to project management, we note that the Commission's Proposals to the Council and the Parliament appear to arise from the Fusion Directorate. On this we have two comments. Firstly, we question whether the problems fundamental to setting a framework for long-term appraisal can be properly formulated, still less resolved, by a Directorate that is responsible for the day to day admnistration of the fusion programme. Secondly, we ask the question, should the Proposals in COM(87) 302 have been more widely discussed? It is arguable both in terms of the very large funding requirements, and the length of time that it is now becoming apparent before technical feasibility can be established, that the implications of the fusion programme are of major importance for the future of the Community. These implications we discuss later, but the point we wish to emphasise here, is that the scope of the discussion does not appear to have matched the importance of the topic. At the organisational level it does not appear that any other Directorates have been involved in the processes that led to the writing of COM(87) 302. If we are mis-informed on this point we request that the documentation is made available. At the political level we are not aware of any significant public debate. To be meaningful, such a debate would have to be open-ended. The rapporteur for the 1985 Parliamentary debate, Mr B Sältzer, confirmed this when he said "The possibility of abandoning the fusion research should not be precluded." (Saltzer 1985).

For an RD&D programme, that of course must be right. Fusion, however does not appear to be an ordinary RD&D programme. Were it so, we would have not laboured so hard in this study to bring it into line with assessment of RD&D generally.

10. Public Acceptability.

Public acceptability we address last, although in political terms it is possible to take the view that it is the most important test for any technology to pass. At present the public know very little about fusion, and what they do know is often misleading. Such a situation will change, not least because the high cost of taking fusion to the Demonstration stage will demand increasing attention. It is possible, partly for this reason, that the first fusion demonstration plant will be internationally owned and operated. In which case it will be all the more important to create a framework in which accountability can be seen to be operating, within which dialogue can take place, and in which public confidence can grow.

In seeking to subject the fusion programme to critical appraisal we recognise the progress that has been made in fusion research. Our comments are not intended in any way to undervalue or undermine the work that has been done, and is being done. We do however believe that there is a need to embrace more broadly, what must be the central problem for any new advanced technology - namely public acceptablity. A commitment today to a technology for the mid-21st century, is an extremely difficult matter for decision-makers. They have to be sure that what they decide today will accord with the perceptions of society two generations ahead. Perhaps this means that the commitment to the programme should not run ahead of what the present generation feels it is able to support. That is why we have endeavoured to search for options rather than commitment to one solution. AS A MINIMUM WE AGREE THAT RESEARCH IN PLASMA PHYSICS SHOULD BE SUSTAINED AND THAT THE CO-ORDINATED INTERNATIONAL EFFORT WHICH HAS BEEN SO SUCCESSFUL IS THE RIGHT APPROACH FOR THE FUTURE.

WE ALSO RECOMMEND THAT A FULL FEASIBILITY STUDY SHOULD BE SET IN MOTION AS SOON AS POSSIBLE; THAT IT SHOULD BE THOROUGH, INDEPENDENT, AND TREAT ALL ASPECTS OF THE FUSION PROGRAMME WITH EQUAL SERIOUSNESS.

WE SUGGEST THAT THIS SHOULD BE UNDERTAKEN BEFORE ACCEPTANCE OF A DEVELOPMENTAL PROGRAMME IS RECOMMENDED TO THE COMMUNITY.

A third option which has been canvassed is that the fusion programme should ONLY go ahead on an integrated international basis. The attractions of this are not difficult to see. Such a course of action would keep the programme alive but at a reduced cost to the supporting states. As so often with compromises there are hidden problems, and we believe that the first importance should be given to deciding on the criteria by which fusion is to be evaluated in the future, and the reasons for which fusion, as a potential contributor to energy supply, should be proceeded with. If the member states are not agreed about that, an international programme which diffuses responsibility is unlikely to succeed.

RECOMMENDATIONS.

The following recommendations cover the broad areas and correspond to the structure of the report. Each recommendation is given in block capitals, followed by supporting statements from the relevant chapter, which give force to the recommendation. (For reasons of brevity, these statements are not always the same as those referenced in the text. The reader should refer to the relevant chapter to place these statements in their full context). Following the main recommendations are suggested amendments to the draft regulation of the Commission which went before the Council of Ministers and the Parliament.

Chapter One. Introduction.

THE POLICY WHICH GUIDES FUSION RESEARCH OUGHT TO BE SEEN AS CONSISTENT WITH ENERGY POLICY AND RESEARCH POLICY AS SET OUT IN THE COMMUNITY'S DOCUMENTS. ITS RANKING IN TERMS OF RD&D FUNDING OUGHT TO BE CONSISTENT WITH THE COMMUNITY'S ENERGY AND RESEARCH PRIORITIES, AND CONSISTENT WITH THE EXPECTED REAL RATE OF RETURN THAT INVESTMENT IN AN RD&D PROGRAMME IS EXPECTED TO REALISE.

> "Given that fusion research takes nearly one half of the Community's energy research budget, we find this separation of expenditure from policy surprising. We believe that if a proper level of accountability is to be realised that this separation ought to be rectified."
>
> (1-1)

Chapter Two. The Framework for Appraising Fusion Power.

A FRAMEWWORK FOR THE EVALUATION OF FUSION POWER IS NECESSARY. IT OUGHT TO REPLACE THE GENERALISED STATEMENTS ABOUT THE INHERENT ADVANTAGES OF FUSION AS A POTENTIAL ENERGY SOURCE WITH A SET OF CRITERIA WHICH ARE CONSISTENT WITH MARKET RELATED ECONOMIC ASSESSMENTS AND THE CHANGING PERCEPTIONS ON ENVIRONMENTAL PROTECTION. UNTIL A FULL APPRAISAL IS MADE, BASED ON RECOGNISABLE (ie. Opportunity Cost) PRINCIPLES NO LONG-TERM DECISION SHOULD BE TAKEN.

> "Correcting this lack of a recognisable framework, is we believe more important as an objective, than any other matter at this juncture."
>
> (1-2)

> "The central problem is dealing with uncertainty in the future. This means that there ought always to be a number of answers to any given question about technical options, time or cost... Because decision-making on matters which affect future generations can only be taken at the political level, then decision-makers ought to be presented

with options."

(2-1)

"We proceed from the view that there are no imperatives and that at any given time there are a variety of scenarios available, all of which may be plausible. Therefore, as with other energy systems, the merits of fusion can only be presented to the public in terms of the of the benefits foregone as a result of not choosing one of the alternatives."

(2-1)

"The decision-maker is given only one choice - to accept the programme (with perhaps some minor modifications in the distribution of funds between centres, etc), or to reject it. This is precisely the opposite of what we would have expected in a major funding programme for a long-term technology."

(2-2)

"There are suggestions in the Commission's documentation that fusion is more attractive than nuclear fission because it is less of a safety or an environmental hazard. We understand this to mean that the environmental benefits are sufficient in themselves to make fusion attractive as compared with fusion. If this is so, we would have liked to have understood the force of this argument more fully, given the public's concern for nuclear hazards. This is an important consideration. Unfortunately no such study appears to have been made for the European programme...."

(2-2)

"By any comparable standards, the total cost before the commercial stage is going to be far greater than for any previous technology, including nuclear fission."

(2-3)

"If an assessment in the context of an RD&D programme (and based on opportunity cost principles) is not available, then progress from the research stage to the development stage ought not to go ahead."

(2-7)

"(1) Ranking technologies in terms of the conventional criteria, fusion is speculative rather than technically feasible and therefore options ranging between a minimum and a high level of funding will have to be set out, as in the OTA report.

"(2) Trying to guess the future we recognise that a large rise in electricity demand (and implicitly the real cost of energy) could bring fusion from a backstop technology status to that of a marginal cost producer. We think however that forecasts of a large rise in electricity demand need to be documented better than they have been.

"(3) Because of its exceptionally long period of development and demonstration, and the long lead times for construction of fusion power stations, in market terms this will prove to be an inflexible technology, especially if demand management becomes more sensitive to consumer choice and market prices. Fusion will be favoured by an economic environment in which long-term RD&D programmes can be funded against the prospect of a predictable rise in energy demand and planned energy growth in the public sector."

(2-13)

Chapter Three. Scientific Feasibility.

THE TERM 'BREAK-EVEN' AS A MEASURE OF PROGRESS IN FUSION RESEARCH IS OPEN TO DIFFERENT INTERPRETATIONS. WE RECOMMEND THE USE OF THE TERM 'SYSTEM BREAKEVEN', AS THIS IS THE CONDITION MOST RELEVENT TO THE AIMS OF THE RESEARCH PROGRAM (NAMELY, TO PROVIDE A POWER PRODUCING REACTOR). WE SUGGEST THAT PROGRESS TOWARDS SCIENTIFIC FEASIBILITY BE ASSESSED IN RELATION TO A SYSTEM BREAK-EVEN CONDITION.

"In our view it is essential that the power of socio-economic criteria as the final arbiters must be acknowledged by a continuous assessment of scientific and engineering progress in social, environmental and economic terms."

(3-3)

"An unrealistic definition of scientific feasibility can lead to an underestimation of the scope, and even the nature, of the engineering problems still to be tackled, and this in turn will obscure the economic and environmental constraints that the program imposes."

(3-3)

"...we take the primary aim of the fusion program to be: to provide a (comparatively) safe, economic, and environmentally acceptable source of electrical power from the use of controlled thermonuclear reactions in a plasma."

(3-4)

"If criteria of scientific feasibiliity for the nuclear fusion program are to be adequate in the sense outlined above, then they must express, not arbitrary staging posts in plasma physics, but the feasibility of attaining scientific landmarks directly related to the declared aim of achieving a useful power source."

(3-4)

"...unless the demands of the technological programme are fully expressed by the scientific criteria, certain implicit scientific demands may become neglected at a very fundamental level, leading to very conisiderable waste of resources through the pursuance of unrealistic or unrealisable goals."

(3-5)

"Neither of the criteria currently advanced as representative of a demonstration of scientific feasibility is in itself adequate to the task, because neither of them takes into account all the power losses relevant to a power producing reactor."

(3-9)

"In our view the correct scientific criterion must dominate the programme from the earliest stages. The danger of not doing this could be that the entire programme is dedicated to pursuing performance parameters which are simply not relevant to the eventual goal. The result of doing this could, in the very worst scenario be the enormous waste of resouces on a program that is simply not scientifically feasible."

(3-12)

Chapter Four. Engineering Feasibility and the Fusion Reactor.

THE DEVELOPMENTAL STAGE WILL BE MARKED BY COMPLEX ENGINEERING PROBLEMS. MANY COSTS ARE NOT YET REVEALED. THE TRADE-OFFS WILL BE INCREASINGLY COST CENTRED. WE DO NOT SHARE THE VIEW THAT THERE WILL BE A RAPID FALL IN CAPITAL COSTS AFTER THE DEVELOPMENTAL STAGE, AND WE ARE NOT CONVINCED THAT THERE WILL BE ANY FALL. WE RECOMMEND THAT THIS KEY PARAMETER IS RECONSIDERED IN THE CONTEXT OF A DETAILED STUDY OF THE DEVELOPMENTAL STAGE.

Three main areas of engineering constraints can be discerned. Firstly, the pure engineering problems. Secondly, the environmental problems such as materials activation and waste, routine releases and accident potential. Thirdly, costs which can also seen to be involved in the already existing trade-offs between engineering and the environment.

(4-2)

There are at least two differing views of what fusion power generation will offer, which may serve as criteria for judging its acceptability. One, the chief benefits are not economic, but in environmental safety. Two, that fusion power does offer cheap electric power and the other benefits are secondary. In the first view fusion power would not be acceptable if it failed to be cleaner than a fast reactor programme. In the second, it would be unacceptable if it failed to be cheaper than existing alternatives.

(4-2)

"In fact it is quite possible that the wall life would be less than two years even with the optimistic data...

"...the choice of design limit can only change an impossible situation (wall life is less than two months) into a difficult one (wall life is of only a few years), depending on the assumptions of tolerable ductility."

(4-7 (IIASA 1977))

Chapter Five. Safety and Environmental Aspects of Fusion Power.

WHILE FUSION DOES NOT REPRESENT THE SAME RISKS TO HEALTH AND SAFETY OR TO THE ENVIROMENT AS FISSION, IT DOES PRODUCE A LARGE AMOUNT OF RADIOACTIVE MATERIAL. A GREAT DEAL MORE ATTENTION NEEDS TO BE GIVEN TO ASSESSING THE RISKS AND COSTS INVOLVED. THE REPORT OF THE NRPB (NRPB 1987) ON RADIOLOGICAL ASPECTS OF FUSION SUGGESTS THAT THE PREVIOUSLY HELD VIEW THAT SHALLOW DISPOSAL WAS ADEQUATE HAS BEEN MISTAKEN. WE RECOMMEND THAT A FULL ENVIRONMENTAL STUDY IS UNDERTAKEN. INDEPENDENT ASSESSMENT OF FUSION REACTORS, INCLUDING SAFETY, WASTE DISPOSAL AND HEALTH RISKS TO THE PUBLIC AND THE WORK FORCE SHOULD BE COVERED.

"INTOR estimates of total tritium inventory in a modern plant vary from 2.5 to 3.9Kg; the 4000MW (thermal) Starfire reactor would possess a tritium inventory of 10Kg... There is therefore insufficient information to make any judgement about possible maximum tritium releases from accidents associated with the DEMO reactor as currently designed."

(5-6,7)

"Irradiation of the structural materials inside the reactor leads to the build-up of radioactive isotopes. At the end of their life they will continue to give off heat and require active cooling for some years, and in the longer term, even when acceptably cool will continue to be radioactive."

(5-9)

"There is little meaningful conception, as yet, of the degree to which the plasma will be containable under conditions of net energy gain, and thus the conditions that the first wall will have to withstand..."

(5-9)

"...Transmutations in certain materials involve the production of long lived isotopes. This is only one consideration among many with respect to the choice of materials from which to construct the first wall and blanket of a commercial fusion reactor."

(5-10)

"The UK NRPB has studied five alternative first wall and blanket structural materials and concluded that all would still be classified as intermediate level waste 100 years after removal from the reactor. The EEC funding application on the other hand writes simply of the 'non-existence of important long-term (>100 years) potential hazards.' It also assumes that shallow burial will be acceptable."

(5-12)

"The decommissioning of fusion facilities with their more complicated reactor arrangement and hot cell complex and above all their very substantial inventory of high level waste, presents as yet an unassessable situation."

(5-14)

"Given current problems in finding sufficient disposal sites for the relatively small quantities of intermediate level waste from existing fission power facilities, it is difficult to envisage where the massive radioactive arisings from a major fusion economy would be housed."

(5-14)

"The greatest hazard lies in the use of lithium... If this material is exposed to water coolant or through a breach of the reactor vessel to air or other substances with which it will react, it will burn to an intense heat, initiating further accident events and itself releasing the tritiym contained in the blanket. If such a sequence is associated with a breach of containment, then a chemical fire which releases a significant proportion of the radioactive inventory to the atmosphere on a scale comparable to that at Chernobyl can be envisaged."

(5-15)

"In their study of solid waste management for fusion reactors, the UK NRPB concluded that only deep ocean disposal would present an acceptably low hazard."

(5-19)

"The radiological consequences from an assumed maximum release from a fusion reactor are substantially less than those which would result from a worst light water accident."

(5-20)

"We would recommend that any further funding of fusion power technologies should be accompanied by funding for structured probabilistic analysis... They need to be carried out by organisations which do not themselves have a direct interest in the success of the project..."

(5-22)

"...the environmental assessment of fusion compared with alternative technologies must be carried out by organisations independent of the interested research organisations."

(5-23)

Chapter Six. The Economics of Fusion as a Resource.

THE ECONOMIC ANALYSIS TO BE FOUND IN THE ANNEX OF COM(87) 302 IS INADEQUATE. WE RECOMMEND THAT IT SHOULD BE PUT TO ONE SIDE AND NOT USED AS A BASIS FOR PROJECTING THE FUTURE OF FUSION POWER. NEITHER IS IT SUITABLE FOR DECISION-MAKING IN THE RD&D PROGRAMME.

It is necessary to place fusion potential in the wider context of the economy. This involves consideration of long-term prices, capital markets, and resources. Specific costs (examined in chapter 7) can only have meaning if the assumptions made in the macro-economic study are both explicit and credible. The Expert group who have modelled specific costs have ignored this requirement.

(6-1)

"...we recommend that the first consideration should be given to basic questions of how the topic of fusion economics ought to be approached."

(6-1)

"The key issue is how to deal with uncertainty. Appraisal of long-term investment in the public sector using well tried methods, ie. setting the discount rate, cost benefit analysis, use of scenarios. The results are usually subjected to sensitivity analysis, and (desirably) probabilities are attached to the results."

(6-1)

"Decision-makers confronted with long-term projects ought not to accept single figure solutions. ...A range of options which include alternatives should be regarded as a minimal way of approaching this problem."

(6-2)

The future of fusion is presented with the implicit or explicit assumption that real energy costs will have risen substantially by the time fusion comes to the market. That is possible. But is it desirable? Higher energy prices are associated with deflationary effects and reduced national income.

(6-3)

The higher cost and risk of fusion suggest that a higher rate of return will be expected than in the economy generally. There is the risk of over-investment in such a scenario, with loss of demand leaving expensive surplus capacity in the system. Both these phenomena - higher prices and surplus capacity - were experienced in the period after 1974 in Europe, with the consumer having to carry the cost. The presentation of fusion as emerging in an economy with high energy demand and high real energy costs has therefore its drawbacks.

(6-3,4)

On the supply side similarly the assumption of rapid growth in nuclear energy is over-simplified. The very rapid increase in the nuclear contribution - from 275 TWh (1983) to 792 TWh (2000) - becoming the market leader in electricity supply, is unrealistic, and on environmental grounds is almost certainly unacceptable.

(6-5)

Because fusion fuel costs are very low, there will be no gains from falling fuel cost. Relative advantage for fusion must depend not on falls in its fuel costs, but on rises in those of coal and fission."

(6-7)

Little is known about the resource costs of fusion. The requirements in terms of large-scale commercial programmes, ie. the trade-offs between engineering design and materials; the environmental spin-offs of the use of Lithium, Tritium and Deuterium; the scale of waste disposal and the time spans involved - all of these are questions that require far more research than has so far been undertaken.

(6-8)

There is, for example, no consensus on the extent of lithium resources. Presently lithium costs around $55/kg. It is estimated that prices will have to rise threefold before adequate reserves would be economically recoverable for large fusion programmes. Estimates of available reserves vary over a wide range. Similarly, estimates of the lithium requirement of fusion reactors are very uncertain. The potential of fusion in resource terms can only therefore be stated in a very wide band. We recommend that some sustained research is undertaken in this field.

(6-8,9)

Current reactor designs studies include scarce metals. Beryllium, tungsten and vanadium in particular. One US study shows that these materials constraints could become severe in a 300GW fusion economy, unless more abundant and cheaper materials were found.

(6-11)

"The ultimate economics and acceptability of fusion energy, as with most other energy sources, will depend to a large extent on the limitations of materials for the various components."

(6-11 (US DOE 1987))

The effects of such resource costs will be felt in the fuel cycle. Depending on the assumptions made, the difference could range from 50% of the generation cost given in COM(87) 302 to less than 1%. The low figure arises from assumptions which are unrealistic. In our view it would be unwise to treat fuel costs as negligible.

(6-11,12,13)

Chapter Seven. The Cost Sensitivity of Fusion Power.

WE HAVE TREATED THE DATA IN COM(87) 302 TO SENSITIVITY TESTS AND THE RESULTS SHOW THAT IT COMPARES POORLY WITH ITS NEAR NEIGHBOURS, NUCLEAR FISSION AND COAL. ON PRESENT EVIDENCE FUSION POWER APPEARS TO HAVE LITTLE PROSPECT OF BEING COMPETITIVE. WE RECOMMEND THAT A FULL ECONOMIC EVALUATION IS ESSENTIAL AND THAT THIS REQUIREMENT SHOULD BE ATTACHED TO THE NEXT FUNDING PROVISION.

We are critical of the Commission's Expert Group's report. In particular "to derive specific costs for fusion power from conceptualised designs, using any number of untested assumptions, is the type of forecasting that fell into disrepute some years ago..."

(7-1)

We disagree with the use of energy accounting, and the mix of energy accounting with statistics derived from conventional sources. "A shot gun marriage of two sets of data, both of poor quality and inherently incompatible, is hardly going to last. What it does do is demonstrate the unique detachment of fusion technology from what is happening in the world around it."

(7-1)

Notwithstanding that we disagreed with the methods adopted we subjected the Expert Groups' results to a sensitivity test in order to see how robust they might be.

(7-2)

The economic parameters that we chose in this test where in all cases less favourable to low cost fusion than those used by the Expert Group. This was necessary because the figures used by the Group had been either very optimistic or neglectful of important variables.

(7-2)

"Experience with fusion's closest technological relatives in the thermal fission programme and the fast reactor programmes suggest strongly that capital costs have historically undergone escalation in the face of predicted cost decreases."

(7-7,8)

In particular we did not accept that the capital cost of fusion reactors would fall by a factor of 2-3 in a series of ten. This was based not on experience with fission reactors (where a large data base was not used) but on engineering-driven costs. We are of the opinion that this is far too narrow a base to project a rapid fall in capital costs. Moreover, there are a number of engineering features; low power density, large size, first wall replacement, which exert important limits on cost reduction.

(7-8)

The costs of R&D ought to be recovered. It adds significantly to the cost of fusion power. The cost of decommissioning and R&D could add 50% to the Commission's lowest Starfire estimate.

(7-9)

"It is unlikely that a fusion plant, in the first series at least, could be built for less than $5 billion."

(7-10)

"The Commission's assumption, built into their total costs is 75-80% availability. Is this high figure a reasonable target or is it just an unrealistic assumption?"

(7-10)

"We are of the view that the Commission should not continue with this [availability] assumption and that they should begin serious studies on a full-range scenario study from which a central estimate can be derived."

(7-11)

"Among the key variables affecting cost, we judged availability to be the most sensitive. The Expert Groups' assumption of an availablity of 75% or higher appeared to have no justification, especially for an immature technology. Tested at lower availabilities, the costs rose very rapidly. We are of the opinion that cost sensitivities should be re-appraised, especially in the light of down-time estimates associated with the first wall and blanket replacement times."

(7-11)

"We are of the view that fusion power cost sensitivities should be re-appraised, especially in the light of the down-time estimates associated with first wall and blanket replacement times."

(7-11)

"To assume that fusion construction will reverse the cost trend of fission and fall by at least as much as the other rose can only be described as heroic. It can only be treated as evidence of blind determination to make the case for fusion by asserting what cannot be reasonably demonstrated."

(7-12)

"The notion that a technology can be brought to the market solely by technical improvement, capable of being anticipated several decades ahead in fractions of a cent per kWh, against an assumed background of constant real prices, and that on such a basis forecasters can claim that it will be competitive, can only be described as the triumph of matter over mind."

(7-12)

The application of energy accounting to argue that fuel costs were close to zero over the lifetime of the reactor, appeared to us to be one of the worst cases of special pleading that we have encountered.

(7-13,14)

We concluded that the capital cost of fusion represents a near intractable problem. Only a very high rate of return would attract investors to fusion.

(7-16)

Chapter Eight. Decision-making and Accountability.

WE BELIEVE THAT MANAGEMENT STRATEGY OUGHT TO BE BROUGHT INTO LINE WITH LONG-TERM ENERGY POLICY AND WITH THE ORGANISATION AND ECONOMIC CRITERIA ON WHICH RD&D SPENDING IS BASED.

WE RECOMMEND THAT THE NEXT FUSION REVIEW SHOULD TAKE THE FORM OF A FULL FEASIBILITY STUDY USING INDEPENDENT EXPERTISE, AND COVERING ALL ASPECTS OF THE FUSION PROGRAMME.

The "Fusion Reviews" of 1981 and 1984 confined themselves largely to technical aspects of the programme. Our view is that a more searching appraisal is required before the next stage of the fusion programme is adopted.

(8-1)

This should be a full feasibility study that should aim to provide options for decisions at the political level. It should be broadly based in its approach and conlusions and seek to inform the wider public about fusion power. It should use independent expertise, and at the same time involve all the concerned directorates and committees within the Community structure.

(8-1,2)

For these reasons we believe that a repetition of the previous type of Fusion Review would not be adequate. The exercise should be rigorous, setting all the complex of factors, and defining the objectives that justify different options, and different levels of expenditure, in order that clear decisions can be taken at the political level.

(8-2)

"We therefore ask the question, "Can we wait until $20 billion have been spent before we decide if the money has been well spent, or whether it should be abandoned? We are aware that cancellation at a late stage has been done before in the nuclear programme (eg the US decision to cancel the Clinch River fast reactor project). But we felt that the answer to the question is

"No", and therefore a full appraisal including a look at the proposed economic benefits is one that ought to be undertaken."

(8-3)

"Widening the scope of appraisal should, we suggest, include widening the scope and skills of the fusion management. We are... concerned to draw attention only to the mismatch between the very complex issues that fusion as a technology of the future involves, and the present management structure. We believe that to get the right balance here, is as every bit as important as the more technical aspects of the appraisal exercise."

(8-5)

"We suggest that it would be desirable to strengthen the Consultative Committee of the Fusion Programme, and to create more effective liaison with other Directorates with an interest in fusion and in advising the Council and Parliament. We also suggest that stronger links are established with the Parliament and its Energy and Research Committee in order to bring policy on fusion in line with policy on energy."

(8-6)

<u>Chapter Nine. Public Acceptability.</u>

"...There is as yet no body of criteria by which judgements can be made for the longer term and which commands wide social acceptance."

(9-2)

"The evolution of large institutional interests is associated with the growth of new technologies. The institutions have been a factor in polarising the debate by the manner in which they have wielded their power."

(9-3)

"There is no reason why fusion should be funded if it cannot meet close scrutiny on safety and environmental grounds. Securing acceptance on these grounds may be more difficult than attaining technical or economic feasibility. Acceptance of fusion on environmental grounds could be the industry's botton line."

(9-5)

<u>Recommended Amendments to the Commission's Proposal to the Council and the Parliament for the Next Stage of Funding (COM(87) 302).</u>

"Whereas thermonuclear fusion is a potential source of energy, it has to be evaluated in the context of all the available energy technologies which the community is funding, and against accepted criteria."

(Page 42, para I)

"The new programme will include a series of studies in the major areas (including economics, and the environment) of fusion development, contributing to a full feasibility study carried out in consulation with all of the Directorates concerned and with the Parliament."

(Page 42, para III)

"Whereas the recent proposal by the Commission is an interim measure, long-term decisions based on a thorough assessment of the project including participation in international projects will be prepared during the interim period, and before the next funding proposal."

(Page 43, para I)

"The Commission shall proceed without delay to a consultation with all the relevant bodies on an evaluation of the project in its scientific, technical, economic and social aspects. This review process will seek to bring fusion as an energy source into close relationship with community energy and research policies. The evaluation shall involve independent expertise in all the major areas. It shall be made available for discussion before the next funding proposal is submitted."

(Article 3)

CHAPTER ONE. INTRODUCTION.

The object of this study is to provide the European Parliament with a balanced appraisal of the European Fusion Programme. We have been commissioned by the STOA Project to consider "What criteria shall be used to judge the success of the European Fusion Research programme." This may be coupled with the call of the European Parliament for a wide ranging public debate, first made in 1984, on fusion power. The Commission began its response to this resolution in 1985 and it resolved also that there should be a Review of the fusion programme in 1987, and that a proposal should be addressed to the Council to provide for funding for "a new five year programme." In addition, the European Parliament, following the Adam Report, in 1987 asked the Commission to "undertake a comprehensive and longer term review of energy objectives." Thus, this study is placed in the context of a review process taking place within the Community on thermonuclear fusion and on energy policy. These processes are documented a), in the Commission's proposal (COM(87) 302) which contains also the results of the work of the two expert groups on the Environmental Impact and the Economic Prospects of fusion, carried out at the Parliament's request, and (b), in the working documents of the Energy Research and Technology Committee of the Parliament, (CEC (85) 324), the Adam Report and the resolution adopted by the Parliament in April 1987.

We are struck by the absence of any obvious inter-relation between the reports of the Commission and the Parliament. The Commission's documents on fusion appear to have been written without taking into account the energy policies adopted by the Parliament and the Council of Ministers. At the same time these energy policy statements make no virtually no reference to thermonuclear fusion as such. The Parliament's resolution of April 1987, for example, refers to fluidised bed combustion as a project in which more research should be undertaken, but makes no reference to fusion in either clause (f) or (g) which covers research into new energy sources in pursuit of the Community's energy policy. GIVEN THAT FUSION RESEARCH TAKES NEARLY ONE HALF OF THE COMMUNITY'S ENERGY RESEARCH BUDGET, WE FIND THIS SEPARATION OF EXPENDITURE FROM POLICY SURPRISING. WE TAKE THIS INTO ACCOUNT WHEN WE ADDRESS THE TOPIC OF PROJECT MANAGEMENT. WE BELIEVE THAT IF A PROPER LEVEL OF ACCOUNTABILITY IS TO BE REALISED THAT THIS SEPARATION OUGHT TO BE RECTIFIED.

Independent assessment of technologies is being recognised as a matter of major importance in giving substance to democratic accountability. The European Parliament, in a manner analogous to the US Congress has found itself in need of external appraisals of advanced technologies about which it has to make major resourcing decisions. We are sure that this initiative of the Parliament reflects the need to bring the processes of accountability into line with the complexities and problems that it presents to our societies, and to broaden the basis of acceptability. The Office of Technology Assessment of the US Congress has also recently completed a major study of fusion power entitled "Starpower" (OTA 1987). It is a document from which we have benefitted, and in a number of important matters our conclusions co-incide, although the situations they analyse, and the methods used, are often very different. We suggest that these two external assessments in their separate ways may also be seen as marking a steep change in bridging the gap between the power of great new technologies, which necessarily seek to project a future compatible to their needs, and the public whose wider concerns have to be expressed through the political processes of the democratic state.

In addition to making technical observations we have found it necessary to discuss the subject of decision-making and accountability. We also devote a chapter to methods of appraisal. We do this because we found it impossible to separate the questions affecting the future of fusion as a technology, from the perceptions of those in the Fusion Centres and Directorate, whose responsibility it is to direct the fusion programme. We are familiar with the institutional problem that those deeply involved in a new field of research will have a natural tendency to indentify their future, implicitly or explicitly, with the benefits of the technology they have been responsible for. They will therefore find it difficult to participate in an open-ended discussion. That is why the discussion at the political level is now particularly important and takes precedence before decisions about further funding are made. We are by no means sure that there has been the right balance between technical decisions and political decisions in the past. Possibly this has been because there is no settled framework for evaluating the fusion programme. CORRECTING THIS LACK OF A RECOGNISABLE FRAMEWORK IS, WE BELIEVE, MORE IMPORTANT THAN ANY OTHER MATTER AT THIS JUNCTURE. We therefore felt it necessary to make clear our view on these matters. We develop the assumptions that we ourselves use in seeking to respond to the STOA terms of reference for this study. They are ones that broadly accord with Community philosophy and practice.

We would like to acknowledge the assistance we have had from the staff and members of the STOA project, the various members of the staff at the fusion centres at Culham (JET), Garching (Max Planck Institute) and the European Commission (Brussels), who have responded constructively to our questions and enquiries. We would also like to thank Dr Gerald Epstein of the OTA office in Washington, Judith Clarke and Gordon McKerron at the Science Policy Research Unit, Sussex University for their help and comments. Earth Resources Research have contributed the chapter on the Safety and Environmental Aspects of Fusion Power and we have benefitted from their participation. The overall responsibility for the report lies with the research team attached to the Centre for Energy Studies.

As an academically based energy research centre we have no commitment to fission power, or indeed to any other form of energy supply. But we are aware that in the last two years public interest in energy from nuclear fission has sharpened considerably, and while fusion is a longer term prospect the public's perception of the role of nuclear power as an energy form will no doubt continue to acquire clearer definition. Necessarily, there will be controversy about fusion as there is about fission, but there is an evident need to extend and deepen the public understanding of fusion power as a potential energy source even while it is still in the "laboratory" stage. We hope that this report will contribute to that process.

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CHAPTER TWO. THE FRAMEWORK FOR APPRAISING FUSION POWER.

2.1 Introduction.

In this chapter we examine three issues.

1. Requirements for Appraisal of Fusion Technology
2. Appraisal of RD&D programmes
3. Guessing the Future and other pitfalls

2.1.1 Requirements for Appraisal of Fusion Technology.

Appraisal of fusion technology is exceptionally difficult. Firstly, because it is technically complex. Secondly, because it is not yet known if it will achieve net energy output. Thirdly, it is not known how long it will be before this is known. Fourthly, because of the very long time horizon, it is difficult to conceptualise the conditions under which commerical output will be be achieved or how, in political or institutional terms, fusion power can be brought to the market. For these reasons it is particularly important that excesses of optimism are avoided, and that the persistent tendency to overstate its potential and understate the cost are countered by introducing scenarios which include pessimistic assumptions.

THE CENTRAL PROBLEM IS DEALING WITH UNCERTAINTY IN THE FUTURE. THIS MEANS THAT THERE OUGHT ALWAYS TO BE A NUMBER OF ANSWERS TO ANY GIVEN QUESTION ABOUT TECHNICAL OPTIONS, TIME AND COST. Targetted solutions or single figure predictions should be ruled out as being inherently more likely wrong than right. BECAUSE DECISION-MAKING ON MATTERS WHICH AFFECT FUTURE GENERATIONS CAN ONLY BE TAKEN AT THE POLITICAL LEVEL, THEN DECISION-MAKERS OUGHT TO BE PRESENTED WITH OPTIONS.

The manner in which these options are constructed ought to be consistent with the manner in which appraisals are made of other energy technologies, especially those which are alternatives in the sense that they have a significant potential in supplying energy if fusion power is not avaliable. Choices are based on the notion that it is the relative advantage of one product over another, that matters. This precludes, and is meant to preclude, the notion that the future reduces down to a overriding commitment to any particular energy form. WE PROCEED FROM THE VIEW THAT THERE ARE NO IMPERATIVES, AND AT ANY GIVEN TIME THERE ARE A VARIETY OF ENERGY SCENARIOS AVAILABLE, ALL OF WHICH MAY BE PLAUSIBLE. THEREFORE, AS WITH OTHER ENERGY SYSTEMS, THE MERITS OF FUSION CAN ONLY BE PRESENTED TO THE PUBLIC IN TERMS OF THE BENEFITS FOREGONE AS A RESULT OF NOT CHOOSING ONE OF THE ALTERNATIVES.

Our first comment on the presentations in COM(87) 302 is that the authors fail to meet any of these requirements. Whereas we might have expected a consideration of fusion in the broad context of energy futures for the next century, what we have is a largely programmatic approach, in which fusion stands in isolation from what is happening in energy research in both the Community and beyond. Whereas we would certainly have expected the use of an opportunity cost approach as basic to the funding proposals, we have been left with the impression that the authors of COM(87) 302 have no acquaintance with such an approach. The treatment of future demand is confined to heroic assumptions which are trivial in their content and necessarily favourable to the case being made. There is no element of a scenario analysis, now an almost universally accepted method for offsetting uncertainty, and one which we would have thought to have been almost obligatory for those in seeking long-term funding in electricity supply. Finally, there are no planning backgrounds, and hence the advantages of alternative technological routes to future electricity supply are not available, and hence no options for decision-makers. THE DECISION-MAKER IS GIVEN ONLY ONE CHOICE - TO ACCEPT THE PROGRAMME (with perhaps some minor modifications in the distribution of funds between centres, etc.) OR TO REJECT IT. THIS IS PRECISELY THE OPPOSITE OF WHAT WE WOULD HAVE EXPECTED IN A MAJOR FUNDING PROGRAMME FOR A LONG-TERM TECHNOLOGY.

It is important at this stage, in order to avoid misunderstanding of the criticism we have felt compelled to make, to emphasise that it is not directed at fusion as a technology or at the scientists and engineers who have worked in the programme and achieved so much. It is a criticism that any independent body of energy expertise, with experience in this field, would almost certainly have made, and it is one that we believe is fundamental to the manner in which public funds are spent and accounted for. The European programme so far has (in round figures) consumed since 1976, 2600 Mio ECU (the Community has provided slightly less than one half of this sum). This rate of expenditure will increase to a total of 4,900 Mio ECU by 1991, and will continue to increase in size and pace. As the sums increase, the possible losses become larger, a situation that can only be justified by a convincing demonstration that the benefits will be at least equal to those in comparable projects, if not larger. We have found no such demonstration.

A demonstration of future benefits has to be made in recognisable economic terms (eg. benefit to cost ratios with an implicit rate of return). The discount rate should be set with reference to projects comparable in scale and risk eg. investment in offshore oil exploration, which is expected to yield a real rate of return between 5 and 15%. (UK DOE 1987). On present perceptions fusion will involve risks for the utilities greater than nuclear fission or conventionally fuelled electricity generation. It may bear comparison with some of the more remote renewable energy technologies. In any event a pattern is required to inform us of the relative merits of fusion and non-fusion technologies. THERE ARE SUGGESTIONS IN THE COMMISSION'S DOCUMENTATION THAT FUSION IS MORE ATTRACTIVE THAN NUCLEAR FISSION BECAUSE IT IS LESS OF A SAFETY OR ENVIRONMENTAL HAZARD. WE UNDERSTAND THIS TO MEAN THAT THE ENVIRONMENTAL BENEFITS ARE SUFFICIENT IN THEMSELVES TO MAKE

FUSION ATTRACTIVE AS COMPARED WITH FISSION. IF THIS IS SO WE WOULD HAVE LIKED TO HAVE UNDERSTOOD THE FORCE OF THIS ARGUMENT MORE FULLY. GIVEN THE PUBLIC'S CONCERN WITH NUCLEAR HAZARDS, THIS IS AN IMPORTANT CONSIDERATION. UNFORTUNATELY NO SUCH STUDY APPEARS TO HAVE BEEN MADE FOR THE EUROPEAN PROGRAMME AND WE HAD TO RELY ON AN INTERNATIONAL INSTITUTE OF APPLIED SYSTEMS ANALYSIS (IIASA) STUDY OF 1977.

The view is implicit in much of what is written that fusion will be acceptable a), because it will displace nuclear fission on grounds of environmental protection and b), succeed it on economic grounds. It needs hardly to be said that these criteria have very different implications for the decision-maker. The study by IIASA (IIASA 1977) came to the broad conclusion that the fusion has much in common with the fast reactor, but fusion has a balance of advantage in environmental terms. They argue, that it is the environmental, not the economic advantage that matters. They were unable to identify any economic benefits in either technology. The Commission should be in search of a methodology by which it can relate fusion and fission in a study of comparative advantage.

We accept that when the Development stage is reached it will be possible to make more certain judgements about the future. But that point has not been reached. Meanwhile it is important to clarify precisely how the development stage is to be defined and what is the expected level of of funding. The OTA figure of an expenditure approximating to $20 billion fits the European picture (see figure 2). (Note: The OTA estimate can be treated equally as the cost of an international programme, or a national or regional programme. The estimate for a wholly US programme to the completion of the Developmental stage (approx. 2010), taken from the beginning of Fusion research opproximates to $20 billion as does the estimate shown in figure 2 for the European programme).

It is a weakness of the sequential method (which we discuss in detail, in chapter 3), that it leaves no choice but to spend such a sum in order to find out if it has been justified.

2.2 Appraising Fusion RD&D.

Appraisal of fusion RD&D is not an easy task. Perhaps, because of its separate status in the Community, this has not yet been done. But we would regard such an appraisal as a necessary basis for the funding of a fusion programme, consistent with the Community's broad commitment to the best use of resources and to an open competition policy as an instrument of allocating resources.

Research on fusion power began nearly forty years ago. While a great deal has been achieved, it is measure of its complexity that it is still not clear when the Research stage will give way to the Development stage, and even less clear when that will give way to the Demonstration stage. BY ANY COMPARABLE STANDARDS THE

TOTAL COST BEFORE THE COMMERCIAL STAGE IS GOING TO BE FAR GREATER THAN FOR ANY PREVIOUS TECHNOLOGY, INCLUDING NUCLEAR FISSION.

In seeking to bring fusion RD&D within an assessment framwework, it is important to define the present position in relation to the past and the future. This is done graphically in figures 1-3. After 1991/2 the figures are only estimates, and they probably understate the likely costs of the programme as projected by the Commission.

There appear to be two ways of justifying an R&D programme:

The first is what may be termed strategic. This may range from decisions of state, the most obvious of which are military, but which may also include long-term energy strategy. The former would not apply to the EEC because it is an economical and not a military based association. (The US OTA (OTA 1987) and ESECOM (Holdren 1987) studies however do study the military implications of fusion power at some length, and it is clear that such considerations must arise in the context of an international fusion programme). Energy Strategy, however, does fall within the concern of the EEC as it has a long term energy strategy which is broadly aiming at conservation of energy, motivated by the wish to be less dependent on the world market (see the Adam Report). Such a strategy however is not based on principles different from those related to the market. While we understand that a strategic emphasis can be applied here, that in itself would not be sufficient to justify supporting an energy policy. The Community cannot divorce such considerations from its own broad economic strategy (which embraces energy policy), and which calls for projects to be evaluated within a market context. It is recognised that there may be reasons for assisting a technology with public funds - to which must be added the all important caveat PROVIDED THAT IT WILL BE COMPETITIVE WHEN IT ARRIVES AT THE MARKET.

The second justification of such a strategy implies that there will be ordering of priorities. This is clearly stated in the Adam Report;

> "Longer term requirements demand that research and development work in new and renewable technologies must be drastically increased. Community expenditure is currently only 97 million ECU compared with 320 million ECU spent on nuclear developments (1986 committments). Equivalent sums to those spent on the nuclear side must be spent on new and renewables."

For privately funded RD&D, the ability to survive will be determined in the normal decision-making of the firm. Within the public sector, however, the question resolves itself into one of deciding at what point funding shall be sustained, and at what point it shall cease. Such decisions can be most difficult if the cut-off is before the product gets to the market. But unless there is the will to do that, then the process of evaluation may become only a cosmetic exercise.

The funding for the Research and Technology (Framework) Programme, approved in 1986, shows that the strategy criticised in the Adam Report has been maintained and, if anything, become more one-sided in support of nuclear as compared with non-nuclear technologies. In the period 1978-85, the spending on European energy demonstration projects totalled 539mio ECU compared with 1051mio ECU for nuclear projects with the Framework programme (see Figure 2.1).

An evaluation framework therefore rests firstly on an opportunity cost matrix being used (in this case) to compare fusion with non-fusion technologies. The rationality that this implies is not yet recognised in the Community allocation of resources for energy research. Figure 2.1 indicates broadly the order of priorities in Community resourcing of energy RD&D. It reveals that the order of priorities is determined institutionally and not rationally. Nuclear research which is located separately from the Energy Research and Demonstration programmes commands more funds. Yet the Demonstration projects which have to be close to the market, in a linked partnership with an industrial or commercial entrepreneur, will yield a far better real rate of return. This inversion of priorities is not uncommon. The UK House of Commons Select Committee found a similar situation in the UK (HMSO 1984). With respect to fusion it made the following pertinent comment;

> "yet a commitment has already been made to a long term programme... This approach seems to be essentially based on faith in the scientists, engineers and technologists concerned. WE DO NOT CRITICISE THE PROGRAMME ON THESE GROUNDS BUT WE PERCEIVE IT AS BEING RUN ON A VERY DIFFERENT BASIS FROM THAT ADOPTED BY THE DEPARTMENT OF ENERGY IN RELATION TO SMALLER NON-NUCLEAR PROJECTS, TO WHICH THE ATTITUDE APPEARS TO BE ONE OF SCEPTICISM RATHER THAN FAITH."
>
> (HMSO 1984, Vol 1, page xxviii)

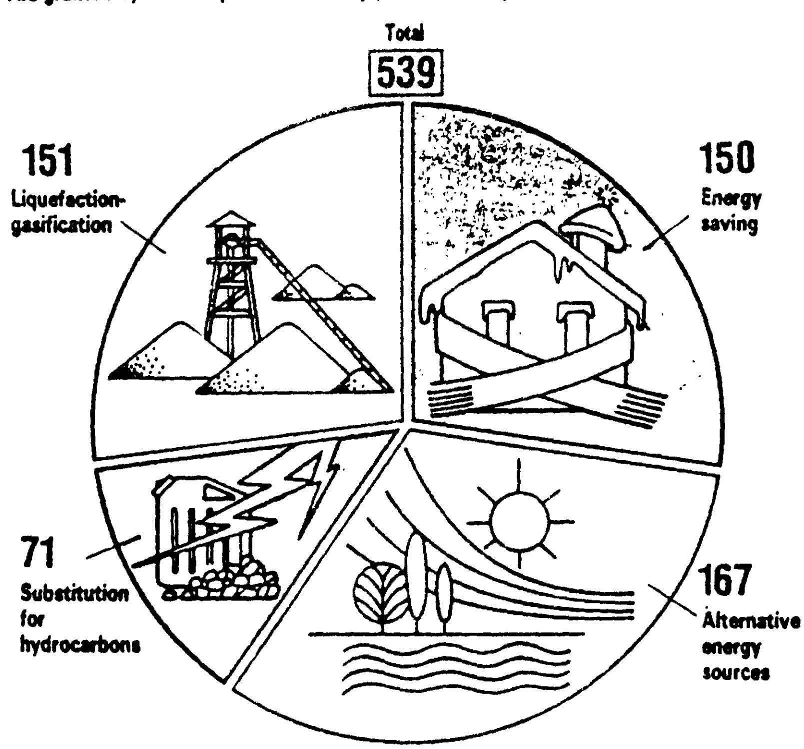
Similarly, faith appears to triumph over scepticism, in the European programme.

Figure 2.1

Research and Technology. Community R&D Policy.
(The Framework Programme, 1987-91).

	mio ECU
Fission - nuclear safety	440
Controlled thermonuclear fusion	611
Non-nuclear energy and rational use of energy	122
TOTAL	1173

European demonstration projects in the energy field, 1978-85
Aid granted by the European Community (in million ECU)



Perceptions about energy futures are subject to continuous change. The Adam Report makes it clear that in the view of the Energy and Research Committee, the Community's research priorities are out of step with its energy policy. What is noteworthy is that the areas in which the Adam Report would like to see more expenditures, especialy in improving energy utilisation, correspond broadly to those which will bring the best rates of return. The matrix method employed in Table 2.4 below (page 2-?) is important for its role in ranking projects. It will be seen that they range from the very attractive (No1) to the speculative and therefore unattractive (No7). It is understood that these are not precise and can change with time. A perspective for the future has to be sensitive to the possibilities for technological innovation, and their likely market impacts.

Innovation is likely to increase rather than decrease competition in electricity supply. The next decade, for example, may see more development in pressurised fluidised bed combustion, gas fired combine cycles, wind and tidal energy. An RD&D assessment that is across the board, will place such prospects in a manageable perspective.

2.3 Criteria and Performance.

In this context the expected contribution that fusion will make when it comes to the market has to be defined - albeit tentatively. IF AN ASSESSMENT IN THE CONTEXT OF AN RD&D PROGRAMME (based on opportunity cost principles) IS NOT AVAILABLE, THEN PROGRESS FROM THE RESEARCH STAGE TO THE DEVELOPMENT STAGE OUGHT NOT TO GO AHEAD.

2.3.1 The Supply Side.

Systems Requirements in Electricity Supply.

The marginal cost of fusion power will be determined in a systems study. We see no reason why the essentials of this should not be studied by the Commission. They would need to address such questions as:

- How good a fit to the pattern of electricity supply, is fusion likely to be?
- What is its likely reliability as a base load supplier?
- To what form of load duration curve does it fit?
- What are the expected availabilities?
- What are the minimum unit sizes for fusion power stations?

- o How flexible is it in adjustment to changing load demand (taking into account the possibility that micro-electronic consumer management may very well be extensive by the mid 21st century)?

These are all questions to which answers should be sought and given according to the limits of present knowledge, in order to give fusion a profile as a provider of power into an electricity system.

Siting/Environmental Issues.

Where would typical fusion stations be sited? Can they be located in urban areas, where they might be competitive with combined cycle or other district heating schemes? Licensing should be no more difficult than that with nuclear fission power stations. However, some attempt should be made to project the direction in which regulations will move.

Waste Management.

What waste disposal problems does fusion give rise to? What will be the decommissioning requirements and costs?

The Fuel Cycle.

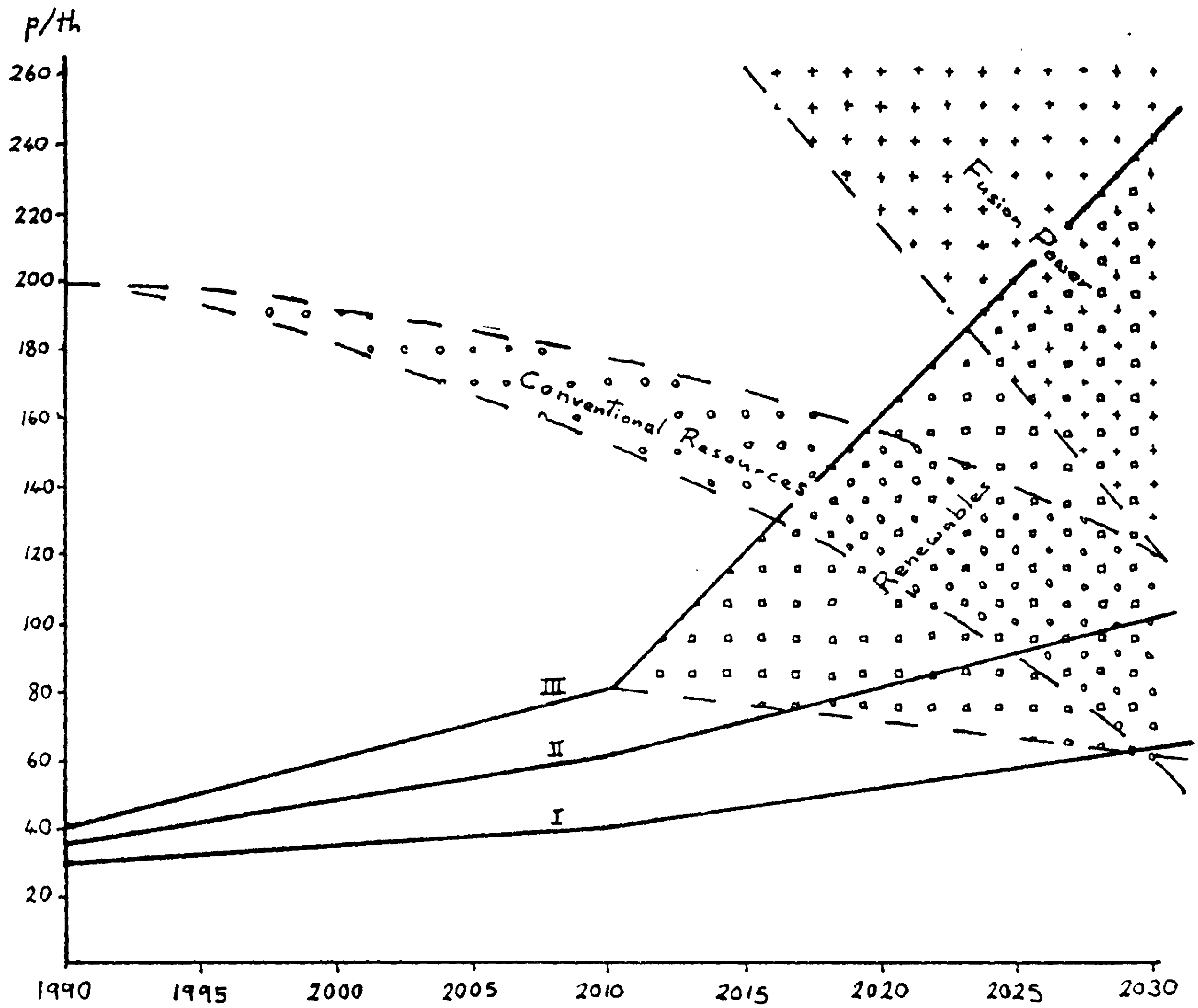
What are the fuel cycle supply logistics? The location of lithium enrichment plants, tritium reprocessing plants, deuterium plants, etc.

Answers to these questions may not yet exist, but models are possible, and these can be refined as the research makes progress. It is necessary from an early stage to define the role of fusion, and as part of this process to indentify the supply side costs which fusion will incur. The answer will only have value in so far as they help us to evaluate fusion relative to other forms of energy supply. It is this task that the R&D assessment is concerned with.

2.3.2 The Demand Side.

On the demand side of electricity supply, the complex of variables might reduce itself to a a scenario analysis offering a range of possible growth projections. Figure 2.2 is a simplified presentation of three scenarios ranging from low to high growth. They might all be regarded in principle as being feasible, although none of them may turn out to be a close fit to reality. Their purpose is to provide a framework for discussion and analysis of the likely trends in the future. If they are judged to be reasonably credible, they will give a feel for the conditions under which fusion might be competitive.

Figure 2.2 Energy Demand Scenarios.



Scenario I. Low growth [0-0.5% GNP pa], price 37pence/therm (2000).

Scenario II. Medium growth [0.5-1.5% GNP pa], price 50p/th (2000).

Scenario III. High growth [2-2.5% GNP pa], price 90p/th (2000).

(Based on internationally traded oil & gas prices).

Scenario I is a low growth future. It should be noted that this is broadly consistent with Community energy policy with its major emphasis on conservation, but Scenario II is a better fit to current Community projections on GNP growth. There is considerable explicit documentation of the low growth scenarios in the energy policies of member states as well as in Community policy overall. Its credibility will be strengthened by the technological changes on the supply side, which increase efficiency both at the primary production level and in utilisation. Here it may be seen that electricity will be under very considerable market pressure both from those technologies which are not so constrained by Second Law Efficiences (which make the overall efficiency of fusion stations low), and also from the advances that continue to be made in the utilisation of energy.

Scenario III offers a view of the energy economy into which fusion might emerge. The assumptions are those which are stated in the sensitivity study in Chapter 7 and it will be noted that they differ markedly from those in the Annex to the Commission's report, which are criticised in detail in Chapter 7. The sensitivity used in Figure 4 is reactor availability, as this is judged to be the most uncertain and important variable that will characterise the performance of fusion reactors as they come on stream.

2.4 The RD&D Matrix.

Bringing the supply side and demand side together with appropriate economic variables, we have what we call an RD&D matrix (see Table 2.4). Although much of it is judgmental it seeks to provide consistency by assuming all RD&D programmes can be appraised using market related criteria. The degree to which they meet these criteria, is shown by the scores given in the matrix. It gives the decision-maker a ranking for all projects on which he can act if he wishes. This holds for those for which a long period is required for the Development and Demonstration stages. Those defined within market values are ranked according to the money values that accrue as benefits. Table 2.4 below, is a summary extract from a recent RD&D study of the UK Department of Energy (UK DOE 1987). The extract shows how performance and expectation can be matched against criteria in electricity supply. The approach adopted to the assessment of individual technologies is essentially an investment appraisal of the technology as a whole, at the point in time when it might be commerically deployed. Annual net benefits are calculated each year to 2030. It is recognised that some technologies cannot expect to yield benefits until after 2030, and attempts to quantify benefits will be "highly speculative."

TABLE 2.4. TECHNOLOGICAL POTENTIAL and RD&D COST EFFECTIVENESS.

	ASSESSMENT OF TECHNOLOGIES				APPRAISAL OF RD&D			
TECHNOLOGIES	TECHNICAL FEASIBILITY	MARKET	TIME-SCALE	ACHIEVABLE CONTRIBUTION - SCALE	ECONOMIC CATEGORY	COST EFFECTIVE-NESS	OTHER FACTORS	RANKING[1] (1-7)
EXTRACTION TECHNOLOGIES								
Conventional Coal Extraction	Deployed	World coal	Now	£££	EA	++++	Export Potential	1
Underground Coal Gasification	Speculative	World gas	Med/Long	££/£££	P	++	International Collaboration	4
Offshore Oil Technology	Deployed	World oil	Now	£££	EA	+++	Import possibilities Export potential Tech. excellance	2
Passive Solar Design (Heat and Fuel)	Deployed	Low temp Heat	Now	££	EA-L	++++	Export Potential International Collaboration	1
Biofuels - Organic Wastes (Combustion)	Dep/Demo	Ind process	Now	££	EA/P	+++		2
ENERGY UTILISATION TECHNOLOGIES								
Building Sector	Deployed	Low temp heat	Now	£££	EA/P-L	++++		1
Industrial CHP	Deployed	Low temp heat + elec appliances	Now	£/££	EA/P	++++		1
Transport Sector (Road Vehicle and Engine Design)	Deployed	Transport	Now Fuels	£££	EA	++++	Export Potential	1
ELECTRICITY-PRODUCING RENEWABLES								
Wind Power	Demo	Electricity Generation	Short/Medium	£/££	P	++	Export Potential	4
Tidal Power	Demo	Electricity Generation	Medium	££	P-L	++	-	4
Photovoltaics	Demo	Electricity	-	£	U	+	Export Potential	6
Geothermal Hot Dry Rocks	Spec/NYD	Electricity Generation	Medium	£	P	++	Techno-logical Excellance	4
ELECTRICITY PRODUCTION TECHNOLOGIES								
PWR[2]	Deployed[3]	Electricity Generation	Now	££/£££	EA	++/+++	International Collaboration	3
Fast Reactor and Fuel Cycle[2]	Demo	Electricity Generation	Medium	££/£££	P/EA-L	+/++	International Collaboration Technological Excellance	5
Fusion	Speculative	Electricity Generation	-	-	U	-	International Collaboration Technological Excellance	7

TABLE 2.4 continued.

KEY.

Technical Feasibility	Achievable Contribution	Economic Category	Cost Effectiveness
Dep: deployed	£: Value < £1 bn	EA: economically attractive	**** RD&D highly cost-effective in all scenarios
Demo: demonstrated	££: Value > £1 bn, < £5 bn	P: promising	*** RD&D cost-effective in all scenarios
NYD: not yet demonstrated	£££: Value > £5 bn	U: unpromising	** RD&D cost-effective in some, but not all scenarios
Spec: speculative		L: treated as a long-term technology	* RD&D cost-effective in no scenario

NOTES.

[1] The ranking is by cost-effectiveness, ie an energy supplier would invest in a technology ranked 1 in preference to one ranked 2-6 and so forth. No 7 has no merit at present because its cost effectiveness is zero.

[2] The cost effectiveness of nuclear fission as given by ETSU has been reduced by one unit, ie by one £ for achievable contribution and one * for cost effectiveness. This is to bring them into line with the change in UK perceptions of nuclear economics.

[3] The PWR is treated as deployed and not in the demonstration stage as given by ETSU, in order to bring the classifications into line with European experience.

The UK RD&D study concluded with the following judgements;

Economic Prospects.

> "Although fusion cycle costs should be lower than for a fast reactor, the difference was judged to be far short of offsetting the much greater capital cost of the reactor island. The latter however, was much the most uncertain part of the analysis, in view of the gross uncertainties of a reactor concept or design.... at this stage all estimates are highly speculative."

Achievable Contribution.

> In a future where fast reactors were the principal source of generation, prospects would appear to be small unless the relative capital costs were reduced. In that event fission power generally, or fast reactors in particular, became politically or socially unacceptable, and if high prices and environmental constraints restricted fossil fuel generation, the prospects for fusion could be much greater."

Timeliness of RD&D.

> In view of the long timescale necessary to bring fusion to deployment, RD&D as part of the

> international collaboration project at JET is judged timely. Equally, in view of the long time before expected deployment and is highly speculative. RD&D outside the framework of international collaboration may be regarded as untimely."

There is an inconsistency in these judgements in so far as the authors suggest that because fusion cannot meet the basic economic tests that it is 'speculative' (which means that there are as yet no benefits to recommend it), that an international option is the only one that can be considered. But is an economically sub-optimal scoring (ie. a negative result in terms of resource allocation) made any better by being shared among a number of actors in an international consortium?

In Table 2.4 above, the approved RD&D technologies (described as economically attractive), are those that score a Benefit to Cost Ratio greater than unity under all scenarios. On the supply side the economically attractive areas are the conventional supply technologies. The best results come from energy utilisation RD&D because most of the benefits come quickly and are able to compete with short term investments with discounts of around 25%. By comparison fusion is classified as speculative, ie it is not yet technically viable. The UK study takes the view that;

> "Fusion RD&D is pursued for strategic reasons only. Viewed from the present, it is never likely to be cost effective in a future where fast reactors are deployed."
>
> (UK DOE 1987)

What emerges from this RD&D appraisal can be summarised as follows. Firstly, that the field of electricity supply is likely to remain very competitive as technologies develop. Secondly, that it will be a long time before fusion power will be able to compete. The time span is not just a function of the learning process in fusion technology, but of the high cost. WHILE WE CANNOT SAY ANYTHING SPECIFIC ABOUT THE COST COMPETITIVENESS OF FUSION (or any other energy form), FOR MORE THAN FIFTY YEARS AHEAD, INTUITIVELY WE CAN SAY THAT ONLY A MARKET IN WHICH THE DEMAND FOR ELECTRICAL POWER IS OUTSTRIPPING SUPPLY BY A CONSIDERABLE MARGIN IS LIKELY TO BE A FAVOURABLE ONE. IT DOESN'T HOWEVER FOLLOW THAT FUSION WILL BE THE ANSWER TO SUCH A SITUATION. INDEED IF ITS INTRODUCTION IS ATTEMPTED BEFORE IT IS A MATURE TECHNOLOGY IT MAY BE THE CAUSE OF SUCH A CRITICAL GAP BETWEEN SUPPLY AND DEMAND.

The choice for decision-makers is not an easy one. From our analysis so far the following emerges:

(1) RANKING TECHNOLOGIES IN TERMS OF THE CONVENTIONAL CRITERIA, FUSION IS SPECULATIVE RATHER THAN TECHNICALLY FEASIBLE AND THEREFORE OPTIONS RANGING BETWEEN A MINIMUM AND A HIGH LEVEL OF FUNDING WILL HAVE TO BE SET OUT, AS IN THE OTA REPORT.

(2) TRYING TO GUESS THE FUTURE WE RECOGNISE THAT A LARGE RISE IN ELECTRICITY DEMAND (AND IMPLICITLY THE REAL COST OF ENERGY) COULD BRING FUSION FROM A BACKSTOP TECHNOLOGY STATUS TO THAT OF A MARGINAL COST PRODUCER. WE THINK HOWEVER THAT FORECASTS OF A LARGE RISE IN ELECTRICITY DEMAND NEED TO BE DOCUMENTED BETTER THAN THEY HAVE BEEN.

(3) BECAUSE OF ITS EXCEPTIONALLY LONG PERIOD OF DEVELOPMENT AND DEMONSTRATION, AND THE LONG LEAD TIMES FOR CONSTRUCTION OF FUSION POWER STATIONS, IN MARKET TERMS THIS WILL PROVE TO BE AN INFLEXIBLE TECHNOLOGY, ESPECIALLY IF DEMAND MANAGEMENT BECOMES MORE SENSITIVE TO CONSUMER CHOICE AND MARKET PRICES. FUSION WILL BE FAVOURED BY AN ECONOMIC ENVIRONMENT IN WHICH LONG-TERM RD&D PROGRAMMES CAN BE FUNDED AGAINST THE PROSPECT OF A PREDICTABLE RISE IN ENERGY DEMAND AND PLANNED ENERGY GROWTH IN THE PUBLIC SECTOR.

The conclusion that would be drawn consistent with an across the board RD&D exercise for allocating funds would be that fusion's benefits to society are as a science research project and have to be evaluated firstly on those terms. As a potential back stop technology its funding would rise as it improves its position relative to to other medium and long-term technologies on a recognisable scale of criteria.

2.5. Guessing the Future.

It is not too strong a statement to say that the case for fusion rests in the eye of the beholder; that is, in the ability to guess the future. Energy forecasting has become an almost obligatory activity, especially for governmental bodies which are charged with the task of looking beyond the market to the longer term needs of society. While in general there can be no objection to this because markets (and very often governments) are notoriously short sighted, the reality is that the art of forecasting has been greatly abused - with the result that it is an activity which is littered with the bones of failed forecasters. This has not deterred the fusion research interests. Indeed if the art of forecasting had not been well developed when fusion science got into its stride, then it would have had to have invented it. As forecasting is so essential to the rationale of fusion, it is a pity that they have treated it so badly. The future can too easily become a convenient way of justifying claims on the present resources of society.

To be specific, the following are the main fallacies which we believe should be expunged from all serious literature, if clarity is going to be achieved.

2.5.1 Heroic Assumptions.

Heroic assumptions should be avoided at all costs, eg to introduce every presentation for fusion funding with the claim that it is an inexhaustible or "almost inexhaustible source of energy" is neither illuminating nor informative. The earth's crust and atmosphere has a super abundant supply of energy. Fusion is one technology capable of unlocking some of that energy. This does not make it unique as an energy source. Essentially it is only another way of boiling water which is then passed as steam over a turbine to make electricity. Fusion therefore is not unique economically speaking. We have many other ways of generating electricity, and most of them, in economic terms, are more rewarding than fusion because on present knowledge they are less complicated and almost certainly much less expensive. In terms of the best use of present resources, making the right investment choices for the future is a pressing problem, because nuclear research dominates the Community's RD&D funding, and thereby pre-empts the development of other energy forms which may be more accessible and less costly.

2.5.2 Over-optimism.

Forecasts about fusion have become couched in an aura of over-optimism. With the resulting risk that serious errors of judgement can be made and, in our view, are being made. The fusion industry could learn from the experience of its near neighbour, nuclear fission, where almost every prediction on the topic of nuclear power has erred on the side of excess optimism. This continues to be done (see for example "Energy 2000" (CEC 1986) which projected a near trebling in nuclear electricity supply between 1983 and 2000. This was in the context of a 50% increase in electricity consumption). Almost every official prediction that has been made in this area has not proved just wrong, but hopelessly wrong. The forecasters, sensing what was expected of them, have fallen into the habit of making the wish the father of the thought. It is true that "To be human is to err", in which case we suggest it would be better now to begin to err in the other direction. Murphy's Law (if the worst can happen it will!) may be extreme, but psychologically speaking it would act as a useful corrective - a more robust way of grappling with the future.

Such a robustness is essential. The biggest problems still lie ahead. The history of technology, and nuclear technology in particular, has shown that the biggest problem is making the leap from the imagination of the scientist to the pragmatism of the engineer. Professor Gowing in her offical history of the UK nuclear industry expresses it thus, when describing the intellectual problems that attended the birth of fusion's near neighbour, the Fast Reactor:

> "The engineers in charge of the project wrote that 'at first sight this fast reactor scheme appears unrealistic. On closer examination it appears fantastic. It might well be argued that it could never become a serious engineering proposition.'...
>
> The physicists might change their minds next year,

> said the engineers, 'but until they do it is left to [us] to get on with the job. "Scientists solve the problems they can. Engineers solve the problems they have to."'"
>
> (Gowing 1974)

No matter how hard the road, the plasma physicist has trod to reach the present point, it is sensible in our view to recognise that the journey to achieve commercial fusion has hardly begun, and the most difficult problems lie ahead. They may prove intractable - at least in terms of social costs.

Lord Marshall perhaps had this in mind when he said of fusion....

> "It is a subject of infinite possibility but zero chance for success"

Of course he had an institutional interest. As a former head of the UK Atomic Energy Authority he not only saw fusion from close up, but he saw its less problematic nuclear neighbour, the fast breeder, come close to being relegated. Making it operational was proving to be more than problematical, and as the problems mounted so did the cost.

As a backstop technology, the fast reactor is being placed on the back burner. If the fusion reactor is to be successor to fission systems then the same logic may apply. The purpose of a feasibility study is to discover if that is the case or not. Being placed on the back burner is not to be abandoned, it is only a recognition that at present our society is not able to manage such technologies. However the longer the gestation period the greater the cost. The opportunity cost of Fast Reactor RD&D has now risen to a point where its future is in question, in a number of countries which has originally invested heavily in a Fast Reactor energy future. There will inevitably be some 'knock-on 'effect from this high premium demanded by the fast reactor, for fusion technology.

CHAPTER THREE SCIENTIFIC FEASIBILITY

3.1 INTRODUCTORY REMARKS

Two important aspects of scientific feasibility in relation to a scientific programme must be clarified before discussing the particular case of fusion. These are:

1) **The relationship of scientific feasibility to the so-called sequential method and**

2) **The adequacy of criteria of scientific feasibility to the declared aims of the program.**

3.1.1 The Sequential Method

The sequential method in a scientific program is generally represented (and is represented in the fusion program - cf CEC 1987b and Carruthers 1976 eg) by the following, supposedly sequential, three-step schema:

(i) scientific feasibility
(ii) engineering feasibility
(iii) socio-economic feasibility.

Socio-economic feasibility is used here to include all criteria of economic social and environmental acceptability.

The sequential nature of the schema is that the initial research thrust of a scientific program is aimed at demonstrating the scientific feasibility of the concept in question. When this has been settled, the engineering issues are tackled and finally the social, economic, and environmental feasibility is assessed.

Whether or not this method is ever actually adhered to in any scientific program (not excepting the fusion program) may be a matter of some contention. As a methodology however, the consequences of adopting such a schema will have considerable financial and environmental impact, both for the immediate sponsors of the program and for the general public. It pays us therefore to devote some attention to considering the force of this methodology.

For the purposes of our discussion we identify two distinct positions which we will call the weak sequential method and the strong sequential method respectively. The weak sequential method is encapsulated in the following propositions:

The Weak Sequential Method

> The demonstration of the scientific, engineering and socio-economic feasibility of a technological program can only

proceed (if at all) in the following sequence:

(i) scientific feasibility
(ii) engineering feasibility
(iii) socio-economic feasibility.

The strong sequential method contains small but essential differences:

The Strong Sequential Method

The question of the scientific, engineering and socio-economic feasibility of a technological program can only be settled in the following sequence:

(i) scientific feasibility
(ii) engineering feasibility
(iii) socio-economic feasibility.

The two positions are denoted weak and strong respectively to reflect the nature of their logical relationship to one another, namely that the strong sequential method implies the weak sequential method but not vice-versa.

It is easily demonstrated that the position we have called the weak sequential method is a valid one in the following sense:

Assessment of the socio-economic impact of a particular technological program depends crucially on the inventory of economic and environmental parameters required by the program. Such parameters include, for example, the material resources required for construction of the technology, as well as the potential environmental hazards involved. These parameters cannot be fully specified (although they may be partly specified) before the exact nature of the engineering constraints has been determined. These constraints depend in their turn upon the proposed engineering solutions to the scientific problems inherent in the program. The only relevant solutions to these problems are those pertaining to a scientifically feasible program. Scientific feasibility must of course be demonstrated before these solutions are known.

Of course the weak sequential position is not really a 'method' as such. Its methodological force lies in the following two methodological imperatives: firstly that all scientific issues must be tackled at the very earliest stages of the program; and secondly, that the final arbiters for or against the implementation of a particular technology in the market are the criteria of social, environmental and economic acceptability. This is not at all to relegate the socio-economic criteria. On the contrary, it assigns them a primary role in assessing the feasibility or infeasibility of a program.

It is tempting to use the validity of the weak sequential methodology to imply the validity of the strong sequential methodology. This would not only be logically fallacious (a weak proposition does not imply a strong proposition) but could prove both costly and dangerous. To assume that the two methodologies are equivalent would be to beg the question not only of scientific feasibility but also of both engineering and socio-economic feasibility. The strong sequential method suggests that socio-economic infeasibility cannot be demonstrated before engineering and scientific infeasibility has been demonstrated. This is a position which we regard as patently false for the following reasons:

The final arbiters for or against the implementation of a particular technology in the market are the criteria of socio-economic feasibility. Long before the issue of scientific feasibility is definitely settled one way or another it may become apparent that the scientific nature of the program makes certain engineering demands which in their turn impose social, environmental or economic constraints which are totally unacceptable.

The dangers of neglecting this possibility are twofold. In the first instance, considerable resources may be squandered pursuing infeasible or unacceptable technologies. Secondly, the momentum of an expensive research program primarily concerned with scientific feasibility may, very understandably, provoke a tendency to demote or devalue the conditions of socio-economic acceptability when the time comes to implement the technology. In this respect the strong methodolgical position is not only false but dangerous.

IN OUR VIEW IT IS ESSENTIAL THAT THE POWER OF SOCIO-ECONOMIC CRITERIA AS THE FINAL ARBITERS MUST BE ACKNOWLEDGED BY A CONTINUOUS ASSESSMENT OF SCIENTIFIC AND ENGINEERING PROGRESS IN SOCIAL, ENVIRONMENTAL, AND ECONOMIC TERMS.

3.1.2 Adequacy of criteria

It is an almost immediate corollary of the position outlined in the previous section that the criteria used to assess the scientific feasibility of a technological program must be sufficient to the scientific demands made by the technological program. Unless this is the case, there is a significant likelihood that unrealistic assessments will be made as to the genuine progress achieved towards the declared aims of the program. In particular, AN UNREALISTIC DEFINITION OF SCIENTIFIC FEASIBILITY CAN LEAD TO AN UNDERESTIMATION OF THE SCOPE, AND EVEN THE NATURE, OF THE ENGINEERING PROBLEMS STILL TO BE TACKLED, AND THIS IN ITS TURN WILL OBSCURE THE ECONOMIC AND ENVIRONMENTAL CONSTRAINTS THAT THE PROGRAM IMPOSES. If this happens, then with the best intentions in the world it will not be possible accurately to assess the socio-economic acceptability of the program.

The first step towards fulfilling a condition of adequacy of the criteria of scientific feasibility must be to express clearly the aims of the technological program. In the case of nuclear fusion, WE TAKE THE PRIMARY AIM OF THE FUSION PROGRAM TO BE: TO PROVIDE A (COMPARATIVELY) SAFE, ECONOMIC, AND ENVIRONMENTALLY ACCEPTABLE SOURCE OF ELECTRICAL POWER FROM THE USE OF CONTROLLED THERMONUCLEAR REACTIONS IN A PLASMA.

IF CRITERIA OF SCIENTIFIC FEASIBILITY FOR THE NUCLEAR FUSION PROGRAM ARE TO BE ADEQUATE IN THE SENSE OUTLINED ABOVE, THEN THEY MUST EXPRESS, NOT ARBITRARY STAGING POSTS IN PLASMA PHYSICS, BUT THE FEASIBILITY OF ATTAINING SCIENTIFIC LANDMARKS DIRECTLY RELATED TO THE DECLARED AIM OF ACHIEVING A USEFUL POWER SOURCE. **It is easy to find examples of scientific criteria which, though tempting, are not adequate to the declared aims. For instance the following:**

(i) the existence of thermonuclear reactions between nuclei

(ii) the existence of thermonuclear reactions between nuclei in a controlled laboratory environment

are representative of such related but not adequate criteria. The first of these is easily satisfied. It has been known for many decades now that thermonuclear reactions between nuclei are the source of the sun's energy. The existence of nuclear fusion in the sun is no indication however of the scientific feasibility of producing electricity through nuclear fusion under terrestial conditions (except perhaps via the intermediate step of photovoltaics which are indeed known to be scientifically feasible). The relevance of the second possible criterion is less easily demolished.

As long ago as 1957 neutrons were observed from the experimental toroidal machine Zeta at Harwell operating with deuterium (an isotope of hydrogen) at about a million degrees Celsius (cf eg Thonemann et al 1958). Neutrons are a product of the fusion reaction between deuterium nuclei and for a short time it was believed that the Zeta results were an adequate demonstration of the scientific feasibility of controlled nuclear fusion. Spectral analysis revealed however that these neutrons were produced predominantly in collisions between deuterium nuclei moving parallel to the axis of the toroid, rather than in randomly directed collisions. The reactions were therefore declared to be reactions between artificially accelerated deuterium nuclei. The unacceptability of the existence of these reactions as a criterion for the feasibility of a power-producing fusion reactor lies in the enormous input power required to artificially accelerate the colliding beams. Such a process could never be a net producer of power.

The problem with both of the above possible criteria is that they express some but not all of the scientific demands imposed by the aims of the program.

There are three distinct ways in which the adoption of such inadequate criteria can be misleading. Firstly, an inaccurate portrayal of the extent of progress towards the declared aims is likely. Secondly, the lack of clarity in assessing the scientific objectives has consequences for the assessment of both engineering and socio-economic feasibility which will obscure the decision-making process. Finally, and most unfortunately, UNLESS THE DEMANDS OF THE TECHNOLOGICAL PROGRAM ARE FULLY EXPRESSED BY THE SCIENTIFIC CRITERIA, CERTAIN IMPLICIT SCIENTIFIC DEMANDS, MAY BECOME NEGLECTED AT A VERY FUNDAMENTAL LEVEL, LEADING TO CONSIDERABLE WASTE OF RESOURCES THROUGH THE PURSUIT OF UNREALISTIC OR UNREALISABLE GOALS.

3.2 CRITERIA OF SCIENTIFIC FEASIBILITY IN THE FUSION PROGRAM

On a simplistic analysis, the generation of electricity through magnetic nuclear fusion requires that a very hot ionised gas, or plasma,(temperatures must be in excess of 100 million degrees Celsius) is confined in a magnetic field at sufficient density and for sufficient times that ions colliding with each other in the plasma release sufficient thermonuclear (fusion) energy to compensate for the power losses from the plasma and be a net-producer of useful power. On this very simplistic analysis, the three crucial fusion parameters become the temperature T, the density of the plasma n, and the so-called confinement time τ.

The mathematical representation of this simplistic analysis reveals in fact a very straightforward relation between the three fusion parameters. It emerges that the relevant performance parameter is the 'fusion product' $nT\tau$ of the three crucial parameters temperature (central ion temperature to be precise), density (central ion density) and confinement time. Using this fusion product it is possible to formulate certain basic criteria which are commonly taken in the literature as the foundations for the scientific feasibility of nuclear fusion. We list these in order of their severity.

(i) 'Breakeven' ($Q_{PL} = 1$)

The quantity Q_{PL} is defined as the ratio of the thermonuclear power P_{TH} generated in the plasma to the power lost P_L from the plasma via radiative processes. When $Q_{PL} = 1$, we have:

1) $$P_{TH} = P_L.$$

ie the losses from the plasma are compensated for by thermonuclear power gained: hence the origin of the term 'breakeven'. It must be noted however that this represents a 'breakeven' within the plasma and NOT within the system. For a Deuterium-Tritium (D-T) plasma the value of the fusion product required to achieve 'breakeven' in this sense are in the region of 10^{21} m^{-3} keVs.

(ii) 'Lawson's Criterion'

In 1955, in the very early days of fusion research, an attempt was made to provide a minimum necessary criterion for a 'power-producing thermonuclear reactor' by John Lawson. The condition that he proposed has come to be known as the Lawson Criterion and has in some sense been the inspiration for subsequent analyses of the problem. Lawson's criterion differs from the $Q_{PL} = 1$ case by including an attempt to come to terms with the empirical limit to the efficiency with which heat energy (released from the plasma) may be converted to electrical energy needed to supply the heating circuits. Lawson assumes that all the power released from the plasma (including both P_{TH} and P_L) may be thus converted. The mathematical expression of the situation in which the total electrical power gained from the plasma is sufficient to compensate for the radiative losses may be written - in the terminology of the previous case - as:

$$\eta (P_{TH} + P_L) = P_L$$

Lawson takes η to be one third so that this condition may be written:

2) $$P_{TH} = 2P_L .$$

Values of the fusion product required to satisfy 2) are correspondingly higher than those required for case 1). For satisfaction of the Lawson criterion in a D-T plasma we require values of $nT\tau$ in the region of $2 \times 10^{21}\ m^{-3}$ keVs.

(iii) 'Ignition'

The fusion process releases two types of energetic particles. Helium nuclei and neutrons according to the following equation:

$$D + T \rightarrow He + n$$

(A similar equation holds for the D-D fusion reaction). About 80% of the thermonuclear energy released during this reaction is carried by the neutrons, while the remaining 20% is carried by the Helium nuclei. The energetic neutrons leave the plasma very quickly and contribute nothing directly to the heating of the plasma. It is possible however that the alpha particles (Helium nuclei) remain long enough within the plasma to contribute significantly to the heating effect. When enough fusion reactions take place for the alpha particles to provide sufficient energy to maintain the plasma at the required temperature without external heating sources the plasma is said to have reached ignition. On a simplistic analysis in which the alpha particles transfer all their energy to the plasma (and do not increase losses from the plasma) the mathematical condition for 'ignition' is given by:

3) $$P_{TH} = 5P_L .$$

Values of the fusion product required for ignition of a D-T plasma are in the region of $5 \times 10^{21}\ m^{-3}$ keV.

3.4 PROGRESS TOWARDS MEETING THE CRITERIA

According to the analysis of the previous section, the crucial parameters for assessing scientific progress towards nuclear fusion are the temperature, density and confinement time of the plasma. In JET, values for these parameters have now been obtained individually which are very close to those required to satisfy the 'ignition' criterion. The problem which has become the bête noir of modern fusion technology however, is that of achieving the required values for these three parameters simultaneously.

For instance it has been possible using a variety of ingenious heating techniques (ohmic, radio-frequency, neutral beam) to increase the temperature of the plasma to well within the range required for fusion, but increased temperatures have resulted in degradation of the confinement time. Another problem encountered is the instability of the plasma under increased densities. Collapse of the plasma under such instabilities can result in significant mechanical and thermal stresses on the apparatus which constitute a safety threat (cf Chapter 5) as well as limiting the operational capacity of the device. Yet another problem area has been the disparity between the response of ion temperature and electron temperature to additional heating power. For fusion conditions it is imperative that these two temperatures remain roughly the same.

Values of the fusion product currently achieved remain a factor of five away from those required for the Q = 1 criterion and a factor of 25 away from those required for the 'ignition' criterion.

The current 'best-shot' for the fusion product at JET is in the region of $2 \times 10^{20}\ m^{-3}$ keVs. This has been achieved using 10MW of neutral beam heating during the so-called X-point operation in H-mode. This mode of operation employs a magnetic configuration in which, as a matter of course, far more interaction exists between the plasma and certain parts of the surrounding structure than in the usual limiter mode, imposing consequently considerable challenges to the engineering and environmental aspects of the program. Furthermore, it is not expected (cf OTA 1987) that neutral beam heated plasmas will be used in practical reactors. Although neutral beam heating is effective in increasing the neutron yield in the plasma (largely due to interactions between particles in the beam and particles in the plasma), the beams themselves require a lot of power to operate.

It is hoped that, with the known scaling laws, and once additional heating has been commissioned in JET and modifications to the poloidal current which drives the plasma current have been made, the value for the fusion product in JET will then approach that required by the criterion (i) above.

3.5 CRITIQUE OF THE CRITERIA

The question we must immediately pose concerning the criteria outlined in section 3.2 is this:

> Do all or any of these criteria meet the requirements of adequacy proposed in section 3.1?

Let us first remark that in order to prove the scientific feasibility of any concept, it is not necessarily essential to demonstrate this feasibility experimentally. It is enough that the theoretical understanding of the concept is both complete, and reliant only on empirical concepts which are experimentally verified. What <u>is</u> essential is that the criteria upon which the scientific feasibility is to be judged are themselves adequately formulated in the sense of section 3.1 to reflect all the scientific issues embodied in the aims of the program.

The analysis leading to the formulation of the three criteria in section 3.2 above was described as simplistic because (with the exception of the second criterion) the only losses taken into account in formulating the mathematical expression are those associated with losses from the plasma itself. In the so-called 'break-even' criterion for instance, the function Q_{PL} representing the ratio of energy produced by thermonuclear reactions to the total energy supplied to the plasma takes into account the losses quantifiable in terms of the 'classical confinement' of the plasma and some radiative losses. Not taken into account in Q_{PL} are the 'circulating losses' in the system, associated with the magnetic confinement of the plasma and the generation of plasma current, and with inefficiencies in the heating circuit. Conductive losses from the plasma through minor disruptions, and increased radiative losses due to high impurity levels are in addition extremely difficult to quantify.

There is no question that the achievement of an ignited plasma will constitute a major scientific achievement for plasma physics and a significant advance towards a power-producing thermonuclear reactor. The analysis for the mathematical definition of the ignition criterion however, is simplistic in yet another respect: it makes the assumption that the behaviour of the alpha-particles in the magnetic field will be such as to allow all the energy of the charged particles to be available to heat the plasma. In fact the behaviour of the alpha-particles in a hot plasma is still very much a matter of guesswork. Plasma physics is still, relatively speaking, an infant technology: its theoretical background is not well established and there is very little experimental evidence concerning the behaviour of alpha particles in a hot plasma - or indeed concerning the behaviour of a hot plasma under the influence of quite high proportions of alpha particles.

Strictly speaking the second of the above criteria does not belong to our 'simplistic' analysis. This is because it introduces a system parameter not directly related to the process of nuclear fusion within the confined plasma, namely the efficiency of electrical conversion. When Lawson published his criterion in 1957 however, he made it quite plain that even including the generation efficiency was not to suggest that a sufficient condition for a practical power-producing reactor was being proposed. He writes:

> 'The analysis is based on simple assumptions; it is designed to illustrate the essential features of the problem and is neither rigorous nor complete. The assumptions made are in all cases optimistic, so that the criteria established are certainly necessary, though by no means sufficient, for the successful operation of a thermonuclear reactor.'

Despite this warning, and despite the fact that the Lawson criterion does not account for circulating power losses either, it remains - even thirty odd years after it was proposed - a more realistic attempt at scientific feasibility than the other two criteria, specifically because it attempts to deal with the issue of scientific feasibility in terms of the declared aims of the project, namely to provide an electricity-producing thermonuclear reactor.

What we have said so far appears to be a very damning indictment of the process of evaluating the scientific progress of the fusion program. It is certainly a very serious criticism of the currently elaborated methodology. Nevertheless it is possible to raise a counter-argument that nothing we have said actually, in itself, invalidates the criteria outlined above as suitable for the demonstration of scientific feasibility. This is true. It is possible, but by no means self-evident, that circulating power losses are in fact irrelevant to the scientific analysis of the problem. Equally it is possible that the simplistic assumptions concerning plasma losses and alpha-particle behaviour are sufficient for the purposes of scientific feasibility.

Our criticism of the methodology however, must remain: NEITHER OF THE CRITERIA CURRENTLY ADVANCED AS REPRESENTATIVE OF A DEMONSTRATION OF SCIENTIFIC FEASIBILITY IS IN ITSELF ADEQUATE TO THE TASK, BECAUSE NEITHER OF THEM TAKES INTO ACCOUNT ALL POWER LOSSES RELEVANT TO A POWER-PRODUCING REACTOR, AND CONSEQUENTLY DOES NOT ADEQUATELY EXPRESS ALL THE SCIENTIFIC AIMS OF THE PROGRAM. Significant additional assumptions must be made, concerning genuinely scientific aspects of the system, for any of the above-mentioned criteria to be acceptable as demonstrations of scientific feasibility. As it stands, these assumptions appear to be inadequately backed by theoretical understanding or by experimental verification. Even if these assumptions are warranted it is essential, methodologically, that they be made explicit.

3.6 A Sufficient Criterion for Scientific Feasibility

Let us now ask the question: what would be a sufficient criterion for the demonstration of the scientific feasibility of a power-producing thermonuclear reactor? Since the term 'power-producing' in this context means that the system as a whole (ie taking into account all circulating power losses as well as plasma losses) is a net-producer of electrical power, the following criterion, which we shall call 'system breakeven' to distinguish it from 'plasma breakeven' is certainly sufficient to demonstrate the scientific feasibility of the program:

System breakeven ($Q_{sys} = 1$)

> System breakeven is reached when the total power recovered from the system (ie the fusion reactor) is equal to the total power, including all circulating losses, into the system.

To illustrate the magnitude of the difference between this condition and the 'plasma breakeven' condition of section 3.2 we quote the following extract from the recent report on the US fusion program's Tokamak Fusion Test Reactor (TFTR) in Princeton, carried out by the Office of Technological Assessment:

> 'TFTR is being upgraded to deliver up to 27 MW of neutral beam power to the plasma. To reach [plasma] breakeven, where the fusion power generated equals the external power injected into the plasma, 27MW of fusion power would have to be generated in the plasma. If reaching breakeven were to require TFTR to draw near the maximum amount of power available from its electrical supply, it could consume close to 1000 MW of electricity. This amount is 37 times greater than the fusion power to be produced at [plasma] breakeven.'

As an example of the kind of system losses which create this sort of disparity between plasma breakeven and system breakeven, let us consider the power consumed by magnetic confinement of the plasma. In JET two flywheel generators (powered by the grid) deliver a peak power output to the toroidal and poloidal magnets during a plasma pulse of 400MW. The current best fusion power output from the device (if operated with a D-T plasma) would be around 1MW. In terms of system breakeven this thermonuclear power output is barely significant in relation to the total system input power. The enormity of this disparity will be greatly reduced in future designs where the electromagnets will be replaced by supercooled superconducting magnets which consume a fraction of the power consumed by the more conventional electro-magnets. Nevertheless, it is worth making two points concerning this example.

Firstly, the demands of scientific feasibility, which in this case are glaringly obvious, force an engineering constraint on the program, namely that of employing supercooled magnets to provide confinement. The technology of supercooled magnets, especially in such a large-scale engineering context, is relatively new. In addition, these magnets supercooled to temperatures approaching absolute zero are in very close proximity to very high-temperature regions of the reactor. This not only presents increased engineering difficulties, it also poses an increased environmental risk, albeit slight, over the use of conventional magnets.

We have here a precise example of the way that scientific constraints enforce engineering and environmental difficulties of significant magnitude. Without a methodology prepared to accept the arbitration of socio-economic criteria, no structure exists for assessing at a sufficiently early stage in the program to avoid wasted resources, whether or not such a solution to the scientific problem is acceptable.

The second point is even more serious. The use of supercooled magnets will drastically reduce the system power losses but it will not eradicate them entirely. (In particular, for a tokamak reactor considerable power must still be supplied to provide the current drive for the plasma.) If these losses are not taken into account in the mathematical formulation of the scientific criteria, it is possible to assume, as is currently done, that the provision for these losses is totally unrelated to achieving the desired performance targets. This is not the case. A reformulation of the 'break-even' criterion to include all relevant plasma and system losses might look like this:

$$\eta (P_{th} + P_L') = P_L' + P_{sys}$$

where P_L' may or may not need modification to take account of disruption and impurity losses, and P_{sys} represents the non-recoverable system losses.

When the losses are included in the mathematical formulation of the breakeven criterion the first thing that one notices is that there is no longer any guarantee that the fusion product is a relevant parameter by which to judge the scientific feasibility of fusion. The fusion product is a mathematical consequence of a particular set of rather simplistic assumptions about the scientific context of the program. It is extremely misleading to divorce this parameter from that scientific context. Whether or not the fusion product remains a valid parameter for the assessment of scientific feasibility depends crucially upon the mathematical formulation of the other system losses.

Worse than this, the scientific feasibility of the program itself depends crucially on the mathematical formulation of the relevant system losses. As a hypothetical example, let us consider the magnetic system losses in more detail.

One of the problems associated with achieving high temperatures and densities in plasmas has been the degradation of confinement time. One way of tackling this problem is to improve the magnetic confinement of the plasma. Various magnetic configurations are currently being investigated to determine which have the best confinement properties. One obvious way of improving the confinement is to increase the magnetic field strength. (This also improves the density limit in the plasma.) Increasing the magnetic field strength however, necessarily involves increasing the circulating losses. Another way, would be to work on increasing the efficiency β with which the magnetic field confines the plasma. Experimental evidence suggests however, that physical properties of the plasma - ie genuinely scientific limitations - prevent β-values from being increased indefinitely.

These are all questions fundamental to the scientific feasibility of fusion. Questions, concerning which, current theoretical and empirical understanding is very limited. Whether or not such issues eventually affect the scientific feasibility of fusion is not the point here. The fact is that by not including all scientific issues relevant to the ultimate aims of the program in our scientific criteria, there is no way that those criteria will in themselves be able to determine whether or not the program is scientifically feasible.

We have established that the system breakeven condition is <u>sufficient</u> to demonstrate scientific feasibility: it necessarily includes all scientific aspects associated with the system. Finally we ask: is the condition <u>necessary</u> for scientific feasibility? Certainly it is necessary to achieve this condition in order to demonstrate <u>experimentally</u> the scientific feasibility of the program. We have already remarked however, that actual experimental verification is not necessarily essential to the process of demonstrating scientific feasibility.

In fact we have no means of knowing whether or not a weaker condition might suffice for the purposes of demonstrating scientific feasibility unless the system breakeven is formulated mathematically, making explicit all assumptions and all theoretical implications.

IN SUMMARY THEN, WE HAVE ESTABLISHED THAT THE CRITERIA GENERALLY REGARDED AS RELEVANT TO THE ASSESSMENT OF THE SCIENTIFIC FEASIBLITY OF A NUCLEAR FUSION ARE NOT IN THEMSELVES ADEQUATE TO THE AIMS OF A POWER-PRODUCING REACTOR. WE HAVE ALSO ESTABLISHED THAT THE SYSTEM BREAKEVEN CONDITION IS SUFFICIENT FOR THIS PURPOSE. A WEAKER CONDITION <u>MAY</u> SUFFICE AS THE CORRECT SCIENTIFIC CRITERION BUT THIS HAS NOT BEEN DEMONSTRATED.

IN OUR VIEW THE CORRECT SCIENTIFIC CRITERION MUST DOMINATE THE PROGRAM FROM THE EARLIEST STAGES. THE DANGERS OF NOT DOING THIS COULD BE THAT THE ENTIRE PROGRAM IS DEDICATED TO PURSUING PERFORMANCE PARAMETERS WHICH ARE SIMPLY NOT RELEVANT TO THE EVENTUAL GOAL. THE RESULT OF DOING THIS COULD, IN THE VERY WORST SCENARIO, BE THE ENORMOUS WASTE OF RESOURCES ON A PROGRAM THAT IS SIMPLY NOT SCIENTIFICALLY FEASIBLE.

We are not suggesting that this is the case with the nuclear fusion program. We are suggesting that insufficient effort has been dedicated to ensuring that it is not the case.

CHAPTER FOUR. ENGINEERING FEASIBILITY AND THE FUSION REACTOR.

4.1. The Interrelations between Engineering, the Environment, and the Cost of A Fusion Reactor.

4.1.1 Introduction.

> "The most difficult problems appear to be those associated with materials science: superconductors to withstand enormous mechanical stresses for years; mirrors and lenses to handle tens of thousands of laser pulses of devastating power daily; first-wall materials, next to the fusion plasma, which must be resistant to swelling, sputtering, blistering, cracking, and loss of strength under intense bombardment by fusion reactions, x-rays, and energetic ions, and which must also be compatible at their elevated operating temperature with the coolant and tritium-breeding and neutron-multiplying materials; electrical insulators that can retain their properties in this hostile environment; and so on. Extraordinary demands will also be placed on vacuum technology, instrumentation and control technology, energy storage and switching technology, and systems integration. If all this can be pulled together to produce a semblance of a power reactor within 15 years or so of the scientific feasibility demonstration - that is, say, by the year 2000 - it will be an amazing accomplishment."
>
> (Holdren 1978)

> "On the basis of current evidence, the Tokamak Fusion Test Reactor (TFTR), now under construction at the Princeton Plasma Physics Laboratory, should demonstrate more than energy break-even after its completion in 1982. Furthermore, extensive technology development programs in the regions mentioned above indicate that there is no fundamental technological obstacle to translating the scientific success of tokamak development to the production of controlled fusion power."
>
> (Clarke 1980)

The two quotes above indicate a disparity of views on the engineering feasibility phase of the fusion R,D&D programme, in this section we shall explore the reasons for such a divergence of opinion in some detail with a view to identifying the critical areas of a fusion power programme.

We have seen that JET, although it may satisfy the Lawson criteria, is not in a position to fully prove scientific feasibility, and it is now seen that NET will have to be flexible enough to finish the task of proving scientific feasibility as well as being an engineering test reactor (and similarly will initially run on H-D plasma rather than D-T (Atom 1987)). NET will have two aspects of engineering feasibility integral to its design. Firstly, those engineering problems associated with obtaining reactor plasma parameters, which we have seen from the discussion of scientific feasibility are proving to be more demanding than was anticipated. Secondly, there are those engineering problems concerned with proving the possibility that fusion power can be used to generate electricity in an actual reactor. There are a large amount of unknowns involved in this part of the programme, and it is likely to be the most demanding and costly part of the whole programme.

Work has begun on possible reactor designs, mainly as an exercise in problem finding rather than problem solving. As everyone involved is keen to point out, these are necessarily tentative and speculative. However, these studies have been extremely useful in identifying the kind of constraints that will apply to a fusion reactor. THREE MAIN AREAS CAN BE DISCERNED. FIRSTLY, THERE ARE THE PURE ENGINEERING PROBLEMS SUCH AS FUELLING AND EXHAUST, EFFECTS OF HEAT AND NEUTRON IRRADIATION OF MATERIALS, TRITIUM HANDLING AND EXTRACTION, BREEDING MATERIALS, ETC. SECONDLY, THERE ARE ENVIRONMENTAL PROBLEMS SUCH AS MATERIALS ACTIVATION AND WASTE, ROUTINE RELEASES AND ACCIDENT POTENTIAL. HOWEVER, IT BECOMES CLEAR IMMEDIATELY THAT SUCH ISSUES ARE NOT DISCRETE, THERE ARE CLEAR TRADE-OFFS BETWEEN IDEAL ENGINEERING SOLUTIONS AND ENVIRONMENTAL SOLUTIONS. THE THIRD AREA, COSTS, CAN ALSO BE SEEN TO BE INVOLVED IN THE ALREADY EXISTING TRADE-OFFS BETWEEN ENGINEERING AND THE ENVIRONMENT. Typically then, major engineering decisions will involve estimating the likely effects on the environment and on electricity supply cost as well the usual engineering choices. Many decisions on the design of fusion reactors have already been taken on this basis. These decisions are not taken in a particularly coherent way, it is more that certain options may be excluded if they are thought to involve too much of a particular sort of cost, although it must be understood that all options involve some costs.

Obviously, extreme solutions are not feasible. A reactor design where every decision was taken in favour of the most environmentally clean alternative would be prohibitively expensive. Similarly the cheapest reactor would be dirty and dangerous in environmental terms. It follows that reactor designs will have to take account of the role that fusion power is expected to fulfil and its acceptability to decision-makers. THERE ARE AT LEAST TWO DIFFERING VIEWS OF WHAT FUSION POWER GENERATION WILL OFFER, WHICH MAY SERVE AS CRITERIA FOR JUDGING THE ACCEPTABILITY OR OTHERWISE OF FUSION POWER. ONE IS THAT ITS CHIEF BENEFITS ARE NOT IN THE AREA OF ELECTRICITY COSTS BUT IN ITS ENVIRONMENTAL SAFETY. ON THAT BASIS DECISIONS TAKEN ON COST ALONE COULD BE UNFAVOURABLE TO FUSION POWER. ANOTHER VIEW IS THAT FUSION POWER OFFERS CHEAP AND RELIABLE ELECTRICITY FIRST AND FOREMOST AND THE OTHER ADVANTAGES ARE SECONDARY. IN THE FIRST VIEW, FUSION WOULD NOT BE ACCEPTABLE IF IT FAILED TO BE SIGNIFICANTLY CLEANER THAN AN FBR PROGRAMME. IN THE SECOND VIEW IT WOULD BE UNACCEPTABLE IF IT FAILED TO BE CHEAPER THAN EXISTING

ELECTRICITY GENERATION TECHNOLOGIES. CLEARLY ENGINEERING DECISIONS WOULD FAVOUR ENVIRONMENTALLY CLEAN OPTIONS IF THE FIRST VIEW WAS PREVALENT AND WOULD NOT IF THE SECOND VIEW WAS PREVALENT. Thus it is during the engineering phase that we begin to understand the likely social cost of fusion power.

In the following we shall attempt to outline the main areas where trade-offs in the design of reactors arise, and assess what implications such trade-offs might have for fusion power. Finally, we shall attempt to assess how fusion reactor design is performing in terms of its energy supply scenario and what problems may be anticipated in a fusion reactor programme.

4.1.2 Terms of Reference.

Throughout this study a number of conceptual reactor design studies are used. There are two main reasons for this.

Firstly, these studies are extremely useful in gaining understanding of the main areas of difficulty which would be encountered by an attempt to build a fusion power plant. As such they are extremely useful to the programme management, showing the critical areas where more work needs to be done. There is no attempt to suggest that these studies bear a very close resemblance to what a reactor will actually look like. The studies are based on long burning plasmas up to 5000 seconds, although it is not yet established that such a quasi-steady state is attainable, let alone what the specific plasma conditions will be associated with such situation. Also, most studies are based on a 'reasonable' extrapolation of existing technologies and clearly there is no certainty about how easy or costly some solutions to problems will be. They may be significantly harder than assumed. Similarly some problems may prove to be significantly easier, although it has to be said that the methodology adopted seems to favour optimistic outcomes to problem areas.

The second reason for studying reactor design parameters, related to the first, concerns the question of assessing fusion's feasibility. While it is clear that one cannot demonstrate engineering or economic feasibility with any certainty until scientific feasibility is established, it is a logical fallacy to say that one cannot demonstrate engineering or economic infeasibility until after scientific feasibility is established. Yet this is the approach adopted in the management of the fusion programme. This has two main effects. One, is that those funding the programme are committed to waiting an unusually long time and spending a large amount of money before one can say whether it has been worthwhile. Secondly, such a approach to the management of the programme leads to criticism of the way the programme proceeds. As Carruthers and Schmitter put it;

> "The demonstration of 'scientific feasibility' in a confinement geometry for which it had to be admitted that there was no possibility of

> proceeding from that point to a fusion reactor could be embarrassing."
>
> (Carruthers et al 1976)

In reality, strict engineering infeasibility is unlikely to be revealed, rather that the range of engineering options available may preclude the establishment of economic feasibility, the economic or environmental costs may be too high. It is not uncommon to hear engineers in the fusion programme saying that the tokamak, although being a device which seems most likely to be able to achieve the reactor relevant plasma conditions, is of a design such that it is unlikely to be able to produce electricity at a price that will favour its introduction.

4.2 Engineering Problems.

Engineering problems fall broadly into three groups;

(1) first wall problems

(2) fuel cycle problems

(3) magnetic confinement problems

There are many sub-divisions and some are more integrally linked with envrionmental problems, eg the first wall, than others. The solutions to these problems will obviously be of critical importance to the key questions of capital costs and availability of a fusion reactor.

4.2.1 The First Wall.

(a) Wall Interactions.

The first wall of the DEMO reactor would consist of a 3mm copper wall backed by Helium cooled Inconel tubes. 2mm thick tungsten first wall tiles would be attached to this wall by means of a support structure made of 1mm thick tunsten. The tiles would have an operating temperature of around 2250 C and most of the thermal energy would be transferred to the wall behind by heat radiation. There are several problems of interaction with the plasma. The main engineering ones are that impurities foul the plasma and that such interactions may reduce the life of the wall and any pieces of equipment in that area, such as heating devices, diagnostics, etc.

There is an inconsistency between the rather optimistic statements, based on JET operating experience and the problems envisaged when considering reactor design concepts:

> "Impurity levels presented a problem, as they reduce the number of plasma ions available for fusion and cause radiation losses. Experiments with low-Z (carbon) tiles on the inner walls and a

carbonized vessel showed reduced levels of metal and oxygen impurities."

(CEC 1987B)

"Practical design solutions for the first wall were found to be heavilly constrained by mutually conflicting requirements. During the plasma heating phase, good isolation of the plasma from the tiles will be required to avoid an excessive concentration of high Z impuritity in the plasma. This has not yet been investigated."

(IAEA 1985A)

Also, after a year of operation, JET was found to have suffered some fairly serious and essentially unpredicted electron damage (neutron damage was negligible due to the fact that JET currently operates only on Hydrogen-Deuterium fuel below reactor levels):

"Erosion features attributable to run-away fast electrons, to unipolar arcing, and to rare power arcs have been identified. Sputtering and evaporation processes are seen and redeposition and cross-contamination of elements within the torus are clearly observed. Severe local effects of thermal excursions are seen on protection plates.

"Detritus recovered from the vacuum vessel includes metallic droplets and films, and fibrour material, probably from clothing."

(Lomer 1985)

Such events may have a number of implications, and it is reasonable to assume that such problems will not diminish when using higher energy plasmas and tritium fuel. This may be exaggerated by operational modes involving routine first wall interaction (limiters and X-point operation) currently being explored in an attempt to improve confinement times. If impurity levels get too high, then it becomes difficult for fusion reactions to take place. Reducing the level of impurities caused by such events is not at an advanced stage so little can be said about the methods that could be used, except perhaps that it will not be easy. If it proved necessary to clean the reactor regularly that would be costly. There are also implications on the environmental side and for the fuel cycle.

(b) Wall Materials.

The question of wall materials in fusion reactors was dealt with in some length in "Fusion and Fast Breeder Reactors" ((IIASA 1977) and (Brandt et al 1980)), and this study serves to indicate the main issues in reactor engineering. In the section on 'Effects of Fusion Reactor Environment on the Properties of Materials', the following problems are mentioned with a brief discussion of the level of importance and knowledge about the behaviour of the materials in fusion reactor conditions. We briefly summarise the section below;

Problem.	Importance.	Level of Knowledge.
1. Dimensional Instability.		
(a) Swelling due to Voids	High	Virtually none
(b) Swelling due to Gas Bubbles	High	Reasonable
(c) Growth [in Graphite]	High	Virtually none
2. Mechanical Property Changes That Could Be Important in CTR Materials.		
(a) Ductility	Very high	Only for SS316
(b) Potential Creep Problems	High	Virtually none
(c) Fatigue	Very high	None (except for Mirrors)
3. Some Physical Properties of CTR Materials That Depend on Radiation Damage.		
(a) Electrical Resistivity	Moderate	Poor
(b) Radiation Damage to Superconducting Magnet Materials	Moderate	Reasonable

Further:

> "The degradation of materials properties by neutrons results in at least the six following major effects:
>
> (1) reduced efficiency;
>
> (2) reduced plants factors;
>
> (3) increased capital costs;
>
> (4) increased operating costs;
>
> (5) increases in the volume of radioactive waste which must be processed and stored; and
>
> (6) demand on scarce elements"

The Chapter concludes;

> "Undoubtedly more problems will be identified in the future. We must, therefore, reluctantly conclude that, next to the plasma physics problems, radiation damage is the second most serious obstacle to the commercialisation of fusion power"
>
> (IIASA 1977)

It seems to be the case that knowledge about the behaviour of materials under high MeV neutron bombardment is sorely missing in the enginerring phase of the fusion programme. The US Department of Energy, in conjunction with Canada, Japan, and Euratom (including Sweden), did plan to build to build a Fusion Materials Irradiation Test Facility and the majority of the design work was completed. However, in the context of "a general contraction of energy RD&D budgets in most IEA countries" (IAEA 1976), the projected $220 million cost was too high, and the project was shelved in 1985. The US programme does not currently include building an FMIT facility until the early 2000s, assuming Congress grants them the funding they require (US DOE 87).

The constraints on the first wall engineering are such that a critical variable becomes the life-time of the first wall, both in environmental terms and cost terms. There is a major trade-off over the thickness of the first-wall tiles. The tiles have to be kept thin to allow the neutrons to pass through to breed tritium in the blanket. The DEMO design estimates that the proposed first-wall reduces the tritium breeding ratio (the ratio of tritium bred in the reactor to the amount used as fuel) by 13%. On the other hand, the thinner the first-wall the more frequently it will need replacing which will obviously have effects on the availability of the reactor and thus on the generation cost. The need to replace the first wall before the reactor has reached the end of its useful life has long been recognised as necessary from an engineering point of view. The length of time usually quoted for first wall replacement is somewhere between 2 and 10 years. However, from detailed reactor design studies it would appear that conditions are so extreme in the first wall of a fusion reactor that not only is replacement after two years being considered, it has actually emerged as a TARGET, and one that may be hard to actually achieve. The first-wall design chosen in the DEMO study is in accordance with a target of replacement every two years. When referring to the problem of ductility due to the large Helium generation rate in a fusion reactor, the IIASA study makes the following observation;

> "Therefore, it is difficult to place a definitive wall life, unless one were to use the most pessimistic data. Such an approach would yield a life of two to three months in a reactor like UWMAK II. If one uses the U.E. design limit of one per cent [elongation], the situation becomes much worse. IN FACT IT IS QUITE POSSIBLE THAT THE WALL LIFE WOULD BE LESS THAN TWO YEARS EVEN WITH THE OPTIMISTIC DATA.... The whole point of this exercise is to point out again that the high helium generation rate will probably place an upper temperature limit on the first wall life, regardless of the corrosion or creep behaviour of the material, Secondly, THE CHOICE OF DESIGN LIMIT CAN ONLY CHANGE AN IMPOSSIBLE SITUATION (wall life is less than two months) INTO A DIFFICULT ONE (wall life is of only a few years), depending on the assumptions of tolerable ductility." [Our emphasis].
>
> (IIASA 1977)

The environmental and cost implications of such a short first wall replacement time will be explored below.

4.2.2 Tritium Handling.

Tritium will be present in a number of areas in a fusion reactor. It will be present in the fuel and exhaust, in the breeding blanket, it will need to be stored, and it will present in varying quantities in the reactor structure. Tritium handling is a problem due to its ability to permeate through solid structures, particularly Niobium, Vanadium and Titanium refractory alloys. Its permeability increases with temperature. The seriousness of this quality of Tritium depends to a certain extent on the material chosen as the coolant in a fusion reactor. To overcome this permeation problem it is proposed to oxidise it upon leaving the breeding elements and then carry it in the coolant as tritiated water. However, tritium is extremely costly to remove from water (Stacey 1984). A dual-purpose coolant is proposed to both remove the Tritium from the breeding blanket and the heat from blanket, methods will have to be devised to remove it quickly and cleanly. One way of doing this under study is to remove the tritiated water by allowing approximately 1% of the coolant flow to be diverted to a tritium extraction plant and remove it the tritiated water by means of molecular sieve beds. Costly precautions will have to be taken to ensure it does not present an occupational hazard to those in the containment area. Tritium storage could also present a large and costly problem, as could the need to transport it. The largest potential problem with Tritium could well be due to the need to build a large enough Tritium stock to fuel a fusion power programme. This will be dealt with under the fuel cycle.

4.2.3 Tritium Breeding.

Tritium can only be bred from Lithium. Natural Lithium is a compound consisting of approximately 7.5% Lithium 6 and 92.5% Lithium 7. Unfortunately, the rarer of the two natural Lithium isotopes, Lithium 6, breeds much more readily than Lithium 7. The ratio of Tritium bred to that "burnt" in the reactor is known as the Breeding Ratio (BR). As the price of Tritium is somewhere around $10,000/g (Stacey 1984), for good economies it is essential that the Breeding Ratio is high enough to supply the Tritium needs of a developing fusion reactor programme. However, the ability to produce a sufficiently high Breeding Gain to offset not only programme requirements but also various losses due to extraction/reprocessing inefficiency and the short Tritium half-life (12.36 years), have been shown to be severely constrained by engineering factors. For example, in the context of the requirements of obtaining an acceptable breeding gain in a solid breeder fusion reactor an IAEA report of an UKAEA discussion reports;

> "Practical design solutions for the first wall were found to be heavily constrained by mutually

> conflicting requirements. It is desirable to keep the wall thin to minimise neutron absorption and thermal stress. On the other hand, IF A MAINTENANCE CYCLE OF 2 YEARS BETWEEN REPLACEMENTS IS ADOPTED, the wall has to be initially thick, if made of stainless steel as in INTOR, to allow for sputtering damage under normal and disruptive operation and to reduce the rate of tritium permeation from the plasma to the coolant." [Our emphasis].
>
> (IAEA 1985A)

The solid lithium metasilicate breeder concept in the DEMO study requires Lithium enriched to 30% Lithium 6 (higher enrichment is obviously undesirable from a cost perspective) contained in breeding elements containing 80% beryllium (a neutron "breeder") with a two year blanket replacement period to obtain an acceptable global breeding ratio (target: approximately 1.1). With neutral beam injector windows (reducing the possible breeding area) in four of the twelve reactor modules, a breeding ratio of only 1.19 + or - 0.004 is obtained, despite the fact that the DEMO study did not allow for any of the breeding area to be taken up by diagnostic equipment in an attempt to reach the 80% cover target. If it proves impossible to use the area under the divertor for breeding this falls to 1.023. Such low breeding ratio figures are worrying when one has take into account tritium losses in the extraction/reprocessing procedure and the fast decay rate of tritium. An alternative liquid breeder concept was developed in DEMO, lithium enriched to 50% lithium 6 and mixed with 70% lead (also a neutron breeder) in breeding cans. The global breeding ratio obtained by this method is 1.117 + or - 0.003. Lead is not a favourable material because it becomes radioactive and it forms a more corrosive mixture than lithium itself when mixed. The resource implications of using enriched lithium and beryllium are not insignificant (for example, one requires 12g of natural Lithium per gram of 90% enriched Lithium (IIASA 1977)). Both solid and liquid breeder concepts make use of beryllium to enhance the breeding process, Beryllium being neither abundant, cheap nor clean.

4.2.4 Extraction/Reprocessing.

There are two types of extraction involved in a fusion reactor. Firstly, there is the extraction needed to re-enter spent fuel into the reactor. In a D-T Tokamak reactor it is envisaged that the spent fuel will consist of between 85-90% D-T, between 1-10% Hydrogen, between 5-10% Helium 4, Oxygen, Nitrogen and Carbon will make up 1% and 0.01% metallic impurities (Stacey 1984). These are necessarily contingent figures given the discussion particularly on wall interactions. This extraction process is envisaged thus;

> "the metallic impurities can be removed from the exhaust by electrostatic precipitation, thus

> leaving a stream of hydrogen atoms and gaseous impurity atoms (C, O, N, He, etc). Oxygen, nitrogen, and carbon are present mainly in the form of the chemical compounds water (DTO), ammonia ($N(D,T)_3$), and methane (CD_2T_2) respectively. After separation from the main stream, all these compounds must be chemically dissociated to separate and recover all the chemically combined tritium and discharge the impurity atoms to a tritium-free waste"
>
> (Stacey 1984)

In the DEMO study there is no reference as to how this this might be achieved, there is no treatment of impurity removal, isotopic separation in the plasma or coolant streams.

The second form of extraction is involved in the breeding process. Given the large number of possible breeding methods, coolants and structural materials, it is hard to be clear about the likely problems associated with this area. However, it does seem to be emerging that there will be a need, given the difficulties of obtaining an adequate breeding gain, to actually reprocess the breeder elements, particularly solid breeders (they appear to be more prone to swelling and distortion) and those components using Beryllium (IIASA 1977).

4.2.5 Magnetic Confinement.

It is generally accepted that super-conducting magnets will have to be used in a fusion reactor as they are much more efficient and are able to reach very high magnetic fields. It is however, a relatively new technology and producing fields greater than 12 Tesla is an engineering challenge in a fusion reactor environment. The main problems anticipated in such a project are the problem of keeping the magnets supercooled (to approximately 4 degrees Kelvin) in such a hot environment, and also that superconducting compounds "go normal" if subject to too much neutron irradiation. It has been estimated that the superconductors will require more than 1m of shielding to escape this kind of threat (Stacey 1984).

4.3 Conclusion.

We have seen how environmental and engineering trade-offs have tended to concentrate around certain key variables, such as wall-life, tritium breeding, etc. It remains to consider the relationship these factors have with costs. There is no simple model appropriate to understanding such a relationship, interdependence between the variables, rather than dependence or independence is the rule.

We have seen that there seems to be little to be gained at this stage, with the limited amount of information available, to attempt to attach specific costs to elements of fusion power electricity production. However, if one took certain generic features of fusion reactor designs then one can understand the type of cost structure associated with fusion. This is a task that can usefully undertaken at this stage. In the absence of such an attempt, the table below attempts to summarize the type of trade-offs associated with key variables and indicate the direction that reactor design appears to be going in.

Table 4.3 Summary of Reactor Design Trade-offs.

VARIABLE	BENEFITS	COSTS	COMMENTS
Wall Materials			
SS 316. Ferritic steel	Cheap. Relatively well understood. Good with He.	Long half-life.	Ferritic appears to be favoured as so little is known about alternative materials behaviour under heavy neutron
Nb, Ti, V Alloys.	Short-lived activation products. environment.	Expensive, rare. Unknown behaviour in fusion reactor bombardment.	
Coolants/Tritium Breeding.			
Water.	Good thermal conductivity.	Difficulty with tritium extraction. High pressure.	Dual purpose Helium coolants seems to be preferred in recent European reactor designs.
Lithium.	Simplicity. Excellant thermal conductivity. Well known.	Pumping problems in high fields. High chemical reactivity. Corrosive	
Helium.	Very stable inert gas.	More complex fuel cycle. High pressure. Poor thermal conductivity.	
Breeding Materials.			
Liquid. (Li/Pb)	Relatively high breeding ratio.	Highly corrosive.	
Solid. (Lithium metasilicate)	Less mobile. Less corrosive.	Hard to obtain good breeding gain. More distortion.	

CHAPTER FIVE SAFETY AND ENVIRONMENTAL ASPECTS OF FUSION POWER

5.1 INTRODUCTION

There is a major problem with any attempt to assess the environmental effects of fusion power in that there is not even one specific technology that can be assumed to represent the real situation that will prevail if and when commercial fusion reactors are in place. However, at any one time, as R, D & D work evolves, there is a current set of technologies which are seen as being most likely to solve the scientific and engineering problems confronting fusion scientists. Whilst these technologies may in the end prove to be technically and/or financially unviable or other technologies prove to be more attractive, it is nevertheless both possible and necessary to identify and evaluate probable environmental impacts of specific technologies from their earliest point of consideration and as designs evolve, so environmental impact assessment needs to be incorporated as part of the design and development process.

This section reviews work that has been carried out recently relating to the potential environmental impacts of currently favoured fusion technologies and also identifies gaps in the assessment process as hitherto developed. The variety and complexity of technologies currently under consideration is such as to make it difficult to subdivide the analysis satisfactorily. On consideration we have decided to look first at problems associated with the fuel cycle, then to look at reactor associated problems and finally to draw the conclusions from these in terms of health hazards. The whole analysis is preceded by an overview that distinguishes 'current' concerns from potential concerns relating to alternative technological developments. The analysis ends with an assessment of needs with respect to further work.

5.2 GENERAL ISSUES

5.2.1 What Technology?

A number of possible technological configurations have been devised to achieve controlled nuclear fusion for purposes of extracting energy and experiments have been devised to test their feasibility. These include 'stellarators', 'tokamaks', laser, mirror and reverse field pinch reactor. Whilst there is at this time no guarantee regarding which of these will prove the most effective (or if any of them will) nevertheless, the great majority of research is currently going into the development of tokamaks and hence there is more information with which to assess the potential environmental impacts of these and at this stage they seem the most likely to achieve commercialisation. This analysis is therefore entirely focussed upon this technology.

It has been hoped since the early days of fusion research that ultimately there will be a possibility of fuelling the controlled fusion process entirely on the hydrogen isotope deuterium (the D-D reaction); this might provide almost limitless quantities of energy from a source which is not itself radioactive. The more accessible route to fusion power is via a reaction between deuterium and a further, radioactive, hydrogen isotope tritium (the D-T reaction). Tritium is not found in nature, but can be produced by irradiation of lithium. Currently all work on nuclear fusion is focussed upon the eventual use of tritium, derived from lithium, as fuel and the following analysis therefore looks exclusively at this option. However, it should be noted that the notion that it might be acceptable to initiate a major D-T based fusion programme in the hope that experience will lead to the development of feasible D-D technology, and that the result will be free of radiation problems must be guarded against. The fusion process itself creates radioactive products and so a D-D reactor would not necessarily generate a lower radioactive inventory than a D-T reactor (1).

As noted elsewhere in this report, the feasibility of achieving a net energy gain from controlled nuclear fusion is as yet unproven. Whilst progress has been steadily made towards this objective, many unforeseen problems have been encountered on the way and there is as yet no guarantee that it is achievable. From the point of view of the assessment of environmental impacts of an eventual commercial reactor, the implications of this are that the achievement of fusion power may yet involve significant changes in technology as yet quite unforeseen. The apparent concreteness of technological options presented below must therefore be tempered and seen as no more than best guesses on the basis of current developments.

Nevertheless, a significant number of experimental reactors are in existence, albeit insufficiently developed to achieve 'system breakeven', which provide some basis for environmental assessment. Furthermore, a number of extensive design studies have been undertaken both as a basis for the next generation of experimental reactor and as a first attempt to estimate the engineering, and to a lesser extent the economic and environmental, parameters of a possible future commercial reactor. The following assessment is focussed predominantly on the possible impacts of a future fusion economy and hence is based on the technical parameters that have arisen from these engineering studies, especially, the United States 'Starfire' project (2) and the joint European 'DEMO' conceptual designs put forward by UKAEA (3).

5.2.2 What Materials?

The construction of fusion power stations and supporting facilities will raise a number of environmental issues including, inter alia, ecological, visual and social impacts, which cannot be very well estimated prior to the development of discrete power station proposals. However, generically the environmental assessment of fusion power is dominated by the question of radioactive release. This, in turn, is crucially determined by the inventory of radioactive materials which will be contained in and/or generated by a fusion power station and its ancilliary facilities. By the time designs for particular commercial fusion power stations are being made, the inventory of radioactive materials will have been well worked out and it is decisions with regard to these materials which need above all to be analysed with respect to environmental impacts.

The choice of materials is affected by several parameters. These include: engineering variables such as conductivity, resistance to heat and abrasion from the plasma and neutron penetration characteristics; cost and availability of materials; the potential hazard involved in their use; and their potential environmental impact. Choices therefore involve complex compromises and trade-offs with no objective criteria as to the relative weight which should be given to the various factors involved. The tendency hitherto has been to seek out any materials which will achieve the basic scientific and engineering objective of 'energy breakeven' and to assume that less hazardous and less environmentally damaging materials will be found in process of developing a commercial reactor, once the first objective has been achieved (4). There is a danger in this approach of over-commitment to certain materials and an inbuilt disregard for hazard and environmental impact developing within the institutionalisation of fusion power and it is therefore advisable, as discussed further at the end of this section, to introduce a more structured approach to hazard assessment and environmental impact analysis integral to the development process. Specific materials currently under consideration for various parts of the fusion reactor and related plant and functions are discussed in more detail in the relevant subsections below.

5.2.3 What Criteria?

Hitherto practically all the serious work carried out on the potential environmental impacts of fusion power, in so far as this has attempted any comparison with alternative means to achieve the same ultimate energy goal, has evaluated it in relation to fast breeder reactors and the universal conclusion has been that fusion power is likely to be more environmentally benign by a very substantial margin. This procedure is well-illustrated by the US ESECOM study (Holdren et al,1987).

The reasoning behind this restricted comparison has been that by the time fusion power has reached the stage of commercialisation - perhaps during the second quartile of the next century - 'conventional' energy sources, including fossil fuels and fission energy, will no longer represent a feasible option.

However, In summarising their views on environmental questions as part of a major review of the possibilities for fast breeder fission and fusion power carried out within the framework of the International Institute for Applied Systems Research, Hafele et al wrote:

> 'It is possible to envision fusion systems in which many of the most important environmental advantages compared to fission do not materialise...The pitfall is that the desire to bring fusion to commercial fruition in time to compete in the transitional time frame may lead in fusion programmes around the world to a disproportionate emphasis on early engineering feasibility at the expense of potential environmental advantages.' (5)

It is therefore necessary to look at fusion in a broader framework that will overcome the tendency to generate a race between fusion and fast breeder fission technologies. There are certainly possibilities to develop a broader front for the solution to future energy requirements, including in particular major investment in efficiency measures and renewable energy technologies. These, too, will have environmental impacts as well as engineering and economic problems to be solved. But they all address the same issue and should be evaluated in the same framework. If the criteria used to evaluate the potential environmental impact of fusion power are restricted merely to that technology or to a slightly broader set that encompasses fast breeder fission power, then major possibilities for solving the energy needs of the next century in a more environmentally benign way may be neglected.

5.3 THE FUEL CYCLE

The basic D-T reaction involves the fusion of one atom of deuterium with one of tritium to produce an atom of helium (He4) and a neutron; although very substantial heat is required to initiate the reaction (at least one million and perhaps one hundred million degrees centigrade), once triggered there is a very large net heat gain. As already noted, tritium is not found in nature but can be produced by the irradiation of lithium. This reaction involves the splitting of lithium atoms to produce helium and tritium. The only radioactive substance involved is tritium, with a half-life of 12.3 years. However, the neutrons produced by the fusion reaction affect an activation of many of the materials in the vicinity of the reaction, including reactor walls, associated machinery and even the air surrounding the reactor - that is to say that atoms of various materials in these components are converted to atoms of other materials, some of which are radioactive. In this subsection, the discussion focuses on the problems associated with the generation and circulation of tritium; the next subsection then deals with problems arising from the irradiation of the reactor and associated questions.

5.3.1 Tritium Production and Processing

Chemically tritium possesses the properties of hydrogen. At normal temperatures it is therefore a gas with a high diffusion propensity: it is thus difficult to contain in gaseous form, passing readily through structural materials. However, it reacts easily with many materials and oxidises to form 'tritiated water' (HTO). Whilst easier to handle, tritiated water is 25,000 times more hazardous than tritium in gaseous form.

The assumption of current work on fusion power is that the tritium fuel cycle will be contained almost entirely within the confines of the reactor site. A new reactor will require initial fuel to be transported to site and the Starfire project estimated this to comprise some 10 Kg for a 4,000 MW(th) reactor. All further tritium fuel will be generated on site in the fusion process. Immediately behind the inside lining of the reactor vessel - termed the 'first wall' - a thick layer, or 'blanket', of lithium will be located. This will thus be irradiated by the reaction inside the vessel and the tritium produced will subsequently be introduced into the reactor as fuel. Besides breeding tritium, the first wall and blanket must also collect the heat from the reaction to be conducted away for electricity production.

There are various possible configurations for containing the lithium, removing the tritium and removing the heat, all with their own advantages and disadvantages. An elegant engineering solution involves the use of circulating liquid lithium metal as breeder, tritium removal medium and coolant and some early designs were made on this basis. However, liquid lithium is an extremely reactive material, combining at normal temperatures with all reactive gases, with water and even concrete. Mainly because of this, more recent designs have substituted other coolants. Nevertheless, thought is still being given to the use of liquid lithium, in alloy form, at least as breeding material; further consideration is being given to the use of solid lithium alloys as breeder. However, it may prove necessary from an engineering or economic standpoint to us liquid lithium in an eventual commercial fusion reactor. Hence it cannot yet be assumed that this potential hazard has actually been overcome.

For the Starfire project, water has been chosen as coolant and helium for extraction of tritium from a blanket made up of pellets of lithium aluminium oxide (LiAlO2). The UKAEA DEMO studies have considered a solid lithium metasilicate (Li2SiO3) and a liquid lead/lithium alloy as alternative breeder possibilities. In both cases helium is proposed as coolant and for tritium removal. The use of a single helium circulation both for heat and tritium removal is complicated by the large volumes involved and by the diluteness of tritium; there is also a hazard involved in tritium circulating in this way in gas form and the intention is to oxidise it at source. For practical purposes only a small volume of the coolant would be detritiated during any one helium cycle and this would mean that the helium coolant would contain a significant inventory of tritiated water throughout the circuit. The level of tritium deemed acceptable by the designers is 10 grams on the assumption that a total loss of coolant accident would release no more than 10 grams of tritium; this is discussed further in subsection 5 below.

The complete tritium processing facility in a fusion power station is a complex arrangement involving three initial streams and then storage and fuelling arrangements. Exhaust gases from the reactor will include significant amounts of tritium which must be extracted through reprocessing. There are then the arrangements for extracting tritium from the helium purging system. Finally it will be necessary to include a system for extracting tritium that has found its way into the atmosphere inside the reactor containment structures and associated facilities.

5.3.2 Tritium Inventory and Losses

The total tritium inventory of a fusion plant will thus be made up of a number of separate elements which in general could include the following:

- plasma and vacuum system;
- cooling and tritium extraction circuits;
- fuel processing plant;
- blanket;
- storage;
- in general circulation.

Various national teams contributing to the international INTOR reactor studies have made separate estimates of the possible total tritium inventory of a plant and its distribution in various plant components (6). These estimates vary from 2.5 Kg to 3.9 Kg. However, the distribution between components estimated by the various teams varies considerably and is clearly influenced first by the configuration of the technology - and in all cases this is currently little more than notional. The inventory for the Starfire project is estimated somewhat higher (this being envisaged as a fully commercial facility). The blanket alone is assumed to contain 10 Kg of tritium and the rest of the system to sum to about 2 Kg.

INTOR ESTIMATES OF TOTAL TRITIUM INVENTORY IN A FUSION PLANT VARY FROM 2.5 TO 3.9 KG; THE 4,000 MW(TH) STARFIRE REACTOR WOULD POSSESS A TRITIUM INVENTORY OF 10KG.

Turning now to the question on losses and releases for tritium from a fusion power station, we first look at routine releases and then at non-routine releases resulting from accidents. It has already been noted that tritium in gaseous form diffuses through structural material. This means that some routine release of tritium from operating fusion plant would be inevitable. In addition to this some losses are likely to occur along the processing and fuelling path.

Although there is as yet no experience of fusion reactors operating with tritium and although nuclear fission does not employ tritium as a fuel, nevertheless, heavy water fission reactors do generate tritium through neutron absorption by the deuterium water. Pickering, a large CANDU nuclear facility, possess a tritium inventory of 3.5 Kg during normal operation. Losses from this facility averaged 5 ppm/day between 1977 and 1981 (7); this represents almost 1/700th of the total inventory released in the course of a year.

The procedures through which tritium will be handled in fusion power stations is clearly more elaborate than that encountered in heavy water nuclear plants. Currently work is proceeding on investigating the practicalities of tritium handling in a test assembly at Los Alamos in the United States (8). Meanwhile, however, although objectives are being set for limits to routine tritium release from future commercial reactors - an objective of 0.5 gram per year for all operating phases has been set by the Starfire project (that is 34 times less per unit of tritium inventory than the existing Pickering CANDU facility) and of 0.36 gram per year in the EEC funding proposal (p.7) - it is not possible to make any useful assessment of what might be practicably achievable in a future fusion power station by way of limiting routine tritium releases. There is also no assessment of the levels of tritium to which workers in a fusion plant might be subjected (9) although there is a recognition for the need to install equipment with which to detritiate the atmosphere within the containment and to deal with accidental tritium releases.

Attempts to conjecture accident scenarios for fusion power stations are as yet little developed. In the next section there is some discussion of possible reactor failure scenarios; here we look briefly at possible maximum releases of tritium due to an accident. As already noted, the DEMO design includes a limitation on the total tritium inventory within the coolant to 10 grams explicitly to reduce the possible release ensuing from a total loss-of-coolant accident to this amount. An estimate was also made of a maximum of 270 grams of tritium contained in the tritium extraction plant at any one time. However, no estimate for tritium inventory in other parts of the plant has been provided. THERE IS THEREFORE INSUFFICIENT INFORMATION TO MAKE ANY JUDGEMENT ABOUT POSSIBLE MAXIMUM TRITIUM RELEASES FROM AN ACCIDENT ASSOCIATED WITH THE DEMO REACTOR AS CURRENTLY DESIGNED.

An attempt has been made in the context of the Starfire project to categorise components of the tritium inventory as being 'vulnerable' or 'non-vulnerable'. The 10 Kg of tritium trapped within the blanket is considered to be non-vulnerable - ie relatively immobilised even under major accident conditions. The total vulnerable inventory that might be released under conditions of multiple failure amounts to under 400 grams. It should also be noted, however, that an accident in the transport of tritium fuel to a new plant might involve as much as 10 Kg. The EEC funding application (p.6) has assumed a maximum conceivable accidental tritium release to be 200 grams. This is more than an order of magnitude lower than accident scenarios in some other studies.

5.4 REACTOR ASSOCIATED ISSUES

A tokamak fusion reactor is a circular tube within which a ring of deuterium and tritium gas, in the form of a plasma, is brought up to a temperature and confinement pressure where it fuses giving off a substantial net energy surplus. The tube is evacuated of all other gases and the plasma is necessarily held away from the walls of the tube: no solid material can withstand a fraction of the temperature at which the plasma burns and, indeed, if the confinement of the plasma is destabilised and collides with the wall, local areas of the wall material may be brought to the boil. However, the amount of gas present in the reactor at any one time is extremely small so that destabilisation of the plasma quickly leads to dissipation of the energy which it contains while burning.

As already noted, the reactor is lined on the inside by a 'first wall' which must withstand severe radiation and heat as well as abrasion from 'sputtering' plasma. The wall is interrupted in places by ducts associated with maintenance of the vacuum and the processes whereby the plasma is initially heated to the point where the reaction becomes self-sustaining. There are also intrusions into the reactor space, the most important of which is a line of baffles associated with the control of the plasma profile, the insertion of fuel and the extraction of exhaust gases, helium and unburned fuel. These baffles, termed 'limiters' or 'diverters' depending on their configuration and precise functioning, are subject to particularly severe operating conditions.

Immediately behind the first wall comes the complex structure of the blanket and cooling system referred to in the previous subsection. This comprises a large number of lithium breeder containers and pipework for the circulation of coolant and tritium purging medium. The whole is contained in a reactor wall designed both to contain the vacuum and provide a shield against radiation. Confinement of the plasma to the centre of the reactor is effected by a series of magnets (poloidal and toroidal) situated immediately contiguous to the reactor vessel. In current tokamaks (JET eg) these magnets are electromagnets requiring large electrical currents. Future designs utilize supercooled electromagnets involving associated cryogenic systems (liguid nitrogen and helium). The reactor is connected by many ducts, pipes and cables to its associated systems, but must remain accessible for maintenance purposes.

The simultaneous solution to the requirements of all these systems necessarily raises a multiplicity of imperatives and constraints which must be weighed up and fitted together. A number of key issues immediately arise in relation to questions of safety and environmental impact. The first concerns the consequences of the irradiation of the materials from which these complex systems are made. The second concerns the consequences of an accident in any one system or an accident involving the interaction of various systems.

5.4.1 Structural Activation

IRRADIATION OF THE STRUCTURAL MATERIALS INSIDE THE REACTOR LEADS TO THE BUILD-UP OF RADIOACTIVE ISOTOPES. THIS WILL MEAN THAT AT THE END OF THE LIFE OF THESE REACTOR PARTS, THEY WILL CONTINUE TO GIVE OFF HEAT AND REQUIRE ACTIVE COOLING FOR SOME YEARS AND IN THE LONGER TERM, EVEN WHEN ACCEPTABLY COOL, WILL CONTINUE TO BE RADIOACTIVE. Different materials display very different radioactive properties under these circumstances and in principle it would be possible to select materials which minimise the residual heat and radioactivity.

As noted at the outset of this section, minimisation of environmental impacts is only one of several considerations which enter into the decisions with regard to materials choice. A whole range of engineering constraints must be considered and once commercialisation is more seriously under consideration questions of materials availability and cost will also enter into the decision-making process.

In the most coherent attempt to date to analyse the possible safety and environmental aspects of fusion power, the ESECOM study (Holdren et al, op cit) conjectured eight different reactor types using different combinations of materials and investigated the environmental impacts of these. However, currently, technological considerations dominate work on fusion power and it is worth looking briefly at these in order to illustrate why nothing approaching optimal environmental effects may in the end be achievable. Clearly the whole reactor vessel must remain mechanically robust under all operating conditions. The immediate inside surface will be subjected to extremely high radiant temperatures and abrasion from plasma 'sputtering'. AS YET THERE IS LITTLE MEANINGFUL CONCEPTION OF THE DEGREE TO WHICH THE PLASMA WILL BE CONTAINABLE UNDER CONDITIONS OF NET ENERGY GAIN AND THUS THE CONDITIONS WHICH THE FIRST WALL SURFACE WILL HAVE TO WITHSTAND UNDER OPERATING CONDITIONS. So designs are currently carried out based upon assumptions with regard to what will be feasible.

If energy breakeven is achieved and design commences aimed at an eventual commercial reactor, the first wall will need to possess a surface coating adequate to protecting the main wall material from abrasion which might release impurities into the plasma. The main wall material - perhaps in the form of tiles - will then be connected back to the blanket. Several materials are likely to be involved in these structures. However, the differential temperature across the first wall and blanket will be extremely large and so materials must be chosen which minimise differential expansion. It is hoped that commercial fusion reactors will be able to operate in a 'steady state', that is that fuel can be supplied and impurities removed on a continuous basis. However, it seems more likely that it will be necessary to operate reactors in a 'pulsed' mode, with fuel burn taking place for only some seconds or minutes at a time, the temperature then being reduced, impurities removed and new fuel inserted, before bringing the reactor back up to power. In this pulsed mode, first wall metal fatigue will be extreme and clearly this will have a further limiting effect on the choice of materials.

The first wall and blanket structural materials will also have to possess good properties of conduction for heat and neutron flux in order to facilitate removal of heat for electricity generation and irradiation of the lithium blanket. A further problem arises through the forms of transmutation which different structural materials undergo through neutron bombardment. For instance although copper may be a useful material for certain structural purposes, under neutron bombardment it is converted to nickel which reduces its thermal and electrical conductivity; in other cases, structural strength is impaired.

TRANSMUTATIONS IN CERTAIN MATERIALS INVOLVE THE PRODUCTION OF LONG-LIVED RADIOACTIVE ISOTOPES. THIS IS ONLY ONE CONSIDERATION AMONG MANY WITH RESPECT TO THE CHOICE OF MATERIALS FROM WHICH TO CONSTRUCT THE FIRST WALL AND BLANKET OF A COMMERCIAL FUSION REACTOR. At present effort amongst fusion design teams is to obtain a wall and blanket design that will possess an acceptable life under the stringent conditions of commercial reactor operation. The Starfire project estimated that a six year wall life will be achievable on the assumption that steady-state operation is achieved. Under conditions of pulsed operation, assumed by the DEMO project team, the objective is to achieve a two year first wall life.

A number of studies have been carried out into the possible residual radioactivity in fusion reactor structural materials and their decay profiles. Two sets of such profiles are illustrated in Figure 5.1. In practice these profiles will depend on the particular neutron spectrum and flux and on the duration of exposure. Furthermore, many of the problematic isotopes result from the irradiation of impurities in the structural materials which are difficult or impossible to remove or reduce below certain levels (10). The UK National Radiological Protection Board (NRPB) recently completed a study into the radiological aspects of the management of fusion reactor solid waste, focusing on a selection of stainless steels and vanadium alloys that might be used as basic first wall and blanket structural materials (11).

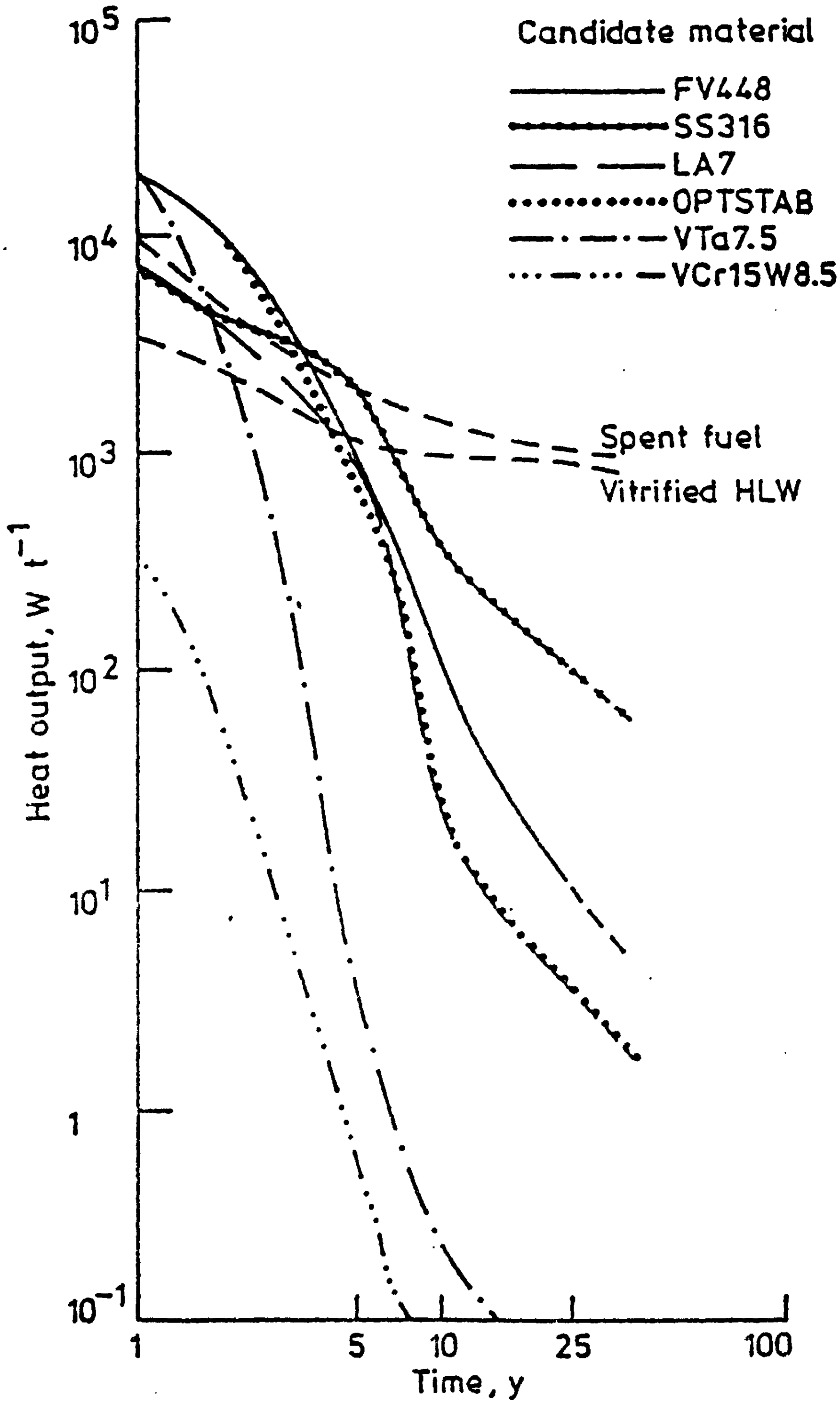


Figure5.1 Heat output of candidate materials as a function of time after removal from the reactor

SOURCE : REF. 10, P. 80

Upon removal from the reactor, all the materials looked at by the NRPB would be classified as 'high level waste', that is they would be heat generating and require storing in conditions involving active cooling. They noted that although it is difficult to be precise about when waste is no longer to be considered heat generating (ie when it changes from high level to intermediate level waste), after five years all the steels, but not the vanadium alloys, would have a heat output of more than one kilowatt per ton. However, even after a hundred years none of the materials would yet have achieved the low level waste category.

THE UK NATIONAL RADIOLOGICAL PROTECTION BOARD HAS STUDIED FIVE ALTERNATIVE FIRST WALL AND BLANKET STRUCTURAL MATERIALS AND CONCLUDED THAT ALL WOULD STILL BE CLASSIFIED AS INTERMADIATE LEVEL WASTE 100 YEARS AFTER REMOVAL FROM THE REACTOR. THE EEC FUNDING APPLICATION (P.20) ON THE OTHER HAND WRITES SIMPLY OF "THE NON EXISTENCE OF IMPORTANT LONG TERM (>100A) POTENTIAL HAZARDS". IT ALSO ASSUMES (P.43) THAT SHALLOW BURIAL WILL BE ACCEPTABLE.

The NRPB considered the possibility of recycling materials rather than disposal. Whilst this would have clear advantages in the first instance in reducing the volume of waste there would be an inevitable penalty in terms of workforce exposure to radioactivity and eventually materials would need to be disposed of and these final materials will probably contain higher concentrations of long-lived radionuclides (12). (The EEC funding application (p.19) took a particularly uncritical view of this possibility). Thought amongst fusion design teams is certainly going into achieving a material choice that will result in the possibility of recycling all reactor elements within 100 years (13) and the Starfire project looked towards the possibility of recycling the reactor shield and all materials outside this within 50 years.

However, it has been necessary for the purposes of the Starfire and DEMO projects to make some assumption about the materials that might be employed in reactor construction. It has been noted that there is no indication that vanadium alloys could be made available on the scale necessary to supply basic structural material for a major fusion programme (14) and neither of the two commercial reactor studies have proposed their use. An investigation of a wide variety of materials for possible use in limiter construction as part of the Starfire project revealed only four which would be capable of withstanding adequately the stress conditions assumed to be confronted by this component; this leaves little room for trade-off with respect to the minimisation of problems arising from activation. For the bulk of the Starfire reactor structure, austenitic stainless steel was chosen.

More complex structural choices were made in the DEMO study. Two blanket options were proposed, one involving a liquid and the other a solid lithium breeder. The first wall tiles in the liquid breeder case are proposed to be made of graphite faced with silicon-carbide/pyrolic carbon combination. No assessment of the activation profile of this material was made. For the sold breeder case, tungsten wall tiles, attached back to a copper alloy substrate lined with nickel alloy (Inconel) coolant tubes is proposed. The DEMO study group concluded that from the point of view of activation, copper and tungsten are acceptable materials (although this is not entirely corroborated by the NRPB (15)). However, activation of Inconel results in the presence of long-lived cobalt and nickel radio-isotopes. Nevertheless, it was concluded that: 'Although the choice of Inconel is unsatisfactory from activation considerations it has been selected for this design because of its good thermal and mechanical properties at elevated temperatures.' (16)

The DEMO text continues: 'It is believed that on the 20 year timescale for DEMO the programme for the development of low-activation materials will have produced a suitable material from all points of view.' This view has been expressed in the EEC funding application (p.8) and, indeed, in papers dealing with the development of fusion reactor design. Whilst it is clearly probable that systematic study of materials is likely to reveal more appropriate alloys from all points of view for the construction of fusion reactors, nevertheless, without reasonable knowledge of the actual working conditions of the first wall and blanket beyond 'energy breakeven', it could equally be conjectured that the range of materials which will eventually prove to be workable is extremely restricted, allowing little option for choice with respect to activation parameters. There is no real rationale for optimism.

5.4.2 Solid Waste Management

It must be clear from the foregoing analysis that an individual commercial fusion reactor and beyond that a programme of fusion power stations will generate very substantial amounts of solid radioactive waste. It is clearly difficult at this stage to know just how much this will come to. The ESECOM study (Table 6, Holdren et al, op cit) looked at a range of eight reactor types amongst which solid radioactive waste varied over plant lifetime by a factor of six. The Starfire project estimated an annual discharge of first wall and blanket structural materials alone of 75 metric tons. THE NRPB STUDY ESTIMATED FOR A FUSION POWER PROGRAMME TO MEET TOTAL UK ELECTRICITY DEMAND AT CURRENT LEVELS AN ANNUAL ARISING OF SOLID WASTE, ASSUMING THE USE OF STEEL, OF 10,000 TONS (IN THE REGION OF 1,250 CUBIC METRES). THIS COMPARES WITH ESTIMATED TOTAL UK HIGH AND INTERMEDIATE LEVEL RADIOACTIVE WASTE ARISING, SINCE THE BEGINNING FROM ALL SOURCES TO THE YEAR 2000 (IE OVER FORTY YEARS) OF 164,000 TONS (NRPB 'Living with Radioactivity').

Some consideration has gone into at least the problem of replacing first wall and blanket structures on a regular basis. It is clear that if they do materialise, commercial fusion power stations will involve very substantial production and processing facilities. Efficient remote handling equipment will need to operate such as to be able to replace reactor parts and especially first wall and blanket on a time scale which does not reduce operation time to an unacceptable level. Whilst the reactor hall would contain radiation levels that would exclude workers during operation, for 24 hours after shutdown and at all times when internal reactor components are being replaced (the EEC funding application (p.19) dismisses this problem), a further substantial part of the facility - termed the 'hot cell' - would also be semi-permanently closed to human access. According to the Starfire study, the hot cell would contain the following activities:

blanket disposal;
solid waste packaging;
holdup treatment of non-tritiated wastes;
remote maintenance of activated components;
decontamination of non-activated components for re-use;
emergency tritium cleanup;
wet and dry storage of activated components.

As is currently the case with nuclear facilities, it is generally assumed that a case by case decision will be made with respect to the disposal route for various radioactive materials and components but it has been conjectured that the bulk high level waste arising from first wall and blanket replacement will be stored in ponds on site until decommissioning (17). It must be stressed at this point that there is as yet practically no experience of decommissioning commercial nuclear facilities of any kind. THE DECOMMISSIONING OF FUSION FACILITIES, WITH THEIR MORE COMPLICATED REACTOR ARRANGEMENT AND HOT CELL COMPLEX AND ABOVE ALL THEIR VERY SUBSTANTIAL INVENTORY OF HIGH LEVEL WASTE, PRESENTS AN AS YET UNASSESSABLE SITUATION (18). The fact that this may be less problematic than the decommissioning of a comparably sized fast breeder fission station is no consolation.

The NRPB report analysed the options for final disposal of the solid wastes arising only from first wall and blanket replacement and concluded that none of the materials they looked at would be suitable for shallow burial but would require either deep geological or deep ocean burial. GIVEN CURRENT PROBLEMS IN FINDING SUFFICIENT DISPOSAL SITES FOR THE RELATIVELY SMALL QUANTITIES OF INTERMEDIATE LEVEL WASTE FROM EXISTING FISSION POWER FACILITIES, IT IS DIFFICULT TO ENVISAGE WHERE THE MASSIVE RADIOACTIVE ARISINGS FROM A MAJOR FUSION ECONOMY WOULD BE HOUSED. In addition, the safe transportation of these wastes presents a further problem of great magnitude.

5.4.3 Reactor Accident Scenarios

Although some thought has been given in recent fusion reactor design studies to avoiding obvious sources of major accident associated with the reactor and related plant, few structured attempts have been made to analyse possible major accident scenarios. Nevertheless, some analysis is available from which a sketch of possible accidents can be drawn. Whilst there is little knowledge of the burning characteristics of plasma within a tokamak, it is generally contended that the low material and energy inventory of the plasma excludes the possibility for an explosive accident originating in the vessel (19). A plasma disruption or 'dump' can certainly result in the melting or vapourisation of a small portion of the first wall if sufficiently focussed, as is demonstrated by events in existing reactors (20) and a further disruption event has led on one occasion in the 120 ton JET reactor vessel being impulsively lifted one centimetre into the air, with consequent distortion of the vessel.

Major energy forces are, however, clustered around the outside of the reactor and accidents initiated by failures here could lead to a sequence of events (21). A localised energy dump in the current driving the magnets or a breach in the cryogenic system serving the magnets could generate missiles disrupting other systems in proximity to the reactor, even if an actual breach of the reactor shield itself were unlikely. A loss of coolant accident is possible and this could, as discussed in the previous subsection, release considerable quantities of tritium into the containment. It would also expose the wall structures to the effects of uncontrolled radioactive after-heating (to guard against which the Starfire reactor has been designed with a dual cooling system). The after-heating in a fusion reactor is not, however, as severe as in a fission reactor, so that a 'melt-down' is unlikely to possess the same immediate consequences at these reactors. Such an event, particularly if associated with a plasma disruption, could nevertheless destroy the reactor interior and if associated further with a vessel disruption, would contribute significantly to a 'maximum accident scenario'.

THE GREATEST HAZARD LIES IN THE USE OF LITHIUM. ALTHOUGH THE PROPOSAL TO USE LITHIUM AS A REACTOR COOLANT HAS BEEN SET ASIDE IN MORE RECENT DESIGNS, IF EVENTUALLY LIQUID LITHIUM IS USED AS COOLANT OR BREEDER THEN A MAJOR HAZARD REMAINS. IF THIS MATERIAL IS EXPOSED TO WATER COOLANT OR THROUGH A BREACH OF THE REACTOR VESSEL TO AIR OR OTHER SUBSTANCES WITH WHICH IT WILL REACT, IT WILL BURN WITH AN INTENSE HEAT, INITIATING FURTHER ACCIDENT EVENTS AND ITSELF RELEASING THE TRITIUM CONTAINED IN THE BLANKET. IF SUCH A SEQUENCE IS ASSOCIATED WITH A BREACH IN THE CONTAINMENT, THEN A CHEMICAL FIRE WHICH RELEASES A SIGNIFICANT PROPORTION OF THE RADIOACTIVE INVENTORY TO THE ATMOSPHERE, ON A SCALE SIMILAR TO THAT AT CHERNOBYL, CAN BE ENVISAGED. THE LITHIUM BLANKET IS PROGRESSIVELY BURNED OUT, VOLATILIZING FIRST WALL MATERIALS WITH THEIR RADIOACTIVE INVENTORY AND DISPERSING THIS TOGETHER WITH THE BLANKET TRITIUM INVENTORY (22).

5.5 HEALTH HAZARDS

It was pointed out at the outset of this subsection that the problem of radioactivity overshadows all other potential hazard and environmental problems arising from a fusion reactor or programme of fusion power. An attempt has been made above, wherever adequate information is available to present quantitative estimates of amounts of radioactive materials that might be contained in a reactor or programme and the potential for their being released into the human environment. This subsection analyses the possible radiological effects of these releases on three categories of people: operators at the plant, the general public living in the vicinity of the plant or in the course of a radioactive plume emerging from a plant accident, and finally, the population as a whole.

5.5.1 Radiation Hazards: Conceptual Issues

First it is useful to say something conceptually about the problem of radiation. Nuclear radiation is generally held to weaken human resistance to disease and more specifically is known to be a direct cause of genetic malfunction and cancer. Much research has been carried out particularly with respect to the last of these and international radiation standards are related to the probability of cancer developing as a consequence of irradiation. We are constantly subjected to a background radiation of which, on average in the UK, some 13 per cent is a consequence of human activity: medical applications, fallout from nuclear bomb tests, occupational exposure and discharges from nuclear installations.

Although natural background radiation is substantially greater than the artificial radiation to which the average citizen is subjected, any increase in radiation as a consequence of human activity is expected to bring with it an increase in the numbers who will die of cancer: there is no experimental evidence to indicate that no matter how small a dose of radiation, there is not some risk of cancer involved. This is the basic assumption upon which international radiation standards operate. Any intervention known or likely to increase radiation exposure of people is therefore to be kept 'as low as reasonably achievable' (known as the ALARA principle).

Analyses of the impact of radiation on people, predominantly derived from the consequences of the atomic bombs at Hiroshima and Nagasaki, have led to the adoption of limits, beyond which radiation workers and the general public should not be exposed. Based upon this internationally recognised work, the UK NRPB considered that it would be unacceptable for radiation workers to have a chance greater than 1 in 2000 per year of developing cancer or for a member of the general public to have a chance of less than 1 in 100,000. Earlier analysis suggested that this would mean that radiation workers should not be subjected to a dose of more than 50 mSv (milli-Sieverts) per year and that no member of the general public should be subjected to a dose of more than 1 mSv per year (Atom 1988 p2/3). This has been the design basis for nuclear facilities until now. However, new evidence has arisen out of an updating of analysis of the Hiroshima and Nagasaki survivors, that is leading to a revision of the dose limits. If the chances of contracting cancer are to be maintained as before, it is now estimated that radiation workers should not be subjected to more than 15 mSv per year and no members of the general public should be subjected to more than 0.5 mSv per year. IN GENERAL THIS REANALYSIS IS CONCLUDING THAT A GIVEN LEVEL OF RADIATION DOSE IS SIGNIFICANTLY MORE LIKELY TO INDUCE CANCER THAN HAD HITHERTO BEEN THOUGHT AND THE UK NRPB IS DOWN-REVISING ITS MAXIMUM DOSE LIMITS FOR RADIATION WORKERS AND THE GENERAL PUBLIC.

Finally in this discussion of the conceptual issues relating to radiation it is necessary to relate the doses received by people to the amounts of radioactivity given off by radioactive substances. Each radioactive substance possesses a distinct pattern of radiation which includes a constant rate of decay and a spectrum of forms of radiation. This is given a general measure, expressed in 'Curies' (although these are being replaced by a new measure, termed 'Bequerels'). Two sets of problems arise in attempting to relate radiation to doses (Bequerels to Sieverts).

Firstly, radioactive isotopes and the many chemical combinations into which they enter do not merely possess a general impact on the human body, but affect different organs and areas of the body in very different ways. For instance radioactive strontium migrates into bone structures and is a potential cause of leukemia whilst radioactive iodine migrates to the thyroid gland, causing thyroid cancer. The range of possible effects of the very large variety of radioactive isotopes is therefore difficult to generalise and in many cases not well known. Secondly, there are many routes which radioactive substances can take from their point of release to the human subject: these include simple dispersion via air or water - substantially affected by weather and hydraulic conditions - to complex biological mobility and concentration chains. Although quantitative models have been devised and are applied to estimate the dispersion of radioactive substances from source and the levels and effects of these on human subjects along their path, it is generally recognised that these are no more than aids to the 'professional judgement and commonsense' of those working in the field (22).

5.5.2 Radiation Hazards of Fusion Power: Tritium

Turning now to the potential radiological impact of fusion power facilities, we look first at problems associated with the tritium fuel. Tritium possess a radioactivity of 10,000,000 curies (1.0 x 107 Ci) per gram. Gaseous tritium does not easily enter the human organism, although it presents a problem of lung irradiation when breathed in with air. Combined with oxygen to form tritiated water, tritium is 25,000 time more lethal, entering the body through ingestion or through the skin and migrating to all parts of the body. However, it passes through on average in ten days (known as its 'biological half life'). Ten curies of tritiated water - one hundredth of a gram - is likely to cause early death in about 50 per cent of a typical population (23); tritium containment is thus extremely important.

In subsection 4 above it was pointed out that no assessment has yet been made of possible worker exposure to tritium inside a fusion power plant. There is no doubt, however, that given the facility with which the gas migrates through structural materials, and the relatively large tritium inventory that such plants will possess, that very stringent measures will need to be applied to keep to an acceptable level the amount of free tritium gas in the plant buildings.

Assumptions have also been made with respect to the routine release of tritium from a fusion plant. In the case of Starfire this is assumed to be no more than 5,000 Ci per year. However, if the same ratio of tritium release to inventory as is demonstrated by the Pickering CANDU plant were to occur at the Starfire plant, then this would sum to 4,700 Ci per day (Hancox and Redpath p4). A number of estimates have been made of the maximum impact on members of the public from regular tritium releases from a fusion power plant, based upon a series of different assumptions. The results, normalised to a daily release of 100 Ci, range from a low of 16.8 uSv (micro-Sieverts = thousandths of a milli-Sievert) (24), to 780 uSv (25). The first of these figures represents about one and a half times current average background radiation exposure from weapons test fallout; the latter figure is over one and a half times the new recommended maximum dose for members of the public and is clearly unacceptable according to that standard. However, it must be stressed at this point that THE AVAILABLE LITERATURE CURRENTLY ADDRESSES ITSELF TO RELEASE LIMITS TO BE ACHIEVED, AND THERE IS LITTLE REAL KNOWLEDGE OF WHETHER THESE WILL ACTUALLY BE ACHIEVABLE IN OPERATIONAL FUSION STATIONS.

5.5.3 Radiation Hazards of Fusion Power: Activation Products

Analysing the potential impact of routine releases of activation products resulting from reactor wall and surrounding atmospheric irradiation is altogether more problematic because the choice and mix of materials is unclear and because the range of activation products resulting from any one of these choices is great. It has been generally contended that the absence of very long-lived actinides (radioactive isotopes of heavy atoms), associated with fission reactors, reduces the radiological risk. Whilst in principle this is correct, the activation products from a fusion reactor nevertheless present a complex long term handling and storage problem. This is severely underplayed in the EEC funding application (p.8).

Some study of potential routine releases of activation products made up of air from the plant buildings, corrosion products from blanket and reactor coolant and storage tank leakage has been carried out for the Starfire project (26) and it has been concluded that, under a range of assumptions, the collective dose for the population within an 80 kilometre radius of the plant will be roughly equal to that associated with tritium releases. There are no estimates of workforce exposure to activation products.

The bulk of radiation from activation will be tied up with the reactor structures. In the first instance, as already referred to in subsection 4 above, removal and on-site storage of first wall and blanket sections and also of other irradiated equipment which might need replacement, will require remote handling procedures to ensure complete isolation from the plant workforce. As yet there is no more than a conceptual outline of how this might be achieved and hence no way to assess the possible radiation hazards to which these procedures might subject the workforce. Nor has any analysis been carried out into the methods and associated radiation hazards which decommissioning and mass solid waste transportation might involve.

The NRPB study did, however, carry out a detailed analysis for six possible structural materials of the potential radiation hazard from various types of waste disposal site (27). These included shallow burial in either simple or engineered (concrete lined) trench, deep geological disposal or deep ocean disposal. Despite the lack of actinides in the wastes under consideration, they still concluded that significant leaching of radioactive isotopes into the ground water system could occur before their radioactivity had sufficiently subsided. Furthermore, accidents, involving boreholes penetrating the waste or excavation for building foundations taking place, following a one hundred year period of surveillance, could lead to short term fatalities. In the case of deep geological burial, they concluded that leaching was unlikely ever to be a problem but that boreholes could conceivably lead to fatalities. Only in the case of deep ocean burial did they consider both hydrological problems and disturbance due to human activity in the future to present no problem. The NRPB did not comment on the problem of finding sufficient sites to store the amounts of solid waste which they envisaged arising through a major fission power programme.

IN THEIR STUDY OF SOLID WASTE MANAGEMENT FOR FUSION REACTORS, THE UK NRPB CONCLUDED THAT ONLY DEEP OCEAN DISPOSAL WOULD PRESENT AN ACCEPTABLY LOW HAZARD.

A further potential hazard arising from activation, that has been highlighted by a number of studies - including the ESECOM study (Holdren et al, op cit, p42), is that of carbon 14. However, the magnitude of this problem is currently little understood, although it is generally recognised that this could be amongst the most significant sources of radiation to the general public from fusion plants.

5.5.4 Radiation Hazards of Fusion Power: Major Accidents

As noted in subsection 4 above, little investigation has yet been carried out into types of accident that might arise in a fusion power plant and the effects which these might have. One attempt has been made by the Swedish nuclear research centre, on the simple assumption that the whole tritium inventory of a fusion station, taken as 4 Kg and dispersed in the form of tritiated water, to assess the radiation effects on the surrounding population (28). If the release were up a 100 metre stack, this might be restricted to a maximum dose of 48 mSv; however, released at 20 metres height, under moderately stable weather conditions, the maximum dose could be as high as 5,000 mSv.

The study went on to analyse possible effects to an actual population assuming that the fusion plant were located at Barseback (site of an existing nuclear power station) in southern Sweden about 25 Km from Copenhagen. This indicated that no more than ten people would obtain exposure over 300 mSv of which two would obtain exposure over 500 mSv. According to this scenario there would be no early deaths but four late cancers could be expected. It was stressed, however, that the study was preliminary and 'should not be taken too seriously'. Furthermore, no consideration was given to activation isotopes that would in all probability be associated with any major accident that would release this quantity of tritium.

Kazimi and Sawdye (29) did focus upon the consequences of a major release of activation products, following from the volatilization of 30 per cent of a reactor wall through a lithium fire and associated with a containment breach. (The EEC funding application (p.20) denies the possibility of such an accident scenario.) They noted that even if 10 Kg of tritium were involved that this would not be a significant factor relative to the radiation effects of the released activation products. The study did not attempt to estimate actual radiation doses in the vicinity of the plant, nor the rate of early deaths and late cancers that such an accident might induce, but compared the accident with the consequences of a maximum accident in a light water fission reactor. Whilst the results are sensitive to the particular materials from which the reactor is built, on those assumed in this study (two alternative materials were looked at) it was concluded that THE RADIOLOGICAL CONSEQUENCES FROM SUCH AN ASSUMED MAXIMUM RELEASE FROM A FUSION REACTOR ARE SUBSTANTIALLY LESS THAN THOSE THAT WOULD RESULT FROM A WORST LIGHT WATER REACTOR ACCIDENT. As light water fission reactors are not the only alternative to satisfying a given energy need, this result is not in itself particularly helpful.

The ESECOM study (Table 8, Holdren et al, op cit) also made great play of the superior characteristics of fusion over fast breeder reactors under accident comnditions. However, that study approached the issue from another angle. For each of the eight fusion reactor types looked at, the study generated estimates of the amount of activation products in the first wall and other structures which would need to be volatilised and dispersed in order to generate off-site early deaths or severe ground contamination. It found that with certain materials only a very small percentage (less than 5%) of first wall material dispersed under accident conditions would be sufficient to cause early deaths, and even smaller amounts (one tenth of a percent) could result in extensive ground contamination.

5.6 A FRAMEWORK FOR ASSESSMENT

This analysis has attempted to accomplish various tasks. A brief outline of existing favoured fusion technologies has been provided, together with conceptual issues relating to these. Attempts which have so far been made to define and analyse the safety and environmental implications of these favoured technologies have then been outlined. Finally some of the most important conclusions reached by these analyses have been highlighted and some further implications drawn. It remains to describe the difficulties encountered in any attempt at this time to present any balanced assessment of safety and environmental aspects of fusion power and to recommend ways in which the situation could be improved.

There is clearly an inherent difficulty in assessing the safety and environmental aspects of a technology as yet only partially developed. In one analogy it was said that attempting any broader assessment of a future fusion economy is like expecting analysts in the first decade of this century to make an assessment of commercial aviation in the 1980s based on the technology of their age. However, given the potential severe safety and environmental problems that could emerge from fusion power development and given the possibility for taking decisions now that would lead to very different energy futures, it is vital that a highly structured process of assessment be designed and implemented at this stage.

Certainly, the safety and environmental assessments which have so far been carried out into fusion power are very fragmentary and focus only on a very narrow range of issues - albeit those that immediately appear as problematic. A further problem with existing material is that it has been largely produced by the research organisations which are carrying out the research into fusion power and which necessarily have a commitment to its success and hence a possible propensity to exaggerate its potential advantages and de-emphasise its potential failings. The restriction of comparisons of fusion power with alternative energy technologies to other nuclear options and particularly fast breeder reactors is particularly problematic in that it gives the impression of alternatives in a situation where these are in practice similar in many respects, whereas the full range of possible future technologies is in practice considerably wider.

There are two areas in which the analysis of future energy options, including fusion power, in terms of safety and environmental effects can be greatly improved. Firstly, proposals can be subjected to a comprehensive probability analysis of accident possibilities and their consequences. The Starfire project involved a major attempt to put together a comprehensive scheme which could be subjected to an all-round economic, safety and environmental analysis and the US ESECOM Committee has initiated a more comprehensive approach to safety, environmental and economic evaluation of fusion technologies. By comparison, the work so far published on the DEMO reactor is too fragmentary to be of much use for such analysis and no structure exists to look into the wider implications of fusion technologies. It was nevertheless commented on by the Starfire team that the primary emphasis of that study had been on deterministic (engineering) rather than probabilistic methods 'due mainly to the timing involved' (30). There was nevertheless an awareness of the need to focus in future on generating sufficient data to perform detailed probabilistic risk assessment.

Reference has been made above to a few attempts that have been made and which indicate lines which can be developed. WE WOULD RECOMMEND THAT ANY FURTHER FUNDING OF FUSION POWER TECHNOLOGIES SHOULD BE ACCOMPANIED BY FUNDING FOR STRUCTURED PROBABILISTIC RISK ANALYSIS OF THE TECHNOLOGIES UNDER SCRUTINY. These studies should be provided with a general structure that is applicable to a variety of non fusion technologies that could potentially substitute in terms of future energy provision. THEY NEED TO BE CARRIED OUT BY ORGANISATIONS WHICH DO NOT THEMSELVES HAVE A DIRECT INTEREST IN THE SUCCESS OF THE PROJECT; on the other hand the results of these studies should be incorporated into the overall structure of research into fusion and more generally into future energy technology development and research. Organisations carrying out the basic scientific and engineering research must have as part of their remit an obligation to supply the risk assessors with adequate information.

The situation with regard to environmental analysis is in some ways similar. No meaningful environmental analysis of a potential fusion power station or programme has yet been carried out. Clearly insufficient information has so far been made available. Nevertheless, it is already quite clear that fission plant and the related materials, transport and waste disposal arrangements, will have very extensive and serious environmental impacts should this technology become widely applied.

It is necessary to develop a generic framework for the analysis of the environmental (including socio-economic) impacts of fusion power now. The UK Department of the Environment's 'best practicable environmental option' (BPEO) framework has indicated the possibilities at least for nuclear waste disposal options and the NRPB was able to make good use of this in their assessment of problems associated with fusion solid waste disposal. However, the required framework must extend throughout the energy supply system to include the power stations and reactors and it must be capable of cross-comparison not merely between nuclear options but also non nuclear options - in particular energy efficiency and renewable energy options.

As in the case of risk assessment, THE ENVIRONMENTAL ASSESSMENT OF FUSION WITH ALTERNATIVE ENERGY OPTIONS MUST BE CARRIED OUT BY ORGANISATIONS INDEPENDENT OF THE RESEARCH ORGANISATIONS DEVELOPING THE VARIOUS TECHNOLOGIES. But it must be able both to require sufficient information from the research organisations and its output must be included in the formulation and development of the research programme itself. It is only through an iterative development of scientific and engineering research into a range of energy technologies that includes fusion technologies in a way that is fully integrated with probabilistic risk analysis and environmental assessment that we can even hope to approach a socially optimal solution to what are clearly going to be difficult years in the future with respect to our satisfying our energy needs adequately.

<u>NOTES</u>

1 Hancox and Redpath 1985, p6.

2 Gross 1984, Chapter 7

3 Cooke and Reynolds 1985

4 eg. Hancox and Redpath, op cit p.9.

5 IIASA 1977, pp18-19

6 Edlund 1983

7 Drolet, Wong and Dinner 1985, 5(1), pp. 17-29.

8 Anderson and Sherman 1977

9 Hancox and Redpath op cit p. 8.

10 Davis and Smith 1987, p2

11 Davis and Smith op cit

12 Davis and Smith op cit p. 34.

13 Hancox and Redpath op cit p. 9.

14 IIASA op cit p. 227.

15 Davis and Smith op cit p. 33.

16 Cooke and Reynolds op cit pp. 4-8.

17 Hancox and Redpath op cit

18 Hancox and Redpath op cit p. 10; Botts and Powell 1977

19 Gross op cit p251

20 Lomer 1985, pp. 18-24

21 Kazimi and Sawdye 1981, pp 87-101

22 National Radiological Protection Board 1986, p. 29

23 Hancox and Redpath op cit p. 3

24 Edlund op cit p. 13

25 Hancox and Redpath op cit p. 4

26 Hancox and Redpath op cit p. 7

27 Davis and Smith op cit

28 Edlund op cit

29 Kazimi and Sawdye op cit

30 Gross op cit p.251

CHAPTER SIX. THE ECONOMICS OF FUSION AS A RESOURCE.

6.1 Approaching Fusion Economics.

6.1.1 Introduction.

Can the economics of fusion be realistically discussed when it has not yet emerged from the Research and Development stage?

It is useful to begin by saying what we mean when we speak of the "Economics of Fusion Power". There are two parts to the answer.

(1) To place the fusion potential in the wider context of the economy. This involves consideration of long term prices, capital markets, resources, etc.

(2) To construct a micro-economic model for fusion costs. This is specific to fusion and taken far enough leads to number crunching results.

The second may not be possible, but if it is, it is only in the wider context (1), not least because it sets the assumptions on which the micro study is made. For a long term technology these assumptions are more important than the numbers that emerge. Indeed unless the assumptions are made clear, the results may be misleading, and even likely to damage the Community's health! While the Commission has done nothing about part (1), it has produced, somewhat perversely, highly numerate statements about part (2). WHATEVER THE COMPULSION THAT LIES BEHIND SUCH AN EXERCISE WE RECOMMEND THAT THE FIRST CONSIDERATION SHOULD BE GIVEN TO THE BASIC QUESTIONS OF HOW THE TOPIC OF FUSION ECONOMICS SHOULD BE APPROACHED.

The expert group qualify their numerate statements by saying:

> "The results given above indicate that generating cost must be used with extreme caution as a measure of future worth of fusion power from D-T driven Tokamaks... It is too early to draw hard and fast conclusions from this analysis."

They add that it will not be before the conclusion of NET (around 2010) that enough is known about fusion costs. Elsewhere they have cast doubt on that, because NET will differ in important respects from the Demonstration plant.

6.1.2 Uncertainty.

THE KEY ISSUE IS HOW TO DEAL WITH UNCERTAINTY. APPRAISAL OF LONG-TERM INVESTMENT PROJECTS IN THE PUBLIC SECTOR CONFRONT THIS PROBLEM BY THE USE OF DIFFERENT METHODS. RISK CAN BE DEFINED IN TERMS OF STATISTICAL PROBABILITY. UNCERTAINTY CANNOT BE DEFINED THIS WAY AND THE USE OF SCENARIOS IS NECESSARY, WITH PROBABILITIES BEING ATTACHED TO THE RESULTS.

1. Setting of the discount rate
2. The use of Cost/Benefit analysis
3. Sensitivities
4. Scenarios
5. Probabilities

The discount rate is the most specific and most sensitive. Cost benefit analysis has the advantage that it yields only a single figure, and this depends on the assumptions used, the quality of the data, the discount rate, and the time span.

Sensitivities are used to show a variation around a limited number of values. They reduce the uncertainty. In investment appraisals, the discount rate will be the most important economic variable.

A probability distribution around a value provides 'judgement' as to how good the value is. They assist the decision-maker, especially with long term projects. It can help decision-makers if the expert commits himself in this way (which he may not want to do because if he is proven to be very wrong, it damages not only his health but his reputation!).

The use of such methods for offsetting uncertainty depends very much on the quality of the data. If it is known that the data is 'soft' then it is more important to use senstivities etc, and eschew single dimensional solutions. Unfortunately the Commission's research compounds both errors. The data is a mixture with much more 'soft' than 'hard' data. To use the Dorfman metaphor this produces a "rabbit-and-horse stew situation." Some recognition of the risks involved, in relying upon such data, and the application of recognised techniques for reducing uncertainty, should have recommended themselves to the Commission's researchers as essential.

DECISION-MAKERS, CONFRONTED WITH LONG-TERM PROJECTS OUGHT NOT TO ACCEPT SINGLE FIGURE SOLUTIONS. (To do so makes them hostage to the expert who not only sets the assumptions, but attaches values to them). A RANGE OF OPTIONS SHOULD BE REGARDED AS A MINIMAL WAY OF OVERCOMING THIS PROBLEM.

The application of the above expedients to grapple with the effects of long term technologies is weakest in the appraisal of environmental factors. Our economic system is ill-matched for appraisal of ecological systems, both in time and in perceptions of what risks are being run. Discount rates can hardly be applied, and cost benefit analysis becomes arbitrary in setting figures to environmental damage. What then can be done? Firstly, a structure for appraisal of environmental impacts is needed to undertake periodic risk analyses to assess the problem. Secondly, a method of assessment which allows for dialogue between specialist and non-specialist is needed. A non-numerate matrix which simply puts scores against issues is a possible starting point. Thirdly, statements that fusion has only a 'moderate' environmental impact (the Commission's description) ought to be eschewed. Such normative statements are perhaps meant to be reassuring, but they in fact only arouse concern that complacancy rules where science ought to prevail.

6.2 Macro Economic Factors.

6.2.1 Energy Markets.

The standard scenario for the introduction of fusion posits that by the time it has arrived at the market, the market will be ready to receive it. This juxtaposition of supply and demand, it is suggested, will come about as a result of a combination of market factors operating in the middle of the next century:

a) Fossil fuels as the main energy supply will be failing, and fusion rather than other technologies will be preferred as the replacement for them.

b) The demand for energy will be rising.

c) The share of the market taken by electricity will be rising.

d) There will be rising market prices which will bring benefits to investors in fusion, sufficient to justify the high capital cost.

Such a scenario is possible, as indeed are those which pose a low growth in energy demand and lower prices. It is hardly possible to say which will prevail; hence the need for a range of scenarios. As we don't know which is the most probable, we can replace that question with another, "Which would be preferable?" This is a question that can be debated and answered according to what point of view is held. We contribute only the following:

(1) A rising real cost, matched by a rise in demand and prices may offer returns to justify the investment in fusion, or any other technology. But rising real energy prices are not desirable for two reasons;

(a) Higher energy prices are associated with deflationary effects and reduced national income. This was demonstrated in the second half of the 1970's following the rise in world oil prices.

(b) High prices and rates of return involve a greater degree of risk for the energy sector as a whole if they are higher than the rate of return in the economy generally, and/or they rise more rapidly than the national income. THE RISK BEING THAT THERE WILL BE OVER-INVESTMENT IN THE ENERGY SECTOR, FOLLOWED BY A LOSS OF DEMAND, LEAVING SURPLUS CAPACITY IN THE SYSTEM. THE CONSUMER

MEETS THE COST. EXAMPLES OF THIS HAPPENING IN THE RECENT PAST ARE ELECTRICITY SUPPLY IN THE UK AND FRANCE.

(2) The capital requirement for a significant fusion supply will be high. Higher than anything yet contemplated. Ten reactors, would cost (in 1985 prices) according to our sensivitity analysis, not less than $50 billion (see figure S.2, page S-3). To this must be added an equivalent sum to bring fusion to the market as a reliable and competitive technology. From where will this capital requirement be met? The money market, or the government(s)? If the market is open and competitive then it will be realised through transfer of the technology from the public to the private sector. This is the assumption in the OTA report, and it is consistent with the economic development in the European Community. It is not easy to envisage how the technology transfer will happen. If capital rationing (which may be more severe than today because of the rising level of investment in complex technologies), precludes such a transfer through the market mechanism, then will the public sector fund the growth of fusion? This is the most likely option, unless a high rate of return can be secured for fusion, to compensate for the very long time period before the benefits show in the power utilities bottom line (a programme of ten reactors with a new one started every two years requires 40 years ie. by the years 2090 (see figure S.2, page S-3). There may be political resistance to the public sector taking all the risk. Consumers perceive that in a mixed public/private enterprise electricity supply structure, that they may be paying higher prices (or taxes) instead of enjoying the benefits of homogeneous supply industry where prices are transparent and contain no hidden subsidy.

(3) Capital markets have moved to a greater degree of freedom in the last decade, especially with the growth of international capital movements. It follows that the marginal cost of capital will move closer to the real discount rate. In such a context high cost long term projects will find capital very competitive. This is already happening. The effect on energy investment could lead to major changes in perceptions of profit and loss. Smaller scale technologies with front capital loading, and returning benefits within a short time, could look more attractive both to the investor and the consumer. This possible trend is already observable in the USA.

The situation for long term RD&D will not be unaffected by the changing economic environment. Historically, RD&D expenditures on nuclear power, fission and fusion have been decided at the political level. But the ability to enter competitive markets is likely to be taken into account in the future.

6.2.2 The Role of Fusion in Energy Supply.

The view taken for fusion supply in the Commission's documentation is that it will progressively substitute fission and coal in electricity generation. The basis of substitution will depend upon the movement of fuel prices which will reach a 'breakeven' point which can be defined as the point when the cost of uranium and coal is high enough to offset the higher capital costs of fusion. We have already drawn attention to the effects on national income of increases in the real cost of energy. In the micro-economic analysis later we show the improbabillity of such a scenario in terms of relative fuel prices. A third element of this scenario is the size and change in shape of the electricity market itself.

As a primary fuel, fusion is not directly accessible to the consumer in the manner of fossil fuels or some renewable technologies. It will only increase its share of the market by substituting for other fuels. The largest sector, by use, is space heating, and the second is transport. Electricity's share of the latter is very small, and in the former the price differential is a considerable barrier to penetration.

Nevertheless the Community view is that the electricity demand will grow more rapidly than that of any other fuel. They have estimated a 40% growth over a 15 year period to the end of the century (CEC 1986b), and an increase in market share by 3-4%. This growth is to take place very largely in the residential and tertiary sector. This presumes a large increase in the 'specific' use of electricity (appliances, electronic equipment, electric motors, etc.). Expectation of a large increase within a single sector of demand must be open to the risk that, as a forecast, it will fail, leading to surplus capacity and higher prices.

Projected as a long term demand, this type of forecast would mean that by the middle of the next century electricity would be raising its market share from something like 15% to 50%. It is in such a context that fusion is seen to be a market entrant.

ON THE SUPPLY SIDE, ALMOST ALL OF THE INCREASED GENERATION COMES FROM NUCLEAR FISSION - FROM 275TWh IN 1983 TO 792TWh IN 2000, INCREASING ITS SHARE OF SUPPLY FROM 22% TO 43%, AND DISPLACING COAL AS MARKET LEADER, FALLING FROM 34% TO 32% (CEC 1986).

OVERALL, WHILE THIS SCENARIO IS POSSIBLE WE FIND IT IMPLAUSIBLE. WE AGREE WITH THE OTA REPORT WHERE THEY ARGUE CONVINCINGLY THAT AN INCREASE IN THE DEMAND FOR ELECTRICITY DOES NOT OF ITSELF OPEN THE DOOR TO FUSION TECHNOLOGY.

> "Economics and acceptability rather than total demand will determine the mix of technologies... If fusion proves inferior to its competitors, it may not be used even at very high demand levels... Should fusion technology prove favourable, rapid growth in demand would facilitate its introduction... NEVERTHELESS DEMAND ALONE CANNOT TURN AN UNATTRACTIVE TECHNOLOGY INTO AN ATTRACTIVE ONE." [Our emphasis].
>
> (OTA 1987)

(1) At the same time as projecting the growth of electricity demand, the Community anticipates a 20% improvement in efficiency, which will exert a downward pressure on demand. The two projections do not appear to be mutually consistent, except in conditions of high economic growth.

(2) The principal competitors in the heating market will remain, as they are today, oil, gas and coal. They will remain competitive well into the next century. Assumptions about large price rises, even if they are taken to be realistic, cut both ways because fossil fuels are the main sources of electricity generation.

(3) The projected growth of nuclear power is unrealistic.

A possible scenario is based on the consistent development of fission power to be followed up by fast reactors, and then by fusion. As technologies ... they have a lot in common. But the displacement of fission on economic grounds looks improbable unless its decline in growth is reversed. The commitment to fast reactor technology has become increasingly stretched into the longer term, largely as a result of the slow down in the thermal reactor programme, but also because of problems with the operation, safety and construction cost of fast reactors, which have caused the economics to deteriorate. It is difficult to be certain about the future of fast breeders, except that they are unlikely to be significant until the middle of the next century - which makes them competitive with fusion. As they have a considerable resources base, matching scenarios for fast reactor and fusion power together is problematical on economic grounds. Those institutions which invest in fusion RD&D are unlikley therefore to favour fast reactor programmes. In taking such a view they will place the emphasis on the environmental superiority of the fusion reactor. Decision-makers will not find it easy to assess this trade off without the aid of a sophisticated model. Overall we share the view of IIASA and OTA that fusion would not benefit by being hurried into the market. The second half of the next century is more likely and it could be later than that.

6.2.3 Conclusions.

Amid the many uncertainties about the economics of fusion, it appears that the biggest hurdle that it will have to face will be the very large investment requirement. Costs of $2-3 billion are being anticipated for experimental reactors. The European and American programmes to date have cost more than $8 billion and world-wide the RD&D costs could be twice as much as that.

Before a demonstration stage is reached in any one programme (ie. either the American or the European, an estimated $20 billion will have been spent. Before fully commercial plant is operating the figure could be $50 billion (depending on the number of prototype reactors) and not less than $40 billion. Will these sums ever be recovered in the commercial phase? In our analytical study we assume a royalty payment for the first ten years of operation on a 10 reactor programme, after which it is assumed that the programme is making a profit. This will be forty years after the first station has been started and possibly after an expenditure, in total, including the RD&D stage, of $100 billion.

This capital burden raises two major questions, one political, one economic:

(1) Will industrial societies be able and willing to invest in fusion?

(2) What constraint will such high investment costs have on the price of electricity in the future?

Before decision-makers answer the political question they may want to have a response to question (2). Briefly our view is as follows:

We cannot predict the marginal cost of fusion generation, which will determine its competitiveness in the market. But the cost structure as it has been presented to us in the Commission's documentation is so heavily capital loaded that we see real difficulties for fusion in the market. Given that capital costs once spent are sunk costs, it is the variable or operating costs which will influence the market price at the margin.

Flexibility in responding to market conditions will depend on the following effects:

> Firstly, on fuel prices. BECAUSE FUSION FUEL COSTS ARE VERY LOW, THERE WILL BE NO GAINS FROM FALLING FUEL COST. RELATIVE ADVANTAGE FOR FUSION MUST DEPEND NOT ON FALLS IN ITS FUEL COSTS, BUT ON RISES IN THOSE OF COAL AND FISSION.
>
> Secondly, on maintenance costs. These are also represented as being so low that they can only move upwards - which will weaken the market position of fusion.
>
> Thirdly, availability of plant. This is the most uncertain variable of all (we discuss it in chapter 7). As availability falls, unit costs rise quickly.
>
> Fourthly, the economic efficiency with which the plant operates, ie. the load factor. On this we have no data, and therefore we take the availability figures of the Commission and subject them to sensitivities. This makes fusion power costs highly volatile.

To be influential in reducing fusion cost, the combined effect of these variable costs must assume major price shifts. As we show later such shifts will certainly be upwards and they could be significant. As a proportion of total costs, they could be important in a competitive market.

6.3 Resource Costs.

6.3.1 Introduction.

Fusion is a finite energy resource. In order to justify development, the resource base should be adequate to achieve a flow of revenues, discounted at the market rate to show a positive value. This would include the cost of building all the plant required for the fuel cycle, the disposal of waste and decommissioning. The time horizion for a positive net present value to be realised would extend into the 22nd century, and the resource base ought to be adequate for that.

What would be the limiting factors? Use of critical path analysis might show them to be in one of a number of areas which we can only conjecture at the moment.

(a) Materials - this will depend on engineering trade-offs. Beryllium in particular is in scarce supply (see below).

(b) Environmental - the extraction of lithium (and possibly deuterium)will involve environmental costs. Likewise the disposal of releases from power stations.

(c) Waste disposal - an outline waste management programme is required to estimate this. The time span involved will be important. There will be significant costs.

6.3.2 The Tritium Fuel Cycle.

(a) Tritium.

The fusion reactor has a very complex fuel cycle. Tritium is only naturally available in minute quantities, the principle? current source of tritium is that made for use in nuclear bomb triggers. Lithium has to be mined and purified and the Lithium 6 isotope has to be enriched (in DEMO to 30-50%, in a Prototype Commerical Sized Reactor (PCSR-E) to 90%) to enhance the breeding ratio. It has then to be installed in a breeding blanket with a neutron breeder for conversion into tritium. The tritium has then to be extracted, purified, and stored for injection into the reactor.

Clearly a fusion power programme would require a large enough tritium stock to allow for the fuelling of new stations as they come on-line. We have found that due to various constraints, it is very hard to design a fusion reactor with an adequate breeding ratio. Variables such as the breeding gain, out-of-reactor-time, tritium decay loss and the initial size of the tritium inventory will all effect the potential size of the programme. Potential problems with the fusion fuel cycle can best be understood by comparison the FBR fuel cycle (see Appendix 1).

(b) Lithium Reserves.

There is no consensus on the extent of lithium resources. Presently, lithium costs around $55/kg. It is estimated that prices will have to rise by a factor of three before adequate reserves are economically recoverable. Estimates of resources vary between 5 and 71 billion kg ((Hammond 1976), (Holdren 1978), (Cameron 1979)).

It is also hard to find any definite figures on the amount of lithium a fusion reactor might use. This is not surprising, the complexity of the fusion process and the large areas of unknowns involved preclude any accurate estimates of resource or financial costs. The main areas of uncertainty that we can identify are the future cost of lithium, the degree of enrichment, as well as the efficiency and cost of the enrichment process. Once in the reactor the lithium will be used to breed tritium and obviously the efficiency of the tritium cycle has implications for the lithium requirements of a reactor. Particularly, what losses will be involved in tritium purification and storage and how will the tritium be out of the reactor (this is important as losses through decay are high)? Accepting that there is little point in trying to assign too much accuracy to any figures, given the level of uncertainty, the table below calculates an optimistic and a pessimistic estimate of the number of reactors that could be fuelled by the known land-based resources of lithium, given different assumptions about key variables;

Table 6.3.2 Estimates of Lithium Resource Base for Fusion Reactors.

Variables	Pessimistic	Optimistic	Units
World Reserves (land) (3x current price) current price)	5.2 (IIASA 1977)	71.3 (Brandt 1988)	million tonnes
Lithium 6 enrichment level	90% PCSR-E[1]	30% DEMO[2]	
Lithium burnt during reactor lifetime	1380 UWMAK I[3]	100 STARFIRE[4]	tonnes/ 1.2GW reactor
Number of 1.2GW reactors	310	176,000	

[1] (Spears 1985a)
[2] (Cooke et al 1985)
[3] (Cameron 1979)
[4] (CEC 1987b)

A number of qualifications need to be made about the figures derived above. Firstly, the estimates are derived simply by taking the most optimistic and pessimistic assumptions from different reactor designs and therefore do not represent any actual reactor. It follows therefore, that the two figures represent limits on current designs and will probably overestimate or underestimate the most likely actual figure. It also needs to be pointed out that no account is taken for other uses of lithium. Fusion reactors are assumed to be the only users of lithium.

There are two striking results in the table. Firstly, there is a huge range between optimism and pessimism, and yet both figures were derived from the research programme's own data. The optimistic estimate suggest there could be enough lithium to supply electricity to OECD countries on low estimates for the year 2000 (IEA 1982) for approximately 3,800 years. The pessimistic figure suggests that total world lithium resources are not enough to fuel a fusion programme for more than a few years. Whatever the actual depletion rates are, somewhere in between these two figures we have to accept that land-based lithium resources are not sufficient to justify statements that a fusion reactor programme would be based on an "practically inexhaustible fuel" (p4, CEC(87) 302).

The only basis of such claims is on the assumption that lithium can be recovered from the sea where the concentration is 0.17 parts per million by weight (IIASA 1977). Because the oceans are so huge, then there are large lithium reserves (240 billion tonnes). However, although nothing is known about lithium recovery, the low concentration of lithium means that the environmental and recovery costs would be large and therefore we take the view that sea-based lithium cannot be treated as a resource under present foreseeable scenarios.

It must also be considered that lithium is not without alternative uses. In 1974, 5 million kg were produced worldwide for about a dozen different commercial applications. Lithium-Aluminium alloys are now coming into use for aircraft manufacture. Similarly, there may be significant alternative uses in the future if Lithium-Sulphur batteries prove successful. The Energy Research and Developemnt Administration (ERDA) observes;

> "planners project that 20 million urban electric cars containing a total of 270 million kg of lithium might be on the road by the end of the century. Utility electric storage - a projected 1000 units capable of delivering 100 MWhrs of power each - might require about twice that amount of lithium."
>
> (Hammond 1976)

(c) Materials.

Also, current reactor design studies tend to include beryllium as a neutron breeder, and tungsten wall tiles (IAEA 1985) which are themselves scarce and expensive resources. Beryllium will be needed in large quantities (approx 52 tonnes in the STARFIRE blanket). It will need to be reprocessed to reduce depletion rates and even allowing for reprocessing, THE IIASA STUDY (IIASA 1977) PREDICTED THAT FOR A PROGRAMME OF 100 1GW FUSION REACTORS THERE WOULD BE ENOUGH BERYLLIUM FOR 1.4 YEARS! A UNIVERSITY OF WINSCONSIN STUDY ON FUSION PROGRAMME RESOURCE IMPLICATIONS (Cameron 1979) SHOWS THAT FOR A 300GW US REACTOR ECONOMY, THE RESOURCES OF BERYLLIUM, VANADIUM, AND TUNGSTEN WOULD OPERATE AS A SEVERE CONSTRAINT ON THE LIFETIME OF THE PROGRAMME UNLESS MORE ABUNDANT AND CHEAPER MATERIALS WERE DISCOVERED.

We can find very little evidence of the appreciation by the Commission of the serious implications of materials constraints for a fusion programme. This in our view underlines the lack of an adequate management strategy capable of dealing with critical problem areas,. The Technical Planning Activity Report (US DOE 1987) develops an interesting way of modelling the materials problem. In their overview they say;

> "THE ULTIMATE ECONOMICS AND ACCEPTABILITY OF FUSION ENERGY, AS WITH MOST OTHER ENERGY SOURCES, WILL DEPEND TO A LARGE EXTENT ON THE LIMITATIONS OF MATERIALS FOR THE VARIOUS COMPONENTS."

The TPA report examines in detail the relative importance and potential impact of each technical issue, in the light of the overall objective which is to develop new or improved materials "that will enhance the economnic and enviromental attractivness of fusion as an energy source." From this the TPA develops a materials programme strategy. (See Appendix One for further discussion of this and historical background of the US fusion programme).

The cost of materials irradiation test facility is estimated at $150-$250 million to build. Work on such a facility was abandoned because of the cost.

6.4 Fuel costs.

As discussed in 6.3.2, there is a deal of uncertainty concerning the possible fuel costs. Adopting the methodology used above of making optimistic and pessimistic assumptions about reactor design and operation (and again ignoring deuterium costs), we derive the table below;

Table 6.4 Estimates of Fuel Cost Element of Fusion Power Electricity Generation.

Variables	Pessimistic	Optimistic	Units
Price of natural Li	$165/kg[2]	$55/kg[1]	
Li 6 enrichmennt	90% PCSR-E	30% DEMO	
Lithium burn-up rate for 1.2GW reactor	1380 UWMAK I	100 STARFIRE	tonnes
Fuel cost	14.05	0.112	mills/kWh

[1] current US price of 99.9% producers ingot
[2] 3xcurrent price is usually considered necessary to make lithium production sufficiently profitable to allow adequate supply. See page £££ for references.

Some provisos need to be added to the above table. Firstly, both the optimistic and the pessimistic figures have some common assumptions which err on the optimistic side;

1. Whilst account is taken of the amount of natural lithium needed for enrichment, the costs of such a process is assumed to be ZERO. In reality the costs of enrichment may dominate the price of lithium but there are simply no figures available on possible cost.

2. No account is taken of possible lithium losses in the enrichment process. 100% efficiency is assumed.

3. The reactors are assumed to have an availability of 75-80%

4. It is generally accepted that the price of lithium has to rise for an adequate supply, so the current price is usually assumed to treble, whereas we

have assumed there is adequate supply at current prices in the optimistic calculation.

Of course the figures are approximate and there may be incompatabilities between the various variables chosen which mean the data has a pessimistic element. Nevertheless, it must be the case this is insignificant compared to the above mentioned bias in favour of optimism.

Again, we must point out that the two results are within limits that are unrealistic, particularly the optimistic one for the reasons listed above. They show how with different and yet, by themselves realistic assumptions, the fuel cost element of the cost per kWh can escalate. Adding to the inescapably high capital cost, the result could be to make costs even less attractive than they might otherwise be.

Fuel costs may vary over a wide range, depending on the assumptions made about the resource base for fusion power. If the persistent bias to optimistic judgements is allowed for, then the effect of fuel resources will be an upwards pressure on varible costs, and a weakening of market power.

WE MUST CONCLUDE THAT THERE IS LITTLE JUSTIFICATION FOR TREATING FUEL COSTS AS NEGLIGIBLE. A LONG-TERM FUEL PRICE MODEL IS RECOMMENDED TO GAIN A BETTER UNDERSTANDING OF THESE SENSITIVE AREAS.

CHAPTER SEVEN. THE COST SENSITIVITY OF FUSION POWER.

7.1 Sensitivities.

7.1.1 Introduction.

We begin this critique of the Commission's expert group report COM(87) 302 on the economics of fusion with a general comment on the method that is deployed.

The economic study in COM(87) 302 produces specific cost figures (levellised generation costs discounted to an unspecified date in the 21st century) for the tenth of a series of fusion reactors. The costing is based on conceptualised designs that have emerged from within the fusion laboratories. These figures have not been tested, and as the authors themselves say, they are very uncertain and are to be treated with caution. In actual fact caution has been thrown to the wind. TO DERIVE SPECIFIC COSTS FOR ELECTRICITY GENERATION FROM CONCEPTUALISED DESIGNS, USING ANY NUMBER OF UNTESTABLE ASSUMPTIONS, IS THE TYPE OF FORECASTING THAT FELL INTO DISREPUTE SOME YEARS AGO, AND OUGHT NOT TO BE REVIVED, LEAST OF ALL FOR A TECHNOLOGY WHERE UNCERTAINTY PLAYS SUCH A MAJOR PART IN ESTIMATING ITS FUTURE.

The value of the cost research emanating from the Fusion establishments is further undermined by their attempts to marry cost estimates with energy accounting. The latter is expressed in energy values (ie. the energy content of the materials involved divided by the energy output). The result is said to provide a comparative basis of the efficiency of fusion as compared with fission power stations. The advantages claimed for this method are that (a) "it is not influenced by relative wage and price changes", b) it "is an easily understood and convenient measure of the value of a project". Neither of these claims hold water. Understanding the movements of energy prices and costs is essential to the art of economic evaluation. To say that putting them to one side is easier, might be analgous to suspending the laws of gravity because they complicate our understanding of the movements of bodies in and out of the earth's atmosphere. As for being more easily understood, that cannot be true. Energy accounting has gained no currency. Firstly, because it is extremely difficult to find any consistent way of measuring the energy content of materials, and no way of measuring the energy content of labour. Secondly, even if it could overcome this difficulty the results are of no value to economic assessment. The value of a commodity can only be determined in a manner consistent with the way other commodities are assessed. That can only be in terms of current or constant prices.

In order to come to a happy conclusion on unit costs the expert group then apply energy accounting to conventional cost data. A SHOT GUN MARRIAGE OF TWO SETS OF DATA, BOTH OF POOR QUALITY, AND INHERENTLY INCOMPATIBLE, IS HARDLY GOING TO LAST. WHAT IT DOES DO EFFECTIVELY IS TO DEMONSTRATE THE UNIQUE DETACHMENT OF FUSION TECHNOLOGY FROM WHAT IS HAPPENING IN THE WORLD AROUND IT. This should be seen as disturbing. Given the very long lead times for fusion technology, and the uncertainties that surround it, it is necessary to be exceptionally careful in applying economic and social critieria to evaluating its potential. It would have been better to apply established methods of evaluation, for then the results would be comparable with those of alternative energy paths.

WE CONCLUDED NOTWITHSTANDING OUR CRITICISMS OF THE METHODS USED THAT WE WOULD BE EXPECTED BY STOA AND THE EUROPEAN PARLIAMENT TO GIVE A JUDGEMENT ON THE FIGURES OFFERED TO THEM BY THE COMMISSION. WE HAVE THEREFORE DONE A SENSITIVITY TEST IN ORDER TO SEE HOW ROBUST THEY ARE

The assumptions used by the expert group relate to three conceptual designs. The reference Tokamak case appears to be to the American Starfire study. (It produces the lowest output costs). This, along with the non-Tokamak MARS study, are both taken to the tenth of a series to arrive at what is intended to be a settled down price. The PCSR-E is a prototype reactor which we assume would correspond in time to the demonstration stage.

We have therefore tracked the costs of these three reactors, as best as we can from the Annex to COM(87) 302. We have subjected them to sensitivities. THE ECONOMIC SENSITIVITES THAT WE HAVE USED ARE IN ALL CASES LESS FAVOURABLE, IN TERMS OF OUTPUT COSTS TO THOSE THE EXPERT GROUP. By doing this we test their robustness against what we judge to be over-optimistic assumptions. We do not claim that our assumptions are central estimates because we have had neither the time or the data to explore a full range of options. however we are quite sure that our economic estimates are more realistic and will be more broadly acceptable to electricity supply utilities.

We have accepted the engineering assumptions of COM(87) 302 for all three types of reactor. It is these assumptions that are the basis for the [illegible] from the prototype PCSR-E to the tenth of the [illegible]. We have then applied economic sensitivities to [illegible] engineering-driven costs, and the results are to be seen in figure 7.1 and in table 7.1.4B.

From figure 7.1 the following can be stated.

(1) The PCSR-E costs [illegible] the most rapidly, because there are no engineering [illegible] benefits, and there are no economies of scale. If this were to be regarded as likely then no justification [illegible] be found for going ahead with fusion. The expert [illegible] would regard this as an improbable [illegible]

(2) The Starfire and Mars [illegible] in bands covering a span of thirty years, during which they fall relative to PCSR-E as a result of engineering driven benefits. The result is to make [illegible] with [illegible] and coal, and [illegible] group. As presented we [illegible]. Given the economic

sensitivites of CES, (Centre for Energy Studies), fusion costs rise and it remains as today, at best 2-3 times the cost of its competitors.

To repeat, we do not claim the results to be central estimates - the uncertainties, especially on capital cost account, are still so large that all figures have to be treated with caution. What figure 7.1 shows is that subject to a fairly conventional economic test, the expert groups predictions are not at all convincing. They give a too sanguine view of the future.

Table 7.1.1 Economic Sensitivities - Assumptions (for tenth of a series).

	COM(87) 302	CES
Reactor Lifetime	25yrs.	the same
Availability	6,600Hrs	4-5000Hrs band
Load Factor[1]	not allowed for	65%
R&D costs (approx to 2010)[2]	not allowed for	$20billion
Construction time	6 years	10 years
Discount Rate[3]	5%	10%
Circulating power losses[4]	----	around 20%
Decommissioning costs	20% of capital cost	the same

[1] The Load factor is not given by the expert group. It is possible that they regard it (incorrectly) as equal to the availability.

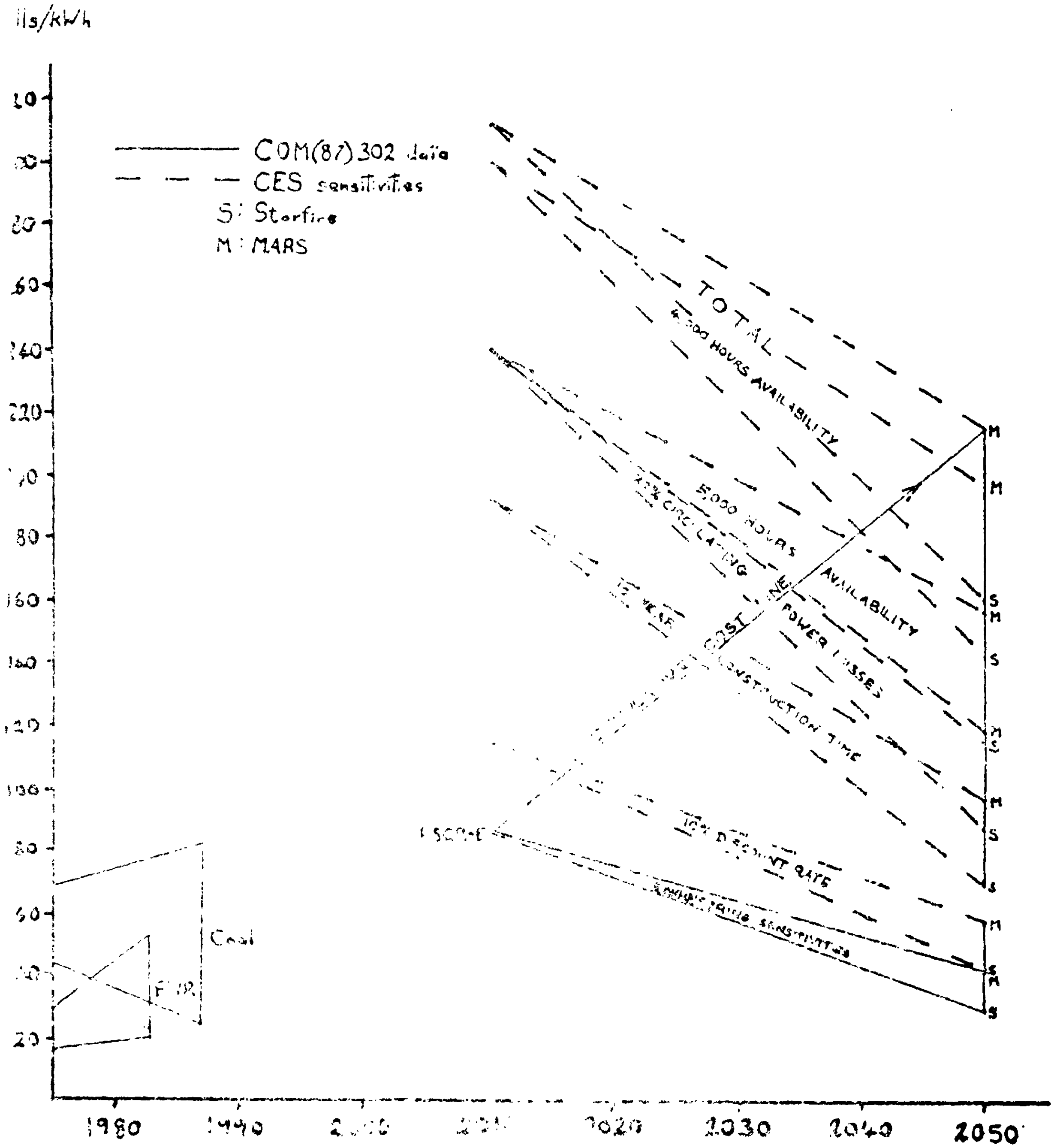
[2] These are not included by the expert group (incorrectly). Presumably they are regarded as an external benefit, a social cost or a hidden subsidy, according to which view is taken.

[3] The highest real rate of return on Community R&D projects will be close to 25% (short term). (For long term large scale projects the range is 5-15%). We choose 10% as a median figure.

[4] We can find no allowance for this in the expert groups data. We assume that they are using gross output figures. Given the exceptionally high power consumption on site this amounts to a major distortion.

Figure 7.1 Fusion Power Costs to the Mid-21st Century.

Cumulative Effect of CES Sensitivities on COM(87)302 Data.



Engineering Sensitivites.

From fig 8 it will be seen that we assume that PCSR-E operates at the high end of the range, and the tenth of the series at the optimum levels given in the report of the expert group.

All these paramaters are presented as linear. This is a simplification. Relative to the coarse quality of the model as a whole this does not amount to a serious distortion.

7.1.4 Generation Costs.

Two sets of results are given. The differences are not great (they arise from different annual capital charges ie. rates of amortisation) and they are shown in the table below.

Table 7.1.4A Generation costs (mills/kWh).

	STARFIRE 10th reactor	PCSR-E 1 only	MARS 10th reactor	PWR (France)	COAL (Italy)
Capital charge	30.44-25.9	70.6	42.56-36.2	10	6.9
Op. & Maint.	4.66-3.3	15.0	3.31-4.0	4	2.8
Fuel	0.00-0.4	0.7	0.36-0.5	5	24.6
Total	35.15-29.2	86.4	46.24-40.7	19	34.4

It is not clear how these figures are calculated. The most important is the capital cost. The engineering based sensitivities have the effect of reducing these costs dramatically and uniformly during series production. The cost for the 10th of a series (Mars and Starfire) are given above, and they are treated as lifetime generation costs and comparable with those for PWRs or coal generation. The latter therefore occupy the competitive price area against which fusion has to compete.

TABLE 7.1.4B Generating Costs for Fusion Power (mills/kWh)

CCM(87)302	PCSR-E	STARFIRE	MARS	PWR	COAL
Capital costs	70.6	[illegible]	35.2	10-34	7-14
Generating costs	86.4	29.2	40.7	19-53	34-82
CES Economic Sensitivities (cumulative)					
1 Discounted(10%)	[illegible].3	41.[illegible]	56.8		
2 Construction	192	[illegible]	94.9		
3 Circulating losses (20%)	240	[illegible]	118.6		
4 Availability					
6,600hrs	*	36.[illegible]	118.6		
5,000hrs	240	113.6	156.5		
4,000hrs	300	142	195.7		
5 R&D Royalty	12	12	12		
6 Decommissioning		[illegible]	[illegible].2		
Total	[illegible]	[illegible]59	218.9		

* As PCSR-E is a prototype we have assumed that its availability will be 5,000 hours.

7.2.2 The [illegible].

The assumption [illegible] CCM(87)302 [illegible] that capital costs fall by at least [illegible] production of 10 reactors. This is based on [illegible] arguments.

Firstly, the so-called [illegible] which are deemed to be a function of increasing [illegible] production. The experience of nuclear [illegible] provides a data base, which the fusion power [illegible]. This shows very clearly that [illegible] power plant construction was an escalation [illegible] (Komanoff 1981).

Secondly, the fusion power [illegible] to assume a short construction [illegible] contradicts experience with nuclear power plant [illegible] early phase where time and cost overruns [illegible] times can be found in a median range of [illegible] years (UK Electricity Consumers' Council (UKECC 198[illegible]). [illegible] construction time of 10 years, which we use, is probably a generous estimate [illegible] reactors are larger and more complex than [illegible].

Thirdly, [illegible] 5% represents a major distortion in the capital market. A rate of 10% reduces the discrimation against other energy projects, and it also signals to the fusion industry that savings will be expected in the operation of plant to offset the higher cost of investment.

7.2.3 Capital Cost Underestimation.

What does the experience of the fusion programme so far tell us about costs? Firstly, capital costs are going to be high, relative to coal or fission. It has been reported that the capital costs for NET are expected to be at least twice the £450 million cost of JET (Norman 1987). The US Engineering Test Reactor (ETR) is expected to cost $3 billion to build (US DOE 1987). A commercial demonstration reactor can be expected to cost considerably more. A major question is whether subsequent reactor capital costs will escalate or diminish. Declining reactor costs appear to be one of foundations of the arguments for the Commission's economic case (CEC 1987b), based on the assumption that once the technology is established serial building will lead to reduced costs. There are, however, reasons to doubt the validity of this argument.

EXPERIENCE WITH FUSION'S CLOSEST TECHNOLOGICAL RELATIVES IN THE THERMAL FISSION PROGRAMME AND THE FAST REACTOR PROGRAMMES SUGGEST STRONGLY THAT CAPITAL COSTS HAVE HISTORICALLY UNDERGONE ESCALATION IN THE FACE OF PREDICTED COST DECREASES.

Analysis of cost growth in "A Review of Cost Estimation in New Technologies: Implications for Energy Process Plants" published by the RAND Corporation (RAND 1979) identifies the major areas where estimation error occurs;

1) plant and process uncertainty,

2) estimation methodology,

3) project organization,

4) exogenous effects on cost, and

5) the effect of changing environmental, health, and safety regulations on estimation accuracy and plant performance

There appear to be no reasons to assume that the fusion programme will be immune from any of these factors which contribute to cost underestimation.

The two sets of figures in a study made by the RAND Corporation obtained for the type of plant that are closely related to fusion, show a strong tendency to underestimate the capital costs. ANY FEASIBILITY STUDY WHICH DOES NOT TAKE INTO ACCOUNT THE STRONG POSSIBILITY OF INITIAL AND SERIES CAPITAL COST UNDERESTIMATION CANNOT BE REGARDED AS CREDIBLE.

Table 7.2.3. Summary of Cost Escalation in Large Plants.

Items Estimated	Mean of Actual to Estimated Cost	Sample Size	Standard Deviation
Major construction	2.18	12	1.59
Energy process plants	2.53	10	0.51

(RAND 1979)

The cost stereotype that is emerging for fusion power is one which shares a lot with the Fast Breeder cost stereotype. Briefly, the FBR cost stereotype is that capital costs will be high but the small fuel costs will more than offset such high costs. The fusion programme cost stereotype is an exaggeration of the FBR stereotype (see Appendix 3). Capital costs will much higher and the fuel costs much lower. The problem with such a cost stereotype, as we have already indicated, is that it tends to lead to a neglect of the fuel cycle costs, assuming they will be negligible, while it is possible that they will be important in setting marginal prices. At the same time, because capital cost are high, both absolutely and relatively, to coal or fission plant, only the assumption of massive economies of scale will reduce the total cost to anything like a competitive level. The result, even if it is taken to be credible, leaves a cost structure that is highly vulnerable to changes over time and inflexible in response to market forces.

7.2.4 Power Density.

Power density is the ratio of power produced, to the volume of the reactor. It affects efficiency and has capital cost implications. Although there is obviously no linear relationship between power density and capital cost and efficiency, it is nevertheless useful as an indicator, especially when comparing similar plants such as fission and fusion. Power density effects thermal efficiency because energy conversion can be carried out more efficiently. It is hard to link this kind of calculation directly to cost, but for comparative purposes it quite useful. Power density affects capital costs through the relationship between the size of a reactor structure and the power it produces. If one has two structures, both made of similarly priced materials of similar complexity, with the same output and one was twice as big (ie. half the power density) it would not be an unreasonable 'first estimate' to say that the larger reactor would cost nearly twice as much. The ratio of the reactor cost to the balance of plant would [illegible] important when comparing overall capital costs.

The power density of a fusion reactor would be relatively low (similar to an early fission reactor, eg the Magnox reactor, but much less than a pressurised water reactor (PWR)). The DEMO design predicts a total power density of 1.9MW/m^3, (similar to the Advanced Gas Cooled reactor (AGR)), on given plasma assumptions, etc, although there is some disagreement whether this figure would be possible. There are several reasons why it is hard to improve the power density in a fusion reactor. The thickness of the blanket is determined by the need to capture the neutrons and to maximise the breeding of tritium. Then there is a shield (approximately 80-100cm) needed to protect the magnets from heating and radiation effects. If the first wall power loading (MW/m2) could be increased through better plasma performance there would be trade-offs concerning magnetic confinement power and first wall erosion rates.

7.2.5 RD&D Royalties.

The cost of RD&D may be treated by the nuclear industry as a social cost. But the normal practice is that RD&D should be recovered. One way is to add a royalty, paid per unit of electricity produced. We take the $20 billion for R&D costs as central estimate to the end of the Development, and a load factor of 5000 hours for the Starfire series. Following the figures given in the table above (Table 7.2.3) we estimate the royalty to be 40% addition to the total cost. At 6,600 hours this would fall to 25% of unit costs. As RD&D is a sunk cost, committed before commercial operation has been determined, we do not discount it forward, although it could be argued that the opportunity cost of RD&D should be treated the same as any other investment expenditure, and that some private companies do follow such a practice. The higher the eventual cost of fusion power the lower, relatively, the RD&D cost will become. We have assumed however that the royalty ought to be recovered in the decade after the first ten reactors have begun operation.

7.2.6 Decommissioning.

Assuming that this is 20% of the Direct Cost, it would add 10% to the total cost of Starfire 10 at 5000 hours load factor, and 6.8% at 6,600 hours. We do not discount the decommissioning cost forward from the commissioning date. To do so reduces it to zero. Decommissioning is a real cost, and by no means an insignificant one given the very large size of fusion reactors. It ought not to be to be made to disappear as a result of an accounting device.

RD&D and decommissioning together add approximately 50% to the cost of the lowest Starfire figure of 29.2 mills/kWh, but as a proportion of total cost it falls as the price of fusion power rises. At a unit cost of 100 mills/kWh, these two costs would be 15% of total cost. (Note: 1 mill = 1/1000th of a US dollar and the exchange rate assumed is 1 ECU=$0.822).

7.2.7 Plant Related Variables.

The biggest impact on capital costs however will be the combined effect of the plant related variables, especially the discount rate and the construction time. These increases are more than proportionate to the time taken. Time-cost overruns on nuclear stations have frequently doubled and trebled the capital cost. The cost of the Starfire reactor would almost double to $4588m from the $2400m estimated in COM(87) 302, (assuming 10 years construction time and a 10% discount rate). This is by no means the full cost even for the lower end of the reactor cost range. IT IS UNLIKELY THAT A FUSION PLANT, IN THE FIRST SERIES AT LEAST, COULD BE BUILT FOR LESS THAN $5 BILLION.

Table 7.2.7 Sensitivities attached to Starfire and Mars (10th of a series).

	DISCOUNT RATE	CONSTRUCTION TIME	DIRECT CAPITAL COST $M	TOTAL COST $M
STARFIRE	5%	6yrs	1729	2440
	10%	10yrs	3808	4588
MARS	5%	6yrs	2365	3266
	10%	10yrs	4836	5895

Note: all of these sensitivity studies assume constant money prices.

7.2.8 Availability.

Fusion reactors are always considered to be supplying base-load electricity. That is, they will supply electricity whenever they can. They will be the last to go off-line. Such an assumption is necessary for fusion as it has such high capital costs relative to fuel and operating costs making generation costs extremely sensitive to availabilities. To maximise revenue it should have a high availability and a high capacity factor.

THE COMMISSION'S ASSUMPTION, BUILT INTO THEIR TOTAL COSTS IS 75%-80% AVAILABILITY. IS THIS HIGH FIGURE A REASONABLE TARGET OR IS IT JUST AN UNREALISTIC ASSUMPTION?

The fusion programme operates at the forefront of technology. Most of the technology for a reactor has to be designed, built and tested before its feasibility is proven. A fusion reactor will be an extremely complex device, far more complex than any power producing plant ever built. Much of the equipment will be expected to operate in hostile conditions, under high energy neutron bombardment suffering exceptional thermal and magnetic stresses with the co-existence of extremes of temperature and pressure. Should one of the subsystems fail in the reactor, access and handling would be extremely difficult. Maintenance requires the creation of robots. Given the very large number of sub-systems, attaining a high availability is likley to prove very difficult, and there is no technical information to suggest that it will be higher, if as good as thermal reactors.

The first wall/blanket will have to be replaced frequently involving the remote dismantling and replacement of the power core possibly as often as every two years. The DEMO team adopted a target of only replacing the blanket modules every ten years, but this was not achievable in the design for various reasons, such as the Lithium burn-up rate. We have already mentioned the environmental cost associated with short first wall/blanket replacement times. The potentially most costly aspect of short first wall/blanket replacement is the amount of down-time that may be associated with this. The DEMO team adopted a 60 day every two years maintenance period in accordance with AGR figures, but this figure cannot be justified in any way at this stage. A benefit on the other hand may be that the requirements of high reliability of the reactor components may be relaxed slightly, but in relation to the costs, this benefit is likely to be small. The costs associated with down-time will obviously vary according to the frequency and length of the wall replacement procedures, and one would expect that here again the engineering problems associated with the rapid and safe replacement of the first wall and breeding blanket will be severe.

In the light of experience we are surprised that the Commission can plan on a 75-80% availability. We doubt if any expert body would regard this as a prudent decision. WE ARE OF THE VIEW THAT THE COMMISSION SHOULD NOT CONTINUE WITH THIS ASSUMPTION AND THAT THEY SHOULD BEGIN SERIOUS STUDIES ON A FULL-RANGE SCENARIO STUDY FROM WHICH A CENTRAL ESTIMATE CAN BE DERIVED.

What is nevertheless apparent, is the dramatic effect that availability has on unit costs. Because of the cost structure, fusion with its extremely heavy capital loading (see the cost stereotype below), the rise in costs when power output falls is very sharp. The cost curve is heavily non-linear, but as we have no way of expressing cost as a function of availability (through lack of data), and in order to not overstate the effect, we have used a linear calculation. The sensitivity study shows availability to be the most volatile of all the cost components.

It would be more accurate to calculate load factors (electrical output as a percentage of design capacity). These are normally lower than availabilities and they are a better guide to the cost efficiency of the reactor. The expert group have in fact taken them to be the same. They appear to assume that the reactor will operate at 100% of design capacity. Another case of unjustified optimism? It indicates that no system analyses have been done nor has thought been given to show how the fusion reactor will affect system loading and system costs. Nor has consideration been given to the possibility of derating. If, however, we follow the estimates given but use 4000 and 5000 hours as the centrally estimated band, the effect on reactor output costs is that they rise very rapidly (see fig 7).

WE ARE OF THE VIEW THAT FUSION POWER COST SENSITIVITIES SHOULD BE RE-APPRAISED, ESPECIALLY IN THE LIGHT OF THE DOWN-TIME ESTIMATES ASSOCIATED WITH FIRST WALL AND BLANKET REPLACEMENT TIMES.

Figure 7.1 shows the effects of the sensitivities we have used on the levellised generation costs of fusion power. The graphs are based on the data for the Starfire and Mars series, plus the prototype PCSR-E (which is a one-off that precedes the series production). The only sensitivities used by the expert group belong to reactor engineering and reactor scaling.

It will be seen in figure 7.1 that the target area is occupied by the low cost coal and LWR estimates. It is instructive to remember that the PWR figure (the lower figure used in COM(87) 302, will understate the real cost of nuclear, but is neverthless a settled down figure of today arrived as the result of a rise, and not a fall, in reactor costs world wide. TO ASSUME THAT FUSION CONSTRUCTION WILL REVERSE THE COST TREND OF FISSION AND FALL BY AT LEAST AS MUCH AS THE OTHER ROSE CAN ONLY BE DESCRIBED AS HEROIC. IT CAN ONLY BE TREATED AS EVIDENCE OF BLIND DETERMINATION TO MAKE THE CASE FOR FUSION BY ASSERTING WHAT CANNOT BE REASONABLY DEMONSTRATED.

The reference case (Starfire 10) is seen to be moving close to the current PWR figures - but the Starfire estimates in COM(87) 302 are not central estimates. Indeed the sensitivities which the expert group have used, (the engineering parameters) in all cases bring the costs down. They find only sensitivities that lower fusion cost and none that raise it.

The underlying reason for this one-sided appraisal is a belief that all costs are driven by engineering. This is a logical result of the choice of energy accounting as a methodology. Which came first, the methodology or the conclusions that it gave rise to, is a matter of speculation. But neither the results, nor the way in which they have deen derived can inspire confidence in the fusion industry's capacity to measure its own performance.

We recommend that a future study take a different approach. The engineering parameters important in cost estimates will require far greater study, and they will require independent assessment. The heavy dependence on the cost effects of scaling cannot be pursued uncritically. The factors which will influence marginal costs will need far more attention. THE NOTION THAT A TECHNOLOGY CAN BE BROUGHT TO THE MARKET SOLELY BY TECHNICAL IMPROVEMENT, CAPABLE OF BEING MEASURED SEVERAL DECADES AHEAD IN FRACTIONS OF A CENT (per kWh), AGAINST AN ASSUMED BACKGROUND OF CONSTANT REAL PRICES AND THAT ON THIS BASIS FORECASTERS CAN CLAIM THAT IT WILL BE COMPETIITIVE, CAN ONLY BE DESCRIBED AS A TRIUMPH OF MATTER OVER MIND.

7.3 Fuel Costs.

7.3.1 Cost Stereotypes.

The expert group's treatment of fuel costs, is central to the their case. Fuel costs for fusion are low and thus offset the capital costs relative to fission, which is seen as the near rival. In all existing power generation the lifetime fuel costs are the most important. In the case of fusion the situation is different. The capital costs are very high both absolutely and relatively. In Starfire 10, where capital costs have been optimised, the fuel costs are shown to range from zero to 0.4 mills/kWh. For Mars they are a little higher. If we treat the zero figure as an aberration, then fuel costs are seen to be around one seven hundredth of capital costs. Over the lifetime of the plant this is seen to give it a decisive advantage over fission. The work of Bunde is commended by the expert group. It shows that fusion has an energy gain over reactor lifetime which is twice that of fission. His results are shown below.

Table 7.3.1 Energy Input and Output over 30 years life (MW thermal/MW electrical).

	fusion	fission
Capital construction	4082	2160
Construction of fuel installations	16	789
Fuel for first operation	3	399
Fuel for lifetime operation	87	5554
Total energy input	4188	8902
Energy generated	$6.3x10^5$	$6.3x10^5$
Energy gain	150	75

MW thermal always means thermal energy and/or primary energy equivalent of electrical energy, and MW electrical refers to electrical power sentout.

(Annex to COM(87) 302 page 66)

7.3.2 The Use Of Energy Accounting.

We have consulted the paper by Bunde which explains his treatment of energy accounting, and we can find no adequate explanation as to how these calculations are made, or what reliability can be placed upon them. It is not made clear, for example, if they are primary energy units and if so, whether the energy losses in the energy production, distribution or in end use are allowed for in a consistent manner. Neither do we see any way in which this method can allow for the constant change in the energy content of commodities.

However, leaving aside the lack of credibility attached to the data, what purpose do they serve? Clearly they have nothing to do with rational decision-making. Decisions to choose one technology as compared with another are not taken on such grounds. Equally clearly the energy content does not in itself tell us if a particular process is economically atttractive. It might, for example, be the case that uranium enrichment by diffusion is a much more expensive method (in energy terms) than enrichment by centrifuges. But it does not follow that enriched fuel from the latter will be cheaper to buy on the market. Indeed it is not difficult to postulate conditions where the reverse may hold.

Energy costs cannot therefore be used in preferring one technology to another, except perhaps in making very broad historical judgements. It becomes very misleading when it is applied to making micro-economic decisions.

The manner in which Bunde presents his case gives us cause for concern. The figures given above by him for the relative energy fusion and fission fuel cycle should not be taken as even approximating to reality. They appear to us to be unconvincing. There is no supporting evidence that the relative energy costs are what he states them to be. The cost of the total fuel cycle, from the extraction of deuterium, mining, or extraction of lithium (from large quantities of sea water?), the enrichment of lithium, the reprocessing of lithium and tritium, the disposal of large quantities of waste, the monitoring and environmental activity that must surround the waste management programme - suggest to us that the burden of the fuel cycle cost is understated. But our principal reason for concern is that we do not see how any weight can be put on figures of energy cost for products that have not yet even be designed. This point is affirmed in a recent paper from the Max Planck Institute, were the authors say bluntly;

> "The construction energy calculations [in (Bunde 1985)] were done on the basis of uncheckable mass tables and are consequently worthless."
>
> (Pfirsch et al 1987)

They conclude by characterising the energy accounting as an exercise "conducted with false logic by unsuitable methods using false or uncheckable data".

7.3.3 Fuel Cost Logistics.

Finally, we turn to the strategic role assigned to fuel cycle costs. In the cost stereotypes that are produced (based on conceptual designs), the balance of cost advantage falls to fusion because of its relatively low fuel costs.

The relevant comparison is made with nuclear fission. However, it has always been understood that the competitive power of fission has lain in its own low fuel cycle costs, relative to those of fossil fuel power stations. The essential logic is exactly the same as that of fusion; namely that competitiveness exists because the high capital cost is compensated for by the low fuel cost. Over the lifetime of the reactor this produces a net savings. Hence fusion is seen to be appealing to the same advantage over fission, that the latter is claiming over fossil fuels. The cost sterotypes for fission and fusion as used by the industry are as follows;

Table 7.3.3A

	Fission %	Fusion %
Capital cost	65	86
Operation & Maintenance	10	13
Fuel	25	1
	100	100

Now, if we assume that in money terms, the cost difference on the capital account between the two is 2:1 in fission's favour, then it is apparent that if fission is to lose its advantage, its fuel cost would have to rise by a very large amount.

To show this in a illustrative example we put figures to the table above, what might be taken to be marginal fission and fusion reactor costs. Adhering to the proportion of costs given in the table above we have the following:-

Table 7.3.3B

	fission (mills/kWh)	fusion
Capital costs	50	100
O&M costs	7.7	15.1
Fuel costs	19.2	1.2
Total cost	76.9	116.3

To achieve equal costs, the required rise in fission fuel costs (ceterus paribus) will be 39.4 mills, to 58.6 mills/kWh, ie. a rise of 205%. Fission fuel costs as a proportion of total cost rise to 51.2% and capital costs fall to 43%. This is an inversion of fission costs, and why it should take place is inexplicable, unless it is assumed that uranium prices rise rapidly to exceptionally high levels while fusion costs remain constant or fall.

Figure 7.2 (page 7-17) looks at a simple fuel price break even model, and it suggests that uranium prices must rise to levels five to eight times their present level. Thus the fuel cost break-even point, which the expert group's model for competitive fusion power implies, could only arise under exceptional circumstances. Is it probable?

Leaving aside the lack of realism of such a scenario, if we suppose an inversion of the cost structure as the result of big price changes it would mean not that fusion costs had been reduced (to the benefit of consumers) but that the cost of fission power had risen, by at least a factor of two in real terms. If this were to happen, two things are evident - one cause, and the other effect. The cause or origin of such a shift in relative economics could only arise as a result of a very large rise in the cost of uranium. We would require a model of world uranium prices, the out-turn of which would show an increase several times the present price. This in turn would only happen if the demand for uranium rose rapidly as the result of rapid growth in nuclear fission construction or alternative uses. As the role of fusion is seen to be one of displacing fission there is an apparent contradiction here. However it does not appear likely that fission power will expand rapidly, and the other scenarios will have to be found if the entrance of fusion into the power market is to gain conviction.

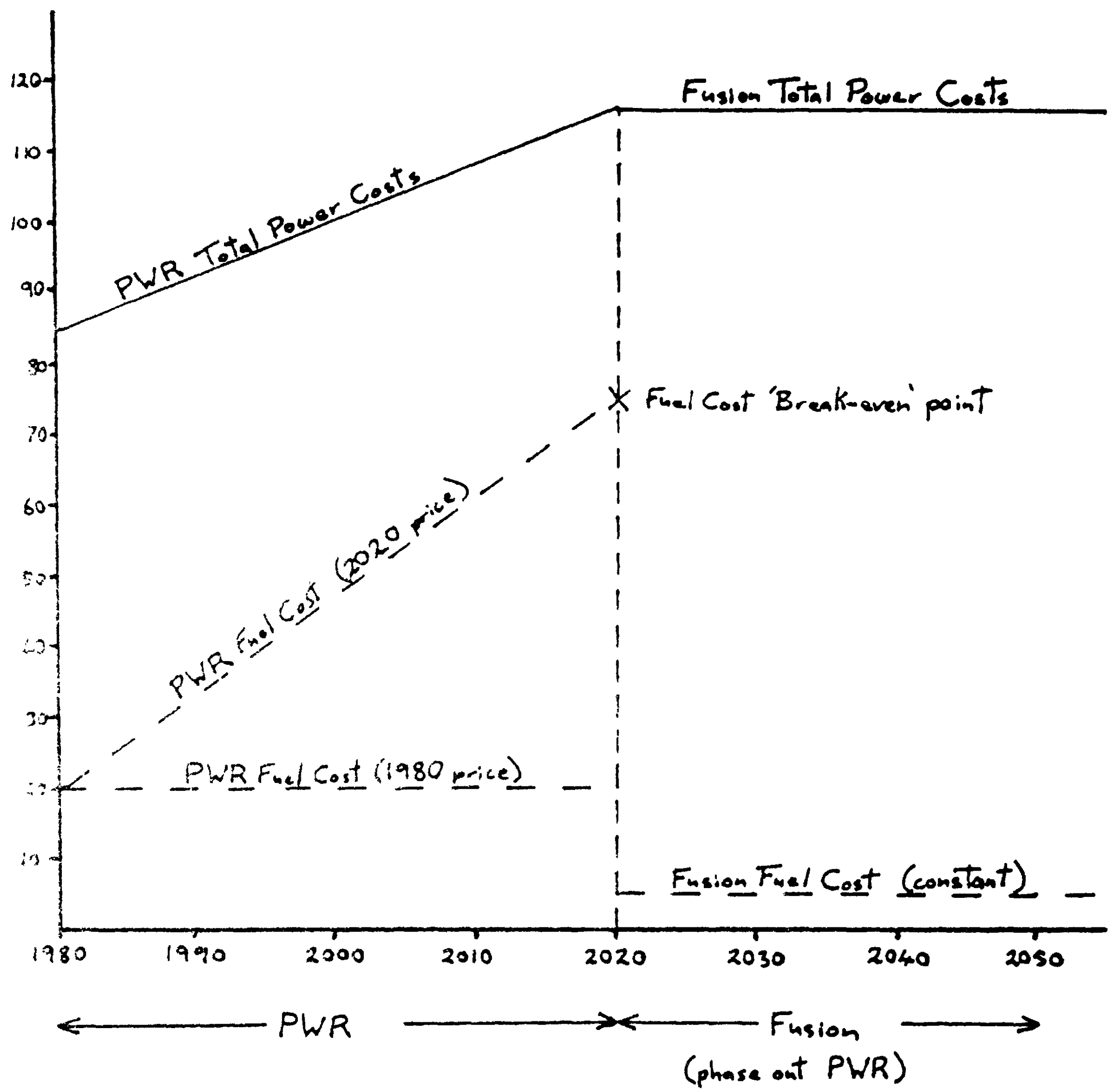
The difficulty that confounds the attempt to devise a credible scenario for competitive fusion power is that a key assumption is a high rise in energy costs. Without such a rise the high capital cost of fusion prevents it from becoming competitive. Secondly, even if the above conditions were all met, and fission costs rose consequent upon a large growth in world demand for nuclear power, the effect would be, as a result of this price rise, that the marginal cost of nuclear generation would rise relative to that of fossil fuel stations. In particular the cost advantage of coal generation over nuclear, which is substantial in some EEC countries (in the UK it is around 15%) would increase dramatically. As a result nuclear power, both fission and fusion would lose their market share, or never come to the market.

Perhaps in anticipation of such a problem in economic logic, the fusion case has arbitrarily stated that the cost of coal fired generation "is up to 60% than thermal fission plants". It "is expected to maintain, or even increase this cost disadvantage" thus worsening its competitive power. What does the expert group mean when it says "up to 60%"? It applies to base load plant only it seems. Coal-fired stations however would not be placed on baseload if they were 60% more costly than nuclear power. In the UK, which has an unusually large proportion of coal-fired plant, old and inefficient coal-fired plant is rarely, if ever, placed on baseload. Modern baseload coal plant is less costly than nuclear.

Looking to the future. There is not one coal price, but many. It depends on what market coal is bought in and to where it has to be taken.The transport costs for internationaly traded coal can be twice the pithead price when it has to be delivered to inland sites. Moreover, coal prices have fallen rather than risen. Models for fission power built on coal price rises have collapsed (CEGB 1983/7) recently. They are not likely to sustain fusion.

THE CONCLUSION THAT WE ARE LEFT WITH IS THAT THE CAPITAL COST OF FUSION REPRESENTS A NEAR INTRACTABLE PROBLEM. ONlY A VERY HIGH RATE OF RETURN WILL ATTRACT INVESTORS TO FUSION. OPTIMISM ABOUT FUSION MEANS EXTREME PESSIMISM ABOUT OTHER ENERGY PRICES.

Figure 7.2 Fusion Fuel Cost 'Break-even' Model.



The inclined dashed line shows the increase in PWR fuel costs necessary for PWR total power costs to reach a point where fusion power costs may become competitive with PWR power costs by 2020.

CHAPTER EIGHT. DECISION-MAKING AND ACCOUNTABILITY.

8.1 Reviewing the Fusion Programme.

The European Fusion Programme is more than ten years old, and we looked for evidence of Appraisals in that time, and what criteria were used. In 1981, the Fusion Review Panel reported. This was followed by an update three years later. They confined themselves largely to the technical aspects of the programme. There is reference in their reports, to the need for 'continuous assessment', and various 'in depth' studies. As the reports raised no critical issues these recommendations went largely unnoticed. There is the intention by the Commission to have another major Panel Review in two/three years time, in order to assess technical progress and look at the next stage. Our view is that a more searching appraisal is required.

WE RECOMMEND THAT THE COMMUNITY SHOULD BEGIN THE PROCESS OF BRINGING FUSION RESEARCH INTO A RECOGNISABLE FRAMEWORK OF ASSESSMENT, CONSISTENT WITH COMMUNITY POLICY AND PRACTICE WITHOUT DELAY.

We make this recommendation for the following reasons. Firstly, because before the next stage of funding (into the NET programme) there is an evident need for something more than a Review of progress. There should, we suggest be a Full Feasibility Study. This would subject the fusion programme to a searching policy examination, as well as a high quality technical appraisal. The result should be more than a short-term go ahead for the next few years of funding. The report which follows a Full Feasibility Study should be one that can bear the weight of the decisions that have to be made into the medium and long term.

Secondly, we ask the question, should not a Full Feasibility Study involve all the relevant interests and expertise within the Community? For example, the Office of Project Evaluation within the Research Directorate General of the Commission; the Science and Technical Options Assessment Project of the Parliament;, the Directorates General for Energy, Environment, Finance and Economics, Social Affairs and Employment; and the relevant Committees of the Parliament. Do not all of these bodies have an interest in the fusion programme and its impact on the Community's future?

Thirdly, the Appraisal ought to be independent. By which is meant independent of the bodies involved in the European Fusion Programme. One reason is that the specialists involved ought to possess a range of experience far wider than nuclear power, and there should be persons with recognised expertise in energy economics, in environment assessment, in project management in different industries.

Fourthly, all sections of the Fusion Programme, including of course the Directorate, should be requested to take part in the appraisal, and especially in its preparation. We suggest that this should include papers covering the technical, economic and other aspects of the programme, and projections for the future. They should be encouraged to present their own distinct view of the future, and to use their knowledge and information to the best effect. Different points of view should be welcome.

Fifthly, the appraisal itself should seek to be cost effective. We understand that the 1981 Panel Review cost 150,000 ECU (we do not know if this figure includes the Commission's staff costs). The results hardly justified the cost, as the expertise of the Panel was too close to that of the Fusion industry. We suggest that the appraisal process involves at least one team of management consultants with a high profile in public sector investment appraisal. The results should be accessible throughout the institutions of the Community and within the member states. It should produce a document which will enhance the understanding of fusion power, and of the importance of energy policy in the Community generally. It should bring about a major step forward in evaluation techniques for the future and improve the quality of decision-making.

Sixthly, we suggest that a Full Feasibility Study would be an open ended appraisal of the costs and benefits of all aspects of fusion - touching the environment, energy futures, safety, social acceptability, as well as the technical and economic aspects. It is generally understood that the next major stage in decision-making will take place in the early 1990's. The next few years are therefore crucial. Fusion will be entering the transition from the Research to the Development stage. No-one knows with any accuracy how long that will be, and beyond that lies the Demonstration stage, and beyond that the Commercial stage, which in our view, will probably necessitate one or more prototype stages before commercial viablity is reached.

THE QUESTION OF "HOW FEASIBLE IS THE FUSION OPTION " NEEDS TO BE ASKED AND ANSWERED IN A FULL AND OPEN MANNER. SUCH MAJOR LONG-TERM DECISIONS SHOULD BE BACKED BY A BROAD CONSENSUS OF COMMUNITY OPINION. THIS IS NECESSARY TO SUSTAIN THOSE WORKING IN THE FUSION PROGRAMME.

FOR THIS REASON WE BELEIVE THAT A REPETITION OF THE FORM OF THE PREVIOUS REVIEWS WOULD NOT BE ADEQUATE. THE EXERCISE THAT WE ENVISAGE WOULD BE RIGOROUS, SETTING OUT ALL THE COMPLEX OF FACTORS DEFINING THE OBJECTIVES AND THE OPTIONS THAT ARE AVALABLE FOR REALISING THEM, AND INCLUDING THE LEVELS OF EXPENDITURE THAT EACH OPTION WOULD DICTATE.

THE OVERALL AIM SHOULD BE TO MAKE IT POSSIBLE FOR CLEAR DECISIONS TO BE TAKEN AT THE POLITICAL LEVEL.

The OTA study provides useful guidance. While it is primarily technological in its range, within that range it meets the requirements of a feasibility study. The European programme has not had the benefit of a study of comparable calibre. Comparison between the OTA study and collection of uncoordinated reports in COM (87) 302 is instructive. The former confronts a number of major problems, including the cost and it offers four options based on the level of spending. We summarise them in Appendix 2. The US Department of Energy makes clear its own inclination, which is to enter into an international programme, (eg ITER), but the document remains open ended. The decision is clearly left as one for the politicians to make. Notwithstanding the differences of political structure, the distinction between the role of specialist, and that of the politician is implicit to the OTA report, and it is a distinction which remains cardinal to the decision-making process - in Europe as in other democratic states.

We have differed on one major matter with the OTA report; namely its treatment of resource allocation. It sidesteps this problem for reasons with which we fully sympathise. We accept that the value of cost/benefit studies applied to a technology which is still in the research stage, must be questionable. Nevertheless we have felt it necessary to make an economic appraisal, and to offer at least a range of options, based not on fiscal considerations (as in the OTA report) but on what we understand to be the best use of resources. Despite the difficulties, we felt that it was necessary to make a first assault on this problem principally because we do not see how politicians can be asked to decide on a programme of such vast expenditure without the benefit of economic judgements, even if those judgements are no more than best estimates.

WE THEREFORE ASK THE QUESTION, "CAN WE WAIT UNTIL $20 BILLION HAVE BEEN SPENT BEFORE WE DECIDE IF THE MONEY HAS BEEN WELL SPENT, OR WHETHER IT SHOULD BE ABANDONED?" WE ARE AWARE THAT CANCELLATION AT A LATE STAGE HAS BEEN DONE BEFORE IN THE NUCLEAR PROGRAMME (eg the US decision to cancel the Clinch River fast reactor project). BUT WE FELT THAT THE ANSWER TO THE QUESTION IS "NO", AND THEREFORE A FULL APPRAISAL INCLUDING A LOOK AT THE PROPOSED ECONOMIC BENEFITS IS ONE THAT OUGHT TO BE UNDERTAKEN.

8.2 Options and Decision-making.

8.2.1 Introduction.

In the discussion above we have confined ourselves to the form and structure of the next appraisal. But what of the objectives that should guide it? In our view, there is a very strong case for saying that it should aim to produce options. Each option should be seen in principle to have equal validity, in that each is technically sound. They will differ in that they offer different solutions, depending on how the authors read the future. In this way the assumptions and the value judgements that underpin the options can be made plain. The decisions that have to made are seen to be the responsibility of the politicians who have had the benefit of being exposed to differing solutions, each advanced with the desirable level of expertise.

We do not suggest that the importance of such an approach to decision-making is not understood and accepted within the

Community - only that it has not been pursued in practice in fusion research. To pursue such a path, rather than one which amounts to a process of self-validation by the fusion research directorate and its associations, is an appropriate change that could now be made to bring decision-making in this field into line with the Community policy. Expert panels can be very effective when technical matters have to be decided (although even then it is important that they do not weaken the ultimate responsibility of the decision-makers). But for major decisions, such as the future programme of the fusion programme, the Expert Panel is too narrow, both in composition and conception. Inevitably it is technologically driven, and understandably seeks to arrive at single minded decisions when more flexible open ended judgements are required which take into account the uncertainties of energy futures and the complex socio-economic issues that are involved in long term decision-making. Providing options as the result of open ended feasibility studies is also a useful way of introducing the specialists within the research programme to the thinking of those outside and causing a cross-fertilisation of ideas.

8.2.2 Independent Appraisal.

One of the reasons why external and independent specialists are necessary to major appraisals arises from the difficulty for those working within a project, in distinguishing their perceptions of its future from their own individual or collective commitment to it. Using external consultants is, we recognise only a part of the answer. It may be only palliative unless those who eventually have to take the decisions accept their responsibility to lay down the ground rules at the outset.

In this study we have found that laying down ground rules for evaluating fusion is more than usually difficult, because of the nature of the technology, but also because of the strength of the institutional interests involved. It is for this reason that we have included in this report discussions devoted to critical issues in the task of fusion appraisal.

The objective of establishing a conceptual framework is not only that a dialogue can take place between those within the project, but also between those within, and those outside the project. Fusion research has so far largely been evaluated through peer review systems. This is appropriate when only questions of plasma physics are under consideration. But not when economic and social criteria are of major importance.

8.2.3 Management Strategy and Management Structure.

In our view this topic needs to be brought within any feasibility study or review process. We consider that it would be a part of the broadening process necessary as fusion makes the transition from a science research project to a development project. We think that the need for this is reflected in the Commission's document COM(87) 302. This encapsulates the conceptual problems that we have identified above.

Because the fusion programme is funded entirely from the public sector it is not subject to any competitive or market constraint. This makes it all the more important that the processes of internal assessment should be rigorous and set against a recognisable set of criteria. The criteria that are used in COM((87) 302 do not meet this requirement. They are not always transparent, and except on strictly technical matters they are not well supported either with data or by reference to accredited sources (eg. the research documents of the Community on energy futures). We have made a small number of specific suggestions for changes in the proposed articles of the Proposal to go before the Parliament and Council in order to bring them into line with generally accepted perceptions of energy futures. We suggest that studies by expert groups are desirable but they should be regarded as inputs into the co-ordinated Feasibility Study, that we have recommended. We recommend that the study of the expert group on fusion economics should be set aside or referred to another external consultancy for a second opinion.

One of the reasons for the shortcomings we have drawn attention to in the Commission's report lies in the unduly restricted range of management skills that they appear to have at their disposal. So far as we could discover the management is undertaken entirely by scientists and engineers, and largely drawn from within the field of fusion research. We do not doubt that they are extremeley able people. But we find it a little incongruous, for example, that the expert group on economics and the environment contains no trained economist or seasoned environmentalist.

The force of the point we are making will we hope, not be seen as being hostile to the fusion management. WE ARE SOLELY CONCERNED TO DRAW ATTENTION TO THE MISMATCH BETWEEN THE VERY COMPLEX ISSUES THAT FUSION AS A TECHNOLOGY OF THE FUTURE INVOLVES, AND THE PRESENT MANAGEMENT STRUCTURE. WE BELIEVE THAT TO GET THE RIGHT BALANCE HERE, IS EVERY BIT AS IMPORTANT AS THE MORE TECHNICAL ASPECTS OF THE APPRAISAL EXERCISE.

The development of a management strategy to fill the space that now exists will take time, but we envisage that it will begin at different levels. One level, the creation of an acceptable conceptual framework, we have already canvassed at some length. With this we link the need to develop the management structure, with the human and material resources to be able to co-ordinate the fusion programme in all its aspects. With regard to the strategy that guides the day to day operation of the fusion programme, we can say little because we have not had the opportunity to observe it in operation. We understand that there are special problems in guiding an international programme, but our general impression is that while the co-ordination may be painstaking, it may not be sufficiently forward-looking in the leadership that its gives, and that it doesn't always succeed in bringing all the parts of the programme and the Associations together to address key issues - particularly the non-technical issues.

Strategy, structure, and conceptual framework are all important. We judge that the fusion programme is in need of changes in all three areas, coupled with better resourcing, in terms of expert research staff, especially in the non-technical fields. WE SUGGEST THAT IT WOULD BE DESIRABLE TO STRENGTHEN THE CONSULTATIVE COMMITTEE OF THE FUSION PROGRAMME, AND TO CREATE MORE EFFECTIVE LIAISON WITH OTHER DIRECTORATES WITH AN INTEREST IN THE FUSION PROGRAMME IN ADVISING THE COUNCIL AND THE PARLIAMENT. WE ALSO SUGGEST THAT STRONGER LINKS ARE ESTABLISHED WITH THE PARLIAMENT AND ITS ENERGY AND RESEARCH COMMITTEE IN ORDER TO BRING POLICY OF FUSION INTO LINE WITH THAT ON ENERGY.

CHAPTER NINE. PUBLIC ACCEPTABILITY.

9.1 Risk Assessment and the Learning Process.

It is possible that the most problematical of the tests that fusion technologies will face, will be that of public acceptability. As the successor designate to nuclear fission, it will inherit a complex situation. It cannot be sure what the social environment will be in the next century. Perceptions about fusion will be influenced by the factors beyond its control. Public attitudes to high technology are now beginning to undergo critical formation, and these will affect the approach to fusion power.

The technical complexity of a fusion reactor will make formidable demands on risk analysis. Risk assessment of the classic kind, putting numbers to each part of the "event sequence" leading to a release of radioactivity, will be speculative until a reactor has been designed, and it will only gain credibility with operating experience. Any statement about safety, must therefore be heavily qualified, not only by the level of available information, but also by the critical attitude that has developed with respect to risk analysis.

Risk assessment is concerned to project the future possibility of an accident. It arose because of public concern of accidents lying beyond the bounds allowed for in the design and the normal operation of the plant. Initially it became a 'contract' betweeen the operator and the regulator. This comfortable relationship was disturbed by the appearance of a third set of actors - interest groups of various kinds, and members of the public not associated with any interest group. Their appearance has greatly complicated the validation of a plant for the licensing authorities, and the planned benefits of the power utilities. Risk assessment has been moved into the area of public acceptability. What we have now is the assessment of risk assessment. The experts themselves are a part of the process; involved, committed, and not without self interest. A widespread view amongst those engaged in this dialogue, including those with expertise in the risk field, is "that despite an appearance of objectivity, risk assessment is inherently subjective" (Fischoff et al 1980). One analyst has described it "as a kind of science of shooting in the dark" (Cannell, 1986).

The broadening out which has taken reactor validation from technical acceptablity to public acceptability has happened rapidly. It is formalised through public inquiry procedures, referendums, etc. Many technical experts have been exposed to social and psychological forces, that are mot only novel, but which appear to challenge their professional integrity. Where judgments and, very often, decisions were taken on technical grounds, this is being checked, if not challenged by those who believe that they have a right to be consulted, and if necessary to assert their right to decide, even though they may only have a rudimentary knowledge of the technical issues involved. The result is a complex interaction between social groups, involving political institutions, the media, industry and specialised interests. Within that interaction, changes are taking place which affect the distribution of power in the decision-making process. It would be a false simplication to present either the 'supply side' interests or the wider public as being monolithic. It is apparent that going up the learning curve in establishing socio-economic feasibility is, if anything, more difficult than in achieving technical feasibility.

Some facets of this learning process can be identified as follows;

(1) Interest groups, and even more so the wider public, have found themselves ill-informed about the nature of the technology that faces them. Therefore they find that they have to try to close the 'understanding' gap. They seek more information. They insist on more 'openness' from government, research and industry.

(2) Risk assessment as a way of determining technical risk, has not proved to be the answer. It has led to the development of another specialism exercising technically based expertise. But it has not gone unchallenged and is subject to considerable scepticism. The attempts to use it to create a 'risk assurance' message to the public, has led to the opposite of what was intended. It has created a widespread distrust of experts and expertise. As a result there is widespread demand for 'independent' judgement.

(3) The public discussion that takes place about new technologies and their impacts on the environment and on economic and social life, is not susceptible to the logic or the rigour that scientists are accustomed to. It is too political to meet those requirements. HOWEVER, THERE IS AS YET, NO BODY OF CRITERIA BY WHICH JUDGEMENTS CAN BE MADE FOR THE LONGER TERM AND WHICH COMMANDS WIDE SOCIAL ACCEPTANCE. Hence the debate may make no headway because there are what appear to be fundamental disagreements. These are not however written in tablets of stone and they can change quickly in the light of experience.

(4) In the interplay between large technologies, and those who resist the consequent impacts, there is no objective source of appraisal or of decision-making. Decisions on high technology are political decisions. That this is not yet understood or accepted is obvious, and it causes understandable frustration.

(5) Public opinion is itself volatile. It can range from acquiescence to extensive hostility. Dramatic events, enlarged upon sometimes by the media can change public perceptions very quickly.

(6) Public attitudes are shaped by belief systems

paradigms, which differ widely from each other, and if these are not recognised and taken into account, the issues which more directly effect new technologies will not be resolved either.

(7) THE EVOLUTION OF LARGE INSTITUTIONAL INTERESTS IS ASSOCIATED WITH THE GROWTH OF NEW TECHNOLOGIES. THE INSTITUTIONS HAVE BEEN A FACTOR IN POLARISING THE DEBATE BY THE MANNER IN WHICH THEY HAVE WIELDED THEIR POWER. Countervailing forces in the public domain, may be the answer, but are not conspicuous, only by the small amount of power that they wield. Regulatory bodies with legal powers are now extensive, but they have powers which normally fall short of what consumer and environmental pressure groups are seeking.

9.2 Assessing Safety.

Assessment of safety, for some of the reasons we have explored, is now perceived by many specialists as something which can only have meaning if placed in a social and economic context. Some would add a political context as well.

Despite the great changes brought about by the emergence of a large, active and frequently disorganised public opinion the regulatory bodies and the industry continue to talk to each other as though risk assessment is a precise science. It is as though they can do no other than wait to be disproved by significant accidents. The Nuclear Regulatory Committee in the USA abandoned the 1:1,000,000 reactor years syndrome as a method of public reassurance on nuclear power, after the Three Mile Island accident. The UK Nuclear Installations Inspectorate still repeat it as though it had some meaning. The result can only be a public attitude that ranges from scepticism to outright disbelief.

The problem for the fusion programme should be defined in the context of the unresolved problem of how public acceptability is to be expressed and recognised. The solution has to begin by abandoning a rationale which is skewed by an assessment process whose first task is to defend the industry by reassuring its critics that risk is minimal. The responsibility for assessing risk has to be placed on a broader basis, with regulatory bodies strong enough to respond to the concerns of those who feel threatened by nuclear power. This implies a research capacity to make evaluations separately from those made by the industry.

What we have read about safety and public acceptability in the fusion programme as it is projected from its present institutional bases, is sufficient to tell us that the lessons of the last decade have not yet been fully interpreted by the fusion industry. Perhaps this is because it is so far from the operation stage as a power producer, that it is felt to be premature to come to terms with what is happening at the interface between the nuclear and other advanced technologies and the public.

The treatment of waste disposal - ever a sensitive barometer of public attitudes - confirms this. The assumption that waste disposal will be easily managed by low level disposal has been contradicted by recent research of the UK NRPB. It is not difficult to see the wider implications of this failure to be sensitive about the waste problem. To a sceptical public, it is sufficient that there is only the appearance of trying to cover over the problem, for the conclusion to be drawn that this could be symptomatic of an attitude to the environmental effects of fusion as a whole.

The environmental control of fusion will mesh with cost. For example, reducing tritium releases by 50% on 1980 costs amounts to around $170,000 per man rem, (Otway et al 1980). The actual cost to a utility would be a combination of clean-up cost and the levels laid down by regulatory bodies. Siting plants in remote areas would relax the clean up problem, but the trade-off between siting and transmission costs rules it out as a solution. A maximum release of tritium could amount to many tens of millions of curies. Risk assessment studies that conclude that such a release is so improbable as to offer no threat to populations potentially at risk, is not only not likely to reassure - it may have the opposite effect.

Because perception of risk is now central to the determining of public attitudes (indeed it may be the most single important factor), then study of what affects public attitudes would seem to be elementary research exercise for the fusion industry. It is a matter of self interest.

In principle, researchers now start from an agreement that perceptions of risk do not necessarily have anything to do with actual risk. The blame for this is something that the industry itself ought to share because the assessment of risk for which it has been responsible has had to be frequently revised. Belief systems, fear, media dramatisation, alarmist statements, disbelief in experts, self-interest, - these and others will bring about the formation of attitudes, causing some to be totally in favour of nuclear power and others to totally reject it. Most attitudes will lie somewhere between. Once attitudes harden dialogue becomes difficult to develop, especially if the attitudes start from different underlying premises that are strongly felt.

In very briefly looking how those who are taken up with recearcr in fusion power are appraising these sort of acceptability problems, we have been seized by a feeling of deja vu. The fusion experts begin by insisting that fusion is environmentally benign. The reason - because it does not produce fission products and actinides. They offer their assurances to tho public on the basis of this key fact. Of course, this is a difference of substance, between fission and fusion. But how great that difference is in practice, is an open question to [illegible] to make it into a protective technological wall separating fusion from its more risky near neighbours is more an atavistic response that an objective fact. It suggests that the first instinct of the nuclear specialist is still to reassure the public without confronting the problem.

We suggest that the first task is not to reassure the public of today, but to discuss how to approach the problem of establishing credibility tomorrow. If the answer to this question is, implicitly or explicitly, to proceed on the same institutional and political manner that has guided fission into the present state of public scepticism, then the future for fusion could go by default. THERE IS NO REASON WHY FUSION SHOULD BE FUNDED IF IT CANNOT MEET CLOSE SCRUTINY ON SAFETY AND ENVIRONMENTAL GROUNDS. SECURING ACCEPTANCE ON THESE GROUNDS MAY BE MORE DIFFICULT THAN ATTAINING TECHNICAL OR ECONOMIC FEASIBILITY. ACCEPTANCE OF FUSION ON ENVIRONMENTAL GROUNDS COULD BE THE INDUSTRY'S BOTTOM LINE.

APPENDIX 1. THE US FUSION PROGRAMME.

1.1 A Brief History.

This section is based heavily on "Fusion Power. Science, Politics and the Search for a New Energy Source" (Bromberg (1981)) for its history, and on the OTA report "Starpower" for more recent material, and upon discussions with Gerald Epstein from the OTA. Needless to say, neither authors are responsible for any errors of fact or judgement in this appendix.

Since it's beginnings in the early 1950s as some part-time calculations of military nuclear physicists, the American Fusion Programme has gone through four broad phases.

The first phase from 1952 till 1958, could be called the 'Age of Optimism'. Born as a combination of cold war military politics and a high belief and confidence in the ability of scientists to harness nature for society's benefit, the developing centres for (secret) fusion research were provided with ample to finance a rapid expansion (1954: $2m, 1958: $29m). The Chairman of the Atomic Energy Commission, Lewis L Strauss, was a typical 'cold war warrior', who feared defeat by the USSR in the eyes of the rest of the world, was ideal for this highly speculative and empirically based phase.

Pressure for declassification developing within the fusion programme coupled with political pressures led to declassification on the eve of 2nd International Conference on the Peaceful Uses of Atomic Energy. The Sherwood team had been furiously attempting to stage a spectacular demonstration of a Controlled Thermonuclear Reaction at the Geneva Conference. Instead they found previously untheorised plasma 'micro-instabilities'. Together with the discovery that the USSR had not in fact succeeded in the goal of producing thermonuclear neutrons, the 'Age of Pessimism' was ushered in. Although declassification did little to dampen the spirit of international competition, particularly at management level, the awareness that other powers were not any further towards a fusion did make the emphasis on short-term reactor development seem a little inappropriate. In the next period, the scientists would be far more concerned with understanding plasma properties than with reactor development. Scientific feasibility took on the meaning of plasma confinement time, density and temperature rather than producing 'thermoneutrons'. The increasingly long-term and "normal" appearance of the fusion research programme coupled with an interest arould the FBR meant a tightening of budgets leading to competition between the research centres.

In 1970, the sudden congressional and public awareness of environmental issues combined with the discovery of a promising confinement concept, the tokamak, restored the fusion programme fortunes, and it re-entered a period of expansion. In 1972, the politically astute Robert Hirsch became Director of the Division of Controlled Thermo-Nuclear Reaction (CTR). Hirsch's period of office can be characterised by:

1. A strong determination to move decision making away from the project leaders and into the Washington Office.

2. Skilful lobbying of Congress, etc, and use of the press to obtain public support.

3. An emphasis on large machines (particularly the Tokamak) and reactor development and on industry involvement.

In 1971, the Fast Breeder Reactor (FBR), previously seen as environmentally benelovent was running into trouble with the environmental lobby, consequently (with a little help from the fusion lobby) fusion began to be seen as an environmentally clean alternative. The 1973 Energy Crisis spurred the search for long-term non-fossil fuel energy supplies. Management of the Controlled Thermo-nuclear Reaction (CTR) programme was moving under more centralised management with correspondingly broader areas of responsibility. As a consequence, fusion would in future be seen as a competitor with fission.

Finally, in 1977, the political context shifted again. Hirsch departed in dissatisfaction with the Carter changes. CTR was moved into the newly created Department of Energy, further shifting it into the area of just another long-term research programme. The first Secretary of Energy, Schlesinger, shifted his interest from fusion to solar, coal and conservation. The programme leaders insisted that the emphasis on tokamaks be reduced as it was no longer thought to be necessarily a good candidate for commercialisation. Funding started a slow decline in real terms to the present, causing slippage in the programme's timetable. The American fusion programme was moving into a situation where it was in danger of being caught between wanting more money for a full programme but not wanting to ask for too much in case Congress says its too expensive. It was also realised that international cooperation is one way of cutting expenditure but there was an unwillingness to commit the Adminstration to a full international programme because there are strong Defence Department pressures against such a move even Congress could accept that a World Fusion Test Reactor would not be built in the US. Today it seems unlikely that the US Fusion community will get the money it needs to reach its target decision date of 2005, but it is also unlikely that Congress will shelve the programme. Similarly, while full cooperation with the Soviet Union is unlikely, there will probably be some increase in US cooperation with the rest of the fusion powers.

1.2 The Present Context

The application for funding in the USA comes from the Department of Energy. The "Fusion Energy Research, Development and Demonstration Act" sent to the US Congress in 1980 from the Committee on Science and Technology, described the objective very clearly :

> "to provide for an accelerated programme for research, development and demonstration of magnetic fusion energy leading to the early commercialisation of this technology to be carried out by the Department of Energy".

By "commercial", the drafters of the Bill meant:

> "utilised for the production of electricity, hydrogen synthetic fuels, heat and other important applications, before the end of this century."
> (US DOE 1980)

This wording is specific. Indeed so specific as to indicate that the Congress Committee did not have any realistic assessments before it. The future of hydrogen synthetic fuels was (and remains) a questionable objective, and the stipulation of acheiving results in fusion research capable of being applied to commerical enterprise, suggests that Congress had been persuaded to pre-empt the scientific evaluation, and to jump the gun. The too specific definition of objectives both in terms of material potential of fusion and in terms of the time taken to reach the commerical stage, suggests that there were lobbies at work, and that they were able to call upon the concern of legislators that they should be at the ahead of international research in big science and could not tolerate the possibility that some other state or states would take the lead. The possibility that the exercise might be non productive, or be a sub optimal use of resources does not appear to have been seriously explored. To legislate for the conversion of an exceptionally complex and therefore long term R&D programme, to be moved from that stage to a commercial stage, before the scientific feasibility of the project has been demonstrated is a contradiction in terms.

The following general points can be made about the US Fusion Programme;

- The initiation and development of the programme in the '50s and '60s was intensely political.

- Although control of the programme was moved away from the laboratory leaders relatively early in the programme, this did not in itself lead to more rational forms of assessment. For example, the emphasis on large machines during the mid '60s and early '70s onwards was a means of the programme leaders maintaining their centres rather than the best means establishing a set of scientific and engineering objectives[1].

- Similarly, the programme was not correctly located within a more broad set of social objectives of securing a cheap and clean long-term energy supply. For example, the project head from 1957 to 1965, Arthur Ruark, through his impartiality and honesty failed to secure high funding that his successors, Amosa Bishop and Robert Hirsch, were able to do so by promising what the programme was supposed to found out - scientific, engineering feasibility and even economic competitiveness[2].

- The advent of environmental concern led to the fusion community expounding a form of scientific journalism that certainly was not within their realm of expertise to offer[3].

- The programme suffered an abnormal level of severe fluctuations in direction due to heavy political pressures and inadequate management structure/programme appraisal

- Despite the willingness of the fusion community, attempts to involve industry in investment in fusion energy have been unsuccessful, industry's involvement being both marginal and short-lived[4].

[1] For example, James Tuck, head of the Los Alamos Laboratory, said in 1964, "We resisted the temptation to build huge machine or hire large staffs... This sound very virtuous, but I have now come to realize that it was suicidal"

[2] Ruark stated the objective of the programme as being to; "determine the possibility or impossibility of fusion machines producing net power,... statements concerning the probability of attaining net power production, or concerning the production of economical power, lie beyond the limits or our present knowledge." In contrast, Bishop declared himself to be "convinced of [the CTR programme's] eventual success."

[3] For example, two AEC staff, BI Eastlund and WC Gough, in 1971, told the press of the physics and techology of a "fusion torch" with the "vision of lrge cities, operated electrically by clean, safe fusion reactors that eliminate the city's waste products and generate the city's raw materials." Similarly, and not untypically, the New York Times told its readers in the same year that fusion "produces little or no radioactive by-products and [is] virtually foolproof against runaway reactions."

[4] Westinghouse and Allis-Chalmers were involved from a early

date (1954 and 1957) in sponsoring a couple of their scientists to work on fusion, but this was little more than paying expert consultants to keep them informed. In 1957, General Atomic and the Texas Atomic Energy Research Foundation put $5m each into joint research, but in 1967 TAERF withdrew and GA persuaded the government to pick up the tab for their continued project. General Electric were the only private utility with a large interest in fusion, having been involved since declassification. In 1965, the GE Cook Committee carried a detailed assessment of the potential of various reactor concepts and concluded that "The liklihood of an economically successful fusion electricity station being developed in the foreseeable future is small" and the programme was terminated in 1967. Standing Committe member Keith Brueckner caused a sensation when he joined KMS in 1969, saying he would demonstrate scientific feasibility for an ICF concept within 18 months, later KMS was only able to continue under Dept of Energy contract.

4.3 Possible Futures.

It may be expected that the US decision about fusion will be taken at two levels. One is the cost, and past Administrations have emphaised that funding must depend upon results, and as the 1980 Act makes clear, they see fusion operating in a competitive market context. That criteria will be impossible to meet within the time laid down, and the fusion programme will be hard pressed to get the funding it needs. On the other hand the strategic implications for the US of not going ahead with the fusion programmee, if the other powers do, are not difficult to see. The compromise solution will be to suport the proposed International programme - ITER. That, however, brings to the surface fresh complexities, by bringing the great powers together (except China) to collaborate together in a sensitive technology. To do that they have to sink their differences. The implications go wider than the future of fusion, and it will be one indication such a possibility if the US government agrees to put its resources into it.

In January 1987, the $1.5m "Technical Planning Activity" was published, a remarkably detailed document, outlining the funding required to reach a decision on building the IFF by 2005. This report was critically discussed in the OTA Report "Starpower" (OTA 1987) published later in the year (see Appendix 2). The omissions perhaps say more than all the detail. For example, $20b between now and 2005 is an extremely large level of investment in an unknown technology before one is going to look at economic considerations of various reactor concepts and supply scenarios. There is also absolutely no mechanism involved in the Planning Activity for termination of the programme, be it expense or engineering/scientific infeasibility or possible environmental damage. Decisions are only acceptable when they involve choosing between alternative paths forward not between carrying on or stopping, eg "the positive E3 [decision] is appropriate, because some type of long-burn demonstration MUST occur for fusion to advance." [our emphasis].

APPENDIX 2. THE OTA STARPOWER REPORT.

The Potential Role of Fusion.

"Research aimed at developing fusion as an energy source, has been vigorously pursued since the 1950s, and, despite considerable progress in recent years, it appears that at least three decades of additional research and development will be required before a prototype commercial fusion reactor can be demonstrated."

The Policy Context.

The US Department of Energy, who are the managers of the fusion programme are responsible for making a positive evaluation to determine if it will be feasible in the 21st century. "However this schedule cannot be met under existing fusion budgets. The DOE plan requires that either the US budgets be increased substantially or that the world fusion programs collaborate much more closely on fusion research."

Findings.

- "Even if no major surprises are uncovered in reactor engineering, "It will take at least 20 years under the best circumstances to determine whether construction of a prototype commercial fusion reactor will be possible or desirable."
- "It is now too early to tell whether fusion reactors, once developed, can be economically competitive with other energy technologies."
- "Even under the most favourable circumstances, it does not appear likely that fusion will be able to satisfy a significant fraction of the Nation's electricity demand before the middle of the next century."
- "The only resources possibly constraining fusion's development might be the materials needed to build fusion reactors."
- "With appropiate design, fusion reactors could be environmentally superior to other nuclear and fossil fuel production technologies."
- "If fusion technology is developed successfully, it should be possible to design fusion reactors to a higher degree of safety assurance than fission

reactors."

- "There is little to be gained and a great deal to be lost, in introducing fusion before its potential economic, environmental and safety capabilities are attained."
- "It would be unwise to emphasise one fusion feature - economics or safety or environmental advantages - over the others before we know, which aspect will be the most important for fusion's eventual acceptance."
- "Due to the high risk and long lead time before any return can be expected, private industry has not invested appreciably in fusion research and cannot be expected to do so."
- "If the international cooperation "can be extrapolated in the future to an unprecedented level level of collaboaration, much of the remaining cost of developing fusion power can be shared among the world's major fusion programs."
- "International collaboration cannot substitute for a strong domestic research program."
- "A variety of potential difficulties associated with large scale collaborative projects will have to be resolved, and Presidential support will be required."

Future Paths.

The Report identifies four options for the future. The level of funding decreases from Option I to Option IV.

Option I. 'The Independent Path.'

> "To aggressively establish the scientific and technological bases necessary to evaluate fusions's potential... "On average between $500 million and $1 billion per year would be required over the next 20 years, with peak annual funding possibly exceeding $1 billion."

Option II. 'The Collaborative Path.'

> "The collaborative would accomplish the same technical tasks as the Independent Path on a similar time scale."

Option III. 'The Limited Path.'

> "...fusion research would continue but would not be supported at the level necessary to evaluate fusion's potential domestically in the early 21st century."

Option IV. 'The Mothballed Path.'

> "...the magnetic fusion research program would be shut down... it would be implemented in a manner that preserved the existing state of knowledge in the field."

> "CURRENT DEPARTMENT OF ENERGY LONG RANGE PLANS FOR THE FUSION PROGRAM ARE AIMED AT THE 'COLLABORATIVE PATH'. IF RECENT FUNDING DECLINES CONTINUES, HOWEVER, OR IF THE UNITED STATES DOES NOT SUCCESSFULLY ARRANGE ITS PARTICIPATION IN MAJOR COLLABORATIVE ACTIVITIES, THE U.S. FUSION PROGRAM WILL EVOLVE ALONG THE 'LIMITED' PATH."

APPENDIX 3. FAST BREEDER REACTOR AND FUSION COMPARISONS.

The closest technological (and historical) relative of Magnetic Confinement Fusion (MCF) is the Fast Breeder Reactor (FBR), and given the level of uncertainty of fusion power at this time one of the most useful ways of foreseeing the kind of problems that may be experienced in a fusion reactor programme is by analogy with the FBR programme. In their early R&D phases they both shared a common vision of clean, cheap, and virtually unlimited supply of energy. The FBR travelled down the road to demonstration much faster and thus came under public scrutiny as many of the proposed gains to society turned out to be illusory. Electricity produced by the FBR has proved to be more expensive than fission or fossil fuelled generation and has caused a recourse to non-economic or speculative arguments. These vary from the crude arguments that essentially that there is no choice because energy will simply run out as fossil fuels are depleted to the point of extinction. An alternative argument is that fossil fuels will become more expensive, as will Uranium Oxide for thermal reactors and at some point in the 21st century, Fast Breeders will become cheaper and/or more secure. After nearly four decades of fusion R&D when engineering concepts are becoming concretised, the vision of the fusion community is also shifting more towards the kinds of argument used by proponants of the FBR, for example, the statement from the ESECOM Report summary that;

> "Neither the economic competitiveness nor the environmental and safety advantages of fusion will materialize automatically... Research is needed to clarify these possibilities, and a commitment to pursuing fusion's highest potential is needed to ensure that the results of such research are embodied in the mainstream of fusion development."
>
> (Holdren et al 1987)

Statements such as these certainly present a picture of an unknown technology trying to find a place for itself in an uncertain future, rather than the earlier impression that fusion WAS the future. Many now see it as replacing the FBR as the dominant technology sometime during the second half of the 21st century as a backstop against rising FBR generating costs. Others see it replacing the FBR as international concern with damage to the environment makes the FBR politically and socially unacceptable. The FBR's promise of being able to burn radiactive waste and thus 'close the fuel cycle' was indeed a tantalising prospect. However, the environmental prospects of the FBR programme have been tarnished both by fear of nuclear accidents, radioactive pollution and the realisation that reprocessing actually increase the volume of low level and intermediate radioactive waste. As a result the fusion community has become more vocal in expressing it's advantage over the FBR. In 1980, John Clarke, then Deputy Associate Director for Fusion Energy in the US, summarised the main 'potential' advantages of fusion as:

[1] "... their intrinsic safety and low environmental impact should allow siting closer to their points of application."

[2] "... a much smaller radioactivity and waste disposal problem than fission reactors."

[3] "... the flexibility of design inherent in fusion reactors, wherein the energy recovery region is external to the reactor region, such reactors may be used for other purposes, such as the production of hydrogen and nuclear fuel, as well as electricity production."

(Clarke 1980)

The last of these points is neither here nor there. The only other purpose given any serious attention is hybrid fusion, creating fission fuels in the breeding blanket of a fusion reactor, which is generally accepted as incorporating the least attractive features of both systems. Also, as we shall see, a fusion programme may well be hard pushed to breed it's own fuel let alone adding to the Pu stock.

One of the unexpected problems which promises to dog the development of the Fast Breeder is located within the logistics of the fuel cycle and critically, the Pu balance. To develop a Fast Breeder programme, inputs to the Pu stock must increase such that the stock can meet the demands of the existing reactors, and any new ones coming on line. The critical variables are the Breeding Gain (BG), the out-of-reactor-time (ORT) while the spent fuel is reprocessed, the Pu loss associated with reprocessing, the intitial size of the Pu stock, and the timing of the programme. There is every reason to believe similar constraints would apply to a fusion programme. Figure Ap3.1 and Figure AP3.2 show the fuel cycle for the Fast Breeder and fusion respectively. Both fuel cycles are subject to four major contraints;

Constraint 1. Inputs to initial stock.

Initially, the FBR's Pu stock is obtained by reprocessing the waste of the thermal reactor programme. It was thought that the phasing out of the thermal reactor programme would be possible after a while as the FBR programme became able to breed its own fuel from $Uranium^{238}$. However, it would appear that given other fuel parameters this would severely constrain the speed of the FBR programme so for the time being at least the two types of reactor are seen as 'complementary'.

The stock of Tritium available for a fusion programme must be seen as limited. Tritium currently costs around currently £10000/g. This would involve a prohibitively high fuel bill for a fusion reactor. Consequently, the substitution of such supplies by Tritium bred in the reactor blanket must be a priority of a commercial MCF programme.

Constraint 2. Reprocessing loss.

Estimates for the Pu loss in reprocessing FBR blanket material generally vary between 2% and 6%. For the proposed British programme with the most favourable parameter values (ORT: 9 months, BG: 1.2), an increase in Pu loss from 2% to 6% results in an increasing in the Doubling Time (DT) from 30 to 86 years (Sweet 1982).

There has been very little work done on the Tritium reprocessing concepts. Current conceptual designs for a prototype commercial fusion reactor involving either liquid Lithium and Lead (Li/Pb) breeding elements or solid Lithium metasilicate involve oxidising the Tritium into T_2O. This will be necessary as Tritium is highly mobile and diffuses readily through structures. In the absence of any detailed studies into Tritium loss involved in reprocessing and storage, one can only assume that it is likely to be greater than loss associated with Pu reprocessing. In absence of any estimates on Tritium loss, values between 4% and 12% might be reasonable.

Constraint 3. Out-of-Reactor-Time.

The time spent reprocessing emerges as a crucial constraint in the fuel cycle equation. For the UK programme with most favourable parameters (Pu loss: 2%, BG: 1.2), the doubling of the ORT from the most favourable 9 months to 18 months would result in a Doubling Time increase from 30 years to 53 years (Sweet 1982). The nine month ORT is recognised as not being achievable for some time (at least 20 years) and is only being pursued because the penalty for doing so would be rapid expansion of the thermal programme!

We can find no estimates for the possible ORT for Tritium reprocessing as the discussion of the technology is at such a rudimentary stage. However, it is possible to predict that an unfavourable ORT for a fusion programme would assume a far more critical role due to the very short half-life of Tritium. The short half-life of Tritium (12.36 years) is usally presented as an unambiguous plus for fusion over fission, as it greatly reduces the environmental risk from fuel leaks, and the storage of long-term nuclear waste.

However, the decay rate is so fast that it makes the reprocessing and storage of Tritium extremely unfavourable for maintaining a positive Tritium balance. For example, an ORT of 1 year would result in Tritium loss through decay of 5.45%, an ORT of 3 years would mean a Tritium loss of 15.48%. This, compounded with an as yet unknown reprocessing loss, could prove fatal to the Tritium balance and the timing of the programme. The question of decay in the Pu fuel cycle is simply insignificant due the very long half-life of Pu. It was thought that Lithium would be suitable to circulate through the breeding area with "online extraction". However, this approach was deemed unworkable due the high fire risk and high stored energy content involved, so present designs use Helium as the coolant with a Copper Oxide bed to oxidise the Tritium content into T_2O (IAEA 1985A). If this process is as straightforward as people hope, then the ORT may indeed be very short, but there may be unforeseen problems in this infant technology such as impurity control and cooling problems. It would not be wise at this stage to ASSUME there will be no problems with reprocessing Tritium which may lead to decay time becoming an important factor in a fusion power programme's fuel cycle logistics.

Constraint 4. The Breeding Gain.

The breeding gain is the ratio of fuel out to fuel in. For an FBR estimates vary between the optimistic value of 1.2 to the perhaps more likely value of around 1.1. Higher ratios are theoretically obtainable but it has to be remembered that there is a highly constrained trade-off between breeding and power output. Simply, the more neutrons used to breed the less are available for conversion into electricity. Current BG estimates for fusion are greater than or equal to 1.35 and 1.5 for solid and liquid breeders respectively (IAEA 1985A). At this stage of design it is unwise to attach too much importance to these figures except as upper limits on breeding gain. The reactor concepts involved may be seen as an exercise in trying to get the most favourable results by running other parameters at their limits. It would be surprising if this exercise could be translated into reality without the relaxation of at least some of these assumptions given the extremely immature nature of the project.

Feasibility of fusion as a source of energy cannot be demonstrated by reference to a single reactor design and operation. The essential requirement is a system model which will display the logistical requirements in the fuel cycle, reactor operation, supply of essential materials, etc. From this model a realistic idel of the scale and cost trade-offs of a fusion programme might be derived. The Fast Reactor system provides a useful starting point because of the similarities between the two systems.

Figure Ap3.1. FBR Fuel Cycle.

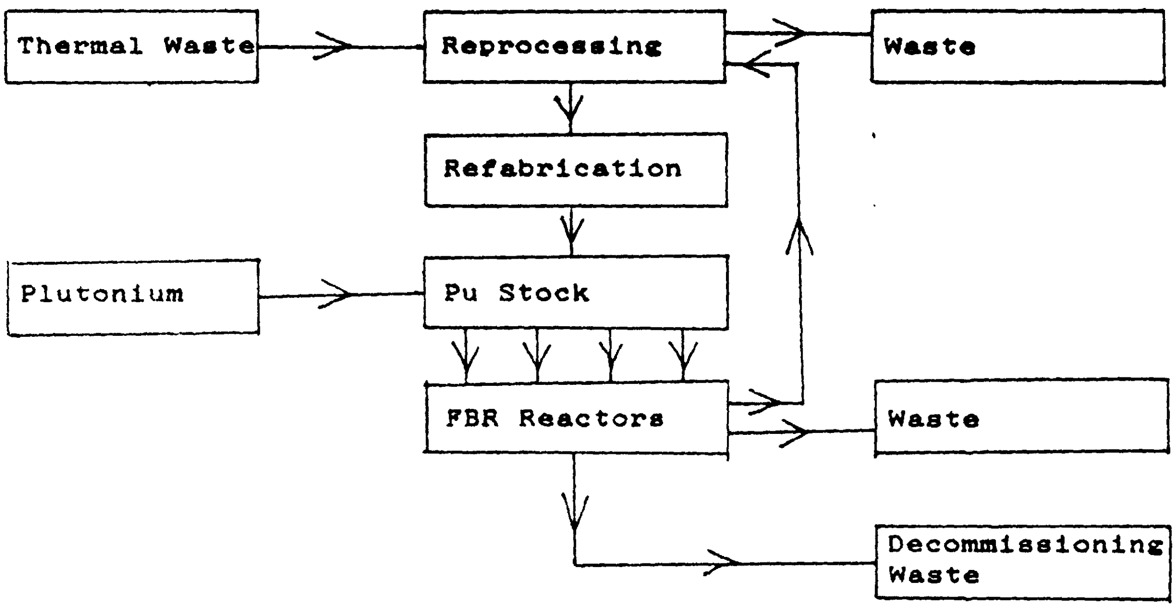
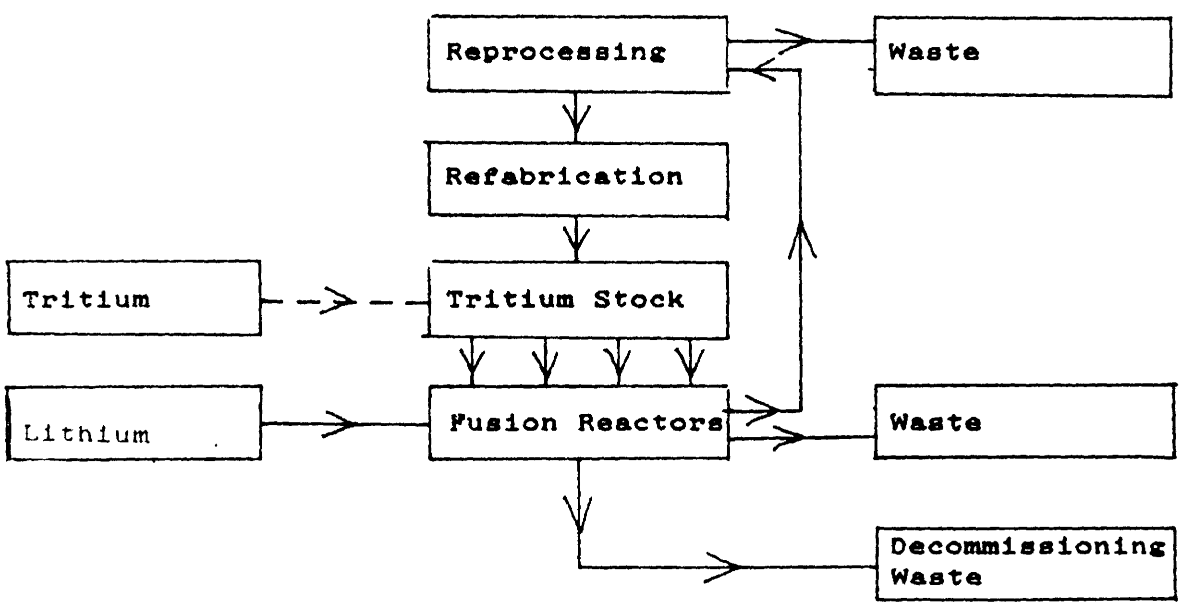


Figure Ap3.2 Magnetic Confinement Fusion Fuel Cycle.



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