February 28, 1994

Dr. Martha Krebs
Director, Office of Energy Research

Dr. N. Anne Davies
Associate Director for Fusion Energy

U.S. Department of Energy
Office of Energy Research
Washington, DC. 20585

Dear Dr. Krebs and Dr. Davies,

We wish to alert you to two critical concerns regarding the enormously productive Department of Energy research program in fusion energy. We write as a collection of fusion scientists—not representing any organization—who have dedicated our careers to the fusion quest.

Fusion is one of a very small number of potential solutions to the world's long term energy problem. Its particular advantages—regarding fuel abundance, air quality, global warming, safety and nuclear proliferation—seem to assure it a central role in future world energy production. Furthermore the international program in fusion energy research has achieved dramatic progress in the past twenty years. Its main figure of merit, for example, has increased by a factor of some ten million since 1970.

Despite such advances—most recently manifested by the DT-burning experiments at Princeton—US funding for fusion has steadily decreased: it is now roughly half its level of 1980. This peculiar and painful circumstance has forced the program to contract drastically, losing skilled technical personnel, even as it faces its most exciting opportunities. The funding cuts also threaten the US position in the international effort, which was once one of clear-cut leadership.

Hence the first purpose of this letter: to emphasize that the fusion energy research program, now at a scientific and technical threshold, deserves major reinvestment. Our letter however has an additional purpose. We wish to draw your attention to an issue, internal to the program, that threatens to retard the development of fusion energy.

The problem is that fusion *science*, including both fusion plasma physics and basic plasma technology, is now endangered. Every success that fusion energy has enjoyed—and the successes of the past two decades are impressive—it owes to creative research in fusion science. High quality scientific research must continue to play a central role if our goals are to be achieved. Yet for the past several years just that sort of research has steadily declined.

Through fusion science we have learned how to produce several megawatts of fusion power (in TFTR), while confidently embarking on plans to produce more than a gigawatt (in ITER). This capability is the result of several decades of vigorous investigation of a broad spectrum of topics: the exploration of new magnetic confinement configurations, the understanding of shaped plasma stability, the invention of current-drive and heating schemes, advances in nonlinear stability theory and model development, new diagnostic methods, and so on. Such high quality research has produced not only exciting advances in fusion; it has also had wide impact in other areas of science, including, for example, space physics and nonlinear dynamics. But most importantly, the issues now confronting fusion—its progression to a competitive and attractive energy source—depend even more urgently on scientific knowledge and understanding.

Today one detects at some levels the notion that only a few large facilities are needed prior to commercialization in 2040, and that additional research is an unnecessary luxury. This attitude threatens the success of fusion. In a rough chronological sense, the program is at a midpoint between its start and fusion's eventual commercialization. It is extremely premature to limit the vision of a fusion reactor, still several decades from construction, to what is allowed by the present state of scientific knowledge. To enforce such narrowing of the program is analogous to terminating aviation research at the Wright airplane, or computer research at the first vacuum tube computer.

Fusion research must sustain a balanced program. It will necessarily include large, integrated facilities, such as TPX and ITER, but it will also need a complement of smaller experiments, along with broadly directed theoretical research. There are numerous problems and opportunities which cry out for research, but which are often best tested for the first time on small- and intermediate-scale facilities. These include enhancements to the tokamak, such as improved current-drive techniques and disruption control schemes, as well as innovations in the fusion reactor concept, such as compact or inherently steady-state reactors. Such endeavors are often pursued most productively and cost-effectively (whether at universities, industry or national laboratories) on a scale much smaller than that of the largest facilities.

Various fusion advisory committees have pointed to the strong need for a balanced research program. However, in accord with their charters, the committees have typically focused upon the role of large experiments. This is a reasonable approach only in the presence of a vigorous underlying research program. Now, when the essential nourishment provided by broad-based research is shrinking, its survival becomes the top priority. In this regard the discouragement of our best young fusion scientists is already apparent: perceiving the limited programmatic support for innovation and scientific curiosity, many have decided to apply their talents elsewhere.

We urge you to establish the means for preserving the scientific basis required for a successful fusion energy program. We do not attempt here to prescribe those means, beyond the first, essential step: recognizing the critical role and presently fragile state of fusion science. The signatories of this letter are diverse in fusion-science interest, institution, and viewpoint. United only in our desire to hasten the advent of fusion power, we hope to foster recognition of the problem by the Department of Energy, so that discussion of a remedy can begin.

We thank you for your attention, and offer our help.

[Signatures follow on next page]

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