

AEC Policy and Action Paper on Controlled Thermonuclear Research

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Published online: 23 March 2011

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Abstract This report was prepared in late 1965 at the request of the U.S. House-Senate Joint Committee on Atomic Energy (JCAE) to review its program on controlled thermonuclear research (CTR), now called just “fusion energy research.” It was transmitted to them on June 11, 1966 by then chairman of the U.S. Atomic Energy Commission (AEC) Glenn T. Seaborg. The AEC is the predecessor agency of the current U.S. Department of Energy (DOE). The report has previously only been available as AEC report TID-23277. This version of the report is complete except for the omission of lengthy discussions of the then current fusion research activities at various AEC sites. This paper includes, as does the original report, a copy of Seaborg’s June 11, 1966 transmittal letter and, as appendices, copies of a 1962 review of the fusion program by the AEC’s General Advisory Committee (GAC), a review of the Policy Paper by a special 1965 AEC committee, prior to its submittal to the JCAE, comments on the report by the GAC and the President’s Science Advisory Committee, and the charter for then newly established CTR Standing Committee.

Keywords Fusion · Fusion energy · Controlled thermonuclear research · U.S. Atomic Energy Commission

Origin and Purpose of this Document

During 1965, the Atomic Energy Commission undertook a major review of its research program on controlled thermonuclear reactions (CTR). In doing so, the Commission was motivated by the desire (1) to carry out an evaluation in depth of the present status and future prospects of its work in this field, and (2) to provide sound answers to penetrating questions raised by members both of the Joint Committee on Atomic Energy and of the House Appropriations Committee.

In connection with this review, the Joint Committee on Atomic Energy requested the Commission use its findings and conclusions as a basis for developing a Policy Paper on the controlled thermonuclear program, similar to that prepared on the high energy physics program. The present document is in specific response to this request.

Section II of this document provides a review of the scientific background for the CTR program, including the nature of the problem, the methods of attack, etc. The material of this section serves as an introduction to the technical discussions given later in the paper.

Section III gives a brief summary of the various reviews of the AEC’s fusion program by external committees. The latest of these reviews, carried out in 1965 under the chairmanship of Professor S. K. Allison (and after his death, under the chairmanship of Professor R. G. Herb) has been especially helpful to the Commission in evaluating its own program and in the preparation of the present document.

Section IV presents the evaluation by the Commission of the overall effort in the controlled thermonuclear field,

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including a comparison of the work in this country and abroad, together with considerations evolving from this comparison.

In conclusion, Section V states the proposed Commission action relating to the AEC effort in this field. This proposed action consists of two parts: (1) the establishment of policy designed to guide the development of the program, and (2) a set of specific steps which serve to implement this policy. The June 11, 1966 letter from AEC Chairman Glenn T. Seaborg transmitting the report to the Joint Committee on Atomic Energy of the U.S. Congress is provided at the end of Section V. “Appendix 1” provides the 1962 fusion review of the AEC’s General Advisory Committee. “Appendix 2” provides the December 1965 review of this report by a special committee set up by the AEC. “Appendix 3” provides comments on this report by the AEC’s General Advisory Committee and the Presidents Science Advisory Committee. “Appendix 4” provides the charter for the new CTR Standing Committee.

Scientific Background

The objective of controlled fusion research is to provide mankind with a new source of energy. Despite the applied nature of its goal, at the present time progress toward this goal depends critically upon the performance of fundamental studies. In this regard fusion research resembles other areas of basic physics research and, in common with these fields, it is impossible to predict in detail the outcome of the research.

The following material is intended to provide a review of the salient scientific features of the controlled fusion problem in as descriptive a manner as possible. In it will also be found concepts and definitions of terms useful in understanding the fusion research programs.

The Nature of the Problem

At first sight, the requirements for achieving useful power from sustained thermonuclear reactions stagger the imagination. They are: (1) to heat a dilute gas of fusion fuel to temperatures of hundreds of millions of degrees, (2) to contain it, free from any contact with material walls or from contamination by impurities, long enough for an appreciable fraction of the fuel to react, and finally, (3) to extract the energy thus released and convert it to a useful form. Not only must this scientific legerdemain be performed with finesse but, to be useful, it must obviously be accomplished with an expenditure of energy substantially smaller than that released by the fusion reactions themselves. Eventually, the whole process must be made compatible with economic requirements, both as to cost and size.

Attacking the Problem

To see how the fusion problem can be attacked, consider the nature of the fusion process itself. Nuclear fusion reactions are “rearrangement reactions” that occur between nuclei of energy-rich light isotopes, such as deuterium or tritium (heavy isotopes of hydrogen). For such reactions to occur two nuclei must “collide”, that is, they must approach each other within a distance about equal to their own diameter, so that rearrangement, forming stabler nuclei, can occur. The resulting nuclei are less massive than the original pair and, as predicted by Einstein’s mass-energy relationship, this difference in mass is released as kinetic energy, i.e., the new pair is expelled from the scene of the reaction with increased energy of motion. The most striking features of fusion reactions as a source of energy are the ready and universal availability, the low cost, and the essential inexhaustibility (billions of years’ supply) of the primary fuel, deuterium. Deuterium occurs naturally, mixed with ordinary hydrogen, in a concentration of about 1:6,000 and is readily and cheaply separated from water.

The nuclear fusion reactions involving deuterium alone (D-D or “deuteron-deuteron” reactions) are not the only ones of interest. Others include the D-T (deuterium–tritium) and the D-Helium-3 reaction (Helium-3 is a light isotope of helium). In fact, in speculating about fuels that might be used for fusion power, D-T is a prime candidate. It has the great advantage that both the probability of reaction and the energy release per reaction are much larger for D-T than the corresponding numbers for the D-D reaction. Since tritium does not occur in nature, any system using the D-T reaction would have to include a regenerating blanket in which the neutron released from the D-T reaction would be captured (probably in lithium, also abundant and cheap) to produce more tritium. Studies have shown that this process should be feasible, both technically and economically.

On the other hand, if the D-D reaction could ever be utilized, it would have several advantages. D-D reactions result in reaction products (either tritium or helium-3) that are themselves fusion combustible. The end products are ordinary helium and hydrogen. Indeed this is a characteristic feature of fusion reactions: the end-products are non-toxic and non-radioactive. Fusion reactors would have to be shielded because of neutrons released, but no pollution, waste disposal, or reactor runaway hazard would result from their use.

Nuclei, being positively charged, repel each other so that fusion reactions only occur when nuclei approach each other with substantial relative energy. This required energy is roughly one percent of the energy released in the fusion reaction itself, giving the available “payoff” ratio for fusion reactions. The situation differs greatly from that

encountered in the fission of uranium by neutrons. A neutron, being uncharged, can readily penetrate a uranium nucleus and cause fission, even when its energy of motion is microscopic compared to that released.

In nature, that is in the sun and the stars, nuclear fusion reactions are said to be of “thermonuclear” origin, meaning that they occur as a result of the extremely high temperatures existing in the stellar core. High temperature implies rapid motion of the nuclei of which the star is composed. This random motion promotes collisions and fusion reactions. “Confinement” against loss of the thermonuclear fuel before it can react is provided by the intense gravitational field of the star.

On earth one cannot hope completely to duplicate a star’s prescription for sustained fusion reactions. Another approach must be sought. Fortunately, the very fact that fusion requires high temperatures opens up a novel possibility: All matter raised to a sufficiently high temperature will be broken down to the plasma state—a gas composed of charged particles—as a result of the violent collisions that “high temperature” implies. Though, as a whole, a plasma is electrically neutral, i.e., equal numbers of negative and positive charges, its constituent particles can be controlled by an electromagnetic field. The guiding of high energy particles by the magnetic fields of a particle accelerator is an example of the control of charged particles. From this general concept evolved the idea of magnetic confinement: the hot plasma is to be contained in space through control of its individual particles by means of a magnetic field, shaped so as to form a kind of “magnetic bottle.” Though generically related to the idea of the guide field of a particle accelerator, the problem of particle control in fusion appears at a different level. First, even though the particle energies are much lower (tens of thousands of electron volts for fusion as opposed to millions or even billions of electron volts in particle accelerators), both the total number and the number per unit volume of particles that must be controlled is vastly greater. Second, whereas the particles in an accelerator move head-to-tail along prescribed orbits, neither appreciably influencing each other’s motion nor disturbing the guiding field, the particles in a fusion reactor must move within the chamber in random directions, to promote the probability of mutual collisions. Also, because the particle density would have to be much higher in a fusion reactor than it is in an accelerator, the plasma particles themselves might, through cooperative motions, generate electric or magnetic fields that could affect the particles’ own confinement.

The last point mentioned, cooperative effects, hints at the salient question for all of fusion research today: How can a magnetic bottle be made, whose effectiveness is not spoiled by the very presence of the plasma, i.e., how can a

plasma be magnetically confined and yet not break into spontaneous unstable motions that spoil the confinement?

The Concept of Magnetic Confinement

If plasma instability can be adequately controlled, theory indicates that magnetic bottles should be ideally suited to solve the containment problem. To visualize the idea of magnetic confinement, first consider what would happen to a thermonuclear plasma were it suddenly established within a long cylindrical tube, otherwise evacuated. A snapshot of such a plasma would reveal that its particles are all moving in straight paths, but in random directions and at very high speeds (over 1,000 miles per second for the deuterium nuclei, and much faster than this for the electrons). Without means for confinement this speed would be sufficient to carry all of the particles to the wall in less than a millionth of a second. But now, if a magnetic field were to be established inside the tube by passing electrical current through a magnet coil wound around it, something very different would occur. In this case, the direction of the magnetic field, visualized by lines of magnetic force or by the direction a compass needle could point, is parallel to the axis of the tube. In such a field the charged particles could in general no longer move in randomly directed straight paths, but would instead be constrained to move in the direction of the lines of force (axially) following tightly wound (coil spring-like) trajectories.

If sufficiently intense, such a magnetic field would provide a very effective degree of containment, as far as sideways motion of the plasma particles is concerned.

Unfortunately, the simple system just described could not function as an effective magnetic bottle. Since a magnetic field exerts a force that is *perpendicular* to its direction (parallel to the axis of the tube), a field such as here described could not prevent the plasma from rapidly leaking out the end of the tube, much as water escapes from an open pipe. Something must yet be added to the picture to produce a magnetic bottle.

Closed or Open-Ended?

The conversion of the concept of magnetic confinement to reality involves both a choice and an inventive step. The reason for this is exemplified by the failure of a uniform magnetic field, directed along a cylindrical tube, to do more than prevent the plasma from escaping transversely. Two fundamentally different ways present themselves for resolving this dilemma: (1) The magnetic field may be caused to close on itself by bending the tube, and the magnetic coil wound around it, into a circle to form a torus, i.e., a doughnut-shaped chamber or (2) The ends of the

open tube may some how be provided with magnetic stoppers to prevent the escape of particles.

Toroidal Confinement

Consider now the closed tube. Here the magnetic lines are all contained within the tube. The particles, moving along these lines in their helical trajectories seemingly cannot escape. However, the simple toroidal magnetic bottle possesses a fundamental flaw, a flaw that prevents it from confining a plasma appreciably longer than if no magnetic field at all were present. The origin of the flaw hints at the subtlety of magnetic confinement. The key point is this: whenever the lines of force of a magnetic field are bent, i.e., the direction of the magnetic field changes in space, then the strength of the magnetic field can no longer be uniform. A simple example is the magnetic field surrounding a straight wire carrying a current. Here the magnetic field lines are circles surrounding the wire, and the intensity of the field falls off as one moves away from the wire. The example also illustrates another point, valuable for understanding magnetic confinement: The local strength of a magnetic field always weakens in directions in which the magnetic lines of force are *concave*, i.e., bulge away from the observer. Conversely, the field always increases in strength in directions in which the field lines are convex, that is bulge toward the observer.

Here then is the flaw in the simple toroidal magnetic bottle: bending a cylindrical tube and its surrounding magnet winding into a torus creates a magnetic field inside the tube with lines of force that are circles. Thus the lines of force will appear concave looking from the inside toward the outer wall of the torus. This means that the magnetic field strength decreases in this direction. Suppose one attempts to use such a field to confine a plasma.

Magnetic confinement implies a balance between (a) the magnetic forces exerted inward on the plasma and (b) the plasma pressure, arising from its tendency to expand (as any gas tends to expand). But in the simple torus there can be no such pressure balance. By simply expanding toward the outer wall of the torus, the plasma will find itself opposed by even weaker magnetic forces, thus it may more readily expand, and so forth, and so forth, until it strikes the outer wall. This resembles what would happen if one attempted to contain high pressure gas in a vessel, one wall of which became progressively weaker with outward movement. The result would be an explosively rapid escape of gas.

The Stellarator and Rotational Transform

One aspect of the trouble with the simple torus is that particles of the plasma that are moving in their helical

paths in the vicinity of the outer wall spend all of their time in regions where the plasma drift is toward the wall, whereas particles streaming along near the inner wall of the chamber spend their time in fields where the drift is away from the wall. The invention of the stellarator solved this problem. The idea of the stellarator is to arrange matters so that the particles, in their flow along the lines of force, spend part of their time nearer the outer wall, but also part of their time nearer the inner wall. Thus if they are moving rapidly enough along the field lines their drifts, being alternately toward and away from the wall, will just average to zero. The way this trick was first accomplished was to bend the tube into a “figure eight”.

It was later found to be just as effective to use extra windings, in addition to the regular magnet coil windings, to accomplish the “twist.”

When these special windings, which take the form of longitudinal conducting bars, are energized, the effect is to cause the field lines, formerly circular, to become helical. The magnitude of the field twisting effect produced is called the degree of “rotational transform.” (In a single torus the rotational transform is zero.) The particles, now spiralling about and guided by these helical lines spend time alternatively near the outer and then the inner chamber walls, thus experiencing the averaging effect mentioned above. This is the basic idea of the Stellarator.

Open-Ended Bottles: The Mirror Machine

It was noted earlier that another possible method of solving “the problem of the ends” would be to provide some kind of magnetic stopper that would keep the fusion fuel ions from escaping out of the ends of the tube before they had a chance to collide and react. Fortunately, there exists an effect, known for many years, that can be used to make a magnetic bottle out of a straight tube. This is the so-called “magnetic mirror” effect. The name refers to the fact that a charged particle, spiraling along a magnetic line of force, will generally be “reflected”, i.e., its component of motion along the line of force will be stopped and then reversed, if it encounters a region of stronger-than-average magnetic field. A mirror machine is therefore simply a confining field of roughly tubular geometry, capped at both ends by locally strengthening the field. The earth’s magnetic field, above the stratosphere, is an example of one such field, a terrestrial “mirror machine” where there exists the Van Allen radiation belts, plasmas of natural origin.

A mirror machine is said to be an “open-ended” magnetic bottle because the magnetic lines of force, though tightly converged at the mirrors, actually pass through the mirrors, diverge, and then pass out the ends of the chamber. For this reason, there always exists a small leak path for particles along the field lines and therefore the containment

of particles in a mirror machine can never be as perfect as it would be in an ideally operating stellarator.

The Astron Concept

Many of the advantages of open and closed magnetic bottles may be obtained in a single bottle by means of the Astron concept. The Astron combines the magnetic field of a simple mirror bottle with the magnetic field created by a very strong cylindrical current of high-energy electrons coaxial to the coils that form the mirror. The combination of the two fields should convert the initially open-ended field pattern to one of closed lines of force. The energetic electrons travelling through the trapped plasma lose energy by collisions and by this means are expected to heat the plasma to fusion temperature.

Other Possibilities

Though all magnetic bottles, of whatever form, must fall into one of the two categories, open or closed, the examples that have been given do not exhaust the possibilities. In an abbreviated discussion of this kind it is not possible to mention all of the hybrid systems or variations on the basic themes that have been proposed or are under investigation. In the several years since fusion research began, many of these ideas have fallen by the wayside. An example of this is the so-called “toroidal pinch.” The idea, intuitively attractive, was to compress and confine a hot plasma by utilizing the magnetic field produced by intense electrical currents transformer-induced in the plasma itself. When tested, toroidal pinches proved to be hopelessly unstable, a fact borne out (indeed predicted) by theoretical calculations. This entire “approach” was therefore dropped, even though it at one time made up a large fraction of the world’s fusion effort.

Creating the Plasma

Up to this point the demanding conditions for achieving fusion have been outlined—including the most demanding of all: containment.

But even assuming that the containment problem is solved by the use of some form of magnetic bottle, there still remains the question of how to create a hot plasma inside the bottle. Though not as basic as the problem of confinement, this problem is by no means trivial. Again a dilemma: Given a magnetic bottle that is adequately leakproof against plasma escape, such a bottle will also be impervious to the entry of the very plasma particles one wished to contain. Solutions: one must either (1) create and heat the plasma in situ or (2) introduce the plasma particles “in disguise” (for example as energetic neutral atoms),

then remove the disguise inside the bottle, or (3) weaken the magnetic field in some way so that it cannot resist entry of the plasma, then immediately strengthen it before the plasma can escape. All three of the above methods or variations of them are used in confinement experiments today. The choice of which method is best to use depends on the type of magnetic bottle being studied. For example, in the stellarator the most straightforward method has been to fill the chamber with neutral gas, ionize (break up the atoms of) this gas with a high frequency discharge, heat it “lukewarm” by passing an electrical current through it, and then finally heat to high temperatures by means of resonant excitation with powerful radio waves, tuned to the “cyclotron” period of rotation of the plasma ions in the confining magnetic field.

Though all three of the basic methods listed above have now been proved successful in creating plasmas of thermonuclear temperature, for years the technical problem of creating the hot plasma proved one of the most difficult elements in experimental high temperature plasma research.

An “off-beat” method of creating a hot plasma, just now being examined, uses a laser beam to produce a plasma inside a magnetic bottle. The laser beam is focussed, through a window in the side of the vacuum chamber, onto a tiny pellet of solid material (such as frozen deuterium) dropped through the beam or else held suspended by electrostatic forces. The concentrated energy of the laser beam vaporizes and ionizes the pellet, producing in situ a cloud of hot plasma.

Loss Processes and Plasma Instabilities

The key question in achieving useful power from fusion reactions is, of course, not whether or how the reactions will occur, but rather, what are the energy loss processes that compete with the fusion energy released, and could they overwhelm it unless minimized? The situation is not unlike that of the gas turbine (a compressor driven by a turbine with a combustion chamber in between) where the concept was born years before the actuality. In this case, success hinged on the development of compressors efficient enough to do their job without requiring the full power of the turbine to do it.

Radiation and Charge Exchange

Energy losses in fusion can be divided into two classes: (1) unavoidable ones and (2) ones that must be minimized by cleverness or increase in size, or both. In the first category are the radiation losses. These losses can be calculated with precision and it is known that they are tolerable, but only (1) if the plasma temperature is high enough (fusion

reaction rates increase faster with increasing temperature than do radiation losses) and (2) if contamination by impurity ions of higher atomic number (e.g., oxygen, or metal ions) is kept to very low levels. The impurity problem, though important, has fortunately been brought under control in most high temperature plasma experiments. There is another loss process, of non-plasma origin, that must be faced. Fortunately this one becomes less important the closer thermonuclear conditions are approached. The process, called “charge exchange”, may occur whenever a plasma fuel ion (e.g., a deuteron) passes in the vicinity of a neutral atom (say an impurity atom). The attractive force between the ion and a bound electron of the atom may cause the electron to be plucked off and captured by the fuel ion, thus converting the hot, confined ion to a fast-moving neutral atom. No longer influenced by the magnetic field, the fast atom escapes, leaving behind a low energy impurity ion. The need to minimize charge exchange losses explains the need for extremely good vacua in fusion experiments where the plasma density is initially low. Charge exchange losses are less important in denser plasmas, which protect themselves by rapidly ionizing (through collisions with the electrons) any neutral atoms that attempt to enter. This process is sometimes called “burn out”.

Collisional Diffusion

Another “unavoidable” loss process is that arising from collisions between the particles. “Collisions” are obviously essential for fusion reactions, yet not all “collisions” between fuel ions lead to reactions; some lead only to deflecting the path of the ion into a new direction. Such deflections represent a mechanism for particles to escape from a magnetic bottle. To see this, note that a sudden deflection in the motion of the particle will move the axis of its helical trajectory sideways, i.e., across the confining magnetic field. An accumulation of such deflections, even though randomly directed, would eventually carry the particle to the chamber wall. The theory of this kind of “collisional diffusion” in closed (toroidal) systems indicates that losses from this process can be made small compared to the fusion energy release, provided strong magnetic fields are used and the chamber diameter is not too small. Neither the strength of field nor the chamber diameter required would be impractically large. The theoretical margin is much less robust for open-ended systems, but even in this case the use of sufficiently high temperatures and sufficiently powerful mirrors should permit a favorable power balance.

Plasma Instability

Up to this point the discussion has been concerned with a well-established body of knowledge from which a

meaningful picture of fusion and its potentialities can be drawn. Yet there remains a key issue at the focus of all fusion research: plasma instabilities. Because of both its relevance and its intrinsic scientific interest, the instability problem has stimulated an almost unprecedented advance in plasma theory, coupled with a rapidly growing body of corroborative experimental evidence. The picture that has emerged can be summarized as follows:

1. Concern about plasma instabilities in magnetically confined plasmas arises because unstable behavior can lead to more rapid loss of the plasma than do the “controllable” losses mentioned above. However, it now appears that the effect of plasma instabilities on magnetic confinement may vary widely—from catastrophic to innocuous—depending on the type of instability considered, and on the type of magnetic bottle employed. Control of instabilities must be adequate but apparently need not be complete.
2. Plasma instabilities can be divided into two general classes: (a) gross or “hydromagnetic” types and (b) wave-particle instabilities. As the name implies, gross instabilities involve unstable motion of the plasma as a whole across the confining field. Means for completely controlling gross instabilities in important cases have now been found. Wave-particle instabilities, often called “microinstabilities”, involve resonant interactions between plasma waves (electromagnetic, electrostatic, or acoustic waves) and some ordered motion of the plasma particles—e.g., the circling of ions at their “cyclotron” frequency in the confining magnetic field. These wave-particle instabilities are therefore analogous to the resonant amplification of a light wave as it passes through the “lasing” medium.
3. Theory has provided a valuable guideline for identifying the basic sources of energy that feed plasma instabilities and for systematically reducing these “energy reservoirs.” The sources relate to the extent to which a given confined plasma is not uniformly randomized, i.e., particles moving randomly. Thus density or temperature variations, or poorly randomized particle velocity distributions are all potential sources of instability. The clue to minimizing tendencies for instability is to remove or to minimize as many of these sources as possible, consistent with confinement. Though such sources can never be completely removed, this points to the value of creating well-randomized plasmas, and to the need for scale-up in size of the plasma resulting in less abrupt density variations.

Not only is the elucidation of plasma instabilities of central importance for fusion research, but it bears on other

possible technological applications of plasma physics. As an example, some wave-particle interactions, that are called “beam-plasma instabilities” in fusion research circles, are closely related to phenomena used to practical advantage in certain ultra-high frequency electronic tubes. Furthermore, our understanding of the complicated astrophysical plasma phenomena seen both in the solar system and in outer space depends heavily on an understanding of plasma instabilities.

Pulsed or Steady-State? The Question of Plasma Density

In addition to the presently unresolvable choice between open-ended and closed magnetic confinement there is another area in which a choice of research emphasis can be made. In thermonuclear plasma the rate of reaction increases with increase in the plasma density, as does the pressure exerted by the plasma on the confining field. Thus, although the use of very low plasma density would greatly simplify the problem of achieving plasma stability, through reducing the plasma pressure, such “low beta” operation would drop the thermonuclear power density which varies as the square of the particle density to uselessly low levels. Beta is defined as the ratio of plasma pressure to magnetic field pressure. In a “low beta” plasma the plasma has little influence on the magnetic field, which permeates it completely. With “high beta” plasma the magnetic field inside is much weaker than it is outside, being “pushed out” by the pressure of the plasma.

Very high density, on the other hand, would greatly reduce the required confinement time but would result in a near-explosive release of fusion energy, require extremely intense magnetic fields for confinement, and could be expected to increase the probability of encountering instabilities. Between these two far extremes lie two possible operating regimes for a fusion reactor:

1. *“Intermediate” Densities.* “Intermediate”, as used here, refers to particle densities of around 10^{14} ions per cubic centimeter, i.e., about 1/100,000 of atmospheric densities. A fusion reactor operating at intermediate particle densities would have fusion power densities of a few megawatts per cubic meter of reacting plasma. To achieve a net energy release from such a plasma as defined by the “Lawson Criterion”, the average ion containment time must be greater than 1 s. The plasma pressure would be modest (a few hundred pounds per square inch), so that the reactor would be one of “low beta.” The “Lawson Criterion” states that a minimum condition for a confined’ thermonuclear plasma to produce a self-sustaining reaction against loss of energy through particle loss, is

that the product of the particle density n (in ions per cubic centimeter), and the average ion containment time, t (in seconds), must be larger than a certain number (typically 3×10^{14} for a deuterium–tritium plasma).

2. *“High” Densities.* As used here, “high” refers to particle densities greater than 10^{16} ions per cubic centimeter, i.e., about 1/1,000 of atmospheric densities. A fusion reactor operating at high particle densities would have fusion power densities of about 10,000 megawatts per cubic meter of reacting plasma. To achieve a net energy release from such a plasma the average ion containment time must be greater than 0.01 s. The plasma pressure would be high (several thousand pounds per square inch), so that the reactor would be one of “high beta.”

Self-sustaining reactions can, in principle, be produced over a wide range of plasma densities and confinement times, so that the choice of these quantities can be made on the basis of other considerations. On the other hand, the temperature of a thermonuclear plasma cannot be so freely chosen. The nature of fusion reactions requires that the plasma temperature exceed about 100 million degrees centigrade (about 10 kiloelectron volts average energy of motion of the particles). If it is assumed that adequate suppression of plasma instabilities will eventually be achieved, then, based on the intermediate density operating mode suggested above, continuously-fed fusion reactors could be visualized. For example, theoretical studies of closed and open-ended systems operating in this mode were made several years ago. These studies all indicated favorable power balances, assuming stability.

It is possible that plasma stability adequate to permit long-time confinement may not be achievable using present ideas. In this case, high density, pulsed operation would be required, in order to minimize the importance of confinement time. Again, one can visualize possible operating modes along the lines of a “pulsed reactor.” Only time will tell which of the two modes will be the more successful.

Minimum-B and Average Minimum-B

One of the most promising recent developments in fusion research has been the theoretical prediction (and experimental proof) of the gross stabilizing effect of “minimum-B” or “positive-gradient” mirror confining fields. Though the idea was first put forth in 1958, in the U.S., the USSR made the first definitive test in 1961.

The basic idea is to arrange the coils that produce the confining field so that there exists a region between the mirrors where the field intensity has a local minimum. Plasma trapped in such a region cannot escape through

gross instability, since any sideways motion of the plasma as a whole will carry it into a region of higher magnetic field. The analog is any well-designed pressure vessel, the walls of which stiffen with any outward motion. The minimum-B idea is not only obvious but it works! Following the first demonstration, this principle has been put to work in plasma experiments throughout the world. Remembering the discussion of convex and concave lines of force, it can be seen that a requirement for forming a minimum-B region is that, looking outward in any direction from this region one must see convex lines of force. That this requirement can also be made compatible with having tightly converged lines of force (the mirror regions) at each end is not obvious, but can be made plausible by a simple demonstration: Roll a sheet of paper into a cylinder. Now strongly squeeze the cylinder at one end, forming a flattened ellipse as viewed from the end. This is one of the mirrors. Keeping the first end compressed, squeeze the other end of the cylinder so as to form another flattened ellipse at right angles (as viewed from the end) to the first one. Looking next at the resultant geometric figure (a sort of trapezoid), one can see that this figure could be generated by a set of convex field lines going along the surface from one end to the other. This is a rough model of a “quadrupole” (four-sided symmetry) minimum-B field. A novel way by which a quadrupole minimum-B field can be produced in the laboratory is to wind the magnet coil in the shape of the seam on a baseball. Such coils have been built and are in use.

The desirable stability properties of open-end minimum-B mirror fields prompted theorists to see if a minimum-B field could be achieved in a torus by winding properly shaped coils around it. It was soon apparent from the geometry that it is impossible by these means to create a toroidal region that everywhere along its length has the minimum-B property. This can be seen in terms of the “convex line” argument by noting that lines of force that close around a circular path cannot everywhere be convex (curve away from) an observer walking along them—somewhere they have to curve back to get around the bends.

Despite the impossibility of finding magnet windings that would produce true minimum-B in a torus, careful theoretical investigations showed that the next best thing could be accomplished. Thus was born the “average minimum-B” concept. In such a field a contained plasma, if it attempted to move in any direction as a whole, would encounter *on the average* a more intense magnetic field.

Provided the communication between “good and bad” curvature regions is adequate, average minimum-B should stabilize gross instabilities. Theory indeed indicates that at high temperatures, where plasma flow along lines of force is free and rapid, stability against gross modes should be achieved. Average minimum-B is another illustration of the

important principle of picking a magnetic confinement geometry on the basis of its stability properties.

Promising though the minimum-B and average minimum-B principles are, it should be recognized that they are not panaceas. These principles are aimed mainly at eliminating grossly unstable behavior; their influence on the “fine grained,” wave-particle, instabilities are much less marked. These instabilities must therefore be tackled by other means; for example, by control of the detailed physical conditions, or the size, or the shape of the plasma.

Many of the problems discussed thus far may be overcome if the Astron concept reaches fruition. The Astron configuration is expected to have two properties of significance: First, the plasma could have a distribution of particle velocities favorable to confinement. Second, the magnetic field intensity should increase outward from the plasma, thus preventing hydromagnetic instability. Such a configuration should eliminate end losses of the simple mirror, overcome the inherent hydro-magnetic instability of the mirror, and provide a means of heating plasma at the same time.

Technological Developments

In the past the most important single factor setting the pace and defining the level of experimental research in controlled fusion has been in the area of technological development. These developments have encompassed an extremely broad range of techniques, including:

1. The storage, switching, and ultra-fast delivery of large quantities of electrical energy.
2. The generation of large volumes of intense, precisely shaped, magnetic field, both pulsed and steady-state.
3. The achievement of extremely high vacua in large chambers, and in the presence of intense particle beams.
4. The development of wholly new methods of generating intense directed bursts of high energy plasma.
5. The development of intense and precisely focussed beams of energetic charged particles or neutral atoms.
6. The extension of spectroscopic, microwave, laser beam, and electrical measurement techniques to cope with the special problems of “plasma diagnostics.”

In some cases the special technological developments required prior to the construction and operation of an experiment have taken several years to complete. Each increase in the level of experimental sophistication has had to be accompanied by extensions of the technology.

Many of the techniques developed in fusion research have been taken over directly by other fields of modern research. As an example, plasma diagnostic methods, ultra

high vacuum techniques and particle beam and plasma generation systems developed in fusion found immediate application in propulsion, simulation, and instrumentation needs of the space program.

Fortunately, at this stage in fusion research, some of the most difficult of the needed technological developments are completed, and the emphasis can accordingly be shifted more heavily toward the planning and execution of the experiments themselves. This is an extremely important change, one that can be expected to lead to a substantial increase in the rate of scientific output in many areas of fusion research.

Some of the developments that will contribute to this increase are in the field of cryogenics. Cryogenic techniques have at least two important applications in fusion research: (1) In the achievement of ultra-high vacuum by “cryo-pumping” (freezing out impurity atoms), and (2) In the generation of large volumes of intense magnetic field with no expenditure of electrical energy, by means of the new superconducting (zero electrical resistance) materials. This latter development seems extremely significant, not only for the short-range needs of fusion research, but for the long range promise of the whole concept of a fusion reactor, where magnetic field would be a critical commodity.

The Shape of Things to Come

In fusion research, as in many other fields, one result is worth a thousand predictions. Speculation as to the course of events is bound to be just that, speculation. Nevertheless, there are certain aspects of the directions in which the search for fusion power might proceed and the possible results that can be seen, at least in outline. Even the outlines excite the imagination. Here is a possible course that fusion might take: In the short range one would see the patient unraveling of critical aspects of the question of plasma instabilities. Here, in addition to selective experiments inter-comparisons between results found in ostensibly widely different circumstances would be valuable. For example, a result found in an open-ended system might supply a key bit of information needed in the evaluation of a proposed closed system. At this phase it is essential to study a wide variety of systems, since the problem of plasma confinement and stability needs comprehensive study if a clear picture is to be obtained. In this period of rapidly developing plasma technology, the pace of research is determined by the time needed to prepare for an experiment; usually much less time is needed to obtain the scientific results. Thus, if the fusion physicist could, at will, wave a magic wand and create his confining magnetic field, in any shape desired, and then fill his confinement chamber with plasma of just the characteristics he desired, his

research timetable would be very different: The salient features of the physics answer he is seeking could probably be exposed in a matter of months, from start to finish.

During the exploratory phase of fusion research, the “bright idea” is an especially important element. Within the last year or so, “bright ideas” have appeared that have had a major effect on the course of research. An example is the idea of plasma stabilization by “average minimum-B,” an idea that may be of profound importance to the future of toroidal confinement systems. It seems highly likely that more “bright ideas” will appear in due course. Their effect could be equally catalytic.

Behind the evolving story of confinement and stability the evolution of plasma technology will go on, with a most significant result: If adequately stable confinement of dense thermonuclear plasma can be demonstrated, the technical means for generating and confining these plasmas will already be well in hand, thus accomplishing a critical technical step on the way to fusion power. This same basic understanding and technology could be carried over into other practical or scientific applications of plasma technology, such as communications, national defense, space exploration, particle accelerator technology or astrophysics. Examples where this is already coming about include: ultra high frequency tubes, upper atmospheric weapons effects, plasma propulsion, new ideas for particle accelerators, and the theory of magnetic clouds.

If it is further assumed that the above exploration and consolidation phase of fusion research is successfully passed, the final phase of fusion research will probably be largely shaped by economic or social-political factors that cannot be predicted with any certainty. This phase might see a rapid application of fusion research results to practical fusion power, stimulated either by intrinsic economic advantages of fusion or by considerations of safety or pollution vis-a-vis other energy sources. Just when fusion power might emerge as a major (or sole) power source cannot be predicted; that it will someday occur, given proof of scientific feasibility, there is virtually no doubt.

What might the fusion reactor of the future look like? It would be premature today to try to draw a blueprint for the fusion reactor of tomorrow. Yet some things are clear from what is already known today, so that the general features of what a “magnetic bottle” type of reactor would look like can be foreseen. Assuming first that stability, adequate to permit long-time confinement of plasma of intermediate density is achieved, this regime could lead naturally to the idea of a large, continuously operating power plant, presumably with power output comparable to large present-day central power plants. The confining magnetic field would probably be provided by use of either superconducting or cryogenic magnets (normal conductors at low temperature). At least at first such a fusion reactor would

doubtless use the deuterium–tritium (D-T) reaction, with regeneration of the tritium through capture of the D-T reaction neutrons in a blanket surrounding the chamber. The constituent parts of the reactor per se would therefore be a large vacuum chamber, surrounded by the blanket and the magnet field coils and provided with auxiliary apparatus for evacuating the chamber and introducing and heating the fusion fuel. A high pressure steam turbine and generator would complete the system, except for equipment needed to regenerate and recirculate the tritium. Thus, the reactor itself would be in some ways similar to a modern steam plant, both as to size and function.

Suppose long-time stability is not in fact achievable? The pulsed reactor then offers a possible alternative solution. Here a smaller, repetitively operated, system would be indicated. Such a system would have to face appreciably greater material and technological problems than would a steady-state system, but these problems do not at present appear insurmountable.

What is the probability that some wholly new approach to fusion power might be discovered that would bypass the need for magnetic confinement? Such a possibility cannot be definitely ruled out, but 13 years of highly motivated, intensive study of the fusion problem have failed to turn up any workable alternative, though many proposed solutions have been advanced. That any solution would not involve plasma phenomena in some way seems highly unlikely.

For the even more distant future if fusion reactors can be perfected, there is an additional possibility for a revolutionary change in power generation. This is the promise of direct conversion of the fusion energy to electricity, particularly if the D-D reaction could be exploited. An understanding of plasma behavior will almost unquestionably permit the investigation of efficient and inexpensive means for direct conversion. What better environment for direct conversion could exist than the fusion reactor itself, with its controlled plasma and electromagnetic environment?

Today, one can only talk of “possibilities” in fusion power. Yet the avenue to finding the reality of these possibilities is open to us today, through pursuit of controlled fusion and high temperature plasma research.

The Fusion Handbook

The most significant aspect of the cutting edge of present day fusion research and its rate of progress toward the goal of controlled fusion does not lie in impressive technological developments nor even in the many milestones (relative to plasma temperatures, densities, or confinement times) that have been passed. It lies instead in the fact that

experiments aimed at elucidating crucial aspects of the physics problem of plasma stability are being performed, and in the fact that these experiments are advancing hand-in-hand with well-founded and relevant theoretical calculations. The kind of solid understanding that is being built in this way will endure long after the specific technical achievements are forgotten, and this understanding will constitute the “handbook” from which fusion reactors may someday be designed.

The process here described is characterized by two trends. First, experiments aided by the new technology are being aimed more and more toward elucidating crucial physics points, and less and less toward unselective techniques. Second, experiments aided by modern digital computer technology are being directed toward more and more “realistic” problems. The convergence of the two trends is obvious, and of great significance. In these trends lies the surest promise that fusion research will find the right answers to the right questions, and in the shortest possible time.

The U.S. program of controlled fusion research began in 1951 and was conducted as a classified program at AEC laboratories until the time of the Second International Conference on the Peaceful Uses of Atomic Energy in Geneva, 1958. At the time of the Geneva Conference, the U.S., the United Kingdom, and the Soviet Union jointly declassified their respective programs. It was clear then that the U.S. was conducting the most comprehensive and intensive of the world-wide programs.

American scientists developed a major part of fundamental plasma theory, with special emphasis on the stability questions of plasma confinement. U.S. theoretical research on gross (hydromagnetic) plasma properties was especially thorough, and the major predictions of these theories have since been experimentally verified.

An important aspect of the U.S. program has been the construction of powerful devices both to produce the required plasma and to provide an environment for the study of plasma confinement. Using one of these devices, U.S. scientists at Los Alamos achieved the world’s first clear demonstration of a thermonuclear plasma in the laboratory. Shortly thereafter, scientists at Lawrence Radiation Laboratory also demonstrated the production of a thermonuclear plasma, using a different type of device.

The U.S. controlled fusion program is conducted principally at four major sites:

Lawrence Radiation Laboratory (LRL) Berkeley and Livermore, California
 Los Alamos Scientific Laboratory (LASL) Los Alamos, New Mexico
 Oak Ridge National Laboratory (ORNL) Oak Ridge, Tennessee

Plasma Physics Laboratory (PPL) Princeton University,
New Jersey

Reviews of the Program by External Committees

There have been two extensive reviews of the Controlled Thermonuclear Research effort in recent years: one by a sub-panel of the AEC General Advisory Committee (GAC) in 1962, and another by a special Review Panel in 1965. In addition, the whole of plasma science has been surveyed by a subpanel of the Pake Committee of the National Academy of Sciences and by a panel contributing to the Energy Resources Report to be issued by the Office of Science and Technology.

The 1962 Report of the General Advisory Committee

This report was prepared by a subcommittee of the GAC, whose members were:

Dr. Philip H. Abelson, Chairman
Professor Manson Benedict
Dr. Robert A. Charpie
Professor Norman F. Ramsey
Professor John H. Williams

Both positive and negative reactions to individual sub-programs were recorded, but the general recommendation was “to continue to support Project Sherwood vigorously.”

In support of this recommendation the subcommittee made the following statement:

In the Sherwood program there remain many unresolved technical questions that can be decisive to the feasibility of controlled fusion as a source of economic power. These relate particularly to the problems of stable confinement of hot plasmas in configurations suitable for power, to methods of initially heating the plasmas, to problems of purity, etc. However, it appears that, if these feasibility problems can be overcome, controlled fusion power could be economically competitive at least with breeder reactors. For this reason we believe that the AEC should continue a vigorous program in controlled fusion research and in basic research in hot plasmas. However, this recommendation should be reviewed periodically since its continuing validity is dependent upon future technical developments.

The conclusions and recommendations of this GAC report are attached as “[Appendix 1](#)”.

1965 Review Panel

This panel was appointed by the AEC General Manager with the concurrence of the Commission. The membership was as follows:

Professor Samuel K. Allison	University of Chicago
Dr. Peter Auer	Advanced Research Projects Agency
Professor Gordon Brown	Massachusetts Institute of Technology
Dr. S. J. Buchsbaum	Bell Telephone Laboratories
Professor Raymond G. Herb	University of Wisconsin
Dr. David D. Jacobus	Harvard University
Dr. Thomas H. Johnson	Retired; formerly Director of Research, the Raytheon Company
Professor Eugene N. Parker	University of Chicago

The first Chairman was Professor Allison. After his untimely death in September, 1965, Professor Raymond G. Herb became Chairman. Special measures were adopted to insure that the Panel could obtain a comprehensive view of fusion power research here and abroad:

1. The four major Sherwood laboratories prepared status reports designed particularly to meet the needs of the Panel. These documents were discussed with the other laboratory directors and the AEC, modified and then presented to the Panel at the time of its organization.
2. In June and July, 1965, the Panel visited the four major laboratories. At each of the sites, scientists made presentations to the Panel.
3. Subgroups of the Panel visited several of the smaller Sherwood sites, and representatives from others were heard.
4. Presentations were also made to the Panel by representatives of Aerojet-General Nucleonics, General Atomic, General Electric, and the Naval Research Laboratory. In addition, discussions were held with Dr. Stirling A. Colgate and Dr. Henry D. Smyth.
5. Three members of the Panel went to the International Conference on Plasma Physics and Controlled Thermonuclear Research at Culham, England, in September, 1965.
6. Supplementing the above contact with scientists in the field, the Panel also had frequent discussions with those at AEC headquarters responsible for the administration of the Sherwood program.

The report of this Panel is presented as “[Appendix 2](#)”. It should be noted that the findings and recommendations

of this report were made *unanimously* by the Panel members.

1966 Report of the Subpanel on Plasma Physics:
National Academy of Sciences

This report was prepared as input to a general survey on the status and needs of physics research as a whole. The members of the subpanel on plasma physics were:

Professor Marshall Rosenbluth, Chairman	University of California at San Diego and General Atomic Corporation
Professor William E. Drummond	University of Texas
Professor Melvin B. Gottlieb	Princeton University
Dr. Arthur Kantrowitz	AVCO-Everett
Dr. Richard F. Post	Lawrence Radiation Laboratory
Dr. Eli Reahotko	NASA Lewis Research Center and Case Institute of Technology
Professor Peter A. Sturrock	Stanford University

Surveys were made of several areas involved in plasma research astrophysics, gaseous electronics, propulsion, direct conversion, fusion, solid state plasmas, military plasma research, general plasma research.

With respect to the controlled thermonuclear research, the Panel concluded:

The fusion program is now showing great promise, with increasing agreement between theory and experiment in the critical stability area, and greatly improved experimental capabilities. A 1964 Energy Resources Study of the Office of Science and Technology recommended an approximate doubling of the fusion program within the next 5 years (up to the 1965 estimated level of the Soviet effort). We agree that fusion is a desirable national scientific objective and recommend that the following guidelines be used in this expansion:

1. It should be kept in mind that fusion is a long-range program and it should be planned as such. In deciding which large-scale experiments should be performed an important criterion should be the possibility of detailed comparison between theory and experiment so that an orderly buildup of knowledge may proceed. Such knowledge will also serve other areas of plasma physics. The short-range objective at this time should

be the attainment of a quiescent plasma with physical parameters in the approximate thermonuclear range with which such comparisons can be made. On the basis of recent theoretical advances it appears clear that the minimum-B concept best meets these criteria at this time.

2. Small-scale basic experiments such as beam-plasma interactions and Cs plasmas, which give a quantitative check on plasma theory in somewhat different physical regimes, are a very necessary component of the fusion program.
3. A strong effort must be made to produce a flow of new people and ideas into the program. The invigorating effect of young people on the U.S.S.R. program greatly impressed the recent fusion exchange team. At the same time, scientists with fusion experience would provide valuable new viewpoints in other areas.
4. It is recommended that support for fusion be diversified and not kept completely within a single agency.

We feel that these aims should be kept in mind even if it should prove impossible to obtain the desired increase in the overall program.

Evaluation of the Overall Effort on Controlled Fusion

In the period since the submission of the Review Panel's report, a number of meetings have taken place between representatives of the Commission, Directors of the major Sherwood projects, and members of the Panel itself. These meetings have permitted extensive discussions not only of the findings and recommendations of the Panel, but of additional topics relating to the progress of the Sherwood effort. In addition, the Report of the Review Panel, together with a draft of the present policy and action document, was examined by a subpanel of the President's Science Advisory Committee (PSAC), by PSAC itself, and by the Commission's General Advisory Committee (GAC). Statements by the latter groups are presented as "[Appendix 3](#)".

The overall evaluation of this program by the Commission is summarized in the paragraphs which follow. As will be seen, the Commission concurs in large measure (but by no means completely) with the findings of the Review Panel. Based on its evaluation, the Commission presents below the main features of the policy which, in its view, should guide the planning of the AEC-supported program in plasma science and nuclear fusion research in the United States. Specific plans for implementing this policy are then outlined.

Evaluation of the Importance of the Program

By any standard whatsoever, controlled thermonuclear research must be counted as one of the most challenging and potentially important efforts in the history of mankind. The original programmatic interest in this work—and still one of the strongest motivations for pursuing it—is the hope of producing useful power from controlled thermonuclear reactions.

Clearly, it is far too early to sketch with any certainty the details of a full-scale fusion reactor. If this program is successful, however, such a reactor could be expected to have a number of general features of potentially great interest:

1. The fuel is readily obtainable. Indeed, for the ultimate case of a reactor burning deuterium alone (rather than a deuterium–tritium mixture) the fuel supply is not only easily obtained from water but is virtually inexhaustible as well.
2. Since the actual amount of fuel within the reactor is exceedingly small, and since the sudden addition of excess fuel would merely quench the fusion reactions, there would be no danger of causing a reactor “runaway” condition.
3. There exists the long-term possibility of conversion from plasma energy directly to electricity.
4. The final fusion reaction products (helium and neutrons) are non-radioactive. In a simple system, as presently conceived, the only radioactivity associated with the plant itself would be that which is induced by neutron bombardment of the construction materials. (For the case of a reactor using only deuterium as fuel, the tritium produced would be immediately consumed in the reaction chamber.)

In addition to the above programmatic interest in plasma physics and controlled fusion research, there are also compelling interests of a more basic nature for studying the behavior of plasmas. More than 99% of the matter in the entire universe (as we know it) is in the form of highly ionized plasmas. The sun and stars are composed of it; the earth and other planets are bathed in it; plasma fills the vast regions of interstellar space. Its properties have received little attention until now. Yet its bearing on other fields of knowledge and on the future activities of mankind may well be profound.

In view of the above vast potentialities—both of long-range fusion power and of the importance of high-temperature plasma research—this nation must maintain itself at the forefront of the research effort in this field.

Evaluation of the Present Status of the U.S. Effort

For reasons of objectivity, the critique of the AEC’s own program, given in the next five subsections, consists of

verbatim statements by the Review Panel, which are then followed by additional brief comments by the Commission.

General Comments on the AEC’s Program

The Review Panel makes the following statements:

Under this program a broad attack is being waged, aimed at determining, under laboratory controlled conditions, the feasibility of power generation by fusion reactions. This requires as a first step the production and containment of dense hot plasmas. The attainment of this step is the immediate goal of the program.

Because of its complex behavior, plasma must be studied under a wide variety of conditions. Many of its salient properties emerge only when it is contained in large volumes offered by large machines. Other important experiments are conveniently conducted in smaller size devices. The present structure of CTR research in this country is well proportioned. Considering the complexity of plasma as a fluid and the variety of techniques used in the studies, there is little duplication in the programs of the U. S. laboratories or in those of other countries, although there is enough desirable overlap to permit profitable comparison between results obtained by different groups.

A wide variety of promising experiments are being pursued at the four major AEC-supported laboratories. Some of these are of quite recent origin while others have evolved naturally from lines of attack adopted early in the history of the program. Certain approaches have made the full round and have been discontinued. For example, the linear pinches, the toroidal Z-pinches, and their daughters, the stabilized pinches and hard-core pinches, have all served their purpose well and retired from the field. It is most gratifying to find that great strides have been made over the past 4 years in reconciling the theoretical understanding of plasma with its actual behavior in the laboratory in a number of experiments.”

The AEC takes pride in the above comments by the Review Panel. It is clear, however, that the task of maintaining a well-proportioned program, with limited duplication of effort, will become increasingly difficult as the experimental devices grow in size. For small inexpensive experiments, appreciable duplication is sometimes appropriate, particularly for training purposes. For large and expensive experiments, however, the degree of justifiable overlap becomes much smaller. This problem will clearly require special consideration in future CTR activities.

Critique of the Program at PPPL

The Review Panel made the following statements:

The effort at the Princeton Plasma Physics Laboratory is devoted primarily to the production and confinement of hot plasma in toroidal (i.e., closed) systems of the stellarator type. Measurements of basic plasma properties in such devices have been extremely thorough and extensive. Diagnostic instruments and techniques of great value, such as the ion cyclotron resonance heating method and the divertor, have been developed. Theoretical understanding of plasma physics has been advanced to a high level. Ingenious experiments in basic plasma physics have been performed in the Q-machines. The Model C stellarator still represents the most valuable test bed in the world today for studying toroidal confinement. It will soon have serious competition, however, since the interest around the world in toroidal machines is increasing rapidly. The loss of plasma from this machine remains anomalously high and the underlying causes are not yet understood. Nevertheless, the series of studies just completed on how various heating methods affect the plasma loss rate have been invaluable in advancing our knowledge regarding the difficulties of toroidal confinement. The Model C stellarator is well engineered and efficiently utilized but somewhat inflexible.

During recent months it has become increasingly clear that plasma temperatures achievable in the C stellarator are being limited by the anomalously high rate of particle loss. While not yet completely conclusive, the cause of this loss is widely believed to be due to one of several important classes of instabilities, or possibly to a mixture of them. Both in order to study these instabilities more effectively and to learn how to suppress them, it appears necessary to shift emphasis at this time to new or modified toroidal configurations incorporating high shear and/or minimum-B fields. In order to focus efforts on this problem of plasma confinement and stability, it may be desirable to halt work on new methods of plasma heating (such as magnetic pumping), at least temporarily.

Critique of the Program at ORNL

The Review Panel makes the following statements:

The effort at the Oak Ridge National Laboratory is devoted to a variety of aspects of plasma research. In experiments directed at buildup of hot plasma via energetic charge particle injection into mirror machines, ORNL has been the leader. However, the

complexity which arises from the large particle orbits in the DCX-2 machine makes interpretation difficult, although considerable success has been achieved in understanding the plasma in the less ambitious DCX-1 machine. The beam-plasma interaction studies and hot electron plasma production via electron cyclotron heating have pointed the way in this type of experiment and have yielded results of considerable value. The measurement of basic cross sections has been invaluable. In the past, ORNL appears to have stressed the empirical approach to experimental plasma research. This method of attack may soon become unprofitable.

Experiments at Oak Ridge can and have yielded unforeseen and unexpected results in plasma physics, providing a challenge for subsequent theoretical interpretation and thereby a growth of the science. Nevertheless, in a directed program such as CTR, it will be increasingly important to pursue those areas of work which permit a detailed comparison between theory and experiment.

Critique of the Program at LASL

The Review Panel makes the following statements:

At the Los Alamos Scientific Laboratory there is a unique involvement with pulsed dense high-beta plasma experiments. (Beta is defined as the ratio of plasma pressure to magnetic field pressure. In low beta plasma the magnetic field which permeates the plasma is nearly the same as that in the absence of the plasma. In a high beta plasma the magnetic field inside the plasma is nearly zero.)

The early work on straight pinches has been discontinued and the series of experiments on open-ended fast theta pinches has culminated in the highly impressive Scylla-IV. A number of ingenious diagnostic instruments have been incorporated with this experiment and many interesting results have been recorded. Valuable plasma gun developments have been made. The plasma focus device which produces intense thermonuclear reactions in a small volume is spectacular in its performance. Although many of the large-scale experiments we viewed were well engineered, the impression remained that the engineering support was inadequate for the type of program advocated. There appeared to be a considerable imbalance between theory and experiment. More intense theoretical effort is needed in the difficult regime of dense high beta plasmas.

Experiments with the Scylla-IV at Los Alamos show no evidence of any instabilities: plasma confinement, while

admittedly brief, appears to be limited primarily by end losses. In order to avoid these losses, attention is now turning to the feasibility of constructing a toroidal theta pinch (Scyllac). If studies show that adequate equilibrium and stability can be expected for a high-beta plasma in a toroidal configuration, such a device would appear to be a tool of major importance in the study of plasma behavior.

Certain other experiments at Los Alamos—notably those involving cross-field injection and injection into cusps—appear to be drifting somewhat. It would seem desirable to review these efforts, together with those directed toward producing miniature explosive fusion reactions, in the light of their relative contribution to the program as a whole.

Critique of the Program at LRL

The Review Panel makes the following statements

At the Lawrence Radiation Laboratory three major lines of attack are making rapid progress and providing encouraging results. These are the minimum-B geometry mirror machine, Alice, employing neutral injection; the E-layer experiment, Astron; and the toroidal experiment, the Levitron. The work on Alice, along with supporting experiments in more modest mirror machines, has played a leading role in reconciling stability theories with experiment and providing the scientific community with a larger measure of hope for the ultimate understanding and exploitation of plasmas.

The performance of Astron has been impressive to date. It is an ambitious engineering undertaking and has demonstrated that its E-layer is stable under conditions of modest field reversal. Astron has no counterpart; it is the only unique method being pursued by this country. (The term E-layer refers to very energetic (relativistic) electrons which form a cylindrical shell. The role of this shell is (1) to help produce a magnetic field which can confine the plasmas, and (2) to heat the plasma.)

The series of experiments recently completed in the Levitron have clarified a number of significant points on the behavior of plasmas in toroidal configurations and pointed to several approaches which may ultimately circumvent the instabilities thwarting our efforts.

The large mirror experiment called 2X appears to be suffering from a succession of difficulties and its future prospects do not appear encouraging.

The supporting studies in basic aspects of plasma physics and theory at both the Livermore and Berkeley sites are of high quality.

It is not yet clear to what extent end losses will limit the usefulness of open-ended devices as potential fusion reactors, even if all instabilities can be suppressed. It is clear, however, that open-ended systems can play a major role in the study of instabilities and of ways of suppressing them. Of particular value in this respect are steady-state systems such as Alice, in which the density is increased slowly by neutral injection until limited by an instability or some other loss mechanism which can then be analyzed in detail. Similar techniques may also become increasingly important in closed systems, such as the Levitron.

While results from pulsed systems such as 2X may be more difficult to interpret, they are expected to give valuable information concerning instabilities in the density range between that obtained to date by injection techniques and that of thermonuclear interest ($\sim 10^{14}$ per cubic centimeter).

With regard to the Astron, results to date have been encouraging. Although the weak E-layer thus far produced appears to be completely stable, the question remains whether it will continue to be stable as the electron density is increased toward field reversal.

Critique of the Non-AEC Program

The Review Panel makes the following comments:

The programs at the Naval Research Laboratory, at General Atomic, and at General Electric are all of high professional quality. The program at Aerojet-General Nucleonics is still in its infancy but appears to be well conceived and offers promise. The interaction between these programs and the more extensive AEC supported effort has been valuable and is to be commended. In particular, the theta pinch programs at NRL and GE have had significant influence on the Los Alamos effort. The development of novel neutral injection sources at AGN is being followed closely by workers at Livermore. The theoretical effort at GA has played an important role in CTR both nationally and internationally.

Critique of the AEC's University Program

The Review Panel makes the following comments:

Work at the major AEC supported university plasma laboratories is yielding excellent returns in plasma physics and plasma diagnostic techniques for funds expended. The programs at Berkeley, at MIT, at Wisconsin and at New York University which were viewed by panel members appeared to be especially

fruitful, with excellence in output of research results and of gifted plasma scientists and engineers. Work at Stanford and at the Stevens Institute of Technology is contributing to our understanding of plasma physics.

Research carried out at university plasma laboratories constitutes a very important part of the AEC's program. Among other things, universities clearly play a critical role in the training of scientists and engineers needed to invigorate the CTR effort. While an expanded university program is an integral part of an intensified CTR effort, special attention must be given to maintaining high quality of the research work, with emphasis on theoretical studies of plasma behavior.

Comparison of the U.S. and the World Effort

At the time of the Geneva Conference of 1958, significant work in the field of controlled fusion was being carried out by only three countries in the world: The U.S.A., the U.K. and the U.S.S.R. Of these three efforts, that of the United States was the largest and most advanced, both in theory and experiment. Since that date, many other countries have entered the field. Initially their research efforts invariably followed the lines of attack previously charted by the three leading countries. Their progress was rapid, however, due to their ability to draw heavily on existing knowledge. Nevertheless, as little as 4 years ago the United States was maintaining a position of real leadership in the field. As pointed out by the Review Panel, at that time the U. S. participation in CTR research constituted nearly one-half of the total effort in terms of weighted expenditures and personnel and its effective contribution to the progress of the program amounted to well over one-half of the total.

In the intervening period, however, the relative role of the United States has decreased sharply. Specifically, as of now, the U.S. participation in the field is estimated to have dropped to one-fifth of the total world effort; furthermore, the effective contribution to progress is now estimated to be only about one-third of the total. This circumstance is in sharp contrast with the situation in many other major fields of physics research.

While the development of strong competition in controlled thermonuclear research is both healthy and desirable, the trend indicated is alarming. If this decline in stature (relative to the rest of the world) is allowed to continue, it is obvious that the CTR program in this nation will soon deteriorate to a secondary role.

Existing Budgetary Considerations and their Consequences

Impact on Manpower and Programs

A key factor in the above development is the fact that during this 4 year period the budget for CTR research has been essentially static and largely inflexible. As a result, there has been a severe curtailment of the normal influx of new people with fresh ideas. Another result of the recent fiscal policies has been the lack in both speed and flexibility of adapting or acquiring equipment to test new ideas (see below) which have originated here and abroad. There has necessarily been a tendency to continue studies with equipment that is becoming out-dated, in an attempt to obtain maximum return from prior investment.

By contrast, the efforts in Western Germany, France, the U.K. and U.S.S.R. have expanded rapidly during this intervening period with new devices, new facilities, and vigorous youthful staff. During the same period, intensive programs were created in Japan, Italy, and a number of other countries.

New and Neglected Areas

Fusion research has a number of new areas that need to be investigated, and of interesting new ways of attacking existing problems. Enough is now known about plasma physics to weed out the unpromising or the obviously impractical. Even after the weeding-out process, however, the list of new or neglected areas that should be pursued has been growing.

Listed below are some of the topics that are either (1) now absent in the U.S. fusion program, or (2) have been allowed to lie fallow for lack of budgetary support, or (3) are in the category of new ideas. It should be noted that some of these are already under intensive study in foreign programs.

1. *Dynamic Stabilization*

The whole area of dynamic (non-passive) means of plasma stabilization, including the effect of high frequency fields on plasma stability and confinement is virtually absent from the U.S. fusion program.

2. *Shock and Turbulent Heating; Instability Heating*

One of the most active fields of research today (in foreign programs) concerns the question of heating plasmas to thermonuclear temperatures by shocks, plasma turbulence or by controlled plasma instabilities. Such studies not only

could have great significance to fusion, but also bear on aspects of upper atmospheric and related effects, that are of substantial scientific and practical value. There is little work in progress in this field in the U.S. fusion program.

3. “Average Minimum-B” as applied to open-ended systems

The success of the minimum-B principle in open systems suggests the value of also testing the “average minimum-B” idea in such systems. In addition to practical advantages, such tests might provide early, inexpensively obtained, answers to some of the questions concerning the use of “average minimum-B” in toroidal systems. Essentially no work of this kind is underway in the U.S. program.

4. Special Technological Developments

There are several areas of technological developments that are not being actively pushed in the U.S. program, for lack of adequate budgetary support. The end result of the inadequacy of support has been that either (1) it has been necessary to “make do” with older technology, or (2) the problem has been put off until it becomes intolerably acute, and it has not yet reached that stage. Examples are, respectively: (1) the development of new types of high intensity ion and neutral beam sources; (2) many questions having to do with the environment of a plasma, particularly the walls and their role.

Consideration of a National Center

As a result of the above-mentioned concern over the decreasing stature of the U.S. effort (particularly in a program which is gathering momentum throughout the world), extensive consideration has been given to methods of reversing the present trend.

In addition to the obvious implications on budgetary requirements for this program, it seemed desirable to find other forceful methods of invigorating the U.S. effort.

One of the strongest recommendations made by the Review Panel was that the Commission should take immediate steps toward establishing a National Center for Plasma Studies and Controlled Fusion. As envisaged, the function of such a center would be:

1. To extend and stimulate the controlled thermonuclear program in this country. In this respect, it would supplement (rather than supersede) the existing national laboratories engaged in this work.
2. To permit the development of a wide variety of different experiments at the same site. Such a development would result not only in important cross-fertilization of concepts and techniques, but also in providing the capability for rapid construction of new

experiments, drawing on the broad competence available there. A capability of this nature would be particularly important when and as really large-scale devices are deemed justifiable.

3. To serve as a major center for national and international exchange in the effort leading to controlled fusion. In this way, it would automatically result in a continuing flow of fresh new blood into the program, both from within this country and abroad. This influx of new and young people would correspondingly bring the flow of fresh ideas needed to invigorate the U. S. effort. It is recognized that to be effective in this goal, such a National Center would necessarily have to be free of all security restrictions.
4. To promote close contact between universities and the controlled fusion program and to implement the training of scientists and engineers in this field of research. This end would be accomplished through the institution of cooperative graduate and postgraduate programs, and through visiting and summer appointments for university scientists and engineers.

Clearly there are many questions and problems which arise in connection with the development of such a center—problems which will require careful study and long-range planning. These problems are discussed later.

Overall Conclusions

Scientific and Technical

Research in controlled fusion and the physics of high temperature plasmas has now become an established scientific discipline. World progress toward a detailed understanding of the behavior of high temperature plasma is rapid and accelerating, so that fusion research, initially a largely empirical effort, is fast becoming a quantitative science. Both in scope of effort and in level of sophistication of theory and experiment, there has been a major change in the last 4 years.

Technical problems that have in the past represented serious impediments to fusion research are being solved, and new technological developments are having a major and favorable effect on the research. As an example, the problem of plasma impurities has been brought under effective control in many cases, through the use of newly-developed vacuum techniques, plasma divertors, and other means. The creation of plasma at thermonuclear temperatures, formerly a goal in itself, is now routine in many experiments. Superconducting magnet coils have been developed and are beginning to be tested in fusion experiments. The effect of such coils in future experimentation is expected to be very important.

Plasma Diagnostic Methods are Now Extensive and Permit Measurement of all of the Important Properties of a Plasma

The world experimental and theoretical effort in fusion research is now being focused almost exclusively on the key scientific issue—an adequate understanding of the stability properties of magnetically confined plasmas, on which all future practical applications of such plasmas hinge. Major steps have been taken, both toward understanding and toward control of plasma instabilities. Experimentally, developments include: (1) The conclusive documentation of the effectiveness of “minimum-B” magnetic wells in stabilizing an important class of plasma instabilities, (2) The confirmation in specific important cases of the validity of the basic equations now used to predict plasma instabilities. Marked progress has been made in developing theory which better represents real situations, and in predicting the actual effects of plasma instability and turbulence. Guidelines, based on sound thermodynamic arguments, have been developed for predicting the “most-likely-to-be-stable” plasma confinement conditions. While no definite time-table can be established for determining the scientific feasibility of controlled fusion power, the rate of approach toward this goal is clearly accelerating.

Administrative

In view of the complexities of the problems and the scarcity of funds, it is essential that effective coordination and cooperation exist among scientists in this field. This statement applies not only to activities which are AEC sponsored, but also to interactions with foreign programs. The entire U.S. effort has unquestionably been hampered by a lack of effective coordination and cooperation among the four AEC laboratories; to a lesser extent, it has also suffered from restrictions which have been placed on foreign scientist participation at three of the four laboratories.

Implications Concerning the U.S. Program

The possibility of fusion power captures the imagination, and its eventual economic impact could be of major significance. From the scope and nature of fusion programs in other major nations, it is evident that this field of research provides a focus for the whole new scientific field of high temperature plasma physics. The United States was one of the founders of fusion research, and made the bulk of the original contributions to this field. Though the fusion problem is more complex than originally thought, years have not diminished the importance of finding out if it can be solved, and if so, what its impact would be.

During recent years, however, the United States has been losing its momentum in fusion research, vis-a-vis other nations, due largely to the effect of a static budget and the many consequences thereof. Definitive steps must be taken to reverse the present trend.

Planned Action

Drawing upon the findings summarized in the above section, the Commission presents below the action which, in its view, is required in order to ensure a timely and responsible evolution of the controlled thermonuclear program. This action consists of two parts: a) the establishment of a set of broad policy considerations designed to guide further developments in the program, and b) specific steps toward implementation of this policy.

Policy Statements

1. The United States program in controlled thermonuclear research is clearly of major scientific and technological importance to the nation as a whole, and should be supported at a level which will ensure that the nation maintains high competence in the field.
2. The AEC program will continue to be motivated by interest in eventually achieving controlled thermonuclear power. It is recognized, however, that there are many other benefits which will accrue from an investigation of this vast and largely-unexplored field. As a result, the effort will emphasize not only detailed understanding of the physics of high temperature plasmas and the means for confining and heating them, but also studies of a basic nature in the broader aspects of the science and technology of plasmas.
3. The overall responsibility for the development of controlled thermonuclear power in the United States clearly falls under the jurisdiction of the Atomic Energy Commission. Since, however, other government agencies have interests in specific aspects of the field, their collaboration in the support of the program is both appropriate and welcomed.

Specific Steps to be Taken

Concerning the AEC's Program as a Whole

The determination to maintain a high degree of competence in the controlled thermonuclear program will require a significant strengthening of the AEC's overall effort in this field. As now envisaged, the major requirements are the following:

- a. *Scientific-technical*: An intensification of the experimental and theoretical effort, both at the major CTR laboratories and at universities. A number of large new experimental devices are now urgently needed within the program in order to test recent concepts for improved plasma confinement. The plans for this work, as well as for other supporting research on the behavior of plasmas, are outlined in some detail in the sections below. These developments will require a concomitant growth in the scientific and technical manpower engaged in the effort.
- b. *Administrative*: Greatly increased coordination and cooperation within the CTR program as a whole. The cost of carrying out all of the plans indicated as desirable elements of the program would far exceed the funds reasonably expected to be available.¹

Careful consideration must be given to the choice of new projects to be supported, and to the continued effectiveness of those in existence. Both to obtain guidance on these and other matters and to ensure a close cooperative effort within the overall program, the Division of Research is establishing a CTR Standing Committee (and supporting ad hoc panels) composed of members from each of the major laboratories and from the scientific community as a whole. The structure and responsibility of these groups are outlined in “Appendix 4”.

- c. *Financial*: The operating funds required for this program consist of two parts:
 1. Funds needed for the fabrication of large new experimental devices designed to test the latest theories of plasma confinement. Such devices are essential to further progress in controlled fusion research. When and as required, each will be justified on a case-by-case basis. The total cost of fabrication is expected to be 3–4 million dollars annually.
 2. Funds needed for normal operation of the CTR effort. In addition to financing the on-going research program, these funds must be sufficient to permit (1) an intensive experimental effort using the above new devices, (2) the influx of badly-needed new scientists into the program, both at CTR laboratories and particularly at universities, and (3) the financing of small-scale experiments associated with this personnel increase. In partial compensation for the cost of an intensified effort with large new devices, activities

¹ As of April 1966, estimates of additional operating funds needed by the CTR laboratories solely for fabrication of major new experiments during FY 1968, FY 1969, and FY 1970 total almost \$6 million per year. Examples of such devices which are in an advanced state of planning are: Scyllac (LASL), Alice II (LRL), improved Astron Injector (LRL), Toroidal Multipole (Princeton), and Superconducting Levitron (LRL).

with some of the earlier experimental equipment will correspondingly decline in value and can eventually be cut back or eliminated. The CTR Standing Committee will work with the Division of Research in helping to identify such activities.

The accomplishment of the above-mentioned goals will require a net increase of about 15% per year in normal operating funds over the next 5 years. This is in addition to the cost for fabricating major new devices discussed in (a) above.

In connection with paragraph (a) above, a major intensification of effort is clearly indicated in each of the following broad areas of research:

- a. The development of experiments which are specifically designed to isolate and study a single type of instability of importance to the CTR program.
- b. Investigations of the confinement of plasmas in absolute minimum-B systems.
- c. A study of the effect of controlling particle energy distributions on the development of plasma instabilities.
- d. The study of toroidal systems possessing average minimum-B configurations.
- e. A study of the equilibrium and stability of high beta plasmas in toroidal devices.
- f. The application of digital and/or analog computers to the prediction of plasma behavior, under conditions as realistic as possible, either through simulation or through solution of the plasma equations.
- g. The study of non-linear theory.

In addition, programs involving participation by graduate students and visiting scientists will be expanded at the AEC sites, as appropriate.

The specific plans for programs at each of the CTR laboratories and off-site projects are outlined below. The manner and priority in which these plans will be carried out will, of course, be greatly influenced by the views of the CTR Standing Committee.

At the Princeton Plasma Physics Laboratory

The action planned by the Princeton Plasma Physics Laboratory includes:

- a. Concentration of the research effort toward understanding the reasons for anomalous diffusion from stellarators and other toroidal devices. In this connection, particular attention will be given to:
 1. The problem of modifying the present configuration of the stellarator into one possessing an average minimum-B and/or a very high shear.
 2. A comparison of the various proposed average minimum-B geometries on the basis of existing

(though incomplete) theory, while at the same time working toward a more adequate theory.

3. The design and eventual construction of an average minimum-B device which appears best according to the above theoretical analyses.
 4. Investigation of the possibilities of establishing the minimum-B and shear stability requirements of a torus by performing suitably designed experiments in simpler linear geometry.
- b. Intensification of the effort on the development and/or improvement of methods for heating plasmas confined in closed systems. Such-methods include not only ion cyclotron resonance heating and magnetic pumping, but also less conventional candidates (e.g., energetic plasma injection via neutral beams or plasma guns, hybrid schemes of turbulent heating).
 - c. Expansion of the already existing program of basic plasma studies.

At the Oak Ridge National Laboratory

The action planned by the Oak Ridge National Laboratory includes:

- a. Intensification of the experiments toward the accumulation of hot, dense plasmas by high-energy injection, with emphasis on the steady state. This is expected to involve stability studies related to theory in appropriate ways, the development of intense particle beams, the development of suitable magnetic configurations and trapping methods, and pertinent studies of the basic atomic physics upon which the various trapping processes are founded.
- b. An intensification of the theoretical effort, with emphasis on relation to experiments.
- c. The further development of a strong program in basic plasma physics.
- d. A continuation of the studies of turbulent heating and of the properties of the electron-cyclotron plasmas.
- e. A continuation of the development of technical and engineering programs related to controlled fusion, including magnetic field engineering, vacuum technology, and the evaluation of problems arising from the interaction of hot plasmas with their material environment.
- f. The continuing development of the instruments of plasma physics, directed to the ready and definite diagnosis of the complexities of plasma behavior.
- g. The continued search for novel ways of heating and confining dense plasmas.

At the Los Alamos Scientific Laboratory

The action planned by the Los Alamos Scientific Laboratory includes:

- a. Intensification of work on high beta plasma. In this connection, particular attention will be given to the rapid strengthening of the theoretical and engineering support.
- b. Continuation and completion of the feasibility study of a toroidal theta pinch (SCYLLAC). If the project is found feasible, appropriate steps will be taken toward the design and eventual construction of this facility.
- c. Continuation of work on hydromagnetic guns, in anticipation of achieving about a factor of 1,000 in intensity. Continuation of studies of injection into transverse and parallel magnetic fields.
- d. Continuation of work on the dense plasma focus with the intent of finding the underlying principles and of maximizing the product nt.
- e. The establishment and then rapid strengthening of an effort in basic plasma physics.

At the Lawrence Radiation Laboratory

The action planned by the Lawrence Radiation Laboratory includes:

- a. Intensification of confinement and stability studies of plasmas produced by the injection into minimum-B systems of energetic neutral hydrogen atom beams, spread in energy,
- b. Emphasis on research aimed at establishing correlations between theory and experiment in toroidal systems with particular emphasis upon the effectiveness of field configurations combining minimum-average-B with shear in avoiding plasma losses due to instabilities.
- c. Continuation at a vigorous pace of the experimental and theoretical investigation of the Astron concept. Emphasis will be placed on studying the stability characteristics of the E-layer. Early attention will be given to the construction of the proposed new Astron accelerator which is designed to give a significantly higher electron output than is now available.
- d. Extension of the theory of wave-particle instabilities as applied to open-ended and closed confinement systems.
- e. Expansion of studies in basic plasma physics and atomic processes basic to the CTR effort.

Regarding Work at Non-AEC Sites

The action planned by the AEC includes:

- a. Continuation of friendly and active contact between its program and allied efforts outside its immediate cognizance.
- b. Steps to ensure minimum duplication of effort.
- c. Steps to encourage exchange of information between AEC and non-AEC scientists.

Regarding AEC-Supported Work at Universities

The action planned by the AEC includes:

- a. A significant expansion of the AEC-supported effort at universities. Specifically, it is planned to expand the support of these activities at a more rapid rate than that of the national laboratories. The university support will extend to basic plasma studies, recognizing that a thorough understanding of the field will be required for achievement of the ultimate objective.

Regarding the National Center

The Review Panel strongly recommended the establishment of a National Center for Plasma Studies and Controlled Fusion Research. Among the more important benefits which could be expected to accrue from the existence of such a center are: (1) that important cross-fertilization of ideas and techniques would result from having a sizeable number of experiments located at the same site, (2) that the possibility of broad and unrestricted exchange of scientific personnel on a national and international scale would serve as a major stimulus to the program as a whole and would help greatly to maintain a position of leadership in the field, and (3) that when really large-scale containment devices are deemed justifiable, the center would be in a good position to move ahead rapidly toward their construction, drawing on the broad technical competence it would have developed through smaller experiments.

While these are all valid reasons for establishing a National Center, it is clear that many of the above benefits can be achieved more simply—and far less expensively—by a strong cooperative effort within the existing program itself. Steps are now being taken to establish a closely integrated program through the medium of a CTR Standing Committee. Among other things, this Committee will actively encourage both a cross-fertilization of ideas and a broad exchange of personnel among the present sites. (Efforts will also be made to free all CTR sites from security restrictions, so that access to these areas is readily available to scientists both from this country and abroad.)

The development of a strong coordinated program will thus remove much of the urgency for establishing a National Center in the CTR field. It is well recognized, however, that such a center could well serve a useful purpose at some future date, when and as really large-scale containment devices are deemed justifiable. With this in mind, the Standing Committee will eventually be expected to consider such questions as: (1) the way in which a National Center could best be established with the maximum possible benefit to the program as a whole, (2) the relative advantages and disadvantages of establishing it at one of the existing CTR sites (versus a wholly new location), (3) the sort of time scale which would seem most reasonable for its development, and (4) the impact of such a center (once established) on the existing CTR laboratories.

Acknowledgments Dr. Samuel King Allison led this panel during the most important period of its work, when information was being gathered. The final information gathering trip was to Culham England to attend the Second International Conference on Plasma Physics and Controlled Thermonuclear Research. During this conference Dr. Allison suddenly became ill. Eight days later he died. Members of the panel became deeply impressed with Dr. Allison during their service under his leadership. To his understanding, his wisdom and his tireless efforts much credit must go for the completion of the assignment. The Panel Members are Peter L. Auer, Department of Defense, Gordon S. Brown, Massachusetts Institute of Technology, Solomon J. Buchsbaum, Bell Telephone Laboratories, Raymond G. Herb, University of Wisconsin, David D. Jacobus, Harvard University, Thomas H. Johnson, The Raytheon Company, Eugene N. Parker, University of Chicago.

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Appendix 1

The 1962 Report of the General Advisory Committee

Conclusions and Recommendations

In the Sherwood program there remain many unresolved technical questions that can be decisive to the feasibility of controlled fusion as a source of economic power. These relate particularly to the problems of stable confinement of hot plasmas in configurations suitable for power, to methods of initially heating the plasmas, to problems of purity, etc. However, it appears that, if these feasibility problems can be overcome, controlled fusion power could be economically competitive at least with breeder reactors. For this reason we believe that the AEC should continue a vigorous program in controlled fusion research and in basic

research in hot plasmas. However, this recommendation should be reviewed periodically since its continuing validity is dependent upon future technical developments.

The crucial problem of the Sherwood Project is the creation of a stable confined plasma, at thermonuclear densities and temperatures. To date, it is not certain that the stability question is a soluble one although there are some reasons for optimism. Although a vast morphology of instabilities has been uncovered, exhaustive theoretical work has not turned up any universal instability which would make such confinement unattainable. However, the complexity of the problem is such that this argument is not conclusive. It should be borne in mind that many heating schemes, such as ohmic heating, have been shown to be unstable both theoretically and experimentally.

To date, there has been at least one Sherwood experiment—Table Top—which has produced plasmas almost at thermonuclear conditions, which has been confined for periods which are at least very long when compared to the fast-growing instabilities which have bedeviled other experiments.

A prime objective of the Sherwood program must be further study of stable plasmas with a view to understanding and extending the regimes of stability. It seems likely that the ultimate fusion reactor will be a closed system such as the Stellarator; thus, some work on closed systems must be continued. However, the vastly greater ease of injecting into, heating and diagnosing the simple open-ended systems, mirror and cusp, make these indispensable tools for plasma study at this time. It is also important to keep in mind that many of the confinement schemes now used for stability studies would not be adaptable to use in a full-scale fusion power system, even if capable of providing stable confinement. It should be emphasized that characteristic times for instability are as much as a factor 10^8 shorter than thermonuclear times, so that, a “leaky” confinement which is not quite adequate for fusion economics may still be very suitable for stability studies. Only after the fundamental existence theorem for hot stable plasmas has been proven should primary emphasis be placed on evaluating confinement schemes relative to their potential as ultimate reactors.

In FY 1961, the Commission spent 93% of its controlled fusion research money in but four laboratories, while only 7% was spent in all other research institutions. We believe that the need for new ideas and additional knowledge is such that increased numbers of creative individuals should be involved in the AEC-supported programs. Both the California and Princeton groups emphasize they have recently found that graduate students can make contributions of particular value to Sherwood research programs. We fully support this conclusion, but we note that there are many other institutions with

well-qualified graduate students who are not reached by the AEC programs in its four principal Sherwood laboratories. It appears to us that as much as 25% of the AEC Sherwood research funds could be used to support research of well-qualified groups of scientists outside the four main Sherwood laboratories.

The entire Sherwood project suffers from insufficient cooperation among the major Sherwood laboratories, both in the planning of research and specific technical cooperation. It is important that cooperation be increased. The achievement of such cooperation may require an increased effectiveness in the AEC management, it should be attainable—and very worthwhile. For example, at the present time the Oak Ridge laboratory could probably provide valuable help to Princeton in the engineering of magnetic pumping, and Los Alamos might well provide plasma guns for the Stellarator, or at least scientists to develop such guns. Likewise, the laboratories could provide effective and constructive criticism of all proposals for major new devices.

The AEC-supported Project Sherwood program is producing important and useful results at a higher rate than before and we expect that valuable results will continue to flow from this effort. Furthermore, recent engineering studies of possible thermonuclear reactor configurations have reinforced the opinion that, if it is possible to obtain a hot confined plasma, it may be possible to produce electricity at costs of the order of those which will prevail in thermal or nuclear plants in the same time period.

A Summary of Principal Recommendations

We recommend that the AEC:

1. *Continue* to support Project Sherwood vigorously.
2. *Require* that the Princeton Stellarator Project produce a plasma in the keV temperature range with a ratio of material pressure to magnetic pressure, beta, of at least 1% within 3 years, or, if unable to do so, the Model C portion of the Princeton program should be closed out. Plasma confinement by the Stellarator principle, or a related closed geometry, appears to be necessary for a practical thermonuclear power system. It is therefore, unfortunate that the Princeton organization has not been adequately effective in dealing with the major engineering and construction problems of the Stellarator project. Men experienced in constructing major hardware (examples are Livingston, Jacobus or Green) should be brought into the Princeton project and given a reasonable time either to get the Model C Stellarator into satisfactory operation or demonstrate that it cannot be done.
3. Redistribute the effort at Oak Ridge by (1) shutting down the DCX-1 facility as soon as feasible (2)

emphasizing the DCX-2 and increasing the strength of experimental and theoretical physics effort on the Sherwood program.

4. Determine that Astron is not part of the AEC Sherwood program. Transfer responsibility for the Astron accelerator to ARPA for DOD-supported research programs.
5. Discontinue construction of the Toy Top 2X experiment until such time as there is clear-cut justification from Toy Top and Table Top experiments currently planned (especially in regard to stability) for going to such a large scale in the magnetic compression and transfer work.
6. Increase moderately the size of the LASL program provided this can be done without increasing the size of the Laboratory or decreasing the weapons effort.
7. Expand support of high-quality plasma research at universities. This could desirably constitute as much as 25% of the total Sherwood budget.
8. Support high-quality investigations of the systems engineering, chemical and other non-plasma development problems of full-scale fusion power plants. Systematic investigation of these non-plasma problems is an essential element of a complete fusion power development program. This effort could desirably constitute as much as 5% of the total Sherwood budget.

Explore means for making the AEC management of the Sherwood program more effective, in view of the changed character and emphasis of the program.

Appendix 2

Report of Review Panel on Controlled Thermonuclear Research

December, 1965

The Controlled Thermonuclear Research Review Panel acknowledges its deep indebtedness to its first Chairman, Dr. Samuel K. Allison. Under his guidance, the panel made visits of two to three days duration to Princeton, Oak Ridge, Livermore and Berkeley, and the Los Alamos sites. At each laboratory, scientists made presentations to the panel. Before each visit, extensive background reports prepared by project scientists were studied by members of the panel.

Presentations were also heard from members of the thermonuclear research groups of the General Electric Company, General Atomics, the Naval Research Laboratory and the Aerojet-General Corporation. Subgroups of the panel visited the laboratories at MIT, at the Stevens

Institute of Technology, and at the University of Wisconsin. Presentations were heard from the New York University theoretical group, from the Stanford University research group, and from several scientists not now formally associated with CTR. Three members of the panel,

Drs. Samuel K. Allison, S. J. Buchsbaum, and T. H. Johnson attended the Second International Conference on Plasma Physics and Controlled Thermonuclear Research at Culham, England, September 6–10, 1965.

As a result of this study, the panel has arrived unanimously at a number of findings and recommendations which follow.

Program Justification

(la) We find that:

The portion of the 1954 directive (AEC 532. 15, January 15, 1954) stating, “Controlled thermonuclear reactions research will be intensified and directed toward the development of a controlled thermonuclear reactor which will provide a source of useful energy for power... has served and can continue to serve for justification of the AEC thermonuclear program and as a useful guide for program planning. The portion of this directive which continues, “... as well as a source of neutrons potentially competitive with fission reactors in producing fissionable materials. “has become partially outmoded at this time.

The nation must know what possibilities lie in the thermonuclear domain. Future generations will obtain their power from uranium and thorium mined from rock or from heavy hydrogen extracted from the sea. At this stage both routes must be followed. Compared with the more advanced fission reactors, fusion reactors have potential advantages: their waste is not radioactive, their fuel is more readily obtained, and their product is less easily diverted to weapons.

The vision of limitless power for generations to come has stirred the imagination of people everywhere. This vision is a spur to scientists in this program throughout the world. All major industrial nations are exploring CTR with vigorous programs. We must participate actively in this effort in order that we may develop the technical competence required to exploit forthcoming results. Plasma may be viewed as a fourth state of matter. The sun is composed of it, the earth and the other planets of the solar system are bathed in it; plasma fills the vast regions of interstellar space. Its properties have received little attention until now. Its bearing on other fields of knowledge and the

future activities of mankind may be profound. We must not remain ignorant of its properties.

(1b) We recommend that:

The Atomic Energy Commission continue its policy of support for Controlled Thermonuclear Research on a scale that will assure continued U.S. leadership and that it direct its efforts in this field broadly towards a thorough understanding of high temperature, high density plasma, the means for confining and heating, it and towards the eventual demonstration of the feasibility of controlled thermonuclear fusion reactions for the generation of electric power.

(2a) We find that:

AEC fiscal documents in which the CTR program is presented as five approaches to the generation of thermonuclear power do not properly represent the present course of the program. Under this program a broad attack is being waged, aimed at determining under laboratory controlled conditions the feasibility of power generation by fusion reactions. This requires as a first step the production and containment of dense hot plasmas. The attainment of this first step is the immediate goal of the program.

Because of its complex behavior, plasma must be studied under a wide variety of conditions. Many of its salient properties emerge only when it is contained in large volumes offered by large machines. Other important experiments are conveniently conducted in smaller size devices. In the opinion of the panel the present structure of CTR research in this country is well proportioned. Considering the complexity of plasma as a fluid and the variety of techniques used in the studies, there is little duplication in the programs of our own laboratories or in those of other countries, although there is enough desirable overlap to permit profitable comparison between results obtained by afferent groups.

(2b) We recommend that:

The AEC fiscal and program descriptive documents be revised to reflect the point of view that several diverse laboratory programs important to the science and technology of plasma are being pursued within its CTR effort. In order to distinguish the programs, it may choose to describe them as confinement schemes using either open-ended (i.e. mirror) or closed (i.e. toroidal) configurations, as long time (i.e. steady-state) or short time (i.e. pulsed) confinement, as tenuous (i.e. low beta) or dense (i.e. high beta) plasma, and as supporting studies involving a large

host of topics such as injection with energetic neutral beams or plasma guns, turbulent interactions with beams, currents and collisionless shocks, instability studies, plasma diagnostics, and the physics of surfaces involved in plasma reflux,... to mention but a few.

CTR Research at the Four Major Laboratories

(3a) We find that:

A wide variety of promising experiments are being pursued at the four major AEC supported laboratories. Some of these are of quite recent origin while others have evolved naturally from lines of attack adopted early in the history of the program. Certain approaches have made the full round and have been discontinued. For example, the linear pinches, the toroidal Z-pinches:, and their daughters, the stabilized pinches and hard core pinches, have all served their purpose well and retired from the field. It was most gratifying to find that great strides have been made over the past 4 years in reconciling our theoretical understanding of plasma with its actual behavior in the laboratory in a number of experiments. We find that CTR is rapidly moving from an empirical art into a quantitative science.

(3b) We recommend that:

The AEC continue to rely for the immediate future upon the four major laboratories for the bulk of CTR effort, that it support energetically a number of current experiments and novel excursions which we set forth, that it be prepared to augment this effort in ways we describe later, and that it exercise courageous management in terminating and redirecting approaches which reach the point of diminishing return.

(4a) We find at the Princeton Plasma Physics Laboratory:

An effort devoted to the production and confinement of hot plasma in toroidal (i. e. closed) systems of the Stellarator type. Measurements of basic plasma properties in such devices have been extremely thorough and extensive. Diagnostic instruments and techniques of great value such as the ion cyclotron resonance heating method and the divertor have been developed. Theoretical understanding of plasma physics has been advanced to a high level. Ingenious experiments in basic plasma physics have been performed in the Q-machines.

The model C-Stellarator still represents the most valuable test bed in the world today for studying toroidal confinement. It will soon have serious

competition, however, since the interest around the world in toroidal machines is increasing rapidly. The loss of plasma from this machine remains anomalously high and the underlying causes are not yet understood. Nevertheless, the series of studies just completed on how various heating methods affect the plasma loss rate have been invaluable in advancing our knowledge regarding the difficulties of toroidal confinement. We find the model C-Stellarator well engineered and efficiently utilized but somewhat inflexible.

(4b) We recommend that

The present series of experiments directed toward improved ion cyclotron heating in the model C-Stellarator be carried out rapidly and that the Model C then be modified to provide the minimum average B property.² This modification is in line with current theories regarding the beneficial effects on plasma stability of minimum average B. PPPL should continue the development of apparatus for magnetic pumping for its ultimate utilization on the modified C-Stellarator.

The recommended modification of the C-Stellarator should be regarded as only a first step toward a new generation of toroidal experiments which are presently evolving as a result of progress in our understanding of plasma stability in toroidal geometries. The AEC should not hesitate to support more than one major excursion in the study of toroidal confinement since the potential rewards are great indeed. Not only is it of interest to investigate a variety of postulated stable schemes, but it is important to have at hand more than one way of preparing the high temperature plasma which is destined for long term confinement in the toroidal bottle. Such candidates as energetic plasma injection via neutral beams or plasma guns, and hybrid schemes of turbulent heating should receive careful consideration in addition to more conventional methods of in situ heating.

Support of research in basic plasma physics and theoretical studies should be continued at a generous level. The close contact which exists between PPPL and the teaching staff of one or two departments at the university is welcome but is too sparse. The

interaction with the university should be enlarged to embrace a wider spectrum of the physical and engineering sciences.

Freedom from security restrictions at Princeton has been of great benefit not only to the Princeton program but to the overall CTR program. The ability to host U.S. and foreign scientists has filled a necessary requirement for the exchange of information with the rest of the world. More extensive exchange visits should be sponsored.

(5a) We find at the Oak Ridge National Laboratory:

An effort devoted to a variety of aspects of plasma research.

In experiments directed at build up of hot plasma via energetic charge particle injection into mirror machines, ORNL has been the leader. However, the complexity which arises from the large particle orbits in the DCX-2 machine make interpretation difficult, although considerable success has been achieved in understanding the plasma in the less ambitious DCX-1 machine. The beam-plasma interaction studies and hot electron plasma production via electron cyclotron heating have pointed the way in this type of experiment and have yielded results of considerable value. The measurement of basic cross sections has been invaluable.

In the past, ORNL, appears to have stressed the empirical approach to experimental plasma research. This method of attack may soon become unprofitable.

(5b) We recommend that:

ORNL emphasize research and development of intense ion sources and techniques for injection and energetic plasma production. The effort should stress neutral beam injection embodying advanced innovations leading to spreads in energy and momentum as dictated by current and improving theories.

Beam-plasma interactions leading to turbulent heating and burnout, and electron cyclotron heating of plasmas should be continued vigorously. Renewed effort toward increasing the theoretical staff and coupling it closely to the experimental program should be undertaken.

(6a) We find at the Los Alamos Scientific Laboratory that:

There is a unique involvement with pulsed dense high beta³ plasma experiments. The early work on straight pinches has been discontinued and the series

² “Minimum B” refers to a configuration in which the absolute value of the magnetic field is nonzero and increases in all directions from some point. A plasma in such a magnetic well possesses exceptional hydromagnetic stability. In a closed, that is, toroidal system, a “minimum-B” well is topologically impossible in vacuum. The next best thing is to ensure that a particle experience on the average a “minimum-B” configuration.

³ Beta is defined as the ratio of plasma pressure to magnetic field pressure. In low beta plasma the magnetic field which permeates the plasma is nearly the same as that in the absence of the plasma. In a high beta plasma the magnetic field inside the plasma is nearly zero.

of experiments on open-ended fast theta pinches has culminated in the highly impressive Scylla-IV. A number of ingenious diagnostic instruments have been incorporated with this experiment and many interesting results have been recorded. Valuable plasma gun developments have been made. The plasma focus device which produces intense thermonuclear reactions in a small volume is spectacular in its performance. Although many of the large scale experiments we viewed were well engineered, the impression remained that the engineering support was inadequate for the type of program advocated. There appeared to be a considerable imbalance between theory and experiment. More intense theoretical effort is needed in the difficult regime of dense high p plasmas.

(6b) We recommend that:

The effort on high beta plasmas be supported strongly and that rapid strengthening of the theoretical and engineering support be undertaken. Careful and prompt attention should be given to the building of a toroidal theta pinch (SCYLLAC). If the project is judged feasible, the design and fabrication of this facility should have high priority. However, in the opinion of the panel the success of this venture rests heavily on the presence of an adequate engineering staff and theoretical support which is currently lacking at LASL.

(7a) We find at the Lawrence Radiation Laboratory that: Three major lines of attack are making rapid progress and providing encouraging results. These are the minimum B geometry mirror machine, Alice, employing neutral injection; the E-layer experiment, Astron; and the toroidal experiment, the Levitron. The work on Alice, along with supporting experiments in more modest mirror machines has played a leading role in reconciling stability theories with experiment and providing the scientific community with a large measure of hope for the ultimate understanding and exploitation of plasmas. The performance of Astron has been impressive to date. It is an ambitious engineering undertaking and has demonstrated that its E-layer⁴ is stable under conditions of modest field reversal. Astron has no counterpart; it is the only unique method being pursued by this country. The series of experiments recently completed in the Levitron

have clarified a number of significant points on the behavior of plasmas in toroidal configurations and pointed to several approaches which may ultimately circumvent the instabilities thwarting our efforts.

The large mirror experiment called 2X appears to be suffering from a succession of difficulties and its future prospects do not appear encouraging.

The supporting studies in basic aspects of plasma physics and theory at both the Livermore and Berkeley sites are of high quality.

(7b) We recommend that:

The Alice experiment utilizing neutral injection into a minimum B mirror machine be pushed vigorously toward the large-diameter short-length geometry favored by current understanding. Simultaneously a concerted effort should be made to develop and use improved injection sources having the required spread in energy and momentum as indicated by current theories. The minimum-B geometry produced by the baseball-seam coil is most interesting and this line of attack should be pursued energetically. Superconducting and/or cryogenically cooled coils should be developed and used to conserve power when such use is judged feasible.

The minimum average B studies in toroidal geometry employing the Levitron and its modification should be pursued rapidly in order to lay the groundwork for the next generation of stable toroidal confinement experiments. Proposals for new major facilities resulting from this program should receive early and careful attention. Coordination with Princeton proposals on toroidal experiments will be required to assure most effective utilization of all facilities.

The Astron experiment should be continued at a vigorous pace with careful attention given to significant bench marks lest the program be allowed either to drag or to outreach itself. Early and careful attention should be given to funding the proposed improved 4 MeV accelerator. Construction of this facility should proceed quickly once it is clear that E-layer stability can be maintained under conditions set forth in the present program. It is important to explore the full regime of E-layer stability and the ultimate potential of this approach to the CTR problem.

The theoretical investigations in direct support of Alice and the Levitron should be maintained at their present high level. The theoretical support of Astron should be increased. The link maintained between the Livermore and Berkeley sites is commendable in that it provides a valuable contact between the CTR

⁴ The term E-layer refers to very energetic (relativistic) electrons which form a cylindrical shell. The role of this shell is (1) to help produce a magnetic field which can confine the plasmas and (2) to heat the plasma.

professional staff and the teaching staff at the University of California, Berkeley. Every effort should be made to strengthen this link. The University should be encouraged to make more extensive use of the Berkeley facilities in their teaching and preparation of students in plasma physics. Similar recommendations apply to the ties between Livermore and the Davis campus.

CTR Research at Non-AEC Laboratories

(8a) We find that:

The programs at the Naval Research Laboratory, at General Atomic, and at General Electric are all of high professional quality. The program at Aerojet-General Nucleonics is still in its infancy but appears to be well conceived and offers promise. The interaction between these programs and the more extensive AEC supported effort has been valuable and is to be commended. In particular, the theta pinch programs at NRL and GE have had significant influence on the Los Alamos effort. The development of novel neutral injection sources at AGN is being followed closely by workers at Livermore. The theoretical effort at GA has played an important role in CTR both nationally and internationally.

(8b) We recommend that:

The AEC continue to maintain friendly and active contact between its program and allied efforts outside its immediate cognizance. These provide valuable additions to the total effort and contribute to the overall health of CTR research. However, the AEC can not afford to rely on outside sources to carry the burden of research in this difficult and time consuming field. The AEC must strive to set the pace and capitalize on the good fortune of having active collaborators.

U. S. and World Programs

(9a) We find that:

World-wide progress toward the understanding of plasmas is now rapid. The problem of heating plasmas is now well on the way to being solved. Considerable progress has been made toward solving the much more difficult problem of plasma confinement. Here the stumbling block is the variety of instabilities peculiar to the plasma state. Many dangerous ones have already been conquered in open-ended systems. The list of remaining instabilities is long and still growing, but at a much diminished rate. Prospects are excellent that with

time and sufficient attention these will be conquered as well. Our optimism rests on the fact that our ability to study plasmas has greatly improved, great strides have been made in the theoretical understanding of plasmas, and much progress is evident in the quantitative correspondence between theoretical predictions and experimental observations. In this the rest of the world has contributed as effectively as this nation.

We estimate the U. S. participation in CTR research at one-fifth of the total world effort in terms of weighted expenditures and personnel. We further estimate that the effective contribution of this nation to the continuing progress is approximately one-third of the total. This is to be contrasted with the situation as it appeared 4 years ago when the U. S. effort represented nearly one half and its contribution amounted to well over one-half the total. We conclude that the U. S. proportion of the total contribution is declining rapidly.

We note that during this 4 year period the AEC budget for CTR research has been essentially static and largely inflexible. This has resulted in a severe curtailment of the normal influx of new people with fresh ideas and new approaches. In this respect the panel notes the preponderance of youthful U.S.S.R. delegates at the recent international conference on CTR.

Another result of recent fiscal policies has been the lack in both speed and flexibility of adapting or acquiring equipment to test new ideas originated here and abroad. There has been a tendency to continue studies with equipment that is becoming outdated in an attempt to obtain maximum return from prior investment.

By contrast, the efforts in Western Germany, France, the U. K. and U.S.S.R. have expanded rapidly during this intervening period with new devices, new facilities, and vigorous youthful staff. During the same period intensive programs were created in Japan, Italy, and a number of other countries.

(9b) We recommend that:

This country continue to play a leading role in controlled thermonuclear research. It can not afford to become second to any other nation in a field of such vital significance. By sharing in the world-wide results, the U.S. has advanced far beyond the stage it would have reached with only its own resources. In order to share and exploit future results, the U.S. program must continue strong and viable.

To this purpose the AEC should adopt and promote a fiscal policy which will implement the recommendations of this report and will attract talented

young scientists and engineers into the CTR program under AEC auspices. In our opinion this will require a doubling of scientists and engineers engaged in CTR under AEC auspices in a period of approximately 5 years.

University Based CTR Programs

(10a) We find that:

Work at the major AEC supported university plasma laboratories is yielding excellent returns in plasma physics and plasma diagnostic techniques for funds expended. The programs at Berkeley, at MIT, at Wisconsin and at New York University which were viewed by panel members appeared to be especially fruitful, with excellence in output of research results and of gifted plasma scientists and engineers. Work at Stanford and at the Stevens Institute Technology is contributing to our understanding of plasma physics.

(10b) We recommend that:

The AEC recognize the critical role of universities in the training of plasma scientists and engineers needed to invigorate the CTR program in the nation. By increase of support to existing centers of good productivity and promise and by careful selection of new university centers, the AEC should promote expansion of these activities at a more rapid rate than in the programs of the major laboratories. This policy will help to provide the youthful talent needed at the major laboratories. AEC support at universities should extend to basic plasma studies, recognizing that thorough understanding will be required for achievement of the ultimate objective, and recognizing that students broadly trained in fundamentals are required for long time productivity in a rapidly developing field. The AEC should recognize that continuity of support and long time support are required for successful university programs.

Future CTR Program Requirements

(11a) We find that:

The CTR program in this nation is declining in stature relative to that of the rest of the world. It will deteriorate rapidly to a secondary role if the present static budget of the AEC is continued. Only bold and imaginative measures will reverse this trend.

During the early period of CTR investigations this country held a commanding position. In the

development of new machines, in the development of diagnostic equipment and refined measurement techniques, in the use of this equipment and these techniques for careful measurements of plasma properties this country made the major contributions. Largely because of our work understanding of plasma properties is immensely superior to the situation 10 years ago. Much remains to be done but the groundwork has been laid and we believe it has been laid solidly.

Other nations are now coming rapidly into the field with vigorous young talented workers. The Soviet and British efforts which began at the same times as ours are now fresh and rapidly evolving programs as a result of expansion and re-direction. Unless we take bold action we shall fare poorly in the competition. After carrying through with the difficult groundwork and making major contributions to the foundation we shall be in a relatively poor position to reap rewards as they come.

(11b) We recommend that:

The AEC take immediate steps toward establishing a national center for plasma studies and nuclear fusion research. This facility is to extend and stimulate the program in this country. It should serve as a major center for national and international exchange in the effort leading toward the development of controlled thermonuclear reactors.

The national center should have an identity of its own. It must be free of all security restrictions in order to engage in cooperative ventures with other nations conducting CTR programs. It should have close links to other CTR laboratories maintained by frequent visits and by exchange of personnel for periods of one to 2 years. The AEC is encouraged to promote exchange programs both on a wide domestic basis as well as internationally.

The national center should have a program broadly based in the fundamental aspects of plasma physics. It should have close ties to one or more universities and should play an important role in the teaching and preparation of students for careers in CTR.

The program of the national center must aim for competence in a variety of containment devices. As the CTR program evolves, the need for new, larger volume containment devices is expected. The national center should play a principal role in implementing plans for large containment devices. Recruitment of personnel should be started quickly on a scale sufficient to invigorate present AEC programs and to provide an experienced cadre upon which the national center can draw.

Appendix 3

Comments of the GAC and PSAC on this Policy and Action Paper

Comments of GAC

Dr. Bishop gave the Committee an excellent summary of the status of controlled thermonuclear research, the recommendations of the Herb Panel and the AEC staff's proposals for policy and action in this field.

The Committee is pleased to note that both the Herb Panel and the AEC staff paper stress the need for better coordination of CTR at the project laboratories. The advisory committee which Dr. Bishop plans to appoint, with membership made up of CTR Project Directors from the four laboratories supplemented by four well-qualified and highly respected experts from universities and industry, will be an effective vehicle for providing the required coordination. We are confident that, under Dr. Bishop's direction, coordination and cooperation among the laboratories will be improved, and that he will exercise dynamic leadership in obtaining the support of the members of this advisory committee in the difficult decisions he will have to make.

We consider that the Commission has been very fortunate to obtain Dr. Bishop's services as Director of the Controlled Thermonuclear Research Program, and we urge the Commission to give Dr. Bishop the fullest possible support in carrying out this program.

We endorse the recommendations of both the Herb Panel and the AEC staff paper that the AEC "expand the support of these (plasma research activities at universities) at a more rapid rate than that of the National Laboratories." We believe that the AEC has not made use of Universities to a sufficient extent in conducting the more fundamental research needed in this program or in training the next generation of investigators who will be needed to contribute new ideas to it.

We concur in the assessment of the Herb Panel and the AEC that this is an important field and one in which the United States should play a leading role. We are not prepared to recommend, however, that the U.S. should strive necessarily to outstrip all other nations in this field.

We are glad to note that both the Herb Panel and the AEC staff paper recognize the need for "studies of a basic nature in the broader aspects of the science and technology of plasmas."

However, the policy statement of the AEC staff paper to the effect that the U.S. program in this field will continue to be directed primarily toward the eventual achievement of controlled thermonuclear power seems too narrow. We recommend that the policy statement be rephrased to read that the U.S. program in this field will continue to be

motivated by interest in eventually achieving controlled thermonuclear power and that the program will emphasize studies of a basic nature in the broader aspects of the science and technology of plasma. Progress in this field in the past has been delayed by attempting to push too rapidly toward the goal of thermonuclear power without taking the time to develop the requisite knowledge of plasmas. Moreover, other valuable applications besides thermonuclear power can be anticipated from a substantial plasma research program. The increasingly favorable prospects for fast breeders also make thermonuclear power a less urgent objective.

The Committee concurs with the recommendations of the AEC staff paper that the number of scientists and engineers in the program be augmented and that the work underway both in existing laboratories and in off-site installations be intensified. We agree also that major new experiments need to be undertaken. This might be done at the rate of one every year or so. To do all this will require a substantial increase in the AEC's budget. At the same time, we do not believe that the urgency of this work is so great as to justify a total increase in the budget greater than the 15% per year for 5 years recommended by the AEC staff paper. With such a control on funds, it will not be possible to do everything which every interested group would favor. To obtain the maximum benefit under these conditions, Dr. Bishop and the AEC must be highly selective in the choice of projects to be supported and should be prepared to eliminate activities which are of declining value so as to release funds for the new facilities, projects and personnel which are needed to provide vitality for the program.

Before deciding to establish a new National Center for Plasma Studies and Controlled Fusion, Dr. Bishop should be given ample opportunity to reorient, coordinate and streamline the present plasma efforts of the project laboratories and other participants in the AEC's program. These groups, working together, should be able to achieve satisfactory progress without the radical and expensive measure of establishing a new National Center.

Appendix 4

Functions of the CTR Standing Committee

Background

- A. The AEC's Controlled Thermonuclear Research Program consists of the activities of a number of scientific and technical groups whose work is (to varying degrees) mutually interdependent. In view of the limited amount of total funding available, it is essential that this interdependence be recognized and

that coordination and cooperation between personnel in the program be sufficient to insure that what is eventually pursued has been duly considered by others in the program with related interests.

- B. To assist in getting the desired cooperation, it is planned to set up coordinating activities on two levels within the overall program:
1. A CTR Standing Committee, consisting of the Assistant Director (for Controlled Thermonuclear Research) of the AEC's Division of Research, the Laboratory Project Directors and several prominent scientists, to ensure a close cooperative effort within the overall program and to provide guidance on major policy decisions.
 2. Ad Hoc Working Panels, consisting of scientists assembled for a limited period of time to study specific programs for extensive research efforts in specialized areas within the CTR program. These Panels will assist the Standing Committee.

Charter of the CTR Standing Committee

- A. The CTR Standing Committee would be composed of the Assistant Director (for Controlled Thermonuclear Research), the CTR Project Directors from each of the major laboratories, and prominent U.S. scientists selected from the scientific community as a whole.
- B. The function of this committee would be to advise and guide the Controlled Thermonuclear Research Program of the AEC's Division of Research on a continuing basis by:
1. reviewing the areas in which plasma physics and controlled thermonuclear research are conducted under the authority of Section 31 of the Atomic Energy Act of 1954, as amended.
 2. assessing the overall balance among the various elements in the AEC's controlled thermonuclear research program, as viewed in the context of national and world plasma physics research.
 3. evaluating the scientific significance and productivity of elements of the program and defining the high priority and low priority elements.
 4. identifying major questions requiring immediate attention and research areas critical to the success of the current and future program.
 5. recommending how available and projected funds can most effectively be used to carry out the Commission's CTR program.

Charter of the Ad Hoc Working Panels

- A. The AEC CTR Office will from time to time, request certain groups to prepare formal documents describing their program plans, rationale and projected costs in sufficient detail to be suitable for program-wide review. To conduct this program-wide review an Ad Hoc Working Panel would be assembled, consisting of one representative from each of the four major CTR laboratories (appointed by the Project Director) and several other representatives from outside those four laboratories (appointed by the AEC Program Director).
- B. The goal of the Working Panel would be to review a specific planned program in a cooperative and constructive spirit, to ensure that the scientific and technical basis is as sound as possible. The functions of the Working Panel would include:
1. Scientific and technical analysis during which the members would request whatever assistance they required from qualified staff at their home laboratories.
 2. Consideration of similar research already performed elsewhere, to the extent that such research affects the scientific objectives under review.
 3. Attempts, via discussions, to persuade those concerned with the specific program to give due consideration to objective and constructive criticisms. Hopefully this would often result in program modifications even during the course of the review.
 4. Preparation of a report covering such items as the salient features of the critical analysis, changes which have resulted in the original plans, recommendations concerning the adequacy of the proposed program and concerning the scientific desirability of proceeding in the manner and at the rate proposed.
- C. The functions of the Working Panel would not include:
1. Making criticisms of a purely subjective nature.
 2. Making recommendations concerning the relative merits of performing research in different areas (e.g., mirror research vs. theta pinch research), except insofar as it was determined that the specific scientific goals could be achieved more logically by methods other than those proposed.



UNITED STATES
ATOMIC ENERGY COMMISSION
WASHINGTON, D.C. 20545

July 11, 1966

Dear Mr. Holifield:

The Joint Committee on Atomic Energy has requested that the Commission develop a report on its Controlled Thermonuclear Program, similar to the Policy Paper prepared on the High Energy Physics Program.

In May 1965 the Atomic Energy Commission formed a panel of leading scientists and engineers to carry out a detailed review of its program on Controlled Thermonuclear Research. The work of that panel was completed in December 1965, and a copy of its report was transmitted to the Joint Committee on Atomic Energy on January 3, 1966. In the period since the Review Panel's report, the Commission has completed a further study and has prepared the attached report entitled, "AEC Policy and Action Paper on Controlled Thermonuclear Research."

Both the Review Panel and the Commission have concluded that an intensification of effort is needed. To ensure that this is done in full consideration of other program requirements, both within the AEC and the government as a whole, the needs of the controlled fusion program were reviewed by the AEC's General Advisory Committee and the President's Science Advisory Committee. After their endorsement of the importance of the program and the need for an intensified effort, the Commission has adopted the attached report, which will serve as a planning guide for the future development of the CTR program. The Commission, of course, will review the CTR program annually with the Bureau of the Budget and the Congress to determine the appropriate level of activity which should be maintained in this important field of endeavor.

The Bureau of the Budget has reviewed this report and expresses no objection to its transmittal. However, the Bureau has pointed out that the degree of program growth is subject to annual budget review.

Cordially,

Handwritten signature of Glenn T. Seaborg in cursive script.

Chairman

Honorable Chet Holifield, Chairman
Joint Committee on Atomic Energy