COMMENTARIES ON CRITICISMS OF MAGNETIC FUSION

by

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Summary—This report provides brief commentaries on a representative set of the criticisms of magnetic fusion which have been published over the years and includes annexes which summarize technical information in support of the commentaries.

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I. INTRODUCTION

The arguments for developing magnetic fusion energy would seem to be persuasive. The energy content of the lithium resources that will be used to produce tritium for the D-T cycle is probably greater than the energy content of fossil or uranium fuels, and the fusion fuel is virtually unlimited when advanced fuel cycles are included. Fusion energy will be environmentally benign—there is no CO_2 produced, and decommissioned fusion reactor materials should qualify as low-level-radioactive waste for disposal by shallow land burial, or even for recycling. Projected costs of electricity from fusion reactors are within less than a factor of two of costs projected for other energy sources, without any credit for environmental advantages.

Within the magnetic fusion program, the majority perception is one of substantial achievement and good prospects for continued success. The main line of research—the tokamak—has been developed to the point that it is now possible to undertake the penultimate step in magnetic confinement plasma physics research—the investigation of ‘burning’ plasmas in which the vast majority of the heating is provided by the fusion event itself. A detailed engineering design, supported by substantial technology R&D, has been developed for a tokamak experiment which would explore burning plasma physics and integrate reactor-relevant technology. In the tokamak research program, recent advances in controlling the internal configuration of the plasma have led to the achievement of substantially improved energy and pressure confinement in tokamaks—the so-called ‘advanced tokamak’ modes—which reduces the projected cost of electricity from tokamak reactors by a factor of two to a value only about 50% more than the projected cost of electricity from advanced light-water reactors. In parallel, progress in the development of advanced, low-activation structural materials supports the promise of environmentally benign fusion reactors, and research into alternate confinement concepts is yielding promise of future improvements in confinement.

Although the scientific and technological progress in magnetic fusion has been substantial and exciting to fusion researchers, funding levels for fusion research have dropped substantially. The perceptions of the status and prospects for magnetic fusion research that are held by decision-makers and some other influential persons outside the program appear to be at considerable variance with the perceptions of the majority of the scientists and engineers working within the magnetic fusion program and more in accord with the views promulgated by the small, but articulate and persistent, cohort of critics of the magnetic fusion program. Thus, it seems timely to examine the technical basis of the arguments set forth by the critics of the magnetic fusion program, which is the purpose of this report.

II. SUMMARY

While each of the critics of magnetic fusion has an unique viewpoint and logic, there are certain common elements which appear in several of the criticisms. At the risk of oversimplifying, an attempt is made in this section to distill from the several specific criticisms a set of common criticisms and to comment briefly on each.

Engineering heat transfer limits inherently require very large magnetic fusion reactors.

With presently existing materials (i.e. stainless steel), the engineering heat transfer limits and the physics constraints both dictate about the same minimum first wall (of the chamber surrounding the plasma) radius for tokamaks. With advanced structural materials currently under development, first wall heat fluxes up to a factor of about six larger can be tolerated, so the minimum first wall radius of tokamak reactors will likely be limited by physics constraints (at about one meter or less), not by engineering heat transfer limits. The miminum size of reactors based on alternate magnetic confinement concepts which project operation at higher power densities than the tokamak may possibly be inherently limited by the first wall heat transfer limit, but this has not yet been demonstrated and is not at present a major concern, provided that the ongoing development of advanced structural materials is successful.

Neutron damage would require frequent replacement of magnetic fusion reactor components.

The high energy neutrons from D-T fusion would damage a first wall made of existing materials (i.e. stainless steel) sufficiently to require replacement of the innermost few centimeters surrounding the plasma chamber approximately annually. However, these neutrons are degraded in energy, hence become less damaging, as well as diminished in number as they diffuse into the material behind the first wall facing the plasma, so that the frequency of component replacement decreases rapidly with the distance from the plasma neutron source, with components more than about one-half meter from the plasma surviving for the lifetime of the plant, even if made of stainless steel. Components such as the vacuum vessel, the shield and the magnets would be designed for the lifetime of the plant. With the advanced structural materials presently under development, which are much more immune to neutron damage, the frequency of first wall replacement will be much reduced.

Magnetic fusion reactors would be too complex to work reliably.

Magnetic fusion reactors, as presently conceived, would be complex, but this does not mean that they would be unreliable. Engineering solutions for dealing with the complexity of magnetic fusion reactors are being developed, most notably in the engineering design of ITER (_1500 man-yrs with _$800M supporting R&D), which embodies many of the features of a reactor. This design was evaluated by four national and one international review boards, who found it to be workable and to have a high probability of meeting the demanding physics and engineering mission. Disciplined engineering development has produced complex systems that work well (e.g. the modern jet airliner), and there is no reason to suspend belief in the efficacy of disciplined engineering development in the case of magnetic fusion reactors.

Magnetic fusion reactors would not be economically competitive.

The projected cost of electricity in the middle of the next century from magnetic fusion reactors is about 25% greater than from coal plants, about 50% greater than from advanced light-water fission reactors, and about 100% greater than from natural gas plants. These projections do not take into account any CO_2 'taxes' that might in the future be associated with coal and natural gas plants nor any waste disposal 'credit' vis-a-vis fission plants that might result from disposal of materials from decommissioned fusion plants as low-level radioactive waste suitable for shallow land burial or by recycling. The improved physics of the 'advanced tokamak' modes of plasma operation which have been developed over the last few years reduce the projected cost of electricity from tokamak reactors by about 50%, and future physics and engineering innovations may further reduce the projected cost of electricity.

The present emphasis on the D-T fusion fuel cycle should be reduced in favor of increased emphasis on neutron-free advanced fusion fuel cycles in order to avoid neutron radiation damage and activation..

Advanced fuels introduce some new problems as well as solving the neutron-related problems. Because of the lower fusion reactivity, the power density in the plasma for a given plasma pressure is 50-100 times smaller for any other fusion fuel than for D-T, and the required confinement is 25-50 times larger, which would require either operation at much higher pressure in order to achieve a comparable power density or a much larger plasma volume in order to achieve the same power output. This size disadvantage may be somewhat mitigated by the reduced shielding requirement with a neutron-free fuel cycle, but the substantial fusion reactivity advantage of D-T with respect to any other fusion fuel should enable the design of more efficient D-T fusion reactors based on any magnetic confinement concept. Another point, which is often overlooked, is that the neutrons from D-T fusion carry 80% of the fusion energy through the first wall to be deposited over the volume behind it, leaving at most 20% of the fusion energy to be transferred as heat through the first wall. On the other hand, as much as 100% of the fusion energy (less any that is diverted) must be transferred as heat through the first wall with neutron-free advanced fuels. There are more promising ways to deal with the neutron problems in the near term---advanced structural materials presently under development should substantially reduce the neutron radiation damage problem and lead to fusion reactor materials which can be disposed of by shallow land burial as low-level-waste upon reactor decommissioning or even recycled for further use. However, advanced fuels provide an interesting opportunity for the long term.

The present emphasis on the tokamak confinement configuration should be substantially reduced in favor of increased emphasis on alternate confinement concepts which have better reactor prospects.

Although the various alternate confinement concepts each has some feature that promises improved reactor prospects, there is no other concept which presently projects to a more economical fusion reactor than the tokamak, based on the substantial fusion reactor conceptual design studies to date, which project little difference in the cost of electricity in the middle of the next century from fusion reactors based on the tokamak, stellarator, spherical torus and reversed field pinch. Furthermore, while there have been interesting recent developments in alternate confinement concepts, they remain far behind the tokamak in stage of development. The tokamak has achieved plasma parameters within a factor of 20 of values required for a reactor, is the only magnetic confinement configuration in which the next major issue of fusion science—burning plasma physics--can be investigated in the foreseeable future (probably the next few decades), and has recently achieved substantial improvements in energy and pressure confinement by operation in ‘advanced tokamak’ modes. The required improvement in confinement needed for a reactor is 20 for the tokamak, _1,000 for the stellarator, and __100,000 for the spherical torus, the mirror, the reversed field pinch and the field reversed configuration. Inertial fusion is sometimes mentioned as an alternative to the tokamak; the required improvement in confinement needed for a reactor is _1,000 for direct drive inertial and _100,000 for indirect drive inertial. These facts would seem to support a continued emphasis on the tokamak in order to move the fusion program forward and exploit the capability of the tokamak for investigation of new realms of plasma science, combined with an exploration of alternate concepts at a level commensurate with actual achievement and improved reactor prospects in order to identify opportunities for future improvements.

III. COMMENTARY on “The Trouble with Fusion” by Lawrence M. Lidsky, published in the October, 1981 issue of *Technology Review.*

Lawrence Lidsky has spent his professional career as a Nuclear Engineering professor at MIT. He had worked in magnetic fusion for about 20 years when he left the field in the early 1980s with a broad denunciation of the direction of the fusion program and its prospects for producing a practical power reactor. His comments received widespread attention at the time because of the prestige of his institution and of his mentor, David Rose, a pioneer of the reactor engineering aspects of the field, and because of professor Lidsky’s own broad interest in the physics and engineering of fusion.

Professor Lidsky’s principal theme is that the choice to develop deuterium-tritium (D-T) fusion, rather some other fusion fuel cycle such as proton-lithium or proton-boron, which produces fewer neutrons or no neutrons at all, is a mistake. He agrees that the choice of D-T fusion makes sense as a scientific objective, acknowledges that “we may well achieve that goal, which would be a scientific triumph”, and bases his objections on the engineering difficulties which he perceives in making a practical D-T fusion reactor. “The most serious difficulty is the very high energy neutrons released in the D-T reaction. These uncharged nuclear particles damage the reactor structure and make it radioactive. A chain of undesirable effects ensures that any reactor employing D-T fusion will be a large, complex, expensive, and unreliable source of power....... The requirement is to develop a power source significantly better than those that exist today, and D-T fusion cannot provide that solution. Even if the fusion program produces a reactor, no one will want it.....The only real hope for fusion is to take the long view....Neutron-free fusion is a quintessential example of a high-risk, high-gain area of physics that *might* also provide a good answer to an engineering problem. ”

Lidsky marshals a number of arguments to support his contention that D-T fusion reactors are inherently large and complex. His argument for large size is based on arguments for the inherent minimum size of each of the several components extending from the plasma chamber outward through the surrounding blanket, shield and magnets.

He argues that the radius of the plasma must be at least 2-3 m if the fusion reaction is to be self-sustaining and further argues that “even if a breakthrough in physics were to allow a smaller plasma, separate engineering requirements would prevent the radius of the first wall from being appreciably less than three meters.” Present physics experience indicates that a plasma radius of 1 m or less will suffice in an ‘advanced tokamak’ mode of operation, a “breakthrough in physics” which is presently being diligently developed. The engineering heat transfer limitation on the minimum first wall radius (_ plasma radius) in a 1200 MWe D-T tokamak reactor with stainless steel structure and water coolant is about 1.5 m, and with an advanced structural material and lithium coolant, which are under development, would be considerably less than 1 m (technical annex 1). Thus, current knowledge indicates that the minimum practical first-wall radius might be more like 1m than Lidsky’s estimate of 3 m.

Lidsky’s estimate of the required blanket thickness of 0.5-1.0 m probably should be replaced by 0.5 m, but his estimates 0.5-1.0 m shield thickness and 1 m magnet thickness have stood the test of time.

From these component thicknesses, we would today estimate a minimum total radius of the plasma column and surrounding components of 3-4 m, as compared to Lidsky’s estimate of 5 m. If this plasma column and surrounding components were ‘bent’ into a torus to form the tokamak configuration, the major (large) radius of the torus would be about 5-6 m. By comparison, a recent detailed study[1] led to a 1000 MWe tokamak reactor with a first wall radius of 1.4 m and a major radius of the torus of 5.6 m.

However, such a comparison of dimensions is only part of the story. Balance-of-plant costs (those costs for systems other than the reactor) usually will be a larger part of the total plant cost than the reactor costs for fission reactors, and balance of plant costs should be similar for fusion and fission plants of similar power output. The cost of fuel (much greater for fission than fusion reactors) and the operating, decommissioning and waste disposal costs must also be taken into consideration. Based on our present understanding, D-T tokamak fusion reactors project[2] a cost-of-electricity that is about 50% larger than the projected cost-of-electricity from advanced light-water reactors in the middle of the next century. Possible materials recycling credits for fusion are not yet included in these estimates. With the structural materials that are under development, there is a strong possibility that decommissioned fusion reactors could be disposed of as low-level-waste[3,4] or recycled[5], which should reduce the true cost differential between fusion and fission reactors.

Furthermore, there is no reason to believe that the substantial progress that has been made in closing the gap between projected electricity costs from D-T fusion reactors and fission reactors (and between fusion and fossil electricity costs) that has been made since Lidsky's article will not continue. The so-called 'advanced tokamak' plasma performance with improved pressure and energy confinement, which has been discovered over the past decade (technical annex 2), projects to 50% reduction in the cost of energy from commercial tokamak reactors, relative to the previous projections based on 'conventional' tokamak plasma performance.

The less tangible complexity issue raised by Lidsky, which bears upon reliability and achievable plant factor, hence on cost of electricity, remains today. However, we know a lot more about the problem and the development of engineering solutions to deal with it today, in large part due to the detailed engineering design[6] of ITER (an international tokamak project with a physics and an engineering testing mission) and the performance of supporting R&D[7] to confirm engineering design solutions for all the major components of a tokamak experimental reactor. This effort involved about 1500 man-years of engineering design effort and about $800M of supporting R&D. The results were reviewed both internationally and separately in Europe, Japan, Russia and the United States; the consensus judgement was that a machine based on this design would have a high probability of accomplishing the demanding integrated physics-experiment/ engineering-test-reactor mission of ITER. Disciplined engineering has made a success of many complex systems (e.g. the modern jet airliner), and there is no reason to suspend belief in the efficacy of disciplined engineering when it comes to fusion reactors.

Lidsky supports his argument for complexity with the disingenious statement "temperatures within the fusion reactor will range from the highest produced on earth (within the plasma) to practically the lowest possible (within the magnets)." While this is true, the plasma consists of confined energetic particles in a vacuum, but the material temperatures in a fusion reactor (which will be the source of any engineering complexity associated with temperatures) will be lower than the fuel temperatures in a fission reactor or the temperatures within a jet engine.

Lidsky returned to the issue of engineering heat transfer limitations for D-T fusion reactors, which led him to "...a devastating critique of fusion. For equal heat-transfer rates, the critical inner wall of the fusion reactor is subject to ten times greater neutron flux than the fuel in a fission reactor." Because of the difference in the number of neutrons and the amount of energy produced in fission and D-T fusion reactions, there are about 3 times more fast neutrons created per second in a D-T fusion reactor than in a fission reactor operating at the same power level. The fast neutrons in a fusion reactor are not only more numerous but also about 10 times more energetic, hence more damaging, than the fast neutrons in a fission reactor. However, development of a structural material that is relatively immune to the damage caused by these fast neutrons has been the principal focus of fusion materials research worldwide for more than two decades, and new ferritic steels and vanadium alloys show considerable promise[8] of leading to fusion reactor 'plasma-facing' components that would only need be replaced a limited number of times during the 40-year lifetime of a fusion reactor. Components separated from the plasma by more than about one-half meter, such as the vacuum vessel, shield and magnets, would by lifetime components. Lidsky's 'critique' certainly highlighted an important problem area, where substantial R&D is still needed, but no one familiar with the subject considers it to be an insurmountable problem or "a devastating critique of fusion."

Our understanding of the physics and the technology of fusion and of their interaction has changed since the time of Lidsky's paper. As discussed above, the heat transfer rate across the first wall is less of an inherent limitation on the size of a tokamak D-T fusion reactor than is the plasma confinement, according to present understanding. Furthermore, Lidsky's assertion that the heat flux on the first wall would necessarily increase as the size of the plasma chamber increased, making it impossible to reduce the heat flux by increasing the first wall area, is not in accord with contemporary understanding.

The lower power density issue raised by Professor Lidsky is just another way of stating the 'larger size' issue discussed above. Because of the lesser density of fuel nuclei in a fusion reactor than in a fission reactor, the volume over which the same amount of nuclear power is produced is about 10 times larger for a fusion reactor than for a fission reactor, but this power-producing region in a fusion reactor is essentially a vacuum containing charged particles. Again, it is cost of electricity that is important, not the volume of the region within which the nuclear energy is produced, per se, as discussed above.

As for the panacea of neutron-free fusion offered by Professor Lidsky and others, there are two notable problems (technical annex 3). The first problem, which is widely recognized, is that any other fuel would have a much lower reaction rate than D-T, which would require either operation at much higher pressure in order to achieve a comparable power density or a much larger plasma volume in order to achieve the same power output. The second problem, which is not widely appreciated, is that with any neutron-free fuel, 100% of the fusion power (less any that is diverted) must be transferred as heat through the first wall of a closed magnetic confinement concept such as the tokamak, exacerbating by as much as a factor of five the engineering heat transfer problem that was central to Professor Lidsky's criticism of D-T fusion reactors

In summary, the prospects for D-T fusion reactors are not perceived so dimly by workers in the field today (nor were they at the time) as Professor Lidsky saw them 18 years ago at the time of his departure from those ranks. The problems that he identified were recognized at the time and are yielding to engineering R&D. His suggestion of alternate fusion fuels would exacerbate rather than solve the central engineering heat transfer problem raised in his article.

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IV. COMMENTARY on "Insurmountable Engineering Problems Seen as Ruling Out 'Fusion Power to the People' in 21st Century", Letters to the Editor by William E. Parkins, James A. Krumhansl and Chauncy Starr in March, 1997 issue of *Physics Today*.

Dr. Parkins is a retired Director of Research and Technology for the Energy Systems Group of Rockwell International who spent his career in the fission reactor field. Dr. Krumhansl is a distinguished materials scientist, a former president of the American Physical Society and a Professor of Physics and Director of the Solid State Laboratory at Cornell. Dr. Starr is retired after a distinguished career in the nuclear fission field, capped by his presidency of the Electric Power Research Institute.

Dr. Parkins' central theme is that "fusion reactors...hopeless because of engineering considerations", citing as "the principal engineering factor....that heat cannot be extracted from within the reacting region....but must be gathered outside of the plasma. Engineering limitations on maximum heat transfer rates....would require the fusion reactor to be of huge dimensions for the relatively small amount of power produced." In fact, the *inherent* engineering heat transfer limits on the size of a D-T fusion reactor are even less limiting than the presently understood physics limits for plasma confinement, at least for tokamaks. For example, a tokamak reactor producing about 1200 MW electrical power would be constrained by first wall (surrounding the plasma chamber) heat transfer capability to a minimum first wall minor radius of about 1.5 m and a minimum first wall area of about 600 m^2, if the first wall was made of existing stainless steel structure and was cooled by water. Advanced structural materials and liquid lithium coolant, which are under development, could reduce the heat transfer constraints on the minimum first wall radius to about 0.4 m and on the minimum first wall area to about 40 m^2, or less (technical annex 1). Dr. Parkin's central thesis that the engineering heat transfer requirement "would require gargantuan dimensions" is not supported by technical analysis based on what is known today.

Dr. Parkins further states " the use of any thermonuclear reaction that releases neutrons results in yet another insurmountable engineering obstacle." This is indeed a surprising statement coming from an engineer who spent his entire career in the fission reactor field; certainly the many hundreds of fission reactors operating successfully worldwide are persuasive evidence that a nuclear reaction that releases neutrons does not constitute an insurmountable engineering obstacle. From his statement that "no material can provide an operating life that does not require periodic (vacuum) vessel replacement", it appears that Dr. Parkins is not aware that conceptual designs of fusion reactors over the last couple of decades have placed about half a meter of blanket material between the high energy neutron source in the plasma and the vacuum vessel, for the express purpose of reducing the neutron irradiation of the vacuum vessel so that it does not need to be replaced over the lifetime of the reactor.

Dr. Parkins goes on to conclude that "leaks (in the vacuum vessel) would be unavoidable. Locating and repairing them by remote means in an inaccessible geometry would not even be imaginable.…because of the size of the vessel and the number of joints and connections...and thermal stresses from variable and high temperatures." He has put his finger on a substantial, but not insurmountable, engineering challenge, although he is wrong about the vacuum vessel operating at high temperature. The engineering problem of locating and repairing leaks in large and complex vacuum vessels has been dealt with successfully in the present large tokamak experiments--JT60-U and JET--as well as in numerous smaller experiments, albeit in a non-radioactive environment. One of the major tasks of the ongoing ITER R&D program[1] addresses the remote fabrication and maintenance of the vacuum vessel.

Fusion reactors will be complex, but this does not mean that they will be unworkable. I suspect that had Dr. Parkins, or almost anyone else, looked at the design of a modern jet airliner 50 years ago, they would have concluded that it was too complicated to work, but the lesson there is that disciplined engineering can make complicated systems work. There is no reason to suspend belief in what can be accomplished by a disciplined engineering effort. That effort has begun for magnetic fusion.

Prof. Krumhansl expresses the concerns that "there is a startling lack of a foundation of information on materials for fusion technology in the US....the current level of support for materials research and facilities is totally inadequate..." and goes on to recommend that "it would be far better if something like a third of the current US fusion budget were spent addressing these materials and engineering concerns..." While anyone familiar with the current situation would agree that the level of support for fusion materials research is incommensurate with the significant technical issues remaining, Professor

Krumhansl is incorrect about the lack of a foundation of materials information. There was until quite recently a substantial fusion materials program in the US, and substantial programs continue to exist in Europe and Japan. The status of materials for near-term fusion applications is well documented in the recent ITER R&D[1] and design[2] reports and in the multi-volume Fusion Materials Handbook[3], and the status of the development programs for advanced structural alloys was recently summarized[4]—these and related publications document a considerable foundation of materials information for fusion. If the fusion program was moving progressively forward towards the development of fusion power with a budget several times larger, Professor Krumhansl's recommendation to redirect one-third of it to materials research probably would have already been implemented.

Professor Krumhansl is also critical that "...neither experienced design engineers nor cost estimators have been significantly involved in the US fusion program...." This statement is incorrect. In fact, there has been a substantial involvement of industrial engineers and cost estimators in fusion reactor design studies. The most significant industrial participation was in the recently completed detailed engineering design of ITER[2] and the performance of supporting R&D[1] to confirm engineering design solutions for all the major components of a tokamak experimental reactor. This effort involved about 1500 man-years of engineering design effort and about $800M of supporting R&D. The results were positively reviewed internationally and separately in Europe, Japan, Russia and the United States.

Dr. Starr bases his criticism of fusion power on parallels with the development of fission, noting that "This (the present fusion) situation is very reminiscent of the optimism based on conceptual designs that pervaded the fission community in its earlier days....It is now obvious that fusion power would face similarly severe barriers (as those encountered in the development of fission power)....and it is difficult to envision fusion power ever approaching the economics of commercial fission power plants...." The fact that much of the same 'nuclear' technology and experience that was developed in the fission reactor program and in the tritium production program can be adapted and extended for the fusion power development program notwithstanding, those knowledgeable of the field would agree that the successful development of fusion power will require a large-scale, disciplined engineering effort integrated with a scientific program, but this does not in any way imply that it can not be done.

Based on our present understanding, D-T tokamak fusion reactors project a cost of electricity that is about 50 % larger than the projected cost-of-electricity for advanced light water reactors in the middle of the next century[5]. Possible recycling credits are not yet included in these estimates, and, with the advanced structural materials that are under development[4], there is a strong possibility that decommissioned fusion reactors could be disposed of as low-level-waste[6,7], or recycled[8], which should reduce the true cost differential between fusion and fission. Furthermore, there is no reason to believe that the substantial progress that has been made in closing the gap between projected electricity costs from D-T fusion reactors and fission reactors (and between fusion and fossil electricity costs) that has been made in recent years will not continue. The so-called 'advanced tokamak' plasma performance with improved pressure and energy confinement, which has been discovered over the past decade (technical annex 2), projects to 50% reduction in the cost of electricity from commercial tokamak reactors, relative to projections based on 'conventional' tokamak plasma performance.

Dr. Starr questions the wisdom of "...assuming fusion's long-term sucess as a policy basis for diminishing the development support for more realistic long-term alternatives, particular nuclear fuel recycling and breeding." Since an adequate and reliable supply of environmentally tolerable electrical power is essential for the well-being of the nation and the world, a rational national energy policy would provide for the development of fusion power, nuclear fuel recycling and breeder reactors.

Drs. Parkins and Starr reiterate in these letters opinions which they developed and expressed more than twenty years ago, as Dr. Parkins acknowledges, and it is not apparent that they are familiar in detail with the developments which have taken place in the intervening years. Dr. Starr acknowledges that his letter is motivated by a concern that money spent on the development of fusion will not be spent on further development of nuclear power (his field), and Professor Krumshansl expresses unabashedly his opinion that part of the money spent on fusion would be better spent on materials research (his field).

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V. COMMENTARY on "The US Fusion Program at a Crossroads", two papers by Weston M. Stacey and by Robert L. Hirsch, Gerald L. Kulcinski and Ramy Shanny in the Summer, 1997 issue of *Issues in Science and Technology* and a series of responses in the Fall, 1997 issue by Kenneth Calvert, Chauncy Starr, Robert W. Bussard, Stephen O. Dean, William E. Parkins and James Adams.

Dr. Hirsch has held several technical administrator positions, including Director of the DOE Office of Fusion Energy in the early 1970s and more recently with the Electric Power Research Institute. Dr. Kulcinski is a Professor of Nuclear Engineering at the University of Wisconsin and a distinguished researcher in fusion materials and fusion reactor conceptual design. Dr. Shanny is a plasma physicist who has worked at NRL and for International Nuclear Energy Systems (see below) and is currently president of Advanced Power Technologies. The major thrust of their paper was that tokamak reactors could not satisfy a set of criteria for new power sources developed at EPRI by a panel of electric utility technologists.

They quote an estimated ratio of 30 for a fusion reactor 'core' cost to a fission reactor 'core' cost to support their contention that a fusion reactor could not be economical. While they do not reference the source of this number, there is probably some 'comparing of apples and oranges' here (the actual reactor core cost is usually less than 10% of the total direct cost of a fission power plant), and the larger balance-of-plant costs (which should be comparable for fission and fusion) and the operating costs are not mentioned. The important cost, in any case, is the cost of electricity, which also includes fuel costs (much less for fusion) and other operating costs. A recent study[1] projects that a tokamak fusion reactor would produce electricity for about 50 % more than an advanced light-water fission reactor in the middle of the next century. This estimate does not yet take into account any differential in decommissioning and waste disposal costs or recycling credits that should result if fusion reactor wastes can be classified as low-level-waste, which seems a strong possibility[2,3], or even recycled[4].

Hirsch, et al. cite additional problems due to neutron damage of materials and the need to produce tritium in a high-temperature environment in a fusion reactor. The fast neutrons in a fusion reactor are not only about 3 times more numerous but also about 10 times more energetic, hence more damaging, than the fast neutrons in a fission reactor. However, development of a structural material that is relatively immune to the damage caused by these fast neutrons has been the principal focus of fusion materials research worldwide for more than two decades, and new ferritic steels and vanadium alloys show considerable promise[5] of leading to components that would only need be replaced a limited number of times during the 40-year lifetime of a fusion reactor. While there is considerable experience in tritium production from the military tritium production reactors, and while a significant conceptual design and supporting R&D effort has been devoted to tritium production in fusion reactors, Hirsch, et al. are correct that tritium production and continuous recovery at high temperatures requires substantially more R&D. These are engineering problems to which there appear to be solutions, and these solutions are under development.

The solution proposed by Hirsch, et al. to the perceived problems of D-T tokamak reactors is to "....devote much more effort to developing so-called advanced fusion fuel cycles with low or zero neutron fluxes....smaller fusion systems are likely to be much more acceptable in the marketplace....the pursuit of concepts other than the tokamak....reorientation away from tokamaks toward more promising, smaller, advanced fuel concepts is in order." The credibility of this 'more promising, smaller' concept is, of course, a big unknown, to the discussion of which we will return. As for the panacea of neutron-free (or 'low neutron') fusion, there is a problem, which is widely known. Any other fuel would have a much lower reaction rate than D-T, which would require either operation at much higher pressure in order to achieve a comparable power density or a much larger plasma volume in order to achieve the same power output (technical annex 3).

Dr. Chauncy Starr is retired after a distinguished career in the nuclear fission field, capped by his presidency of the Electric Power Research Institute. He raises three objections to the arguments put forward by myself and others justifying federal support of fusion. "(1) The fuel supply is not 'virtually unlimited' because the availability of lithium, which is essential to the D-T fuel cycle, is similar to the availability of uranium--ample now, but finite." I am sure that Dr. Starr would agree that exploration for uranium has been much more vigorous than exploration for lithium, so the ratio of actual resources to known reserves must be much greater for lithium than for uranium. Furthermore, with the D-D fusion cycle, the fact that one in every 10,000 water molecules has a D really does justify 'virtually unlimited'.

Dr. Starr's second objection is "(2) The contention that the tokamak concept might eventually compete with advanced nuclear fission and fossil plants is wishful thinking that ignores the reality of the tokamak's complexity and size.....Today's estimate by fusion enthusiasts of the capital cost of the ARIES tokamak plant is at least three times that of a nuclear fission plant...." The important quantity is the cost of electricity and, as discussed above, the estimated[1] coe from a fusion reactor is projected to be 50% greater than the projected coe of an advanced light-water fission reactor in the middle of the next century.

His third point is "(3) ...the environmental benignity of fusion is a matter of degree, only slightly better than fission...." With the advanced structural materials that are under development[5] there is a strong possibility that materials in decommissioned fusion reactors could be disposed of as low-level-waste[2,3] or recycled[4], which is more than a matter of degree. There is considerable experience from the operation of tritium production reactors which can be drawn upon to design for environmental benignity of fusion reactors with respect to tritium release.

Dr. Robert Bussard was Assistant Director for Technology of the DOE Office of Fusion Energy, under Hirsch, in the early 1970s. He then formed a company (International Nuclear Energy Systems) to build a small tokamak that would ignite (be self-sustained energetically). However, he was unable to convince a series of DOE technical review panels that his designs could withstand the large electromagnetic forces which would be produced. He subsequently found support for his company for a year or so from the publisher of *Penthouse* and for another couple of years from a Saudi sheik. He currently is founder and Chief Scientist of Energy/Matter Conversion Corp.

Dr. Bussard believes that "...a national fusion program can be saved only if the current budget is reduced to zero as swiftly as possible. Then the program can be restarted with wholly new directions (and new management at DOE headquarters and the DOE labs) toward concepts that really do offer small, quick, clean, and cheap fusion power systems---if they work....If no such concepts can be identified within the DOE framework, there should be no DOE program in fusion. Rather, the national effort should solicit and support such concepts directly in private industry...." As mentioned previously, the big question, to which we will return, is in the credibility of such "small, clean and cheap" concepts.

Dr. Parkins a retired Director of Research and Technology for the Energy Systems Group of Rockwell who spent his career in the fission reactor field, repeats the arguments he made in his *Physics Today* letter, which are discussed in a separate Commentary.

James Adams, who is with the Safe Energy Communication Council, a lobby for renewable energy interests, argues that "the excessive funding for tokamak-based fusion is disproportionately high in comparison to the numerous and diverse renewable sources available and creates competition between the two programs for scarce federal dollars...." His proposed solution is that "DOE should phase out its tokamak reactors and fund a modest alternative program oriented toward basic science research....the United States and its international partners should increase their commitment to sustainable energy resources...."

Thus, among these critics of the magnetic fusion program, there is a certain consensus that the tokamak should be dropped in favor of pursuing alternate concepts, although this recommendation seems to be motivated more by a desire to redirect the bulk of the fusion budget into other coffers than from a realistic assessment of the prospects for finding a concept with more promise as a fusion reactor. Developing a better confinement concept is not a new idea. A large number of alternate magnetic confinement concepts have been investigated over the 50 years or so of magnetic fusion research. Most fell by the wayside because the performance envisioned by their proponents could not be realized in the laboratory; they simply were not as successful nor did they seem as promising at the time as the tokamak. Regrettably, in recent years, in an effort to maintain progress in the tokamak program in the face of a declining fusion budget, experimental research on concepts of varying degrees of promise was terminated or curtailed in the United States.

Research on alternates (technical annex 4) has continued apace in Europe and Japan, where the fusion budgets have been growing. Achievement of higher pressures than have yet been achieved in conventional tokamaks in a small spherical tokamak at an early stage of development has caused some excitement, as has the demonstration of confinement in disruption-free, steady-state stellarators similar to that achieved earlier in tokamaks at a similar stage of development, and recent advances in the US in suppressing turbulent transport in a reversed field pinch is leading to a reappraisal of the low field class of confinement concepts.

However, all of these alternate confinement concepts are at a much earlier stage of develepment than is the tokamak. The most commonly used measure of confinement performance is the nT_ triple product. The improvements in confinement (nT_) from present experiments to the requirements for a reactor are about 20 for the tokamak, about 1,000 for the stellarator, and about 100,000 or greater for the spherical torus, the mirror, the reversed field pinch, and the field reversed configuration[6].

Each of the ‘alternate’ confinement concepts presently under consideration is considered by its proponents to have the potential to lead to a reactor that is better than the tokamak in some particular aspect. A series of conceptual reactor design studies has been completed recently for commercial reactors based on the tokamak[7], the stellarator[8], the spherical torus[9] and the reversed field pinch[10] (technical annex 4). In each case, the extrapolation in physics parameters from present experience to the reactor regime was based upon the considered judgment of the expert proponents of the respective systems, but all designs satisfied comparable engineering requirements, for the most part, and their costs were estimated on a comparable basis. These reactor design studies conclude that the projected cost of electricity are about the same for tokamak, stellerator and reversed-field-pinch reactors and are about 20% greater for spherical torus reactors.

In summary: 1) there have been interesting recent advances in the development of alternate confinement concepts; but 2) the required confinement improvement from present experience to reactor requirements, hence the required development time, is enormously greater for any other confinement concept than for the tokamak; and 3) based on the substantial fusion reactor design studies to date, there is no other concept which presently projects to a more economical fusion reactor than the tokamak; furthermore 4) there have been exciting results recently from tokamak research--the so-called ‘advanced tokamak’ plasma performance with improved pressure and energy confinement (technical annex 2) projects to 50% reduction in the cost of electricity from commercial tokamak reactors, relative to the previous projections based on ‘conventional’ tokamak plasma performance. These facts simply do not support a rational decision to redirect the magnetic fusion program from an emphasis on tokamaks to an emphasis on alternate confinement concepts, but rather argue for continued emphasis on the tokamak in order to move the fusion program forward and exploit the capability of the tokamak for investigation of new realms of plasma science, combined with an exploration of alternate concepts at a level commensurate with actual achievement and improved reactor prospects in order to identify opportunities for future improvements.

Among those people within the magnetic fusion program who have thought broadly about the matter, there is a strong degree of international consensus on the major R&D issues that should be addressed in the program (technical annex 5). Differences of opinion among people within the program are primarily with regard to priorities in a severely constrained budget situation, reflecting differences in experience, perspective and institutional and/or personal self-interest.

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VI. COMMENTARY on "Turbulence May Sink Titanic Reactor" by James Glanz in the December 6, 1996 issue of *Science.*

A commonplace disagreement between proponents for particular models--for energy transport in a tokamak plasma and for extrapolating experimental confinement data--and the majority of the scientists actively working in this area became sensationalized into a broad indictment of the possibility of achieving energy self-sustainment, or 'ignition', in ITER, and by implication in other large tokamaks. Nature is complex, and fusion plasmas are no exception. Theoretical predictive models of plasma transport are inherently limited in the number of simultaneously interacting physical phenomena which can be treated and in the completeness of the representation of these phenomena; simplifications, guided by the physical judgement of the modelers, are inevitably made in order to obtain a tractable calculation. Likewise, the proper extrapolation of experimental results depends upon the proper understanding of the dominant underlying phenomena, which is imperfect.

Researchers at the University of Texas and at Princeton had by 1997 developed a very sophisticated model (IFS-PPPL model) of the turbulence-dominated transport in tokamaks which yielded more pessimistic predictions for the energy self-sustainment of ITER than were made with transport models having more simplifications. To complicate the matter, the predictions of the more simplified models agreed somewhat better with existing experimental data than did the IFS-PPPL model. Coincidentally, some of the same researchers became concerned that scaling laws used to extrapolate experimental confinement data for ITER performance projections were too optimistic. While two of the scientists who were principal developers of the IFS-PPPL model were certain that they were right and that ITER was doomed to failure, the majority of the scientists who were involved in the world-wide evaluation of transport and confinement predictions for ITER did not share this opinion. The *Science* article was built around the minority view, failed to convey that the issues had been under discussion for over a year by an international working group of experts on the subject, and had a sensational title.

However, the *Science* article had the positive effect of galvanizing an effort within the US fusion community to resolve the issue. Some seventeen US plasma transport theory and computation experts, including one of the developers of the IFS-PPPL model, were organized into a working group to compare the physics bases and the reliability of tokamak transport models which are widely used for predicting the performance of ITER[1]. The simplifications that had been made in the IFS-PPPL model, as well as those made in other models, were identified and evaluated. A so-called 'gyro-kinetic' model, with even fewer simplifications, was found also to lead to more optimistic predictions of energy self-sustainment in ITER than did the IFS-PPPL model. Other transport models, with more simplifications than in the IFS-PPPL model, were found to better match the experimental data from a DIII-D 'ITER-like' shot. It was found that some of the simplifications made in the IFS-PPPL model perhaps should be revised somewhat, resulting both in better agreement with experiment and a more favorable prediction for the energy self-sustainment of ITER (and other large tokamaks) more in line with the predictions of the several other extant models. A follow-up article in the November, 28, 1997 issue of *Science* acknowledged that maybe things were not as bad as they had been portrayed in the earlier article.

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VII. COMMENTARY on “Complexity and Availability for Fusion Power Plants”, *J. Fusion Energy,* 16, 1997 and “The Role of Inertial Fusion Energy in the Energy Marketplace of the 21st Century and Beyond”, *Nucl. Instr. Meth. A, 415* 44, 1998 by L. J. Perkins.

Dr. L. J. Perkins is a physicist at LLNL who worked in magnetic fusion research, primarily in conceptual reactor systems analysis, for about 20 years before his recent transfer into the inertial fusion program. His principal criticism of mainline magnetic fusion concepts is that “…fusion power core has about an order of magnitude greater complexity than an equivalent fission heat source”---which he supports by a comparison of the number of welds, pipe bends, penetrations, etc. between a magnetic fusion reactor blanket conceptual design and a light-water reactor---from which he concludes that a magnetic fusion reactor will be inherently unreliable. While I suspect that a comparison of a dirigible and a modern jet airliner following the same logic would reach a similar conclusion about the unreliability of jet airliners, no one familiar with the field would deny Dr. Perkins’ point that magnetic fusion reactors will be complex, but this does not necessarily mean that they will be unreliable. Disciplined engineering has made a success of many complex systems (e.g. the modern jet airliner), and there is no reason to suspend belief in the efficacy of disciplined engineering when it comes to fusion reactors.

The second point made by Dr. Perkins is that inertial fusion reactors will be intrinsically more reliable than magnetic fusion reactors because of “…lifetime fusion chambers to be designed with renewable liquid coolants instead of solid, vacuum-tight walls that must be renewed on a frequent basis due to radiation damage. The relative advantage that this feature affords the core (i.e. the chamber) of an ICF power plant can not be overstated relative to the availability issue.” This is an attempt to make a virtue of a necessity---inertial fusion chambers would be destroyed in a relatively few shots (explosions) unless some scheme such as the flowing liquid metal wall that he mentions can be found to protect them. The primary ‘vacuum-tight walls’ in a magnetic fusion reactor, as presently conceptualized, would be designed to last the lifetime of the plant by placing them behind a (fixed) blanket for protection from the high-energy neutrons. It is the first-wall facing the plasma, not the primary vacuum chamber wall, that would need to be renewed several times, if made of presently available materials, but infrequently if made of advanced structural materials presently under development[1]. The flowing liquid metal wall scheme cited by Dr. Perkins could also be used in magnetic fusion to achieve reactors with lifetime components, if it works, but its success is not prerequisite to the feasibility of a magnetic fusion reactor.

However, the primary issues in contrasting the prospects of inertial and magnetic confinement reactors are the target physics and laser efficiency extrapolations required between present experience and a reactor. For the important nT_ confinement parameter (see technical annex 4), the required extrapolation is about 20 for the tokamak, about 1000 for the stellarator, about 1,000 for direct drive inertial confinement, and about 100,000 for indirect drive inertial confinement[2]. For magnetic fusion, the efficiency of the driver—the heating and current drive systems--is not critical to projected reactor performance and is already at or close to the level that would be acceptable in a reactor. On the other hand, the projected performance of inertial fusion reactors, which are projected to have a large recirculating power, is very sensitive to laser driver efficiency, which must be improved substantially from the present level in order to achieve a net power producing reactor.

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Technical Annex 1 First Wall Engineering Heat Transfer Limits for D-T Fusion Reactors

The engineering heat transfer requirement on the first wall area (A_{fw}) for a D-T fusion reactor is related to the heat flux (q”$_{max}$) limit and the thermal power output (P_{th}) as A_{fw} = 0.2f_{pk}(1-f_{div})P_{th}/(_q”$_{max}$), where _ _ 1.5 accounts for the enhancement of the fusion energy due to exoergic neutron-nuclear reactions in the blanket, f_{pk} _ 1.5 is a heat flux peaking factor over the first wall, and f_{div} _ 0.5 is the fraction of the plasma power exhaust which is removed to the divertor (or elsewhere). For a toroidal configuration, such as the tokamak, the area of the first wall is related to the minor radius of the first wall in the horizontal midplane, r_{fw}, by A_{fw} = 2_R2_r_{fw}(1/2(1+_^{2}))$^{1/2}$ = (2_r_{fw})^{2}A(1/2(1+_^{2}))$^{1/2}$, where _ _ 2 is the plasma elongation (ratio of the vertical to the horizontal plasma dimensions), R is the major radius of the torus, and A = R/r_{fw} _ 4 is the aspect ratio. Combining these two relations, the engineering heat transfer requirement on first wall radius in a D-T tokamak reactor is r_{fw} _ [0.2f_{pk}(1-f_{div})P_{th}/((2_)^{2}A(1/2(1+_^{2}))$^{1/2}$ _ q”$_{max}$]$^{1/2}$. Thus, the minimum first wall radius of a D-T reactor that is allowed by heat transfer requirements depends on (P_{th}/q”$_{max}$)$^{1/2}$.

Maximum allowable heat fluxes (limited by stresses and maximum allowable temperatures) were recently calculated for representative water-cooled[1] and lithium- and helium-cooled[2] fusion reactor first wall designs with several different structural materials. The maximum allowable heat fluxes with water cooling were calculated[1] to be q”$_{max}$ _ 0.5 MW/m^2 for austenitic stainless steel, _ 1.1 MW/m^2 for ferritic steel and ≈ 2.1 MW/m^2 for vanadium alloy. The maximum allowable heat fluxes calculated[2] for a vanadium alloy cooled by lithium was 5.3 MW/m^2 and was 2.7 MW/m^2 for both ferritic steel and silicon carbide cooled by helium. For a vanadium alloy cooled by liquid lithium, conceptual designs have been developed[3,4] with first wall heat fluxes of 3.0-4.5 MW/m^2, although more recent fundamental analysis[5] indicates that a practical heat flux limit for vanadium alloys may be about 3 MW/m^2. Even higher limits would be obtained with advanced refractory alloys of molybdenum or niobium cooled by liquid lithium. These calculations take into account a proportional amount of neutron heating in the first wall.

We are now in a position to make some quantitative estimates of how engineering heat transfer limits will inherently constrain the minimum size of a D-T reactor. Using the representative parameters (f_{pk} = 1.5, f_{div} = 0.5, _ = 1.5, _ = 2, A = 4), the minimum first wall horizontal radius for a tokamak reactor is r_{fw} _ [0.0004P_{th}/q”$_{max}$]$^{1/2}$. This expression is evaluated for a range of thermal power outputs and possible first wall heat flux limits in the following table. The first three columns roughly correspond to the limiting heat fluxes for stainless steel, ferritic steel and vanadium alloy, respectively, cooled with water[1], and the fourth column probably corresponds to an upper limit heat flux that could be accommodated by a vanadium alloy first wall cooled by lithium[5]. As a point of reference, the electrical power output from a fusion reactor would be about 35-45% of the thermal power output, depending on the coolant and operating conditions.

Inherent Engineering Heat Transfer Limits on First Wall
Horizontal Radius in a D-T Tokamak Reactor (meters)

q”$_{max}$ (MW/m^2)	0.5	1.0	2.0	3.0	5.5
P_{th} = 1000 MW	0.89	0.63	0.45	0.37	0.27
P_{th} = 2000 MW	1.26	0.89	0.63	0.52	0.38
P_{th} = 3000 MW	1.55	1.10	0.77	0.63	0.47

It is unlikely that the minimum size of a tokamak fusion reactor will be inherently limited by the ability to transfer heat across the first wall. However, there is a significant incentive to develop advanced structural materials and lithium coolant which would result in the type of constraints indicated in the last column, rather than the present constraints shown in the first column (note that a copper first wall can tolerate higher heat fluxes than 0.5 MW/m^2, but copper is not considered reactor-relevant for other reasons).

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Technical Annex 2 Advanced Tokamak Modes of Operation

There are several plasma physics constraints on the minimum size of a tokamak reactor. Perhaps the most fundamental is the power balance constraint—the self-heating of the plasma by the fusion alpha particles plus any external heating must be greater than or equal to the cooling of the plasma by radiation and transport losses. As discussed in annex 4, this leads to a requirement nT_ _ R(T) _ 100 ($10^{20}m^{-3}$s-keV). The pressure, p = nT, is limited by plasma instability constraints[1] to nT _ _$_{N}$(IB/2_$_{0}$a), where I, B and a are the magnetic field, plasma current and minor plasma radius, _$_{0}$ = constant, and _$_{N}$ is a parameter which depends on the internal configuration of the plasma. The quantity _ is the energy confinement time, which depends in a complicated and not yet fully understood manner on many parameters and is commonly represented by _ = H$_{89}$_$_{ITER89P}$, where _$_{ITER89P}$ is an empirical fit[1] to the experimental tokamak confinement data as of 1989 and H$_{89}$ is an enhancement factor. (There are more recent fits to the data, but this ITER89P fit is widely used and serves the purpose of this discussion.) Thus, H$_{89}$, _$_{N}$ and the product H$_{89}$_$_{N}$ are important parameters that characterize plasma confinement and hence required plasma size. Since the plasma power scales as P _ _$_{N}^{2}$Vol , _$_{N}$ also affects the minimum plasma volume needed to produce a specified power P.

The ‘conventional’ tokamak data base circa 1990 could be characterized by _$_{N}$ _ 2.5, H$_{89}$ _ 2 and H_$_{N}$ _ 5. Perhaps the most exciting development in plasma physics over the past decade has been the evolution of ‘advanced tokamak (AT) modes’ of plasma that could considerably increase these plasma performance parameters[2]. An ‘intermediate’ level of AT performance (_$_{N}$ _ 4, H$_{89}$ _ 3 and H_$_{N}$ _ 12) had been achieved transiently by 1995[3], and theoretical extrapolations indicated the possibility of achieving a ‘superior’ level of AT performance characterized by (_$_{N}$ _ 6, H$_{89}$ _ 4 and H_$_{N}$ _ 24) in devices with greater capability for controlling the internal plasma configuration than is present in current experiments. As of the end of 1998, the best AT results had been achieved in DIII-D[4]---H_$_{N}$ _ 15 for _ t _ _, and H_$_{N}$ _ 10 for _ t _ 10_.

Systems studies have been performed to project the effect of advanced tokamak modes on the size of tokamak fusion demonstration reactors and on the cost of electricity from commercial tokamak reactors. The major radii of 500 MWe tokamak demonstration reactors sized with ‘superior AT’, ‘intermediate AT’ and ‘conventional’ tokamak plasma performance projections are 5.50, 6.25 and 8.0 m, respectively[5]. The ‘superior’ and ‘intermediate’ levels of AT performance project 48% and 31%, respectively, reductions in cost of electricity for a 1000 MWe commercial tokamak reactor, relative to a reactor designed on ‘conventional’ tokamak performance assumptions[6].

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Technical Annex 3 ‘Advanced’ Fusion Fuels

The problems of radioactivity and materials damage caused by the high-energy neutrons from the D-T fusion reaction have led many observers to call for the development of so-called ‘advanced’ fusion fuels which would create a fusion reaction without a neutron product, or at least with relatively few neutrons compared to D-T. The two ‘advanced’ fuels generally considered most important are ^{2}D+^{3}He and ^{1}H+^{11}B. There are two notable problems with such ‘neutron-free’, or ‘reduced-neutron’ fusion.

The first problem, which is widely recognized, is that the rate at which a given number of fuel particles will react to fuse is a factor of 10-100 times greater for D-T than for any other possible fusion fuel at the ‘lower’ temperatures in the thermonuclear range[1] (about 50 million degrees centigrade). The largest fusion rate occurs for D-T, and the maximum fusion rate occurs at a lower temperature for D-T than for any other fusion fuel. The fusion rate that is required for a self-sustaining D-T reactor is about 10^{-22} m^{3} /s, which requires temperatures T _ 10 keV, for D-T. Fusion rates required for self-sustaining reactors with other fuels will be higher than for D-T because of the higher radiation at higher temperatures, and this may in fact make it impossible[2,3] to achieve self-sustaining (ignited) reactors with fuels such as ^{1}H+^{11}B and ^{3}He+^{3}He. The minimum required value of nT_ is about 25-50 times larger[2,3] for ^{2}D+^{3}He than for ^{2}D+^{3}T, and even larger for ^{1}H+^{11}B and other ‘advanced’ fuels, indicating that confinement must be at least about 25-50 times better for ‘advanced’ fuels than for D-T. Since confinement depends on, among other things, the plasma size, reactors with ‘advanced’ fuels would have to be significantly larger than D-T reactors designed to the same technological limits in order to achieve this greater confinement.

In order to achieve the same fusion power with another fuel as with D-T, the product n^{2}<_v>QVol must be the same (where n = N/Vol is the number of fuel particles per unit volume, <_v> is the fusion reactivity, Vol is the volume, and Q is the amount of energy released in the fusion reaction). As may be seen in the accompanying table, the value of Q is significantly smaller in most other fusion reactions than in the D-T reaction. The maximum pressure (p = nT) in the plasma is limited by plasma stability constraints (technical annex 2) which are probably more-or-less independent of fuel type for a given magnetic confinement concept. For the same plasma pressure and magnetic field, the peak plasma power density (n^{2}<_v>Q) is about 75-100 times smaller[2,3] for ^{2}D+^{3}He than for ^{2}D+^{3}T, and even smaller for ^{1}H+^{11}B and other ‘advanced’ fuels. This peak plasma power density occurs at 17 keV for ^{2}D-^{3}T, at 58 kev for ^{2}D+^{3}He, and at higher temperatures for other fuels. This line of reasoning leads inexorably to the conclusion that the plasma volume of a fusion reactor based on a given magnetic confinement concept and given physics limits must be at least 50-100 times larger with other fuels than with D-T, if the physics limits determine the minimum size. This disadvantage is mitigated to some degree by the fact that the 0.5-1.0 m of neutron shield needed with D-T reactors can be reduced substantially with neutron-free ‘advanced’ fuels[3].

Since the required confinement nT_ = p_ is at least 25-50 times larger and the achievable power density n^{2}<_v> = p^{2}<_v>/T^{2} is at least 50-100 times smaller for other fuels than for D-T, because of the much smaller <_v> for ‘advanced’ fuels, most proposals to use fuels other than D-T are linked to alternate magnetic confinement concepts (technical annex 4) which are projected to achieve substantially higher plasma pressure, p, for comparable magnetic field strengths than are currently projected for tokamaks (or stellarators)—e.g. a factor of 5-10 increase in pressure can increase nT_ = p_ by the same amount and increase n^{2}<_v> = p^{2}<_v>/T^{2} by 25-100. The same gains in confinement and power density would also be achieved for D-T, of course. However, at some point the size of D-T reactors capable of operating at increased pressures would be determined by engineering limitations (e.g. surface heat flux, magnet volume, shield volume) rather than the physics confinement and power density constraints discussed above. Beyond this point the physics-constrained size disadvantage of reactors operating with ‘advanced’ fuels relative to the engineering-constrained size of reactors operating with D-T would not be as large as when the size of D-T and ‘advanced’ fuel reactors are both constrained by the foregoing physics considerations (e.g. as for the tokamak or stellarator). These mitigating considerations notwithstanding, the substantial fusion reactivity, <_v>, advantage of D-T with respect to any other fusion fuel should enable the design of a more efficient fusion reactor based on any given magnetic confinement concept, although there is one systems scoping study comparison of ‘optimized’ D-T and D-^{3}He field-reversed configuration fusion plants which comes to a different conclusion[4]. Moreover, the alternate confinement concepts with higher projected pressures which are required for plausible ‘advanced’ fuel reactor concepts are at a very early stage of development (technical annex 4).

The second problem, which is not widely appreciated, is that without the neutrons the heat which must be transferred through the first wall may increase by a factor of five. With D-T fusion, 80% of the fusion energy passes through the first wall in the form of energetic neutrons (only a small amount is actually deposited in the wall) to be distributed as heat over the volume of the material located behind the first wall, and only 20% of the fusion energy, at most, is actually transferred as heat through the first wall. With any neutron-free fuel, up to 100% of the fusion power may need to be transferred as heat through the first wall, increasing by perhaps a factor of five the engineering heat transfer requirement for the first wall. This difficulty could in principle be mitigated to some degree in confinement configurations in which the energy of charged particles escaping the confined plasma can be transferred magnetically out of the confinement volume and distributed over a broader surface area, such as the magnetic mirror, the reversed-field pinch, and the spheromak[3].

FUSION REACTIONS[1]

REACTION	Q (MeV)
Some neutron producing fusion reactions	
${}^{2}D + {}^{3}T \rightarrow {}^{4}He + n$	17.6
${}^{2}D + {}^{2}D \rightarrow {}^{3}He + n$ $\rightarrow {}^{3}T + {}^{1}H$	3.3 4.0
${}^{3}T + {}^{3}T \rightarrow {}^{4}He + 2n$	11.3
${}^{3}T + {}^{3}He \rightarrow {}^{2}D + {}^{4}He$ $\rightarrow {}^{5}Li + n \rightarrow {}^{4}He + 2\,{}^{1}H$ $\rightarrow {}^{5}He + {}^{1}H \rightarrow {}^{4}He + 2n$	14.3 12.1 12.1
Some neutron free fusion reactions	
${}^{1}H + {}^{6}Li \rightarrow {}^{3}He + {}^{4}He$	4.0
${}^{1}H + {}^{9}Be \rightarrow {}^{4}He + {}^{6}Li$ $\rightarrow {}^{2}D + 2\,{}^{4}He$	2.2 0.7
${}^{1}H + {}^{11}B \rightarrow 3\,{}^{4}He$	8.7
${}^{2}D + {}^{3}He \rightarrow {}^{4}He + {}^{1}H$*	18.3
${}^{3}He + {}^{3}He \rightarrow {}^{4}He + 2\,{}^{1}H$	12.9

* neutrons produced in D+D reaction

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Technical Annex 4 Comparison of Magnetic Confinement Concepts

A large number of magnetic confinement concepts have been investigated over the 50 years or so of magnetic fusion research. From the time in the 1960s when the Russians reported their early success with the closed, toroidal confinement scheme known as 'tokamak', it became the front-runner, and the gap between tokamak performance and that of all other concepts has steadily widened over this period. Whether this is because the tokamak is inherently better or because a very large fraction of the world's experimental fusion budget has long been devoted to tokamaks remains today an open question. Most other concepts fell by the wayside because the performance envisioned by their proponents could not be realized in the laboratory; they simply were not as successful nor did they seem as promising at the time as the tokamak. Admittedly, in recent years, in an effort to maintain progress in the tokamak program in the face of a declining US fusion budget, experimental research on concepts of varying degrees of promise was terminated or curtailed.

However, research on alternates has continued apace in Europe or Japan, where the fusion budgets have been growing. Achievement of higher ratios of plasma to magnetic pressures than have yet been achieved in conventional tokamaks in a small spherical tokamak at an early stage of development has caused some excitement, although the actual pressure achieved was only about 1% of the values that have been achieved in tokamaks. The achievement of confinement values in disruption-free, steady-state stellarators similar to that achieved earlier in tokamaks at a similar stage of development is also a promising result. Recent advances in the US in suppressing turbulent transport in a reversed field pinch is leading to a reappraisal of the low field class of confinement concepts.

However, all of these alternate confinement concepts are at a much earlier stage of develepment than is the tokamak. The most commonly used measure of confinement performance is the nT_ triple product which comes about from a consideration of the fact that the heating power must be greater than or equal to the power loss by transport and radiation in order for the plasma not to cool. This may be written P_{heat} _ P_{tran} + P_{rad} . If the heating power is provided mostly by the alpha particles from fusion, then P_{heat} = 0.25n^2<_v>U_ (there are 0.5n each D and T nuclei per unit volume, <_v> is the fusion reactivity, and U_ is the 20% of the energy of the fusion event which remains in the plasma). The transport power loss is P_{tran} = 3nT/_, where T is temperature, and the radiation power loss is n^2fL_z, where f is a factor accounting for the concentration of impurities and L_z is the radiation function for the particular plasma composition. These forms can be used in the above power balance equation to solve for nT_ _ R(T), where R(T) _ 100 in units of (10^{20}m^{-3}s-keV) is a known function of temperature only. The nT_ values achieved in the various confinement concepts are contrasted with this required value in Table 1.

TABLE 1: FUSION CONFINEMENT TRIPLE-PRODUCT (n-T-_) FIGURE OF MERIT (10^{20}m^{-3}s-keV) FOR DIFFERENT MAGNETIC CONFINEMENT CONCEPTS[1] (S-T = spherical torus, RFP = reversed field pinch, FRC = field reversed configuration)

Reactor Requirement	Tokamak Today	Tokamak 1970	Stellarator Today	S-T Today	RFP Today	FRC Today	Mirror Today
100	5	0.002	0.1	0.0005	0.0002	0.001	0.0005

The value of nT_ for a tokamak has improved by _5,000 times since 1970 and is within a factor of _20 of the required reactor value. Relative to present experience, the stellarator must be improved by 1,000 times and the spherical torus, reversed field pinch, field reversed configuration and mirror must be improved by _100,000 times or more. Although we have the experience of tokamak development to draw upon, it is hard to envision the other magnetic confinement concepts shown in the table reaching the present level achieved by tokamaks in less than 15 to 30 years, based on the logistics of the development effort involved.

Each of the 'alternate' confinement concepts presently under consideration is considered by its proponents to have the potential to lead to a reactor that is betten than the tokamak in some particular aspect. The stellarator routinely operates in steady-state without the disruptions which are a major engineering problem in tokamaks. The spherical torus, the reversed field pinch and the field reversed configuration are stable at significantly higher ratios of the plasma pressure to the pressure of the confining magnetic field than is the tokamak, which provides the potential for a higher plasma power density than can be achieved in the tokamak.

A series of conceptual reactor design studies has been completed recently for commercial reactors based on the tokamak[2], the stellarator[3], the spherical torus[4]and the reversed field pinch[5] In each case, the extrapolation in physics parameters from present experience to the reactor regime was based upon the considered judgement of the expert proponents of the respective systems, but all designs satisfied comparable engineering requirements, for the most part, and their costs were estimated on a comparable basis[6]. The plasma volumes and estimated costs of the four designs are given in Table 2. Interestingly, the potential of higher power density was not realized in the spherical torus design because of engineering constraints. There is not a large difference in the estimated cost of electricity among the confinement concepts.

TABLE 2: SIZE AND COST OF 1000 MWe FUSION REACTORS BASED ON DIFFERENT MAGNETIC CONFINEMENT CONCEPTS FROM ARIES CONCEPTUAL DESIGN STUDIES

	Tokamak[2] (Rev. Shear)	Stellarator[3]	Spherical Torus[4]	Reversed Field Pinch[5]
Plasma Volume (m^3)	349	735	745	111
Capital Cost, $B (1992$)	4.2	4.3	5.3	
Cost Electricity, mill/kWh (1992$)	76	75	91	74

Predicted cost of electricity and achieved $nT\tau$ are perhaps imperfect and oversimplified measures of potential and performance, but they have the virtue of being very relevant, generic among concepts and readily understandable, thereby providing a useful perspective.

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Technical Annex 5 Scientific and Technological Issues of Magnetic Fusion R&D

Physics Issues

There is a worldwide scientific consensus that the next major scientific issue to be addressed in magnetic fusion is the physics of burning plasmas (i.e. plasmas in which most or all of the heating is by the fusion process) and that the only magnetic confinement concept which is sufficiently advanced to be able to investigate this issue within the next decade or two is the tokamak. A second major scientific issue is the long time sustainment of ‘advanced tokamak’ modes (technical annex 2) and the achievement of quasi-steady state operation. A third major scientific issue is one of physics integration--the simultaneous achievement of reactor-relevant burning plasma conditions, sustained advanced tokamak modes, quasi-steady-state operation, reactor-relevant plasma core and edge conditions, plasma power and particle exhaust, etc. within a single experiment. A fourth major issue which must be addressed prior to the demonstration of the feasibility of fusion power is a joint physics-technology issue---the integration of reactor-relevant physics conditions and reactor-relevant technology into a single facility and the accumulation of operating experience on that facility. A fifth major scientific issue is the development of an improved confinement concept with better reactor prospects than the tokamak.

In addition to these major issues, the continued resolution of several important tokamak physics issues (disruptions, divertors and edge physics, density limits, etc.) is necessary in the near term. However, these issues can be addressed in existing experiments and in the experiments dedicated to the first three issues enumerated above.

The ITER Agreement and the ITER collaborative project and its many positive reviews reflect the former international technical consensus that the world’s fusion programs were sufficiently advanced to address the first four of these major issues in a single facility and that this was the appropriate next step for fusion development. Support for ITER within the US scientific fusion community has decreased as recent decreases in the fusion budget have made simultaneous participation in ITER and pursuit of new initiatives in the US domestic program appear incompatible. However, support for ITER appears to remain strong among the other ITER partners, who are proceeding with the completion of the ITER R&D and with the development of a revised ITER-RC design that would include changes intended to reduce the cost by about 50% and to allow increased access to advanced tokamak modes of operation. The Japan Fusion Council decided in November, 1998 that no change in the Japanese fusion strategy was technically warranted in spite of the recent change in US fusion policy, confirmed that ITER remains the most important project in fusion research, and judged that the ITER EDA could be successfully completed even in the case of US withdrawal. The JAERI Council accepted the Fusion Council report, noting the importance of preparing for a concrete decision on construction.

Meanwhile, in the face of the de facto US withdrawal from ITER, the pre-ITER plan to address the first two of the above issues (burning plasma, steady-state--now including advanced tokamak) in separate, single-purpose experiments has been revived as part of the ‘modular’ strategy. The advantage of the modular strategy is that it addresses the first two issues in facilities each of which (and probably combined) will be less costly than ITER-RC. The disadvantage of the modular strategy is that it does not adequately address the third and fourth major issues (physics integration, physics and technology integration), with the consequence that a subsequent facility of the scale of ITER-RC will be required in addition—thus, the first four major issues will be addressed at greater overall cost and at a later date than in the ‘ITER-RC’ strategy.

The fifth major scientific issue is the identification and development of confinement concepts which offer better reactor prospects than the tokamak. Most scientists in the magnetic fusion program support such an effort on alternate concepts, commensurate with their demonstrated progress and perceived improvement in reactor prospects, but only a small minority would endorse this as a primary emphasis of the US fusion program. However, since the cost of a state-of-the-art experiment is much less for ‘alternate concepts’ than for the much more developed tokamak, an emphasis on ‘alternate concepts’ appeals to those who would redirect the fusion budget into other programs and to those who must find a balance among competing claims for limited resources.

Technology Issues

The first major technological issue for magnetic fusion is the development of the 'enabling' technology (e.g. magnets, heating and current-drive systems, fueling systems, plasma-facing components, vacuum systems, etc.) which enables the plasma operation required to achieve the above physics objectives. As a result of the ITER R&D projects[1], such technology is now relatively highly developed.

The second major technological issue is the development of the 'nuclear and tritium' technologies which are required to convert the fusion energy to sensible heat and remove it for electricity production, and to "breed" tritium and extract, process and store it. While fission reactor and tritium production technologies can be adapted to provide a basis for the development of fusion heat removal and tritium technologies, the demonstration of tritium self-sufficiency remains a major challenge. This technology, at least the 'nuclear' part, is at a relatively early stage of development[2,3].

The third major technological issue is the development of structural and other materials that are sufficiently resistant to radiation damage to enable long fusion reactor component lifetimes and that have neutron activation characteristics which would enable decommissioned fusion reactor materials to qualify as low-level-waste for shallow land burial or to be recycled. This technology is at an intermediate stage of development[4].

The fourth major technological issue is the development of remote assembly and maintenance technology for the complicated fusion reactor geometry in a radioactive environment. As a result of the ITER R&D projects[1], such technology is now at an intermediate stage of development.

The fifth, and perhaps most demanding, major technological issue is the achievement of reliability and high plant factor for an integrated fusion reactor system. This is at a very early stage.

International Collaboration Issues

It is the strategy of the US magnetic fusion program to carry out the burning plasma and energy development aspects of the program internationally. The ITER project was the mechanism for doing this, but now the US has de facto withdrawn from that collaboration. Some people in the US are hopeful that the US can initiate new collaborations with some or all of our former ITER partners on a copper-magnet burning plasma experiment. However, these partners have indicated repeatedly over the years that they are not interested in such a collaboration, for now seem to be intent on going forward with ITER-RC, and would be predisposed by their ITER experience to view any new collaboration with the US with some skepticism. It is not at all clear how the US can carry out the burning plasma and energy development aspects of the magnetic fusion program internationally, unless it can 'rejoin' the ITER collaboration.

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