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FROM: OSTRACH, Simon: CASE WESTERN UNIVERSITY

TO: DR. D.A. BROMLEY

DATE OF
CORRESPONDENCE: 07/24/92

SUBJECT: HE IS WRITING REGARDING COMMENTS DR. BROMLEY MADE
REGARDING SCIENCE IN A MICROGRAVITY ENVIRONMENT AT
THE RECENT CONVOCATION OF PROFESSIONAL SOCIETIES AT
THE NATIONAL ACADEMY OF SCIENCES.

DIRECTORATE
ASSIGNED: PHYSICAL SCIENCES

STAFF
ASSIGNED: Doug (look at) *discuss after visit w/KE*

ACTION
REQUIRED: FOR DAB'S SIGNATURE

STAFF
ACTION:

SENDER'S DUE DATE:
OSTP DUE DATE: 08/17/92
DATE COMPLETED:
STAFF DUE DATE
DATE COMPLETED/DEPT: 8/3/92

COPIES TO: D. Allan Bromley
Steve Olson

WHITE HOUSE TRACKING #: CONTACT PERSON:
PHONE: EXT:

REMARKS: *Dr. Erb signed ltr for D.A.B.*

OSTP RECEIVED: 08/03/92 DEPT RECEIVED:
FILE: P-PHYSICAL SCIENCES

CENTRAL FILES: *Physical Sciences*





CASE WESTERN RESERVE UNIVERSITY

July 24, 1992

Dr. D. Allan Bromley
Assistant to the President for
Science and Technology
Executive Office of the President
The White House
Washington, DC 20500

Dear Dr. Bromley:

At the recent Convocation of Professional Societies at the National Academy of Engineering I was distressed to hear you say that you and your staff know of no valid science that could be performed in a microgravity environment. As a scientist who has been involved with NASA's microgravity program for many years and just completed an outstanding series of experiments on the USML-1 shuttle mission, I would like to know the bases for your conclusions.

I would call your attention to three reports, Refs. 1 to 3, issued by the National Research Council which indicated many important and fruitful research areas for microgravity research. The members on the committees that developed those reports included distinguished space scientists and a Nobel laureate in physics in addition to people involved in microgravity research. Another report by the Space Studies Board, Ref. 4, has just been issued and still another SSB Committee is developing a report, "A Strategy for Microgravity Research for the 1990's," which is to be completed by the end of the year.

In short, this unusual multi- and cross-disciplinary research area has been evolving over the years and has received careful scrutiny and endorsement by a broad segment of the science community. This research is quite different in many ways from the space sciences and seems to be different things to different people.

I have enclosed a White Paper on microgravity research that was recently released which helps to describe the nature and importance of microgravity research. I believe it is a precursor for other cross-disciplinary research which is essential for the Nation's technological leadership and it impacts the lives of the citizens more directly than much of the research in the pure sciences.

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References

1. Industrial Applications of the Microgravity Environment, Space Applications Board, 1988.
2. Space Science in the Twenty-First Century: Imperatives for the Decades 1995 to 2015: Overview, Space Studies Board, 1988.
3. Ibid: Fundamental Physics and Chemistry, Space Studies Board, 1988.
4. Towards a Microgravity Research Strategy, Space Studies Board, 1992.

WHITE PAPER ON NASA-WIDE MICROGRAVITY RESEARCH

EXECUTIVE SUMMARY

The NASA microgravity science and applications program is a fusion of the Office of Aeronautics and Space Technology (OAST) Physics and Chemistry Experiment (PACE) program and the then separate Office of Space Applications Materials Processing in Space program and was ultimately assimilated into the Office of Space Science and Applications (OSSA) as a separate discipline office. It has been managed by OSSA since 1983 as a separate science discipline division and, reflecting its origins, is still considered by many observers to be essentially a "materials processing in space program". In recent years it has become much more broadly based and scientifically oriented. It has been designated as one of the two main research activities for Space Station Freedom and has been receiving an increasing budget for flight hardware development.

Microgravity research is directed to such topics as materials science, combustion, fluid flows, heat and mass transfer, fundamental physics, and biotechnology. Thus, it represents interdisciplinary research activities in space and on the ground in which the underlying theme is the study of transport and interfacial phenomena and their effects on physical, chemical, and biological systems. Because of its inherent breadth and cross-disciplinary nature a strong and unified constituency has not yet developed. As a result of this and its early history as an applications program there is some skepticism and criticism of the program's scientific

integrity and merit.

A group of respected and experienced scientists (see Appendix) who represent most major areas of microgravity research and who participate in every major advisory group, both internal and external to NASA, together with a distinguished fluid and thermal scientist, who has had no personal involvement in NASA's microgravity research program, were assembled under the auspices of the Universities Space Research Association's (USRA) Microgravity Science and Applications Science Council to provide NASA with a more global view of the challenges and opportunities in developing a comprehensive world-class microgravity research program and to recommend what is required to meet them. This "White Paper" is a report of their deliberations and recommendations. In no way should this report be considered as an evaluation or criticism of any existing NASA programs. Such considerations were beyond the scope of the study. The purpose of the study was to take a forward-looking view, without constraints of existing NASA organizational structures, to identify rich and meaningful microgravity science and applications research so that the program can flourish and contribute to NASA's success and the Nation's welfare.

The scientific basis for microgravity research is often misunderstood and is articulated here to allay misconceptions and to entice high-quality researchers to the field so that a constituency can be developed to participate in the program and serve as its advocate. While the low-gravity environments of orbiting spacecraft and extra-terrestrial bodies present unique conditions for serious scientific study, an often overlooked critical

fact is that the reduction in gravity also directly and dramatically influences transport phenomena which play an essential role in a wide range of processes that are important in both space-based and terrestrial technologies. Furthermore, since space experimentation is both extremely costly and limited in accessibility, mission-enabling technologies (e.g., life- and operating-support systems) and commercial opportunities cannot be successfully developed unless the approaches are well coordinated and based on a scientifically sound foundation. A succession of primarily empirical investigations, however well justified each individual experiment may be, will not likely succeed. A broad-scope, well-coordinated microgravity research program is essential to provide the scientific and engineering base for such developments. Therefore, for NASA to accomplish its numerous and diverse, manned and unmanned, space missions it is essential that a productive microgravity research program should both serve the science community and also provide a practical knowledge base on which space technologies can be developed in a cost-effective, reliable, efficient, and timely manner. Such considerations will require broadening the scope of the present microgravity research program and will significantly enhance its value to NASA and contribute to the Nation's industrial competitiveness.

A number of direct needs that follow from such a viewpoint are identified herein. Some deal with scientific and technical issues, some with policy matters, and others with policy implementation and public relations. Together they indicate the need for enhanced continual interaction among the various NASA units, external scientists and engineers, and industry and advisory

bodies. To satisfy such needs working groups are recommended, viz.; discipline working groups to identify a broad range of relevant scientific topics, intra-agency groups supplemented by external scientists and engineers to define gaps in knowledge for which research is required for efficient space technology and commercial endeavors development, and interdisciplinary "tiger" teams of the most mature and experienced people in the program to help focus, prioritize, and give advice on top-level decisions which can satisfy both the desires of scientists and the needs of technologists. Cross-disciplinary workshops and special lecture series are recommended to stimulate interest in microgravity research and to disseminate the results to the various constituencies, such as industry. This will help ensure that the most significant and highest-quality research will be performed to accomplish NASA's many missions and that a constituency for the program will be molded to serve as the program's advocate, to give it credibility, and increase its visibility.

The main points made herein are that:

1. Microgravity provides an unique environment (not unlike that of ultra-high vacuum facilities, supercolliders and high-magnetic field laboratories) for conducting world-class scientific research.
2. Transport phenomena which play an essential role in many processes that are important in both space-based and terrestrial technologies are significantly influenced by gravity and the research on these phenomena in the current program is highly relevant to activities throughout NASA. This type of research is of an interactive laboratory type which requires scientists in space and is different from the observational-type research associated with the more traditional space sciences.
3. Because access to space is limited and costly, empirical means alone should not be used to develop space technologies and commercial

opportunities. Scientific scrutiny must be given to the associated processes to identify gaps in knowledge that require directed research which will enhance the timeliness, cost-effectiveness, reliability and efficiency of the developments. This scrutiny will broaden the scope of the present microgravity research program and it emphasizes that microgravity research is essential Agency-wide.

The essence of the recommendations made herein can be summarized by one word, interactions. There must be direct and continual interactions among all NASA units and with scientists and engineers both in academia and industry. Working groups (different from advisory committees) are suggested as one means of ensuring the interactions. There may well be other ways to accomplish the objectives. Time and resource limitations precluded extensive exploration of alternatives or of the implications of what is presented herein to existing NASA activities. Furthermore, it would be inappropriate to do so. What is intended herein is to bring to NASA's attention a more global and integrated view of microgravity research and to indicate its importance to the entire Agency. A great deal of experience and wisdom exists both within NASA and in the science and engineering communities and these should be integrated more closely than ever before to determine specific ways to meet the challenges and opportunities of the future.

FOREWARD

Despite three National Research Council reports^{1,2,3} that indicated numerous valid reasons for microgravity research, NASA's microgravity science and applications program has different connotations to the various constituencies on which it depends for support and implementation. To the Congress, some NASA administrators and scientists, and many researchers it is a "materials processing in space program," which reflects its early history. Space scientists (e.g., astronomers, planetary scientists, etc.) are skeptical of the microgravity program's scientific content and some openly assert that there is no scientific validity to the subject. The program has received relatively little support and visibility and, therefore, has been constrained to an effort of limited scope.

basic
science
only

In this White Paper the nature of microgravity research will be outlined and its scientific basis will be identified. It will be shown that microgravity research is broader in scope than presented in Refs. 1 to 3, is central to NASA's overall mission, and is of profound importance Agency-wide, not only to the Office Of Space Science and Applications (OSSA) but also to the Office of Commercial Programs (OCP), the Office of Aeronautics and Space

¹Industrial Applications of the Microgravity Environment, Space Applications Board, NRC, 1988.

²Space Science in the Twenty-First Century, Imperatives for the Decades 1995 to 2015: Overview, Space Studies Board, NRC, 1988.

³Space Science in the Twenty-First Century, Imperatives for the Decades 1995 to 2015: Fundamental Physics and Chemistry, Space Studies Board, NRC, 1988.

Technology (OAST), the Office of Space Exploration (OSE), and the Office of Space System Development (OSSD) which is responsible for Space Station Freedom (SSF).

In this way, the scientific relevance of microgravity research will be demonstrated and it will also be shown that applications are an essential part of those activities. The implications of this global viewpoint are that the microgravity research program must not only broaden its scientific scope but also must expand its activities to provide a knowledge-base from which space technologies and commercial applications can be more efficiently and economically developed. A corollary of this viewpoint is that the research activities and mission effectiveness in all the offices cited above (OSSA, OCP, OAST, OSE, OSSD) will be enhanced.

INTRODUCTION

The low-gravity environments aboard orbiting spacecraft and on extra-terrestrial bodies offer unique conditions for scientific inquiry and also present challenging problems and opportunities for commercial developments and for mission - enabling technologies, such as life- and operation-support systems. The main unique characteristic of low-gravity environments is that gravitational body forces are reduced. This leads to diminished buoyancy-driven flows and sedimentation and to the increased importance of surface tension at liquid interfaces. Thus, multiphase and multicomponent fluid behavior, e.g., mixing and separations, and interfacial phenomena are significantly affected. Almost all fluid transport phenomena (flows and heat, momentum, and mass transfer) are directly influenced by gravity. It must be recognized that such phenomena are vital elements in a wide range of physical, chemical, and biological systems, many of which are important in both terrestrial- and space-based technologies. Thus, if NASA's overall program in space science, exploration, transportation, and commercialization is to be successful, cost effective, reliable, and efficient it is essential that a comprehensive understanding be obtained of low-gravity fluid phenomena and extraterrestrial process operations.

Traditionally, NASA's microgravity research program has been centered in the Microgravity Science and Applications Division (MSAD) in OSSA. Although primary emphasis was originally given to materials processing, there now is an appreciation that microgravity science and applications comprise a broader

range of research activities that are to be conducted terrestrially and in space. The current microgravity research program includes such topics as materials science, combustion, fluid flows, heat and mass transfer, fundamental physics, and biotechnology.

An important aspect of microgravity research is its inherent breadth, which encompasses a number of sub-fields. Accordingly, microgravity science is not a distinct discipline, but rather is a set of interdisciplinary space research activities, in which a unifying and almost ubiquitous feature is the study of transport phenomena in physical, chemical, and biological systems. The fluid and thermal sciences, which are among the most highly developed areas in classical physics, constitute the core of such studies.

In considering new and important directions for microgravity research emphasis is given herein to transport phenomena because they are highly gravity dependent and occur in so many technologies. This focus is in no way meant to diminish the significance of other microgravity research topics. It should be noted that research on transport phenomena requires scientists to control and observe the experiments, i.e., it is of an interactive laboratory type in contradistinction to the observational research performed by space scientists.

It is the purpose of this White Paper to indicate that microgravity research is of vital importance Agency-wide, i.e., it both impacts and depends on activities not only in OSSA, but also in OCP, OAST, OSE, and OSSD. Thus, it will be shown herein that the microgravity science and applications program must be expanded in scope and depth specifically to include more emphasis on

the fundamental problems of extraterrestrial process operations. It follows then that the important microgravity research will need to be recognized and supported by all relevant offices within NASA.

CHALLENGES AND OPPORTUNITIES

The assertions made in the Introduction have implications for many offices and divisions within NASA.

Microgravity Science and Applications Division (MSAD) -

The goals of the microgravity fluids research program to be sponsored by MSAD are to:⁴

"Improve the understanding of those aspects of fluid dynamics and transport phenomena whose fundamental behavior is limited or affected by the presence of gravity, and where low-gravity experiments will allow insight into that behavior; Provide basic scientific and practical knowledge needed to design effective and reliable space-based systems and facilities which rely on fluid processes in their operations;

and,

Assist the other MSAD disciplines, such as materials processing and combustion science, by developing an understanding of those gravity-dependent fluid phenomena which could affect the success of the programs."

A number of promising research areas that were identified by the Fluids, Interfaces and Transport Discipline Working Group include capillary phenomena, multiphase flows and heat transfer, double-diffusive transport, magneto/electrohydrodynamics, colloids and nucleation phenomena, and solid-liquid interface systems. These topics are certainly interesting and highly gravity-dependent and, therefore, are of scientific significance. Moreover, to satisfy the need to broaden the microgravity research program a

⁴NASA Research Announcement, Microgravity Fluid Dynamics and Transport Phenomena, NRA-91-OSSA-17, pg. 5, August 23, 1991.

series of NASA Research Announcements was issued.^{4,5,6,7,8,9} However, if the goal of developing a knowledge base for the "design of effective and reliable space based systems and facilities" is to be satisfied then the gaps in knowledge that need to be filled to satisfy that goal must be clearly identified. This, especially with regard to technological needs, could lead to topics in addition to those cited above resulting in an enrichment of the overall program. Furthermore, the associated research must be done over the appropriate parametric ranges of operating conditions and configurations (directed research) so that the information will be meaningful for enhancing the technology design.

The direct implication is that there should be intimate and continual interaction among MSAD, the Life Sciences Division, OCP, OAST, OSE, and OSSD and also among both scientists and engineers in NASA, the universities, and industry. The last is particularly important because the industrial personnel who will design and fabricate the technologies are not as likely to read the scientific papers scattered among diverse journals and, thereby, will not be familiar with the nuances of the microgravity environment. Thus, another

⁵Combustion, NRA-89-OSSA-22

⁶Containerless Materials and Fluid Sciences, NRA-90-OSSA-23

⁷Biotechnology, NRA-91-OSSA-18

⁸Materials Science, NRA-91-OSSA-20

⁹Fundamental Science, NRA-91-OSSA-24

implication is that there must be directed outreach programs developed for industries so that they can be made aware of the differences between ground- and space-based operations and performance and include them in their designs.

Office of Aeronautics, and Space Technology (OAST) -

The mission statement for OAST¹⁰ is that it "shall provide technology for (present and) future civil space missions and provide a base of research and technology capabilities to serve all National space goals." A well managed and focused program will provide at least the following:*

Development uncertainties will be reduced so that mission safety and reliability would be increased.

Flight program development and operations costs would be reduced as would also, thereby, the cost of access to space.

Mission performance would be enhanced.

New missions would be enabled.

Developed technologies would be more broadly applicable so that national competitiveness would be improved.

¹⁰Advanced Technology for America's Future in Space: A Review of NASA's Integrated Technology Plan for the Civil Space Program, Preliminary Draft, Space Systems and Technology Advisory Committee, June 24-28, 1991.

*Paraphrased from the cited source.

Although some of the technologies required for NASA missions, such as communications systems, robotics, and specialized instruments, do not directly involve transport phenomena, many others do. Examples are: various power systems; thermal management devices and systems; spacecraft fire hazard management, cryogenic engines, fluid systems, and tankage; physical and chemical life-support systems; and more mundane user-support subsystems, such as toilets and refrigerators. More specifically, multiphase flow phenomena which are highly gravity-dependent, are central to heat transfer in many space-based systems, gas-liquid contacting for air purification and energy generation, and also will play a major role in key space related and extraterrestrial chemical processes, such as catalysis and ore beneficiation. Fundamental studies are required to quantify the effects of gravity on such phenomena prior to their application to process designs for non-earth gravity levels. Therefore, as a specific example of the types of interactions proposed herein, the MSAD and the OAST should be in close continual contact so that the latter will be familiar with the on-going research in the former and utilize the expertise which has and will be developed as a resource in the development of OAST's long-range design considerations. Accordingly, OAST should identify its planned technology developments so that the gaps in the knowledge base required can be identified in order that the MSAD can be supported to extend its basic research to those areas that will most significantly assist OAST programs. If the basic knowledge is available, then the OAST should support the required directed research for immediate application.

As a specific example of the needed interaction between technology development and basic research, safe, rational, and efficient heat transfer design for reduced gravity applications is currently not possible because fundamental knowledge of very low Reynolds number forced convection, boiling and condensation in microgravity is lacking.

NASA's long-term mission to explore the universe is also clearly dependent on understanding and exploiting the behavior of materials and processes in reduced gravity. For example, the mining, winning, and processing of extraterrestrial materials will be essential for extensive exploration of the solar system. It would be too costly to transport earth-based materials to the moon and other planets and, consequently, in situ materials must be used. Current processing techniques based on terrestrial experience will not suffice for obtaining large masses of materials under the unusual conditions encountered in extraterrestrial environments. Technologies to enable processes such as fluidized bed hydrogenation, electrolysis and electrowinning, and vapor-phase pyrolysis must be developed and they all involve transport phenomena modified by gravity. In addition, the technologies for life- and operations-support systems, such as power generation and storage, water purification, oxygen production, fuel and fluids storage and management, all involve transport phenomena that are influenced directly by gravity.

Thus, if reliable, cost-effective, and efficient technologies are eventually to be developed to enable the NASA missions on the Shuttle, Space Station Freedom, and other spacecraft then acquiring a great wealth of

fundamental understanding regarding the effects of gravity on many transport phenomena becomes essential. This understanding should be pursued in NASA's microgravity science and applications program as soon as OAST and the mission offices have defined their technology and operations needs.

Office of Commercial Programs (OCP) -

The OCP sponsors industrial research and program initiatives that encourage the participation of U.S. industry in commercial space endeavors. The OCP-sponsored Centers for the Commercial Development of Space form partnerships in which universities, non-profit institutes, and industries participate in industry-driven focused and/or applied research.

Commercial microgravity programs require directed research on subjects not very different from those pursued under basic microgravity science (MSAD) and space-technology development (OAST) because biological, physical, and chemical transport phenomena, which are highly gravity-dependent, are vital elements in the chemical, pharmaceutical, food-processing, and biotechnical industries as well as in materials processing. These transport phenomena include: catalytic reactions, adsorption/desorption at solid and liquid surfaces, dissolution and precipitation of crystals from solutions and solidification of crystals from melts, electrochemical reactions, evaporation, sublimation, condensation, etc.

Although most industrial processes and their associated equipment were developed empirically, the same Edisonian procedures cannot be followed in the

costly, complex and limited access environments of orbiting spacecraft.

Microgravity research can enable preliminary evaluation to be made of the myriad of space commercialization possibilities to screen out and focus on those likely to benefit most from a low-gravity environment as well as to provide the knowledge base for subsequent developments which serve to accelerate commercial endeavors.

Life Sciences Division (Code SB) -

Life scientists have long appreciated the effects of gravity on the structure, function, and evolution of terrestrial organisms of both the plant and animal kingdoms. Geotropism in plants and the vestibular system in mammals are but two examples that have a long history of detailed study. Other effects such as calcium resorption from bones and lymphocyte activation in the immune system have emerged as important biomedical issues only since the entry of man into space.

It is already clear that if we are to carry biological systems into space, both human and otherwise, a substantially more detailed understanding must be acquired of the effects of gravity, and its absence, on living organisms. This knowledge must be acquired not only at the organism level, for the purpose of insuring human health, safety, performance and comfort, but also at the cellular and molecular level as well. Only by penetrating the fundamental biophysical and biochemical mechanisms by which cellular processes are influenced, can we ultimately control the most visible manifestations. Accordingly, the Committee on Space Biology and Medicine of the Space Science Board defined the major goals of the scientific program to be:¹¹

1. To describe and understand human adaptation to the space environment and the readaptation upon return to earth.
2. To use the knowledge so obtained to devise procedures that will improve the health, safety, comfort, and performance of the astronauts.

¹¹ A Strategy for Space Biology and Medical Science, For the 1980s and 1990s, National Research Council, 1987.

3. To understand the role that gravity plays in the biological processes of both plants and animals.
4. To determine if any biological phenomena that arise in an individual organism or small group of organisms is better studied in space than on earth.

The structures and mechanisms by which all cells operate rely upon the same physicochemical processes of fluid flow and heat and mass transfer in nonliving systems. These processes are ubiquitous and extend to cells, tissues, organs, and entire living organisms.

To understand properly the higher physiological processes that govern, for example, fluid distribution, heat dispersion, or muscle strength it ultimately will be also essential to understand, in turn, the phenomena of transport at the fundamental level as well as the interrelationships among structure, stress and their biochemical consequences. If living organisms are eventually to be maintained in a microgravity environment for extended periods of time, then a far better appreciation and definition of how such processes are affected by gravity will be essential.

Even at this time, attempts to describe accurately fluid redistribution, bone resorption, and altered immune states in astronauts all suffer from a lack of understanding of the key physicochemical and biochemical processes operative in microgravity. Human health and protection of human physiology and tissue integrity will be the direct and important beneficiaries of microgravity research that is currently underway and planned for the future. The interrelationship of the microgravity research within the Life Sciences and the Microgravity Science and Applications Divisions (MSAD) programs should

be evident.

Another obvious area of interdependence of microgravity research between the Codes is in biotechnology. This is a classic case where the pursuit of the basic science and methodology is occurring in MSAD whereas the applications should be addressed by the Life Sciences Division. The implications of the combined activity are increasingly clear in that the rapidly emerging biotechnology industry, perhaps destined to become the prevailing industry of the early 21st century, is reliant on the development and understanding of new processes for the growth, separation and characterization of entities ranging from macromolecules to cells. Thus, techniques such as cell culture, electrophoresis, X-ray crystallography, chromatography, and isoelectric focusing have come to the forefront. Experiments are still in their early stages, but it is already clear that microgravity can be used in novel and important ways to understand and enhance these methodologies, and in some cases make possible entirely new technologies now unknown or unworkable in the presence of terrestrial gravity.

To cite one example, the value of preparing protein crystals of high states of perfection in the elucidation of protein and nucleic acid structure and the subsequent use of the conformational data for pharmaceutical design and protein engineering is now beyond question. Data are accumulating that demonstrate the benefits of a microgravity environment for growing certain macromolecular crystals which are essential for achieving rational drug design and therapeutic applications because the quality of the solid state depends on what occurs in the fluid state.

Of course, the ultimate impact of microgravity research on the improvement of human health, the development of novel treatments and medicines, the better understanding of plant responses, and the ultimate enhancement of our quality of life cannot be predicted. It is, however, certain even at this very early stage in the development of microgravity research that the information gained over the next decades can be applied to obtain benefits which may be profound and wide reaching.

SUMMARY

Microgravity science is highly interdisciplinary and broad. It consists of subjects as diverse as materials science, combustion science, physics, chemistry, and biotechnology. The common denominator are the phenomena affected by gravitational body forces, specifically, fluid flows, multiphase mixing and separations, heat and mass transfer, and interfacial behavior.

Space system designers, engineers and the ultimate users of those systems will continue to be confronted with the challenge of developing many new, enabling, and critical concepts and technologies that involve transport phenomena reacting to alien gravity environments. Unfortunately, predictive models for low-gravity performance and operation of those technologies are currently non-existent, inadequate, and potentially misleading. The same can be said of industrial programs attempting commercialization in low-gravity environments.

Normal-gravity mechanistic and/or empirical developments are not appropriate for low-gravity applications. In the past, empirical means were used to overcome difficult engineering problems encountered in the space environment, but these have been costly, time consuming, and have resulted in products or systems that are neither reliable, high-quality, nor efficient. In addition, flight opportunities are much too scarce for purely empirical approaches to advance technology in low-gravity environments. There is today a growing awareness that by giving scientific scrutiny to all technology developments many of those deficiencies could be overcome and, thereby, one

could enhance the Nation's global competitiveness. An important implication of this logic is that researchers and engineers should interact much more closely within a deliberate well-coordinated microgravity research program to identify and solve the unique problems imposed by space-based operations.

Microgravity research on transport phenomena is directed to studies in space which are of an interactive laboratory type and, therefore, differs from the observational type of space science research made of space. This indicates another significant role for "humans in space" in addition to those of exploration and spacecraft repair.

RECOMMENDATIONS

The foregoing is not meant to imply that NASA is unaware of the problems and challenges discussed. In fact, many steps have already been taken to address some of these issues. For example, MSAD has recently issued NRAs^{4 to 9} to broaden the scope of its research program. OAST has an In-Space Technology Experiments Program (INSTEP) and is suggesting the use of SSF for technology development and qualification; the OCP's CCDSs do foster the interaction of universities and industries. Also, NASA has sponsored a number of workshops dealing with some of the subjects discussed above. Therefore, what is to be presented in this section comes from an external and a more global viewpoint and is intended to identify Agency-wide needs that follow from the challenges and opportunities set forth earlier and the means of satisfying them. Also, some aspects of fostering the broader NASA-wide implications of microgravity research that have not been explicitly identified will be indicated below.

Although each of the NASA program offices mentioned above seem to be aware of the problems, there has not been a clear articulation of the fact that microgravity research is an Agency-wide necessity. Overt actions, with perhaps a few exceptions, have not been taken. The required continual and direct intra-NASA interaction are lacking. Also, only a relatively few high-qualified scientists and engineers who do research on transport and interfacial phenomena are involved in the microgravity research program either as investigators or as participants on advisory committees. The microgravity research program has low visibility to most technologists and scientists

because relatively few transport phenomena studies have been funded and there have been insufficient workshops disseminating information on such topics. Thus, there is a great need for providing greater visibility to the microgravity research program so that people with the proper expertise and who are interested in interdisciplinary research become participants at all levels. (The recent MSAD NRAs are a good first step in this direction.) There is also a critical need for greater involvement of engineers who can both identify the technological problems that will serve to broaden and focus the science program and then use the research results in their designs.

The responses recommended to meet the critical needs facing NASA are to:

- I. Recognize the relevance of low-gravity research to many of NASA's missions:
 - (a) Expand the scope of the current microgravity science program to new areas and increase its breadth in order to develop a knowledge base for mission-enabling technologies and to identify the best commercial possibilities and enhance their development.
 - (b) Develop inter-divisional and intra-Agency cooperation in microgravity research and identify scientists and engineers to help support and further such Agency cooperation.
 - (c) Obtain broader interaction and representation in all NASA units of distinguished scientists and engineers with expertise in disciplines vital to researching and resolving microgravity related problems.
 - (d) Determine the best uses of Space Station Freedom for microgravity research and for space qualification of critical technologies and testing of prototype systems for both life- and operating-support systems and for promising commercial enterprises.
 - (e) Establish a balance between fundamental and applications oriented research.
 - (f) Expand the applications for which microgravity can provide an advantageous environment.

- II. Improve NASA's flight experiment development and support structure so that it can operate in a more cost efficient and timely manner.
- (a) Simplify and unify inter-Center and Headquarters interactions and ensure close and continual involvement of the principal investigators in all aspects.
 - (b) Explore opportunities for international cooperation and technology transfer.
- III. Improve assimilation and dissemination of low-gravity research results.
- (a) Develop means of interacting with industry to obtain their input for relevant research areas and to impart to them the latest state of knowledge applicable to their responsibilities and concerns.
 - (b) Develop programs to support scientific manpower, training, and education.
 - (c) Develop user-accessible archiving of important research data and facilities information.
 - (d) Improve the visibility of the entire program by informing the appropriate scientific and engineering communities, Congress, other government agencies, and the public of microgravity research objectives and results.

Some of the above are concerned with scientific and technical issues, some with policy matters and others with implementation of policy and public relations. They all indicate the need to involve a broader range of people, e.g., more transport and biotechnology researchers, engineers, businessmen, etc., than have, heretofore, been associated with the program. Much more extensive interactions among these groups are essential, both within NASA and with an external community. Thus, the over-riding challenge is to mold a broad constituency of people with diverse disciplinary interests and scientific, technological, and industrial orientations for identifying and performing the highest-quality research and for providing the advocacy and expansion of the microgravity research program.

Discipline Working Groups have served NASA well in identifying important scientific topics. It is, therefore, recommended that other working groups (as contrasted to advisory committees) be formed as well to meet the broader range and scope of issues described above. For an example, intra-Agency groups could be formed supplemented by external scientists and engineers to define gaps in knowledge for which research is required for efficient and reliable space technology and meaningful commercial endeavors development. Interdisciplinary "tiger" teams of the most mature and experienced people in the program could help focus, prioritize, and give advice for top-level decisions. It would appear that the formation of such working groups is one good way to broaden, enrich, and enhance all aspects of NASA's microgravity research program and, at the same time, bring about the interactions among the various types of people, both within NASA and the external community, that are so necessary.

APPENDIX

USRA Microgravity Science and Applications Science Council

Dr. Simon Ostrach (Convener)
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Dr. Dudley Saville
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Princeton University
Princeton, NJ 08540

March 11, 1991

SCIENTIFIC RATIONALE FOR THE RESTRUCTURED SPACE STATION

INTRODUCTION

At the request of the Vice President, the Office of Science and Technology Policy has studied the restructured plan for the Space Station Freedom that was prepared by NASA at the direction of Congress. We have focussed our attention on the broad scientific and technological goals of the restructured program. In particular, we have reviewed the merits of the two scientific thrusts -- microgravity materials science, and life science -- that have been advanced as motivations for the Space Station project, and have attempted to place these in perspective with other goals of the program.

It bears emphasis that our study of this new design and its scientific rationale has been carried out under substantial time pressure and in many cases we have had to rely on previous reports and analyses. To the extent possible, in the time available, we have discussed these questions with a number of experts in the field. We cannot, and do not, however, claim to have conducted an exhaustive study of the questions involved; and we are perhaps more aware than most that the history of discovery in fundamental science is consistently marked by surprises.

In the FY 1991 appropriations process, Congress directed that NASA assign highest priority to the establishment of a fully equipped microgravity laboratory, in its redesign of the space station. This mandate contrasts strikingly with the Augustine

Committee's recommendation that the "Space Station Freedom be revamped to emphasize life sciences and human space operations, and include microgravity as appropriate." In view of these opposed recommendations, a careful evaluation of the motivations for the microgravity laboratory is essential.

MICROGRAVITY SCIENCE

The magnitude of the force due to gravity is a variable, together along with temperature, pressure, electric and magnetic field strength, etc., that can influence the outcome of an experiment in materials science. Any experiment that extends any of these variables into a new range is likely to yield new results. The influence of gravity is studied routinely in terrestrial experiments through the use of centrifuges that increase the force of gravity. The novelty of the space environment is that it permits researchers to study the effects of gravity at decreased levels, for extended periods of time. In the early 1980's, when the space station research program was initially formulated, there was considerable optimism for early commercial benefits. However, commercial interest in microgravity materials science experiments aboard the space station has waned over the years. Our review produced no evidence for a significant commercially-driven motivation for a space station microgravity facility, as well as a number of specific indications of decreasing interest. As a very recent (February 1991) CBO study put it, "the last ten years have confirmed that processing materials in space is an expensive, high-risk activity that is not likely to produce economic returns in the near future ... [and] consequently, the private sector has not invested substantial resources in microgravity activity".

With commercial applications no longer the driving force, interest in space station microgravity experiments has shifted to basic research. Here, the arguments for materials microgravity research aboard the space station are generically identical to those used to justify any other materials science basic research initiative, and thus the microgravity work should be evaluated according to the standard merit criteria. We have not attempted to compare in detail the scientific merits of the space station microgravity experiments with materials science experiments conducted on earth or on rockets, Spacelab, Spacehab or on other space vehicles, but we have found no convincing evidence that the space station experiments are dramatically superior to the other materials science research. (In fact, Nobel laureate and American Physical Society President Nicolaas Bloembergen recently stated, in testimony before the House Committee on Science, Space, and Technology, that "Microgravity, to be blunt, is of microimportance to science...."). It has been argued, however, that the space station will offer superior facilities, at little additional cost, for the research that will be underway aboard the Spacelab system in the mid 1990's. In this view, NASA's transfer of these activities from Spacelab to space station simply takes advantage of a "free ride", constitutes a programmatic decision, and poses no policy issues.

With regard to space station microgravity science, we conclude that (a) its commercial prospects are remote, (b) its value as fundamental science is not qualitatively superior to that of other research, (c) its attractiveness depends on the space station being constructed for other purposes. It is important to note, in addition, that many of the primary microgravity experiments cannot be conducted during periods when astronauts are assembling or inhabiting the station. Thus, materials microgravity

research would only represent a transient use of the space station. Given that microgravity research involves little incremental investment, that its benefits are similar to those offered by other basic research projects of comparable incremental cost, and that it will be a temporary activity aboard the station, this activity does not provide a significant rationale for the space station.

Having concluded that microgravity materials research is at most incidental to the space station, we turn now to the only remaining potentially important purely scientific rationale for proceeding with this project.

LIFE SCIENCE

The primary thrust of the life sciences research program for the space station is to support the manned space exploration program. There is widespread agreement that manned exploration of space depends on understanding the health effects of exposure to this environment and on developing the ability to counteract such effects. Issues of critical importance to the health of astronauts include bone and mineral metabolism (including loss of bone calcium resulting in osteoporosis); long terms results of cardiovascular adaptation to action under microgravity conditions; muscular atrophy from changes in exercise patterns; behavioral aberrations resulting from operating in a high-stress situation in a far from ideal working/living environment; and potential carcinogenicity or reproductive toxicity of exposure to radiation.

A number of studies have been conducted to identify the research that is needed to address these issues. Some of this research can be carried out on earth. However,

experience on manned missions to date, including accounts from the Soviet space program on flights of long duration, indicates that extensive manned exploration is not feasible without the support of a comprehensive clinical research program conducted in-flight. Long-term, well controlled trials, will be needed to determine whether serious, potentially irreversible effects on physiology may be avoided or ameliorated by exposure to artificial gravity, or by other means. These trials, as well as essential studies of life support systems, will require a permanently-manned space station. It is essential that data be obtained on a substantial number of human subjects to insure that effects of importance are not masked--or simulated by--individual peculiarities. Indeed, we find that a man-tended station, if that were the ultimate goal, would have little merit.

With the case for the redesigned space station thus resting squarely and solely, in our opinion, on its role, from a scientific point of view, in supporting manned space exploration, two issues require resolution. First, the current plan is ambiguous as to whether microgravity experiments would continue into the permanently-manned phase. We believe that they should not, and that the design should focus strictly on the critical space medicine experiments. In any event, the human habitation of the station is fundamentally incompatible with the requirement that the microgravity experiments be unperturbed, and the latter requirement should not be permitted to compromise the former. Second, it is the judgment of experts in the field that the questions related to supporting human life in space cannot be fully answered without the ability to control exposure to gravity. It may well be that some of the abovementioned effects will show thresholds or nonlinearities and in consequence, if

we are to be able to extrapolate observations made in the space station to longer duration exploration activities it is essential that at least one intermediate gravitational field value--between 0 and 1 g--be studied. Intermediate gravitational fields may, in themselves, be essential for long duration manned spaceflight.

We consider that the planned animal centrifuge is an essential feature of the station; however such studies must be extended to humans and thus it is essential that the design of the station not preclude a human centrifuge. The redesign has no such provision, at present.

INTERNATIONAL CONSIDERATIONS

From the outset of the space station program the U.S. has enjoyed active collaboration with scientists and engineers from Japan, Canada, Europe and Japan. In the most recent rescoping of the station, at Congressional direction, these international collaborators have been involved in detail and we are assured that the new design will satisfy their requirements.

This is an important consideration inasmuch as several of us in OSTP have been told personally--and pointedly--by the most senior representatives from Canada, Europe and Japan, that having renounced major components of their domestic space programs in order to participate in ours, should we fail to follow through on our commitments regarding the station it will be very difficult to arrange further cooperation in other areas of science and technology in the foreseeable future.

CONCLUSION

Given a national commitment to manned space exploration, the space station is needed to find means of maintaining human life during long space flights. This is its only scientific justification, in our view, and all future design efforts should be focused on this one purpose. Neither the commercial processes nor the scientific merit of the microgravity experiments come close to justifying the cost and effort required to build, deploy and operate the station; moreover, provision for neither processes nor experiments should, in any way, compromise the life science goals. The issue of compatibility of microgravity experimentation with human habitation makes it questionable whether microgravity experiments should be done at all in the space station, except during the few years when the station is being assembled, and before it becomes permanently manned. Finally, we recommend that high priority be given to including a human centrifuge in the implementation of the design.

Having considered all the above, we are convinced that the ultimate, compelling argument for the space station configured, as the Augustine Committee has suggested, is that of a first step in the great adventure that will take mankind away from the home planet for extended periods--and the true beginning of manned exploration of the solar system and beyond. Although we focus here on the importance of the life science data to be obtained from a permanently manned station, we should not forget, or minimize, the importance of developing much of the space technology and know-how required, for all subsequent exploration, in the relatively convenient low-earth orbit environment to be occupied by the station.

Without having examined the detailed engineering features of the redesigned station, we conclude that its conceptual design is appropriate to the goal of advancing man's exploration of space.

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IFSUSS Sagamihara Statement, October 19, 1989

EXECUTIVE OFFICE OF THE PRESIDENT
OFFICE OF SCIENCE AND TECHNOLOGY POLICY
WASHINGTON, D.C. 20506

August 17, 1992

MEMORANDUM FOR THE FILES

FROM: DOUG BEASON 

SUBJECT: SIMON OSTRACH'S LETTER TO DAB ON
MICROGRAVITY MATERIALS SCIENCE
RESEARCH

- Main grievance is with a "no valid science" claim by OSTP for microgravity materials science
- His White Paper describes **very generically** the importance of microgravity research
 - He concludes it mostly benefits basic science
 - His arguments are limited to:
 - 1) Microgravity provides a unique environment
 - 2) Transport phenomena are significantly influenced by gravity
 - 3) Empirical means alone should not be used to develop space technologies
- From OSTP's internal paper, **OSTP does not have a problem with this**
 - OSTP argues that because microgravity materials research will mostly benefit basic research, it should therefore be judged according to standard basic research merit criteria
 - OSTP further found that there was no convincing evidence that space station experiments are dramatically superior to other materials science research
- OSTP's main point that **microgravity science's** value as fundamental science is not **qualitatively superior** still makes sense

RECOMMENDATION: OSTP's March 11, 1991 analysis stands -- OSTP does not argue the **need** for microgravity materials science research, we only argue that it be judged according to the same standards as other basic research . . . and microgravity experiments appear not to be dramatically superior to other materials science research

STAFFING SHEET

EXTERNAL COORDINATION:

DAB signed
ltr on 2/1.
He did not
sign cover
sheet.

[illegible]

THE WHITE HOUSE
WASHINGTON

August 31, 1992

Copy letter
only!

Dear Dr. Ostrach:

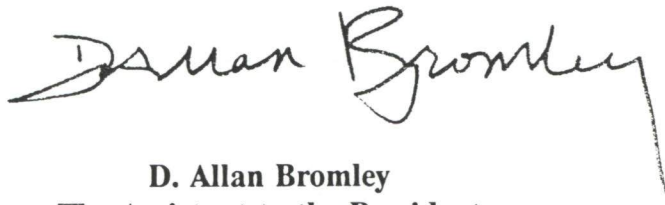
Thank you for your recent letter and accompanying materials. I am writing to correct a misunderstanding in your correspondence concerning the statement that "no valid science" could be performed in a microgravity environment.

Certainly I would not support such a statement. I have no doubt that both the validity and quality of science in a microgravity environment would be very good. I should add, however, that I do not believe that the \$40 billion space station program can be justified on the basis of microgravity science.

Rather, that justification is found in the space station's critical role in the manned exploration of space in my view. I do anticipate very interesting ancillary research as a byproduct of this endeavor.

I appreciate this opportunity to clarify my position.

Sincerely yours,



D. Allan Bromley
The Assistant to the President
for
Science and Technology

Dr. Simon Ostrach
Wilbert J. Austin Distinguished Professor of Engineering
Department of Mechanical and Aerospace Engineering
Case Western Reserve University
10900 Euclid Avenue
Cleveland, Ohio 44106-7222

"Document Control"

TYPE: ACTION DOCUMENT NUMBER: 9202027
ORIGINATOR: 02 STATUS I DIRECTORATE STATUS

FROM: FINNERTY, Michael P.

TO: DR. D.A. BROMLEY

DATE OF
CORRESPONDENCE: 05/14/92

SUBJECT: HE IS FORWARDING MATERIAL FOR DR. BROMLEY TO REVIEW
REGARDING A SYMPOSIUM ON A NATIONAL PULSED NEUTRON
SOURCE.

DIRECTORATE STAFF
ASSIGNED: PHYSICAL SCIENCES ASSIGNED:

ACTION STAFF
REQUIRED: AS NECESSARY ACTION:

SENDER'S DUE DATE:
OSTP DUE DATE: 06/17/92 STAFF DUE DATE
DATE COMPLETED: DATE COMPLETED/DEPT: 6/8/92

COPIES TO: D. Allan Bromley

WHITE HOUSE TRACKING #: CONTACT PERSON:
PHONE: EXT:

REMARKS: Dear Mr. Finnerty,

Thank you for your letter to Dr. Bromley concerning your
concept -- FRAC -- for a linear accelerator, and its possible
application as the neutron accelerator component of an intense
pulsed neutron source. We in OSTP do not conduct detailed technical reviews of new
OSTP RECEIVED: 06/03/92 DEPT RECEIVED: ~~concepts~~ ^{accelerator} concepts, and thus I am unable
FILE: P-PHYSICAL SCIENCES to comment further on your letter. ~~I should note~~
CENTRAL FILES: ~~that you are in contact with~~ ^{that you are in contact with} your concept must be qualified
by scientists with detailed experience in related areas ~~that we in OSTP do not~~
~~that you have written to~~ ^{that you have written to} ~~postpone~~ ^{postpone} I
I wish you well in your endeavors.

20
Michael P. Finerty
Finerty Consulting
APDO # 210 33 37
Zapopan, PO 45101
Jalisco, MEXICO

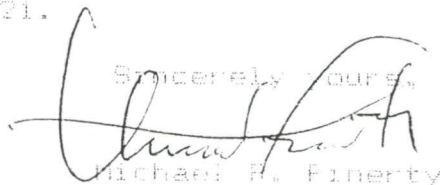
Dr. D. Allan Bromley
Scientific Advisor to the President
The White House
1600 Pennsylvania Ave.
Washington, DC
USA

14 May 1992

Dear Dr. Bromley,

Please review the enclosed materials. If you believe that a symposium on a National Pulsed Neutron Source is in the best interests of Science, Medicine and the national interest, Please communicate your support to: Dr. Michael A. Cussanovich, Vice President, Research, University of Arizona, Tucson ARIZ 85721 and to Dr. Cassidy, Head, Department of Radiation Oncology, Arizona Health Science Center, University of Arizona, Tucson, ARIZ 85721.

Sincerely yours,



Michael P. Finerty

Michael P. Finerty
Finerty Consulting
APDO # 210
Zapopan, PC 45101
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To whom it may concern

Re: TLAC and Frac accelerators and the FFPNS

I became interested in linac (linear accelerator) design while visiting the Lawrence Hall of Science in Berkeley, California in the early 1970's. Finding that I had time on my hands in 1978, I proposed to the Surgical Biology Department of the Arizona Health Sciences Center in Tucson a superconducting design of an Alvarez linac for use in Cancer Therapy, using the linac to irradiate a tungsten target to produce gamma rays. At that time, I had in mind the Tuned Linear Accelerator design, but was too disturbed to solve the velocity problem left by Albert Einstein with his Relativity, the special and general theories.

In 1986, by chance I happened to hear a lecture on food irradiation given to the Institute of Food Technology in a lecture at LSU (Louisiana State University). As my wife, Meggane Wiseman Finerty, was expecting a baby and as I, suffering from a disability induced by Army service, was expected to be the primary care giver, I found myself with time on my hands to pursue the problem of the TLAC (Tuned Linear Accelerator).

By mid-October, I had a general schematic of the electronic configuration worked out: all the cavities were to be of the same length, in order to avoid the "Beat frequency" problem, which all but cripples the Alvarez design. The gaps between cavities were to be capped by perforated end plates. There was to be a half wavelength (of the exciting radio frequency radiation applied to the end plates) connection between adjacent end plates at a single gap. The exciters were to be synchronized by using an integer multiple of the wavelength wire between grids of the exciters (modulators) and the wire between the excitor plate and the end plates was to be varied (hardwired delay) so that a pulse of charged particles coming down the cavity chain would meet a charge arriving at the leading end plate simultaneously. The result of this design was that a TLAC would come into sync several orders of magnitude per second oftener than an Alvarez accelerator.

The only difficult part was calculation the length of wire needed at each gap for the delay and to this point, Einstein was of no help. Elemental analysis later showed that the formula that he gave in chapter 15 of Relativity, the special and general theories applied to Bosons, not Fermions, and that the case of Fermions was only alluded to in passing. In exasperation, I solved the Newton Lorentz (found by substituting the Lorentz mass in the Newton Kinetic Energy Equation) formulation first. It

proved to be a quartic, quadratic in v squared. Realising that the particle in a accelerator has only three defining characteristics for the purposes of calculation (ie, rest energy, kinetic energy and charge) I was quickly able to formulate a program in Basic that would calculate the delay by noting $(c/v\lambda)$ gives the length necessary (neglecting the medium in which electricity travels.)

At that point I went to the Patent Office in Washington, where Meg's brother, Tom Wiseman, was a senior Patent Examiner, and went to work. Consulting with the Particle beam experts there, I concluded that the work was new.

A friend in Baton Rouge, David Cassell, had a father working in personnel at Lawrence Livermore National Laboratory. The father, Alfred Cassell, was looking for Mechanical Engineers, of which I am not one, when he visited for the ceremony commencing David's Doctorate in Geology. After talking with him for some time both as a guest in my house and at David's house, I gave him the little I had written up on the subject, including the program used to calculate excitor wire length for a hypothetical accelerator. As this was my last copy of the manuscript, I asked Alfred to return it when he had shown it to a physicist at LLNL.

I waited approximately three months for a reply. Then one day I found in my mail an envelope from LLNL. Inside I found a note from Al, saying, "I practically had to wrestle with my physicist friend to get this back for you! AL." Within two months Science magazine was full of news about the new linear accelerators that "are almost as good as synchrotrons."

I did not wait for the articles to appear to send a Xerox to Al for his physicist friend. I had mentioned possible Einsteinian solutions to Al during his visit. Within 2 weeks, I had worked out a "pseudo-Einsteinian" solution (found by subtracting out the rest energy from Einstein's binomial expansion of the "Kinetic Energy expression.") And I determined that it was the same as the Einstein one when one started counting the Einstein Energy from Rest energy upto Kinetic Energy, as demonstrated by two computer programs. I sent the results to Al, care of his Freemont California address, as I did not want curious eyes intercepting my message.

At this time I turned my attention to the problem of accelerating Cations, having designed a TLAC of 300 MHz and a cavity length of 95 cm (5 cm gap.) I found to my surprise that the TLAC required 12.48 meter cavities and a 48 MHz excitor frequency for proper acceleration in the critical first gaps of the accelerator. The heavier the ion, the longer the cavity and the lower the excitor frequency. But, something struck me: using the TLAC as an injector, one could fill the ring of the "Superconducting Super collider" with bunches of protons every few meters instead of just two counter-circulating bunches interacting every now and then. (This observation has made it necessary to design a whole new series of detectors for the SSC.) I sent my

analysis to Al.

While waiting for a reply (which never came), I set about solving the TLAC problem for any ion at any state of ionization. This accomplished, I sent my program, IONS.BAS to Al, again at his Fremont California address.

The entire time that I worked on this problem I was taking Loxitane for a service connected psychosis. Every time I ran into a road block, I dropped my dosage by 5 mg. The following day I was useless, but starting from the second day and continuing for a week, I had the experience of "psychic energizerization" as previously described by Freud for Cocaine. Eventually this caught up with me, and rather than seek a patent as I had planned, I had to leave my daughter in care of a friend and seek psychiatric help in Tucson.

The problem with the TLAC for heavy ions is that the cavities become unweildy and long. In Tucson 1988, I solved this problem with the Fractional Wavelength Tuned Linear Accelerator or FRAC. After one false start, it proved simple: one merely had to change the excitor lengths for shorter cavities. I sent out copies of the solution and waited. This time, I sent copies to Art Robinson at the ALS as well and to Hellen Edwards at the SSC. When Hellen could make no progress with the management of the SSC over injector design, she resigned.

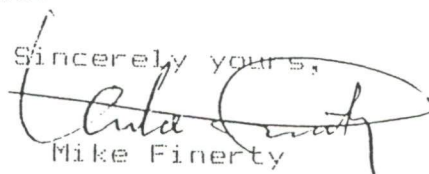
In the meantime, Congress appopriated money to start construction of the Relativistic Heavy Ion Collider at Brookhaven and someone published my idea of coliding piggybacked neutrons at the SSC by colliding deuterons and triterons as well as proteum. Fine and good: That is all history.

More recently, I have been promoting a Frac Pumped Pulsed Neutron Source, using a 800 MeV FRAC and a Uranium (238) or tungstun (W) target for cancer research, Accelerator Transmutation of wastes (ATW) and as a high flux neutron source for testing components of the magnetic confinement controled fusion experiments. It will cost around \$500,000,000 and will stretch two miles across the desert.

In October of 1990, Scientific American published an article on Boron neutron capture and cancer therapy. The low thruput of existing pulsed neutron sources (50 to 60 pulses a second) is due almost entirely to the ramp up time of the synchrotron booster used to reach energies with good spallation characteristics in the available target materials. The answer is a FRAC that comes into synch 50,000,000 times a second for protons and has no ramp up time at all. Obviously if all the heat of spallation is retained by the target, we can expect an explosion of some magnitude, if the thing is run continuously; however, as for all the experiments mentioned above a 100 fold over current increase is desired, I am quite sure a FRAC is the way to go.

The problem with Boron Neutron Capture Therapy for Cancer is no longer the lack of a suitable design. The most pressing problem is cancer specific boron compounds localizing themselves in tumorous tissue and staying put. At the very least boron injection in a procaine doped vehicle can be used in inoperable brain tumors with the device mentioned. If boron can be tagged onto fetal antigen antibodies so as not to destroy the specificity and activity of those molecules, we may finally have a treatment for deceminated cancers of the liver and bone. For my money, it is worth a chance. It will not be my highest priced invention but \$500,000,000 is nothing to sniff at.

Sincerely yours,

A handwritten signature in dark ink, appearing to read "Mike Finerty", written over a horizontal line.

Mike Finerty

Guadalajara, May 11, 1992

Michael P. Finerty
Finerty Consulting
APDO # 210
Zapopan, PC 45101
Jalisco, MEXICO

Dr. Cassidy
Head, Department of Radiation Oncology
Arizona Health Sciences Center
University of Arizona
Tucson, ARIZ 85721 USA

Dear Dr. Cassidy,

Dr. Tim Hunter of the Radiology Department of The Arizona Health Sciences Center (and former president of the Tucson Amateur Astronomers Association) suggested that I get in touch with you.

I design Linear Accelerators as a hobby. Recently, several articles in Science and Scientific American have called for a better neutron source. For this reason, I think it is time for a symposium on a "National Pulsed Neutron Source, its Design and Uses."

The Linacs, TLACs and FRACs, that I design are high thruput devices. While they produce particles of the same energy as a comperable Alvarez linac, they are high flux devices, producing many more particles per second than an Alvarez linac because they have no "beat frequency."

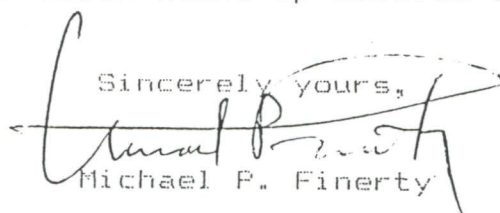
Recent advances at Ohio State University in the treatment of cancer by "Boron Neutron Capture Therapy" suggest that surgury assisted by this particular kind of radiation therapy lengthens lives (see Sc Am 1990.) I talked with Dr. Klaus Brendel of the Pharmacology Department of the Arizona Health Sciences Center about Boron drugs in use for this purpose. Apparently the Japaneese have produced a Buckyball type molecule to attach to various haptors to treat cancer.

I have kept Dr. Douglas Donahue, former head of the Physics Department, and Dr. Norval A. Sinclair, former acting head of the Department of Microbiology and Immunology, of the University of Arizona current on the status of my designs, particularly of the FRAC (FRACtional Wavelength Tuned Linear Accelerator) which I believe would make a really beautiful pulsed-neutron Proton source. As the desired fluxes of protons are only 2 orders of magnitude greater than those available from an Alvarez linac, I believe that the FRAC design is the most desirable for a National Pulsed Neutron Source, having a theoretical maximum of 6 orders of magnitude over current plsed neutron sources. (See Contemporary Physics two or three years ago.)

I am therefore taking the liberty of advis-

ing several people of the desirability of a Symposium on pulsed neutron sources and asking them to get in touch with you and Dr. Michael A Cussanovich, Vice President in charge of Research at the UofA. Due to financial constraints, I will be here in Mexico until 30 August 1992. I suggest that the first week in October would be a good time to hold the meeting. I am sure that Drs. Sinclair, Brendel, Donahue and Cussanovich could get together with you to discuss meeting and arrange for the setting up of a committee to look into it.

To me, it would be a highly desirable outcome if a National Pulsed Neutron Source were to be set up near Tucson under the direction of the University of Arizona. Tucson needs the jobs that it would provide and Tucson should be given first shot at "Desertron II," which may in the end prove more useful than Desertron I (the SSC) which wound up outside of Dallas.

Sincerely yours,

Michael P. Finerty

Zapopan, May 13, 1992

List of recipients of Proposal for a Symposium on the Subject of
National Pulsed Neutron Source FFPSS

Drs: Cassidy, Radiation Oncology UofA
Klaus Brendel, Pharmacology, UofA
Norval A Sinclair, Microbiology and Immunology, UofA
Douglas Donahue, Physics, UofA

Vice President, Michael A. Cussanovich, Research, UofA

Dr. D. Allen Bromley, Scientific Advisor to the President,
The White House, Washington, D.C.

Dr. James Watkins, Secretary of Energy, Energy Department, Wash-
ington, DC

Director, Lawrence Livermore National Laboratory, Livermore CA

Director, Ronald Reagan National Laboratory (SSC), Dallas, Texas

Director, Brookhaven National Laboratory, Brookhaven, NY

Milos V. Chvapil, Surgical Biology, UofA
Tim Hunter, Radiology, UofA.

Michael P. Finerty
Finerty Consulting
AFDO # 210
Zapopan, PC 45101
Jalisco, Mexico

Dr. Douglass Donahue
Physics Department
University of Arizona
Tucson, ARIZ 85721
USA

Dear Douglass,

On considering how to proceed on the National Pulsed Neutron Source, I decided to ask Dr. Cassidy to throw a symposium. I gave him your name as a possible warm body for the steering committee. A FFPNS would give the UofA a leg up in Physics and Engineering as well as in Medicine.

On the physics side, I am volunteering to give a short talk on FRAC and TLAC design, including deriving on a blackboard the relativistic velocity of bosons and fermions, something that I have searched for unsuccessfully in the literature. As you may well appreciate, knowing how fast a particle is going is the first step in figuring out how best to accelerate it.

Things go well here. Mom is off visiting my sister in Nashville. When she returns, I will go to the mountains where I have a couple of water projects and two baptisms to attend (I am to be Godfather to a couple of Huichol kids.)

Anyway, If you can find the time and believe that a symposium is in the best interests of your department and Science in general, please give Drs. Cassidy and Cussanovich a call and voice your support.

My best to you, Bea, Patsy, Paul and Ben,

Mike

Guadalajara Phone: 0115236-332109

Michael P. Finerty
Finerty Consulting
AFDO # 210
Zapopan, PC 45101
Jalisco, Mexico

Dr. Norval A. Sinclair
Department of Microbiology and Immunology
University of Arizona
Tucson, ARIZ 85721
USA

Dear Dr. Sinclair,

Please review the enclosed materials and contact Dr. Cassidy and Dr. Cussanovich if you think that a symposium at this time is in the best interests of Science and Medicine. I have given Dr. Cassidy your name as a possible body for the steering committee.

I imagine that if the symposium takes place it will be necessary to edit the proceedings. Would you be willing to help in this regard?

Immunology comes into play in that there have been attempts to attach boron molecules to tumor specific antibodies for irradiation with epithermal neutrons. The short range of the alpha particle radiation so produced is adequate to kill tumor tissue but to leave surrounding tissues intact. Unfortunately, so far, the boron compounds appear to bind also to the active site on the antibody (the variable region on the arms), so that the specificity of the antibody is lost. I am sure that this problem is not insurmountable and that a non-specificity-blocking chemical reaction will be found to attach boron to these molecules, given enough time and scientists.

Anyway, the symposium will be the fruits of my labors on Linacs. I hope to see you in September.

Mike

Guadalajara phone: 0115236-332109

Michael P. Finerty
Finerty Consulting
APDO # 210
Zapopan, PC 45101
Jalisco, MEXICO

Dr. Klaus Brendel
Department of Pharmacology
Arizona Health Sciences Center
University of Arizona
Tucson, ARIZ 85721
USA

Dear Klaus,

I think that there is merit to your suggestion of modifying the SSC injector to make a FPPNS; however, there is so much research to be done using a dedicated source that I am suggesting a dedicated facility in the desert near Tucson.

If you would be willing to serve on a committee setting up a symposium on this matter, Please call Dr. Cassidy in Radiation Oncology and volunteer.

If you would be interested in giving a paper on "the Pharmacology of Boron compounds for use in Epithermal Neutron Therapy," or be interested in helping to edit the proceedings of the symposium, please also volunteer this information.

I hope to see you in September, keep well

Mike

Phone in Guadalajara 0115236-332109

Michael P. Finerty
Finerty Consulting
AFDO # 210
Zapopan, PO 45101
Jalisco, MEXICO

Dr. Michael A. Cussanovitch
Vice President, Research
University of Arizona
Tucson, ARIZ 85721
USA

May 14, 1992

Dear Dr. Cussanovitch:

Please be advised that the University of Arizona is proposed as a location for a symposium on "The National Pulsed Neutron Source," and that I am proposing that the UofA build such a device in the desert near Tucson.

800 MeV neutrons impinging on a U238 or Tungsten (W) target release approximately 26 spallation neutrons at around 10-14 MeV per proton.

Until recently, proton accelerators have been low thruput devices; however, designs now exist (the TLAC for electrons and the FRAC for cations) which make it possible to design and build high thruput linacs, suitable for the purpose of a high flux pulsed neutron sources.

If such a device is built by the University of Arizona with Government funding, there will be a large augmentation of the graduate programs in Immunology, Physics, Chemistry, Radiation Oncology and Pharmacology as well as Nuclear and Mechanical Engineering.

I urge you to take on the task of Chairman of the Steering Committee for the Symposium and Editor in Chief of the Proceedings of the Symposium to be published by the U of A press.

Accelerators of this nature are not cheap and the price on this one may well run a half a billion dollars, when laboratory and clinical plants are included. The research conducted at such a facility would augment programs in controlled fusion, radiation oncology and waste disposal. I would like to see the work done at the UofA, as it is my alma mater.

Sincerely yours,

Michael P. Finerty

Class of 1965

Michael P. Finerty
Finerty Consulting
APDO # 210
Zapopan, PC 45101
Jalisco, MEXICO

Dr. D. Allan Bromley
Scientific Advisor to the President
The White House
1600 Pennsylvania Ave.
Washington, DC
USA

14 May 1992

Dear Dr. Bromley,

Please review the enclosed materials. If you believe that a symposium on a National Pulsed Neutron Source is in the best interests of Science, Medicine and the national interest, Please communicate your support to: Dr. Michael A. Cussanovich, Vice President, Research, University of Arizona, Tucson ARIZ 85721 and to Dr. Cassidy, Head, Department of Radiation Oncology, Arizona Health Science Center, University of Arizona, Tucson, ARIZ 85721.

Sincerely yours,

Michael P. Finerty

Michael P. Finerty
Finerty Consulting
APDO # 210
Zapopan, PC 45101
Jalisco, MEXICO

Dr. James D. Watkins
Secretary
Department of Energy
Washington, DC
USA

May 14, 1992

Dear Secretary Watkins,

Please review the enclosed materials regarding a symposium on a National Pulsed Neutron Source. If you find that the proposal is in the best interests of the United States Government, Science and Medicine, please communicate your support to Dr. Michael A. Cussanovich, Vice President, Research, University of Arizona, Tucson, ARIZ 85721 and to Dr. Cassidy, head of the Department of Radiation Oncology at the same University.

A reasonable estimate of the cost of such a venture is a half of a billion dollars, when salaries and laboratory facilities are counted in. I expect that the Congress would expect the Energy Department to take charge of the project, meaning that the project would either go to an existing National Laboratory or that a new National Laboratory would have to be set up outside of Tucson.

Hughes Tool, located in Tucson, has long supplied sophisticated materials to the Airforce, including servicing atomic weapons attendant to the Titan Missile sites surrounding the city. I am sure that with the proper incentives that they could undertake and complete the construction of a FPPNS for the National Pulsed Neutron facility, if asked to do so.

I would have sent this up through channels, but Al Cassell, my contact at LLNL has proven refractory with my last two missives.

Sincerely yours,

Michael P. Finerty MA MA

Michael P. Finerty
Finerty Consulting
APDO # 210
Zapopan, PC 45101
Jalisco, MEXICO

Director
Lawrence Livermore National Laboratory
Livermore, CA
USA

May 14, 1992

Dear Sir,

If after a review of the enclosed materials, you are of the opinion that a symposium on "A National Pulsed Neutron Source (FPPNS)" is in the best interests of the USA, Science and Medicine, Please contact Dr. Michael A. Cussanovich, Vice President, Research at the University of Arizona, Tucson (85721) and Dr. Cassidy, Head Radiation Oncology, UofA.

If the 150 man accelerator team that was transferred after I wrote Mr. Cassell, formerly of personnel and when last heard from in Administration at LLNL about the TLAC is still in existence, design of the FPPNS is a suitable task for them.

As your man, Mr. Alfred Cassell, has training in History but not in Physics or Mathematics and has stoped receiving my mail, I suggest the following configuration: a 2.75 meter cavity length, standard, a 25 cm gap, standard, 1 MV excitors operating at a frequency of 50 MHz, a U238 target, raster scanned by superconducting magnets diffracting the beam horizontally and veritcally by at least 1 meter at the target. The target is to be water cooled and tungesten jacketed. Moderation of the neutron beam is to be moderated by water bath and/or carbon block (graphite). The FRAC configuration sent to Mr. Cassell at 48213 Cottonwood, Fremont, CA 94539 will suffice. As the intent was that Mr. Cassell was to communicate my findings to his "physicist friend" at LLNL, all letters from me to him are property of the University of California and may be disposed of as you see fit.

Sincerely yours,

Michael P. Finerty

Michael P. Finerty
Finerty Consulting
APDO # 210
Zapopan, PC 45101
Jalisco, Mexico

Director
Ronald Reagan National Laboratory
Superconducting Super Collider
Dallas, Texas
USA.

Dear Sir,

The enclosed is for your information.

If on a careful review of the materials contained here in and a careful investigation you are of the opinion that a dedicated National Pulsed Neutron Source is doable and desirable, please contact Dr. Michael A. Cussanovich, Vice President, Research, at the University of Arizona, Tucson (85716) and Dr Cassidy, Head, Radiation Oncology at the Arizona Health Sciences Center, Tucson, voicing support for a symposium on the matter.

Sincerely yours,

Michael P. Finerty

Michael P. Finerty
Finerty Consulting
APDO # 210
Zapopan, PC 45101
Jalisco, MEXICO

Director
Brookhaven National Laboratory
Brookhaven, NY
USA

May 14, 1992

Dear Sir:

The enclosed is for your information.

I on review and investigation you are convinced that a symposium on a National Pulsed Neutron Source using a FRAC pumped pulsed neutron generator is in the national interest, please contact Dr. Michael A. Cussanovich, Vice President, Research at the University of Arizona in Tucson (87521) and Dr Cassidy, Head of the Department of Radiation Oncology at the Arizona Health Sciences Center run by the same institution.

Sincerely yours,

Michael P. Finerty

Michael P. Finerty
Finerty Consulting
AFDO # 210
Zapopan, FC 45101
Jalisco, MEXICO

Dr Tim Hunter
Associate Professor, Radiology
University of Arizona
Tucson, ARIZ 85721

Dear Tim:

Please have Terry send me the adress of the TAAA treasurer so that I can renew my membership.

The enclosed is for your information. If you would like to take part in the symposium, serve on a committee or any thing, call Dr. Cassidy and volunteer.

Hope all is well with you. I should be back in time for the September meeting.

Sincerely yours

Mike Finerty

Michael P. Finerty
Finerty Consulting
APDO # 210
Zapopan, PC 45101
Jalisco, MEXICO

Dr. Milos V. Chvapil
Department of Surgical Biology
University of Arizona
Tucson, ARIZ 85721
USA

14 MAY 1992

Dear Milos,

I followed the various leads that you suggested to no avail. But I thought that you would be interested in this. If you are sufficiently interested the Steering Committee could probably use your aid (if this thing flies) and you might call up Dr. Cassidy and volunteer.

This is a long way from Collagen but the surgical approach augmented by boron-neutron capture radiation therapy (boron (10) releases short range alpha particles (high energy helium nuclei) on irradiation by relatively slow neutrons) has proven useful in the treatment of some neoplasms. Other uses for the machine are testing components for controlled fusion experiments and Accelerator Transmutation of Wastes proposed by scientists at Los Alamos.

The excellence of the programs at the U of A Hospital for oncology and cancer treatment suggest that the desert outside of the UofA is the proper place for this device. You are a witness to my priority in accelerator design. So, if you have the time, Please contact Dr. Cassidy and volunteer. We need your brain.

My best to you,

Mike

EXECUTIVE OFFICE OF THE PRESIDENT
OFFICE OF SCIENCE AND TECHNOLOGY POLICY
WASHINGTON, D.C. 20506

June 8, 1992

Dear Mr. Finerty:

Thank you for your letter to Dr. Bromley concerning your concept--FRAC--for a linear accelerator, and its possible application as the proton accelerator component of an intense pulsed neutron source.

We in the Office of Science and Technology Policy do not conduct detailed technical reviews of new concepts, and thus I am unable to comment further on your letter.

I wish you well in your endeavors.

Sincerely,

A handwritten signature in dark ink, appearing to read 'Karl A. Erb', with a stylized flourish at the end.

Karl A. Erb
Associate Director--Designate
for Physical Sciences and Engineering

Mr. Michael P. Finerty
Finerty Consulting
APDO #210
Zapopan, PC 45101
Jalisco, MEXICO