REPORT OF THE REVIEW PANEL

Sixth Symposium on Current Trends in International Fusion Research Washington DC, March 7-11, 2005

Edward C. Creutz, Ronald C. Kirkpatrick, Irvin R. Lindemuth (Chairman), Richard F. Post, Hans J. Schneider-Muntau, Norman Rostoker

September 6, 2005

Executive Summary

The Symposia on Current Trends in International Fusion Research have now been observing fusion activities for a decade. In reviewing the reports of previous panels and the papers presented at the Sixth Symposium, we have found a fascinating picture of ongoing research and the prospects for fusion as an energy source. To share our impressions and recommendations, we have developed a new scheme for classification of fusion approaches. The fundamental quantity used in the new classification is the plasma density, and our discussion emphasizes that there is a large, essentially unexplored gap between the two conventional approaches, magnetic confinement fusion (MCF) and inertial confinement fusion (ICF). We review the major emphases of MCF and ICF and observe that the two approaches together have essentially eliminated all other approaches, significantly reducing the probability that fusion can become a viable energy source in the shortest possible time at the lowest possible development cost. We are concerned about the lack of open scientific discussion and objective peer review in the US fusion program. We call on the US Congress to explore all possible avenues to increase funding for fusion research. Independent of the level of funding, we call on Congress to ensure a balanced fusion program that will maximize the benefit to the taxpayers that are paying the bill for fusion. This symposium and previous symposia suggest that the only way to ensure that the funds available for fusion research are used in the most-cost effective way is for Congress to order a complete reorganization of the US fusion effort. In this report, and the panel reports that preceded it, there has been a strong sentiment that fusion research is not progressing as rapidly as deemed to be possible because of bureaucratic impediments. The reorganization of the US fusion program as suggested here would go a long way to alleviating previous problems, restoring fusion's credibility, revitalizing the national fusion program, and hastening the arrival of fusion as a source of nearly endless energy.

This report will be published in the Proceedings of the Symposium. This preprint is made available with the understanding that changes may be made prior to publication.

(this page intentionally left blank)

REPORT OF THE REVIEW PANEL
Sixth Symposium on Current Trends in International Fusion Research
Washington DC, March 7-11, 2005

Edward C. Creutz, Ronald C. Kirkpatrick, Irvin R. Lindemuth (Chairman),
Richard F. Post, Hans J. Schneider-Muntau, Norman Rostoker

Executive Summary

The Symposia on Current Trends in International Fusion Research have now been observing fusion activities for a decade. In reviewing the reports of previous panels and the papers presented at the Sixth Symposium, we have found a fascinating picture of ongoing research and the prospects for fusion as an energy source. To share our impressions and recommendations, we have developed a new scheme for classification of fusion approaches. The fundamental quantity used in the new classification is the plasma density, and our discussion emphasizes that there is a large, essentially unexplored gap between the two conventional approaches, magnetic confinement fusion (MCF) and inertial confinement fusion (ICF). We review the major emphases of MCF and ICF and observe that the two approaches together have essentially eliminated all other approaches, significantly reducing the probability that fusion can become a viable energy source in the shortest possible time at the lowest possible development cost. We are concerned about the lack of open scientific discussion and objective peer review in the US fusion program. We call on the US Congress to explore all possible avenues to increase funding for fusion research. Independent of the level of funding, we call on Congress to ensure a balanced fusion program that will maximize the benefit to the taxpayers that are paying the bill for fusion. This symposium and previous symposia suggest that the only way to ensure that the funds available for fusion research are used in the most-cost effective way is for Congress to order a complete reorganization of the US fusion effort. In this report, and the panel reports that preceded it, there has been a strong sentiment that fusion research is not progressing as rapidly as deemed to be possible because of bureaucratic impediments. The reorganization of the US fusion program as suggested here would go a long way to alleviating previous problems, restoring fusion's credibility, revitalizing the national fusion program, and hastening the arrival of fusion as a source of nearly endless energy.

Introduction

The series of Symposia on Current Trends in International Fusion Research was initiated in 1994 to identify and review the benefits, uncertainties, current status and potentialities of various approaches to fusion energy and to assess industrial spin-offs and other applications. The objective of the symposia is to review the approaches, especially alternative and exploratory approaches, to fusion-based research in terms of the following criteria: achievements; scientific and technological risk; potential advantages; steps required and issues which need to be addressed to reach scientific demonstration of feasibility; steps required and issues which need to be addressed to develop a demonstration power plant and to estimate its physical size and cost.

The International Advisory Board of the Symposia tasked the Review Panel of the Sixth Symposium on Current Trends in International Fusion Research to review and comment on:

- The quality of the scientific opportunities in fusion research
- The potential role of fusion energy in the world energy picture
- Industrial applications of plasma research
- Any measures to increase public understanding and support of fusion energy, and the other fusion related issues that the panel considers appropriate.

The panel members are honored to have been selected for this role and to join a distinguished list of scientists and engineers who have served on previous panels:

First Symposium (November 14-18, 1994)—E. C. Creutz, A. R. Kantrowitz (chairman), J. E. Lannutti, H. J. Schneider-Muntau, G. T. Seaborg, F. Seitz, W. B. Thompson;

Second Symposium (March 10-14, 1997)—S. A. Colgate, E. C. Creutz, A. R. Kantrowitz (chairman), J. E. Lannutti, H. J. Schneider-Muntau, G. T. Seaborg, F. Seitz;

Third Symposium (March 8-12, 1999)—S. A. Colgate, E. C. Creutz, A. R. Kantrowitz, H. J. Schneider-Muntau (chairman), G. T. Seaborg, F. Seitz;

Fourth Symposium (March 12-16, 2001)— S. A. Colgate, E. C. Creutz, A. R. Kantrowitz, R. F. Post (chairman), H. J. Schneider-Muntau, G. T. Seaborg, F. Seitz;

Fifth Symposium (March 24-28, 2003)—E. C. Creutz, H. J. Schneider-Muntau, R. F. Post, N. Rostoker (chairman).

So that the reader may become familiar with the present panel members and their background and qualifications, we append brief biographical sketches of the panel members at the end of this report. As with previous symposia panels, most of the present members receive little or no financial support from either the Office of Fusion Energy Sciences (OFES) of the Department of Energy (DOE) or the Office of Defense Programs (DP) of the DOE National Nuclear Security Administration (NNSA), the two US Government offices that fund by far most of the fusion research in the United States.

The Symposia on Current Trends in International Fusion Research have now been observing fusion activities for a decade. In reviewing the reports of previous panels, we have found a fascinating picture of ongoing research and the prospects for fusion as an energy source. The previous panels have noted many ebbs and flows of the fusion tide and have offered many suggestions for improvements. Because of the continued validity of many previous comments, we have incorporated many into our report. Where we quote from previous reports, we will write the comment in *italics,* and either enclose the comments in quotes and specifically cite the symposium number or we will simply follow the comment by the symposium number in parentheses, e.g.,*…text…(IV)* indicates a quote from the 4th Symposium panel report. We strongly encourage the reader to review the reports of previous panels.

Quality of Fusion Research

As with previous symposia, the 6th Symposium has demonstrated unequivocally that high-quality controlled fusion research is being conducted at many organizations throughout the world. The quality of the papers presented at the symposium was generally excellent. The papers covered a wide range of fusion experimentation, theory, and computations. The authors are to be commended for their work and for taking time from their busy schedules to participate in the symposium. In many cases, excellent work is being conducted in spite of the scarcity of funding for exploring new ideas and understanding their significance. Because the papers presented have already been summarized by session chairmen and will be published elsewhere, we will cite only those papers that we feel represent notable developments or that seem to us to be especially significant.

Although papers presented at the symposium covered a wide spectrum of fusion research, there were two notable absences: (a) the merits and status of the International Toroidal Experimental Reactor (ITER) in magnetic confinement fusion (MCF); (b) target design and the physics of the fusion fuel in inertial confinement fusion (ICF).

The Potential Role of Fusion Energy

Fusion represents one of the three possible permanent energy sources, the others being some convincingly safe development of conventional nuclear power, and some economical and practical way of using solar energy (I). Although experts disagree on when the demand for energy will exceed that which can be provided primarily by fossil fuels, all agree that the time is coming, quite likely within the next 100 years or so. When potential environmental impacts, including climactic effects of energy utilization and production, are taken into account, the time when carbonaceous fuels must be phased out comes even sooner. In the ten years since the first symposia, there has been little to improve the prospects of conventional nuclear and solar power. During the same period, there has been little to discount the prospect that the fusion process will eventually be harnessed and fusion will provide an abundant source of energy for many centuries.

We emphasize that fusion's role is a far distant, "potential" one. A fusion source that could move directly, through engineering only and with no further advances in the technology and plasma physics, to a working reactor producing useful energy, even if not economical, has not yet been demonstrated, in spite of numerous articles in the popular media that suggest otherwise. Later in this report, we will give our recommendations regarding how fusion may reach its potential.

Industrial Applications of Plasmas

Although plasmas are being used in a number of industrial processes, no industrial applications of plasmas were discussed at this symposium. The relevance of fusion to the electrical power industry is discussed later.

Classification of Fusion Schemes

The panel report from the 4th Symposium categorized fusion approaches according to their "philosophy" and noted four discernable philosophies:

- "closed field" magnetic confinement fusion (MCF) approaches
- "open field" magnetic confinement fusion (MCF) approaches
- the magnetized target fusion (MTF) approach
- the inertial confinement fusion (ICF) approach

Recognizing the very strong and fundamental differences between the MCF, MTF, and ICF philosophies, Prof. Richard Siemon of the University of Nevada/Reno has referred to these three—MCF, MTF, ICF—as three distinct, mutually orthogonal "pathways," i.e., they are more than just philosophical approaches, they are legitimate pathways, each with its own advantages and disadvantages.

We would like to take a somewhat different, but complementary, approach to categorization of concepts. The most important quantity for categorization is the ion density of the fuel at fusion temperatures. Although our divisions are somewhat arbitrary, we will call low-density (LD) any density less than $10^{17}/cm^3$, high-density as any density greater than $10^{23}/cm^3$, and intermediate density (ID) as any density between LD and HD.

A second feature for categorization of fusion concepts is magnetization of the fusion fuel, or lack thereof. In many concepts, e.g., MCF, a magnetic field within and/or surrounding the burning fusion fuel is required. In others, e.g., ICF, no magnetic field is required or desired, and, sometimes, steps must be taken to ensure that no magnetic field is present in the burning fuel. Those concepts requiring a magnetic field utilize either open magnetic (OM) or closed magnetic (CM) field topologies, whereas those that do not use a magnetic field within the fuel are unmagnetized (UM).

Finally, we consider the time-scale of the burning fusion fuel in a fusion reactor embodiment. Many approaches are predicated on "steady state" operation (SSO), where the fuel inventory and characteristics are more-or-less constant for periods of days, weeks, and longer. In contrast to SSO, some fusion concepts inherently lead to pulsed operation (PO), where fusion typically occurs in a burst lasting only a very small fraction of a second.

MCF provides the most common examples of steady-state operation (SSO). In fact, a simple dimensional analysis taking into account energy loss mechanisms and burn times shows very quickly that any steady-state concept must be a magnetic confinement concept, utilizing a magnetic field to reduce thermal losses that would otherwise make SSO impossible on earth. Essentially all MCF concepts—tokamaks, mirrors, reversed field pinches, field-reversed configurations, etc.—are predicated on SSO, and research focuses on extending operation of the concepts from milliseconds to seconds, then seconds to minutes, and ultimately steady-state. In addition, when the strength of materials used for support structures, etc., is considered, SSO operation limits the burning plasma pressure to about 1 atm, i.e., an ion density of approximately $10^{14}/cm^3$ (LD), although some advanced SSO concepts seek to increase this density limit by an order of magnitude or two, i.e., by a factor of 10-100.

ICF provides the most common example of pulsed operation (PO). With burning fuel densities > $10^{25}/cm^3$ (HD), ICF is inherently pulsed in nature, because it would be impossible to contain the burning fuel pressure, > 10^{12} atmospheres. In fact, both the intermediate (ID) and high (HD) density regimes are of necessity pulsed because of the fuel pressure. ICF is forced to operate at such high densities because of the limits on energy that can be delivered by existing drivers to the fuel in the short time scales (ns) required. At the high-densities of ICF, an extremely high magnetic field would be required to reduce thermal losses, and, since ICF is achieved by target implosion, such a strong magnetic field would be likely to interfere with the high degree of implosion symmetry required. Therefore, ICF is an HD-UM-PO concept.

Using the categories defined previously--LD/ID/HD, OM/CM/UM, SSO/PO—and with some minor ambiguities, we can group papers presented at the symposium as

- LD-OM-SSO 3
- LD-CM-SSO 9
- ID-CM-PO 5
- HD-UM-PO 11
- HD-OM-PO 1
- General theory and others 11

The distribution of papers at the conference in most ways reflects the emphasis of the US fusion program: LD-OM/CM-SSO and HD-UM-PO concepts. The Office of Fusion Energy Sciences (OFES) applies the bulk of its funding towards research and development of steady-state, magnetized concepts, i.e., LD-OM/CM-SSO concepts, most notably the tokamak, an LD-CM-SSO device. The NNSA Office of Defense Programs (DP) focuses, almost exclusively, on lasers to implode targets to achieve fusion ignition via ICF (HD-UM-PO). Each funding office has its "flagship," the International Toroidal Experimental Reactor (ITER) for OFES and the National Ignition Facility (NIF) for DP. Whereas NIF has been funded and is more than 50% complete at the time this report is written, ITER has been embroiled in international politics for more than a decade, as discussed later. *What is clear, however, is that the major-flagship-programs in both*

ICF and MCF offer chiefly massive engineering, when the required scientific basis still has important uncertainties (I), i.e., fusion engineering in both MCF and ICF continues to proceed based on current scientific understanding even though the current understanding of plasmas is full of uncertainties.

A relatively large number of papers discussed ID-CM-PO concepts that continue to attract interest in the scientific community despite very limited funding.

ICF/IFE & NIF

As with previous panels, this panel was generally impressed with the technological advancements towards ICF that were reported at the symposium. A number of remarkable engineering accomplishments, e.g., precision optical work, have been made in the National Ignition Facility (NIF) project (paper by E. Moses, "The National Ignition Facility: Exploring ICF Burning Plasmas in the Laboratory"). Nevertheless, many physics issues remain to be solved if the goal of fusion ignition via target implosion is to be achieved. These issues include the driver requirements (energy, symmetry, pulse time-dependence, optimum carrier--photons, ions, etc.) and the hohlraum and capsule specifications (structural precision, surface finish, pusher and cold fuel properties—opacity, equation-of-state, grain structure, etc., cryogenics).

A notable absence at this symposium was a discussion of target design and the physics of the fusion fuel in ICF. Because classification restrictions limit the amount of detail that can be presented in open forums, ICF suffers from a lack of wide-spread "peer review." Therefore, it is impossible for scientists not directly involved in classified DP programs to make an assessment of the likelihood that NIF will reach its stated goals. With this lack of peer review, the NNSA ICF program appears to be much more coherent than MCF. Comments made at the symposia suggest that any dissent within DP programs is quite effectively suppressed. Hence, the coherence of the program may be more a consequence of a research environment that does not encourage a diversity of scientific opinions than a consequence of the scientific results.

In this area, there is an unfortunate situation with nomenclature that confuses the layman as well as fusion scientists. The term ICF is used officially in the US Government system to refer to the NNSA programs in inertial fusion for defense, explicitly non-energy, applications. Therefore, the term Inertial Fusion Energy (IFE) was coined to identify target-based, high-fuel-density fusion research that is relevant specifically to the energy application. Confusion arises due to an overlap in the goals and experiments for ICF and IFE. Target fusion ignition, for instance, is central to the goals of both communities, whereas an inexpensive driver that can implode pellets at a reasonable repetition rate is of interest for IFE. Often, it is not quite obvious what is ICF and what is IFE, and sometimes the distinction appears variable at the convenience of NNSA.

As its name implies, NIF is first and foremost a fusion ignition facility. However, a number of impressive high-energy-density science applications of NIF have been put forward in the years since NIF was first conceived, hence justifying funding the multi-billion dollar facility through the stockpile stewardship program and managing it through the Defense Programs (DP) office. Fusion is understandably of interest to DP because of the fusion process in hydrogen bombs.

However, it is generally conceded that fusion in NIF targets differs significantly from fusion in nuclear weapons. Therefore, it becomes unclear why one specific approach—laser-driven hohlraum targets—is consistent with the DP mission but other concepts, e.g., fast-ignition, are not deemed to be suitable for funding through DP as ICF but must be relegated to seeking funding as IFE through OFES, which has significantly smaller funds available for such work.

We note that as the cost of NIF escalates and the date for completion recedes, DP has attempted to retreat from some of its stated fusion goals. However, the US Congress has repeatedly held DP to task. This appears to the panel to be an indication that Congress has supported NIF for its role in moving fusion towards a useful energy source. Therefore, it seems prudent to ask whether or not the fusion mission of NIF is best managed through DP.

As indicated previously, there are many unanswered physics questions associated with ICF and IFE. The answers to some of these questions may lead to only design trade-offs to minimize cost and optimize efficiency. Others could lead to show-stoppers, and it must be acknowledged that NIF may not live up to its name.

MCF—The ITER Saga

A second notable absence at this symposium was any presentation on the merits and status of ITER. It is our understanding that ITER advocates were invited to participate but declined to participate for unstated reasons. This is unfortunate because of the continuing controversy over ITER. In the decade covered by these symposia, the OFES has been steadfast in its insistence that ITER is the next logical step in fusion research, in spite of insistence by the US Congress that OFES diversify.

The panel reports of previous symposia chronicle the ITER saga. The first panel noted

> *"the physical scale is enormous, the engineering challenges formidable, the time scale for results at least a decade, the physics basis of its performance uncertain, and the price is astronomical…regarding ITER we endorse the position taken by Thomas H. Stix and his 8 cosigners in a letter to SCIENCE (Vol. 271, p.891, 16 February 1996). They say 'at present it is not known how to construct a fusion reactor economically.' Therefore, our first priority should be to pursue alternate approaches until we know how to construct an economical fusion reactor."*

Because Stix and his cosigners had devoted much of their working lives to fusion and to the physics of tokamaks in particular, the first panel felt that the letter was so important that it was included in its entirety as an appendix to the panel's report.

The panel of the second symposium noted that the *"tokamak is viewed by many as being too large, too complex, and too expensive to meet the goal of economic practicality…There appears to be a common thread that provides a rationale for establishing a change in course in magnetic fusion research."*

Evidently, Congress agreed with the sentiments expressed by Stix, the second panel, and others. The review panel of the third symposium observed *"the 'Post-ITER' phase has begun, at least in the US, and as a result the fusion community is trying to define a new program and vision. Not only here, but also internationally, a contemplation of the past efforts and considerations of future activities, requirements, and research directions are being undertaken."*

The redirection of the US magnetic fusion program, ostensibly focused on "alternate concepts," was relatively short-lived, and the fourth panel noted that the *"major governmentally funded fusion research centers have given only lip service to approaches other than the tokamak…Compromises in the design, aimed at reducing the cost, diminish the projected performance, leaving some to question whether ITER will in fact achieve its goal of net fusion release, and, even if it does, whether the tokamak will represent an economically viable approach to fusion power."*

The panel of the fifth symposium lamented *"about 13 years ago the decision was made by OFES to narrow the focus of fusion research to tokamaks and other directly supporting research. As a result, various ongoing programs were terminated including mirrors, FRCs, plasma focus, and reversed field pinches."* The panel also noted decreased interest in fusion by the electrical power industry: *"The Electric Power Research Institute stopped supporting fusion many years ago based on a study that concluded that very large power plants burning D-T* (deuterium-tritium) *were not economically feasible."*

Some of the LD-CM-SSO papers presented at the sixth Symposium addressed various issues of tokamak physics, all related to much smaller machines than ITER. Of particular note was the paper presented by E. Mazzucato of Princeton Plasma Physics Laboratory, the US' lead tokamak facility. Mazzucato (paper entitled "About the Next Step in the Development of a Tokamak Fusion Reactor") argued for a machine much smaller than ITER as the next step in the development of a tokamak fusion reactor. Using essentially the same scaling arguments used to justify ITER, Mazzucato described the conceptual design of tokamak that in his opinion—an opinion not shared officially by his home institution--"must be considered fully equivalent, if not superior, to ITER."

Discussion at the sixth symposium focused on the prospect that OFES' most recent thrust to bring the US back into the ITER program was endangering the US domestic fusion science program. Participants expressed concern that a consensus achieved at a community meeting held in Snowmass CO (2002) on moving forward with ITER was predicated on the assumption that funding for ITER would come from additional allocations, whereas OFES FY06 budget requests showed a trend that would lead to the decimation of the domestic program in order to construct ITER. A participant claimed that ITER was not only too costly, it actually had design flaws, being macroscopically unstable according to ideal magnetohydrodynamics (MHD).

Shortly after the symposium, the U. S. Department of Energy's Fusion Energy Sciences Advisory Committee (FESAC) sent a letter to DOE Office of Science Director Ray Orbach, saying

> "However, FESAC is deeply troubled by the President's proposed budget for FY 2006 and its implications for later years. In particular, the core program cannot

> shoulder a significant portion of the ITER construction costs without dismantling the fusion scientific enterprise."

Orbach subsequently told the House Science Committee Subcommittee on Energy that "I would like to look at that in terms of a reorientation of the domestic program rather than a reduction."

As this report is written, the ITER saga continues, with a very uncertain outcome. Even if ITER is initiated in early 2006, present projections (which history would suggest are optimistic) are that ITER will not even make its first plasma until 2020. A fusion quality plasma will still have to await solution of the significant problem concerning evolution of impurities from the divertor and the first wall. Since there is no plan for breeding tritium, there is no plan to create a sustainable reactor. ITER, if built, will operate only periodically for short-lived experiments. ITER may fail to live up to its name—it is not a reactor.

Alternate Concepts

We mention some alternate concepts here to remind the reader that there are many promising fusion approaches other than tokamaks and laser-driven targets. In fact, much of our present knowledge of plasma physics is closely tied to alternate concepts work in the past. Unfortunately, the limited funding of alternates precludes any of them from reaching a level of maturity to make them truly alternates to the flagship concepts. We note that, at present, the application of fusion to practical energy production for electricity generation or any other non-defense application is not explicitly within the charter of the NNSA. Thus, NNSA does not request Federal funds in the President's budget to support ICF or any other fusion research for energy applications. Because of the emphasis of the OFES on steady-state concepts, there are no proper funding channels and concurrent advocacy within the Federal system for fusion approaches to economical energy production in the intermediate- and high-density pulsed operation categories.

The term "alternate concepts" came into vogue when Congress instructed the Office of Fusion Energy (OFE) to investigate approaches other than tokamaks. The OFE name was changed to Office of Fusion Energy Science (OFES), to reflect a change of emphasis from engineering to science, and some funding was made available for alternate concepts. Most OFES-funded alternate concepts, e.g, field-reversed configuration (FRC), reversed field pinch (RFP), stellerators, spheromaks, etc., fall into the same LD-CM-SSO category as tokamaks and are *a further set that described lines of research that had been followed quite seriously until cast aside in the tokamak frenzy (I).* However, as discussed by previous panels, their various stability properties are substantially different and, in many cases, they appear to be more stable than a tokamak. Many *started with promising ideas but soon showed the deadly intrusion of Empire Building into research (I).*

Mirror machines, an LD-OM-SSO concept and once the leading US contender in magnetic fusion, continue to be of interest in Japan (paper by T. Cho, "Progress in Mirror Plasma Activities") and in Russia. The US mirror effort is limited to theoretical and computational studies, but new results on the feasibility of a kinetic-stabilizer (paper by R. Post, "Axisymmetric

Tandem Mirrors: Status of Kinetic-Stabilizer Studies") suggests that mirrors should be re-examined.

The Congressional demand for unconventional approaches led to the formal identification of Inertial Fusion Energy (IFE) as an alternate to Magnetic Fusion Energy (MFE). As previously mentioned, the distinction between ICF and IFE often seems to be somewhat ambiguous and inconsistent, except in funding channels. "Alternate" in an ICF context includes alternate target drivers, since IFE requires repetition rates and efficiency not likely to be achieved by the glass-laser technology used in NIF. Alternate drivers include solid-state lasers (paper by C. Bibeau, "Commissioning Status of the Mercury Laser, a Scalable Option for Inertial Fusion Energy"), krypton-flouride lasers (paper by A. Mostovych, "Progress Toward Fusion Energy with Direct-Drive Krypton Fluoride Laser Drivers"), heavy-ion beams (not presented at the symposium), and z-pinch radiation generators (not discussed). Alternates to hohlraum-encased targets include fast-ignition (paper by K. Tanaka, "Current Status of Fast Ignition Research Using a Long Pulse Laser for Implosion and a PW Laser for Heating;" paper by M. Key, "Progress in US Fast Ignition Research") and direct-drive (paper by J. Soures, "Direct-Drive Inertial Confinement Fusion Research at the Laboratory of Laser Energetics;" paper by Mostovych).

As presently considered, all MCF mainline and alternate approaches fall into the LD-CM/OM-SSO category, whereas all ICF mainline and alternate approaches fall into the HD-UM-PO category. *To illustrate the extreme difference between the usual magnetic confinement regime and that of inertial fusion, there are twenty orders of magnitude in fusion power density (ten orders of magnitude in plasma density) between the two regimes (IV)* (in scientific parlance, ten and twenty orders of magnitude mean factors of 10,000,000,000 and 100,000,000,000,000,000,000, respectively). Since the required confinement time is inversely proportional to the density, the minimum confinement time for MCF is ten orders of magnitude longer than that for ICF. *In principle, fusion power systems could operate at any density between these extremes, if means were to be found to exploit this possibility (IV).*

The significance of this large difference in power density ($> 10^{20}$) , plasma density ($>10^{10}$) , and confinement time ($>10^{10}$) cannot be emphasized too much. There is an extremely large, essentially unexplored parameter space between MCF and ICF, and a growing number of researchers are beginning to realize that the optimum path to fusion energy may lie somewhere between the two extremes. Nevertheless, the focus on big machines (ITER, NIF) effectively precludes the major programs (MCF, ICF) from asking "is there anything in between MCF and ICF."

Of the various LD-CM-SSO plasma formation concepts, the field-reversed configuration (FRC) has the intriguing potential of reaching a high enough density that its mode of operation is necessarily pulsed. A proposal to achieve ID-CM-PO by forming, compressing, and translating an FRC was presented at the symposium (paper by J. Slough, "Small Scale Fusion: The Pulsed High Density FRC Experiment").

The staged z-pinch (paper by H. Rahman, "Staged Z-Pinch For Fusion") is a second concept that would attempt to access the ID-CM-PO regime. It was claimed that existing machines (the Z accelerator at Sandia National Laboratories, the Atlas capacitor bank at the Nevada Test Site) could reach breakeven energy production based on this approach. Unfortunately, this panel notes that NNSA ownership of such facilities, coupled with NNSA's charter and its disinterest in fusion other than laser-driven targets, means that there is no forum to review such concepts and, potentially, to lead to experimentation.

A second way of compressing an FRC was discussed at the symposium (papers by Zhang, Degnan, Siemon). In this approach, one of several possible embodiments of Magnetized Target Fusion (MTF), a relatively high density, sub-fusion-temperature FRC plasma would subsequently be compressed to fusion density and temperatures by a magnetically imploded liquid or solid metallic liner. This approach also falls into the essentially unexplored ID-CM-PO category and uses two separate magnetic fields, one inside the fusion fuel to provide magneto-thermal insulation and one outside the liner (which is analogous to the pusher in ICF) to drive the liner inward.

An FRC having a temperature of 150-250 eV and density of 2-4 x $10^{16}/cm^3$ has been demonstrated in joint pre-implosion plasma formation experiments between Los Alamos National Laboratory (LANL) and the Air Force Research Laboratory (AFRL) (paper by S. Zhang, "Field-Reversed Configuration Plasma for Magnetized Target Fusion at Los Alamos National Laboratory"). Furthermore, liners of the type needed for FRC compression have been demonstrated (paper by J. Degnan, "Compression of Field Reversed Configurations for Magnetized Target Fusion"), i.e., the driver required for FRC-based MTF is already in place, in contrast to the conventional target compression approach, ICF, where billions have already been spent, and even more is required, to develop a driver. Progress in FRC-based MTF is remarkable considering that the joint LANL/AFRL effort is funded at a level of $2M/year by OFES (this is essentially the only pulsed operation concept supported by OFES).

An MTF-related paper by Siemon ("Prospects for Magneto-Inertial Fusion Using The Atlas Facility at the Nevada Test Site") noted that the Atlas capacitor is, serendipitously, well suited for critical tests of magneto-inertial concepts, of which MTF is one. Siemon's analysis of fusion systems suggests that cost of a facility to achieve fusion burn in the ID-CM-PO regime that would be accessed by MTF and other concepts would be much lower than the cost at the two ends, MCF and ICF, of the density spectrum.

According to the first panel, *"An important study here is magnetized targets. If, in spite of their inherent lack of symmetry, they can be scaled to ignition and make LIF beams usable, the field of ICF would be transformed."* The fourth panel recognized the magnetized target approach to be a separate, discernable approach where *"the required time for net fusion power release is shorter than all but the most virulent plasma instabilities…only needing to function for microseconds to establish this end."* The fifth panel added that MTF *"is, essentially, a combination of the essential features of magnetic confinement and inertial fusion…The time scales for fusion are fast compared to magnetic confinement fusion and slow compared to inertial fusion."*

We note that two papers covering the Russian MTF embodiment, MAGO, were to have been presented but were withdrawn when the US State Department did not issue visas in time.

MTF illustrates the difficulty in obtaining funding that is encountered by concepts that are truly orthogonal to the mainline approaches. In 1995, when Russian officials were trying to determine whether or not the US was interested in only projects within its own narrow national security interests, a letter proposing a joint US/Russian program in MAGO and related MTF approaches was written to the US Secretary of Energy by the Russian Minister of Atomic Energy. Similar letters were written to the DOE Assistant Secretary for Defense Programs by leading Russian scientists, including Evgeni Velikov, Vice-President of the Russian Academy of Sciences and a leading tokamak advocate. The Assistant Secretary for Defense Programs, and not the Secretary of Energy, responded: "*We are committed to NIF...if the situation should change, I will contact you.*" Subsequently, MTF was reviewed by two OFES panels. MTF was rated mature enough to be considered a "proof-of-principle" by the first panel (1998) to hear proposals in this area, but a second OFES panel called together soon thereafter (1999) downgraded MTF to "concept exploration," with concurrent reduced funding that ensures that reaching any technical maturity will be difficult.

Fusion and Private Investment

An advanced fuel alternate concept that has been overlooked by OFES has obtained substantial private support (paper by M. Binderbauer, "Progress on the PEG Program"). The Plasma Electric Generator (PEG) will use a proton-Boron^{11} plasma formed by injecting ion beams into the plasma volume. Ion-ion collisions will produce a thermal ion distribution in a rotating frame of reference. Both protons and boron ions will have drifted Maxwellian distributions with the same mean velocity and temperature. The FRC target plasma will use an electrostatic equilibrium field to prevent anomalous transport of energy by electrons and magnetically confined, large orbit ions to achieve near classical ion confinement.

Private and public support is being obtained for building a fusion prototype reactor that could be operational by the end of the year 2010 (paper by E. Panarella, "The Institute for Fusion Studies in Southern Italy: Progress Report on the Design and Construction of the First Fusion Prototype Reactor"). Rather than using a fusion source that has high gain, this approach will focus on recovering and reusing all energy losses to obtain "a few watts of net fusion energy." This reactor will be located in Southern Italy, with technical backing by a Canadian firm.

Fusion materials

Unfortunately, this symposium had no presentation or discussion on the difficult problems of materials in a first-generation fusion reactor that will generate massive quantities of high-energy 14.1 MeV neutrons. There appears to be little data in the high fluxes of neutrons that will be generated. The lessons learned during the development of fission reactors are worth remembering. On this clearly much simpler problem, essentially three materials were critical for success: graphite, uranium, and aluminum. Although graphite and aluminum had been used for many years, they could not be used in a fission reactor by standard engineering practices. The graphite contained boron, so a new method of production to eliminate the boron had to be developed. Aluminum could not be welded to be reliably leak free by standard methods. And, of course, uranium was almost totally misunderstood. Its handbook melting point was off by about 300^{o} C and its three solid metallic phases were unknown. Solution to these engineering problems was estimated by some experts to have caused significant delay in the operation of the Hanford reactors. The job of engineering a fusion reactor will be very much more difficult. Therefore, it is important to anticipate materials issues and get a head start on solutions.

Fusion and the Electrical Power Industry

As mentioned previously, the fifth panel noted that *"The Electric Power Research Institute stopped supporting fusion many years ago based on a study that concluded that very large power plants burning D-T were not economically feasible."* Little evidence can be found that the electrical power industry takes fusion seriously. There appears to be a consensus that fusion would not be used as an energy source in any of its currently envisioned main-line embodiments even if they existed today. The initial cost, and the cost of operation, of present reactor designs make use of fusion prohibitive in today's environment. However, the record price of oil that occurred shortly after the symposium reminds us that the energy environment of the future may be quite different from that of the present.

This symposium benefited from a tutorial given by a member of the electric power industry (paper by Page, "Desirable Fusion Reactor Qualities for Commercial Electrical Generation Applications"). Data was presented which showed the trade-off between fuel cost and first cost for a wide range of powerplant types, including fossil fuel, fission, and fusion. Using this data, researchers can determine early in the design process whether the fusion powerplant in question is a good candidate for commercial power generation. A point was made that the public believes that fusion reactors are not radioactive and that a backlash will occur when the public realizes otherwise. On the other hand, it was noted that nuclear power is the only way to make enough hydrogen for a hydrogen economy. Because tokamaks are not only far too expensive but also far too complex to be of economic viability in a commodity market like electrical energy, it was argued that support should be given to alternate concepts that could potentially demonstrate net energy production in less than a decade and at a total research and development cost of less than $75M. With regards to ITER, it was felt that "government is walking barefoot in the desert following the mirage of ITER." The presentation emphasized that fusion powerplants must have an installed cost of less than $6700/KWe (kilowatt electrical) for commercial viability while being able to recoup the incremental first cost over more traditional utilities in less than 30 years.

Status of Fusion Research in the US

At present the US fusion program is funded as follows. Congressional appropriated FY05 funding for OFES, which represents and controls all of the advocacy and funding for energy and non-defense applications of fusion, totals about $276M. Out of this, tokamaks and large stellarators are funded for a total of about $200M, including the tokamak-oriented plasma technologies, materials, theory, computing and diagnostics research. Another $10M is used for supporting International Collaborations and for ITER preparation. Magnetic alternates are funded for only $28M (including Reversed Field Pinch which is funded at $6.5M). Inertial

Fusion Energy (IFE) is funded for only \$15M, of which \$12M is for developing the heavy ion beam as a driver for IFE, and only \$3M for innovative alternative approaches to inertial fusion. \$12M is used for supporting the stewardship of general plasma science whose connection with fusion is not yet obvious, and \$7M for SBIR.

The NNSA ICF program is funded at approximately \$535M/year. However, this funding is explicitly directed at non-energy related inertial fusion research – that is, this funding cannot be used explicitly for funding fusion research with energy applications. However, Congress has on its own initiative included \$25M in the NNSA budget for IFE research through the Naval Research Laboratory in a program called High Average Power Lasers (HAPL). HAPL is a program to develop repetitive laser drivers for the laser driven inertial fusion. This supports our previous conclusion that Congress tends to view ICF as fusion energy research, not just a defense application.

This panel subscribes to a number of recurrent themes of past panels.

A first recurrent theme, one that must be emphasized, is that the fusion problem is nowhere near solved. Fusion reactors are far from reality. Fusion has not yet been reduced to simply engineering. As mentioned previously, the first panel saw fit to include as an appendix, the full text of the Stix letter, a letter that states "at present it is not known how to construct a fusion reactor economically." *The reality is that, despite nearly 50 years of hard work, and despite the impressive accomplishments that have been made during that period, we cannot at this point in time put the label "solved" on any of these three areas of concern (II):* (1) the physics of the high-temperature plasma state; (2) the quantitative requirement of achieving a net fusion power release; (3) economic practicality. *There does not exist an adequate knowledge of the plasma state as it interacts with magnetic fields (II).* Papers at the third symposium identified *"the important need for a better understanding of plasma physics and its many aspects." Though fusion research's goal has from the start been precisely defined, namely, to obtain a net release of energy from controlled nuclear fusion reactions…the difficulty of the problem has spawned…a wide variety of approaches to the problem (IV). The fusion power plant of the future will probably not resemble any of the present flagship programs (unless we are desperate) (V).*

A second recurring theme is program balance, particularly in programs funded by OFES, and the role of alternates. *The mainline programs are on such a scale, both in money, effort, and time, that even rather unlikely alternatives are worth a close scrutiny, and in most cases, more theoretical and experimental investigation (I). It is also not clear why what funds there are, are concentrated in only a very few, not completely promising avenues of research (I). In addition to a need for increased emphasis on basic plasma physics issues, there should be a return to a better-balanced state as between the so-called 'mainline' approach (the tokamak) and those approaches that differ substantially from the tokamak (II). At this time of a new beginning, it has to be the goal that a broad spectrum of different fields and directions in fusion physics be implemented (III). We are convinced that we do not have to wait another 50 years, if the recommended diversification is pursued (III). On the international stage only a few countries and/or institutions have been able to retain a more balanced approach in the face of these two pressures (IV),* a "bandwagon" effect in funding and the sensitivity of the "performance index" to size of the facility. And the fifth symposium concluded that *"There are few completely new* *ideas judging from the conference. There are many new ideas related to previous fusion research subjects where the support disappeared due to strong focusing of resources. Perhaps some of these new ideas will find support and there will again be diversity and competition."* Unfortunately, the premature focus on a single fusion approach is akin to focusing cancer research on, for example, chemotherapy while essentially ceasing all research on radiation therapy.

Another recurrent theme is that fusion funding has not been consistent with its potential. *It is far from obvious why more resources are not being devoted to the exploration of fusion (I). Internationally and in the US in particular, the funding level for fusion is grossly at variance with the need to solve the fusion problem in the shortest possible time (IV).* Nevertheless, we fear that funding for fusion will not increase substantially in the near future.

We are concerned about the lack of open scientific discussion and objective peer review in the US fusion program. With keen competition for funds, a "don't bite the hand that feeds you" sentiment has far too often permeated the fusion community. At the symposium, the most critical discussion of the US program came from senior fusion researchers who are much more independent of funding cycles than the younger generation.

Recommendations

We call on the US Congress to explore all possible avenues to increase funding for fusion research. Independent of the level of funding, we call on Congress to ensure a balanced fusion program that will maximize the benefit to the taxpayers that are paying the bill for fusion.

Even if sufficient funding can be found to make US participation in ITER possible, we are not convinced that ITER is the best way to use such funding. With the President of the US evidently strongly backing ITER, the fusion community and Congress must make every effort possible to ensure that the President is not misinformed about ITER's prospects. The question about whether or not the President's international policy goals for US ITER involvement can be met by international collaboration in other, less costly, approaches must be seriously evaluated. We are confident that less costly approaches can be found to make fusion energy a reality in a much shorter time period, if an environment that encourages scientific innovation can be restored in the US fusion program.

We are concerned about the availability of adequately trained fusion research personnel in the future. Many early fusion researchers are now reaching the end of their careers. Although it is quite easy to capture a young person's imagination with the potential of fusion, it is not quite so easy for main-line programs to provide sufficient excitement, and a guaranteed future, to attract the young person into the field. A strong university program is an absolute necessity.

This symposium and previous symposia suggest that the only way to ensure that the funds available for controlled fusion energy research are used in the most-cost effective way is for Congress to require a complete reorganization of the US fusion effort. A reconstituted fusion program would have three major offices: (1) the Office of Steady State Fusion (OSSF); (2) the

Office of Unmagnetized, Pulsed Fusion (OUPF); (3) the Office of Magnetized, Pulsed Fusion (OMPF). These three offices would cover the natural division in burning plasma density (LD/ID/HD) and the resultant natural division in plasma physics and materials science issues. The offices should be essentially independent with approximately equal funding once programs under the two substantially new offices (OUPF, OMPF) are well underway. In some ways, this proposed division of effort would parallel similar divisions of effort in the nuclear fission power research program.

The OSSF would replace the OFES. Existing steady-state MCF programs would come under OSSF's purview. Even within this office, history has shown that care would have to be taken to prevent a single concept (e.g., tokamaks) from excluding other promising, but unexplored, avenues.

The OUPF would remove IFE and the various ICF alternates that are not being pursued by DP out from under the OFES, ensuring the proper advocacy that has not previously been possible (we note that in this area Congress has repeatedly had to include in the budget funding for concepts for which DP or OFES have chosen not to request funds). An issue that must be addressed is the overlap, if any, between this office and DP. Perhaps the best way to ensure that IFE is adequately evaluated in a cost-efficient and timely manner is to include in this new office all fusion components of the NIF project, and related projects now under DP. This would force DP to defend its contention that fusion ignition in ICF, albeit substantially different than fusion in nuclear weapons, is critical to DP's stockpile stewardship mission.

The OMPF would ensure that concepts that are attempting to access the essentially unexplored parameter space between the extremes of existing MCF and ICF research would have a chance to reach technical maturity. Furthermore, the existence of such an office, with its concurrent funding and advocacy, would stimulate new thinking in this area that has been neglected.

Together, OUPF and OMPF would overcome the impediment to progress that has resulted from OFES's historical, and well-known, bias against pulsed operation. In fact, OFES and its programs have been permeated with an apparent arrogance that steady-state MCF is the "only" viable fusion energy research program, and all other concepts are not truly fusion research.

Having three separate and independent offices would greatly enhance the prospects of objective, effective and critical peer review. Far too often, review panels have been "stacked" with members financially dependent on the program to be reviewed. Peer review for one office would come from those funded by the other offices, although care would have to be taken to avoid the "you scratch my back, I'll scratch yours" mentality that has sometimes infected the US national laboratories.

In this report, and the panel reports that preceded it, there has been a strong sentiment that fusion research is not progressing as rapidly as deemed to be possible because of bureaucratic impediments. Even by the time of the first symposium, it was recognized *"that the unkept promises of near term results had already discredited fusion research."* The reorganization of the US fusion program as suggested here would go a long way to alleviating previous problems, restoring fusion's credibility, revitalizing the national fusion program, and hastening the arrival of fusion as a source of nearly endless energy.

Contact Us

The panel members welcome feedback from the fusion community and those interested in seeing fusion reach its full potential. We can be contacted by e-mail at:

Edward C. Creutz	ec2creutz@cts.com
Ronald C. Kirkpatrick	rck@vla.com
Irvin R. Lindemuth	irvl@att.net
Richard F. Post	post3@llnl.gov
Hans J. Schneider Muntau	smuntau@magnet.fsu.edu
Norman Rostoker	nrostoke@uci.edu

Biographical sketches of Panel Members

Edward C. (Ed) Creutz

Dr. Edward C. Creutz received his B.S. degree in Mathematics and Physics from the University of Wisconsin (UW) in 1936 and his Ph.D. in Physics from UW in 1939. His Ph. D. thesis was on the Resonance Scattering of Protons by Lithium. He has been the Director of the Bishop Museum in Honolulu, HI (1977-1984), the Acting Deputy Director of the National Science Foundation (NSF) (1976-1977), the NSF Assistant Director for Mathematical and Physical Sciences and Engineering (1975-1977); the NSF Assistant Director for Research (Presidential appointee; 1970-1975), the Vice-President for Research and Development of the General Atomic Company in San Diego, CA (1955-1970), a Scientist-at-large in the Controlled Thermonuclear Program of the Atomic Energy Commission (1955-1956), Professor and Head of the Department of Physics and Director of the Nuclear Research Center at the Carnegie Institute of Technology (CIT) (1948-1955), an Associate Professor of Physics at CIT (1946-1948), a Group Leader at Los Alamos National Laboratory (1944-1946), a Group Leader in the Manhattan Project in Chicago, IL (1942-1944), and an Instructor of Physics at Princeton University (1939-1942). Dr. Creutz has been (1960 ff) a consultant to the Atomic Energy Commission, the National Aeronautic and Space Administration (NASA), and industry. He has been (1960 ff) on the Editorial Advisory Board for the American Nuclear Society, Annual Reviews, Handbuch der Physik, Interdisciplinary Science Reviews, and Handbook of Chemistry and Physics. He has published 65 papers in the fields of Physics, Metallurgy, Mathematics, Botany, and Science Policy. He is the holder of 18 patents in nuclear energy applications, including US patent #2,910,418 granted in 1959 to Eugene Wigner and co-workers (A. Weinberg, G. Young and E. Creutz) for the Wigner team's 1945 conceptual design for the Hanford reactors for plutonium production. He has been inducted into Phi Beta Kappa, Tau Beta Pi, and Sigma Xi. He is a recipient of the National Science Foundation Distinguished Service Award, the University of Wisconsin's College of Engineering's Distinguished Service Citation and the American Nuclear Society Pioneer Award. He is a Fellow of the American Association for Advancement of Science (AAAS) and the American Physical Society. He is a member of the National Academy of Sciences, the American Association of Physics Teachers, and the American Nuclear Society.

Ronald C. (Ron) Kirkpatrick

Ronald C. Kirkpatrick was a staff member at Los Alamos National Laboratory (LANL) from 1973 until his retirement in 2004. He has degrees in Electrical Engineering (BS, 1959) and Physics (MS, 1963) from Texas A&M University and in Astronomy (PhD, 1969) from the University of Texas at Austin. His background includes work in the electrical power industry; flight instrumentation; high temperature experiments; shock wave physics; numerical computation of partition functions, radiative recombination coefficients and transition probabilities; modeling of planetary nebulae; A/D interface design; teaching Physics and Astronomy; investigating the physics of fusion ignition; development of special purpose research codes for the design of fusion experiments; studies of laser interaction with matter; and calculation of charged particle energy deposition in magnetized plasmas. He worked at Gulf States Utilities Company, NASA Ames Research Center, Southwest Research Institute, the University of Texas, Applied Research Laboratory (UT), NASA Goddard Space Flight Center, and Texas A&M University prior to going to Los Alamos National Laboratory. He is a member of the American Astronomical Society and the International Astronomical Union, and has written several dozen scientific and technical papers.

Irvin R. (Irv) Lindemuth

Dr. Lindemuth retired from full-time employment in November, 2003 after more than 32 years with the University of California, first at the Lawrence Livermore National Laboratory (LLNL) and then at the Los Alamos National Laboratory (LANL). At LANL at the time of his retirement, Dr. Lindemuth was a Special Assistant for Russian Collaboration in the Office of the Associate Director for Weapons Physics, the Team Leader for Magnetohydrodynamics and Pulsed Power in the Plasma Physics Group, and a Project Leader for Pulsed Power Science, Technology, and International Collaboration in the High Energy Density Hydrodynamics Program. His primary responsibility was to provide technical leadership for a scientific collaboration between LANL and LANL's Russian counterpart, the All-Russian Scientific Research Institute of Experimental Physics (VNIIEF) at Sarov (Arzamas-16). Prior to joining LANL in 1978, he was a technical staff member in A-Division at the LLNL where he was involved in fusion research. Dr. Lindemuth received his B.S. degree in Electrical Engineering from Lehigh University in 1965 and his M.S. and Ph.D. degrees in Engineering--Applied Science from the University of California, Davis/Livermore in 1967 and 1971, respectively. His thesis research was conducted under the advisorship of Dr. John Killeen, founder of the National Magnetic Fusion Energy Computer Center. He has been an Adjunct Professor at the University of New Mexico Los Alamos branch. He spent the 1991-92 academic year as a Visiting Professor in the Nuclear Engineering Department of Texas A&M University. He has published numerous papers in refereed journals and proceedings of major international conferences. Dr. Lindemuth played an essential role in establishing the collaboration with VNIIEF, a collaboration that has helped integrate Russian weapons scientists into the global scientific community, has resulted in more than 250 conference papers and archival publications, and was featured in the Discovery Channel documentary, "Stockpile," first aired in 2001. In 1992, Dr. Lindemuth was the recipient of a Los Alamos Distinguished Performance Award for his work in the formative stages of the LANL/VNIIEF collaboration. In 2004, he was named a Fellow of the Institute of Electrical and Electronic Engineers (IEEE).

Richard F. (Dick) Post

Dr. Richard F. Post received his B.A. from Pomona College and completed his Ph.D. in physics at Stanford University in 1950. He taught physics at Pomona College, worked as a physicist at the Naval Research Laboratory and spent four years as a research associate in physics at Stanford before joining the Lawrence Livermore National Laboratory as research group leader in the Controlled Thermonuclear Research group in 1951. He was appointed deputy associate director of the Magnetic Fusion Energy Program at LLNL in 1974 and senior scientist for the program in

1987. Dr. Post has been associated with the Department of Applied Science at the University of California/Livermore as Professor-in-Residence since 1963. He is the holder of more than 25 patents in the fields of nuclear fusion, particle accelerators, electronics, and mechanical energy storage. He has received many awards, including the Meritorious Civilian Service Award from the U.S. Navy, an honorary Sc.D. from Pomona College, the Robert Henry Thurston Award, the American Academy of Achievement Golden Plate Award, the U.S. Energy Research and Development Administration Distinguished Associate Award in 1977, the Outstanding Achievement Award from the American Nuclear Society in 1978, the James Clerk Maxwell Prize of the American Physical Society in 1978, and the Distinguished Career Award from Fusion Power Associates in 1987. He has been elected a Fellow of the American Physical Society, the American Nuclear Society, and the American Association for the Advancement of Science.

Hans J. Schneider-Muntau

Dr. H. J. Schneider-Muntau is Chief Technology Officer of the National High Magnetic Field Laboratory (NHMFL) of the Florida State University (FSU). He has been instrumental in the building-up of the $150M laboratory. He has extensive knowledge in project management. He defined and established the Magnet Science and Technology Program of the NHMFL. He has initiated and managed many successful research and development programs, which have established the NHMFL as the leading laboratory worldwide. He has introduced the "Poly-helix" magnet concept in Grenoble and invented the "Florida-Bitter" magnet design at the NHMFL, which made it possible to build the 33 T Bitter magnets and the 45 T hybrid magnet. He introduced the "Poly-layer" design with internal reinforcement to pulsed magnets, an application which can immediately be transferred to high-rpm motors and flywheels. In view of the crucial role of materials development for advanced magnet systems, he has promoted and initiated microcomposite development activities, such as CuAg and CuNb, at the NHMFL and in industry, and encouraged the improvement of high-strength, high-modulus reinforcement, such as MP35N, and the introduction of Zylon in magnet construction. He is very much interested in initiating industrial applications of the so-called second generation ("2-G") superconductors, such as YBCO. Dr. Schneider-Muntau is a member of many conference committees, has more than 150 publications and given over 60 invited talks. He has written several book chapters and edited three books. Because of his international stature, competence in magnet technology, including advanced magnet development and materials for magnets, his advice is sought frequently. Hans J. Schneider-Muntau has been a faculty member of FSU since 1991 and Professor for Mechanical Engineering at the FSU-FAMU College of Engineering. He has been Deputy Director of the NHMFL from 1991-2002, and its Director of Magnet Science and Technology from 1991-1997. From 1972 to 1991, Dr. Schneider-Muntau was Chief Engineer at the French/German High-Magnetic-Field Laboratory in Grenoble, France, operated by the Max-Planck Institute and the CNRS. He led the Development Laboratory of the European Space Research Institute in Frascati, Italy, from 1968-1972. There, he worked on space simulation experiments, development of high-voltage nanosecond discharges, capacitor banks and pulsed laser sources. From 1962-1967, he worked on fusion technology as scientist at the Institut für Plasmaphysik, Garching, of the Max-Planck-Gesellschaft, Munich, and developed pulsed neutron sources, and fast high voltage discharges up to 300 kV.

Norman Rostoker

Prof. Norman Rostoker received his BA Sc. degree in Engineering Physics in 1946 and his MA degree in Mathematics in 1947 from the University of Toronto. He received his D. Sc. degree in Phsyics in 1950 from the Carnegie Institute of Technology. From 1993 until the present, he has been Professor Emeritus and Research Professor of Physics at the University of California, Irvine (UCI). He has been Professor of Physics at UCI (1972-1993) and served as Chairman of UCI's Department of Physics (1975-1978). He was IBM Professor of Engineering and Chairman in the Applied Physics Department of Cornell University (1967-1972). He was Manager of Fusion and Plasma Physics projects at General Atomic Company in San Diego (1965-1967). He has also been Professor of Physics at the University of California, San Diego (1962-1965), a research staff member at General Atomic (1956-1962), the Supervisor of Theoretical Physics at the Armour Research Foundation, Chicago (1953-1956), an experimental physicist at Carnegie Institute of Technology (1948-1953), and a teaching fellow in Mathematics at the University of Toronto (1946-1947). He has published 130 papers in archival journals. He has written chapters in four textbooks. He is the holder of five patents. He is a fellow of the American Physical Society (APS) (1961) and he was Chairman of the APS Plasma Physics Division in 1972. He was awarded the UCI Distinguished Faculty Award for Research in 1988, the APS James Clerk Maxwell Prize in Plasma Physics in 1988, and a UCI Medal in 1999.