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The Honorable Bennett Johnston  
136 Hart Senate Office Building  
Washington, D.C. 20510

Dear Senator Johnston,

I write in my capacity as president of the University Fusion Association (UFA) to express our view of Senate bill S.646, "The International Fusion Energy Act of 1993." The UFA is a national organization of fusion researchers from every U.S. university active in fusion research. Our members are involved in all aspects of the fusion program, from small experiments to ITER design. Thus, we believe that our view (as developed by the executive committee listed in the attached sheet) is not a parochial one.

We admire and appreciate your support of the development of fusion energy. The bill's "findings" captures both the potential of fusion and the significant progress to date. We fully share your desire to accelerate the pace of the development of fusion power, and we are pleased that the bill aims to strengthen the commitment of the U.S. to a long-term program. The restructuring proposed in the bill presents a useful opportunity for frank discussion of the various paths for fusion research and development, from which the optimal route to fusion can be devised. In this spirit we offer our comments.

We wish to sound a clear alarm that the redirection proposed by the bill, as we interpret it, would have consequences opposite to your intention. The proposed restructuring would severely retard progress in fusion.

As we understand it, the bill sends the following very strong message. ITER will address nearly all the scientific issues of fusion necessary to build an economical fusion reactor. The present knowledge base is sufficient to predict with some confidence that a tokamak of the ITER type will prove successful. Thus, the U.S. should focus the research program to eliminate nearly all work which is not directly tied to the ITER project. This major streamlining of fusion program will be symbolized by hereafter referring to the entire fusion program as the ITER program.

We make the following three points. (1) ITER is a major milestone in fusion research, but will likely not by itself provide sufficient information to proceed to a practical reactor. (2) Additional research of equal importance is essential. (3) The time scale for fusion demands a strong and innovative research effort in addition to ITER. We elaborate on each item below.

### (1) The Role of ITER

ITER will be the first experiment to produce a large amount of fusion power, to achieve ignition, and to test many key aspects of fusion technology. It will be an exciting and worthwhile experiment. We emphasize that we support ITER and all of our comments should be viewed in that context. However, ITER is not a blueprint for a fusion reactor. ITER is a large and complex machine. It remains an open question whether a tokamak of the ITER design will extrapolate to a practical reactor. In addition, we anticipate that in the next 40-50 years fusion research will evolve in ways that we cannot predict. It is likely that a commercial reactor will look quite different than ITER. These statements do not diminish the role of ITER. ITER represents an enormous scientific and engineering milestone in fusion research. It will prove the reality of fusion, much as the initial Wright airplane of 1903 proved the reality of flight.

### (2) The Need for Additional Research

Research not directly coupled to ITER is essential. There are specific problems which must be resolved (but will not be solved in ITER) and many ideas for reactor improvements which must be pursued. To name a few issues, there is need and opportunity to improve current drive techniques, to develop inherently steady state reactors, to develop more compact reactors, to develop reactors with reduced magnetic field requirements, to develop disruption control techniques, to investigate and reduce transport in plasmas, to develop reactors without the need for auxiliary heating. This is only a small list of the critical topics for which there are existing ideas and plans, but which are not directly tied to ITER. They fall into two categories: those which attempt to solve known problems in the tokamak and those which aim to improve our concept of a fusion reactor ("advanced tokamaks" or close relatives of the tokamak).

To put a halt to such research, would eliminate the program which has given us the knowledge to build ITER. This is the base research program which has been and will continue to be the lifeblood of the development of fusion. Without it we will likely fail. The non-ITER research is necessary to proceed beyond ITER. It is also needed to operate ITER most effectively, and to fully utilize the results from ITER. Both Europe and Japan are maintaining strong programs in addition to ITER. It is not clear that the U.S. will continue to be a viable partner in the ITER research project without a comparably strong program.

### (3) The Structure of a Long-Term Development Program

The present fusion program is expected to culminate in a commercial fusion reactor in 40-50 years. There is no scientific predictability on this timescale. It is drastically premature to commit the fusion program to a well-defined reactor concept at this time. To do so is analogous to terminating aviation research at the Wright airplane or computer research at the first

vacuum tube computer. At those times, neither jet aircraft nor solid state supercomputers were foreseeable some 50 years later. To stop non-ITER research now would condemn us to a 2040 reactor based upon 1993 science.

Often the planning of the fusion program is framed as a choice between two undesirable alternatives. The first is that we have an ITER-only program, based on the belief that our present view of a reactor will prevail decades into the future. The second is that we do not build ITER and abandon fusion energy, based on the belief that after all these years we still do not know how to build a reactor. This is a false choice, not in the best interests of the country. The truth is that progress has been steady, remarkable, and tangible. The benefits, both to science and technology have been enormous. We have come a long way, but there is still a long way to go. We are about to fly for the first time, but not commercially. It is precisely the time that a renewed national commitment is appropriate, as you propose. However, the extreme narrowing of the effort will not accomplish your aim. Virtually every external and internal fusion review committee, including the recent Fusion Energy Advisory Committee, has lauded the progress in fusion, and recommended a strong base program in addition to ITER. Such advisory committees have included senior industrial representatives, as well as members of the scientific community.

Our aim here is to assist you in formulating the most expeditious route to fusion power. To this end, we strongly urge you to enlarge the focus of the bill (consistent with the energy act of 1992 which calls for a "broad-based" fusion energy program in addition to ITER).

Thank you for your consideration. I would be happy to provide any further input which would be useful to you.

Sincerely,



Stewart C. Prager

President

University Fusion Association

c.c. Dr. W. Happer, Department of Energy  
Senate Sub-Committee on Water and Energy

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