NASA Conference Publication 10094

INTERNATIONAL WORKSHOP ON VIBRATION ISOLATION TECHNOLOGY FOR MICROGRAVITY SCIENCE APPLICATIONS

Proceedings of a conference held at the Middleburg Heights Holiday Inn
Sponsored by National Aeronautics and Space Administration
Office of Space Science and Applications
Hosted by NASA Lewis Research Center
Space Experiments Division
Cleveland, Ohio
April 23–25, 1991

NASA

NASA Conference Publication 10094

INTERNATIONAL WORKSHOP ON VIBRATION ISOLATION TECHNOLOGY FOR MICROGRAVITY SCIENCE APPLICATIONS

Proceedings of a conference held at the Middleburg Heights Holiday Inn
Sponsored by National Aeronautics and Space Administration
Office of Space Science and Applications
Hosted by NASA Lewis Research Center
Space Experiments Division
Cleveland, Ohio
April 23–25, 1991

NASA
National Aeronautics and
Space Administration

Office of Management

Scientific and Technical
Information Program

1992

PROCEEDINGS

of the

INTERNATIONAL WORKSHOP ON VIBRATION ISOLATION FOR MICROGRAVITY SCIENCE APPLICATIONS

Proceedings of a Workshop, sponsored by the National Aeronautics and Space Administration Office of Space Science and Applications and held under the auspices of the NASA-Lewis Research Center, Cleveland, Ohio on April 23-25, 1991.

PREFACE

The International Workshop on Vibration Isolation Technology for Microgravity Science Applications was held on April 23-25, 1991, at the Holiday Inn in Middleburg Heights, Ohio. The workshop was sponsored by the National Aeronautics and Space Administration Office of Space Science and Applications and was held under the auspices of the NASA-Lewis Research Center in Cleveland, Ohio. The major objective of this conference was to explore vibration isolation requirements of space experiments and what level of vibration isolation could be provided both by present and planned systems on Space Shuttle and Space Station Freedom and by state-of-the-art vibration isolation technology.

Over 80 individuals attended the workshop, representing a broad spectrum of experts from industry, universities and NASA, and including representatives from both Europe and Japan. The two day session comprised 16 presentations, represented by the papers printed herein, followed by panel discussions held by two separate working groups. After the final working group session, summaries of the working group meetings were given to a plenary session to conclude the workshop.

A transcript of each workshop working group discussion session, based on a court stenographer's record of each session, is included herein. In developing these transcripts some loss of content may have occurred in translating the sessions from the audio tapes and stenographer transcriptions; however we have tried to preserve both the general tone and technical content. The editor apologizes for any oversights or omissions that occurred in the translation, and for any errors that may have been introduced.

Much of the content of this publication came directly from handouts of the speakers at the workshop and the quality, particularly in some illustrations, is not optimal. In some instances, speakers were able to provide the conference organizers with amended versions of their presentations after the workshop. These should be helpful in better understanding the context of the presentations, and the workshop organizers are grateful for these submittals.

The purpose of most workshops is to develop a common dialogue between parties with mutual technical interests, and in so doing, to identify key issues and potential problem areas. This was accomplished in this workshop. The organizers wish to thank those who participated in making the workshop a productive and thought-provoking experience.

Joseph F. Lubomski
Workshop Organizer/Proceedings Editor
NASA Lewis Research Center
Mail Stop 500-217
21000 Brookpark Road
Cleveland, OH 44135
Office Phone (216) 433-3907

TABLE OF CONTENTS

Section/Page

TABLE OF CONTENTS (CONT.)

SECTION I

EXECUTIVE SUMMARY

EXECUTIVE SUMMARY

Introduction

A fundamental advantage of doing materials and fluid physics science experiments in a space environment comes from the reduced gravitational force field, whereby the gravity-driven forces normally encountered in an Earth-based laboratory environment are greatly reduced in a space environment. This presents an unique and beneficial advantage if properly used. However, experience with manned spacecraft, such as the Space Transportation System, has shown that the acceleration environment in a spacecraft relative to g-jitter disturbances is not as low nor as pure as would be desired for sensitive science experiments. Vibrations and transient disturbances from crew motions, thruster firings, rotating machinery, etc. can have detrimental effects on some proposed microgravity science experiments. These same disturbances are also expected to be encountered on Space Station Freedom (SSF).

The Microgravity Science and Applications Division (MSAD) of the Office of Space Science and Applications (OSSA), NASA Headquarters, recognized the need for addressing this issue. An Advanced Technology Development (ATD) Project was initiated in the area of Vibration Isolation Technology (VIT) to develop methodologies to meet future Microgravity Science needs. This effort is coming to a conclusion with a successful demonstration of an actively controlled, six degree-of-freedom, magnetic isolation system in the low gravity environment of parabolic aircraft flight tests. The workshop discussed here is the second conducted in this effort. The first workshop, held in September 1988 during the first year of the project, was conducted to ascertain the state-of-the-art of isolation technology, to determine the perceived science requirements for vibration isolation, and then to organize the Vibration Isolation Technology Project to best meet the science needs. The workshop discussions were centered around two working groups -- a Science and Users Group and a Technology Group.

The Science and Users Working Group concluded that there were two principal issues. One issue concerned the microgravity environment and recommended a systematic documentation in a meaningful data format of the existing environment onboard shuttle and an early definition of the proposed SSF Environment. A strong recommendation for source control for SSF was made similar to the approach proposed for the European Free-Flyer Eureca. A second issue regarding requirements had two parts. First, it was recommended that users should address "real" science needs systematically and realistically, and secondly, that the engineering limitations on meeting these needs must be defined, especially with regard to the impact of umbilicals.

The Technology Working Group's highest recommendation at that workshop was that VIT be developed to extend capabilities into the sub-Hertz frequency, microgravity range, and that this technology should be demonstrated. In conjunction with this, actuator technology to support the control developments must be successfully demonstrated within a multi degree-of-freedom system in a low-gravity environment. The limitations of passive isolation should also be considered. It was also recommended that the problem of umbilicals be addressed, the use of non-contacting methods be encouraged, as well as characterizing spring rates of other umbilicals. The use of umbilicals on sensitive experiments should be evaluated early in the design to minimize their effects and control strategies to cancel these umbilical effects should be explored. Using these findings, the Vibration Isolation Technology Project (ATD) was focused on the high priority recommendations. Concurrently, as was soon discovered, other efforts were initiated throughout the World Space community to accomplish similar goals. Coordination was established between participants to keep abreast of developments. Eventually, as it became obvious that a considerable amount of work was being carried out in the area of Vibration Isolation Technology for Microgravity Science, an International Workshop sponsored by MSAD and hosted by the NASA Lewis Research Center's Space Experiment Division was held in Cleveland, Ohio in April of 1991. The purpose of this workshop was to generate a dialogue to specifically evaluate the relevance of the current work in progress, and to make recommendations as to what needs must be addressed in the future to create a meaningful microgravity environment to assure productive international microgravity science programs. The subject matter and results of this Workshop are summarized herein.

Summary of Workshop

The workshop had 80 attendees, representing U.S. and international industry, U.S. and international universities and several governments. Seven NASA installations were represented, as were the Canadian Space Agency (CSA), the European Space Agency (ESA), and the Nippon Space Development Agency of Japan (NASDA). The presentation part of the workshop consisted of four sessions.

Session 1: Session 1 was dedicated to the "Sensitivity of Microgravity Science Experiments." Two presentations were made summarizing current NASA efforts: (1) in the area of numerical modelling to predict the behavior of fluid experiments and protein crystals exposed to g-jitter, and (2) an examination of the anticipated g-jitter effects on Space Station Freedom. The results of these studies do indicate that g-jitter will impact sensitive science experiments; it will be a three dimensional multifrequency phenomenon and will vary in orientation. The fundamental understanding of heat and mass transport, as well as fluid phenomena is still not well understood in microgravity. It was recommended that a sound, coordinated experimental/numerical effort with fully characterized conditions be undertaken. Also, consideration should be given to alternate environments for more sensitive processes, e.g., free flyer.

Session 2: Session 2 was dedicated to "Isolation Technology Development," which was the main theme of the workshop and thus the longest session. Eight presentations were made summarizing the work being sponsored by ESA, CSA, NASDA and NASA in the area of Vibration Isolation Technology for Microgravity Science Experiments. A common element in all of the programs was the use of active, magnetic isolation techniques. There were variations in controller concepts and types of actuators, but the selection of these components will be a function of the particular application. The scope of each technology presentation is outlined below.

The ESA's major effort is the development of the Microgravity Isolation Mount (MGIM), which is a facility for providing active vibration isolation for sensitive experiments to be flown on the Columbus Attached Laboratory and the Columbus Free-Flyer Laboratory. The facility is designed to be accommodated in a standard Columbus rack, and interfaces with existing rack utility services. The facility design is based on a non-contact strategy, which includes services to the experiment. The concept was developed for ESA by a team at the University College of North Wales in the United Kingdom. This facility is the only known microgravity science facility being developed to consider the effects of g-jitter on the science payload.

The CSA's work in progress involves the development of a Large Motion Isolation Mount (LMIM) for providing a high quality environment of 10^{-4} g for 5 to 15 seconds on the KC-135. The work is being conducted by the Canadian Astronaut Program Office with the University of British Columbia. CSA and NASA/MSAD are sponsoring the work, with NASA/JSC and NASA/MSFC participating.

NASDA has an extensive vibration isolation program in progress to develop isolation concepts for use in Japanese Experiment Module (JEM). A unique aspect of the NASDA effort includes an investigation into rack passive damping methods, as well as investigating active, electromagnetic methods for isolating the payload. Validation of the performance of the various concepts being developed has been done using both ground-based laboratory testing and low gravity aircraft flights. In principle, the NASDA work in progress in active magnetic isolation is similar to the NASA Vibration Isolation Technology ATD in-house effort.

The NASA work in progress that was discussed has several elements, most of which are being done within the MSAD-sponsored ATD. The in-house work being conducted at the Lewis Research Center has the objective of developing and demonstrating the proof of concept of a six degree-of-freedom active, magnetic isolation prototype-system for low frequency application. This was done by developing the necessary control and actuator concepts in a laboratory, building a laboratory six degree-of-freedom prototype for validation of performance, and then building a prototype system that was flown in low gravity flight tests. In addition to the in-house work, grants were funded to two universities.

A grant with Pennsylvania State University investigated digital control algorithms for microgravity science isolation systems. This resulted in a new method for controller design algorithms with improved performance over the conventional phase lead/lag method. Using the methodology developed, the controller transfer functions are determined for a specified transmissibility. In theory this assures that in the frequency domain the transmissibility will be below its upper bound.

The University of Virginia is also conducting research and development under a grant. The work being done includes:

- developing a concept for a compact, large stroke Lorentz Actuator and experimentally evaluating its performance characteristics;
- investigating additional controller concepts, including development of optimal control laws;
- investigating the use of the Stewart Platform concept as a means for isolating a science payload.

There are also two Phase II SBIR's (Small Business Innovative Research) being funded through Code C that are contributing to NASA's Vibration Isolation Technology effort. NASA Lewis is managing a Phase II SBIR conducted by Applied Technology Associates of Albuquerque, New Mexico, which is developing an innovative inertial actuator concept for stabilization in micro-gravity. The inertial actuator concept is best suited to control direct disturbance from entering the environment, e.g., isolating exercise equipment. NASA Marshall Space Flight Center also had a Phase II SBIR that was conducted by SatCon Technology of Cambridge Massachusetts. That effort developed a six degree-of-freedom Lorentz-force vibration isolator with a nonlinear controller. The concept was validated in the laboratory by off-loading the weight of the isolated platform by hanging it from a spring.

After Session 2, a special report was presented on NASA's Acceleration Characterization and Analysis Program (ACAP), which is funded from NASA Headquarters by MSAD and is managed by the Marshall Space Flight Center. ACAP was established to assist investigators and mission scientists in understanding and evaluating the microgravity environment of experiment carriers for NASA. ACAP performs and/or coordinates data analysis and serves as the Project Scientist for NASA flight accelerometers. ACAP is responsible to MSAD for organizing scientific analysis of the effect of the mission environment on microgravity science objectives.

Session 3: The theme for session 3 was "Microgravity Environment". Two presentations of vital interest were made concerning the effects of cyclic exercise equipment onboard the shuttle and the Space Station Freedom Environment. Dr. William Thornton of the Astronaut Office made a presentation entitled "Shock and Vibration Isolation for Cyclic Exercise in Space Craft." The need for cyclic exercise was discussed and the resultant disturbing forces of the various exercises were presented. Concepts for isolating and minimizing the effects of these forces were also presented. Disturbances generated by exercise equipment are direct disturbances that, as stated previously, are best controlled or stabilized by using inertial actuators. It was concluded that for long duration space flight, cyclic exercise is mandatory, but will need source-isolation to minimize the effect on the space environment.

The second presentation of this session was prepared by Level II of the Space Station Freedom Office and was entitled "Space Station Freedom Microgravity Environment Requirements and Assessment Methods." There was considerable interest in this subject. The program status and the Space Station Freedom microgravity requirements were discussed, as well as quasi-steady, low frequency and vibro-acoustic assessment techniques. A spirited discussion on this subject carried over into the working group sessions.

Session 4: Session 4 was entitled "Microgravity Measurements" and consisted of three presentations. A presentation on the Space Acceleration Measurement System (SAMS), entitled "Early Mission Science Support," described the SAMS hardware, discussed the capabilities of SAMS and detailed the configurations to be used in the missions over the next two years.

The presentation entitled "Microgravity Accelerometer Characterization on Columbia STS-32 Mission" discussed the use of the Honeywell In-Space Accelerometer (HISA) on the STS-32 Mission in support of the Microgravity Disturbance Experiment (MDE). A description of the HISA, along with the principle of operation and performance specifications was given. The objective of the MDE was to investigate the effects of various disturbances (e.g., crew motion, treadmill operation, thruster firings) on the microstructure of Indium crystal grown using a float-zone method. The Fluid Experiment Apparatus(FEA) was used to grow the crystal and the HISA, mounted on the front side of the FEA, measured and recorded the disturbance levels.

The final presentation in this session, entitled "Development of a Residual Acceleration Data Reduction and Dissemination Plan," addressed the developing problem area of how to handle the large volume of data that will be generated by various accelerometer systems. This work is being performed by the University of Alabama in Huntsville in support of the ACAP. Gigabytes of data will be generated on each mission flown with a measurement system. The approach being taken is: (1) to first identify the experiment characteristics and those mission events that are meaningful so as to limit the amount of accelerometer data an investigator would be interested in, and then (2) to determine how the data will be processed so that it will be meaningful and relevant to the experiment objectives.

Session 5: Session 5 was a split session consisting of two working groups, one involved with isolation technology needs and the other with science requirements and the environment definition.

Session 6: Session 6 was a plenary session, wherein the findings and recommendations of the working groups were summarized and discussed.

Working Group Finding and Recommendations

Isolation Technology Working Group: In the first workshop held in 1988, this working group felt that the three most important issues to be addressed were:

(1) Control Technology
(2) Actuators
(3) Umbilicals

Somewhat surprisingly, these three issues are still deemed important, but not in the same order. The working group findings were that the top three issues in 1991 were:

(1) The umbilical problem
(2) Actuators
(3) Control issues

These are followed by source vibration control, sensor technology, active vs. passive methods, cost effectiveness, and specifications or requirements.

It is not surprising that the umbilical problem is now considered the most important issue, since it was of importance in 1988. Also, control technology and actuators have been addressed extensively in all of the international programs, while the umbilical problem has not. The working group felt that, absent umbilicals on an experiment, the problem of successfully isolating a science payload has been solved. In 1988, the lower frequency limit on state-of-the-art hardware was about two or three Hz. As a result of the several international programs, the technology is now available to isolate down to near 0.01 Hz and microgravity levels. The use of non-contacting methods will enhance the solution of this problem. This is being done, for example, in the ESA program being conducted at the University College of North Wales.

It may be necessary to make a sensitive experiment self-contained by including the source of required services to on the isolated platform. In most cases this will not be feasible, so it was felt that the umbilical problem needs to be addressed, particularly when dealing with vacuum lines and mass transport services such as fluids. The following suggestions or recommendations were made:

(1) Obtain a better quantitative understanding of the dynamics of umbilicals. Measure stiffness values.
(2) Develop the technology to make smart umbilicals such they track the payload.
(3) Originate or emanate the umbilical from a breakout box and isolate that box actively.
(4) Incorporate the umbilical into the isolation actuator.

The issue of actuators resolved into two categories. First, if there will there be a need to handle large strokes in order to isolate the ultra-low frequencies and if so whether this should be done in stages or with one actuator. The consensus was that for most applications range-of-motion requirements can be handled with current technology, but there may be instances where a large motion or stroke actuator may be needed (e.g., a device like a Stewart Platform). The other issue discussed was the preference for the Lorentz or voice-coil actuator versus the ferro-electro-magnetic actuator. There are champions for both in industry; both work and both perform well. Both have advantages and disadvantages, and the issue resolved down to a matter of personal preference and in a case by case evaluation, to use the type that best meets the need. This is not a major issue.

There were no major control issues. The discussions centered around using position feedback or inertial feedback. With no direct disturbances position feedback would be more than adequate. With direct disturbances and/or umbilicals, inertial feedback is required.

Source control of vibration disturbances was generally accepted; however, how much source control vs. payload isolation to be used was an issue. In principle, source control is common sense planning. In designing equipment it is sensible to use techniques and components that will tend to be quiet; the issue can be implemented by setting limits on equipment builders, but exactly what these limits should be may be hard to define. Actively isolating all sources is not feasible. The effort of the SSF Level II Office to try to institute a vibroacoustic plan for SSF was highly endorsed.

Sensor technology discussion focused on the fact that any active isolation system is now limited by the performance of the sensor being used. It is recommended that some effort be endorsed to develop lower cost sensors with better performance.

The active vs. passive isolation issue is reoccurring. Passive isolation will be most cost effective, but for the majority of science requirements now known, its use will be limited. It was suggested that consideration be given to exploring improved passive system performance.

The cost effectiveness issue is and will continue to be a factor. Isolation costs money, and most principal investigators (PI's) would rather spend their money on science than on things that they may need to make the science meaningful.

Cost effectiveness can be manifested in little things, such as using passive isolation mounts on racks to reduce disturbance transfer or develop low-cost hardware and sensors. A facility such as the ESA MGIM, which takes into account vibration isolation will, in the long term, be cost effective. There is a distinct advantage in isolating a facility system versus isolating many individual experiments.

The issue of specifications and/or requirements basically comes down to "What do the science people really need?" Requirements have be generated based on simple analysis, and these are constantly being challenged as to applicability. The workshop had very few attendees from the PI ranks and this was somewhat discouraging because the International Vibration Isolation Technology effort is for their benefit.

<u>Science Requirements and the Environment Definition Working Group</u>: The discussions in this working group were dominated by the SSF microgravity Requirements. This was due in part to the fact that four of the workshop attendees were from the Level II staff office, which made for spirited and meaningful discussions. A principal outcome of these discussions was that the "Nauman" or lower curve in the requirement is necessary to do meaningful science for some experiments, particularly for doing sensitive crystal growth experiments. There is also a need to firm up SSF requirements quickly. In the permanently manned mode there will be times when crew activities will have to be limited; so crew training will be an important element.

The original monochromatic requirements curve has been discussed a lot and at times criticized primarily because it only represented a part of the problem, i.e., a single monochromatic source. The "real" environment will be quite complex, consisting of many sources that will have random, periodic, and impulsive elements. Trying to define requirements for the real environment is no simple task nor will all people be satisfied. The approach being taken for SSF is using Power Spectral Density (PSD), narrow band and transient analysis to account for the major elements of the environment.

It was pointed out that the high frequency end of the current requirements are unrealistic since the displacements involved are in the nanometer range. It also became apparent that isolation will be required in some instances, but this must be done cost effectively, and that a vibro-acoustic plan is needed. The U.S.Navy has been using vibroacoustic planning in their submarine program for many years. The dialogue at the workshop between the SSF Level II group and the other attendees was quite productive.

An issue of major importance to most people trying to define requirements and effects is that there is a critical need for a well-designed, coordinated experimental and numerical effort to validate modelling techniques. The vast majority of current modelling is being done with simple models and methods, and there is uncertainty in the results. Some of the experimental work can be done using the KC-135.

The working group discussed the issue of whether users understand what they really need and whether they have a clear understanding of what the real environment in SSF and STS really is. The concern here is that a set of requirements can be established on paper for a carrier but this does not insure that there will no disturbances that will exceed the requirements. The users would be prudent to realize this and plan for it.

Flexibility in hardware design, particularly for material processing, was discussed. For example, if a furnace is being developed, it would be wise in the design process that the furnace be applicable to a class of experiments rather than to a single experiment.

The free flyer concept was discussed, and it was felt that the concept should be pursued for those experiments that will require long duration and a very pristine environment.

The umbilical issue, as well as the use of source control, were briefly discussed. It was recognized that umbilicals will be a problem and that source control has merit.

SECTION II

WORKING GROUP SESSIONS

SUMMARY SESSION

I - ISOLATION TECHNOLOGY WORKING GROUP SUMMARY

CARLOS GRODSINSKY: Basically we made a list of items that we thought were pertinent to the technology and wanted to discuss. We are going to go over this list and I'll ask those people designated to do the summaries to come up and do so. Then, when they are done, anyone who wants to bring up discussion points should feel free to do so. The first item is the umbilicals.

JOHN BLACKBURN: Umbilicals are a significant problem, they include cabling, piping, and other things connected to the payload that you are trying to isolate, that provide the electricity, vacuum, air, or whatever you need. Umbilicals introduce both the mechanical spring to the system as well as transmit disturbances from the positions that you are trying to isolate the payload from. We had a number of people who said intelligent things.

The first statement, by R. Gareth Owen, was that at some point it's probably a good idea if we get a better quantitative understanding of the dynamics of tethers, which umbilicals really are. In other words, we need to get in the lab and do dynamic testing on them. There are a number of ways you can do that.

Dr. A. von Flotow mentioned that it would be unreasonable to try to characterize every possible umbilical configuration because there would be too many of them. He also stated that research into the technology of umbilicals themselves is in order. In other words, there are ways to build smart umbilicals, such that the umbilicals track the payload around and thus reduce the spring effect. However, this may not reduce the vibration disturbance passing through the umbilicals. Another aspect of what he called umbilical technology would be tunable spring constants; in other words, being able to adjust the dynamic effect of the umbilicals.

He also pointed out that if there are no umbilicals, then the isolation problem has basically already been solved. In other words, if we have no external spring that's due to an umbilical, then we have already solved the isolation problem, perhaps by using attractive or repulsive magnetic actuators, things similar to what the University of Wales have been doing and similar to what Carlos has done also. Then the question came up as to whether umbilicals are really necessary. Can we live without them? If so, then the problem, as I said, is simplified.

Paul Allaire and R. Gareth Owen mentioned that because of the fact that we do need vacuum hoses in many of the materials processing applications, and large power cables in other applications, you probably do need umbilicals at some point. So the group consensus is that ultimately there will be a need to support at least some sort of umbilicals at some point. So the problem has to be addressed. With that in mind then how do you solve the problem? What are some of the solutions to these, to the problem?

Carlos Grodsinsky suggested that we could set up a system such that the umbilicals emanated or originated from a breakout box, and the breakout box itself could be isolated actively. That would solve the problem of the disturbances playing through the tether or the umbilical. There is probably some good possibilities in that solution.

I suggested that the dynamic spring effect of umbilicals could be more predictable and controllable if the umbilicals were incorporated inside an electromagnetic or Lorentz-force-type actuator, these are cylindrical voice-type actuators. If you were to place, say, a flexible tube inside of that, you can get a very predictable linear-type spring that you could run cabling through, from the isolated payload to the place where the tether originates.

Paul Allaire suggested that supply air pumps and all of the equipment from which the umbilicals are generally run could be mounted on the payload itself, so that all of the umbilicals are local to the platforms and so there would be no interconnection between the base and the payload. I don't know whether that's feasible, in that the pumps and these sorts of devices tend to be large and weigh a lot. Incidentally, one of the problems with this technology is that the tethers, umbilicals, and so forth are entirely dependent upon what experiment you're talking about; some experiments may require no tether or umbilical at all, while others require sizeable umbilicals.

Some general comments that were made. Carl Knospe and A. von Flotow mentioned that if an umbilical is not required, the form of the control law changes. That's true. The reason is that if there are no umbilicals (i.e., mechanical springs attaching the payload to the base structure), then you can basically solve the isolation problem in the way that R. Gareth Owen and his people have, by using attractive-type actuators, and the magnetic actuation forces are a very soft spring. Unfortunately, it seems to me that direct disturbances, acoustic noise and so forth, will still play through to that system, in which case you would need an inertial sensor.

In the soft spring approach, all you need are relative-position sensors. That's one form of the control law. The second form of the control law would involve feedback of acceleration or inertial sensors. If there is no umbilical and if you ignore the direct disturbances, the acoustics and so forth, then you should be able to use pure relative-position feedback.

So the point they are trying to make is that the control laws change as the function of whether or not there is an umbilical.

Mr. Tryggvason mentioned that the nature of umbilicals will change with the application, as I stated before. So it's a good idea, in whatever configuration you finally come up with, to have some means for flexibility and be able to adapt to different applications.

Ralph Fenn of SatCon has done a lot of interesting stuff, and part of their research involved investigating the effects of a tether or umbilical on the dynamics of the system. They performed several tests, one of which was that they ran their six DOF table, as I understand it, with and without umbilicals, where the umbilicals were 30 gauge copper wire, about six inches long. They looked at PSDs with and without, and noticed the difference of about one micro-g; so they are saying that the effects of the umbilicals, when these are small, becomes negligible. In other words, if you can get away with using just a couple of tiny strands as your umbilical, then perhaps you don't really need to worry about the spring effects of those, or of the disturbances playing through them.

They also ran a test wherein they took five of these 30 gauge pieces of copper wire and treated them as, I guess, cantilevers and looked at what their spring constant ought to be deflected over by 30 degrees or something. They found that the force that they exert, given the spring constant of that tiny little strand, was less than a micro-g. So again a different application is going to have a different requirement for umbilicals, and the resulting umbilicals are going to have a different impact on the dynamics. That was all that was said about umbilicals.

We then talked very briefly about sensor technology, which was listed as one of the things that people wanted to talk about. But when we came to that subject not a lot was said. Probably this is because, if anyone has any really novel ideas as to how to build an inertial sensor down at low frequencies, and it's better than what is available, they probably want to keep it to themselves rather than telling the world.

I pointed out that the sensor technology is really what limits active control in the inertial sense at very low frequency. In other words, the noise floor with low frequency flicker noise, as they call it, is what prevents being able to look at vibrations at very low frequencies. Relative-position feedback, however, is usually much better at low frequency.

Sensors that have good noise performance at very low frequencies, in other words they go down to DC or what have you, tend to be very expensive; these are gyros and the like. They have problems associated with them as well, such as drift and the null and things like that. They have to be recalibrated from time to time.

Doug Havenhill mentioned that inertial sensors are required whenever there are direct disturbances acting on the platform. This was what I was mentioning about the system of R. Gareth Owen's and his people, that even though there is no tether and you have this very soft spring that isolates you from inertial disturbances down to the frequency corner of the spring, you still can be susceptible to the acoustic or pressure-wave environment of the noise, and so forth. In this case, you can still use that system, but you just have to add some sort of inertial sensor to sense the motions and actuate them out. I guess we decided ultimately that the use of both inertial and noninertial, or relative position sensors, are needed in such a system; some combination of the two is required. And that's all I have.

MICHAEL HORKACHUCK: I was just wondering if anybody has looked at some of the small ring laser gyros that they have out on the military market nowadays and in using that as a sensor technology. I am not sure what their thresholds are.

DOUG HAVENHILL: I can speak to that. One thing you have to watch about ring laser gyros is that they have a dither motor in them, and they are not usually really good for vibration isolation because of that. Because they shake what you are trying to isolate, you have to be real careful in using those.

CARLOS GRODSINSKY: To keep going on that, Doug, I have seen pictures of these things and they are small. What kind of frequency is that dither motor going at, and do you know the band width of those things?

DOUG HAVENHILL: No, I don't.

MR. KERN: I would like to comment on a couple of statements concerning about acoustics directly impinging on the payloads. Our experience with quiet rooms indicates that at very low frequencies, below 20 Hertz, the air conditioning has a much bigger effect on the payloads than acoustics; you really can ignore the acoustics.

CARLOS GRODSINSKY: Do you mean that's the actual airflow?

MR. KERN: Right.

CARLOS GRODSINSKY: Well, maybe John could address that. I know that in your SSP work you have had problems with that.

JOHN BLACKBURN: You have the low frequency disturbances, due to whatever causes them, whether it be air conditioning or seismic motion, and certainly those are the ones that have the most impact on things like materials processing applications. However, in some cases you can't ignore the acoustic disturbances. We have a quiet room, as you call it, in Alamogordo, New Mexico, where the seismically stable platform is, and we noticed tremendous differences in the measured PSDs of motion on the platform when acoustic disturbances are present. Even someone talking or scuffling about upstairs, you can pick up; and that acts as a direct disturbance into the platform. Those are the types of things that I was mentioning. I don't disqualify the fact that the low frequencies are a problem, but the acoustics can play into it depending on what your specifications are. It's true that acoustic disturbances tend to be higher frequency component behavior, though; usually that's true.

CARLOS GRODSINSKY: I'm sure a big factor in that as well is the surface area of what you are isolating.

CARL KNOSPE: One other thing on direct disturbances is that onboard machinery needed to service payload is a problem, as well as whatever acoustics or airflow there is, so that has to be considered too.

CARLOS GRODSINSKY: We will now go on to the next subject, which is actuators and there are several things under that topic. Doug Havenhill from Honeywell is going to speak to you on this.

DOUG HAVENHILL: I am not nearly as organized as John was and I didn't get everyone's name for each comment; so. I am going to try to give you a group consensus of what we talked about.

In terms of actuators, I broke this discussion down into what issues were raised and then what recommendations we made. Basically, the issues were range of motion; that broke down mostly into, do we really need to have a two-stage control or can we take up all of the range of motion that's required with the magnetic suspension and the gap. Our conclusion was that most of us felt that we probably could accommodate the magnetic gap, but for certain experiments we may want to have a rather large range-of-motion type of system, wherein you would have something like the Stewart platform that the University of Virginia talked about earlier that would follow up the magnetic actuators to make it so that the gap doesn't get too large.

There was some discussion about stray fields in terms of which actuators have more propensity to emit stray fields. We are not sure what the sensitivity of the experiments are or what the EMI/EMC type of requirements might be for this type of device.

Andy von Flotow showed us some work that he is doing, in which he actually has a mechanical connection between the base and the isolated mass; the connection actually contained the actuators. These are piezo type actuators, and this looks like it has some interesting possibilities, so we talked about that.

We also talked about versatility, that is, we would like to make anything that we build with the actuators versatile, so that as many different payloads as possible can be accommodated.

There was some discussion about the use of passive actuators versus active and hybrid systems, and whether or not it makes sense to use active and passive together.

And then there was a big discussion about Lorentz-field versus attractive actuators. And Ralph Fenn from SatCon likes the Lorentz quite a bit. Honeywell, where I am from, uses what is referred to here as attractive; the British have decided also to use those type actuators. And while there are a lot of points about which one is best and whatever, they both work and they both perform; the selection may be just a matter of personal preference.

In terms of things that we would like to see that come out of this meeting, we would like to get a definition of what the range-of-motion envelope might be. That all relates back to what the disturbance input is going to be. I know that there are lots of people working on that problem. In the meantime, while that's being defined, I think we should pursue both large range of motion and the normal sort of magnetic suspension that we are working on presently. Then we would also like to get definition from experimenters as to what their stray magnetic field sensitivity might be. That's all I have; are there any questions, or did I forget anything?

CARLOS GRODSINSKY: Thank you. I guess to keep going, Carl, you are going to talk about the control issues. I might have left some things off of this list, but we will make sure everything is straightened out before we are finished here.

CARL KNOSPE: John Blackburn of ATA covered a lot of my points already and did a pretty good job, so this might not take too long. I didn't take exact notes of who said what, but I tried to get the general feeling of everybody who was there, and the general feeling was that we are going to need active isolation control. As long as the specifications stay like those we have been seeing for the last couple years, it is going to be required, especially for the low frequency type problem, 0.1 Hz or in that range. Then it seemed that the problem sort of boils down to the two different areas -- category one and category two.

In category one you don't have an umbilical and you don't have any appreciable direct disturbances. That is, whatever support utilities needed are onboard the payload, the experiment; if disturbances arise from any machinery aboard, like shutters or whatever else, the pump is small enough that you can handle that; then the control problem is solved, it's low gain, relative position feedback. A classic example of this is the FEAMIS system that is here on exhibit, which has wonderful protection against vibration and works real well; the technology is there now and the problem is solved. One nice thing also, is that the quality of your relative-position sensor doesn't have to be too great. From what I understand, a relatively moderate one will work just fine because you are talking about low, gain acceleration feedback. You are not rejecting accelerations; it basically works on the principle of a soft spring.

In category two, you do have direct disturbances that are appreciable or you have appreciable umbilicals -- and the number that was sort of being thrown around was 0.5 N/m(stiffness) -- if you had greater than 0.5 N/m, and 100 kilograms I think was the rough-number payload, then your quarter frequency is already low enough. So if your umbilicals are less than 0.5 N/m in stiffness and your payloads run 100 kilograms then you are back in category one, as long as you don't have direct disturbances.

But your umbilicals might be more in the 50 N/m(stiffness) range, which some people were saying was a reasonable number. A telephone cord, according to Dr. von Flotow is about 20 N/m, one of those nice flexible telephone cords from his comments, so the feeling is the range might be more like 50 N/m or maybe even the 100 N/m if you are talking about something like a garden hose; and in that case you are going to be in category number two. If you have direct disturbances which are appreciable which you are going to have to reject, in that case the control problem becomes more difficult and you are going to need to have some sort of inertial measurements. To most of us that requires some sort of acceleration feedback, although there you might use some of the other technology people have mentioned, like ATA's technology with inertial velocity sensing.

So we will break category two down into two elements; in the first, you have no umbilicals and you just have direct disturbances; here you are still going to need to use something like a inertial feedback, but the problem probably isn't too tough. There is not a whole lot of coupling between the various axes and you can just use some sort of decoupling and SISO control. Single input, single output design is what I mean there, and it's not too terribly tough a controls problem.

If you have an umbilical, that umbilical may be pretty strong. There is going to be uncertain coupling between axes and that coupling is going to be geometrically dependent on how the payload moves, and it's yet to be seen how problematic that is as far as from a controls viewpoint. We had a debate on what methodology you should use for this problem when you have an umbilical in there. Some people favored modern control methods; some people favored decoupling, single input, single output and thought that method would be good enough. Other people favored some sort of hybrid between the two. One thing that we all agreed on was it was important to characterize the umbilicals, in some rough order of magnitude of what you need (for your experiment), if it's 5 N/m or 50 N/m or 100 N/m, because as the umbilical gets stiffer, it gets to be a much tougher problem.

One of the last things on this issue, there was general agreement on the fact that when you are using an accelerometer to reject disturbances, the quality of your accelerometer is equal to the quality of your isolation. The better accelerometer you get, the better isolation you are going to get with the same control system. So it would be interesting to try to get better accelerometers for this problem, especially when you get to lower frequencies. There was some discussion of temperature effects on accelerometers and the impact this might have on controls. The general feeling from the people who have looked at this is that it requires calibration. Jeff Schoess from Honeywell remarked that they had done a lot of work on this temperature calibration of accelerometers and evidently got some good results. So that's a doable technology, it seems, to calibrate your accelerometers for temperature variations.

The last issue would be cost-effectiveness. This is sort of borrowing it from later discussions, but I think the general opinion on this is that it depends upon what the experiment people need. If they need a lot of vibration rejection, and if they have heavy umbilicals, it's going to be a much more costly solution than if they have no umbilicals and no direct disturbances. So as far as cost-effectiveness, basically the controls people really need a feeling of what the disturbances are, what the required isolation is, and what umbilicals you are going to have on the experiment. If that information can be given to the controls people, we can get a pretty good idea about what level of system we need. So that would be the general group's recommendation as to what the experiment side community should be supplying us; it's a characterization of those things.

CARLOS GRODSINSKY: To go on to the vibration source control issue, there is both a lot to be said and not much to be said. The basic consensus was this would be the most cost-effective thing to do in the space station, but you can't really have much impact on what they do with the shuttle. It is highly recommended for space station. In the last year or two a lot of things have been done by Phil's group, and I know Gary and a lot of people have been working at NASA Headquarters in trying to get a vibro-acoustic plan worked out as well as trying to impact some of the work packages, and I think that's great. I know you have an uphill battle and I hope it all works out.

But one thing that came out that could help you people is that a lot of people have looked at the use of a vibracoustic plan and were concerned about what will disturb an environment. Since the Navy has been doing this kind of work for years in submarines, you might get some good ideas from them. They should have reams of information.

I think, as the gentleman from Honeywell also said, the Hubble space telescope, or any kind of large pointing system has source control problems. I know they had problems with their solar arrays, which I think they solved by torquing the torque motors on the arrays to get rid of this swing they had in going from night to day, and that worked pretty well.

Another thing to look at are simple fixes. While I don't know how exactly everything is managed in the space station, the human factors people say that it's supposed to be a laboratory environment, and that the astronauts are there to do science and help out. They are going to have to read meters and look at things, and (when they do) they are going to have to hold on to something. Since every time they drift away, they are going to have to bring themselves back to these meters or whatever, one of the simple fixes is not putting handholds right on the experiment racks; just simple fixes like this helps to control the man-machine interface. As to rack isolation, we talked about just trying to isolate the racks themselves from the major structure. If simple fixes like these are not in your plan, hopefully, all these things will get in there.

Last, we need to make sure that these requirements get the hardware developers to make the most quiet fans and pumps, etc. so you don't have to fight the problem from only one side. That would be another one of our recommendations.

JOSEPH LUBOMSKI: I think that's a very good idea. I think the most important thing I have seen in the last year or so is looking at the source control problem as you are doing now. It's very important to us that we do look at this source control approach.

JOHN BLACKBURN: Dr. von Flotow mentioned there would probably be some value to trying to assess the difference between the cost of isolation at the sources and the cost of the isolation at the point at which isolation is being done actively. The reason is that intuitively it would seem to make sense to first actively isolate those things which are introducing the forcing functions, but there tends to be, as someone pointed out yesterday, a gagillion of those. There are a million forcing functions, but there aren't so many places where active rejection is required, so someone could do a cost analysis to figure out whether it is actually cheaper to isolate at the location where isolation is required or at the places where the forcing functions enter the system.

CARLOS GRODSINSKY: If I may add to that, I don't know how you can really figure that out, in that at the top the money comes all from the same place -- part goes to the space station facility, and at the end the scientists have their money. To them, of course, it would be more cost-effective if somebody else takes care of the problem for them, but on the other side, it works vice versa. I guess I don't know how to handle that. Both groups actually got together a year ago and got this thing going. So that's the first time I have seen it working, and hopefully it will all work out to the benefit of everyone.

CARL KNOSPE: One thing that's important to bring up again is that source isolation is good for the machinery and whatever else is on there and that's a great idea, and the simple fixes you mentioned are all good ideas. However, one of the fundamental problems here is the crew motion, which is impossible to source-isolate against, and that's the one that is really hitting the range where we are probably getting hurt the most -- toward the low frequencies.

CARLOS GRODSINSKY: I think it's not going to be a simple solution by any means; I agree with you. But to go on, I think I left out passive isolation. We will go on to that and then hit the cost-effectiveness, which I guess we have been talking about a little.

GEORGE McCANLESS: Actually my topic is cost-effectiveness, and this is a subject near and dear to my heart; there is always a shortage of money for payloads. Mr. Havenhill pointed out that the scientists want to spend their money on science and not on isolation. He speaks from the heart, because he is with Honeywell, and they have had a payload isolation system available for about five years that they haven't been able to sell to anybody, or to get anybody to use it. Dr. Owen from Wales says their approach is to build a basic facility, not anything excessively fancy, hope to get a couple of users, and then after some people have used their system and demonstrated its utility, people would get in line. They will have a significant usage of the facility based on that.

I stated that I thought the cost considerations would cause us to first use passive systems, and one of the things that seems terribly obvious to me is that on the Space Station Freedom racks we should put some sort of rubber grommets on feet or connections. The way the space station is being built, it's an almost perfect transmitter of disturbances; and so I thought passive systems will get the first shot. Mr. Blackburn pointed out, however, that passive systems are only effective above the natural frequency of the system. So we will only be able to isolate the higher frequency stuff. The stuff that's really killing us is down in low frequencies, so I had to back up a bit.

He did point out that there is a company in Boston that makes this stuff that they sell to industry to isolate machines of some sort or another. I told him he ought to get a commission from these folks for what they sell for the space application, because I think they are going to get some business. Realizing that there are limits to passive isolation, I still think there are some benefits to be gained in a pretty cheap, pretty low cost approach.

Dr. Allaire from the University of Virginia said that these active systems are low cost in terms of power. I am not talking about dollars, but he said they don't suck up a whole lot of power, which is certainly a cost consideration on any sort of space vehicle. You tend to get into light bulb numbers when you look at power.

Then Mr. Blackburn again pointed out that we need to get the sensor cost down. In the active system the sensor cost seems to be a major factor and that needs to be looked at. Any questions? (There were none.)

CARLOS GRODSINSKY: I think the last subject was specifications (requirements). We have been all working to certain (requirements) curves that have been changed a bit in the last couple of years, but yet they are all pretty much similar. They basically drive everything we do. It would help if we could really get a handle on more specifically what experimenters need. Now, I know that in certain cases they really don't know as of now, but I think we still need to work more in this area to make sure that whatever we do, we don't overkill or underkill, and that we have something that works.

We didn't talk about characterization of the space environment but that's very important. We need to know exactly what is going to impact isolation systems. Many people are now working that area.

We will be getting the SAMS data as well as the low frequency type of measurements from OARE, and HIRAP. Honeywell also has their accelerometer measurement system and it has flown on shuttle. In the next couple years I feel we are going to get a lot of data that will characterize the shuttle environment. I know the people at UAH are looking into how to get the data out to the users as well as to our community. That's going to be very helpful. I know there is a lot of work being done in looking at what kind of disturbances impede someone's science. I know Ram and the people at UAH are doing this kind of work .

To wrap up, that's about all we had as a summary, unless there is more anyone else would like to add that we could discuss now.

BJARNI TRYGGVASON: I think the basic gist is that while we need (both) some source isolation and some payload isolation, the other thing is that the payloads themselves are going to generate disturbances. From the experience gained in doing a lot of different kinds of experiments, both on the KC135, on sounding rockets and also in preparing to fly 12 experiments in the shuttle in the next few years, a lot of the experiments have moving parts that are going to cause the experiment to create its own disturbances. That should be borne in mind when you are trying to isolate everything perfectly. For a lot of these, the experiment itself is going to cause the main disturbance, so keep that in mind.

GEORGE McCANLESS: Seconding that, the Eureca people did a very thorough job on the disturbance situation. They found that the big source of trouble came from actuated valves, as these valves were being popped off there would be a surge, and this really generated disturbances through the whole system. Also confirming what Bjarni said, Dr. Lindquist of UAH made the statement, concerning their unmanned rocket program, that the disturbances set off in one payload will upset those in others.

CARLOS GRODSINSKY: To add an opinion, I think that somehow we are going to have to get together with both the science people and the payload developers and fly something to validate the technologies being developed. It doesn't need to have all the bells and whistles, to isolate against all the umbilicals and everything, but just to prove that something specific can be done. We will learn through doing this and then have something better for the space station, so that we are able to do some good for all these people and to get good science consistently or hopefully consistently. I don't exactly know how to make that happen, but we are trying. Somehow we have to figure out how to do that in near term, not ten years from now.

II -SCIENCE REQUIREMENTS AND THE ENVIRONMENT WORKING GROUP SUMMARY

N. RAMACHANDRAN: What I've tried to do here is summarize what transpired during the Working Group discussions. We made some observations and then some followup recommendations. Most of our effort was spent in categorizing or trying to understand the STS environment and the space station environment, and what the specs meant, whether they were realistic, what's missing, and what we would like to have. Then we followed on with discussions regarding whether the user knows what he wants or how much analytical work, if any, is required. Then we had topics in common with the other group, isolation technology, umbilicals and their requirements, etc.

The first topic then, was whether the STS environment been properly defined. There were quite a few topics raised to that end. And what is required to improve the understanding of the environment? To start off, we have the famous Nauman curve, which is a frequency versus amplitude kind of distribution. Then we have the PSD curve which displays the restriction of amplitude of frequencies in certain octave bandwidths.

There was a lot of discussion regarding the transient impulses onboard the space station and the shuttle. As somebody pointed out in the previous discussion, a lot of the transient impulses are crew-induced, and there is no way we can either predict when they will occur or try to even control them. It's really beyond our capability to define that.

There was discussion about the high frequency amplitudes, as to whether the space station requirement really meant anything, and should anything be done with regard to that, and some discussion on the acoustics transmission and what it really meant. The modeling out of JPL was also discussed to some extent.

Issues regarding time domain categorization of requirements were also raised. This has to do with the (requirement) curves that we use. The Nauman curve and the PSD curve specify the frequency regime requirements. What about the time domain? Is that important? I think the consensus was that we need something to address that aspect. Also, where these specs are to be applied was discussed, at the payload itself, at the rack, at the station walls, etc.

That's a broad summary. Phil Bogert will now talk in a more detail relative to SSF requirements.

P. BOGERT: I don't have any slides, but I just wanted to give a little summary of the session we had yesterday afternoon. Perhaps more importantly this is my summary as a person with staff responsibility at Level II for bringing all this micro-g stuff together, just the big picture on where we go from here and what I have gained from this conference and what I think some of the major action items are that we need to accomplish.

One conclusion that's probably obvious to all of you, but it's not obvious to all the management at Level 2, is we really do need that lower curve, the Nauman curve, to do all of the science that really needs to be done. This is especially true for the sensitive crystal-growth-type furnace experiments, and not everybody at Level II is aware of that. I think the work Emily Nelson did in pulling together a lot of that data and the curves from the science community kind of demonstrates that, and I'll certainly make that point to my management. That's the question that's come up a few times -- what do the scientists really need?

And maybe we don't need that environment for every experiment, and if it comes down to a real money issue, perhaps we can be a little fancier about how we isolate what we really need to a certain level, and kind of work the other experiments to the level they need. Maybe some experiments only need 10^{-5} g, though up front we'll shoot for the whole environment as defined in the requirements. We have a little room for negotiation, I think, but I am convinced that for some experiments we do need that lower curve, and that's kind of a key point.

I also recognize the need to get our requirements firmly defined in the very near term. We talked about this update of the requirements to fit the restructured station, the man-tended utilization phase, and we talked about basically not changing the nature of the requirements and the values of the curves, that's true. But one subtlety that really is a significant change is the fact that we are going to try to get some nominal crew activity in that lower curve. To me that's kind of necessary, otherwise we can spend millions on isolation and then have the crew activity ruin everything.

So I think that will dictate what kind of crew training we have. Hopefully, the crew is up there to do science, as was pointed out earlier this morning, and during those key times they'll be trained to be fairly quiet. We should have some numbers on that soon, but we need to do more research and we'll look to some help from Charles on that. I really like the idea that Carlos brought up this morning about running an experiment at some point on shuttle to get all of this off paper, and to see in a real environment with an isolated system what can we really achieve with machine-induced vibration and some crew activity. From what I have seen here, all the instrumentation technology is available to do that. So that's a major point now I'm going to include in the overall plan I present to my management and I thank Carlos for that idea.

We do need to get our requirements set. I think that everybody's basically happy, and correct me if I'm wrong, with having the PSD approach in addition to the narrow-band approach, and so we'll look at monochromatic sources. And maybe one should add in a very, very narrow frequency band and check those with the narrow-band curve, but we also will take this power approach and the broad- band approach. The thing that we got some insight on yesterday which we need to bring to closure concerns the transients. When we work with transient forcing functions, in addition to the power check and lumping those in with everything else in a bandwidth, we need a couple of additional checks. That's because of the windowing techniques. When you do Fourier transform, you can tend to spread the results over different octave bands, depending on how you do that window. So we think what we're doing is okay for the power approach, but the two ends of the spectrum we want to check individually, and that's some kind of spike check, like a maximum time domain limit of the acceleration for very short-duration-type spikes. We decided it might be somewhat of a moot point, because they're not likely to affect things too much, but I think we will include some kind of spike check in our requirement.

We talked about a 1×10^{-3} g type of level, and then we also talked about an impulse. We'll probably include some kind of impulse to cover the other end of the spectrum we might miss with our windowing. So we're thinking of doing those checks in addition to the power check.

The other thing in the requirements that we had a little discussion about was to look at the displacements that are allowed out in the higher frequency end of our range. If you do some simple calculations, the displacements become so small that they're not real, and while we're aware of the problem, we're not quite sure what to do with it. We will go ahead and work that and make sure the requirements make sense in the higher frequency range. We don't want to be limited to 10^{-10} meters of deflection. Somebody pointed out that is molecule-type size levels, and we don't want to have a problem with that. So we'll work these requirements in the short term. We really hope to submit our change requests in the next week or two and get it through our space station control board by late May or early June, so they'll be on the books.

The other message that was really reinforced to me was that time really is of the essence here. We were planning on doing our allocation analysis by sometime this fall, and we'll go ahead and do that with a fairly decent model that we'll be collecting from the loads people and some vibro-acoustic modeling. But I'm really reconsidering once again, based on what I've heard here, the need to do something sooner. Because there is a major contract renegotiation coming up in the May-June-July time frame on space station because of restructuring. I know Gary feels very strongly that we need to kind of catch that window or maybe lose it forever. So I'm going to see if I can push my system back in Reston into doing something sooner. Maybe we can use that preliminary model I showed you of the restructured station and, at least in the dynamic, lower frequency dynamic range where we know we're real concerned about crew activity, getting some kind of allocation for the work packages.

That's where the integration job is going to come. Once we characterize the environment, then we can really talk about how we isolate. I think the solution to this problem is going to be some kind of cost-effective combination of everything we've heard here. We'll do what we can with passive isolation at the sources, that might cut it down somewhat, and then do active isolation of payloads where we really need to. We do need to make our specification such that it does not preclude that. Right now we define the requirements at the structure payload interface and that kind of precludes any motivation you would have to actively isolate experiments, because we're not defining our requirements at the experiment. We'll put some words in the spec that won't preclude that, because I think the solution will be a combination of all of the above. Once we have the environment so that you know what you have to isolate against, I would envision inviting a lot of you to one of our future workshop meetings for space station.

Up till now the emphasis has been on dynamic analysis methods and quasi-study analysis methods. But as soon as we get to the point where we have this characterization, then we really start talking seriously about how we put our heads together and isolate in a smart, cost-effective way. And it's going to have to be cost-effective, because we just cut $6 billion out of our budget and there have been layoffs taking place all over. We're kind of down to bare bones. My management keeps giving me the message that there's no more money, but I really feel that if we can come up with something very cost-effective, maybe some of it can be done without any additional money. We need to try to find the best way to do this.

For example, the gentleman from Honeywell pointed out that, because no requirements have filtered down into his spec, he's designing a motor that is going to be noisier than if he would use some kind of brushes that would be low noise; and that's the reason we have to do something soon. We have to get these requirements to filter down from Level II through a vibro-acoustic control plan into equipment specs. If we can do that in the near term, it might not cost that much to solve this problem. I'm certainly an advocate of providing a good microgravity environment and I am going to be a strong proponent of that to my management. I think these are the things we have to do and we'll get you all involved as soon as we have that characterization of the environment, so we can then really decide what's a cost-effective way to do it. At that point, I'd envision having a few good working sessions, with many of you involved; then you can give me something I can present to my management that shows we've really tried hard to do it in the most cost-effective way. I'm optimistic that we can sell that and still provide a good environment for science.

So I think that's pretty much where we're at right now. This has been real enlightening for me, and I appreciate all the work that's being done. It just motivates me more strongly than ever to try to make it happen. We've got so many resources out there right now doing all this kind of work, and for the relatively small amount that it would cost to bring it all together and integrate it into something that will work for station, it would be a real shame not to do it. I'm really looking to all of you for help in just how to do that in the future, and I think if we work as a team we can make it happen.

G. MCCANLESS: Let me urge that you try to get something into these new space station contracts. Back in the fall when we went through PDR with Work Package One, the Boeing people stood up first off and they said, "hey, we're not allowed to accept any RID's to anything unless it's a requirement in our contract". That contract was three or four years old, and it's my understanding it still hasn't been updated after all these changes.

I'm not faulting the Boeing people because they do have to work to a contract, but I could not find whatsoever any disturbance requirements in that contract placed on Boeing or Work Package One. It may have been there but I just couldn't find it.

So I do think that this is the last chance to influence what the contractors do is in this renegotiated contract, whenever that is.

One other point. You mentioned doing some sort of precursor tests on the shuttle; I suggest you also look at the KC135. It's much cheaper, much easier, and not nearly as formidable bureaucratically. Of course, you only get 20 seconds or so of microgravity, you don't get as good an environment and a level as you do on the shuttle, but I think it's worth taking a look at.

P. BOGERT: Thank you, George. It really is Level 2's responsibility to make these requirements trickle down. The reason they haven't, as I think I explained the other day, is because the directives stating the requirements that made it into our PDRD were done without cost and schedule impact, because there was no suballocation. The work packages came up with huge numbers in the absence of any firm guidelines as to what they had to isolate.

That's why our focus now is to provide that allocation, so that rather than tell a work package or a partner of some general environment we need, asking what's it is going to cost, and getting a $200 million number, we can say "here are five of your machines that are causing 90 percent of the problem -- in a very quantitative way if you cut their response or their output, transmissibility output by a factor of three, in an integrated sense you'll solve the problem." Then we can give them something firm that they can go to their contractors and get numbers with.

And so it's our responsibility to do that just as soon as possible and once the impacts are accepted by the program, then we can trickle this down into contracts. But I keep hearing the same message, we
need to do it real soon or we're going to miss the window; it sounds like this summer, not nine months from now.

C. GRODSINSKY: I'd like to just maybe further comment on what the gentleman just said, that we really need to use all our resources in learning exactly how to do science in space. That includes using aircraft and everything else at our disposal, because we find that a some programs go right to flight and develop hardware. Then they find out that they have problems, and that it was not exactly the best way they should have done things for that environment. They would have learned a lot of things just by building prototypes, and just going to what we have on the ground is much cheaper, and in the long run weeds out all these bugs. I think that is happening more often now, irrespective of whether they can actually do their science or not in a Lear Jet or KC135, but at least they can test the hardware they will need to do their science.

P. BOGERT: We'll definitely recommend that. That's a good thought.

J. KOSTER: From a scientist's point of view, I want to emphasize again that in your negotiations with your upper management you include in your systems analysis also the science support from Code SN or from MSAD, because I think if they spend $20 or 50 million on experiments that fail, that is a lot of wasted money. So it is in order to optimize the facilities and not just save right now some few millions of dollars. I think that has to be emphasized.

P. BOGERT: That's an excellent point. We've thought of that just in the last week, and I've asked Gary to help me get a handle on what's being spent by NASA in the science area, say, up to the year 2000. That'll be just the kind of leverage I use to make the point, that if we can spend ten percent of that to make that other hundred percent investment work, that's a pretty strong argument in our favor. So I appreciate that.

N. RAMACHANDRAN: That was really the main emphasis of the discussions that went on as to what the space station requirements should be; and as Phil mentioned, he's going to take with him all the suggestions that have been put forth at this point. The next question that was brought up was whether the users understand what they really need for a microgravity environment and the limitations of the space station or STS environment. As somebody had pointed out, if you asked a potential experimenter what he wants, he'll probably say "the best that you can give me"; he'll probably give out numbers 1×10^{-6} micro-g, or better than that, without having a real feel as to what this might do to the experiments. The bottom line is that he may not have the essential analysis done for every experiment that is planned for the station or any space flight.

A clear understanding of the specifications is perhaps also not evident. As Damon Smith pointed out, there might be some misunderstanding as to what a micro-g environment really means. Does that really mean that at no time in the flight will that micro-g thing be exceeded? That is really not the case; that is what is required, but there are no absolute guarantees, so to speak. You have to take precautions to that effect. As Dr. Koster pointed out, you might spend millions of dollars preparing an experiment and if you look at his experiment, you find it is really sensitive to any impulse-type of disturbance that can completely spoil the experiment, and thus get no results. It might be prudent to envision ahead of time if you could do something with vibration isolation or something else to make that experiment work. A small effort ahead of time, a small investment ahead of time could make the whole thing beneficial.

Also it was pointed out that VIT may not be required for all classes of experiments. From a cost point of view, it would be prudent to at least isolate that class of experiments that might be most sensitive to vibration isolation, and then gauge if vibration isolation will benefit it at all. Maybe you need
something other than that; certain diffusion experiments might be better off free flying, for instance. We have to look into that and see if that may indeed be the route to take.

Also, as the gentleman here from Marshall pointed out, passive isolation of experiments might be an easier route. All the space station racks might benefit tremendously from passive isolation that may not transmit everything that the walls of the station feels.

One more thing to point out is that from the analysis that has been carried out, for some time now, the low frequencies are the most detrimental to an experiment, and vibration isolation technology at this level, from what I've heard, has a 10^{-1} Hz limit. Below that, we're not too sure, and the major problems lie in that area. So maybe that's an area where the technology people can work to try and to push that limit further down, because that's where really we are hurting.

There was some discussion on the numerical modeling of flight experiments and, as I mentioned earlier, this is a prudent, relatively low-cost first step. We don't have to do anything, other than just have a graduate student work for you and do it. I know what it involves, and it's relatively low cost and we can gain a lot of insight.

Of course, an order of magnitude analysis, and most of the requirements like the Nauman curve are based on such an analysis, gives you a rough rule of thumb as to what you might expect. In some instances it is two orders of magnitude off, but I think we should always be looking at the worst case scenario. If you can say that's the worst case we can envision, and then if you analyze that, I think it'll give us a good insight as to what we might expect or to what frequencies we are most sensitive. That involves identification of potential g-jitter hazards. Certain experiments involving free surface phenomena really are the ones that are more sensitive to g-jitter, and we have to identify those in that class of experiments.

There is a critical need for well designed, coordinated, experimental, numerical effort with fully characterized conditions. What I mean is, if you have done all this modeling and predict that if you take a crystal growth apparatus and strike it with a hammer with a 10^{-2}g impulse, for fluids it might take a thousand seconds for it to die down. That's all what we have done and can do, based on a two-D simplistic analysis. We have to have well characterized experiments, and maybe the MDE experiment could be one of those. When we have a well characterized experiment, we know what the disturbances are, as for example the treadmill. We have accelerometers on board, and we know what the response of the system is via video and other means, so that we can try and tie the two, modeling and experiment. It doesn't necessarily have to be on a shuttle flight, it could be on KC-135 flights, as Carlos pointed out. We could tie modeling and experiment together so that we know that we are headed in the right way. We tend to be simplistic in our modeling, and we can impose an isothermal situation or a flat surface; but then in a real situation when you try to maintain a flat surface, that's a different story. On paper we can do wonders, so we have to try this on a flight and see how well this modeling corresponds with the real experiment.

Another point brought out was the flexibility in the hardware created for materials processing. If you have a furnace, you had better make sure in the design process that it's not suitable just for one experiment, but for a class of experiments. That's something that came out of some presentations on the first day.

Another item is thinking about a free flyer concept for experiments that we know are going to be very susceptible. We would like a very pristine environment, no impulses, nothing. That's what we'd hope for in that 30 day period in the space station, but there are no guarantees. On the average, in a year's time,I think, statistically speaking, we might experience a hundred bumps or a hundred impulses. That might very well occur when the most sensitive experiment is going on -- Murphy's Law says it'll happen. So we have to be prudent when you are spending these millions of dollars, to try to see that it doesn't go to waste by (properly) planning the experiment.

We would like to see VIT capabilities for lower frequencies if possible, maybe this involves large stroke lengths and other things, I'm not the expert on that. The science needs are for isolation at the lower frequencies.

Charles Baugher will now address the data reduction aspects of ACAP.

C. BAUGHER: I'm trying to avoid getting the label of data analyzer hung on us, although I guess the last several months that's all we've been doing and what we're going to be doing for the next few months. That's going to be the foundation under all the science work that we've got in front of us, and we better get it right up front because this is the flight schedule that we're looking at across the next five years is for accelerometers on the STS missions (fig. 1).

The basic plan that we're now running with the accelerometer data is to essentially report on two levels. At the first level we're going to do is get some early, very quick reports out to try to scope what the environment is. Those are to be turned around for the investigators just as quickly as possible so they can look through them and try to reach some initial judgment on whether or not the environment is actually affecting their experiment and whether they need to go into it further. At the second level we'll then try to analyze the accelerometer performance so that we can provide the investigators with enough information that they can take the raw data and analyze it themselves for their purposes, should they wish to do so.

Now, I hadn't given a lot of thought to people outside the investigator community who will be utilizing data from SAMS and OARE, but I guess that's the community that we've got here now, and there are some other people who will be interested in these accelerometers data besides the science investigators. But as an internal effort within our work at Marshall, we're certainly aware that there are some other questions that need to be answered about the shuttle, STS, space lab system, besides those connected with the experiments.

I actually had a chart with me that I didn't show the other day, because I was rushing through the evening. But we've been sitting around scratching our heads a little bit on this subject of what we really might want to get out of the data besides just the direct support to the investigators. And I guess we started out a few months ago, or last summer, with kind of this big question in front of us as to what the microgravity environment for the experiments is. After looking at some past data, we're starting to subdivide that question down into a set of other questions and just as soon as we look at a little more data, I presume we'll start subdividing it further. But the questions that relate to vibration isolation, and the characteristics of the unit, right now are, first of all, can we go in and model this low frequency regime rather than measure it, or what part of it do we need to measure in order to model the rest?

A question that we're going to look at very hard is whether the vibration modal patterns are really characteristic of every location in the shuttle that we might be on, and also how they vary between the space lab, the cargo bay and the mid-deck, and within the space lab.

In the area of disturbances, we're beginning to get the impression that the shuttle is like some sort of bowl of Jello or something; you hit it and the impulse gets soaked up into the vibrational modes fairly quickly and doesn't survive a long distance propagation. But those are just impressions we've gotten from looking at some of the data. These missions we have in front of us are the first ones that will give us some direct data on that, because we'll have accelerometers in different locations.

Then the final thing that we're looking at, to try to get to some sort of answer about how the vehicle itself behaves, is compiling lists of observed disturbances that we see during the mission, such as thruster firings and crew motion.

Now, because of the various distribution of the accelerometers on the missions to get at the answers to these questions, is going to take a while. For example, on this first mission we've got the accelerometers back in the space lab. However, some of the disturbance sources are located at long distances, such as the mid-deck, the primary one of interest these days being the treadmill, which we don't have instrumented until this mission. So at least in looking at the treadmill and its disturbant effect on experiments, we're going to have to string two missions together and it's going to be downstream a little while before we put those two missions together, and get that full answer.

Some of the other questions, particularly those that involve propagation around the vehicle, probably have to wait until after USML-I. And USMP is the first chance we really get to use the SAMS and the OARE systems out of the cargo bay. So we're looking at a stretch of a couple of years or 18 months before we'll really get a full fix. But our objectives right now are to start getting the questions identified and then try to answer them within our ability from the data that we see on the missions. I'm not sure I know what all the questions are. As a matter of fact, I know I don't. But I guess trying to find the questions sometimes is always the first step in trying to find an answer.

But anyway, from the position of Marshall, that's the program we have laid out in front of us; we're looking at a very active period over the next 18 months to start driving towards these things that I now perceive will have a lot of influence on the question of vibration isolation. And right now our plans are to go after these questions as fully and as fast as we can, and to report back to this group and other groups. We certainly don't feel very protective of this data, and anybody else wants to get the SAMS data and work on it, we will do our best to cooperate.

N. RAMACHANDRAN: Okay. The other topic that was discussed yesterday was umbilicals. That it is a critical issue, although I don't have a great deal of insight on that. So I'll just leave it at that point.

Source isolation was talked about and I think it is an important issue. We don't want big generators or fans that create a whole bunch of noise sitting by themselves. That's an obvious source that we can locate and we can probably isolate.

Then we want to identify experiments that need isolation, hopefully by doing some numerical computations ahead, rather than coming back and doing the numerical analysis after the fact to try and see why it failed. So a first step would be the numerical study up front to try and see if a particular experiment might need isolation of some sort, active or passive. We would try to stay passive if possible, because that is cost-effective.

In talking to NASA-Ames, Michael Horkachuck was telling me that with the EVA activity I anticipate on the space station, any minor tug on the astronaut umbilical can create milli-gs, which excites all the frequencies; if that is the case, we're in big trouble.

I think vibration isolation has a future, but unless we try some things out and demonstrate its utility, users may not be aware of the full capabilities that you might have to offer.

J. LUBOMSKI: Gary Martin would like to make several comments before we show the video on STS-32.

G. MARTIN: I want to thank on behalf of Microgravity Sciences and Applications everyone's participation here, and especially to thank Lewis and Joe Lubomski for planning and organizing this excellent conference. I think there has been a lot of good dialogue and excellent recommendations coming out of the working groups that I'll take back with me to headquarters, and hopefully we can improve our situation as far as the ability to operate sensitive experiments within noisy spacecraft.

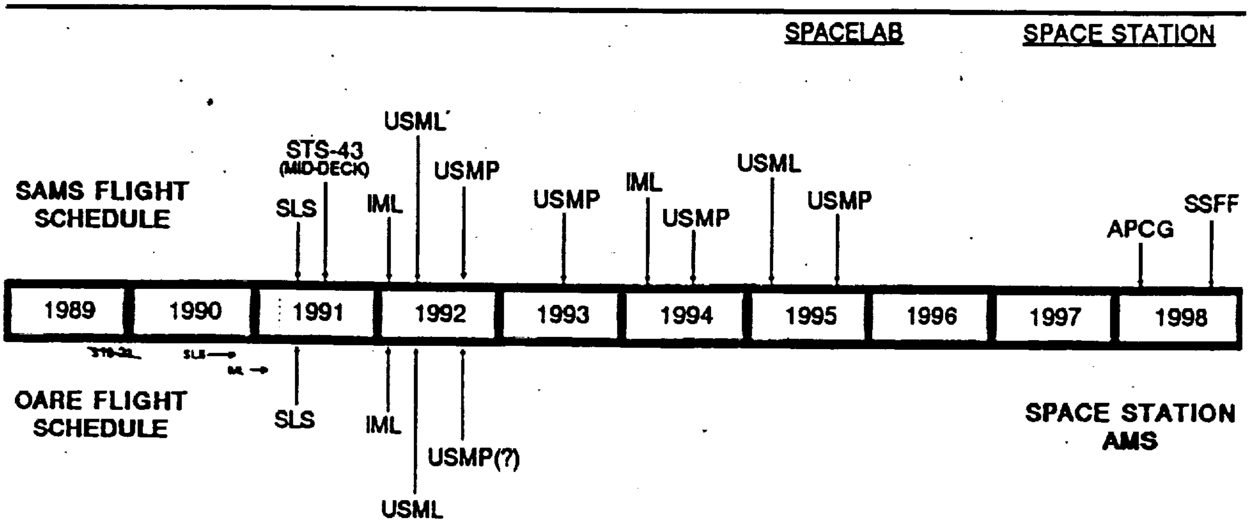
I'm glad to see so many people here from different areas, universities, corporations, both within the United States and outside, the different space agencies that participated. I think this is a very important area, and to get this kind of participation is beyond what I thought, when Joe and I started talking about it, we would ever achieve.

The only technical thing that I wanted to point out, and I think we talked about this the other day, was that I'm really happy to hear that at least in certain situations the problem's been solved, the control algorithms exist, and we can isolate certain experiments with what we know today.

I do want to point out that where we have this low frequency sensitivity, and especially when you get into the gravity gradient and the drag effects, there's nothing we can do about that. That's got to be the spacecraft's domain, what it provides, and we know we have that problem. We need to understand the limits of what we can isolate against. It's very important.

And again, I want to thank everyone. I'll be taking these recommendations and using this information in the future and passing it on to our hardware developers and our PI's. Hopefully we can now accommodate experiments that we earlier couldn't do. Thank you.

MICROGRAVITY PAYLOADS
SCHEDULE OF MAJOR FLIGHTS



TECHNOLOGY REQUIREMENTS

MODERATORS: Gerald Brown and Carlos Gradsinsky, NASA Lewis Research Center

G. BROWN: I think the way that we can get started is with list of a few possible topics, to which I would like to invite other people to add others. Then, once we've assembled the longer list, I'd like to give each of you three votes as to which ones you're the most interested in. We'll quickly go through that vote, and then we'll take topics for discussion in the order of the most votes.

The question of active versus passive or even hybrid systems was just raised. And we put that up on the top of the list there. If we have active actuators, there are many issues that may be raised there; we've heard two points of view as to whether Lorentz or attractive types are better. And I think under that category could still come "Should we ask for staging or some kind of rack and pinion coarse stage and then a fine magnetic or other type of stage?"

I don't think I heard anybody mention it, but should we ask them to control this center mass of the space station? That would make it a heck of a lot easier for us to isolate at the end. When the people go down for that conference one gentleman mentioned, they might consider pushing something the other way to keep the station stationary. To what extent would we like to have other people solve the problem at the source?

The umbilical problem has just been brought up. There are a number of control issues. The issue of transmitted versus direct disturbances on the payload was just brought up. Do we still have any questions on digital versus analog control approaches? What about nonlinear strategies? Someone mentioned a cubic control law yesterday or other non-PID type control laws and sensor technology. Now, what other general categories should we put up here? Does somebody think that there's something being missed?

A. VON FLOTOW: There's a major gap under actuator, PVDF.

G. BROWN: Okay. I suspect actuators will be a reasonably favorite topic, so we can list all those at that point. Are there other general categories that didn't catch somebody's particular interest or an item that may be overlooked and cause us trouble later on?

G. MCCANLESS: You might have caught this in your active versus passive systems, but I think a very significant thing is down in the lower frequency regime, from 0.1 Hz upwards; it's just a very difficult arena to try to isolate there. I don't know if that should be a separate topic, but it needs to be discussed.

D. HAVENHILL: I'd like to see cost added to that list. I know this is a technical meeting, more or less, but these things aren't going to sell themselves unless there is some payoff in cost dollars.

G. BROWN: Okay. Can I call it cost effectiveness?

D. HAVENHILL: Yes, that's fine.

B. TRYGGVASON: I'm just wondering if we should add to this, I'm not sure if it's this session or the other one, but the fact is that in the high frequencies, you're really not going to get the isolation that the spec calls for. Then one day, the spec's going to be changed to bring you back into a displacement rather than acceleration. Perhaps some time should be spent on talking about the specification.

G. BROWN: Okay. Specification characterization -- do we have reliable or good specs?

J. BLACKBURN: Anytime you describe a control system, you both need that number and you also need to know a similar number describing the disturbances; so disturbances are equally as important as the specifications.

C. GRODSINSKY: The specs are good to talk about, as well as perceived environments, but I don't think we should spend a whole lot of time discussing that, because there is a lot of work being done in that arena. Our best guess is to assume space shuttle conditions. And if we want to assume a space station, we can push that lower frequency regime problem more than just that we're going to get disturbances from astronauts in the lower frequencies.

J. BLACKBURN: I think a good example of where this has bitten you guys is the treadmill problem. It's a totally different type of vibration environment than you're accustomed to isolating on the space shuttle. And for that reason you're going to have to look at some other mode of control, such as momentum interchange or something like that to solve that problem, rather than simple Lorentz force actuators. It's just not going to work for you.

C. GRODSINSKY: Agreed. I guess that in actuators I would look at all the possible actuation schemes as well as the control. There are certain specific ways you want to go about tackling certain problems. And then there's the idea that in a general sense we do know some kind of background environment (i.e., the type of general floating platform or certain kind of system that you might devise to stick somebody's experiment on, and that has certain power transfer, etc.).

G. BROWN: Is there anything that is of minor importance or anything that is maybe underneath actuators or under controls that should be a separate item by itself? Does anybody see any errors in comparability? Okay. Remember you have three votes apiece. Who wants to talk about active versus passive or type of control? (At this point the votes were taken and totalled)
The subjects considered significant in order of priority were:

-The umbilical problem
-Actuators
-Control issues
-Source vibration control
-Sensor technology
-Cost effectiveness of isolation
-Specifications and disturbance criteria
-Passive versus active isolation
-Center of mass control

G. BROWN: So we're going on with umbilical problems. Why don't we just shoot for ten minutes of discussion on each one as a starter? I would also like to ask for a volunteer to report to the (plenary) group tomorrow on each one of these issues. Who likes this problem and will be here tomorrow morning and can report the general gist of what comes up here? (Volunteers were selected.)

C. GRODSINSKY: To start on the umbilical problem, we at least at Lewis have been looking at issues to be looked at next, and this is one area where I have some ideas. What you said, Andy, about smart umbilicals makes sense, we need to how to build a better umbilical. There are going to be many of them and, as you said, we need some specifics on who wants what -- if someone wants vacuum pumps across, and whether you can disconnect them or connect them.

But anyway, to start on umbilicals -- what are people's ideas or has anybody else have been looking at these things? I know that some of you looked a little at the problem, and we've all been assuming that we have a constant stiffness, which I don't think is at all realistic.

R. G. OWEN: What we did is just to use a plastic spring and that's it. We didn't have time to look into it in more detail. But what we really want to do is get hold of a piece of plastic, and do some tests on it. Has anyone done this? I don't think anyone has, so we need to look at the damping or the hysteresis of the actual umbilical and its actual structure -- what it's going to be made of, etc.

As far as we're concerned, we looked at a number of different alternative concepts to our sort of noncontact system, which I explained yesterday. So we're aware of the problem of the umbilical and lots of other things, but we can't actually incorporate all of these things into one design. The umbilical problem is a real problem. And it would be a good idea if somebody did do some tests on them to look into the thing in a lot more detail.

A. VON FLOTOW: I'd like to make a comment; the argument against that is it's kind of boring. Okay? Testing hoses is not as sexy as designing. I'd like to make the strong statement that if you can build a mount without an umbilical, then it's a solved problem, and I think that statement is only a minor exaggeration. As evidence that it's a solved problem I'll use Sperry's FEAMIS and the Welsh project, although that has not gone as far as Sperry's FEAMIS.

So if you can say the mount doesn't need an umbilical, I think then research is at a close. And I'll admit that it's a minor exaggeration, but not a big one.

P. ALLAIRE: I think that the umbilical problem is very important. We've designed our experiment that you saw yesterday to have a umbilical in it, and we currently have picked out what is simply an air dashpot, which has both some stiffness and damping characteristics. But there's a place in there for other units. We could put a number of different possibilities into that region and look at the effect that they would have. I think a very important factor is to design the control to take that into account. And somehow, we've got to have a match between what the umbilical characteristics are supposed to look like and the control algorithm you're going to use. But I agree that it's a very simple problem if you don't have umbilicals.

R. G. OWEN: A fluid science experiment is probably the easiest kind of experiment to accommodate in a microgravity isolation mount, because its power requirements are quite low, and it doesn't need this kind of vacuum venting problem. But as I said before, we identified lots of experiments. All the material science experiments seem to be the types of experiments that would benefit the most from a microgravity isolation mount, and because of vacuum venting etc., they also appear to be the most difficult to actually fit onto a microgravity isolation mount and get them working.

J. BLACKBURN: One thing that occurred to me while observing Dr. von Flotow's presentation was that the design of an actuator which incorporates the umbilical within it is a viable option. In other words, perhaps the umbilical could be flexible or of some sort of elastic material that could be embedded in, say, a voice coil or Lorentz force type actuator. If the actuator is characterized individually with that tether in place, then you could still obtain for that actuator, a sum scale factor that gives you Newtons out per volt in, and would be independent of the spring effect of that tether, or umbilical incorporated in it. Thereby, you would have the umbilical running directly to the payload through the actuator without any additional spring added to your model. That would only work in translation, and it's just an idea, but I think that someone ought to investigate that.

C. GRODSINSKY: Another point and maybe this is a question directed to Stuart. I know, looking at the combustion facility module and fluid physics module, there are not that many connections in the fluid physics type. But on furnaces, we may be tackling difficult problems with all those lines coming in and perhaps a number of different racks. What may be better is to have a self-contained facility where you can then isolate the facility, or a rack. If everything was ideal, everything would be totally rigid to this rack and the appropriate dynamics of the rack would be such that you didn't excite anything internal to that rack. You just have to take care of any type of transmission of energy from outside of the facility module. That's something that I feel could be manageable, as opposed to these vacuum and fluid transfers across interfaces -- using some big garden hose, as someone stated.

S. GLAZER: I'm not aware of the exact requirements for all of those facilities, but I would imagine that there would be quite a bit of engineering and thought going into the development of that in the facilities themselves.

P. ALLAIRE: As another possibility that you might like to take into account in a vacuum situation, you might consider using magnetic bearings on the vacuum pump and putting the vacuum pump on the same platform as the rest of the experiment. We've done measurements on vibration reduction using magnetic balancing in our laboratory, where we've gotten over 40 dB reduction. So that's another possibility, to combine isolation at the source with this kind of technology.

B. TRYGGVASON: I'm not sure what's to come out of this part of the workshop in the discussion of each of these elements, but I think that in order to come up with some reasonably useful statement on what to do about umbilicals, you have to pre-guess what a lot of these experiments require. And for some things, it's not that difficult a problem. We know we can get power across; people in Wales have shown that. We know we can get heat back out; they've shown that as well. We know we can get data in and data out. We know we can get control signals in through infrared links and so on. So a lot of the umbilical problem, I think, is not that difficult. For something like fluid in/fluid out from the experiment side, it's a little harder to conceive than transferring heat in or out, since we know how to transfer heat in and out without rigid connections,

When you look at what a experiment requires in a vacuum, if you have a bell jar or something in which you have your vacuum, then quite likely you can apply the isolation inside the bell jar to the actual small experiment. You're not going to have a huge chamber that is evacuated on the space station, like a whole double rack or something. But even if you do, maybe the isolation is inside the vacuum chamber.

G. BROWN: If the vacuum system is just to remove gases evolved during the experiment, then you might not be able to use a bell jar. You may need continuous pumping.

R. G. OWEN: Some of these experiments require high vacuum pumps actually on the platform and actually running throughout the experiments. So these are going to create disturbances on the platform as well.

B. TRYGGVASON: But isn't this where you isolate the sensitive part of the experiment inside the vacuum chamber, rather than having an umbilical going across from the experiment rack to the station?

R. G. OWEN: Actually, what I found out was that there were some experiments that required the high vacuum, and there was a pump actually on the platform. And they also required a continuous access to the Columbus lab high vacuum system as well. So their vacuum requirements were so high that they had to have two rigid pipes, first the venting pipes, and then the high quality vacuum, plus another pump on the platform to produce the required vacuum quality. Perhaps that's an extreme example.

B. TRYGGVASON: The point I was just suggesting is really that you have to do a lot of pre-guessing as to who requires the vacuum and for what reason. That will answer to size and things, to try to solve the isolation problem between the experiment rack and the station, rather than inside the vacuum chamber or inside the experiment itself.

C. KNOSPE: Maybe the best we can do in this meeting is just to go to the experiment people and first demand that we have no umbilicals; then if they won't give us that, demand they tell us what umbilicals they have to have. Ultimately they're the ones who are going to have to decide what they need, but we should try to get them to live without the umbilicals, if possible, because that will make our job easier.

R. G. OWEN: The actual situation between us at Bangor and ESTEC at the moment is that we're designing this facility, and we've more or less decided on the design; it's up to the experimenters to fit their experiment onto this platform. And I think that's the only way you can get people to actually get experiments on the platform. If they know that a facility exists, then they'll start doing something, but not until somebody's actually designed and built one.

G. MCCANLESS: In general, vacuum is only required before and after the experiment. I don't question that there are some experiments where they would want to have full vacuum, but mostly it's a purging vacuum to get rid of something that's toxic or bad. But you mentioned we can reject heat, and the gentleman from Wales was talking about one kW or thereabouts through the fins. If we use a commercial-size furnace for growing electronic crystals, we're talking about 15 kW of power and that heat has to be removed. I would like to get your comments. Based on the quantity of fins you were showing, it would seem to me that we would have to run cooling fluid into a furnace like that.

R. G. OWEN: Yes, definitely. This kW was kind of a hard spec to achieve. It was a spec given to us by ESTEC and in the actual (subunit rack) design, which I showed you yesterday, it was impossible to get that, because you're actually reducing the available volume for the payload platform. In other words, you won't have anything left over in the end. But to get back to this kW figure, the way we reached this is that we did a quick survey on space furnaces and we just found out that a lot of these are rated about one kW; obviously some of them are much higher, but they are sort of few and far between. So at a certain point, we had to decide on a figure for the heat in, to design the power transformer, and also to get the heat out via these fins. So this is where this figure of one kW actually originated from.

G. BROWN: Could I ask a clarification question on that? I mean one kW of power in doesn't mean that you need to dissipate the kW out.

R. G. OWEN: Well in a typical material science experiment, when you have this sort of furnace melting a sample, you perhaps might only need the kW or near one kW for 10 or 15 minutes to actually melt the sample. Once you've done that, then your power requirement drops to a few hundred watts.

So we've done analysis on this kind of thing and we actually disagree that you need that kW of continuous heat dissipation. Also, you have to think of the thermal dynamics of the experiment and the platform. The master platform will absorb a lot of this heat and this will prevent the temperature on the platform from rising to a high level by the time that the heating part of the experiment has stopped. Then your heat input or the power input is a lot less, and the temperature on the platform stabilizes and goes down. So all you have to do is make sure that you don't repeat the experiment too soon, to ensure that the temperature on the platform has gone down. You may need one or two fins to help out with that, but you certainly wouldn't need all these cooling panels that I showed you yesterday.

J. BLACKBURN: There seem to be two issues here: first if you need an umbilical to begin with, and second, if so, what you do about it. And I think that it would certainly be preferable to have the option to use one if you need one. A number of possible reasons for needing them have been brought up: vacuum, flow of fluid, etc. Perhaps we should make a decision as to whether or not they are necessary -- I believe they are -- and then move on to whether or not there are possible solutions to the problem.

G. BROWN: Okay. Perhaps we could leave it at that point, because certain aspects of it will come up both under actuators and more properly maybe under controls. Can we deal with them there? Let's move on now to actuators. So who thinks they know the central issue under actuators? Who wants to start to make a comment on actuators? We heard preferences for Lorentz actuators and preferences for attractive actuators. I believe someone -- I believe it was Carl -- maintained that for long stroke you can make just as efficient a Lorentz actuator as you can an attractive one. Does anyone want to take issue or support that?

N. GROOM: Let me muddy the waters a little bit and add another one: large gap suspension. If you can do away with the umbilical, then you can introduce the possibility of using large gap suspension systems, of the order of four inches or more. This is the same approach that you would use in wind tunnel magnetic suspension systems.

G. BROWN: Very large air core coils?

N. GROOM: Right. And there is another issue that would have to be resolved: if the process itself would not be disturbed by magnetic fields (and from what I've heard I don't think that a lot of them will) you would still have the problem of shielding throughout the surrounding area. But I just introduced that as a possibility; that would cover the coarse/fine requirement.

G. BROWN: I'd like to remark that many processes that you don't think have anything to do with magnetic fields do get affected by them. For instance, 10 or 15 years ago people discovered that concrete cures faster to a certain hardness if you expose the slurry to a magnetic field before you let it start setting up. It's hard to imagine why that happened, but the concrete includes some ferrites or some iron oxides and such. It might surprise you.

N. GROOM: Yes. I was discussing that and that very same thing came up, that it might help, as a matter of fact.

G. BROWN: It might help, or it might hurt, but it can have an effect sometimes.

C. GRODSINSKY: I might add to that, if you have a large gap in these magnetic fields and you have data or something moving in your lines through the fields, you're going to be getting signals that aren't strictly due to your science or whatever, just due to the motion of your conductor in the magnetic field.

P. ALLAIRE: I guess I'm a proponent of magnetic actuators, whether they're Lorentz or attractive actuators, depending on the stroke and whatnot. But it seems to me some sort of actuator with a mechanical connection between the platform and the spacecraft introduces an umbilical-like effect, which we've just said isn't very good. So, I wonder how we can use mechanical actuators. I'd like to hear some proponents of mechanical type connections of one sort or another, if there are any here, and why that would be better than a magnetic non-contacting actuator.

J. BLACKBURN: While it is true that the mechanical contact between the payload and the surface, or what we call the base and the stabilization control, does introduce or has some transmissivity between the base and the platform, it's also providing your actuation force. It's much like the problem that von Flotow brought up with Carlos yesterday. You have a DC bias to hold up a mass against one G, and you have that effect in the disturbance sense in that you're introducing disturbance through the conductivity of the actuator. But the actuator is itself canceling out the motion.

I think the real issue when you connect any sort of actuator to the platform, whether it's physically connected or not, is whether you have to have some sort of inertial control. In other words, a lot of the attractive actuator control schemes are not inertial, but position-related. I think that's one of the most important issues there is, to see to it that the control algorithm institutes some sort of inertial actuator, i.e., an accelerometer rather than a LVDT sensor; otherwise it won't track it.

Another issue is the throw of an actuator: In general, the mechanical type connections, i.e., the Lorentz force actuators and even our momentum-type actuators, are able to impart a longer throw, which at lower frequencies is very important. The lower the frequency of the disturbance, the higher the throw you need from the actuator to cancel it out. As I understand it -- and someone can correct me if I'm wrong -- the attractive-type actuators are limited or have a smaller capability in their throw than to do the Lorentz force. Is this true?

D. HAVENHILL: No, it's not true. I'm not a magnetics expert, I'm a controls guy, but we have done a lot work on attractive actuators. To preface this a little bit, the work that we've done in terms of trading off Lorentz versus attractive is normally for applications where we have to apply large forces at sometimes large gaps, and weight is a big problem. That's not necessarily the case here. And in fact, I think if you go through a tradeoff study here; I don't think that in terms of power and weight you're going to have much difference between Lorentz versus the attractive type.

However, you could make the throws as big as you want, just as you can with Lorentz. When you're talking about throws on a Lorentz, if you do like the University of Virginia did where you have a small gap in the cross-axis direction, you can make a very long throw very efficiently. But if you've got to do that in six degrees of freedom, you've got to leave yourself a big gap in order to be able to move axially, and those things get big faster than the attractive actuators do, much bigger.

J. BLACKBURN: What about linearly?

D. HAVENHILL: Linearly is solved by using the flux feedback in our case, or any of the various other forms of feedback. You can linearize those actuators.

R. FENN: I also work mainly in the controls area. There is a fellow at SatCon who did an analysis for Lorentz force and attractive, and he found that if you look at the weight, the volume and per equivalent force, capability and stroke, there's a breakeven point. Beyond that the

Lorentz force wins out in terms of mass. I can't remember the analysis now, but I guess that in either case you need a certain volume with a certain flux in it.

Actually, I don't think you can really work it out, but that's what he came up with. I don't think either of us are really experts in that.

D. HAVENHILL: That's contrary to what we found; we found that when you start getting down into small forces it's not going to make a lot of difference. But as you move up on the force curve, the attractive will always end up weighing less and using less power, or whatever trade-off you want to make; you can always trade off weight and power in the actuator.

R. FENN: One other aspect is continuous power versus peak power. In an attractive actuator you have saturation problems, whereas in Lorentz you can have a constant flux. Then your peak power and your coils are just limited by the thermal heating. So if your application requires impulses, you can get a tremendous force for a short time, if your electronics can do it.

D. HAVENHILL: You're right. They do saturate and so you get an LDIDT term. However, when you're looking at a force loop or a control loop band with the .01 or 1.0 Hz, you're not going to have huge swings in current rapidly; it just doesn't happen.

R. FENN: No. I'm talking about magnetic saturation where the attractive forces are limited by the saturation flux.

D. HAVENHILL: It's limited in Lorentz also. If you saturate your coil, you have to put an infinite amount of current in to get force out, so I guess I don't see that.

R. FENN: Well, typically the coil doesn't add that much to the flux in the circuit unit, push/pull changes.

D. HAVENHILL: I guess I don't understand where you're coming from in terms of saturation. If you saturate the iron it doesn't matter which actuator you're using.

R. FENN: Well, no. The primary effect of the coil is -- the current is what drives the Lorentz force, but only as a second order does it affect the magnetic flux? Ferro-magnetic effect, where you're turning the dipoles, saturates at some point.

N. GROOM: Let's see if I understand what gap you're talking about. For a Lorentz force actuator, are you talking about the gap between the permanent magnets or the flux-producing elements?

R. FENN: Right. Half of it may be the stroke, plus or minus.

N. GROOM: Yes. For a ferro-magnetic attractive, I guess there is a practical limit to the gap that you can go to before fringing starts to kill you.

R. FENN: I guess you can imagine having a gap and you can either put a piece of iron inside it, or you can put a coil inside it.

N. GROOM: Right. But are we all on the same gap here?

R. FENN: If the iron were the same thickness as the coil, you'd have the same stroke. Then you look at the attractive forces generated in the gap versus the Lorentz force generated when you put current through that coil, which is in the gap.

N. GROOM: Gap-wise you may be about the same. I guess you may have more freedom and one dimensional than with the Lorentz force actuator -- but then you have the other limit that Doug alluded to, I guess.

R. FENN: Yes.

C. KNOSPE: Neither of the actuators we've discussed, the Lorentz force or electromagnetic, are typically very good if you're talking about going to something like a four-inch stroke. It's very difficult to design either one to be very efficient and linear, or whatever else you want, when you're talking about strokes of that size. And as far as trade-offs, for a centimeter stroke you might find one to be a little bit better than the other. Some people say one, some people say the other. The real question here is whether we're going to need a larger stroke than, say, a centimeter and, if we do, do we want to start thinking about using a coarse/fine type scheme?

G. BROWN: Yes. Why don't we discuss that a little bit. I think that's an important topic. Certainly, like in the design that you presented yesterday, you can keep your magnetic gap or any other kind of gap to, say, 50 mils or something, and have a follower system that can be very noisy and have mechanical contact. Let me ask if that presents any kind of controls issues? It seems to me that follower can't foul things up, but I'm not a controls guy. Does anybody find any problems with that?

D. HAVENHILL: Well, with the SAVI system that was built at the Air Force Weapons Lab over the past few years, that's exactly the scheme that we used. This system was a little bit larger than what we've been talking about. It's about three stories tall and it's about 15 feet, 20 feet in diameter. But we do exactly what the University of Virginia has presented and it works very, very well. We control it a little differently than they talked about, but basically it's a very fine system. We are 80 dB down with that system at 1.0 Hz, and using nothing but the gap feedback. There is no inertial sensing or what have you, because that wasn't part of what we were supposed to be doing.

Also, in that system we're able to transmit 27,000 Nm of torque across the gap while isolating. So we slew a rather large payload while we are doing that, and the follow-up system works great. It just follows right along with it. What we did though, is to build in an actual inner-gimbal structure. Some people talk about mounting an actuator itself on a follow-up actuator, like a magnetic actuator on another actuator, but we found that wasn't very practical. The Stewart table technique that the University of Virginia came up with is much better; at least that's what we found.

G. BROWN: Right. And could that be incorporated into a rack so the Stewart table sort of surrounds the experiment so it doesn't take up so much space? Is that geometrically feasible?

C. KNOSPE: I can't say we've looked into that level of detail, because we're still working out the concept, although the idea is that, yes, maybe you could. It seems to me it might be more compact than the kind of a carriage gimbal systems that University of Wales was looking at, where they had a lot of space being taken up by the carriage and gimbal systems.

We haven't done any studies about how much space it actually takes up. In our design that we showed yesterday (that's obviously a lab kind of concept) we have the electromagnetic isolator on top of the coarse platform. You could put it beneath between the legs, in an effort to try to conserve space; you have to do a lot of the mechanical kind of design. I think there's technology you could use to try to conserve space on it and make that kind of scheme work.

G. BROWN: Other comments there? It seems to me that staging is almost sure to be the best way of dealing with large motions. The large motions, I gather, can come about in two ways: either from first-mode flexures of the station or from gross motion of the mass on the station. We keep talking about actuators that have to take big strokes, and maybe we're doing that unnecessarily. So I would almost recommend that people always consider a double-staged system, but maybe others think that's too strong a statement. Does anybody disagree with that?

J. BLACKBURN: You may just not have the large throws, in which case it's certainly cheaper to go with the single actuator. That's the only exception I can see. For instance, in Alamogordo, New Mexico, my company works on a project called the Seismically Stable Platform, in which we're trying to control three degrees of freedom down to nano ratings and nano Gs. We get by all day long with throws that are less than a millimeter; so it just depends on the application.

G. BROWN: Maybe that's true only if you're sitting on bedrock or something.

J. BLACKBURN: Well, yes. You have to decide what your application is, but I personally don't think you're going to have six-inch throws on the shuttle either. It depends on what you're looking at.

G. BROWN: But I think a good fraction of an inch is to be expected, right?

J. BLACKBURN: BEI actuators have several models that will do that in the linear sense that we've looked into. I don't know what else is out there, but I'm certain that there are Lorentz force actuators that will do that.

D. HAVENHILL: Yes. I agree with the gentleman down front. A lot of whether or not you need staged actuation, or two stage, or coarse/fine, or whatever you'd like to call it, depends on what your input is. In the system I just mentioned, the reason for doing it was because you had to gimbal the payload as well as to isolate it. In that situation you definitely need to do something like that.

However, for the disturbing environment that I've been seeing on Freedom and on shuttle, it doesn't look to me like you're going to see much over a half-inch throw anywhere. I think for the small forces that we're talking about that's required here, I think you're going to be spending a lot of money for it and not getting much return.

C. KNOSPE: That may be true. When you look at this situation, the first thing that I did when I looked at the problem was to make it one centimeter stroke. That makes the problem a lot easier, because you don't have to worry about coarse/fine and you can do stuff like the Honeywell people have done with the flux feedback to linearize a magnetic bearing or whatever, and it's a very nice scheme.

Perhaps we want to decide right now that we're going to only have a half inch of stroke or a centimeter stroke or whatever. It's a very simple calculation to figure out what G level you can no longer isolate against. In fact, you can probably get it out of my paper, it's in there. Then we go straight to the science people and say, "Okay, can you live with this?" And if they say "Fine, that's great," then and we don't need to talk about it anymore.

G. BROWN: Well, the problem is if you design to a centimeter of throw and you actually get a half an inch of station displacement, you don't just miss the spec a little bit, you bang the wall.

C. KNOSPE: Well, you put some centering in obviously, but that comes in with -- (inaudible.)

J. BLACKBURN: The gentleman up here from Honeywell hit on something that I think is relevant. That is, there are two different kinds of controls that we're talking about here: one is pointing control and the other is stabilization.

Generally, in a pure stabilization realm all you need is a very small throw; so if stabilization is your primary goal, it's likely you won't need these huge throws. But in a pointing type application, like a huge telescope, for instance, you both need to command the position that serves as your first stage of isolation, so to speak, as well as to stabilize the line of sight of the telescope. It sounds like that's similar to what they're doing.

And in Carl's case you'll be able to both command the position of that thing and isolate what's going on, and that's one of the advantages to the two-stages of actuation. Pure stabilization in itself shouldn't require that much of a throw.

C. GRODSINSKY: I'd like to add that a lot of these issues are pretty much based on specifications of what someone might need or what one might use. But it seems to me the way this technology will go forward in a space-based type of system is to somehow go with the approach like the people in Europe have; they built a system and it does this.

Now they can go out with proposals to say, that they have this system and can you do some science with it? You're going to get a line of people saying, "Yes, I can do something with that."

Basically, the way we work at NASA is to come out with these requests or announcements of opportunity; but these are strictly based on science. Then what happens is that they get peer-reviewed and they pick somebody; then we have a list of requirements. But what we've seen in the last three years is that these lists of requirements are first not very realistic in many cases; second, they don't really know what they want; and third, they go at their science in the way they know how to do it in their laboratory. I don't know if we'll ever bridge that gap except by actually building a flexible system that will take care of a certain volume and a certain realm of inputs with certain data and stuff like that, and then go out with that in the announcement of opportunity. I don't know if this is the right place to say that. I don't know how it's going to ever be resolved, but this is what I see. Otherwise, we're going to keep going on forever talking about how we can do this and we can do that for such a problem.

J. BLACKBURN: Right. And that's the reason -- I think one of the motivations for these kinds of groups is for them to figure out how to make such systems as versatile as possible and be able to do as many things as possible for the same dollars.

C. GRODSINSKY: But there's a limit as to where you go. You got to cut it off at some point and then you have to go on and build something.

J. BLACKBURN: That's true.

C. GRODSINSKY: And before you ever go to the next step, no matter how simple it is, you need to just show that you have something that works; then you move on to the fancy stuff.

G. BROWN: Speaking of limits on where you go, I think after this next comment we'll have to move on to the next issue.

D. HAVENHILL: I guess this is sort of off the subject Carlos was talking about, but we've tried that. I mean, we have a piece of hardware that was flight-ready in 84, and there's nobody beating down my door to fly that thing. Maybe it's a marketing problem on our part or whatever, but I think
you've got to really be sensitive to what the experimenters want and you've got to design around what the experimenters want in a lot of cases, and try to be as versatile as you possibly can.

C. KNOSPE: I just want to make a little bit more pitch for our coarse/fine type of scheme. One of the nice thing about looking at that type of concept, as opposed to trying to accomplish all of the problem at once, is that if you can work and isolate with the umbilical for just using a fine control, say electromagnetic or a Lorentz control, just handling a half inch of stroke, you develop that technology and it works. If that's all they need in space, great, you've got it for them; if they need more, you just stage that on top of the coarse. So you're not throwing anything away by developing only one.

G. BROWN: Let's get a little bit in the controls issues. Who thinks that there's something seriously unresolved at this point?

J. BLACKBURN: I probably talked five times already, but I'm very interested in this area and I hear a lot of people mentioning that they're going to do multivariate modern control on these MIMO systems. I just wondered if anybody has really built one of these and got one of these systems working. It seems like all the ones I've seen that are working are parallel control schemes. Has anyone got a modern controller running yet?

R. FENN: Yes, there are a couple of DSB programs we have installed. One of them is a linear one and it's a MIMO controller. It's not very complex, because it has a lot of symmetry. But the eight sensors signals are -- transformed into the center of mass coordinates for the modal system. So, I guess that's a simple case of one.

J. BLACKBURN: Is it a state space controller or are you just manipulating matrices? Is it a real state space controller where the entire dynamics are described? Do you know the difference between the two that I'm talking about?

R. FENN: It could be written in state space form. Once you decouple it, then your second order dynamics can be expanded into two first-order equations. I think the answer would be -- that's about what we have.

J. BLACKBURN: Can you operate one of your degrees of freedom without operating the other?

R. FENN: Yes.

J. BLACKBURN: Yes. And generally in a state space modern controller that's not possible. You have to do the whole thing at one time. I just wondered if anybody had done that.

G. BROWN: Do you really have to get into that if you don't have a flexible structure?

J. BLACKBURN: Well, the reason for the modern control approach is generally because of the coupling problem in parallel controllers. Some systems are inherently non-coupled, but if your system is mechanically coupled such that the degrees of freedom bleed into one another, then that introduces instability in parallel controllers.

G. BROWN: How can you have that if you have a rigid body where there certainly are six degrees of freedom?

J. BLACKBURN: The very simple case would be a von Flotow's model, in which he has springs attached all around the perimeter. In order to actuate one horizontal direction, he's going to tell two of those actuators to go. They will be of slightly different scale factors and therefore will exhibit two slightly different forces. Put a little bit of moment in it, tilt it about azimuth and you get both motion in the degree of freedom that you want and motion about the azimuth. Those kinds of coupling destabilize parallel control loops.

G. BROWN: But does the error only arise because you have a slightly unknown actuator characterization?

J. BLACKBURN: They can be any number of things, and that's why you either go to the multivariate approach, or you can characterize what the coupling is, correct the coupling mechanically, and go with the parallel control schematic. There are one or two arguments there.

G. BROWN: Okay. I also work with magnetic bearings, and there you can have a flexible enough shaft so that you may be forced to go into MIMO or something. But I didn't think that for the rigid experiment support you'd probably have to resort to that. I'm surprised.

J. BLACKBURN: I don't know. I'm not sure.

C. KNOSPE: It all depends really upon the whole question of the umbilical. If you don't have an umbilical, as von Flotow was saying, the problem is essentially solved. We know how to do this. People have done it before as far as isolation. But if you've got an umbilical in there, this umbilical is going to be running from some point A to some point B, and it's not going to be lined up with the center of mass or anything else. You're not going to know where the center of mass is, and the center of mass may shift through the various things going on.

So the question becomes whether you can design a decoupling controller, considering that you don't know the dynamics perfectly. The whole point of doing a decoupling control is that you design single-loop controllers, you recouple back through some matrix transformations, essentially. Can you do that when you don't have a very good model of the plant? Considering the fact that when you've got a stiff umbilical in there, you're going to have to use high-gain acceleration feedback, and when you start using a lot of gain, any sort of mis-modeling you do really gets you in the end.

I say more power to you if you can do it with just decoupling; it's an easier way. But the MIMO control strategies aren't too terribly hard. It's a pretty straightforward synthesis procedure, so we shouldn't get too afraid of them. Whether they'll work any better or not is dependent upon the skill of the designer who is using the machinery of the synthesis procedure he's using. And he should try to use that synthesis procedure in order to try to do the same thing that the fellow from ATA is talking about: to minimize the coupling.

Now, which way you go through it mechanically or if you try to do it in the control loop, that's another thing. There's no question we must do something about uncertainties due to coupling and center of mass, because that's what's going to kill you if you try to do high-gain acceleration feedback.

A. SINHA: But when you assume that you know all the six degrees of freedom accelerations and six relative displacements, essentially you are saying that you know all the states equivalently, and in that case, robustness is not that big a problem. Still you have to make sure that it's stable. But there is more of a problem when you are not using all the sensor measurements. That's where, particularly if you are using observers to estimate some of those states -- robustness is a major problem, but not as much as parametric uncertainty.

J. BLACKBURN: That's if you have a sensor to measure all these states. A lot of states have pretty weird quantities. They're not always something like velocity or acceleration feedback, but a second derivative of jerk or something weird like that. Who knows?

Even worse, even if you have all the measurements, you've gone in there and gotten transfer functions of all the plant and the disturbances, and these change. You go in there tomorrow and turn the thing on, and the frequency of the primary mass or something is moved over three or four Hz; then you all of a sudden have a pole that's not modeled in your MIMO system.

A. SINHA: So if you have a measurement uncertainty, that is a more difficult problem than parametric uncertainty, because what you are feeding back you don't trust. Obviously, you're not going to get too far.

Another thing that I would like to point out is that we should seek a general control strategy. For instance, once we have specified what performance we want, we should try to come up with the controller that will give the baseline performance. Then, if somebody changes the specification, and specifications keep changing, we know that there is a controller that will do the baseline, rather than going through a trial and error procedure for every specification change.

C. KNOSPE: As I remember reading through a paper that was by the people from Wales, you all looked at the aspect of using local feedback, didn't you? What was your experience with that?

R.G. OWEN: Well, on the actual test rig, I actually didn't do the tests on the feasibility study, but we managed to reach the spec okay. Is that your question?

C. KNOSPE: That was what I was asking.

R.G. OWEN: Okay. Actually, I wanted to add something; I agree with what you said about the acceleration feedback when you sort of have high-loop gain and you might excite a structural mode of the platform. Something else that might be a problem is if you've got an accelerometer and it's on the platform and -- going back to these experiments again -- you have a furnace sort of thing. Has anyone considered the effects of temperature on the accelerometer itself? We've done a little bit of modeling on this and we find that the effect of the temperature changes the accelerometer bias. So you either have to contain your accelerometer in a sort of temperature-controlled environment, or perhaps do a compensation or something. I don't know exactly how you would do that, but that's a problem as well that perhaps nobody has mentioned up to now.

J. BLACKBURN: This is another argument in favor of adaptive controls; the temperature and many other transient phenomenon are arguments in favor of adaptive control.

A. SINHA: Well, if we are worried about robustness, then making things adaptive is going to present another set of problems. Robustness of adaptive control, even under the assumption that you know the measurement perfectly, is not well known. You cannot guarantee that you have an adaptive control strategy and that the overall system is going to be stable.

J. BLACKBURN: I'd like to add to that. I've experimented with several different adaptive algorithms, and there are two different ways to look at it in the single or parallel control, parallel classical loops format. I have very little experience in the modern control state space controller, MIMO scheme. But in other words, in the parallel controllers I'm looking at one degree of freedom. I'm controlling, say, motion in one axis. Then there are two things you can do.

Either you can do adaptive control -- that is, the controller inside the closed loop is adapting. Or you can do something else -- you can do what we call adaptive noise cancellation. In this you look at a measurement of what the plant is doing and a measurement of what the disturbances are, and you have to figure out how you get that, whether it be through a microphone in the air or an accelerometer on the ground. With a measurement that is correlated to the motion of the ground, the motion of the ground being the noise, you can isolate exactly what the noise is and inject that into the closed-loop system.

Therefore, the answer to your question is "Yes, you can be guaranteed a stable system." You just have to inject a system into the guts of the closed-loop controller that you know is stable. Any closed-loop system is going to be stable no matter what you inject into it. So there's a way to do that without damaging the stability of the system.

A. SINHA: Okay. I was viewing the adaptive control value -- identifying the barometers.

J. BLACKBURN: Yes. There's been a lot of work done in that area on the state space controllers and I'm not very familiar with it, so I have to profess some ignorance on that.

R. FENN: Yes. We bought a Sundstrand accelerometer and they have an option for temperature compensation. They'll tell you how much the signal changes for a certain number of degrees.

C. KNOSPE: On the thing you were mentioning on the active noise control -- the methodology I think you're addressing is trying to get rid of some sort of wide-band disturbance using this -- sort of like what they're doing in pipes or something where they have a fan and they're trying to get rid of the noise from it.

Generally the problem with schemes like that is you get propagation back from whatever actuator you're injecting into the sensor you're using to feed forward. It ends up being a feedback path if you don't do a cancellation around it. So the activity can drive you unstable even in that case, unless you're doing something that's a completely open-loop control scheme like some of the stuff we're doing at UVA, where there's no way for a feedback path to be closed around your reference input.

J. BLACKBURN: I haven't had that problem though. In other words, I think your are saying that if I were able to read what the disturbance was (say we were looking at a seismic disturbance entering the system), I could take a feed-forward measure of that and run it through an LMS adaptive controller or something, and feed it back in. Then, if the ground were not rigid, what I sent in there would then affect the ground motion and I would get a closed loop. I haven't had that problem.

D. HAVENHILL: I have had that problem; it was a pointing system on the shuttle, where we were sensing acceleration at the base of the pointing mount, then actuating a gimbal to prevent overturning moment due to shuttle disturbances. The flexibility was so bad that in a rigid body (we had about a 20 Hz accelerometer), we had to drop it to less than two Hz in order to keep the system stable.

So when you're on something that's flexible and you know what you're talking about in terms of seismic masses and that kind of thing, it works great. It's fantastic. But if you get on something flexible you got to really watch yourself.

J. SCHOESS: I have a comment on the accelerometer. The Sundstrand characterization is a good rough order of magnitude temperature compensation, but it is by no means accurate. At Honeywell, when we buy Sundstrand accelerometers we do our own full-up calibration, because we cannot trust the Sundstrand calibration coefficients.

As one other comment, I think there's a good trade-off of adaptive control versus compensating the sensor and then doing your control. You may be going too far. You may be designing a Cadillac if you do adaptive control, because with today's technology you can do temperature compensation very easily with regression analysis.

R. FENN: I was curious, was there a very low mechanical resonance in that system? Is there some way you characterize flexibility?

D. HAVENHILL: Yes. The resonance was about five Hz on what we were mounted on. But on some of the station things you're talking about, you're mounted on tenths of Hz and things like that. Locally it's not going to be that soft, but it still can cause you some problem.

G. BROWN: There were a couple of things left on controls that I'd like to hear comments on. A couple of years ago I thought digital versus analog was an issue. I don't really think it's much of an issue anymore. I guess you do analog if you can, and if you have something more complicated, then use the digital systems. Does anybody have any comments on digital versus analog controls or should we drop it at that? (There were none)

What about the non-linear strategies? I've been working on that a little bit for magnetic bearings, and so I got Fenn's cubic control law yesterday. Some people at some points have suggested that you don't exert any forces on the package whatsoever until you're afraid it's going to run into the wall, and maybe you push back with a micro-g or some such level. I guess you might call it just an on-off control, as opposed to PID or whatever kinds of things that we implicitly have been thinking about here. Any comments on non-linear or oddball control strategies, free-floating strategies, whatever?

C. KNOSPE: As far as free-floating strategies, that worked fine. The only question is once again whether you have the umbilical in there or not. If you have the umbilical, you obviously must do something about the vibration that's being transmitted through it. But if you're free floating and you're only worried about basically centering, any of these types of schemes where you just turn the control on when you get too close to a wall will probably work fine. I agree that if you're free floating there's nothing you have to do until you get close to a wall. You don't have to do anything, just let it float.

R. FENN: I think there are some advantages. The cubic control approximates a dead zone, but it has some stability advantages. It's more stable, I think, if you have a dead zone, a bang-bang control where you either turn your actuator on or turn it off. You're going to end up throwing it across. The actuator we're using has a nice damping so that it won't just shoot across the center line.

G. BROWN: Okay. If we have no more comments on that, our next issue is source vibration control, for which we had ten votes. Who wants to kick us off on source vibration?

J. BLACKBURN: I wanted to comment that it really would make sense to do source vibration control; but a step beyond that would be to make it cost-effective for people to institute some sort of on-board isolation for their projects before implementing them in the shuttle environment. In other words, if you have some mechanism in place that, for instance, lets you promise NASA that you won't introduce any more than such and such a forcing function, then NASA would maybe give you a discount on the cost of implementing your experiment. Money talks.

D. HAVENHILL: I guess what I'd like to say on this, as far as source control, is that we ought to encourage source control very soon. The approach that we've looked at here over the past few days is to just let everything come in however you want, and then we're going to take care of it all at the experiment. I think it's going to be a lot cheaper if you try to control things at the source.

Now, when we're talking about source vibration control here, some people will immediately jump to the conclusion that yes, you ought to put an isolator under your mechanism or whatever. There are a lot of things you can do in the design of a mechanism to make them run smoother, like magnetic bearings, for instance, or just very low ripple torque motors: those kinds of things. And currently, experience is that's not happening on this space station, or at all as far as I know. I mean, they're still using the same stepper drives that are causing the 17 Hz problem on the shuttle, and there's just no control that I know of for those vibration sources.

So there are a lot of things that can be done without adding a lot of cost here. A lot of things that can be done right now are fairly inexpensive and will pay off big in the long-run.

C. GRODSINSKY: Just to add to that, the space station is going to a vibro-acoustic plan, which has been a long time coming. I know they're under the gun and they're under budget constraints like everybody else, but they should definitely be looking at these things -- there are people who have been worrying about these kind of things for many years. In designing submarines, they use their vibro-acoustic plans, and they won't put anything in that thing that's noisy. They don't just say, "oh, we'll just put a spring and damper in between that and the outside hull," because then they would just have a big mess on their hands.

But these kind of things are pretty well-known and I don't think they should be as costly as they say they will be. In implementing such a thing, it's going to take some time, and there will be a learning curve for these people. But I believe that there should be a data base available from people who have done this kind of thing and who have worried about these problems.

D. HAVENHILL: Another example is the space telescope. Although it doesn't have as many mechanisms and fans and everything as the space station does, the space telescope went through this experience very early in their design, and did tremendous things to reduce vibration of the reaction wheels, to make sure the antenna pointing system ran very smoothly, and to make sure all of their mechanisms didn't interfere with their pointing. They have a different problem; they want a point, but still those disturbances become pointing disturbances. They didn't take care of the solar arrays, but other than that they really did do a lot of work. So there is some precedent in how to go about approaching the problem.

C. KNOSPE: My comment on the whole issue of source vibration control is that I think it's a great idea to do it. The question I would have about it is if most of your source isolation control -- maybe I should be corrected if I'm wrong -- would be tackling stuff in the relatively high frequency range coming from machinery (50 Hz, 10 Hz, or whatever), while most of the vibration isolation problem is probably somewhere between 0.1 and, say, 5.0 Hz where people were having trouble. If you're above 10 Hz you can get pretty good isolation from just passive mounts. So maybe somebody would like to comment on that as far as source isolation control versus the frequency spectrum, and the offending frequency spectrum that you're isolating against.

D. HAVENHILL: Yes, I agree with that. On those charts that we saw, the machinery was all up high in frequency. However, I still think we ought to take care of them, and it should be done either passively or through a control scheme wherein you're making a smooth actuator, rather than throwing money at an isolator.

C. KNOSPE: I agree that you should do everything you can to get rid of even the high frequency stuff. The only thing that I was trying to bring out, maybe for the record, is that no one should go away thinking that source isolation alone is going to solve our problems, because it isn't. We're going to have to do something else also.

P. ALLAIRE: I have a question. Does anybody know how these treadmills are mounted? Right now are they just rigidly mounted.

J. BLACKBURN: Well, the NASA folks actually made this video and they've seen as much as I have, but basically this stuff looks like they just come down like an angle iron. It looks like about an inch-faced angle iron. It comes down to the base and then it feathers out to two little screw holes and they can either put screws in there or not. I believe the one I saw was bolted down to a panel, and then the panel itself was not rigidly affixed to the ground; so when you're running up and down the thing is a source vibration problem in itself for sure. That problem is unique. It was brought up earlier that what you need to do -- in an active sense, if you want to control that actively, which I think you have to do because the disturbances are below the natural frequency corner of the passive system -- is look at force rather than acceleration, because acceleration will vary with the mass properties of the structure, depending on where you are and so forth.

What you want to do is use some sort of momentum interchange very similar to the actuator that I presented yesterday. What you want is an actuator similar to that, with much larger proof masses and much larger throws located at the legs of the treadmill itself, so that every time you land on it you take a measurement, using a load cell or something rather than an accelerometer to get a measurement of the force. That force is then fed back by an inertial motion of one of the proof masses to keep the thing sitting still, because there's no way to physically attach that treadmill to the base. The treadmill problem is a whole can of worms by itself.

But if you affix that treadmill to the base of the shuttle and attempt to actuate it using Lorentz force actuators that act between the base and the treadmill itself, then the only way that you can keep the forces from transmitting through the treadmill to the base is to have zero force between the runner and the treadmill. In other words, if every time he hits it gets real soft and he goes no place. So you need to have it provide a force to his foot, while at the same time isolating it from the base; there has to be a momentum interchange. That's sort of a unique problem. I agree with what you said before though that the excitations above the passive corner are the way to go. Just using passive isolation to get rid of stuff like rotating machinery in the 30 to 100 Hz range is probably the way to go.

G. MCCANLESS: Let me say a few words about how we got to where we are now. The fact that we can have a workshop like this with a hundred people is just a great sign of progress. A couple of years ago we couldn't have done this. The way NASA got into this business began back in the Apollo days; we saw pictures of astronauts floating around and doing somersaults, and we called that zero G, and somebody says, hey, let's go and do some material things.

It's a different environment here. Number one, the big focus was that we don't have convection and convection, we think, is messing up some crystal growth processes and some other good things. And so we were very content with zero G. Then we noticed that things were going to the wall here and there and so it really wasn't quite zero G. And there were some studies done, I think it was by a gentleman named Bob Brown at MIT who said, "Hey, you need 10^{-6} g, and we don't have 10^{-6}g." But all this was viewed somewhat skeptically in that you just couldn't imagine that there was much convection going on when you saw movies of astronauts turning somersaults and so forth. Now other people have conducted studies, and results presented in the other room yesterday indicated that we do need really quite low levels of gravitation or acceleration, and this is just kind of sinking in. The gentleman from Honeywell says he's had an isolator for five years or more and he can't sell it. This FEA is the only thing that I know of that anybody really tried to isolate.

But slowly the message is getting across. I think we are going take some steps and try to hold down the disturbance levels and an effort is going to be made. The problem is that it's going to be costly, and the payload arena doesn't have much money relative to other things. The shuttle budget is something like six billion a year and the gentleman from NASA headquarters -- I may have said too much. I might be in trouble. (Laughter).

S. GLAZER: No. I was just going to mention that in terms of cost -- How expensive is it to build a $50 million experiment and not get any good data out of it? Isn't it better to do it right the first time?

G. MCCANLESS: I'm sold, but it's really difficult to reach the people at NASA headquarters, especially those who can really allocate a half a billion here or something. It's a tough proposition.

S. GLAZER: I'd like to just comment that it seems like it's microgravity science and applications kind of applications, MSAD, which is code SN, which has a lot of problems. We recognize we need a quiet environment, but it seems like many of the disturbances come from equipment or operations controlled by other codes, if you will. I don't mean to suggest that NASA fights itself. I think we are trying and I think we're starting to succeed in convincing the other parts of NASA that our problem is an agency-wide problem. SB, which is life sciences, is part of code S as is SN, and we're starting to get the message to them now.

And of course, in the past we have had political problems of having to deliver hardware on time. We've also had problems of not really recognizing that we did need vibration control or isolation in some cases, and I think we're going to pay some penalties. But I think once we have paid some of those penalties, the impetus will be very strong to try to do something about it in the future.

C. GRODSINSKY: May I add to that it seems to me (as Stu was saying) that we're going in the right direction and I think a fundamental decision has to be made. I mean that there have been a lot of microgravity science experiments flown to date without vibration isolation, and while I probably don't read the right journals or anything, I'm sure that the people who have flown and published papers have gotten something useful. Otherwise they would just simply refuse to keep flying unless they get some isolation; and I have not seen this.

Now on the experimenters side, they also have a budget, and they spend every last penny to get the best science they can possibly get, and they either close their eyes to isolation or it doesn't bother them. If you look at the requirements curves, it's should bother them, but, again, we don't see anybody demanding isolation.

Now, another possibility is that maybe they don't understand their science well enough to even know that having no isolation bothers them. If that's the case, where do these curves come from? I feel that sooner or later we're just going to have to make a decision to fly something with isolation, and just answer the question one way or the other.

G. MCCANLESS: Very little has come out of all of this experimentation.

C. GRODSINSKY: Okay. If that is the case, then who makes that decision to fly an experiment with a system that may solve these problems? Again, we're funding science and none of the science experiments that I'm associated with have any type of isolation, even though some have isolation needs. Now, that means we're going to wait, get the data again and again say, "Oh my God, we'll wait until we fly the next time." And it's not easy to fly; you don't just turn around and fly again the next year.

C. KNOSPE: You said that very little had come out of these experiments; I'd like you to elaborate on that.

G. MCCANLESS: I'm going to give you my personal views, and I'm not speaking for headquarters or the code that funds me or anything. The protein crystal growth, I believe, has the greatest potential. I believe that most of what we have accomplished in space has been the information products like communication satellites, weather satellites, and the spy satellites the military used very effectively in the recent war.

Protein crystal growth is an information product. What the protein crystal growers do here on the ground is that they grow proteins. It's not the kind of proteins you eat, but there are 10,000 proteins that have been identified in the human body. I never got passed frosh chemistry, and to me a molecule ought to be something like H_20 or O_2. Well, these things have 600 atoms in them, like you see in Scientific American, there are all these things that sprawl around. They can grow crystals of these things that look like little pieces of glass or something, and they x-ray them. From these x-rays, they can back out the crystal structure. The drug industry is very active in this field. It appears that we can grow bigger, better protein crystals out in space, in the microgravity environment. There are some proteins that we haven't been able to grow in space. If you can grow a real good protein crystal on the ground and get it x-rayed, you can then throw the crystal away. Once you get the x-ray, you've got the structure.

But there have been some successes in space. There's a mercuric iodide crystal that has been grown successfully in space that was really a winner. When they grow this mercuric iodide on earth, the weight of the crystal itself tends to destroy. It's a very fragile crystal in the growing process. But aside from that, there's been surprisingly little that's come out of this whole arena.

J. SCHOESS: I'd like to comment on that. I've been involved with two material processing experiments and I would back that comment up. It seems like it's hard on get on board, but it's ten times harder to learn anything from it. The energy it take to do the analysis thereafter, seems to fall in the crack and people move on. I don't know why, but that tends to be the case. People don't want to dig.

C. GRODSINSKY: I guess the next on the list is sensor technology, and Jeff Schoess suggested that this might actually be characterization of environments. He also brought up the point that you have these sensors doing characterization of the environment, and that some people might like to use them in a control scheme to control hardware or a rack and something. That's an idea. So I guess, I'll throw that out for discussion? And I guess I'll throw something else out; I've been using QA 2000s in the lab for quite a while, and someone from ATA asked why I don't use angular rate sensors. At the time they just weren't really available, and it would have been a paperwork nightmare to get one. But now that PCB has them, we'll see.

But anyway, there's technology now coming out with micromachined silicon proof mass accelerometers that looks really promising. These things are digital output sensors, an inch square -- and in fact, I've talked to some people who make these things for the Navy, and have been making them for a quite a long time. They only recently are able to sell them to the public, I guess because the Navy probably has something a lot better. But anyway, the specs on them look just like the QA 2000s.

J. BLACKBURN: I hope there's nobody here from B & K Technology, but they send out on their spec sheet that their sensors go all the way down to DC and that they have some resolution. And there are a lot of people who read these kind of specs and just take it for granted that this is what this sensor will do. But ATA, my company, has been trying to beat it into people's brains for a long time that this cannot be taken for granted, and that the noise floors on sensors destroy the measurements. I think that all you who have dealt in the active controls arena are aware that the isolation of low frequencies in the inertial sense is limited by sensor technology. That's really the bottom line.

Actuators are extremely quiet; even a bad actuator is quiet, and sensors are really the problem. Yet throughout the discussions yesterday you saw most of the arguments put forth by the materials processing experts present here. One of their statements was that the disturbances that really bite you are the wide, large-travel. low-frequency type disturbances. Those are the ones that we're having trouble isolating and the limitation is the sensor technology. So what do you do about that? In that sense, I think that you have to address the sensor technology from the control standpoint. But if you can't measure it, then you can't control it, and the converse is also true. How can you characterize the environment at low frequency if you don't have the sensor to control it? So they're one and the same problem really.

G. BROWN: Can I ask a naive question? At low frequencies, why can't you just use displacement measurements?

J. BLACKBURN: That's not an inertial measurement. Let me explain what the difference is again. I gave this example yesterday. Let's say you have a single-mass system supported by a simple spring and a simple dashpot. Between the base or the support structure and that mass you place an LVDT, which is a non-inertial measurement device. If you tie a control loop around that, then when either the base or the mass moves (but you can't tell the difference which moved) it will track the mass -- this is the bottom line. So that's not an inertial measurement if you're trying to isolate it from base motion disturbances. On that, if you don't have an umbilical it will work just fine, just make the spring soft enough. If you have an umbilical, you've got to make something inertial.

D. HAVENHILL: That brings up an interesting point, too. You're right if the only thing you want to get rid of is base motion. What we haven't talked about, and this kind of falls in the sensor technology line, is what happens if the experiment creates its own vibration sources. Are we going to want to cancel those out as an isolation platform? If we have to do that, then we have to put some kind of inertial sensor up there.

R. FENN: I think that would be a good selling point if an experimenter couldn't make a quiet apparatus. We'd make it quiet for them if we had that technology to create some mechanism or something.

C. GRODSINSKY: I agree. No matter what else you do, you want to inertially reference your payload.

J. BLACKBURN: Yes. You need a DC measurement, you need active stabilization in an inertial sense or relative sense, and you do need some datum point. The thing can float off from here to Tokyo before we know what happens to it and it's still inertially stabilized. So obviously you got to lock it down somehow.

It should be pointed out that relative sensors, unlike inertial sensors, don't have the low frequency problem. They have very low noise floors at low frequencies; at least many of them do. The technology is better than it is for inertial sensors. It's surprising to me that there isn't more available out there for the measurement of low frequency motions, both in rotation and translation. Bell has made a killing on some of their sensors strictly because they can go down to DC, and they're basically just an accelerometer. It's an example of using some different form of technology to get at those low frequencies, and I think there's a lot of work that somebody needs to do to come up with something as effective but that isn't so expensive. Maybe they could perfect the one they have, such that it isn't as expensive.

D. HAVENHILL: As far as whether or not it's absolutely necessary to have inertial feedback, I sort of disagree with it. If you have a payload that is not inducing it's own vibration, and the only thing you're worried about is base vibration, and you don't have the umbilical like Paul mentioned, I just don't see why you want to spend the money and all the hassle that's associated with using inertial feedback. The only thing you're going to gain by that is trying to get some low frequency performance out of it, and that's where the inertial sensor doesn't do very well anyhow.

So I think you end up spending a lot of money to get that, while I think just by building a very soft spring with an active control you get as good a performance as you're going to need. We have to remember that when you go to buy an accelerometer package you might be spending $100,000. That's a lot of money to be putting into such a system, when you can buy a position sensor for $5,000 or something like that.

G. BROWN: So, for the really low frequency stuff you're mainly interested in just keeping the experiment from running into the wall, isn't that correct? Would it make sense to have one big accelerometer attached to some major structure in the experiment area that anybody else can reference. And they can add that in to get an absolute inertial reference for their experiment. You wouldn't necessarily need one on every experiment, would you?

J. BLACKBURN: I might point out that the shuttle has a very, very expensive accurate inertial measurement unit that has just about everything coming out of it that you can imagine. As I understand it, access to those signals is available to experimenters. You can use that to back out from your relative position on the shuttle some of the motion, to feed it forward or whatever to eliminate some of the seismic motion.

However, I disagree that the only thing you want to do with low frequency motion is to keep the experiment from hitting the wall. A lot of specs are based on the RMS of the motion spectrum, and if that's the case then you have to actually bring the level of the spectrum down in a low frequency range.

G. BROWN: Yes, I guess that's right. You're really trying to reference your payload, your experiment, to the inertial space, so you're using it for more than just to avoid the wall.

J. BLACKBURN: And also, a very soft spring will work, but again, like Carl said, that's entirely dependent on whether you have a tether attached to it. I don't know what everyone else got out of our discussion of umbilicals, but it appears to me that we do have to somehow accommodate a limited tether system. Somehow in there you're going to have to be able to accommodate a tether in some experiments. Sooner or later you're going to have to address that problem, I think.

C. GRODSINSKY: I'd like to agree with you wholeheartedly that in a specific case you do the simplest fix. I mean, why spend the money and the aggravation in doing something you don't need to do. But that, again, is for this specific case. If we look at the general case and the specs that we are given, we have to go at this problem by trying to inertially reference the mass, and we have this umbilical, etc. If it is such a simple problem for many of these science experiments, and if they are having problems, and if the fix is so simple, why didn't they do it? This assumes a simple spring mass system, with enough damping that it didn't just oscillate forever or something.

And I'd like to just ask one on the protein crystal growth. Well, I know that on these R/IMs they have something like 40 of these proteins, so maybe they're working on the logic that even if they only get five crystals, there are 40 and they always are flying. So, why spend the money on isolation when, as you said, you get a picture of what the crystal structure is and then you throw everything else away?

G. MCCANLESS: They're growing 40 different proteins. They're using different proteins. Is that your question? Oh, yes, and those guys will never run out. As I say, there are 10,000 human proteins and by the time they would use those up they'll probably have another 10,000.

S. GLAZER: I believe that they have 60 drops in a R/IM; but of those 60 I believe that several are identical. Out of, say, half a dozen of the same protein they may be lucky to get one that grows a viable protein crystal.

But even so, I'm not sure if anyone really knows whether the acoustic environment affects the degree of symmetry within that one crystal that does grow. So I'm not at all convinced that providing an isolation system for that R/IM wouldn't, in fact, improve the quality of the crystals that do grow.

C. KNOSPE: Getting back to the sensor question, I'm not much of an expert on that, but it seems to me that the ultimate question of whether we need an inertial sensor basically boils down to whether we have an umbilical and whether we have any direct disturbances. And we've talked about the umbilical question . Does anybody want to address the question about direct disturbances?

G. BROWN: Let me clarify that. What is obvious, I guess, is that if you don't have direct disturbance, you want a real soft spring. If you have direct disturbance, you need a real hard spring to keep the payload from moving. You need to push against something to keep the off-center mass from jiggling. Those seem to be contradictory, and I would think a controls guy would be the first person I'd ask.

C. KNOSPE: Are we going to have direct disturbances?

R.G. OWEN: The actual control system design doesn't do anything for direct disturbances. If there is any disturbance, the platform won't do anything about that. But there are quite a few experiments with disturbances, such as fluid science experiments. You have video cameras and different kind of things moving on racks, so there's definitely a possibility of having a disturbance on a platform.

We haven't really looked at it all. So you really have to look at something a little more complex, or about some control system to do that. Perhaps that's something we would like to look at. I think there will be something coming out from ESTEC soon, a new contract to actually look at the disturbances that these payloads on a platform might actually impart to a platform. Perhaps some people are starting to look at that.

J. SCHOESS: I just wanted to make a comment about the inertial measurement as an input. In this last flight we were looking at the HiRAP data, which is inertial quality data, considerably away from the mid-deck, and they are two different worlds in terms of what they provide. The inertial is good data, but it's not the level that you need for materials processing. They're just totally different. Unfortunately, I couldn't show you some of the disturbances due to the thrust or firings and things, which are quite complex.

The other comment I wanted to make was that I do think that you can have a reference accelerometer in the area of materials processing experiment or experiments, depending on where it's positioned. But I don't think you necessarily need to have individual ones, unless it's a requirements-driven problem. But you might be able to get by if they are common kinds of experiments.

C. GRODSINSKY: If the environment were characterized well enough that you could tell the scientist exactly what he had for g-jitter during the growth of his crystal, he ought to have enough information to tell him exactly what happened: in a dynamic sense, how he's exciting his system. But he still gets the low residual G. Then they could answer the questions to see if they need isolation.

R. FENN: This goes back a few minutes, but I recall that we did some analysis on an umbilical for the one degree of freedom test bed that we built. We had no contact except for the accelerometer power and signals and we used, I think, 32 gauge wire or something of that order. The five wires together gave us less than one micro-g as we analyzed the bending stresses. It might be possible to carefully design to have very low forces and low spring constants.

G. BROWN: Getting back also to the umbilicals and the MGIM, I was wondering if you do have heat transfer problems, and if you can't meet them with the heat transfer method that you had. Was that primarily radiation or was there some appreciable conduction between plates through the air: do you know?

R.G. OWEN: As far as I know it was just thermal radiation. That was actually a question that came up yesterday, but I'm not a thermal expert.

G. BROWN: Right. I wondered whether mass transfer might be feasible. It always beats any other method of heat transfer easily, but we seemed to throw it out immediately here, thinking it's going to disturb the payload. Is it conceivable that some kind of very gentle means, maybe transpiration emission of air from the stationary plates, could provide some mass flow?

R.G. OWEN: Perhaps the best way to answer that question is to tell you the experience we had in actually doing the isolation tests on this feasibility study back a few years ago. We found that in order to go down to .01 Hz, we have to eliminate everything. We had to build a chamber which, in fact, turned out to be a garden shed which we bought, and we enclosed the whole test in this shed. We found that we had to carry out the test very late at night, because we had the mechanical workshop about 12 feet away and we didn't want people walking in the corridors and everything. So we really had to work to eliminate every vibration source, to have a very quiet building and also to eliminate all air currents, anything at very low frequencies.

G. BROWN: I can see a problem with the air currents that are naturally turbulent in a room, but I would think you might be able to design a very gentle mass flow system that would not perhaps introduce much disturbance.

R.G. OWEN: Okay, you may have a good point there and actually, that sort of reminds me about something else. In our study, this double rack study I was talking about earlier on, there was a requirement with using spacelab racks that you have to have a certain amount of airflow for safety reasons, for fire precautions, right. So that's something that might have some effect on this microgravity isolation mount. In other words, we'd have to have some enclosure, but we wouldn't have all the sides completely open. Some of the sides would have to be closed, obviously, but there's got to be some airflow there just to satisfy the fire precautions. That's a point that was raised by somebody at ESTEC.

D. HAVENHILL: Moving to cost-effectiveness, I guess I've said that the people who are doing science want to do science and they want to spend their money on science; they don't want to spend their money on isolation. We ought to look at our isolation systems as a tool to give them isolation at the lowest possible costs that we can. And we shouldn't go overboard here in terms of making the thing overly complex just because isolation's fun to do and there are some neat controls to do, and the other kind of things that motivate us. We really have to keep in mind that we're trying to provide a facility here at a very low cost, or at as low a cost as we possibly can.

R. G. OWEN: Yes, I'd just like to add the point that what we're trying to do is just to build a basic facility. We're not trying to do anything really complicated at all as far as control schemes and so on are concerned; we're just trying to offer a facility. And when people see this facility, perhaps they can make use of it, although perhaps they do have some really extreme requirements, like vacuum venting and so on. But they'll be able to see that if they want to investigate the effects that reduced or improved isolation has on their experiment, then they'll have to tailor their experiment to fit our facility. Once somebody does this, and we have a few flights that produce some results. Perhaps that will encourage other people to do the same thing.

So basically, we perhaps want one or two experiments that can be used as control experiments, just to see if there is any effect or any improvement to be gained by putting your experiment on the microgravity isolation mount. A small-scale experiment like a fluid science experiment would probably be the simplest and the most appropriate experiment. Until somebody does that, the interest among the experimenters may be very limited.

C. GRODSINSKY: To add to that, we've been trying for a long time to get some experimenter to commit to doing something like that, and we're still working on it. If anyone can get together with an experimenter to fly some science and to answer these questions on how isolation works with the science, then the rest will follow. To date, I haven't seen that, but hopefully that will happen.

R. G. OWEN: Yes. Just an example I can give you, the gentleman from MBB is not here anymore, but about a year ago we had an inquiry from a group within MBB interested in putting a fluid science experiment on our microgravity isolation mount. As I said, that's sort of an ideal kind of experiment. So there is definite interest within Europe in doing that.

Recently at ESTEC they had a meeting concerning the Columbus precursor missions that are coming up to try to solicit experiments from people, to see what people's interests were, and what facilities they wanted. The microgravity isolation mount has been put forward as a facility that is available, so if anybody among the experimenters think that they can make use of it, then the thing might take off. The actual development might go further from that sort of beginning. I do know one experimenter in particular who has a definite interest or would like to be accommodated on a microgravity isolation mount.

R. FENN: Do you have any – I gather you don't have any particular experiments lined up at the moment and are you planning to fly your equipment?

R. G. OWEN: Well, I'm not too sure. As far as ESTEC is concerned, I don't know all the details of what's happening. As I stated, we're stuck out at Bangor, so I hear things secondhand. But I think the general idea is that if there are a lot of people with an interest in putting experiments on the mount, then I think it's the people involved in the Columbus group within ESTEC, who are actually prepared to finance this. I think the money is there, if there's sufficient experimenter interest. But as I said, I do know of one or two specific experimenters with interest, and one of them may actually develop.

R. FENN: So you're actually talking with two possible experimenters in Europe about potentially using your equipment?

R. G. OWEN: Yes.

R. FENN: Is this in a year or two, or something along that line?

R. G. OWEN: Yes.

G. MCCANLESS: Cost is going to be a real factor in what we do, and so far this afternoon really nobody's said anything about passive systems. I think it's the nature of the group; you people are all from the control business or Honeywell or something. and we don't have anybody here from the bubble gum industry or the Silly Putty industry or something.

On the space station, you know, if we'd look at the U.S. lab, it looks like a big drum. If somebody would stand outside and beat on it, it would just reverberate. It's just a terrible thing. And then as to the international standard rack, you couldn't ask for a better vibration transmitter. It appears to me that just a simple kind of thing could be installed on the feet of these racks: some sort of rubber coupling device or something. You might not launch this, but there ought to be some way that we could remove things and put in some rubber isolators here and there. While this wouldn't take care of the really low frequencies, it would be very cheap to do.

My wife and I buy a lot of mail order stuff, and when the package comes, it's wrapped in these little plastic bubble things. If you went to the guy who wrapped this package and ask him for a Fourier analysis and all this kind of stuff, he wouldn't know what you're talking about. But experience shows that when those packages get dropped or banged around, that packaging damps out a lot of the unwanted disturbance that would break whatever is in the package. So, I really think that we will first turn to some sort of passive spongy sort of viscoelastic, to use a fancy word, and use some sort of passive steps to cheaply work on our payloads and isolate some of the disturbances.

J. BLACKBURN: There's a company called Fabrica International in Boston, and their whole function in life is to provide various types of passive isolation. They sell just exactly what you're talking about, a sort of viscoelastic material padding of various thicknesses and damping properties. And you can basically tell them what damping ratio you want, and they can sell you a yard of this stuff. That's what it's for, and they sell it to people in the heavy industry to put under machinery and so forth. The trouble with passive isolation, as has been mentioned throughout the conference, is that it's only good for reducing vibrations that are above the natural vibration frequency of the system. In other words, let's say you have one of these isolators that you're talking about, a rubber grommet or something. If I take the mass and pull it down and let go of it, it's going to sit there and oscillate at its natural frequency. Any frequencies above that it will take out, but those below it won't. But in the materials processing problems, those low frequencies (at least from what little information we got from those people yesterday) are what they said are biting them.

G. MCCANLESS: Right. I second that.

S. GLAZER: It's been my experience that one of the reasons why we've had a lot of cost problems on a lot of the hardware we built is that we haven't gotten scientists involved early enough in the hardware design. We've come to recognize that, due to schedule pressures, we've had to start designing hardware before we got scientists involved. It really is necessary to get them involved at an early opportunity to be cost-effective with this type of microgravity isolation equipment.

But I'd also like to ask for some opinions on one other thing in addition to cost-effectiveness, I'd like to hear any comments about the whole subject of flyability or being able to qualify hardware. In addition to being able to afford it, we have to be able to build it, qualify it, and test it. And it has to be rugged, reliable, and with low power use.

We're fighting a tremendous power problem on the space station right now. The last I heard, was that something like 13 kWs was available to the entire U.S. Lab for some period of time. One could imagine a number of independent isolation systems that, if they all required to kick at one instant, could cause a power surge that could do some very nasty things to the station.

G. BROWN: Which they're likely to do, right?

S. GLAZER: Yes.

D. HAVENHILL: I'd like to expand a little bit on the passive approach. I was one of the votes that wanted to talk about that a little bit. We also do passive isolation, and we actually have an isolator flying on the space telescope reaction wheel.

The comment I wanted to get in is that you can make the passive isolator. The thing that typically limits you, in terms of frequency response, is how long the spring gets, because the softer it gets the longer it gets. So you start taking up huge amounts of volume when you try to do it passively. So I think there needs to be some combination of active and passive. Maybe passive can take care of some of the higher frequency sources and active can take care of other things. I think that mounting those racks on rubber bumpers or any type of passive isolator is a great idea. We ought to be recommending things like that.

C. GRODSINSKY: I agree with you. Also there are a number of simple things they can do on space station with all these astronauts and their kickoff loads and pulloff loads. They're asked to do a number of things for scientists: to look at samples and check readings, and this is all to help out in the science. But when they do that, they have to hold on to something, because they're not going to look at a gauge or at anything if they can't attach themselves. They need to be aware of simple things like making sure that what they grab on to is not hard-mounted directly to the experiment. And they have to somehow minimize low frequency disturbances caused as they drift off, come back, and drift off.

We need to do simple things, like not putting a handle on the rack. Maybe part of the problem is that everyone works on a different thing and they don't worry about the simple things, and they don't talk to the next guy. Hopefully this group that's been formed at Reston now is going to filter these things and everybody's going to worry about these basic things.

P. ALLAIRE: I have two comments. The first comment is about the power. The kinds of devices we're talking about are rather low power devices, and I really don't think that that's a major issue. No one's talking about consuming kWs here; we're talking about light bulb type numbers. So I don't think that that's a big issue.

The second topic is the cost effectiveness. I think you have to balance the cost of solving a single problem that you know about versus the potential cost of not solving one that could really hit you badly. We're all basing a lot of what we say on what we know about the space shuttle, but we all recognize also that there are future space platforms and structures and whatnot that are going to have frequencies that are a lot lower than the ones we now have. If we just assume that the real low frequencies power and long-stroke problems are not there, and then later they bite us, it could be extremely expensive.

I think that we ought to look at this from the strategic point of view. If you think most of your problems are going to be fairly short strokes and some sort of standard actuator systems that we have now will work for a lot of problems; that's good for now. But there also ought to be somebody looking at the next stage. If we really do have a major problem at very low frequencies that requires long strokes, then someone ought to be looking at that problem as well. Maybe you don't put a lot of money into that now, but if you know nothing about it and all of a sudden it becomes a big problem, ignoring it is probably not the best approach.

C. GRODSINSKY: I'd like to comment that some people, like the Europeans with Eureca, have looked into that. There's just no way to get a good environment for 40 days or whatever it is, to let them do science on the space station. And no matter what we do, I think that's going to be a problem that will not be solved. As to power, I agree with you that we're imparting small forces, and so that's not going to be a problem. But if there are only 13 kWs, are you going to fly a half of a furnace or what? One?

S. GLAZER: There will be small furnaces, and no one else can be doing anything while the furnace is running.

G. BROWN: I had a question on that furnace. Are people just thinking of buying an off-the-shelf furnace? If you make the walls twice as thick, you use half as much power, don't you? At least in a steady-state phase -- and less during the transient.

G. MCCANLESS: These furnaces are custom-built and cost tremendous quantities of money. You can't simply solve the problem with insulation. You take a long bar of stuff and you heat it, and you have to have a certain gradient. This means you've have to be sucking heat out of the bottom of the thing to preserve this gradient; you can't just simply insulate it. There are lots of processes and they're all different.

Getting back to this power issue, there are 24 kWs that go into the U.S. Lab, and then there are a whole bunch of so-called systems like the heating and air conditioning system and all this stuff to operate the shuttle, and we really don't know how much that's going to sop up. The users get what's left over. So, while we had initially hoped to run some 15 kW furnaces, that is just out of the question.

G. BROWN: What are the vibration sources below one Hz? Is it anything other than astronaut motion?

C. GRODSINSKY: The space station structure is going to reverberate at its natural frequencies, especially going in and out of the sun.

G. BROWN: It's lowest mode is what?

G. MCCANLESS: It's 0.18.

G. BROWN: Okay. As to the passive control method, when you get up there, can't you just stick a graphite rod off to the side of the station, put a bungee cord around it and get a nice passive damping of the first mode? Can you do that sort of crude thing? Cheap?

G. MCCANLESS: I don't know, but if you can come in and do some of these things, more power to you. But that is a real flimsy kind of light thing in contrast to the shuttle, which is heavy, has engines and landing gear and things like that, and is all sort of small and compact.

G. BROWN: If you don't have much stiffness, you don't need much damping to get a certain fraction of critical. Right? Zeta's related to K/C or something, isn't it, or C/K, I guess?

A. SINHA: C/2.

J. BLACKBURN: A lot of work has been done in the passive isolation area: the damping of large space structures. It's a big area, which gives you some scope of the problem; it's a very large, complicated problem. Generally, in the process of the design of these things, they build up these huge finite element models that look like trusses and so forth, and then they do animations to see what the mode shapes look like at various modes. And the motions are quite large. Besides, with a bungee cord attached to itself, you're still self-contained within the structure, so that wouldn't help you.

They're now looking at some active applications of momentum exchange actuators and so forth to solve the problem. The trick is to make it stiffer, because the stiffer it is, the higher the modes are. But with these vast trusses that are some 60 feet long, that is difficult to do.

C. KNOSPE: When you say the motions are quite large, how large do you mean and at what frequencies?

J. BLACKBURN: They put strain gauges all over similar test structures on Earth and at one point saw deflections of a couple of millimeters. In some places, then, if you have a structure that's 60 feet long at that same oscillation, you may be looking at several inches of oscillation. What's worse is that in space there's nothing to damp it out, so once the forcing function gets this thing going, there's nothing to stop it. And it can build upon itself.

G. BROWN: That's why a long viscoelastic element like a bungee cord, with a stiff stick between it and the station, would damp a lot, wouldn't it?

J. BLACKBURN: Well, it would probably be wiser to design the structure more stiffly. Truss design is another large discipline which people are involved in.

G. BROWN: Well, I would assume that they've already got the highest specific stiffness that they know how to make.

J. BLACKBURN: You have a lot of faith in their design. I'm not sure if that's true or not.

N. GROOM: What you've touched on is another area where work is being done. That's the trouble with a lot of our space structures, and methods are being investigated for providing damping of our structures. Maybe, the two will overlap.

There's a large control structures interaction effort that's trying to come up with a way to design structures and controls. This would be an integrated approach, which would mean that in some instances you might need stiffer structures. The vibration isolation area might provide a driver for some of that work, and might provide justification for adding active damping systems to space station.

R. G. OWEN: Just going back, I believe you asked what disturbances are below 0.1. I have a couple of graphs that I published in a paper taken from an MBB report. They did a vibration analysis on the predicted vibration disturbances on the test laboratory and the free-flying laboratory. Looking at the one for the free-flying laboratory, we have curves here below 0.1 for gyroscopes and some reaction wheels. These go from .01 and break frequency about .1. Then they go up. I don't know if that's an answer to your question.

G. BROWN: Are those really cyclic disturbances or are they just the result of analyzing a very slow transient into some Fourier series?

J. BLACKBURN: The gravity gradients and air drag are cyclic, are they not?

G. BROWN: Yes, but we know we can't isolate against them.

J. BLACKBURN: Only because you don't have the ability to sense or actuate it. That's why you want to press that technology forward. If you could, you might be able to actuate those throws; isn't that true?

C. GRODSINSKY: You'll never have the volume. As to whether you could or you couldn't, actually there's nothing to say you can't.

G. BROWN: How can you isolate against air drag?

J. BLACKBURN: Well, you don't. You don't isolate against the drag itself, but if it causes some oscillation you might. In other words, if there is some severity at T and a different severity at T1, then there's some motion that you could compensate out.

G. BROWN: Right -- the average.

J. BLACKBURN: But then you asked about the sources below one Hz. I believe the ones they usually cite are the gravitational gradients and air viscous drag, right?

C. GRODSINSKY: There will also be a lot of structural modes. They keep redesigning the structure. The first one, hopefully, will center all the mass in the middle, except for the solar arrays. So most of those modes are the trusses out here and the node is at the space lab. Now, when you get into some of the higher modes, you have the space lab at the biggest excursion of motion. I don't know how that comes about, but I can't see that it's going to be a big motion. The biggest motions you have are these big wings flapping and, hopefully, you don't have an experiment out there.

G. MCCANLESS: Let me comment that the space station got to be the way it is because of the limitations of the shuttle. The shuttle can only accommodate roughly 40,000 pounds, and there's a problem in that the CG has to be to the rear of the shuttle cargo bay. We're talking about a space station that weighs a bit less than a million pounds so, when you do the arithmetic, it takes 28 launches to get it up there and put together. It all seems kind of improbable, but the reason it has gotten into the shape it is in is due to payload limitations of the shuttle.

C. KNOSPE: I want to comment on the whole question of the space station versus the microgravity science experiments that we're supposed to be talking about isolating. The most cost-effective way to isolate these microgravity experiments is to first tackle the sources. If that's not good enough, then isolate the actual payload you're interested in. The least cost-effective way (and one we should never get ourselves thinking that we're going to rely upon in order to get our microgravity environment) is doing something to suppress the whole truss structure's vibration. That's a very, very difficult problem. The best minds in the country are working on it now, and as far as I can tell, they're not getting very far. It's also overkill. We just have this one little package somewhere in our space station, and we should just be worrying about just getting the vibration down on that right now. That's the most direct, straightforward way to achieve our goals, and it would be the most cost-effective too.

G. BROWN: We have been quite a while now on this cost-effectiveness and related things. Maybe we can very quickly dispose of the last couple of items we selected. Some people voted on whether the specs and the disturbances were known, and realistic, and appropriate. Have we a comment or two on either one of those, on that topic?

How about the active versus passive versus hybrid? I think we've touched on that a lot in other areas. Maybe we don't need any more on that. Anyone disagree?

Okay. And then there's the center-of-mass control, that's a cure for one of the biggest sources of disturbances below 0.1 Hz. Well, one cause for needing for large throw actuators is if the station's center of mass moves.

D. HAVENHILL: I have a question on aero drag. Has anybody looked at perhaps installing a little ion engine or something like that to compensate the aero drags for the space station? That's a question for NASA in general. I don't know if that's been done.

G. BROWN: It sounds like a good idea to me.

G. MCCANLESS: Yes, it's an obvious thing to do, and it's not being done. Part of the reason that we go into this reboost thing is that the shuttle can just barely get up on these resupply missions, so the station has to decay into a lower orbit for the shuttle to get to it. That's the only explanation I have heard. But, it's just an obvious thing. The drag is one-half a pound to a pound, or something like that, and we could just exactly cancel out the drag.

The other approach that is obvious to do is to take the truss structure and bend it down to put the center of gravity within the U.S. lab or the Columbus lab or down where we want to do the experimentation. As you get further from the CG, you pick up a gravity gradient problem. That's being talked about, but no action is being taken.

G. BROWN: Okay. Then maybe we can go back rather briefly and hit each one of these items and just see if the person that's reporting tomorrow has what he needs.

(The balance of the meeting was review of the prior discussion to ensure that the item reporters had enough notes to prepare a summary presentation.)

SCIENCE REQUIREMENTS AND THE ENVIRONMENT DEFINITION

Moderator: N. Ramachandran
NASA Marshall Space Center

STEVE DEL BASSO: Good afternoon. I work with SSEIC in supporting the level two NASA office in Reston. Perhaps a good way to start off is looking at the requirements in the space station program right now and where they are headed. Sometimes where they are headed does not have the full input from the user community. I think this would be a good forum to see where we should go with these requirements and hopefully to generate some discussion.

In this first chart (fig. 1) I am basically dealing with the dynamic requirement, and not talking about the quasi-steady or the duration aspects. What I have just outlined here is the basic requirement, where it is, in what document, and that PDRD Rev J stands for the space station program document 30,000. The current requirement I jotted down is the standard g versus Hz curve, which in my understanding is originated for single monochromatic steady state sources, and this is the way it appears. At this point, I would just like to pay attention to the lower curve, which is the user sensitivity lower limit. So this Revision J is currently out there. However, as we know from discussions, it has not filtered down to the work package level and down to the equipment builders at this point.

Within the next week, the governing requirements document is going to come out with a Revision K, and it basically reflects the restructuring of the program. It addresses those restructuring concepts, the ATC versus the PMC, as Phil mentioned, etc. What's in there still is going to be this one curve, g versus Hz, and in process right now is a change request that Phil and Kevin Schaeffer have been working on. In that change request, the impetus, in effect, was to clarify the requirement, and to not introduce any new requirements or substantially change them.

One of the items that was done was taking this original curve for single disturbance sources and an amplitude curve and deriving, as Phil showed earlier, a PSD equivalent. Whereas one could have the narrow band curve as shown here originally, if this were the only input into the requirement, one could put an infinite amount of frequencies in here and an infinite amount of sinusoids. But what was done, in moving to the PSD version, is assuming that you have only a certain number of simultaneously acting sinusoids, these being at the center frequencies of third octave bands. There are some other criteria also, so the third octave bands are basically nine to ten center frequencies, or you would be allowing nine to ten sinusoids in each decade.

Going from the conversion basically is saying that the narrow band requirements at one Hz allow you 10 micro-g's; that's an amplitude, and the RMS level for that is 7.07. But let's have the same area in the PSD in the third octave band, and the area under there reflects the same RMS level. So that's how the PSD curve was generated. That's the format in the CR right now, and this is the CR that I have put up here.

There should be two curves in there: (1) a monochromatic disturbance source, a monochromatic or narrow band type curve in which all your steady-state type of disturbances could be summed at discrete frequencies to make sure that the total system doesn't exceed this level, and then (2) a combined environment curve due to all the possible disturbance sources, which could be analyzed and compared against the PSD criteria in some average power approach. Now, there are a couple of questions with this, and they revolve around the transients. There is this question of transient requirements to try to bring to closure.

This chart (fig. 2) shows typical problems in analyzing transient data. For instance, we have the acceleration response at the module-to-rack interface for crew pushoff, and for pushoff and stop maneuvers. You can't read it here, but the mean value of this time signal is very close to zero, 0.007, and the RMS value is 2.729, which is an RMS average across that whole data window of 101 seconds.

Here, one question that we had when treating these transients is, when using the PSD type of requirement, what window should you use on this transient? Obviously, if you brought the window down to perhaps 50 percent of this peak, you would have a higher RMS level that would process through into your PSD to be compared to the requirement. So here is a sample. There is this signal, in effect cut off at 2.8 seconds, where the difference between the initial peak and the 50 percent reduction of that peak.

And shown here (fig. 3) are the results when you bring it down, integrate the PSD in these third octave bands and compare it to the curve.

Now let me mention that the mean value here is not centered around zero, but there is a 2.8 micro-g bias because there is an initial impulse that has not yet been encountered. And the mean value here, the RMS level, is 9.38 micro-g's. So you have increased your root mean square level by a factor of three, and your mean is not centered about 0. Now, in terms of going to the PSD and integrating in these third octave bands, you have 0.333 Hz delta f, and so you have a very coarse field of data that you are integrating.

There is also the question of what DC components you have and what you should evaluate that as. Should that be evaluated against the quasi-steady number of 10^{-6}, and should you believe the DC components in all cases, since it is more a fact of signal reconstruction than of the physics? You can see that there is a substantial difference between these two values. So the question of what to do with transients is next on the agenda.

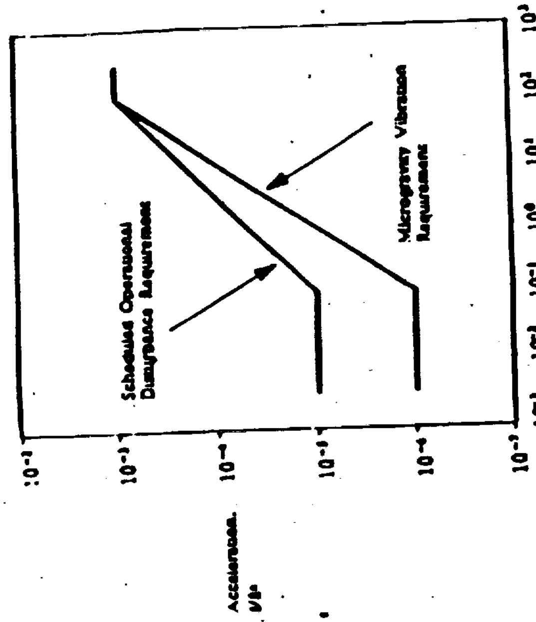
PHIL BOGERT: You might just comment on some of the advantages and disadvantages of the bigger 100 second window and the smaller window, about being able to fit those little things within third octave bands and things like that, just to put it into perspective.

STEVE DEL BASSO: Okay. This doesn't show it directly, but I might have some other charts that show it more clearly. But the point is that if your data comes off with a .33 Hz delta F, that energy is now being averaged over a number of third octave bands. But if I used the whole 100 second window, that energy would be reported in .01 Hz bands, and that would basically fit into the third octave band bins that we have set up. So, by spreading that energy across more third octave bands, in effect, you could meet the requirement easier. Going to the transient, back in 1988 the Office of Space Science and Applications published a CR that had something to do with transients. I think it was trying to address not so much a peak detection type of thing, but rather was looking for a DC bias kind of a signal and wanted to limit this. After an iteration within the program, 10×10^{-6} g seconds was determined acceptable.

So, for dealing with transients, one of the suggestions is that each individual transient disturbance could be compared to a criterion such that the integration of the transient signal in any 10 second window is limited to 10×10^{-6} g seconds. Physically, I don't have a feel if this is a linear effect on the experiment systems, but if it is for peak detection or something. If I took this down to .01 seconds or so, where typically our structural analysis time steps are about that fine, I'd have a one micro-g peak limit. So, these kind of things might be applicable, but of course the transient of the entire combined environment is not available. We don't have the acceleration data for the combined environment.

SSCBD/SSCN 8800010A | SPACE STATION PROGRAM DOCUMENT CONTINUATION SHEET | PAGE ? OF ?

TITLE: Microgravity Environment



Microgravity Vibration Requirement

For $f \leq 0.1$ Hz: $a \leq 1 \times 10^{-6}$ g
For 0.1 Hz $< f <$ 100 Hz: $a \leq f \times 1 \times 10^{-5}$ g
For $f \geq 100$ Hz: $a \leq 1 \times 10^{-3}$ g

Scheduled Operational Disturbance Requirement

For $f \leq 0.1$ Hz: $a \leq 1 \times 10^{-5}$ g
For 0.1 Hz $< f <$ 100 Hz: $a \leq f^{2/3} \times 1 \times 10^{-13/3}$ g
For $f \geq 100$ Hz: $a \leq 1 \times 10^{-3}$ g

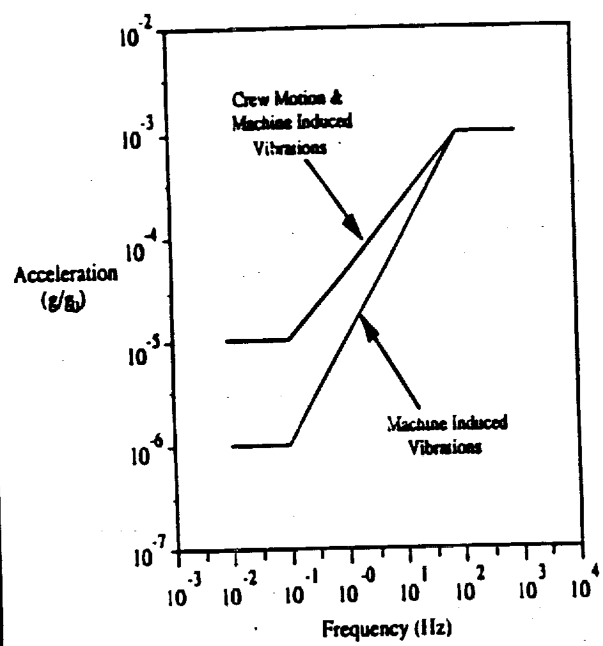
where: f = frequency
a = maximum amplitude of the acceleration

Note: Frequency spectrum valid only over the range of structural response.

FIGURE 3-23 MICROGRAVITY ENVIRONMENT OSCILLATORY/TRANSIENT DISTURBANCE ACCELERATION LIMITS

SSEIC	DYNAMIC REQUIREMENTS	Dynamic Assessment Methods	
		Steve Del Basso/Grumman (703) 438-5674	Date: 4/2/91 Page: 6

NARROW BAND



BROAD BAND

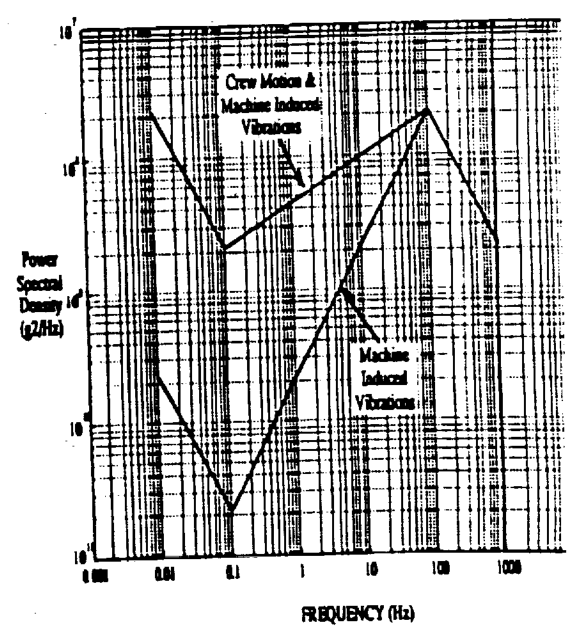
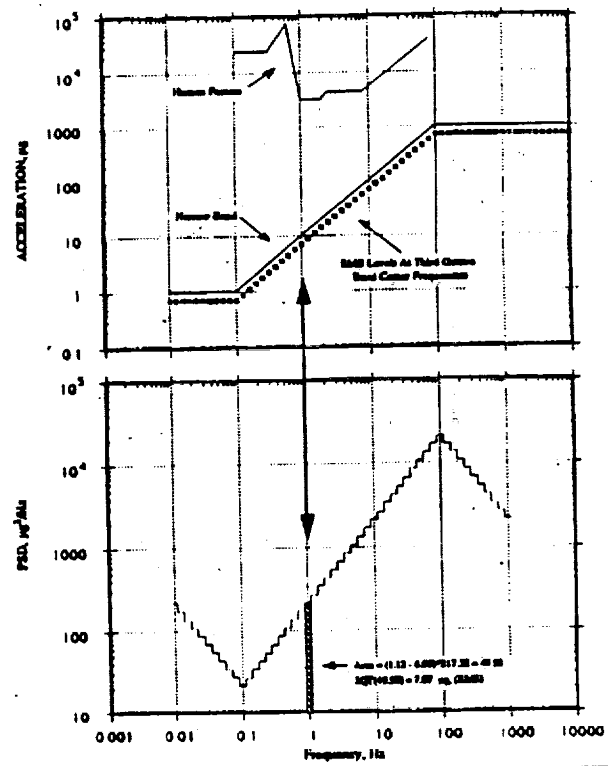


FIGURE 2

DYNAMIC REQUIREMENTS

Steve Del Basso/Grumman
(703) 438-5674

MICROGRAVITY ACCELERATION REQUIREMENTS

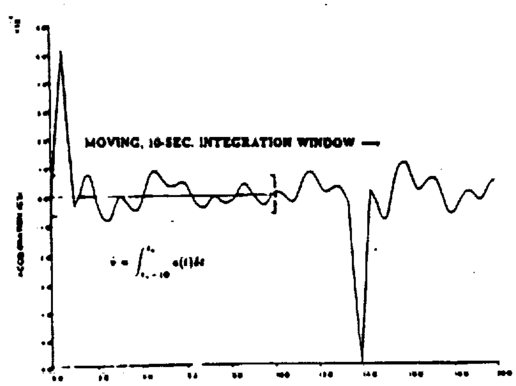


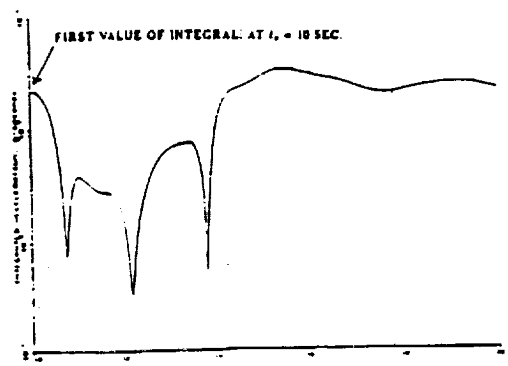
FIGURE

REQUIREMENTS

RESPONSE TO INDIVIDUAL TRANSIENT DISTURBANCES

- Peak limit of 1000 μg.
- Integrated acceleration limit of 10 μg-seconds over any 10 seconds.





FIGURE

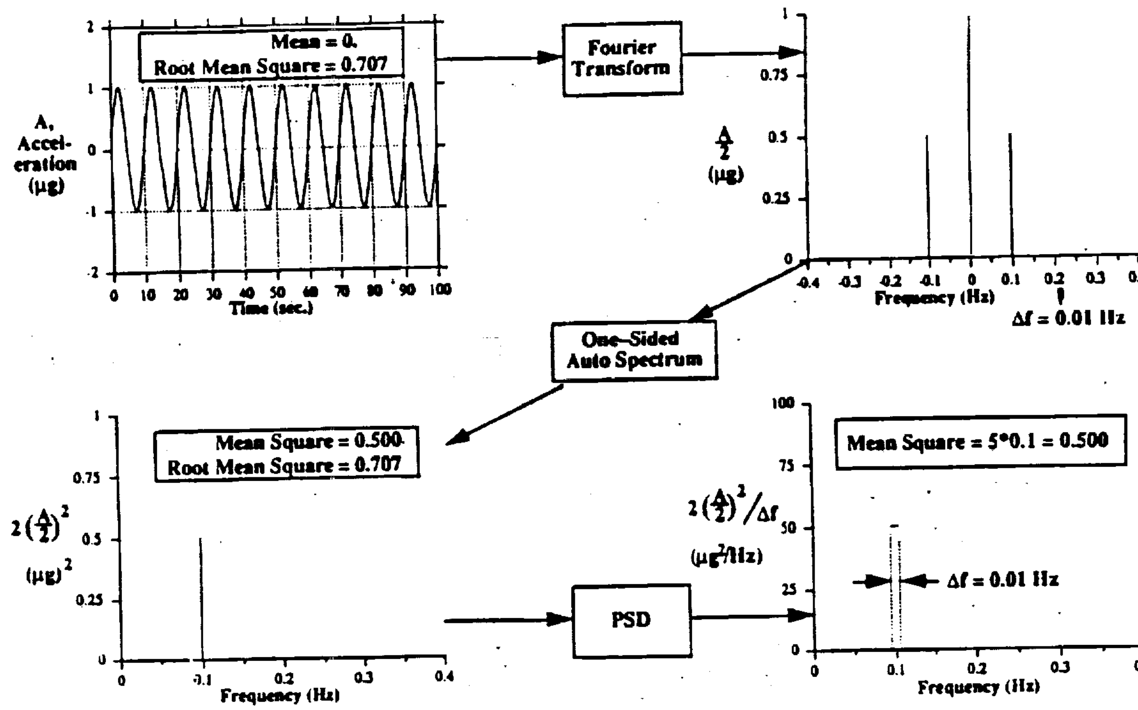
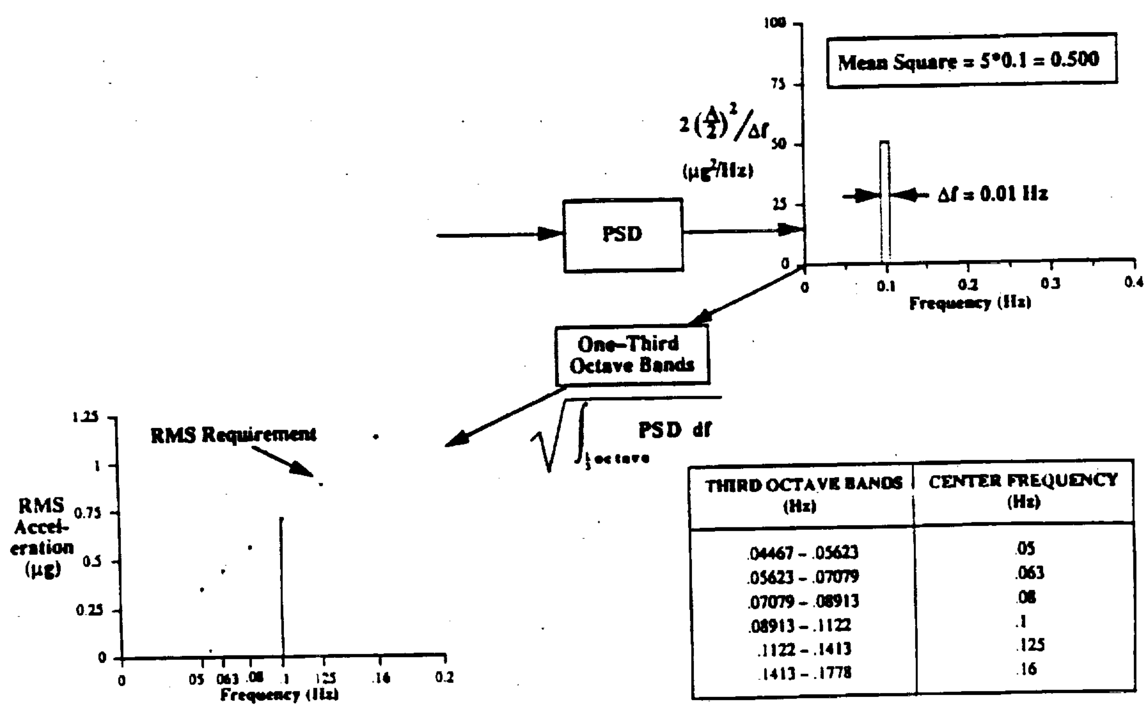


FIGURE 5

TERMINOLOGY

Dynamic Assessment Methods

Steve Del Basso/Grumman
(703) 438-5674



THIRD OCTAVE BANDS (Hz)	CENTER FREQUENCY (Hz)
.04467 - .05623	.05
.05623 - .07079	.063
.07079 - .08913	.08
.08913 - .1122	.1
.1122 - .1413	.125
.1413 - .1778	.16

FIGURE 6

	ASSESSMENT METHODS <15 Hz	Dynamic Assessment Methods	
		Steve Del Basso/Grumman (703) 438-5674	Date: 4/2/91 Page: 19

TIME DOMAIN RESPONSE ANALYSIS

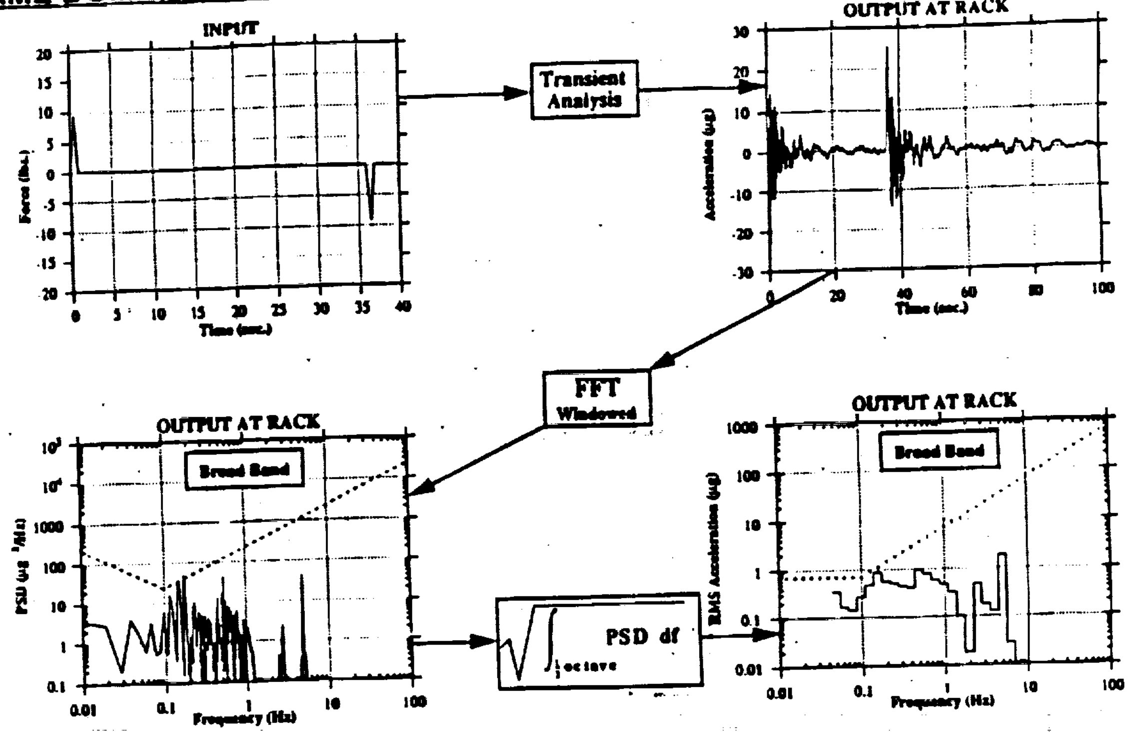


FIGURE 7

	ASSESSMENT METHODS <15 Hz	Dynamic Assessment Methods	
		Steve Del Basso/Grumman (703) 438-5674	Date: 4/2 Page: 2

Narrow band processing dependence on time window.

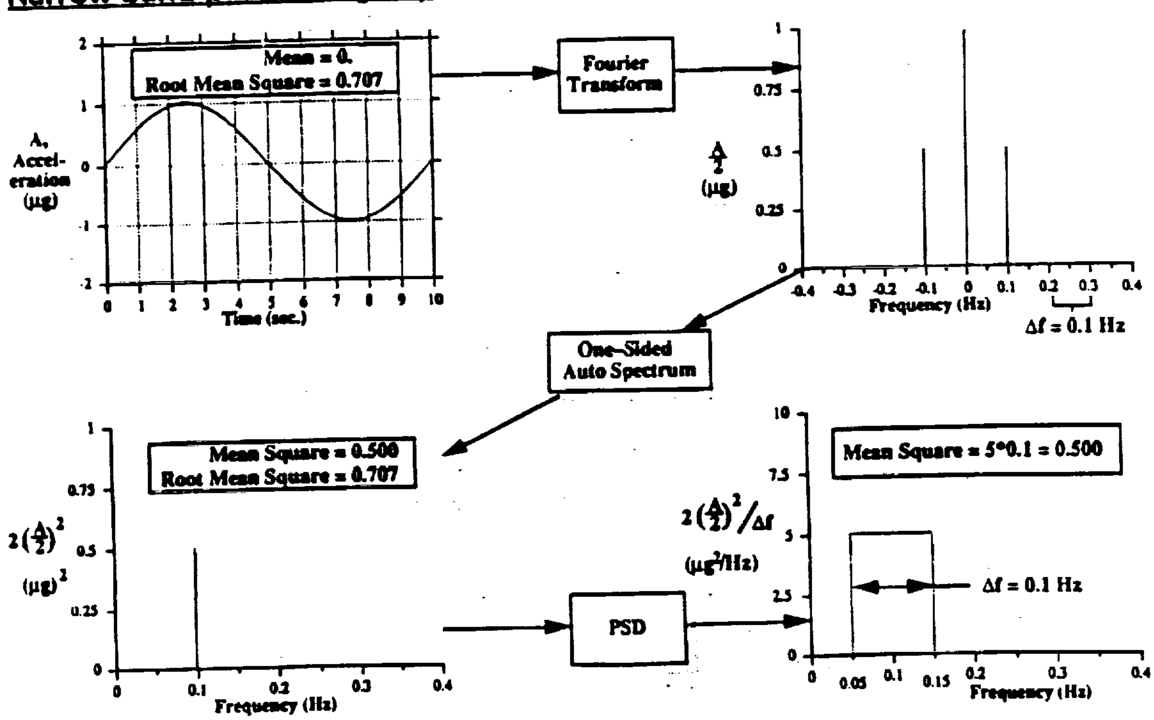
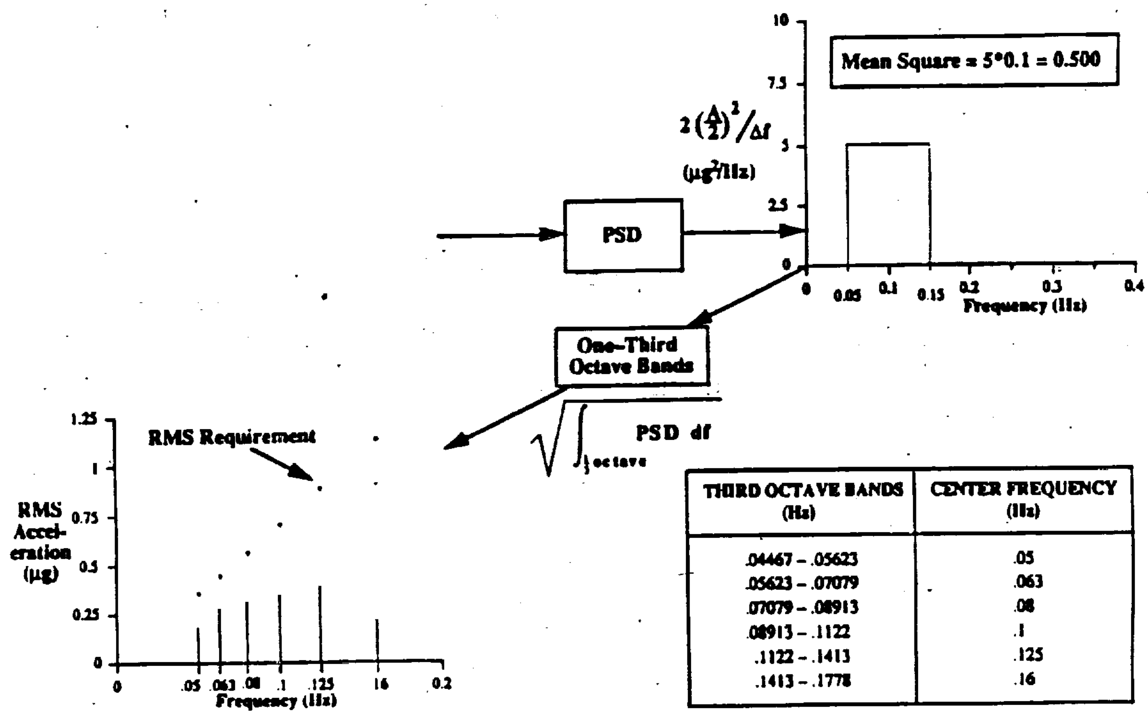


FIGURE 8

ASSESSMENT METHODS <15 Hz	Dynamic Assessment Methods	
	Steve Del Basso/Grumman (703) 438-5674	Date: 4/2/91 Page: 22

Narrow band processing dependence on time window.



THIRD OCTAVE BANDS (Hz)	CENTER FREQUENCY (Hz)
.04467 – .05623	.05
.05623 – .07079	.063
.07079 – .08913	.08
.08913 – .1122	.1
.1122 – .1413	.125
.1413 – .1778	.16

FIGURE 9

ASSESSMENT METHODS <15 Hz	Dynamic Assessment Methods	
	Steve Del Basso/Grumman (703) 438-5674	Date: 4/2/91 Page: 24

TIME DOMAIN TRANSFORMATION METHODS

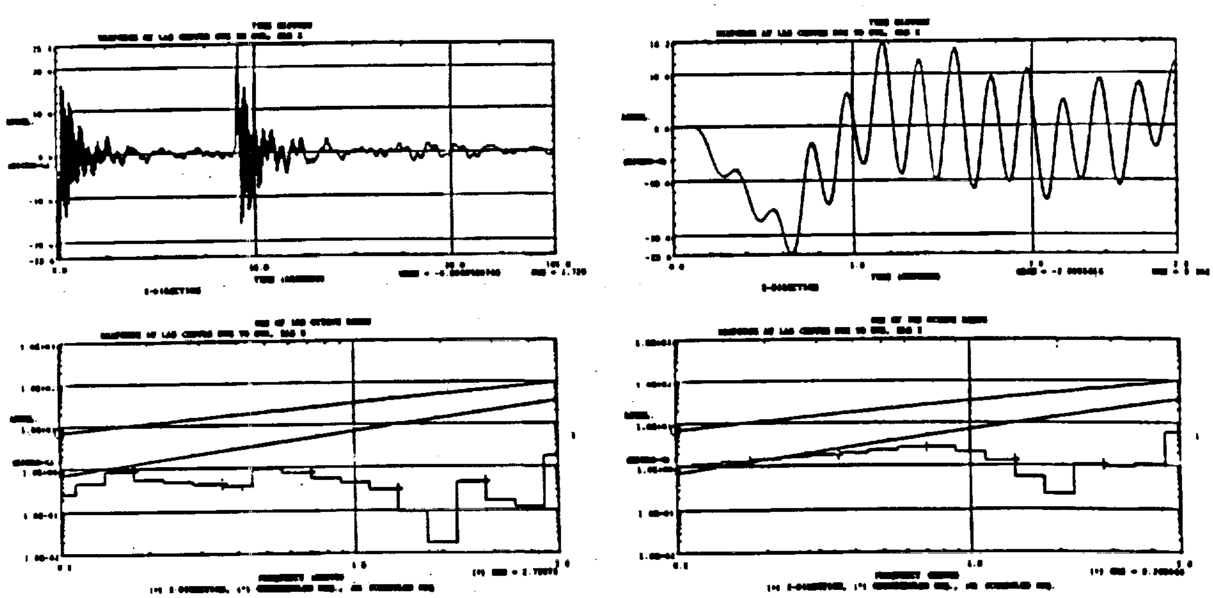


FIGURE 10

TIME DOMAIN TRANSFORMATION METHODS

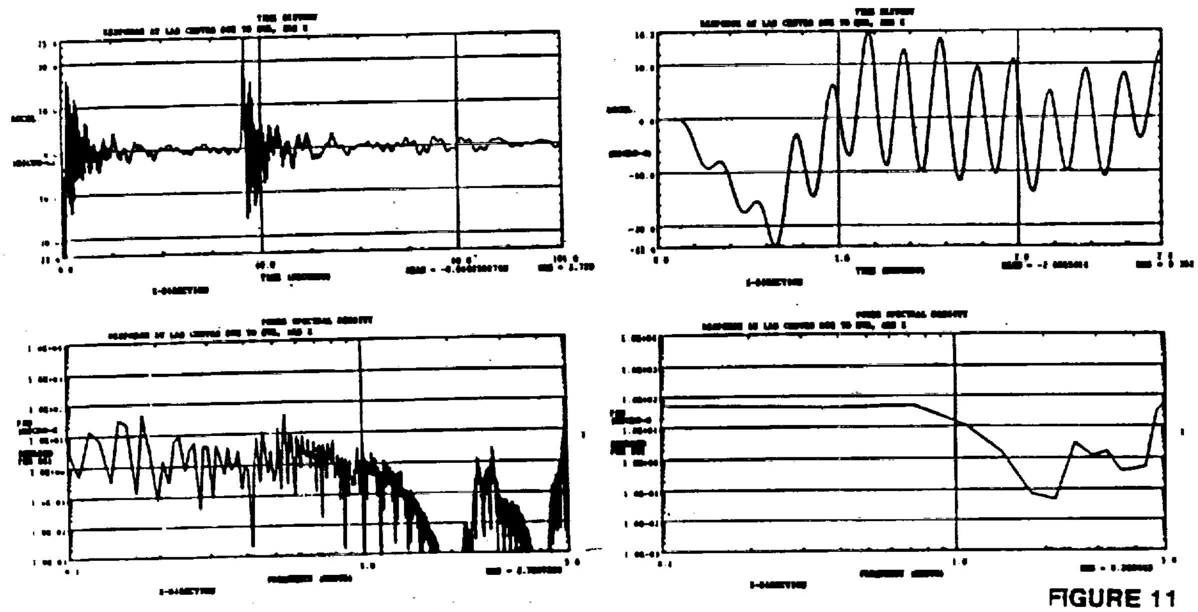


FIGURE 11

One could at least check individual disturbances, such as the crew pushoff, and limit that against this kind of criteria. But then also throw in that crew disturbance into this broad-band requirement; then you use some engineering judgment on your window. But typically, the 100 second window or 50 second window is something that I think would preserve the data going into the third octave and into the third octave analysis, rather than spreading it across the number of octave bands.

Just to show that in the PSD curve that we showed before (or the one we referred to as broad band, in terms of RMS levels), these are the acceleration bounds. So, if I went from 1 Hz to .11 Hz in that decade or so, I'm actually allowing 11.6 micro-g RMS with that PSD, and here are similar levels. And here is some of the rationale for those third octave bands.

N. RAMACHANDRAN: What if you were to look at this based not on the octave bands, but were to consider, say, that same one Hz? What if you were to look at that first curve, the original requirement with no PSD?

STEVE DEL BASSO: Yes, at one Hz it would be 10 micro-g's amplitude.

N. RAMACHANDRAN: You said doing it this way will allow you to reach requirements, but not the other way. Did I get you right there?

STEVE DEL BASSO: No, that was the sensitivity of the third octave approach to the delta F, to the window-length Fourier record. If that Fourier record is 100 seconds long, then I have data in my frequency domain at .01 Hz. Therefore, if my energy is concentrated at one of those discrete .01 Hz frequencies, it will be shown in that third octave band. But let's say that the delta f is .33 Hz, and now I am going to spread that in the spectrum; I have energy at .33 Hz, but that's going to be spread out across a number of third octave bands.

Is there another question?

DAMON SMITH: When you say you are spreading the energy out, it really isn't affecting the energy distribution of the physical process. What you are saying here is that you are spreading out the energy as it is defined in your Fourier transform, as you look at it.

STEVE DEL BASSO: That's right. And in my evaluation technique, in some cases it could evaluate out; it could evaluate differently, depending on the window.

DAMON SMITH: So you have to specify the window, don't you?

STEVE DEL BASSO: Right. I would like to say a 100 seconds window.

DAMON SMITH: Okay. One thing that is confusing is that for about half of this talk I thought you meant third octave bands and I thought -- that it is three times that I am going to double the frequency. But what you really mean is one-third of an octave.

STEVE DEL BASSO: That's true.

DAMON SMITH: So that's very different. Okay. Thank you.

STEVE DEL BASSO: I think basically those are some suggested approaches, and again the question is if it is reasonable to add any kind of transient requirement.

CHARLES BAUGHER: Let me just comment that your recollection of the transient situation and mine are pretty close. But as I recall, we were working with 10 micro-g seconds as a requirement. We were thinking of applying it to the total impulse that you would see at any time, and not to each source, on the basis of one person versus ten jumping up and down together, or something like that. But as I recall, there was some difficulty with space station. They had some mechanical problems such that they thought that was a very stiff requirement to actually be able to meet. I do not recall precisely where the difficulties of the space station were. We never did follow up, but we can go back and see where the modelers were.

STEVE DEL BASSO: Well, I might have some of that old data with me. There were items such as mobile transporters that have a long acceleration period, like 30 seconds with four pounds of force input. I think that numbers that it exceeded were of the order of 100 times 10^{-6} micro-g seconds.

Now at this point, I guess I'm looking at the requirement from another aspect, that the requirement treats all the issues and makes sense to the user, rather than whether it can be met in each aspect of the space station.

GARY MARTIN: When we negotiated the requirement for impulse, we really didn't let it go away. What we were told was the station would work with us to look at the size of the windows; and that they would be doing the analysis to see what we needed to capture the impulse, because we knew we had a problem. Yet here you are still looking at 100 second windows. Have you done anything or thought about what we could do to work with you to maximize that window or to capture the requirement?

STEVE DEL BASSO: Well, to me, that's the window size of the record length of the Fourier transform; that issue can't be worked, really.

PHIL BOGERT: I think that's kind of what we are trying to do here. Steve showed an example in which, depending on where you pick the window, you can either miss the impulsive things or the spikes. If you look at the area under both of these alternative curves here, they both kind of add up to one milli-g. Incidentally Steve, if you take our original curve and if you sum up the total allowable g's between .1 Hz and a 100 Hz, it's also about one milli-g; isn't that correct?

STEVE DEL BASSO: Yes.

PHIL BOGERT: So there is some rationale for the window approach. It's a good window in terms of resolution, and it smears in fine with the power stuff. Then, when you add the steady state and random and do the whole power thing, and look at our PSDs, that's good. But we are concerned that we are not limiting peaks and also that we are not limiting the almost quasi-steady type stuff like mobile-transporter motion or DC components -- impulsive-type things.

So we are proposing to do those two checks for transients in addition to the normal process, where we lump everything into the PSD with all the other stuff happening at that frequency. So it is some extra insurance; we are not canceling out what we plan to do. What I showed earlier in each third octave band has to do with the problem of converting a true transient into the frequency domain. Because of the Fourier transform technology, the windowing makes a difference. There is no exact way to do it.

So this kind of covers the two extremes that people have talked about. You can spread it in different ways in a PSD and that's fine. But how do you limit short spikes that might affect the experiment? And how do you limit what you yourself have been very concerned about lately -- these impulsive type of things, like something that is almost quasi-steady, that so-called region of the curve you say we are ignoring. We think this somewhat covers both ends of the spectrum by also allowing us to do the power thing to add to everything else? So it is kind of "in addition to" and not "instead of."

N. RAMACHANDRAN: Let me add something from the fluid mechanics point of view. Whatever you do here, you can't get away from the fact that we get impulses. And whether you track them or not, they are going to be there. They are random in nature, unpredictable, and they do occur.

Previous experiments on Spacelab D-1 missions have shown that you could break off a flow zone because of this impulse. You might not be concerned about that, but the flow zone is totally broken up, and there is no experiment to conduct. So I think that when you talk about a pristine environment being provided for a three month period on a space station, we should have some control. I don't know how, but we should have some kind of means to ensure that impulses are kept to a minimum, if that is possible, and to not lose track of that fact, because it is critical.

STEVE DEL BASSO: Yes. For assessment purposes, this approach would be applied to an individual disturbance or maybe to some operationally phased combination. If you said individual disturbances, it would be clear to me that then you could go to your analytical model and do one disturbance source at a time. I don't think it's possible to predict the overall transient environment.

DAMON SMITH: I think about half the people I know who work in this field believe that if you say you have a 10 micro-g limit, that means that at no time will the experiment see 10 micro-g's. I think one of the biggest sources of confusion in this whole subject is the complete lack of understanding and lack of clarification about that aspect.

It's compounded by the fact that sometimes we see amplitude-frequency graphs like the Naumann curve, with triangles on them indicating the frequency and amplitude of a disturbance like a treadmill. That is a time domain disturbance, I am sure, because they have never done the Fourier transform of that disturbance. So here we see some time domain, and I guess what someone has done is looked at the 5000 micro-g that the Honeywell accelerometer reported. We have no idea what the frequency makeup of that was, it may have included transients that were very high. They plot that thing as a triangle on that frequency graph as though it were one frequency, and that compounds the confusion among the people I talk to.

Let me just finish one more thing. It worries me that -- so far, I see the concerns here about the frequency, and here I am playing devil's advocate -- I can make a force signal that will break every experiment on the shuttle or space station and still meet your requirements. It worries me that can happen.

STEVE DEL BASSO: Well, there we are back to the user, because I am not aware of what kind of force makeup you would be sensitive to. As I showed that PSD, I said we are actually permitting 20 or 30 simultaneously occurring sinusoids with that approach. You could view it that way.

Now, the user should be aware that in the case of random data , we are talking RMS levels. If you look over here at the questions and add transient requirements, another problem with the implementation is what these curves are.

Are these absolute sense curves, should we express some probability with them, or should we express the limits with the number of events of exceedance, or maybe the mean time between events?

DAMON SMITH: Can you tell me why you can't just say that at no time will you exceed a certain micro-g level?

STEVE DEL BASSO: We can't because, given some random data and knowing the distribution of those data, I might have to go three to four times the mean value of those data to, say, 99.9.

DAMON SMITH: But you could impose that on the contractor who is building the important piece of equipment, and it would then be their responsibility to check it.

STEVE DEL BASSO: That's correct. You could do that. I think it's a question of these being strict requirements at mean levels. If this is a mean level and you want to talk about three sigma, four sigma or whatever-type more absolute levels, for certain items I can see that as doable, given the standard deviation of that particular event.

It could be a bigger hit. I think that's a question that's still up on the board, because we really haven't resolved it in the requirements. The requirements don't say anything to this effect until maybe in this change request. There is a phrase of two sigma in there. I believe that it was assumed that these were absolute. I think that to the users, this was something that you would not exceed one time in a 30 day period, given the dynamics of that kind of environment to ensure that.

DAMON SMITH: I work for Lockheed, and I don't want Lockheed to build a centrifuge that crashes a motor against the side of a wall when it turns on. Right now, we could do that and still meet the requirements, because here is an example of a transient that is composed of frequencies, all of which fall below the frequency distribution line. If I could make a spike of 100 Newtons or a thousand Newtons, composed of frequency components then that, even with your third octave averaging, would survive your requirements.

STEVE DEL BASSO: In a mean square sense, doesn't the third octave PSD averaging limit the values to these micro-g levels within, this frequency?

DAMON SMITH: Yes. But I could position 100 or 1000 sinusoids in the middle of each of your bands, and add up each one of these micro-g levels, and I can give you g's.

STEVE DEL BASSO: Yes, but your g's have to be limited to this within that band.

DAMON SMITH: They will be within each frequency band, but it will still knock you across the room, because in the time domain it becomes a big signal.

KEVIN SCHAEFFER: I'm from Space Station Headquarters, and that's one reason why we are talking here today, because we already recognize that problem and Steve is up there showing possible restrictions on impulses because we recognize that.

DAMON SMITH: Yes. That's what I want to talk about.

KEVIN SCHAEFFER: With respect to the absolute things, a little history is needed here. We have had that requirement for a while now (about a year) and, as everyone knows, one of the biggest variables is the crew activity. And the range of inputs that this can have is huge. What we are finally settling on is that we want to design the machines to meet the lower curve, because the machine dispersions are very narrow as compared to the dispersions for the crew. We also found out that is considered an absolute number and that we can't just design the machines to the low curve and forget the crew who use the machines. So, while we are going to include some crew activity, we can't include all crew activity, because the dispersion is too great.

DAMON SMITH: Right.

KEVIN SCHAEFFER: In the example used earlier, letting crew members play football up in the lab just wouldn't hack it in terms of microgravity.

DAMON SMITH: Right.

KEVIN SCHAEFFER: So what we are doing, -- and we are still struggling with this -- is still trying to get a grasp of what the dispersions are for crew activity. And what we are targeting right now is that the lower curve would be an absolute limit for all the machines and some mean of crew activity.

DAMON SMITH: I guess I am more concerned about this when it comes to big machinery. I assume the crew doesn't start playing football, but only whatever else they normally have to do to live on the space station. Let me just explain what this graph is that I have just put up. I just spit this out of my computer before I came to the conference and it is kind of crude. At the bottom I show an assembly of 10 sinusoids of .05 Hz each, which are summed over to 0.5 Hz if you go halfway across the graph there. So those are 10 sinusoids with a magnitude of one, which are spaced at 0.05 Hz intervals. The top graph is the time domain, a summation of those. Here you can see a peak amplitude of nine units, whatever the units are. So essentially, what is happening is the phase bewtween each of these is the same. One of the big factors in whether the sinusoids add constructively or destructively is the phase relationship between them.

Here in this plot is an example of an assembly of -- let's call them one Newton -- four sinusoids. And you can see that I can manipulate them to meet the Naumann graph. I could have 100 of those and I can come up with a very large transient that's occurring in the time domain. And I could space those so that I could meet the requirements you show on the left, and in the time domain still give you a very large destructive transient.

CHARLES BAUGHER: But it still can't get any bigger than ten times.

DAMON SMITH: Absolutely. In fact the worst case is the total summation of all the components in amplitude.

CHARLES BAUGHER: So the worst case we can have over here is the sum of these things.

DAMON SMITH: Yeah. So what would that get you?

CHARLES BAUGHER: About six milli-g's.

DAMON SMITH: So I could meet that requirement and still completely invalidate every micro-g experiment.

CHARLES BAUGHER: Right. We recognize that, and the problem is that we would like to establish some transient level that satisfies the experiments.

DAMON SMITH: Right.

PHIL BOGERT: I guess this is a case of a resonance building up.

DAMON SMITH: Or it could just be one hit.

PHIL BOGERT: What we are doing with all the buzzers and shakers acting together is a SRSS approach to account for phasing. We are not adding them algebraically; this is a worst case.

You do have a point, because we thought about this for the gyros also; there are beat frequencies that kind of work with each other. You might get a real buildup that the SRSS won't pick up. So, perhaps yet another check we should add, in addition to our smearing over the power and these impulse and spike checks, would be to look for specific resonance responses where things are correlated. The SRSS might not be a good representation of phasing. That's another candidate requirement.

KEVIN SCHAEFFER: This check you are referring to, though, I wonder how often it would occur? I mean, do we just do that as a part of smart design or smart engineering, or is there some sort of automatic check that can be done on computer? I mean, how does one do that?

DAMON SMITH: At the supplier level, you should impose a limit in the time domain. The problem is that it's very confusing when you are thinking on the total system level. As to how you are going to measure it and verify it once it's on the station. I know that's difficult, but you should at least require the contractors to meet the frequency requirements, and at no time to exceed some limit. Then, let them go and do their vibration tests to make sure that when they turn on their rotating equipment, there is nothing that bangs really hard in the time domain.

As to the way I am dealing with the ergometer project, and I was looking at this and seeing what requirements we would have, I decided to say that at no time in the time domain will the steady state pedaling of the ergometer exert more than three-quarters of a pound on the shuttle mid-deck. Now that's the most sensible interpretation I could make out of the Naumann graph, because to me the time domain is essentially a summation, just as we show here the summation of those frequency components. I feel more comfortable in saying to NASA that at no time am I going to exert a force which violates their micro-g limits. So I really think you should specify something in the time domain, because every time you go into the frequency domain you are going to give up some way of specifying in the time domain. The easiest way is just to specify a limit and say never to exceed it.

STEVE DEL BASSO: Just one comment on the number of disturbance sources that you might have to place time domain limits on. You have to build some kind of model to predict what their total combined effects would be, and then work back down into the time domain.

DAMON SMITH: If you don't do that, you have no way of enforcing time domain activity. The contractors are going to say they met the requirements, but you may still have a horrible, unlivable time domain situation.

JEAN KOSTER: As a fluids experimenter I want to emphasize that I am flying an experiment on IML-2, and I could simply say that we need both sets of information, time domain and frequency domain. We cannot separate them. That is I think a very definite statement.

DAMON SMITH: Yes.

JEAN KOSTER: Most of the experiments -- my experiments especially -- will probably be destroyed at the moment of the first hit, and especially if we have a couple of hits, even if it is in the specs, the experiment will fail.

DAMON SMITH: So you are concerned with the time domain, aren't you? You think time domain?

JEAN KOSTER: We need both. Also, for the field mechanics information we are also very often interested in the time domain and the frequency spectrum inside the liquids; so we need both kinds of information.

DAMON SMITH: I should point out that you have the same problem we were talking about, in how we are going to deal with the summation of all of this on the station, and then to back it out to say what the supplier has to live up to. You have the same problem in the frequency domain, and I don't know how you are going to deal with that -- and I sympathize with you. Resonance modality is another big headache and, of course, that's a frequency domain issue. So really, all I want to emphasize is that you please do not walk away from the time domain. I know it is hard, but you lose something if you don't include it.

STEVE DEL BASSO: Thanks for your comments.

CHARLES BAUGHER: Steve, the page that you had up there before (fig. 4) on the transient was how we were trying to deal with that problem previously.

KEVIN SCHAEFER: I can see a difference between this and what Damon was saying, this is an impulsive force you can see, like an astronaut bumping a wall or snapping a latch shut. What Damon was talking about was some source that produces multiple frequencies.

CHARLES BAUGHER: It is the same thing. If you take these multiple sources and add them up, you are going to see a spike every so often in time. If you take that impulse and break it down, it is going to spread into multiple frequencies. It is basically the same problem; you take the spike and break it down into frequency components, or you get a lot of small components or you take a lot of small frequency components and add them up and you get a spike over in the time domain.

DAMON SMITH: So you can do the Fourier transform on a single transient event and get a distribution. Then, if you reconstruct it, go back into the time domain and you go beyond your time window, it will repeat, so there is really no difference in whether, say, the guy is slamming a payload bay door and then he waits 30 seconds and he does it again and again.

CHARLES BAUGHER: This is a chart similar to Figure 4, showing some real data with several spikes in the frequency domain, all of which are a few hundred micro-g's. We are seeing spikes in the milli-g range at a number of spectra components at lower levels turning into fairly high spikes in the time domain.

KEVIN SCHAEFER: It is the same thing, except the chart you showed is much narrower; there are far more spikes than are indicated here. One thing I'm worried about is that while I could see what you are saying, somewhere a lot of pieces of equipment don't necessarily deal with periodic components at all. I don't how, out of that chart, you get what to tell them about the maximum force they can produce.

DAMON SMITH: Are you going to have trouble including a time domain limit?

KEVIN SCHAEFER: An example of what I'm talking about is a latch on a drawer, and it snaps shut. What you want to tell that designer is that his latch cannot produce X pounds of force when it shuts, which tells him how he is going to design his latch. But there are X pounds of force that he cannot produce and that's how he is designing the thing; he's not designing things about frequency domain or time domain or anything.

DAMON SMITH: It would be that at no time could his latch exert that force during the closing action, wouldn't it? So that's a time specification.

STEVE DEL BASSO: Can I just make a point or two, then we can continue the discussion. For me, the last points are: if we could perhaps write down some approach to these transients, which we might have altered from the ones that we had, then the other comment is whether we are to deal with this data in an absolute sense, a probabilistic sense, or in a number of events type of sense. These are two issues on which I would like to have some discussion.

DAMON SMITH: Just one more comment and I'll shut up. As your parting challenge was on how we deal with this, I think you should ask the principal investigators who are running the experiments what they need. You heard a man who is flying on IML-2 say that if he takes a big hit, his experiment is shot; now, that's a time domain limit. It is a time domain issue and you cannot appropriately deal with it in the frequency domain. It is as simple as that; you can't deal with it with RMS acceleration. You can't deal with it with frequency plots. It is a time domain issue.

STEVE DEL BASSO: It seems like an obviously large canvassing job right now, to reach each of these PI's and to try to address all of their concerns in the requirement.

GARY MARTIN: I would like to make one comment. We don't have any PI's currently picked out on the space station, at least for microgravity sciences, but this is obviously a question that we have been concerned about for some time, especially because you are talking about the 100 second window.

We always thought that we could window -- maybe you have to do that to capture everything -- but you could also do smaller windows to look at impulses. What I have been waiting for is to ask Mr. Eilers, who participated on the only spacecraft that I know that has been constructed for microgravity using these kinds of requirements. I was wondering if he could give us some insight into whether they had a time domain? I know they had a RMS requirement.

DIRK EILERS: Okay. Thank you. I tried to report what we did on Eureca, which was the first spacecraft where a micro-g requirement was introduced and implemented. It was a design that was done starting about 1983 to 1986, and now the spacecraft is ready for launch, maybe next year. The specification that was defined for Eureca was based on a more or less generous amplitude acceleration spectrum; but we discussed this also under the various aspects that we heard today, and we did not change it. We proceeded with the design and found that we could work with this approach. From our system analysis, equipment testing and the final system analysis, and now the integrated system test that we had last August, I think that the results presented are within the specification we had, and were accepted by the agency.

We considered the aspects addressed here by you, in the sense that we understood this amplitude spectrum as a simple amplitude spectrum. So, we transformed all of the information that we had coming from the time of simulation, coming from frequency response analysis and coming from test data, often resulting in power spectrum density. We considered dynamic disturbances and transfer functions, which are also narrow band information, and we combined them together. The major point is -- when we talk about this power spectrum density definition of the time series signal that we converted this back into an amplitude spectrum, considering that the power depending on the analysis bandwidth was concentrated at the center frequency of our spectrum such that it would be equal to harmonic vibration component. This was our understanding and our interpretation in handling and dealing with this specification.

These experiences were transformed into the Columbus project. There are slight modifications, because we decided to specify the micro-g requirements in the time domain, and in the frequency domain. We said the disturbances that may be used by the systems and equipment payloads that may have an impact on the rigid body of the system should be limited within the time domain, and should be limited to time domain acceleration limits, considering the system as a rigid body and the disturbance forcing function, which may interact with the system dynamics. The structural dynamics of the system should then be specified in the frequency domain. Therefore, in Columbus we now have these two types of specifications.

The frequency domain specification was more specifically defined, compared to the Eureca approach. We found that we always have the problem with the definition of the analysis bandwidth when we perform frequency transformation. So, the proposal was to proceed with the power spectrum density limit, or to integrate power spectrum to one-third octave band levels. This is really the situation that we now have on Columbus, that our limit acceleration spectrum is based on one-third octave band levels, which are derived either from filter according to one-third octave bands or from filter analysis. These have bandwidths smaller than the corresponding one-third octave band, which then needs to be integrated up to the octave band level. Maybe this is the information that I can give.

PHIL BOGERT: Dirk, do you have all of this written up somewhere, like in one of your specs that we could read and study? Maybe in your vibro-acoustic control plan?

DIRK EILERS: On Columbus we defined our approach to these control plans that we issued on micro-g and auto noise and the results, and we issued some papers on the Eureca activities. We gave one presentation here at Lewis two years ago on the Eureca micro-g environment control, and on this there is a brochure that you may have here already. If not, we can send you one.

PHIL BOGERT: That would be good; we would like to study that a little bit and relate it to this. I just have one more question for Damon and Charles. After hearing all of this about the time domain, did what Steve put down there in any way capture what you guys are getting at? You didn't seem satisfied with it, yet I thought that's kind of what he was trying to do.

DAMON SMITH: Would you put the chart (fig. 3) back up that he's referring to, the one with third octave band limits? Is that what we are doing? Also the third octave -- whether or not that requirement would really deal with the true time domain issue depends on whether you occupy each of those bands with perturbations? If you excite each of those frequency bands up to their limit and have them add constructively, you will have the sum, that right-hand column, and that's what your micro-g would be, which winds up being milli-g's.

CHARLES BAUGHER: In my view, depending on how everything added up, that might be a very sharp spike.

DAMON SMITH: That might break his fluid bridge and there he goes.

CHARLES BAUGHER: In general, it would not affect very many experiments, though there might be a specific one it affected.

DAMON SMITH: That's an area that I don't profess to be expert in. I don't know the nature of all of the experiments. And I have begun to realize that there are experiments that are not sensitive to time domain events; but if there are any, then whether you try to accommodate all of them is more or less a managerial and political decision. But I can still build you a signal that would destroy the experiment that Jean Koster described and yet would meet that requirement.

CHARLES BAUGHER: Well, he's got one of the rare experiments that is sensitive to a sharp spike that doesn't go very far into the milli-g range. But, basically, we still have the limits even with this; we still have the monochromatic limit in the requirement. We still are limiting any constant steady- state sources.

DAMON SMITH: Again, I can build a signal that will meet both requirements and yet destroy his experiment.

CHARLES BAUGHER: Well, wait a second; I'm not sure about that.

KEVIN SCHAEFER: I guess Phil's question is -- a couple of charts back (fig 4), we showed something that dealt with time impulses and max peak during a 10-second window -- does that answer your question?

DAMON SMITH: It helps. It comes closer. It depends on how big your window is. Clearly, if you have an hour-long window, the average over that will be minor, even if you have a very long transient. If it is shorter, then, of course, you are tracking it better. My suggestion is to make the window very small and track the real signal. I don't see what is gained by averaging over that window.

CHARLES BAUGHER: I agree that we need a transient spec., and I'm not trying to argue that we don't. I'm still searching for what that is.

BJARNI TRYGGVASON: I'm from the Canadian Space Agency, and I came in at the tail end of this discussion, so I am probably going to speak out of context. But when he is talking about the time domain versus a frequency domain on a structure like the space station, this is kind of like a big piece of rubber up there and vibrating in its low frequency modes. It is really hard to visualize a spike in the time domain getting into the experiment in other than one way, and that's if one of the astronauts bumped into that particular module.

He's not likely to do that. Now, he may deliver a spike to that module at that point in time, but that spike is not going to also make a spike to any other experiment in any other rack. You have to look at what generalized forces are on each of the modes of vibration of the space station. Each one of those gets a hit, but it is much less than the spike that the astronaut puts into the particular part of the station that he bumps into. So, while he may destroy that experiment, that's not going to deliver spikes to everything.

CHARLES BAUGHER: I'll agree 100 percent with that. That's sort of what we are seeing in the shuttle, that these high frequencies don't propagate. I guess that means to me that these guys have got an easier job than they think they have. We still need to be able to find that out.

BJARNI TRYGGVASON: I think it is true that looking at the frequency domain is the more appropriate way. You have to look at all of the things that are going on that are driving vibrations in many of the modes of the station. Look at what they input into the base of the mounting of a particular rack that holds a particular experiment, and you are not going to see any spikes like that. It is very hard to conceive a big spike coming through. You can't protect yourself from the other end, other than to train your astronauts very well.

DAMON SMITH: If you don't impose a time domain limit on the equipment manufacturers, they are not constrained to be reasonable. You are assuming that the people building all the equipment that is provided will say, "Gosh, we don't want to have a time domain 100 Newton force when we turn on our fan." So, while I agree with you that the spikes that you are talking about would be mainly crew-generated, because those more localized things don't propagate that far, why do we have to have an either-or situation? Why can't we have both?

BJARNI TRYGGVASON: A spike could also be generated by a piece of equipment, like a door closing. But when you look at what a door closing on that piece of equipment delivers to the rest of the station, it is not going to deliver a spike on another part. It is going to deliver vibrations in many modes at once, which are supposed to meet the criteria. So, it still comes back to what that particular piece of equipment does to itself, and to the experimenter designing a piece of hardware. If there's a door that is to be closed and he's either doing that before he starts his experiments or if he's doing that during the experiment, he has to solve that problem in his design.

For his experiment, the rest of the station can't deliver -- through the mounting hardware and back to that experiment -- the vibration that breaks the criteria in the frequency domain. It is not going to deliver a spike to his experiment.

N. RAMACHANDRAN: As you say, in most instances these spikes that have been observed have been crew-generated. I don't know that it has to be sheer chance that all of these frequencies will add up to generate that spike, but that is also possible. We cannot predict these random spikes at all so it would be hard to justify a requirement, because we don't know when a spike will be produced and how it will be produced. That's why I think we can't have that requirement in writing. Of course, we can ask the astronauts to be nimble, gentle, whatever. What we might suggest is that for each experiment that is sensitive to something like that, some kind of passive isolation might be provided ahead of time. I think that might be a fruitful thing to do.

DAMON SMITH: What is happening here is we are confusing points of analysis. You are talking about the summation of all activities at a system level; but I'm talking about imposing limits on the manufacturers of sources of vibration, like fans and rotors and pumps and centrifuges. Just because you have trouble analyzing the summation of all of those effects doesn't mean that you shouldn't impose some time domain limitation on the people who make the components. If you don't, then you are totally at their mercy.

KEVIN SCHAEFER: As to the crew, there are a couple of things that we have to think about when we are talking about crew activities and the disturbances that they create. There are the direct crew disturbances, wherein they literally tap the sides of the module or experiment, things like that, and that's direct crew contact with the structure.

But there's also the other kind of crew disturbance, in which the crew member uses some mechanical device, there's some machine/man interface that he uses and the disturbance comes from the machine/man interface. There the man does not directly input something, it is from a machine. A perfect example of that is a latch being shut. We have to split out those two categories, and we have to tell the designers of those machine/man interfaces that they cannot exceed certain values; maybe put it in terms of force limits. In the case of latches, you have to tell them a maximum force or whatever. But we can't always say that because man is something we can't control, we can't place limits on it. It's true we can't place limits on how a crew moves around inside a module, because that's too random. But we can place limits on the machines that the men use, and we will place those limits. I also agree with Damon when he says that when we get to the equipment specs we will have to include these max transients or peaks, I think you call them, in the time domain. We haven't quite got around to doing that yet, and so I'm glad we are having discussions like this today.

STEVE DEL BASSO: As one more point, there was a question raised about the effect of the window length and why I suggest not going to a short window in the Fourier analysis in terms of evaluating the data. I have a couple of charts here that would address that (figs. 5 and 8).

This graph first shows a 100-second data window of a 10-second, 1 micro-g sine curve going through the Fourier spectrum. Now, the delta f in this case is .01 so that, basically, when I get to the PSD level, I'm taking the mean square value and smearing it over that .01 band. Here, I'm going to integrate that PSD into these third octave bands, and here is the feeling of the third octave bands with these center frequencies in this level. We chose this allowable at 1 micro-g, 0.1 Hz; therefore, the root mean square allowable is .707 micro-g's. You see with this kind of 100-second window, I just nail that level, and the evaluation comes out within that one-third octave band.

If, on the other hand, I chose one period of that signal, or a 10-second window, I have now the same mean square level. In this case, we have a 10-second window, so we have .1 Hz delta f, and when I convert to the PSD, I now average that over .1 Hz band.

Now, at these low third octave band center frequencies the integration of this .1 Hz gets spread out over six one-third octave band ranges. This assessment would suggest that you meet the requirement. The comment here is that in the Fourier analysis, I would want to keep my delta f's small enough that I can fit into these bands where I expect my response to be, which is structural modes, or the steady- state forcing function.

There is the approach to the window; I think a 50-second window would give you .02, which would give you at least one frequency line in the 1/3 octave band, and that's about as low as I would suggest you go. But the limitation is simply the way we evaluate against the requirement. That's my last point.

PHIL BOGERT: I would like to try to summarize here if we can. We recognize that the Naumann curve does not catch everything. We know that there are rare experiments that might be sensitive to transients, but as somebody mentioned, whether to cover these is a political or managerial decision, since we have so much work to do already, as we explained this morning. At this point, I want to think about it, but I don't want to commit right now to doing more research. If we did use the spike criteria and the impulse criteria, I'm wondering how we would go back and impose that on the equipment manufacturer.

Right now everybody is providing forcing functions and we are running these through a big system-integrated model. Then whatever comes out in the wash at the experiment we somehow allocate, and we have a transfer function -- force-in at the machine versus acceleration-out at the experiment. So maybe you have to cut the force in half, if everybody is over by a factor of 2 milli-g's of the experiment. Now, if we do our spike check, for example, and find that we are over there, I guess we have to think of a way to go back and limit that kind of force. We will go that far.
I'm just trying to summarize, so help me.

DAMON SMITH: Now that we have raised the awareness to this, frankly I don't think it is a big problem. Almost all of the equipment has a few dominant frequencies, like the centrifuge having, almost completely, one main rotational frequency. There are harmonics here, but they are minor; they don't really contribute much to the time domain signal. If you are meeting the Naumann curve, I frankly think it would be the rare piece of equipment that would give you a big spike.

The main reason I brought that up is because I think it is important for the people who make these requirements to understand that you give up a certain degree of control over these providers of equipment when you walk away from the time domain. It may be that you have essentially limited him in the time domain by imposing the Naumann curve, because of the nature of the equipment.

I'm not saying that you necessarily have a serious problem. I think that you may want to establish some reasonable time domain limit -- just right off the top of your head you could say we know we don't want to break the windows in the shuttle, that kind of thing. There has to be some number that you can come to in the time domain that -- even with our present confusion -- is clearly unacceptable. So if you could just put that number in, then you have some upper limit on what the equipment providers could do. As I have said, I don't think it is going to hurt you much, because almost all of this equipment has one dominant frequency with a few harmonics.

MIKE HORKACHUCK: I think you are starting to hit on what we are going to have to specify to the equipment manufacturers. I'm going to have the pleasure of specifying to either Lockheed or McDonnell Douglas what those requirements are for the centrifuge. For the designers, I think you are going to have to specify a force that they can't exceed that doesn't excite the space station. As a designer, trying to convert from a PSD curve until you have actually built the hardware is going to be real tough to do, almost impossible.

There is another point I would like you to consider. I grant that you are only respecifying the same basic intended requirement, but every time you impose another condition, the contractor is going to come back to me with a bill for verifying that they met that requirement -- it costs money.

PHIL BOGART: That is true, except that they have never done impacts on the first set of requirements, so I think we can sneak this change in kind of as a freebee.

MIKE HORKACHUCK: If you have them do three or four more checks to verify the same thing, that is going to make it much more expensive as an overall program.

KEVIN SCHAEFER: One thing that you have got to bear in mind is that all of the contracts are supposed to have man/vibration specs on them. So those checks or those tests should already be included. Earlier today, someone from Honeywell said that they haven't even heard of vibration specs. That is something that we are going to resolve, but our contracts do include them.

MIKE HORKACHUCK: They didn't though, not in the form that you were talking about. Even just changing the form of the test is going to cost.

KEVIN SCHAEFER: Actually the main system specs are in the same format as ours, and the tests are there. But whether the specs are for microgravity or man/system is really immaterial, the real cost impacts would be at the lower levels. For microgravity, the cost effect is in going from man/system vibration level to microgravity vibration level. That's the real cost impact.

MIKE HORKACHUCK: Each is a cost impact.

KEVIN SCHAEFER: I'm not convinced of that either. There are a lot of things that we can do that are very simple, yet which can reduce the magnitude of the problem. Just specifying our requirements is a help. Just educating the designers on what these mean also helps. It is going to be really complex, and as Phil and I have been saying, it is going to be uphill all of the way. It will be hard, but I think we can do it.

MIKE HORKACHUCK: You definitely have to get a format that the designers are going to be able to understand, because right now I can tell you that they don't.

PHIL BOGERT: We will try to get it right this time. We had planned to have this change request in by April. We are holding off a couple of weeks to get everybody's inputs.

BJARNI TRYGGVASON: This is a question for Phil. Have you dealt with the high frequency part of the spec? My comment this morning is that you are never going to achieve that high frequency part of the spec above about 100 Hz, and to try to impose that on any designer of any equipment is just totally unrealistic.

PHIL BOGERT: We haven't discussed that this afternoon, but to understand it better, I had a little discussion this morning, which had to do with the absurd deflections you would have to limit yourself to in order to meet that. Maybe we should discuss that a little bit.

As just one closing comment for Mike, if the cost impacts are going to be huge across the whole station, we are probably not going to have the money to do this. That is -- the impetus for all of us working together as a unit, maybe through some of our microgravity working group sessions and sessions like these, is that we need to can find a good integrated way to do it.

DAMON SMITH: From Lockheed's point of view as a contractor, really all that we can do is look at the forces that we exert, as for a washing machine or a rotating piece of equipment. We really can't look at requirements in terms of the total system, because nobody knows the effect of each of these components, and especially the summation of all of the various equipment. What you really need to do for the contractors is to specify unambiguous, clear and reasonable requirements of force that we can design to with reasonable confidence, and test. Part of the reason that things cost so much is that there's such ambiguity in the specs that you don't know how far to go in meeting the requirements. So that's what would really help us.

STEVE DEL BASSO: Can I respond to that one? We have talked about the requirements here, and the other aspect that Phil mentioned is the allocation process, and we are aware of that problem. The thought was that in the big simulation model, you are getting first-cut forcing functions for the centrifuge and for all of these other things, and they are going to build up in the overall broad-band curve. We are going to see their contribution in a particular band; then, based on some weighting function that we might develop, say that the centrifuge is eating up 90 percent of this particular band on an average and that there's a little work to be done, once you see the whole picture in front of you, though. It isn't clear, step by step, at this point, but you would come back and say that the input force you gave me needs to be modified by 0.9.

DAMON SMITH: That has to be done before we have moved downstream working with the builder, because then it costs ten times more to fix it.

STEVE DEL BASSO: That's right.

PHIL BOGERT: I think we are done on this point at any rate, because that is exactly what we intend to do -- have your work packages give you the forces to meet. I also keep hearing a message that we have to do it sooner, or it is going to cost more. I guess it is up to Kevin and me to convince our management of that, and we will give that the old college try.

N. RAMACHANDRAN: We will now shift gears and talk a little bit about analytical modeling and its role in vibration isolation. What do you think can be done to improve analytical modeling to support experiments? Before we start off on this, Professor Koster has a few words to say on his experiment and the fluids modeling that goes with it.

JEAN KOSTER: I was asked to give a quick presentation of a microgravity science experiment. The project deals with multi-layer fluid physics, and some background on this project is that this is essentially the encapsulated flow zone, where we have a liquid layer contained with an encapsulant.

Now, as a fluid mechanician, I made an ideal case of this, and proposed a box filled with three layers of liquid. One layer should represent the liquid metal, the second layer represents encapsulants, and then a third layer, the outside layer, would be a gas. When I proposed this experiment and we discussed the design with Dornier, they said not to use a gas because of the trouble keeping these flat interfaces in low gravity environment because of the poor gravity environment. As you know, a flat interface isn't a high energy interface, because a minimum energy or the low energy interface is a spherical droplet, essentially; so any flat interface, if it gets disturbed, will try to change into droplets.
From watching the space shuttle videos, you all know that astronauts like to play with spherical droplets of orange juice and that is essentially what could happen in this experiment.

So, because of this acceleration requirement, I defined the experiment as using three liquid layers. We keep the layer interface flat in this case to be on the safer side, but still we have a problem in that we have a relatively large layer. The official, or latest design of the test cell, has 50 by 50 centimeters area of interface, and each area is one centimeter high. So we are talking about relatively large areas. For the three liquids that we have defined for the space experiment, the first layer is FC75; then we have ethyl glycol; and then the third layer is hexadecane, since we need three immiscible liquids. Now all these three liquids have a different density, and consider what happens if we impose an acceleration to such a system. Heavy liquids like FC75 will tend to go with the gravity in the direction where the gravity acceleration points. So if we have a g-jitter acceleration or spike in this direction, that's fine, and the FC75 stays where it is. But if we have an acceleration level or force in the opposite direction, the FC75 wants to invert the whole system and get to the upper part of the cell, and the hexadecane liquid wants to go to this side.

If we have a perturbation in any of these horizontal directions parallel to the interface, we have a similar problem, because the FC75 will be accelerated in one direction to one end of the system, and the heavier FC75 will push the lighter liquid to the other side. So we have a destabilizing effect in any kind of direction from a spike, or whatever perturbation we get into such a system.

So one of the major concerns that we have right now is, if possible, to keep these liquids relatively flat, or what do we get out of it? We are trying to get a solution to this problem with a few KC135 flights. I don't know the latest results from those that were flown only a few weeks ago, but what we generally see is that these interfaces tend to not stay flat. On the KC135, we have real problems. Although we don't have a very good low gravity environment there, this is one of the cases in which spikes could eventually disturb or terminate the experiment.

Also keeping the FC75 fixed to the sidewall is crucial, because of the coatings that we have to put on the side walls as the wetting coating. If FC75 floats over this coating, it dissolves most of the coatings. Then what we get is one big droplet inside this liquid, and we would bring hexadecane in contact with FC75 -- since both are not immiscible. Your whole experiment will be lost in this case.

We know that FC75 and ethyl glycol are about 100 percent immiscible, as are ethyl glycol and hexadecane, but FC75 and hexadecane are miscible. So this is just as an example of a case where it is important to know the time series, essentially the time domain, of an environment.

And due to the size of these interfaces, the magnitude of the spikes that are required to perturb the interfaces are not too high. Again, the ideal experiment would be to replace hexadecane by a gas, to mix a connection to the real problem, where we have essentially only two layers, a liquid metal and a gas encapsulant. But the recommendation from Dornier is that we don't use a gas, because that would make the experiment impossible.

N. RAMACHANDRAN: Do you want to talk just a little bit about the numerical modeling effort.

JEAN KOSTER: Yes, a colleague is doing some numerical modeling of our system. We are not yet at the point where we calculate the g-jitter environment, but essentially we are trying to get an idea about the stability of these interfaces in the ideal cases that can be handled by a numerical modeling for a zero g environment and any kind of mechanical perturbation to the cycle. We then essentially, will shake out the whole test out numerically and see what will happen to these interfaces.

Numerical modeling is certainly important in these cases to get a feeling of what could happen, although the numerical modeling is never the same as an experiment. With an experiment we have more difficulty, I would say, because of the real wetting angles that we get at the sidewalls, the meniscus may occur, and all these other little details, which complicates a system even more.

But there are strong requirements for some numerical guidance for such a system. And such a numerical modeling essentially could predict eventually some magnitude of the amplitudes of spikes that will hit such a system.

What we also would like to propose to ESA or to Dornier, is to build a laser diode and put it above this test cell, to measure the interface and a reflection, which we will measure with a ray diode. Thus, we would get an idea of how the interface will respond to any gravity level. I requested that NASA install one of the SAMS heads on the BDPU, which is the facility where this experiment would be performed, so that we get a correlation between the deflection of these interfaces, or one of the interfaces, and some of the data that we get from the SAMS site.

N. RAMACHANDRAN: I think a basic observation is that from the results you have seen, none of the numerical analysis cases has been validated. We have yet to take an experiment that is well categorized and measured, and for which the time history is known completely, and then to correlate a numerical modeling with that. That is yet lacking, and we hope that the recent STS-32 mission will provide some data that we can take to numerically model an experiment to help us with this validation phase.

BJARNI TRYGGVASON: I think one can only agree that the numerical modeling is essential, not just because it adds a lot more insight, but also it should help refine our experiments so the cost is spent more wisely. We have in fact done experiments, KC135 related and T33 based, wherein we have tracked bubble motion in a fluid, and at the same time developed a computer simulation. This was done at UBC, where you numerically solve for the motion of the bubble, and this includes the effects of the departures from ideal free-fall that the aircraft has. We have actually been able to produce numerically the random sort of track that the bubble would take due to the g-jitters of the aircraft. So there is work going on like that and it can be done, and a lot more of it should still be done. In fact, the KC135 does provide a very nice place to actually verify this kind of a code.

N. RAMACHANDRAN: That's encouraging to hear, and of course every experiment is different. That is, if bubble migration happens in four seconds, you can do it in the KC-135. But for other experiments that take longer, like protein crystal growth, they like two days, and 30 days is even better. Of course those experiments that will take longer have to wait for a future space station module or something like that; but I think validation and numerical modeling for any experiment would be helpful in designing those better, as you pointed out.

To be aware of certain pitfalls, do you require a passive vibration isolation system or an active one? These questions have to be answered in the near future, and I think your numerical simulation will offer guidelines to that effect.

ANDREAS VON FLOTOW: I have heard there is a fluid slosh experiment coming up this fall, which is supposed to be launched in October onboard the mid-deck; I think small containers of silicon, oil, and water are being shaken and the fluid response is being measured. There is a close numerical modeling effort going along with that. There is a free surface on that fluid, so there is a free surface tension and contact angle hysteresis that is important there, but there is also a numerical effort going along with that; that's been the topic of two MIT Ph.D.'s already. There have also been two or three different sequences of KC135 flights. I would just like to point this out for those people who want to try to compare numerical results with experimental results from the KC135 milli-g and then potentially this fall in October, if the thing flies on schedule, mid-deck milli-g.

N. RAMACHANDRAN: Is there anything else anybody would like to contribute? I would like to move on to something else if everybody is in agreement on this.

ANDREAS VON FLOTOW: One topic I would like to see addressed is umbilicals -- I would like to see a wish list of umbilicals; the guys who think they are going to do micro-g isolation with mounts would really love to see that, so they could start scratching their heads about how to isolate in the presence of all the umbilicals their experiments might want. And that wish list should be specified in terms of power and cooling and vacuum. Then we can let the technologists figure out how to provide that level of power, that level of cooling, and that level of vacuum.

KEVIN SCHAEFFER: In the space station it might be suitable to have a passive or even active isolation on the racks themselves, and that might help; yet we do have to carry power and gases and cooling fluids across the interface somehow. Those umbilicals are somewhat hard and so are ideal transmission paths for vibration; Phil told me just now that the Navy has some means of handling that.

PHIL BOGERT: Apparently, the Navy has some ways of running power busses right through the isolators themselves. My particular concern, because we have already seen how we might pass power across gaps and various things about some of these techniques of isolation, is about the big crystal growth furnaces. I am always told that these are the experiments that we can't really isolate. But before we agree to that, I think we want to be a little more creative. Maybe that's what you were getting at.

ANDREAS VON FLOTOW: That's exactly what I am getting at. Before you say it can't be done, tell this to the guys who are thinking about doing isolation. They have almost universally been ignoring umbilicals and just saying "use something like magnetic mounts." Well what about umbilicals? "I will think about that in two years." I don't think that's fair, and I think they should be thinking about it now. And they really can't think about it now unless they know what kind of umbilicals are required.

If you say it's a vacuum pipe, you can't assume that because it's a vacuum pipe it's going to be made of quarter-inch steel -- that's a mistake. The guy who designs isolation will say he can provide that vacuum pipe and will make it appropriately soft, and maybe he'll wrap an active feedback loop around it to make it actively soft.

But there is complete confusion among the isolation crowd as to what our real umbilical needs are for experiments. I think it's appropriate to specify our needs in terms of power, cooling, and vacuum, and further to specify these in terms like how many gallons per minute of such-and-such a fluid flowing through a hose needs to be designed for by them. If it's some comparable thing, like some vacuum pump sucking on some diameter or moving so many molecules per second, then if the designer can do it with a soft hose, great, let him do it with a soft hose.

PHIL BOGERT: A good place to start would be to define those requirements for the crystal growth furnaces and to get some of the good isolators out in this audience to start working on those problems.

DAMON SMITH: Sometimes you go to the scientific community and ask them what they would like for some experiment. and of course they want everything. And you are sorry you asked. The other way to approach this problem is to first see what only costs $10 million to do, in terms of a utility umbilicals, and what costs $100 million to do; then you go to the scientist and tell him what you have. Since that is what the space station can afford, he has to modify or adapt his experiments. If you only ask each of them what they want, costs go through the roof. I think it's much better to find out what a cost versus tube stiffness curve looks like, and then to stay down where it's realistic and approach it that way, starting out with what is possible.

KEVIN SCHAEFFER: You hit on a really good point. I personally don't know the cost to do isolation in terms of simplest isolation, or what it costs for the most advanced state-of-the-art active isolation. That's one thing we need to know in the station community, what costs are involved to do things from the most simple way to the most complex.

ANDREAS VON FLOTOW: Well, I don't know a lot of numbers, but I can tell you a couple. I found out yesterday that Wales developed as far as they have gotten on about $600,000. Now, that is not ready for flight. Then I found out today that Sperry developed their FEMA, which has no umbilicals but just magnetic fields going across the gap, for 350,000 IRAD dollars five years ago. They claim this is ready for flight, but since it has no umbilicals it is not ready for any useful flight that might include umbilicals; but they say it is flight hardware. I don't know what the flight version is going to cost on top of that $350,000, nor do I think they would be willing to sell it for $350,000. They have been trying to sell it, and have all kinds of overhead costs tacked on to that, as well as interest costs for five years. If you want to start adding things together, it's not a big price for what they built.

KEVIN SCHAEFFER: I am thinking of an even simpler approach than that from an overall system point of view. The past few days, a lot of people here have been thinking only of isolating the payloads, but I have to think of a broader view. I have to think of how could to isolate vibration sources and how to isolate vibration transmission paths, because those are options that are available to me. And when I ask how much options cost, I mean that when you have an interface between two large pieces of hardware, what kind of things can you do at that interface to lower the transmissivity, and how much does each of those options cost? I am not necessarily talking only about high technology, state-of-the-art, active vibration isolators. I am also thinking of what it costs to put a rubber hinge on it, or what it costs to do this or do that. Those are the things that the station needs to know, as well as the state-of-the-art in terms of isolation technology and its costs.

ANDREAS VON FLOTOW: I think you will isolate the worst source in a fancy way, and the not-so-bad sources with rubber footpads and stuff. We should have one price tag in the room here today. The worst source was the treadmill or the ergometer, and since we have the guy who knows the price for what that is going to cost to isolate sitting over here, we can ask him what it is going to cost.

DAMON SMITH: You have to remember this is a DSO experiment; I am not going to respond to that anyway.

KEVIN SCHAEFFER: Anyway, that's a classic example of the type of things I am thinking of. Here you have a potentially very disturbing source, yet you see it isolated to several orders of magnitude lower than what everyone thought was possible two months ago or six months ago. But that's not the only disturbance source. There are sources all over the place, and we have to consider those in the design. We have to think of isolating the sources and the transmission paths, as well as the experiments. That's the kind of thing we need most from groups like this, basic technology; because, quite honestly, a lot of people in the station program don't know this technology. This is a very specialized field, and not too many people know about it.

N. RAMACHANDRAN: Let's shift gears again and talk a little about data reduction. Charles has something to say.

CHARLES BAUGHER: Let me tell you what we are doing on data reduction right now and see if you have any suggestions. We are starting to figure out how to wrap our arms around that problem. These are the missions that we have in front of us on spacelab (fig. 12), starting on the 22nd of May with SLS, and here are the spacelab missions, the materials processing missions, and the microgravity missions aiming toward the space station payloads. We have SAMS on all of these.

We are using a kind of a two-prong approach; Melissa talked about one this morning. Melissa is working primarily on an IML mission, which really is the first of this microgravity series, is a life- size mission, and a mid-deck mission. IML is pathfinding for the rest of the spacelab missions and Melissa is working tightly with other investigators trying to find out what they want.

Now, in the event that they don't know what they want, we are trying to work another option. Starting all the way back at STS 32, we got our hands on the data from the Honeywell instrument and right now, from STS 32, we have other data coming in on SLS. We are going to go ahead and process that data in the way we think the investigators want it to be processed; then we will send it out to them for comments. Our approach right now is to try to pass through the whole mission and give the frequency data in time, frequency and amplitude format. That is frequency domain data as a function of time as shown in this color slide (fig. 13).

Then for the time domain, we will do a bunch of line plots for the time domain data of things that we think might be of interest, things like RMS value, the acceleration alone or integrated to get the velocity, or integrated twice to get the displacement. We will put those plots out also to give the time domain story. The biggest problem in all this really is that sometimes, especially in the time domain, is getting the data under control so that you can do it. so that's where we are now.

But anyway, the two-prong approach is one that Melissa and IML are working with investigators simultaneously. And we will take the real mission data we get from these early missions, put out example reports, and show these to investigators for comments.

Finally, we have those investigators who are interested in something specific that nobody else is interested in. And our scheme -- I wouldn't call it a plan yet -- is to work with those investigators in terms of using a commercial data analysis package, of which there are several around. We can take the accelerometer data and process it. The commercial package that we have identified now as the probable front runner in the group is something called DA DISP. It runs on the IBM AT, and to use it you have to throw in a couple of megabytes of memory. But it has a big long menu of things you can do to the data, and so we will try it. Our current scheme is to get copies of that output to the investigators, supply them, after the flight, with a small amount of sample data during the runs, and then let the investigators figure out precisely what they want to do. Then we will try to do this across the entire data, or across the number of hours they want, or something else. So that's basically the way we are approaching the idea of working with investigators and their data.

We are trying to get all this together, so that we can put out a very quick, early report on the missions to the investigators; then they can look at things like our line plots to determine if they need to look at the data any further in order to interpret their experiment. Presumably, if some investigator noted an anomaly in his experiment, he would decide to go look at that more closely, or he might expect anomalies because his experiment results are questionable. At the same time, if the environment looks nominal, he might conclude that he really doesn't need to look at the environmental data any further. Those are really our objectives for these first early reports. Then, after they have identified areas where they want to look further, I think a lot of investigators will probably want to do that looking on their own. But if they don't, we will try to work with them.

MELISSA ROGERS: I think one of the important things to stress, which we were discussing out in the hall earlier, is that this whole data analysis process has to become an iterative process. We have to analyze the numerical analysis that has been done to date and then, as several people have mentioned already, to compare that to experimental results and to the analysis of the experimental results in conjunction with the residual acceleration data. Then we can go back and revise things such as sensitivity limits, tolerances, and the requirement-type curves that we have been talking about today and this week. I think that eventually we will be able to isolate what the quiet orbital periods are and, as Charles just said, investigators then won't have to bother looking at these vast periods of data where their experiments seem to have come up with the correct results or workable results -- nice crystals or whatever the goal was.

ANDREAS VON FLOTOW: I'll bring up a new topic. We talked earlier here about the specs and it seems to me that the specs, whether they are time domain or frequency domain, have to do with the motion. I really don't want to get back into that argument, but they have to do with the motion of the rack, I think. I don't know who enforces those specs. Is somebody building these payload modules? I am talking about the specs and where in the process and in what space they are applied physically. That's what I'm trying to bring up as a topic. I don't understand the bureaucratic mechanism and how it fits together with contracts between people and the companies, how you apply them and where in the space station you apply them?

KEVIN SCHAEFER: As to the specifications, I assume that you are referring to the broad-band and narrow-band vibration specs and all that stuff we have talked about.

ANDREAS VON FLOTOW: I really don't want to get into time versus frequency.

KEVIN SCHAEFER: The requirements apply at the space station payload interface, which is defined as the physical interface points, the physical attachment points or the fluid interface planes where cords and lines from the payload rack meet the cords and lines from the space station. It also applies to the volume of the rack, itself. For vibration, this primarily refers to acoustically-induced vibration, so that is the volume of space and the points at which these requirements apply. They apply to half the racks in each pressurized module.

ANDREAS VON FLOTOW: Whose problem are they? If you say they are the problem of company X, could company X come along with a solution saying that they are going to meet your requirements by isolating those racks, or are the requirements somehow explicit?

KEVIN SCHAEFER: Here you get into complexities. Because the requirement is essentially the sum total of all sources on the station as they are transmitted through the structure and reach these interface planes or points that I talked about, you cannot point to any one work package or partner, or even another payload, and say, that is the problem.

That's why we are attacking this from an allocation viewpoint. We are taking the total requirement and dividing it up into the contributions from each of the work packages. Programmatically, we feel that is the only way to divide up the total environment among all the different political entities within the station program.

Now, within each political entity, work package, or partner or payload group, you will have different systems and elements, and subelements and subsystems. These will use the same techniques we use to come up with the total allocation, and will take their suballocation and subdivide this among their systems. This process goes down and down until you finally reach the specific box or component, and then it will stop.

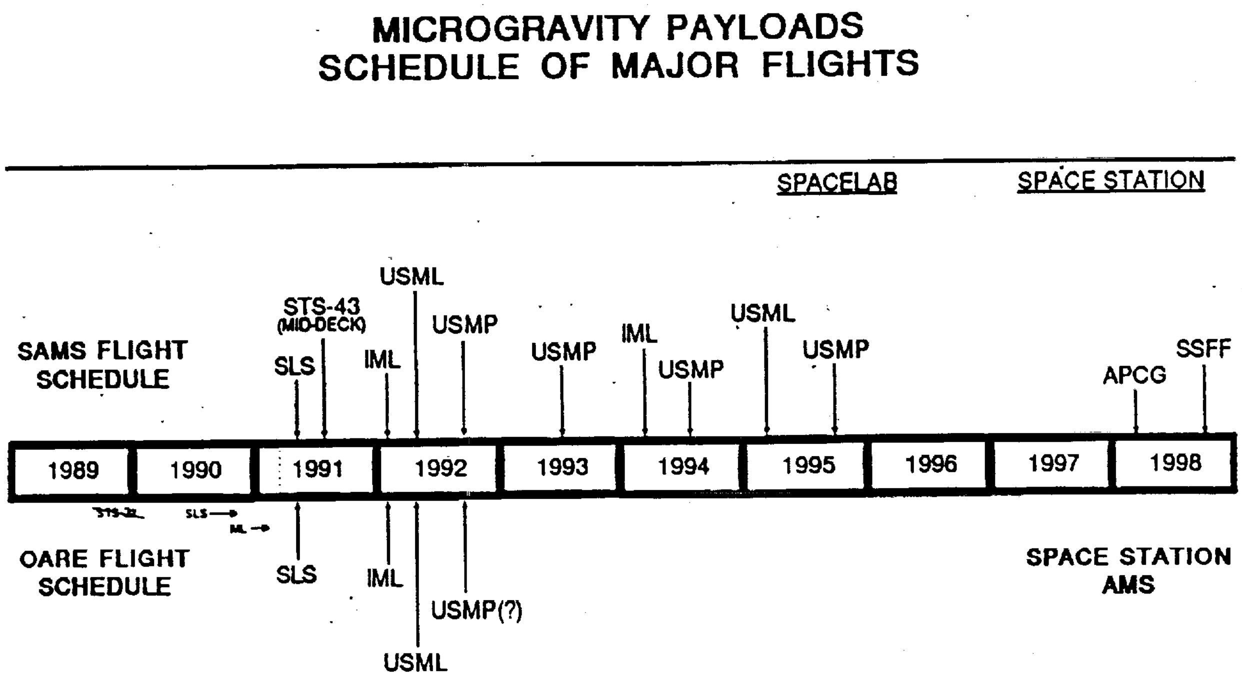


FIGURE 12

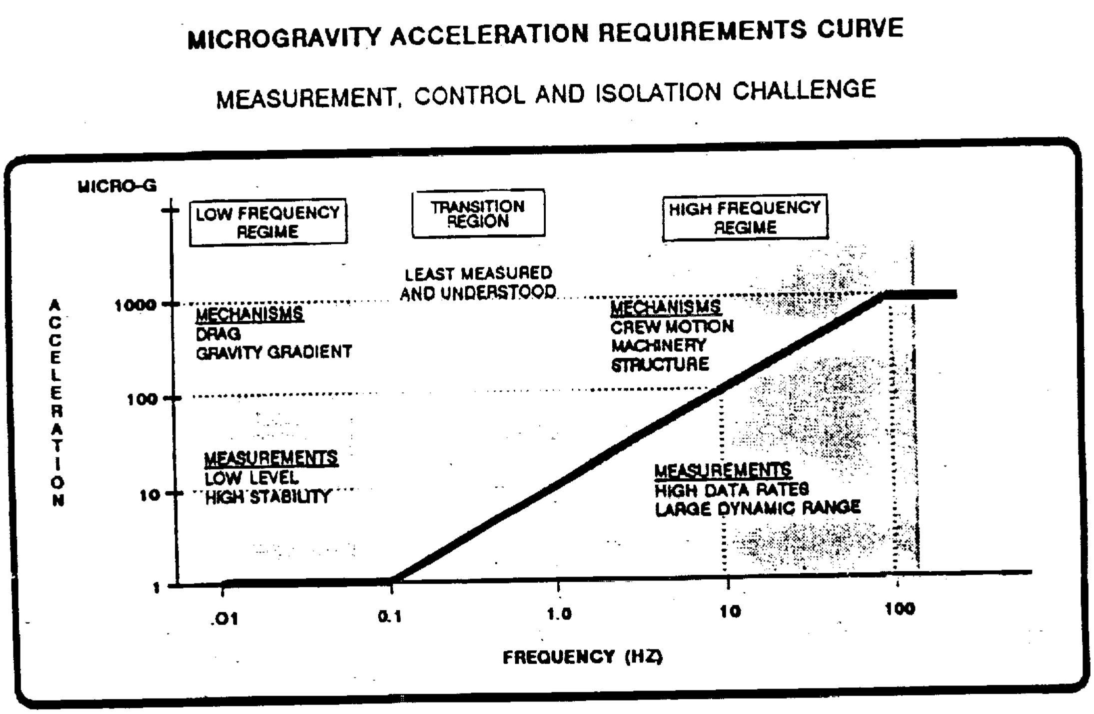


FIGURE 13

ANDREAS VON FLOTOW: If you keep up with that approach, won't you will completely ignore the possibility of isolation at the payload, the micro-g amounts?

KEVIN SCHAEFER: No, what we are going to do will help that. The allocations are something that you can assign to the individual work packages and then you can work that down to define specifications for individual pieces of equipment or boxes or whatever: things that Damon and other people have talked about.

That's where we, as level two systems engineers, start looking at isolations of transmission paths, and where we dictate isolation of specific big sources of vibration. So isolation at the payload rack or at the rack interface is something that we can dictate that the payload community can do. But a lot of the isolation, especially a transmission path, is going to fall out as the responsibility of level two, as an integration function.

ANDREAS VON FLOTOW: Yesterday there were solutions proposed. Whether these are believable or not is another issue, but there were solutions proposed for doing all of the isolation, including the treadmill isolation at the payload interface, at the micro-g payload interface and not at the treadmill. So you, as system engineers, could believe that and put Damon Smith out of business by saying we don't need that at all, that we are going to do it all at the interface.

KEVIN SCHAEFER: You can believe that, but the station community has signed up to provide that kind of environment at the interface point -- at the racks. If we say the payloads have to do it all, that means the station is not having microgravity at all, it is all payload. Also I don't think that is quite realistic, because we can't expect the payloads to isolate everything. Doing it that way would reduce the payload volume down to just about nothing, and what the station is trying to do is provide a facility to do research. If we put all of the requirements on the payloads to provide the environment, then we are not doing our job.

ANDREAS VON FLOTOW: Are you proposing to let this workshop at all influence the way you distribute the isolation. This workshop has emphasized mostly isolating at the payload, and a very little bit of isolation of the source.

KEVIN SCHAEFER: That's right.

ANDREAS VON FLOTOW: Unless you change your specs, the way you impose your specs, the isolation at the payload interface will become completely unnecessary, because the payload interface will be quiet enough already. So all the guys who are developing payload isolation techniques might as well quit and go home, because there will be no need for them.

KEVIN SCHAEFER: I'm not saying that either.

ANDREAS VON FLOTOW: So we split the job some, right, and do some at the payload interface and some at the treadmill.

KEVIN SCHAEFER: That's what we will probably end up doing.

ANDREAS VON FLOTOW: That will change the spec. to allow more interface motion than the current spec, by factors of 10 or 100, or something like that.

KEVIN SCHAEFER: We in level two do not want to relax the requirements right now in any way. If we relax them now, let's say we relax them at the low frequency range from 10^5, that means they will come in at 10^4. And so, no matter where you lower it, they won't meet it. That's the basic philosophy.

ANDREAS VON FLOTOW: So you are holding isolation in your back pocket as your Band Aid to what applies in the future.

KEVIN SCHAEFER: A Band Aid to be applied in case of cuts.

PHIL BOGERT: I think we will probably use some combination of the two really. I mentioned that this morning that we do want to look at isolation of sources and racks. I think it is going to be difficult to meet this environment and that it is going to take a really creative effort to arrive at what is most cost effective.

ANDREAS VON FLOTOW: I would like to emphasize that it seems to me that the best way to ensure that both techniques get used is to rewrite the specs so that people are motivated to use both techniques. The way the specs are written now, people are completely unmotivated to use payload isolation.

PHIL BOGERT: I think I mentioned this morning that we ought to put a sentence or two in the specs that opens up the possibility of the isolation being done at the payload as well.

KEVIN SCHAEFER: Yes, I agree that we have agreed to supply this environment. But saying in the specs that the payloads have to isolate themselves is saying that we are not going to meet the environment spec, and though I know it is a difficult thing to deal with, we are going to have to. I know it is going to be extremely hard to do this, but I think it is all very possible.

MIKE HORKACHUCK: I guess I was under the impression that the space station was providing OSSA with the racks, so the real interface with the user community is internal to that rack. You built an isolation system in that rack and that's where your interface with the user really is. It is going to cut up some bond and we may need to provide more racks to equal out the total volume, but I think the users may actually be better off, because they can guarantee their environment then. They are not relying on the space station, who may be providing it.

GARY MARTIN: I want to comment on the ability to put isolation on racks. I believe that currently microgravity sciences is going to be allotted something like seven racks. If we are going to isolate within the racks, we can probably just about cut that useful space in half. You saw what the MGIM did; they took that huge rack and were left with that little square in the middle.

Then there's what we talked about the other night, where we had the problem with the furnaces, which is really looking at these low frequencies. You are going to need long strokes, plus you have the problem of getting the coolant across the gap, so it is nontrivial on the payload side also. If you are really going to do isolation at the racks, we have lost half of our capability on the station. Plus there is also the power head.

KEVIN SCHAEFER: As one last point, it's true that the racks are going to be purchased from the space station, but this is a top-level programmatic decision. The rack itself is considered to be part of the payload, and the reason for that is because once the payload community takes that rack and starts building payloads within it, the station completely loses control over it until it is back into the station. So, in terms of our designing equipment, we have to design things that are within the control of the station program, and the rack itself is not in that control.

JOE LUBOMSKI: Kevin, I would like to make a statement here. I think you were talking before about having isolation at the source or at the payload. We discussed this in detail in 1988. What we pretty much came up with then was the strongest recommendation that we define the space environment and then control the sources whenever possible. If you can do this, you will solve a majority of the problems. You won't solve them all, I'll grant you that. But you will solve a lot of problems and also save a lot of money in the process. Now that's basically what you are trying to do, and we endorse that. We are not putting any isolation equipment on the payloads because that costs us money and resources. If we can do it by source isolation, we will solve more than half of our problems. I think we all agree with that.

PHIL BOGERT: It will probably take a systems engineering approach and I think, Kevin, that when we see the allocation, we will have an idea of where we stand, and that we are absolutely going to have to do what is most cost effective. There are mounts on the payloads, mounts on the sources, simple rubber-type mounts, and there are fancy active state-of-the-art isolation systems. We are going to look at combinations and permutations that give us the most bang for the buck. Otherwise, there's no way the program is going to buy it. They have already told me that. So, I think we will take a systems approach. But your point, Professor, is well-taken -- that we shouldn't limit ourselves by our spec. to demotivating the whole technical community.

GARY MARTIN: Phil, I would like to say one more thing. When you say you are going to see what is most cost effective, are you talking about cost effective to NASA as a whole?

PHIL BOGERT: That's right. I'm not sure how the money flows exactly.

DAMON SMITH: One of the reasons that the space station costs so much money is that equipment is being designed that bears the entire burden of isolating the vibration, while the payloads have no isolation built into them. By the way, there are people who believe that Naumann graph is the graph that they must totally satisfy by themselves; they don't realize that this is the summation of the entire thing: another misconception, but really a big problem. But what if you just put little washers like we were saying here. If you could reduce the needed isolation on the payload by a factor of ten, you might come way down the cost curve on isolation of this big heavy reciprocating equipment, because it is exponential.

When you push the state-of-the-art, it costs millions and millions of dollars to have the sources try to bear the whole burden. I'd bet you anything that if you will distribute the burden using passive systems, the total cost may be much lower. You don't have to lose a lot of volume to put washers in where right now you have hard mounts on the payload.

GARY MARTIN: I do agree and I don't think it is all on the station's part. We want the best environment that we can get to do our experiments in. However, if you notice in almost all of our requirements, we have a very sensitive part, the knee of the curve, at 0.1 Hz. In fact, the lower frequencies -- if that's where we really need the isolation -- are not solved by a washer.

DAMON SMITH: Yeah, you are right. That's true. I don't know how to deal with that, frankly.

JEAN KOSTER: I want to emphasize also another point, and this is of interest to Gary. If we get a grant or a contract to fly an experiment on the shuttle, that usually results in a very expensive experiment. So if the experiment fails, a lot of money is lost, and we do not have a good chance to refly in the same year, but maybe five years later, or something like that. So small improvements, in vibration environment for example, for payloads would probably also pay off in that sense. I think that is a very important issue also to consider. So is providing all the payloads or racks with some kind of good environment probably a good recommendation. For some specific experiments, that one facility would get a little bit better environment, but that should be only the one rack that is essentially designed for very high quality low gravity environments. Every individual rack should have such quality. I do think we have also to consider the total lifetime of a space experiment, which may be five to ten years. And if it fails, a lot of money is lost.

CHARLES BAUGHER: I guess the final point that I would add is that there are classic experiments that go beyond that curve and will need isolation just to do their thing, even though that curve is met. Pragmatically I think this isolation is going to be done on both sides of that boundary.

BJÅRNI TRYGGVASON: I want to revisit my comment that I made a couple of times before, and that is on the high frequency end of the spec. I think that it would be a bad mistake if no comment comes out of this group to address that, which is an impossible part of the specification. It is going to be an impossible thing to ask your designers of the equipment to even try to meet. You really should consider replacing the acceleration term by an amplitude term, which is basically just continuing the line, increasing the frequency access.

KEVIN SCHAEFER: I'm not sure that everyone quite understands the problem -- Gary can correct me if I'm wrong -- the upper frequency under that curve seems a little bit arbitrary -- Gary is nodding.

BJARNI TRYGGVASON: I think the origin of the upper end of the curve is totally immaterial to the comment that I'm making. But whether it is arbitrary or whatever, it is an impossible part of the specification. It simply cannot be met, because you are looking at nanometers of motion to try to meet that.

CHARLES BAUGHER: Why don't we project a fix, and we will go back and talk to the investigators who are interested in that part. The fix I would suggest is just extending that curve on up until it intersects the acoustic curve. Then we will go back and make that as a recommendation; we are not smart enough to know for sure if that is right until we go back and talk to some of the investigators.

GARY MARTIN: I want to second that. We do have investigators who are interested in those upper frequencies, which mess up their detectors. We have at least one, Bob Gammon, who is flying an experiment on the USMP, and who is really worried about these upper frequencies. We need to look at it to see if it is not physical.

CHARLES BAUGHER: We need to come back with a proposal to merge that to the acoustic spec, and one possibility is running it up and intersecting? Is that reasonable? I guess that's the thing that I'm going to work with.

KEVIN SCHAEFER: When you say run it out the acoustic branch, are you saying make it somehow meet the man/system vibration spec? Is that what you are talking about?

CHARLES BAUGHER: Well, does that make sense?

KEVIN SCHAEFER: The man/system specs have several curves. I don't know if you have seen them, but they are curves as a function of duration; and when you say met man/system specs, that's not very crisp.

CHARLES BAUGHER: I don't know that I can be very crisp standing here with a microphone. I guess we will have to go think about it.

BJARNI TRYGGVASON: I think the point has been made that this end of the specification should be looked at. When you get somebody to really look at it, and it is really level two who should do that, you will realize that the amplitude of motion is so small that you are starting to look at molecular vibrations.

DAMON SMITH: With the temperature.

BJARNI TRYGGVASON: We would have to bring the station down to zero degrees temperature to meet the spec. So I think when you look at that, the thing that makes sense is that the line just continue to extend upwards. It will, at some point, intersect what the acoustics are going to do with walls of the space station. At that point you will probably stop drawing the line, or something.

DENNIS KERN: I would just like to comment on the spec above 100 Hz. Even though the displacement is low, the vibration at higher frequencies does attenuate quite rapidly with frequency. What you have to worry about are the sources near your instruments, thus you have fewer things to worry about. Secondly, there are a lot more methods of attenuating the vibration environment at higher frequencies, specifically damping. So, although it will be difficult, I'm not sure it is any more difficult to meet the higher frequency spec than to meet the lower frequency spec.

PHIL BOGERT: Does everybody understand the nature of this problem? If anybody doesn't, raise their hand.

CHARLES BAUGHER: What does our spec say right now?

GARY MARTIN: If you look at the equation part of the spec, it is open-ended; it says greater than. I know it by heart.

CHARLES BAUGHER: Okay. So it is open-ended.

PHIL BOGERT: But if we look at, for example, a frequency of 1000 Hz, we have 10^{-3} micro-g's. So in a rough amplitude sense of D equals A over omega squared, you get 10^{-9}. You got 10^{-3} squared into the denominator and 10^{-3} up top, and you end up getting an absurdly small deflection.

GARY MARTIN: But, Phil, as we talked about earlier, that's the spec at the payload rack. What is vibrating at 1000 Hz? What is going to be there unless you are right in the rack? What is going to get there? You can't do that. I mean, it is probably not right to have a spec like that in there, because you can probably write it better and be more correct. But the thing is: in real life, do we really have a problem in that area?

BJARNI TRYGGVASON: I think you will find in real life you are going to have a problem, because you are talking about nanometers of motion at the support point on the racks. Even though you are isolating the rack, you have such a small amplitude of motion allowed on the station side of the rack that it is impossible to meet the spec. You are talking of nanometers of motion.

CHARLES BAUGHER: You are not going to make that measurement. Okay. You are saying of 1,000 Hz. I bet I could measure 10^{-3} g vibration at 1,000 Hz.

KEVIN SCHAEFER: Gary was mentioning that perhaps this is a dead frequency range. We are not saying that in real life it is not a problem, but that the requirement should reflect reality. If it isn't a problem, the requirement should somehow reflect that.

BJARNI TRYGGVASON: I'm sure your accelerometers will measure it, but the point is that it takes such little motions that you will always see something.

CHARLES BAUGHER: I was sitting here picturing a tuning fork with an accelerometer tied on it and striking it and letting it die. 1000 Hz is well within the audio range.

BJARNI TRYGGVASON: I know that's it's not a question of whether your accelerometers will measure it.

CHARLES BAUGHER: But this argument started out with a non-physical spec.

BJARNI TRYGGVASON: What I'm saying is that it is not a realizable spec. Suppose an astronaut is talking at one end of the station or one end of a module to another astronaut. The acoustic noise of his talking, feeding into the structure of the station, will vibrate the other end by more than that spec, because there's such a low amplitude. Especially when you have a hard system, it will transmit through the walls of the station.

GARY MARTIN: To make that even worse, I would think that temperature alone will probably violate that spec, if you carry it out all the way, since it is open-ended. There's probably some need -- I agree with the need.

KEVIN SCHAEFER: If you are talking about temperature, that's much different than what we were talking about before.

GARY MARTIN: If you carry the spec out open-ended, you get to unphysical. We do need to figure out where the end needs to be and what the spec needs to be. That's why I would like to reserve the right to go back to our PI's after an expressed interest in that high frequency area, to understand the problem that they have.

CHARLES BAUGHER: Clearly we talk around our payloads. I think there's room there.

KEVIN SCHAEFER: If it is unrealizable, because of what Bjarni said, then we are asking the designers to do things that are impossible, and you can expect huge cost impacts, and that's exactly the opposite of what we really want.

PHIL BOGERT: Kevin, I don't know if I got this right. I don't think well on my feet. I did this real quick, but it might give us the idea. If D is equal to A over omega squared, if the side of the angle is 10^{-3} Hz we have 10^{-3} micro-g's. So D is A over omega squared and the acceleration allowable is 10^{-3} so that's micro-g's, which 10^{-6} g's micro-g and then to convert (inaudible) and then at the bottom you have got omega squared, which is 10^{3} squared, which is 10^{-6}, that becomes 10^{-6} and you bring it up and string it all together and you get a displacement allowable of 10^{-6} meters. I might have made a mistake there.

BJARNI TRYGGVASON: You have the idea, but you are off by a factor of 10^{6}; it is only 10^{-10}, because it is not 10^{-3} micro-g's. It is 10^{-3} g. It is still small.

DENNIS KERN: Gloria gives an example of a fan in a payload rack and what the response of the rack is to that fan. She can show it again there.

GLORIA BADILLA: Right there is what the input source was. Basically if you took a fan and suspended it inside a rack with bungee cords, then that was what you were getting on the walls. You are still getting measurable sources. I can tell you for a fact that we had an experiment that we were just doing for a planetary camera up in our labs, and we were having to do some high level optics, and we were measuring levels around there.

BJARNI TRYGGVASON: My comment is that it is not a question of whether you can measure it; I guarantee that you can measure it. The reason you can measure it is because a very, very small amplitude gives you a very large deceleration, and that's the point. The trouble is you cannot get rid of it. What you have is a fan suspended by bungee cords in air. The transmission to the wall is the noise of the fan, and that exceeds the spec. An astronaut talking at one end of a space vehicle to another astronaut, is also going to exceed the spec for the far end of the structure.

DENNIS KERN: No, he won't. That will not be as loud as that fan.

GLORIA BADILLA: This is really close; this is a fan suspended inside the rack, right? Your noise source dissipates at $1/R^2$, right? I hope your astronaut is not speaking at such high levels.

KEVIN SCHAEFER: In this area you are talking about acoustically-induced vibration. The only way to deal with that is at the source, and as you said, I'm not sure how to deal with it. It is something that we have got to look at.

DENNIS KERN: The point I was trying to make is that it is possible to attenuate the higher frequencies to get rid of that. I don't consider that a more impossible problem than the low frequencies, though it is definitely a problem. I don't think we should just do away with the requirement, unless we know it is really not a requirement.

CHARLES BAUGHER: I believe that concludes this session.

At this time the session adjourned.

SECTION III

PRESENTATIONS

N92-28437

Numerical Studies of Convective Transport Associated With Crystal Growth in Microgravity

N. Ramachandran
USRA, NASA Marshall Space Flight Center
Huntsville, Alabama

International Workshop on Vibration Isolation Technology
NASA Lewis Research Center
April 23-25, 1991

(LACK OF) SCIENCE REQUIREMENTS

- Experiments Performed in Space Have, in Many Cases, Been Suspected Dependent on Residual Acceleration Environment.
- Diverse Effects of These Accelerations May be Suppressed with Implementation of Vibration Isolation Systems.
- Inadequate Data Describes the Sensitivity of Fluids Experiments to Time Dependent Low Gravity Disturbances
- If Fluid Sensitivity can be Characterized in a Quantitative Way, Isolation Systems Might Be Tuned to Filter Bandwidths Expected to Produce Adverse Effects.

DEVELOPMENT/PLAN

1. Select Classes of Fluids Systems Expected Sensitive to High or Low Frequency Vibration:

- Containerless Liquid Bridges and Float Zones

A) Float Zone and Encapsulated Float Zone Crystal Growth Techniques May Prove Promising for Space Processing

B) Free Surfaces Present in Thermocapillary Fluid Systems May be Susceptible to a Wide Range of Acceleration Magnitudes and Freq.

C) Over 50 Separate Low Gravity Investigations Have Been Initiated to Study Thermocapillary or Liquid Zone Stability Characteristics

DEVELOPMENT/PLAN (CONT.)

2. Review Fluids Experiments Within These Classes Which Have Been Performed In A Reduced G Environment, and Employ Associated Fluid Parameters In Modeling
3. Perform Order of Magnitude Estimates of Fluid Sensitivity As a Function of Acceleration Amplitude and Frequency. (Permits Only Single Disturbance Input)
4. Use Order of Magnitude Estimates as a Preliminary Guide for More Detailed Computational Analyses which Involve Both Single and Multiple Disturbance Inputs
5. Consider Realistic Vibration Isolation Disturbance Filtering Limits and Examine Fluid Response At and Below These Limits
6. Consider Anticipated Spacecraft Environment and Examine Fluid Response To Typical Disturbances (Acceleration Magnitudes and Frequencies)
7. Determine Sensitivity Range of Experiments with Parameters Similar to Those Performed in Space and Determine Benefits of Vibration Isolation.
8. If Possible, Verify Sensitivity Analysis with Experimental Research

The Marshall Space Flight Center Team

Cheryl Winter (NASA)
N. Ramachandran (USRA)
I. Alexander (UAH)

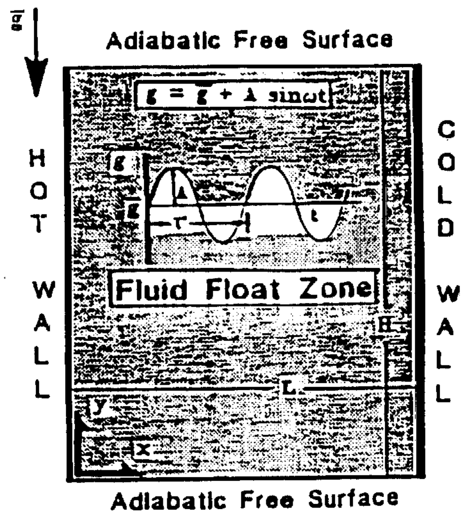
Research Objectives

- Determine Vibration Sensitivity of Selected Fluids and Materials Processing Experiments
- Determine if these Experiments can Benefit from Vibration Isolation Techniques
- Provide Realistic Requirements for Vibration Isolation Technology
- Highly Detailed Modeling of Experiments is Not Performed Sensitivity of the Overall Fluid Response is Sought

  (For Example, While Internal Flow and Thermal Fields are Computed, Interface Growth and Zone Meniscus Change are not Modeled.)

Class of Problems Investigated

- **G-Jitter Convection**
  - **Floating zone and Liquid bridge computations**
  - **Enclosure problems**
  - **Protein Crystal Growth**
- **Surface tension driven convection**
  - **Prandtl number effects**
  - **Aspect ratio effects**
  - **Curvature effects**
- **Encapsulated Crystal Growth Methods**
  - **Two fluid systems**
  - **Marangoni and Interfacial tension effects**
- **Analysis of G-Jitter data - Project ACAP**
  - **Data digitization**
  - **Analysis**
  - **Graphics**
- **Three dimensional computations**
  - **Code development and validation**
  - **Ampoule flows**
  - **Effect of residual acceleration**



$Gr = \frac{g \beta \Delta T L^3}{\nu^2}$; $Pr = \frac{\nu}{\alpha}$; $Ma = \frac{\sigma_T \Delta T L}{\mu \alpha}$

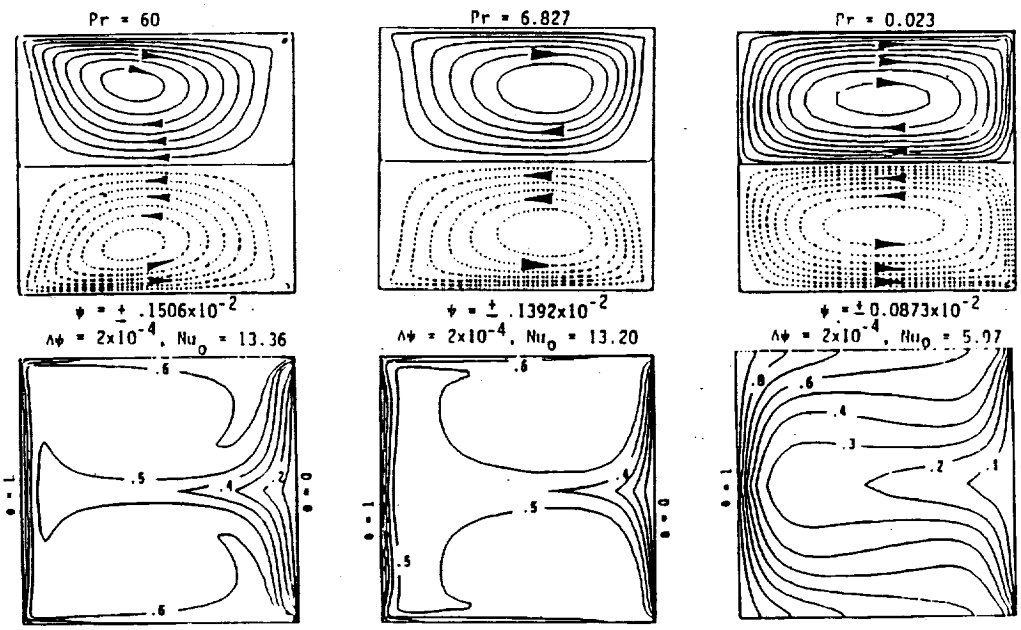
Table 1 (Float Zone parameters and properties)

Fluid	Silicone Oil	Methanol	Silicon Melt
Flight	D-1, 1986 [17]	TEXUS-7, 1984 [18]	Spacelab-1, 1984, [1]
Pr	60	6.827	0.023
L (cm)	6.0	1.0	2.0
ΔT (K)	32	10	50
σ_T (d/cm.K)	- 0.0655	- 0.0773	- 0.43 ← σ_T
μ (gm/cm.s)	4.56×10^{-2}	5.84×10^{-3}	7.5×10^{-3}
ρ (gm/cm^3)	0.91	0.792	2.50
ν (cm^2/s)	5.01×10^{-2}	7.374×10^{-3}	3.0×10^{-3}
α (cm^2/s)	8.40×10^{-4}	1.08×10^{-3}	0.13
β_T (1/K)	1.05×10^{-3}	1.19×10^{-3}	1.43×10^{-4}
Gr (1 g$_o$)	2.66×10^4	2.147×10^5	6.235×10^4
Ma	$- 2.97 \times 10^6$	$- 1.226 \times 10^5$	$- 4.41 \times 10^4$

USRA NASA/MSFC

THE NUMERICAL CODE

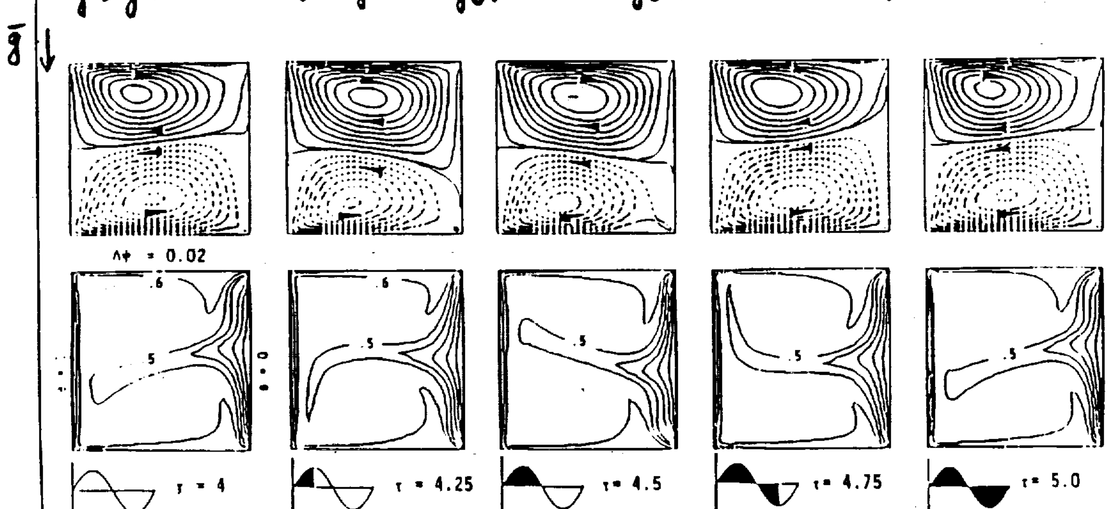
- Primitive Variables
- Implicit Control Volume Finite Difference Method
- SIMPLEC Pressure-Velocity Correction Algorithm
- Body Fitted Co-ordinates
- Central Differencing: Diffusion terms
- Positive Coefficient Skew Upwinding : Convective terms
- Convergence: Sum of residuals (U, V, P, T) < 10^{-4}
- Transient Computations
- Surface tension driven flow



ORGANIZATION: Fluid Dynamics Branch	MARSHALL SPACE FLIGHT CENTER	NAME: N. Ramachandran
CHART NO.: AIAA 28th Sciences Meeting	Effects of G-Jitter and Surface Tension Induced Convection on Float Zones	DATE: January 1990

G- Jitter - Silicone Oil (Pr = 60)

$g = \bar{g} + A \sin \omega t$; $\bar{g} = 10^{-5} g_0$; $A = 10^{-3} g_0$; $\omega = 10^{-2}$ Hz ; $Ma = -10^5$



- Calculations over 5 cycles
- Periodicity after ≃ 2 cycles
- Same effects
- No phase lag in flow; π/2 in Temperature.

ORGANIZATION:	MARSHALL SPACE FLIGHT CENTER	NAME:
Dynamics Branch		N. Ramachandran
CHART NO.:		DATE:
AIAA 28th Sciences Meeting	Effects of G-Jitter and Surface Tension Induced Convection on Float Zones	January 1990

Encapsulated Float Zones

Ma = -1

Table 2 (Cases simulated and inferred results)

Case #	Fluid	$\bar{g}$ (g_o)	A, B (g_o)	ω (Hz.)	$\frac{(Nu_{max}-Nu_o)}{Nu_o}$ %	ΔT_{max} (K)	$\lvert U_{max}\rvert$ cm/sec
1	Silicone Oil	10^{-5}	10^{-3}	10^{-3}	281.65	20.89	78.12
2				10^{-2}	124.83	9.472	30.94
3				10^{-1}	7.75	1.110	13.36
4	1 second Impulse		10^{-5}		2.58	0.569	9.002
5	1 second Impulse		10^{-3}		31.32	5.120	81.48
6	Methanol	10^{-5}	10^{-3}	10^{-3}	41.51	2.71	0.845
7				10^{-2}	11.54	1.29	1.010
8				10^{-1}	1.406	0.043	0.467
9				1	0.149	0.0003	0.083
10	1 second Impulse		10^{-5}		0.229	0.003	0.303
11	1 second Impulse		10^{-3}		3.988	0.0221	2.819
12	Si Melt	10^{-5}	10^{-3}	10^{-3}	1.10	1.725	0.0567
13				10^{-2}	0.35	0.890	0.0352
14				10^{-1}	0.03	0.119	0.0133
15	1 second Impulse		10^{-5}		0.02	0.0625	0.0111
16	1 second Impulse		10^{-3}		0.19	0.5650	0.0300

Ref. Case

$Nu_o = 1.466$ due to Ma + $\bar{g}$

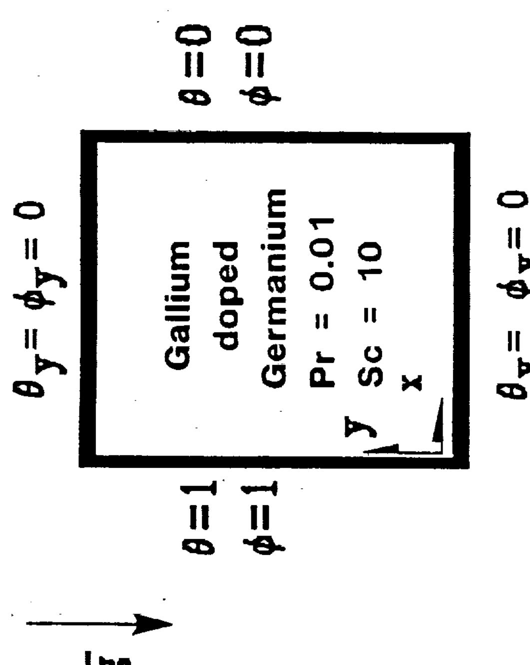
$Nu_o = 1.003$ due to Ma only

$Nu_o = 1.000$ due to Ma only

Frequency: $g = \bar{g} + A\sin\omega t$

Impulse: $g = \bar{g}$ + Impulse for 1 sec.

Combined Thermal & Solutal Convection



$g = \bar{g} + A_1\sin\omega_1 t + A_2\sin\omega_2 t + A_3\delta(t_1 - t_o)$

$\bar{g} = 10^{-5}g_o$; $g_o = 9.81\ m/s^2$

$A_1 = A_2 = A_3 = 10^{-3}g_o$

$\omega_1 = 10^{-3}hz$ $\omega_2 = 10^{-2}hz$

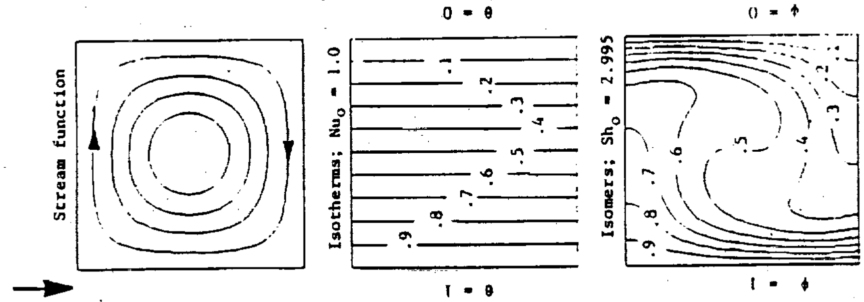
$t_o = 25$ secs., $t_1 = 26$ secs.

Combined Thermal & Solutal Convection

System dimensions and parameters

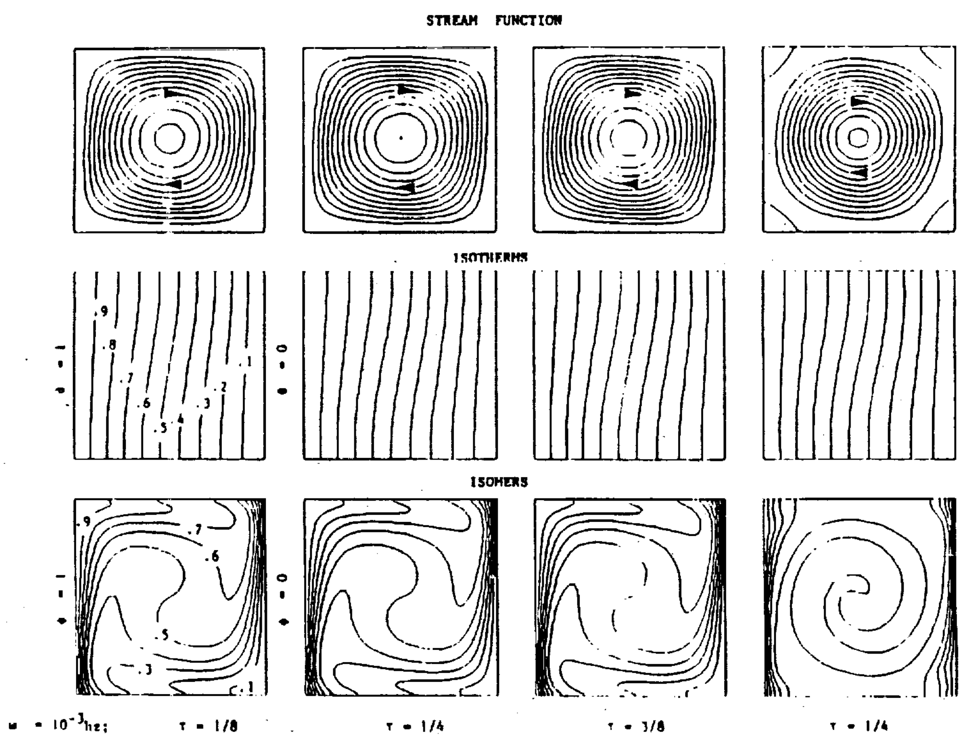
Gallium doped Germanium

Melt size	:	2 cm x 2 cm
ΔT	:	100K
ν [m^2/s]	:	1.30×10^{-7}
ρ [kg/m^3]	:	5.60×10^{3}
α [m^2/s]	:	1.30×10^{-5}
λ [m^2/s]	:	1.30×10^{-8}
β [/K]	:	2.5×10^{-4}
Pr	:	0.01
Sc	:	10
Gr_T (due to $\bar{g}$)	:	5.805×10^{2}
Ra_T (due to $\bar{g}$)	:	5.805

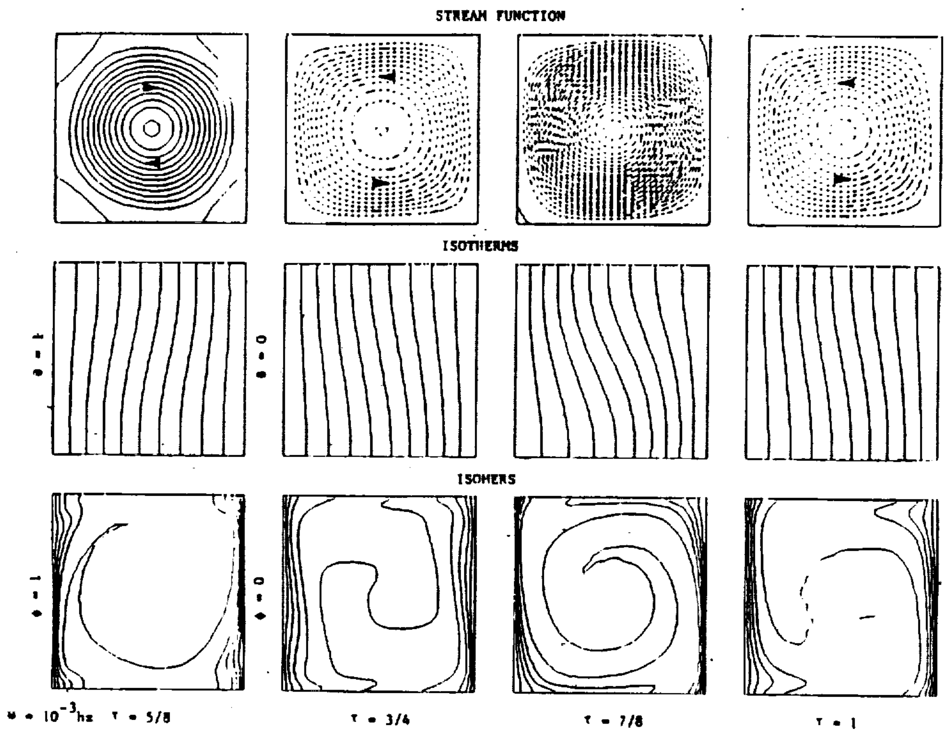


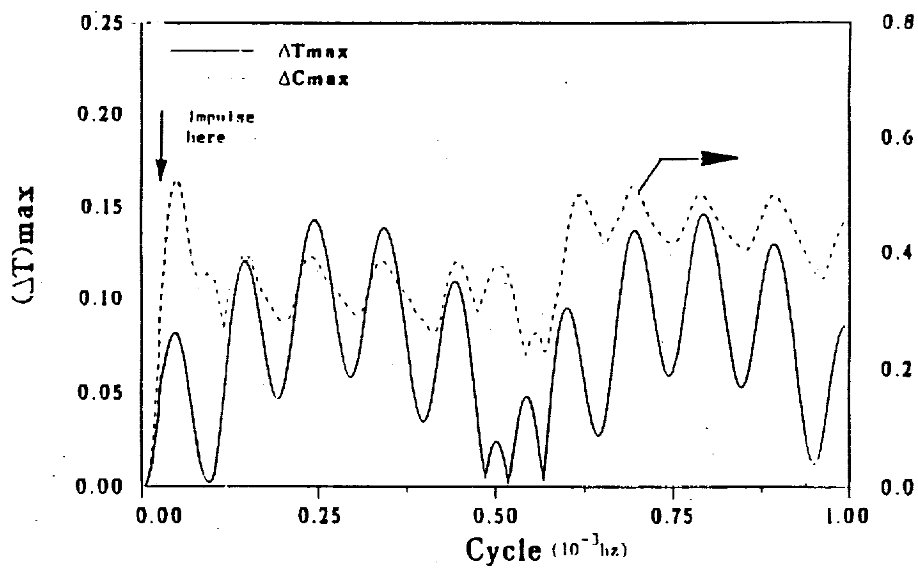
Steady State Results
Reference Case

Flow, Thermal and Solutal Fields

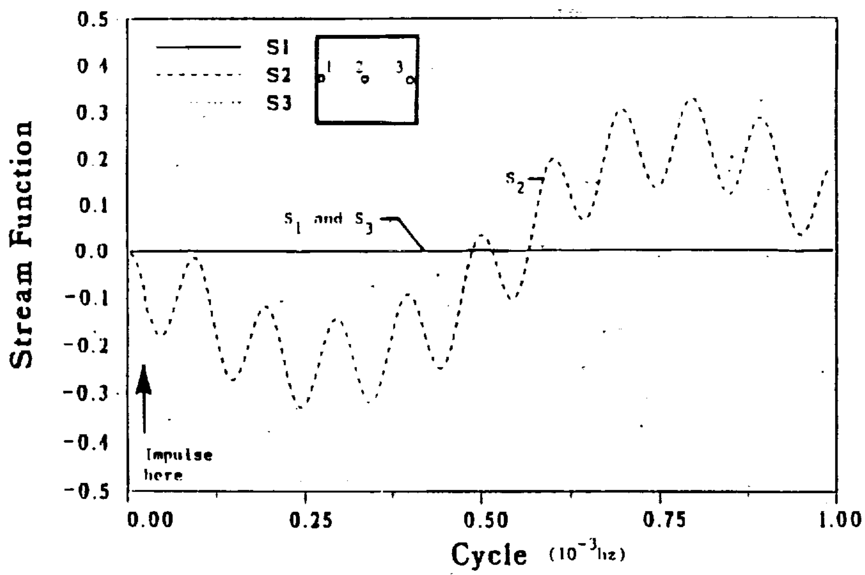


Flow, Thermal and Solutal Fields

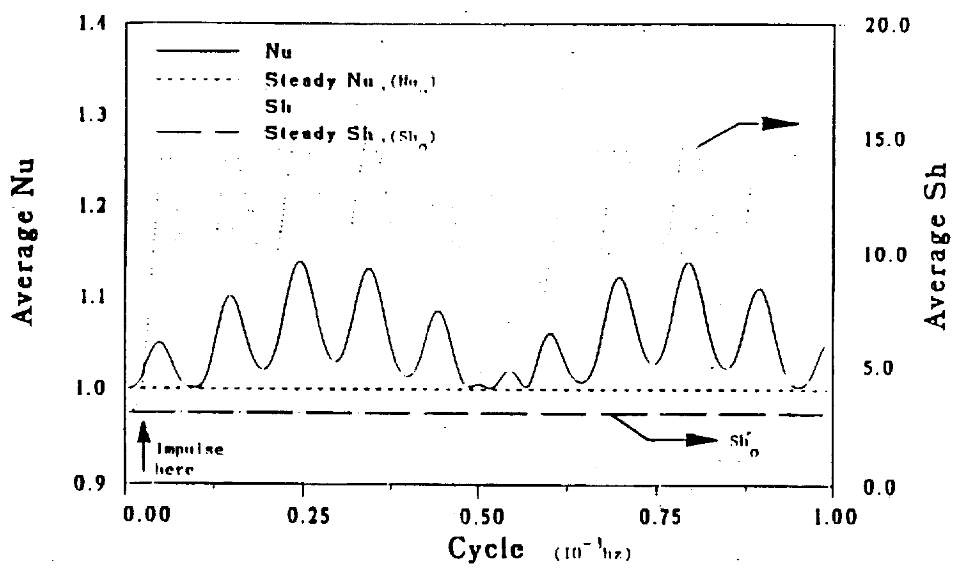




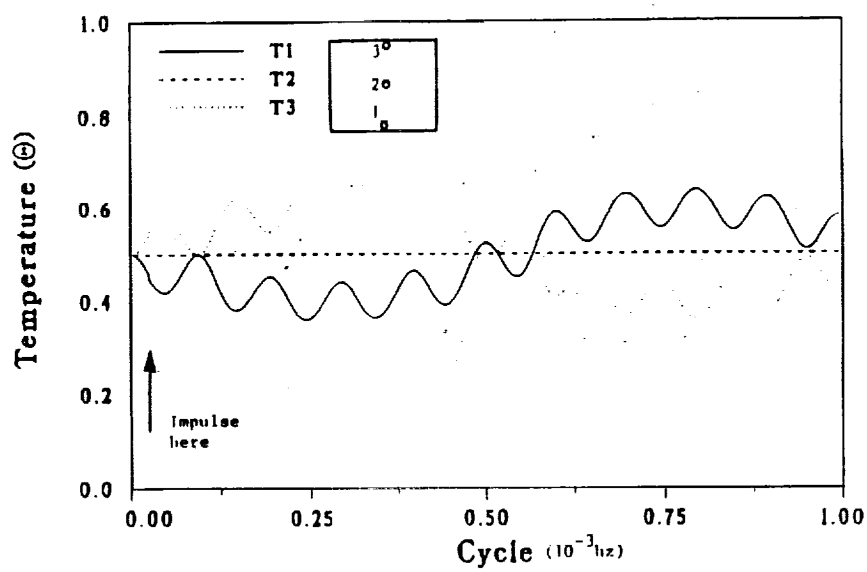
TEMPERATURE AND CONCENTRATION SENSITIVITIES



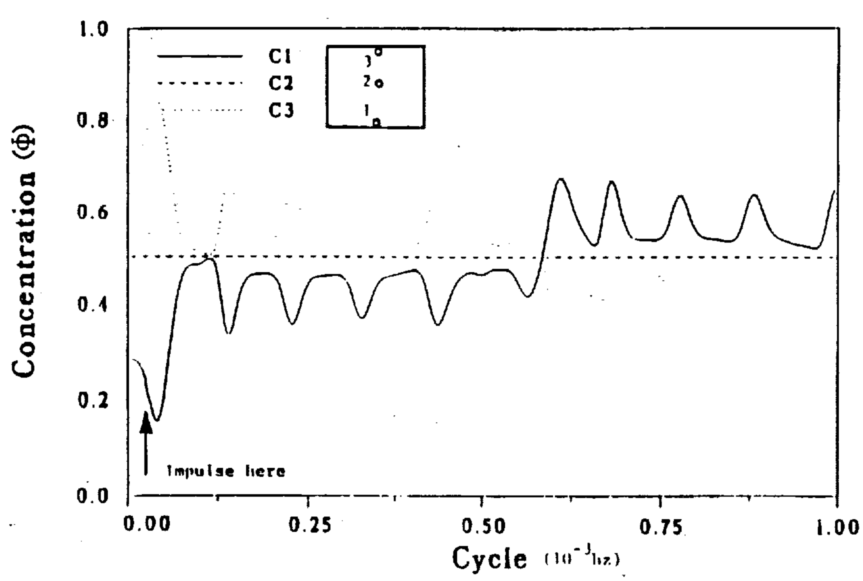
STREAM FUNCTION PLOT



AVERAGE HEAT AND MASS TRANSFER



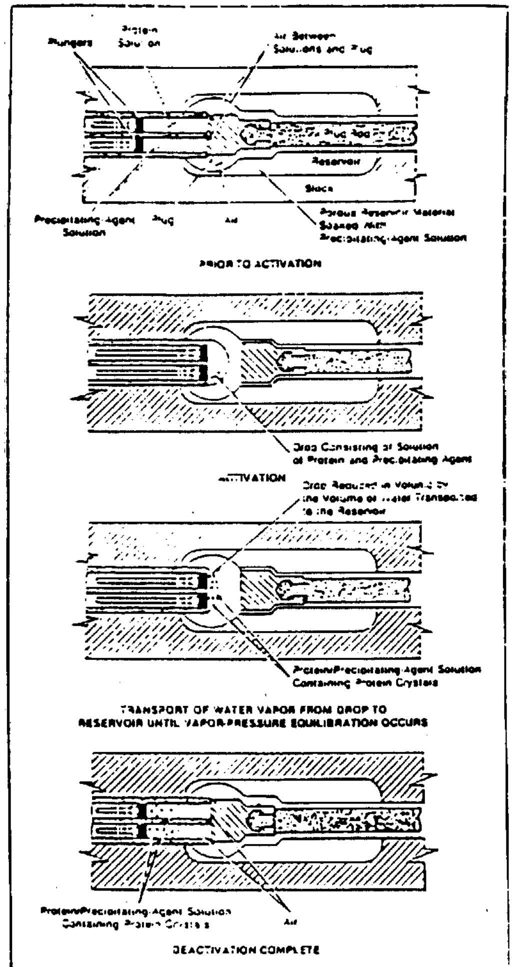
TEMPERATURE AT MONITORING POINTS



CONCENTRATION AT MONITORING POINTS

Protein Crystal Growth

PCG Vapor Diffusion Apparatus Principles



Crystals of Protein grow in a drop ... The drop containing the crystals is then withdrawn into the syringes.

Reprint Series
3 November, 1989, Volume 246, pp. 651–654

SCIENCE

Protein Crystal Growth in Microgravity

Lawrence J. DeLucas, Craig D. Smith, H. Wilson Smith, Senadhi Vijay-Kumar, Shobha E. Senadhi, Steven E. Ealick, Daniel C. Carter, Robert S. Snyder, Patricia C. Weber, F. Raymond Salemme, D. H. Ohlendorf, H. M. Einspahr, L. L. Clancy, Manuel A. Navia, Brian M. McKeever, T. L. Nagabhushan, George Nelson, A. McPherson, S. Koszelak, G. Taylor, D. Stammers, K. Powell, G. Darby, Charles E. Bugg

The crystals of most proteins or other biological macromolecules are poorly ordered and diffract to lower resolutions than those observed for most crystals of simple organic and inorganic compounds. Crystallization in the microgravity environment of space may improve crystal quality by eliminating convection effects near growing crystal surfaces. A series of 11 different protein crystal growth experiments was performed on U.S. space shuttle flight STS-26 in September 1988. The microgravity-grown crystals of γ-interferon D[1], porcine elastase, and isocitrate lyase are larger, display more uniform morphologies, and yield diffraction data to significantly higher resolutions than the best crystals of these proteins grown on Earth.

Why should Proteins grow better in μ-g ?

Possible Advantages:

- **Free Suspension**
  - **Elimination of interfacial effects**
  - **More uniform growth environment**
- **Reduced Hydrostatic Pressure**
  - **Eliminate internal stresses**
- **Reduced Convective Flows**
  - **More uniform growth environment**
  - **Establish diffusion controlled transport**

Protein Crystal Growth

- As crystals grow larger, convective transport maintains $\sigma_I \doteq \sigma_B$

- Implies growth rate independent of size

BUT

- Crystals in microgravity grow faster !

- Could convective flow influence attachment kinetics ?

Protein Crystal Growth

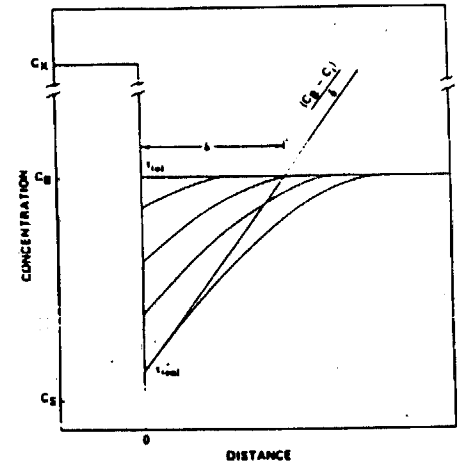


FIG. 1. Representation of solute concentrations about a growing crystal surface. At time $t = 0$, $C_I = C_B$. As crystallization proceeds, $C_I \to C_S$ at a rate dependent upon the relative importance of diffusion (fast) versus surface kinetics (slow) control of growth. The solute gradient at the growth interface is defined by $(C_B - C_I)/\delta$.

Dimensionless Supersaturation ratio

$$\sigma_I = (C_I - C_s)/C_s$$

C_s : Saturation concentration of solute
C_B : Bulk solution concentration
δ : Boundary layer thickness

$\delta = L/(ScGr)^{1/4}$
Gr = Grashof Number
Sc = Schmidt

Forced Flow Experiments - NASA MSFC (Pusey et al.)

- Transport is rate limiting step (most small molecule crystals)
- Forced convection increases growth upto a point where surface attachment kinetics becomes controlling factor
- Tetragonal Lysozyme, forced flow of 30 to 40 microns/s slows and eventually stops growth of 10 micron crystals
- These flows typical of natural convection flows - May be key to phenomena of growth cessation

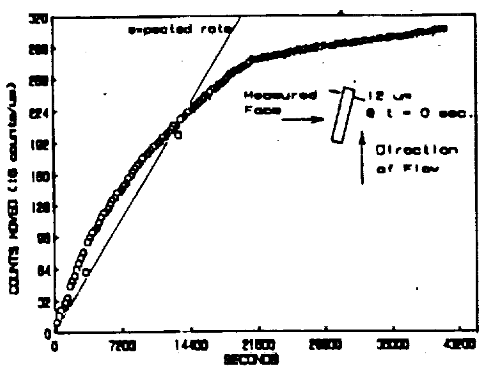


Fig. 6. Results of a macro flow cell experiment. In this case, the protein concentration was 8.73 mg/ml, the net flow velocity was 28 μm/s, the initial (110) dimension was 15 μm, and the angle of the measured face to direction of flow was $-2°$.

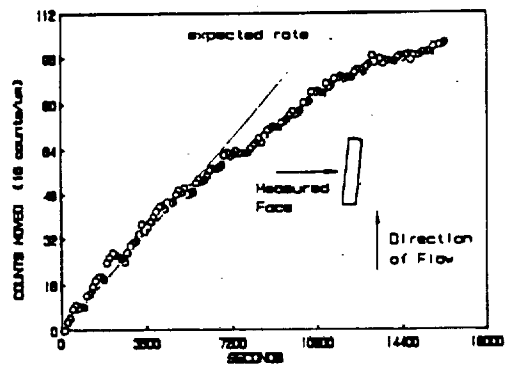
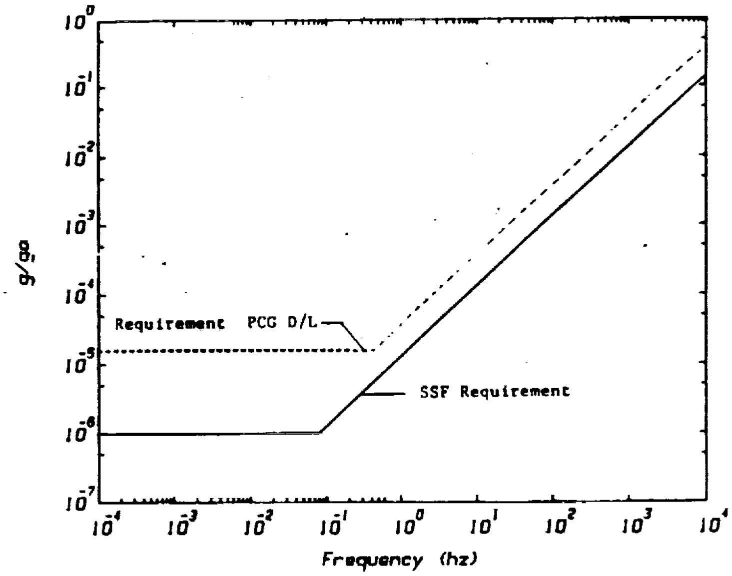


Fig. 5. Representative micro-flow cell experimental data. In this instance, the measured face was at $-12°$ to the direction of flow, new solution velocity was 40 μm/s, the protein concentration is 11.7 mg/ml, and the initial (110) face length was 12 μm.

Order of Magnitude Analysis (OMA) - (Naumann et al.)

- Acceptable g-levels for Diffusion Controlled transport
- 1-D solution of time dependent transport equations
- Criterion for diffusion controlled growth $\delta > a$ (L = 2a; L:crystal length; a: crystal diameter)

- Example: $D=10^{-6}$ cm^2/s, $\nu=10^{-2}$ cm^2/s, $\Delta\rho/\rho=10^{-1}$, a=0.05 cm
- $g \leq 1.6 \times 10^{-5}\ g_o$ for steady state (residual acceleration)
- $g\Delta t \leq 4 \times 10^{-6}\ g_o$-sec for $\Delta t \ll 0.25$ sec (impulses)
- $g \leq 1.6 \times 10^{-5}\ g_o$ for $f \ll 0.64$ hz (low frequencies)
- $g \leq 4.0 \times 10^{-7}\omega\ g_o$ for $f \gg 0.64$ hz (high frequencies)



PCG Toerable G-levels

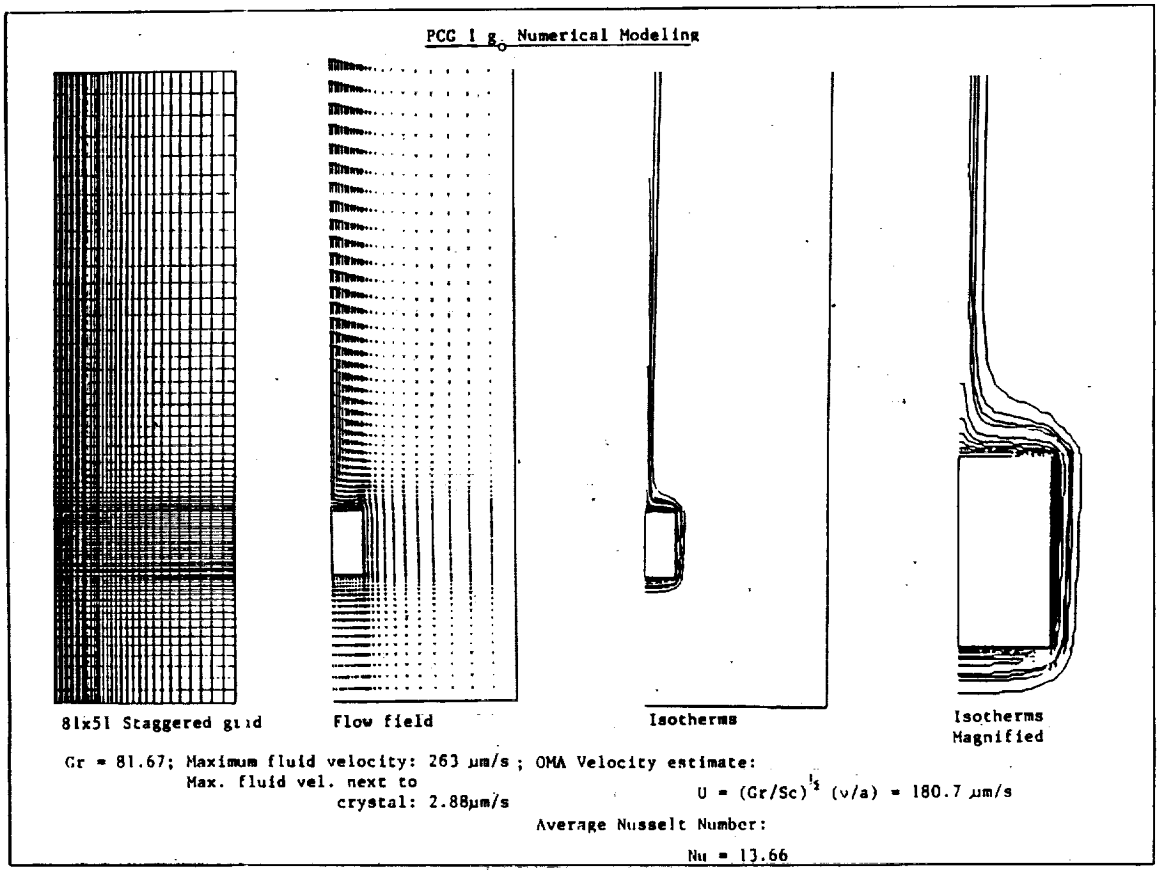
PCG MOdeling

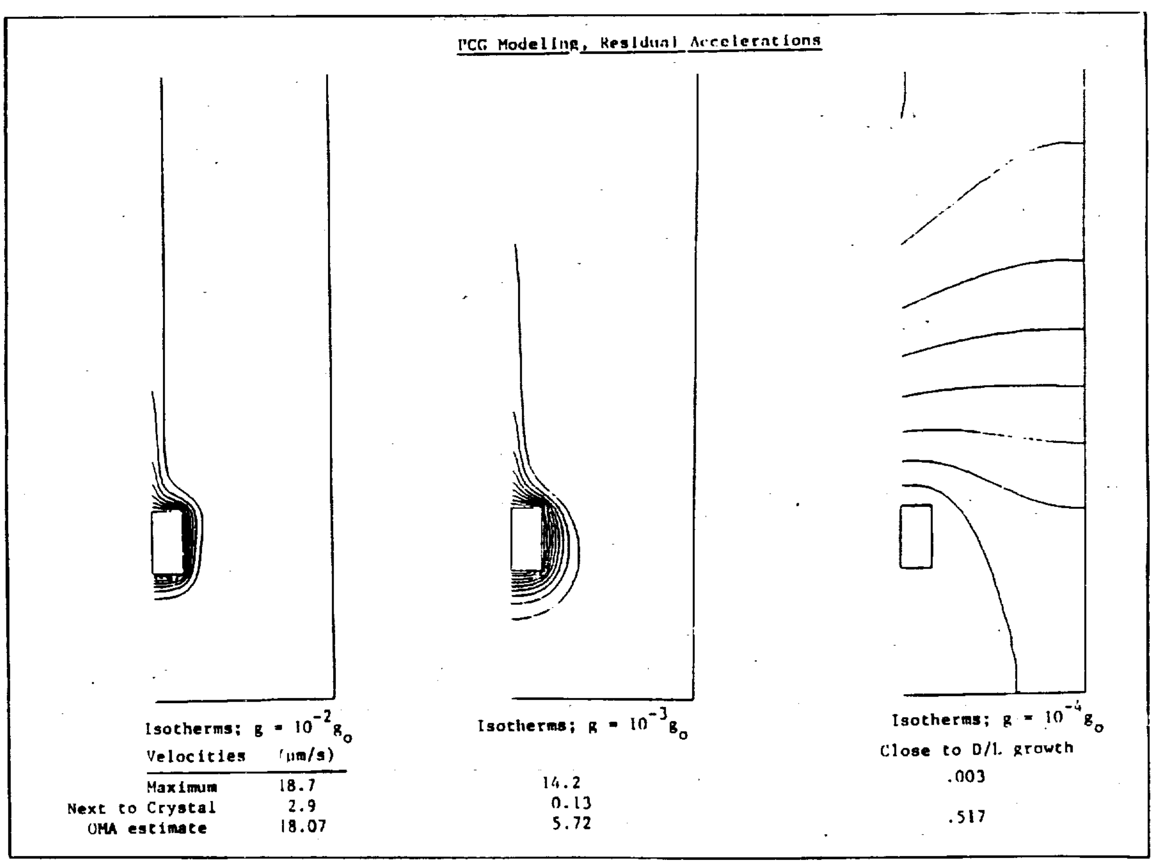
- Order of Magnitude Analysis (OMA)
- 2-D Cartesian with symmetry plane
- Quasi steady
- 81x61 staggered grid (boundary layer reolution)
- System parameters:

  a : 0.05 cm
  ν : 0.01 cm^2/s
  ρ_B : 1.2 gm/cm^2
  $\Delta\rho$: 0.01 gm/cm^3
  g_o : 981 cm/s^2

- Grashof number : 81.67
- Cases investigated:

1. Residual acceleration:
   1g, 10^{-2}g, 10^{-3}g, 10^{-4}g, 10^{-5}g
2. Transient Conditions:
   Impulse and step functions (underway)
3. Periodic accelerations:
   Single sinusoidal inputs (underway)


PCG 1 g_o Numerical Modeling
81x51 Staggered grid
Flow field
Isotherms
Isotherms Magnified
Gr = 81.67; Maximum fluid velocity: 263 μm/s ; OMA Velocity estimate:
Max. fluid vel. next to crystal: 2.88μm/s
$U = (Gr/Sc)^{1/2} (\nu/a) = 180.7$ μm/s
Average Nusselt Number:
Nu = 13.66


PCG Modeling, Residual Accelerations
Isotherms; $g = 10^{-2} g_o$
Isotherms; $g = 10^{-3} g_o$
Isotherms; $g = 10^{-4} g_o$
Close to D/L growth
Velocities (μm/s)
Maximum 18.7 14.2 .003
Next to Crystal 2.9 0.13
OMA estimate 18.07 5.72 .517

Protein Crystal Growth (PCG) - Inferences

- Protein crystals seem to grow at much larger relative supersaturations than small molecule crystals
- Growth rate is limited by attachment kinetics than by transport
- Attachment kinetics is apparently influenced by convective flows - mechanism uncertain
- Acceleration levels required to achieve D/L growth within SSF specifications may not be achievable on manned vehicles
- D/L transport can help minimize incorporation of impurities but growth cessation cannot be explained

Protein Crystal Growth (PCG) - Issues

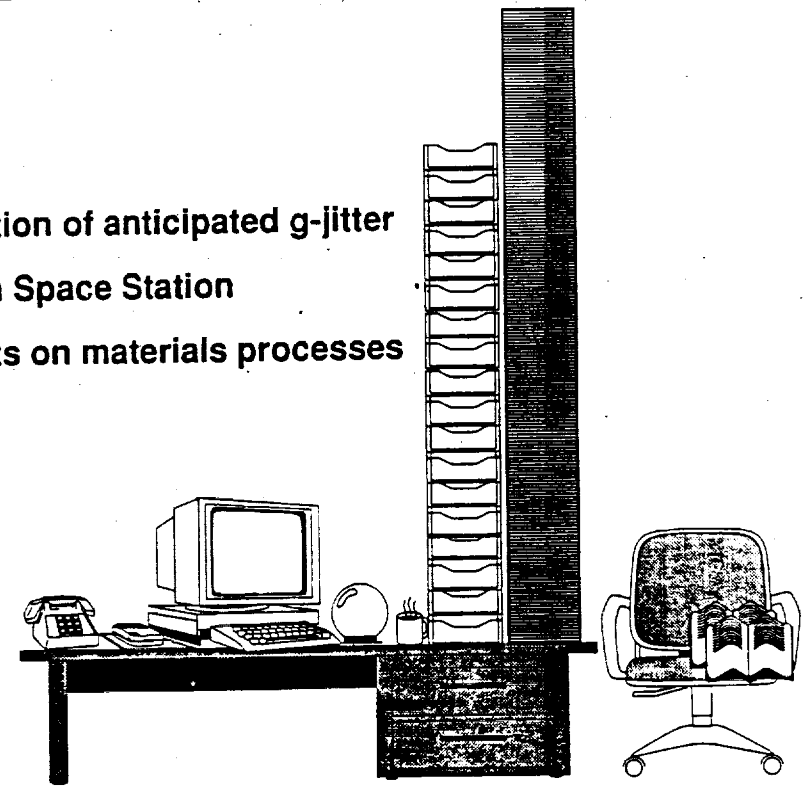
- Do proteins actually grow faster in microgravity ?
- Why does forced flow affect one form of lysozyme and not another? Do other proteins exhibit this behavior?
- Do some proteins strain under thier own weight? Can we grow crystals in microgravity that are too fragile to withstand 1-g or higher accelerations?
- Will Vibration Isolation benefit PCG? Will it help produce better results?

N92-28438

An examination of anticipated g-jitter on Space Station and its effects on materials processes

Written by:
Emily Nelson

Presented by:
Arnon Chait



AEROSPACE TECHNOLOGY DIRECTORATE
COMPUTATIONAL MATERIALS LABORATORY
NASA Lewis Research Center

Objective

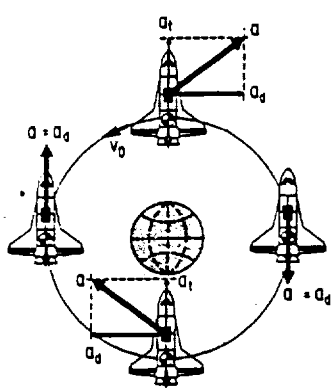
- Characterization of low-g environment
  - Sources of residual acceleration
  - Measurements based on space experience to date
  - Space Station specifications
  - Vibration isolation
- Assess effects of g-jitter on materials processes
  - Directional solidification
  - Protein crystal growth
  - Crystal growth from vapor
  - Float zone
  - Other concerns: sedimentation, drops, bubbles, sloshing
- Identify areas of concern, future research

The low-g acceleration environment

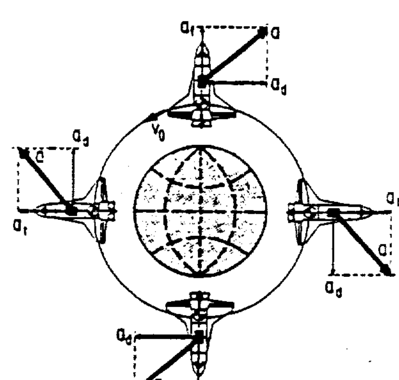
AEROSPACE TECHNOLOGY DIRECTORATE
COMPUTATIONAL MATERIALS LABORATORY
NASA Lewis Research Center

Orbital Modes

from Feuerbacher et al. (1987)



Inertial



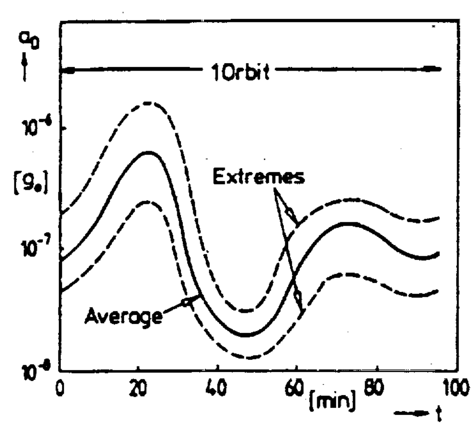
Gravity gradient

Calculated or Measured Sources of Residual Accelerations

Body Force	Type	Shuttle	Space Station
Tidal -gravity gradient -centrifugal	Q.S.	$4 \times 10^{-7}\ g_0$ / m	$4 \times 10^{-7}\ g_0$ / m
Euler (Ω)	Q.S.	usu. neglected	$10^{-7}\ g_0$ at 10^{-4} Hz
Atmospheric drag	Q.S.	$3 \times 10^{-6}\ g_0$ at 170 km $2 \times 10^{-8}\ g_0$ at 560 km	10^{-8} to $10^{-5} g_0$ at 10^{-4} Hz
Altitude control -primary thruster firings - vernier thruster firings	T	$3 \times 10^{-2}\ g_0$* 10^{-3} - $10^{-4}\ g_0$	5x per year
Gas and fluid dumps	T	$10^{-5}\ g_0$	‡
Structural vibration	O	app. 5, 7, and11 Hz	fundamental 0.17 Hz
KU band antenna		$10^{-2}\ g_0$ at 17 Hz*	‡
Crew motion	T, O	10^{-2}-$10^{-5}\ g_0$*	‡
Machinery	O	$10^{-4}\ g_0$at >100 Hz*	‡
Centrifuge	O	- N / A -	$10^{-5}\ g_0$ at 0.3 Hz †
Solar radiation pressure	Q.S.	$4 \times 10^{-9}\ g_0$	$1 \times 10^{-8}\ g_0$
TOTAL		$2\text{-}4 \times 10^{-3}\ g_0$	

* Experimentally measured acceleration peaks
† Estimate from calculation for 1.8 ft. dia. centrifuge (Searby, 1986)
‡ Unknown or not applicable

Aerodynamic Drag



Predicted a_d of Space Station over one orbit. (Monti et al., 1987).

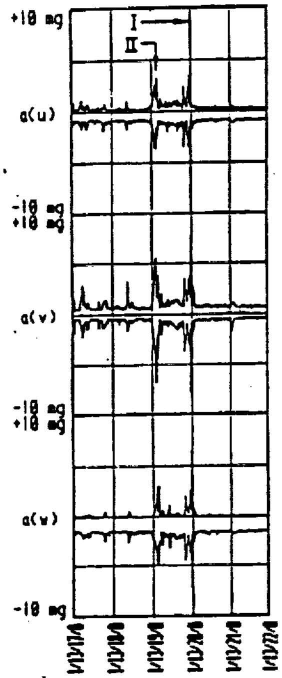
Atmospheric drag is a function of:
- density of fluid medium
  - orbiting altitude
  - diurnal bulge
- projected area / mass ratio
  - solar panel attitude
  - pitch

Design: **2 - 4 x 10^{-7} g_0** with variation by a factor of 6 over one orbit

Est. altitude: 350- 400 km

Shuttle Acceleration Measurements

Crew disturbances on Spacelab during D-1 mission (Monti et al., 1987).



- 3D disturbances over a wide frequency range
  - Accelerometers must have adequate resolution and bandwidth (e.g., SAM's)
  - Multiple sensor arrays? Sweet spots?
- Difficulties due to sheer volume of data
  - Acquisition
  - Data reduction
  - Correlation to mission events and experiment

I = Closing of container door
II = Operation of FPM

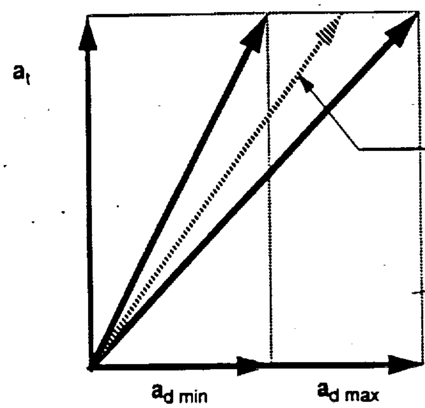
ATD AEROSPACE TECHNOLOGY DIRECTORATE
COMPUTATIONAL MATERIALS LABORATORY
NASA Lewis Research Center

Orientation of g

- Preliminary results from SL3 show that orientation varies dramatically in all directions (from Rogers and Alexander, 1990)

Space Station Environment

- LV/LH will be flight mode
- Orientation of body force relative to experiment
- Torque equilibrium attitude error adds additional uncertainty

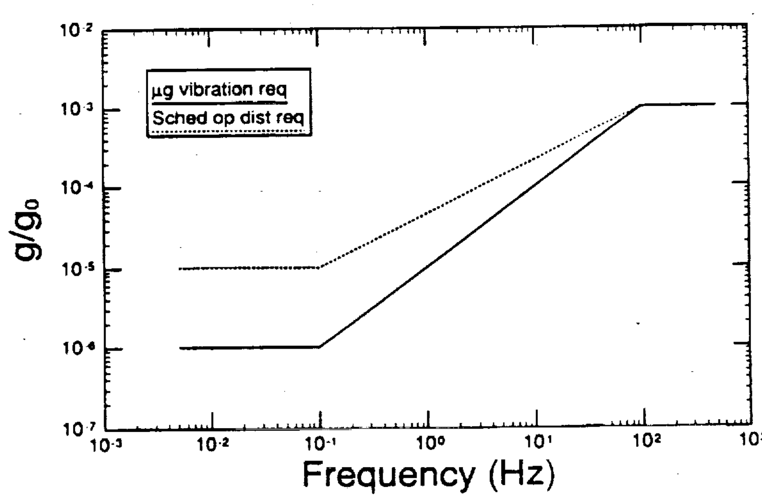


These are primary *quasisteady* forces. However, g-jitter *dominates* the acceleration environment

AEROSPACE TECHNOLOGY DIRECTORATE
COMPUTATIONAL MATERIALS LABORATORY
NASA
Lewis Research Center

Space Station Environment (cont'd)

- Evolution of specs w.r.t. g(f)
  - originally called for blanket 10^{-5} g_o
  - current specs have g(f)
  - unbounded in terms of energy



Effect of low-g environment on materials processing

AEROSPACE TECHNOLOGY DIRECTORATE COMPUTATIONAL MATERIALS LABORATORY NASA Lewis Research Center

Types of disturbances modeled

- Magnitude of g only
  - Impulsive
    - Step
    - Successive pulse(s)
  - Periodic
  - Multiple frequency
  - Random
- Orientation of g varied
- Orientation and magnitude of g change

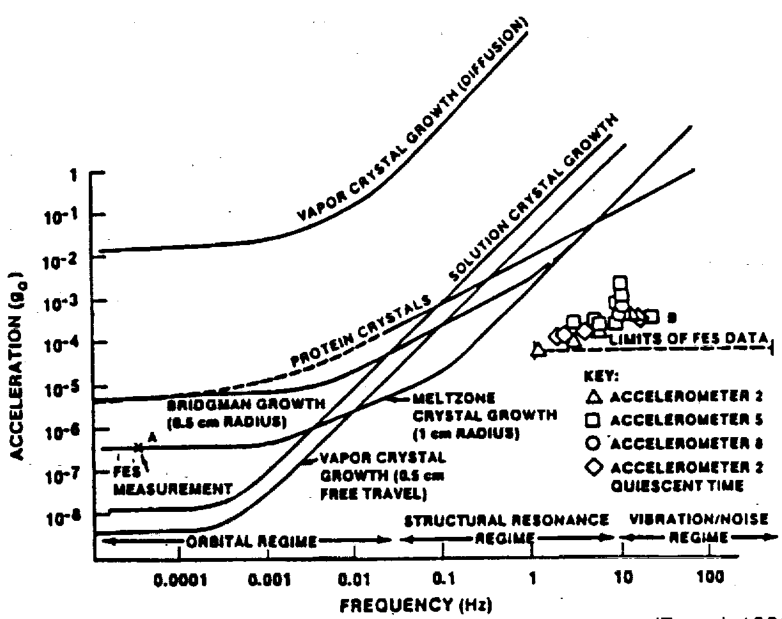
Why should we care about g-jitter?

- Orientation and magnitude of **g** dictate flow phenomena for many processes
  - sensitivity to orientation
    - D.S. -- Arnold et al., Alexander et al., McFadden and Coriell
    - TGS -- Nadarajah et al.
    - Benard convection -- Duh
  - sensitivity to magnitude -- transition to another flow regime
- Even if mean **g** is zero, there may be some net transport
  - sinusoidal oscillation ==> steady streaming (Amin; Kamotani et al.)
  - pulse/antipulse do not always cancel in D.S. (Alexander et al.)
- Excitation of instabilities
  - sinusoidal modulation alters stability and flow mode (Gresho and Sani, Biringen and Peltier; Coriell, McFadden and Murray)
  - random modulation alters stability and flow mode even more dramatically (Biringen and Peltier)
  - resonant frequencies at interfaces
    - liquid bridges even at very low magnitudes (10^{-6} g_o) (Langbein; Bauer; Meseguer; Zhang and Alexander)
    - liquid/liquid interfaces (Jacqmin and Duval)
- Decay time and net effect on materials process

AEROSPACE TECHNOLOGY DIRECTORATE — COMPUTATIONAL MATERIALS LABORATORY — NASA Lewis Research Center

Tolerable g-levels as a function of frequency

FREQUENCY VS ACCELERATION G-LEVEL TOLERANCE FOR MONOCHROMATIC OSCILLATING DISTURBANCES POINTS AT A & B ARE SPACE LAB 3 ACCELERATION MEASUREMENTS

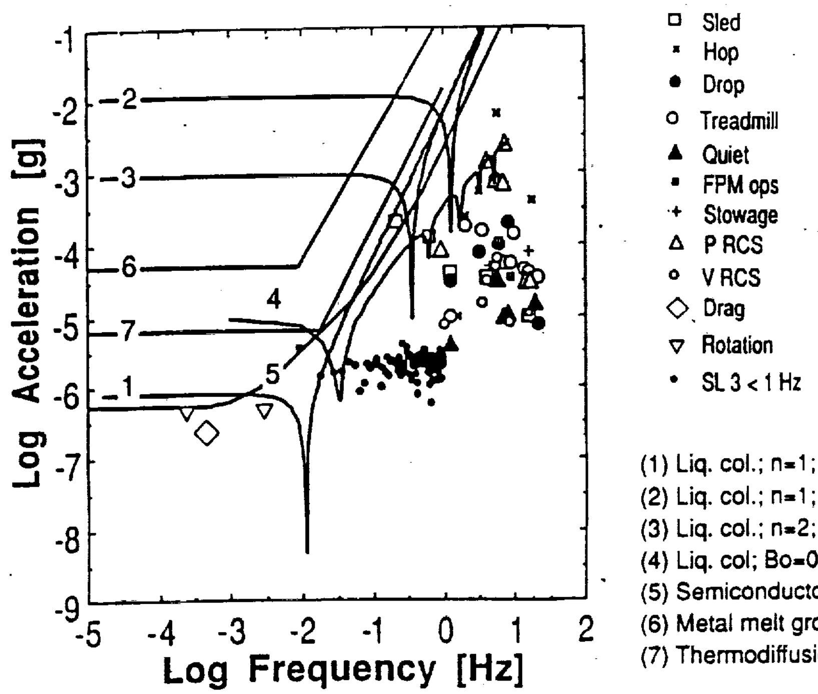


(Demel, 1986)

COMPUTATIONAL MATERIALS LABORATORY

AEROSPACE TECHNOLOGY DIRECTORATE — NASA Lewis Research Center

Tolerable g-levels and experimentally measured acceleration peaks



(1) Liq. col.; n=1; Λ = 0.9999
(2) Liq. col.; n=1; Λ = 0.9
(3) Liq. col.; n=2; Λ = 0.9999
(4) Liq. col; Bo=0.002; g=1.42x$10^{-5}g_o$
(5) Semiconductor melt growth
(6) Metal melt growth
(7) Thermodiffusion expt (Monti et al.)

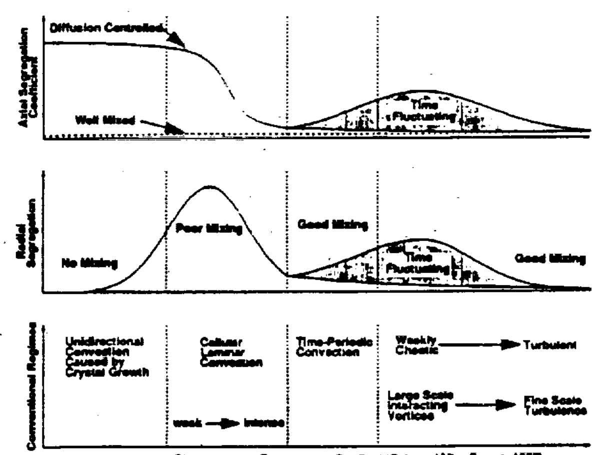
Alexander (1990)

COMPUTATIONAL MATERIALS LABORATORY

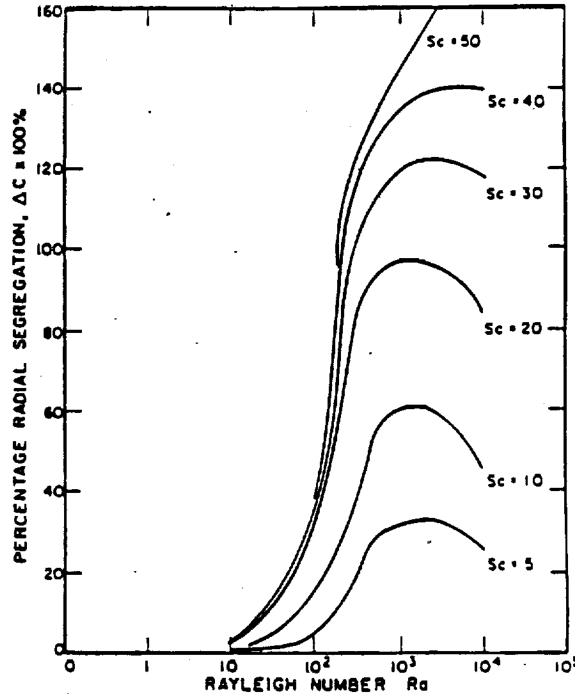
AEROSPACE TECHNOLOGY DIRECTORATE — NASA Lewis Research Center

Directional Solidification

- Steady **g** sets up basic fluid regime -- fundamental variability in segregation characteristics



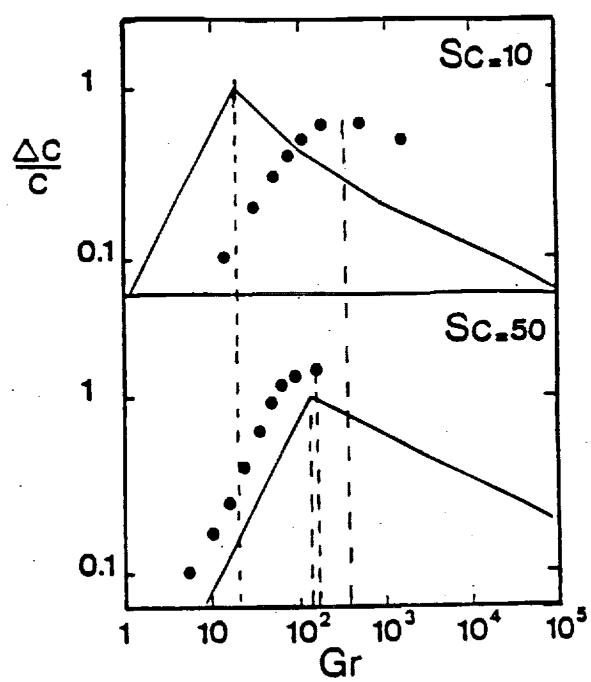
Compositional nonuniformity as a function of Ra in D.S.



(Chang and Brown, 1983)

AEROSPACE TECHNOLOGY DIRECTORATE COMPUTATIONAL MATERIALS LABORATORY NASA Lewis Research Center

Comparison of O(M) estimates to numerical simulation



Dopant nonuniformity in D.S.

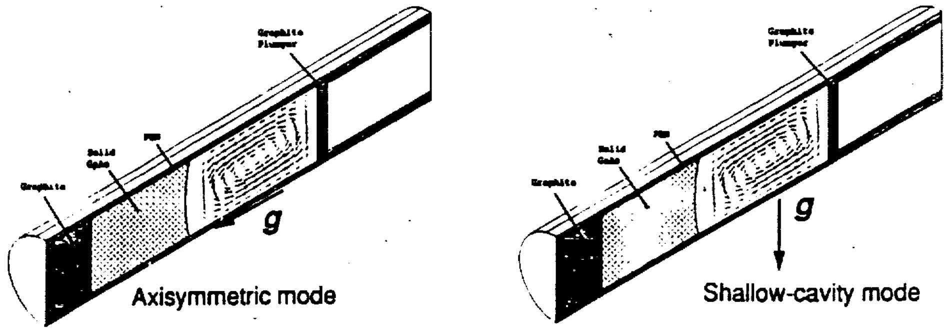
Solid lines = O(M) estimates of Camel and Favier, 1986

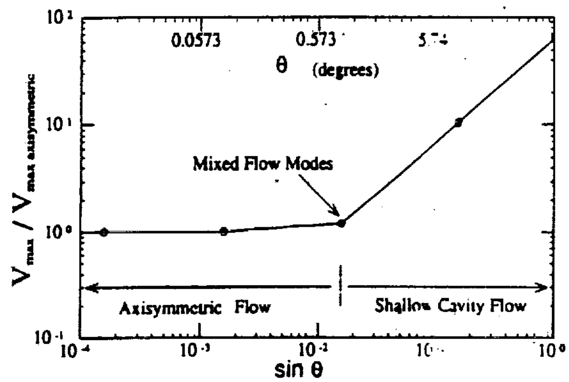
Dots = Direct numerical simulation of Chang and Brown, 1983

from Alexander et al., 1989

Sensitivity of D.S. to orientation of g

Comparison of flow patterns for the d.s. of GaAs at 10^{-5} g_0 as a function of gravity orientation. (Arnold et al., 1990)



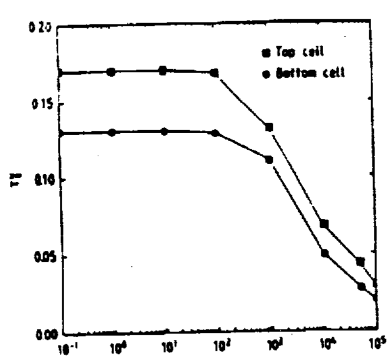


AEROSPACE TECHNOLOGY DIRECTORATE
COMPUTATIONAL MATERIALS LABORATORY
NASA Lewis Research Center

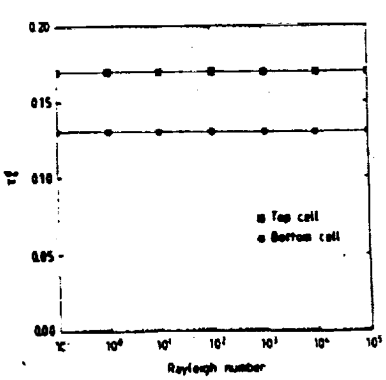
Response to step changes in g

D.S. of Ge (Griffin and Motakef, 1989)

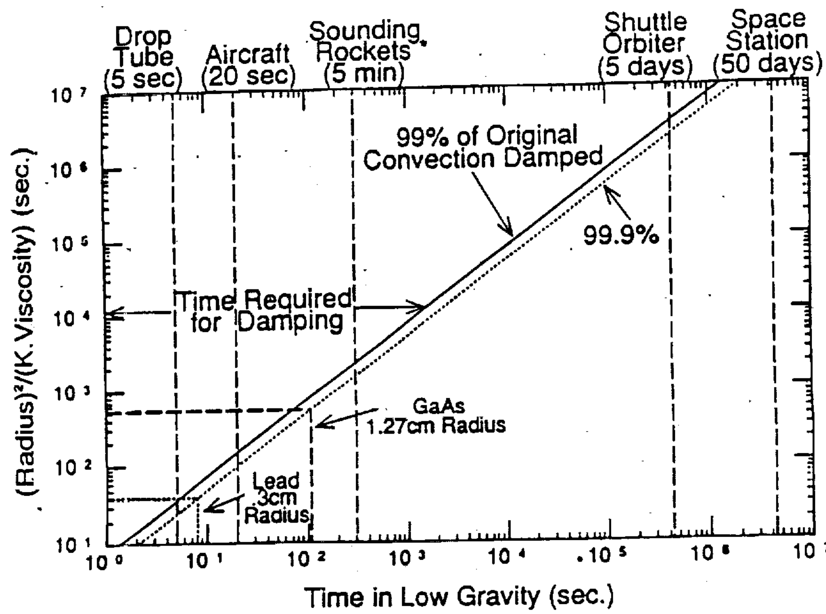
$\tau = \tau v / r_s^2$



Step increase from 0 - g_0.



Step decrease from g_0 - 0.

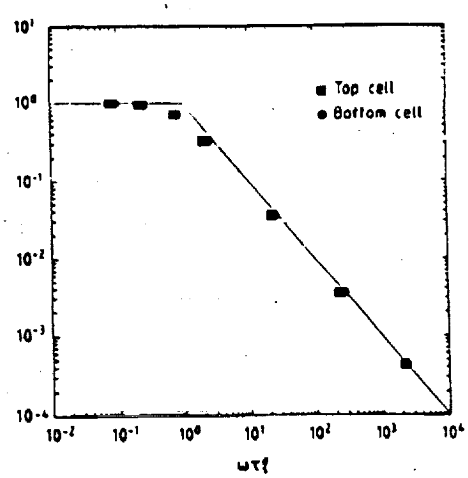


AEROSPACE TECHNOLOGY DIRECTORATE
COMPUTATIONAL MATERIALS LABORATORY
NASA Lewis Research Center

Response to sinusoidal disturbance

D.S. of Ge (Griffin and Motakef, 1989)

$$G = \frac{(\Delta\psi)/\psi}{(\Delta Ra)/(Ra)}$$

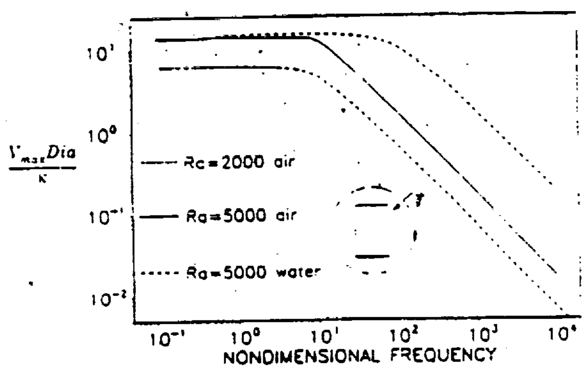


AEROSPACE TECHNOLOGY DIRECTORATE — COMPUTATIONAL MATERIALS LABORATORY — NASA Lewis Research Center

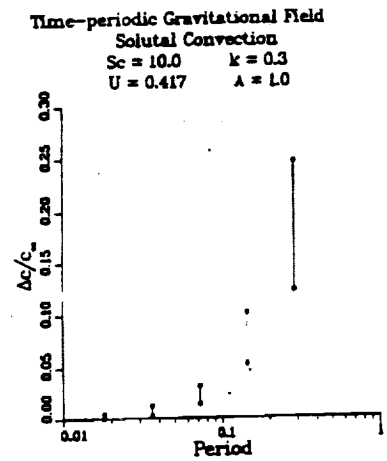
Rotation of g

$$(g_x, g_y) = g\,(\cos 2\pi t/P,\ \sin 2\pi t/P)$$

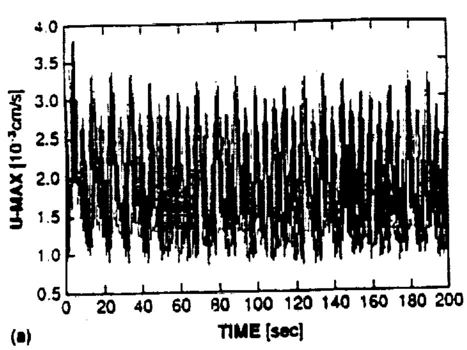
Thermal convection in cylinder
(Schneider and Straub, 1989)

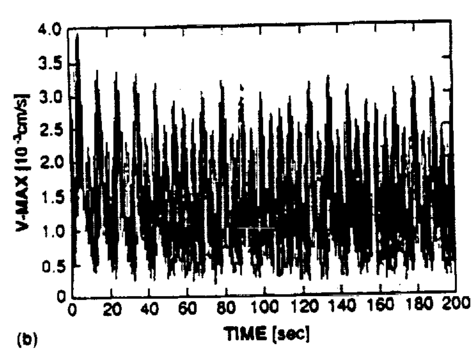


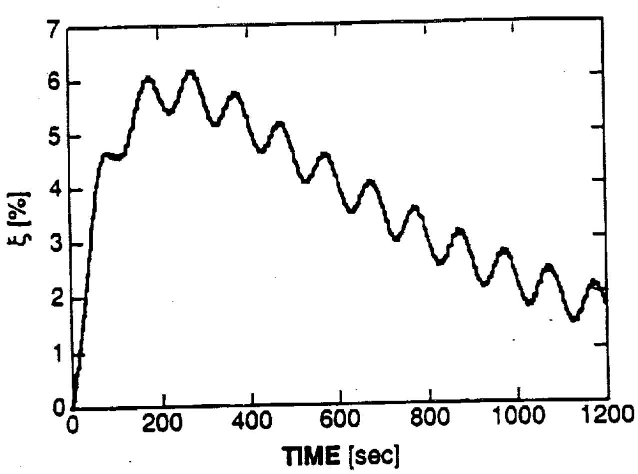
Solutal convection in cylinder
(McFadden and Coriell, 1988)



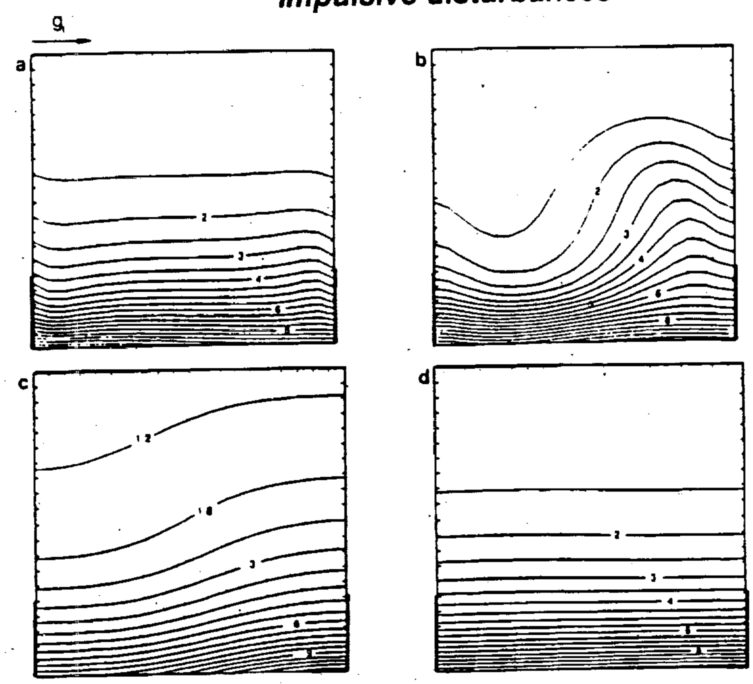
AEROSPACE TECHNOLOGY DIRECTORATE — COMPUTATIONAL MATERIALS LABORATORY — NASA Lewis Research Center





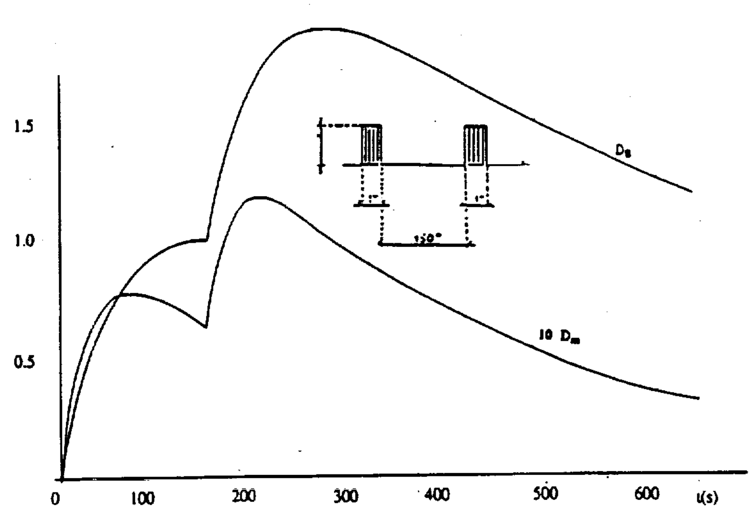


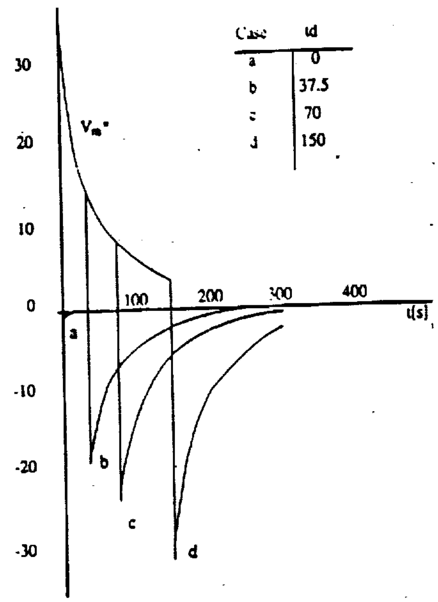
Impulsive disturbances

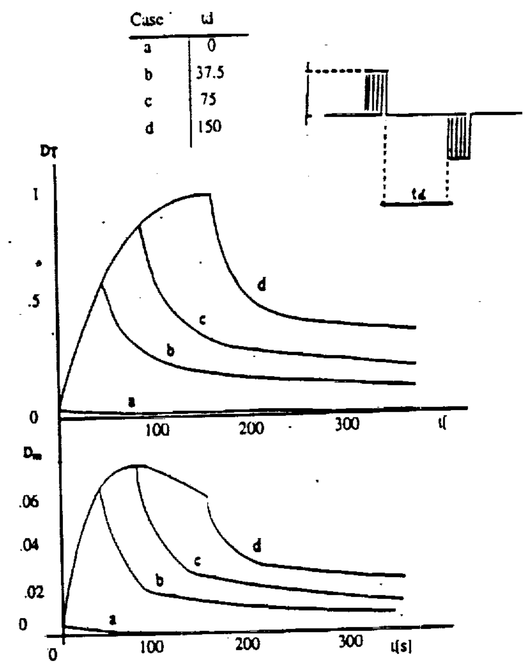


Solute field development in D.S. of GeGa subjected to 1-sec pulse (Alexander et al., 1989)

AEROSPACE TECHNOLOGY DIRECTORATE
COMPUTATIONAL MATERIALS LABORATORY
NASA Lewis Research Center







AEROSPACE TECHNOLOGY DIRECTORATE COMPUTATIONAL MATERIALS LABORATORY NASA Lewis Research Center

Random disturbances

- Biringen and Peltier (1989) find Benard convection unstable to random disturbance, which for the same conditions were stable to sinusoidal modulation

Protein crystal growth

(Nadarajah et al., 1990)

The maximum change in growth rates resulting from transient disturbances for the top face.

Amplitude of disturbance	Duration or frequency	Maximum change in growth rate	St(Ra$_c$*/ReSc)g
$10^{-3}g_0$	1 sec	3%	3
$10^{-4}g_0$	10^{-3} Hz	9%	50
$10^{-4}g_0$	10^{-2} Hz	4%	5
$10^{-4}g_0$	10^{-1} Hz	0.4%	0.6
$10^{-3}g_0$	10^{-3} Hz	238%	50
$10^{-3}g_0$	10^{-2} Hz	49%	50
$10^{-3}g_0$	10^{-1} Hz	3%	15
$10^{-2}g_0$	10^{-1} Hz	29%	15

AEROSPACE TECHNOLOGY DIRECTORATE COMPUTATIONAL MATERIALS LABORATORY NASA Lewis Research Center

Vapor crystal growth

- van den Berg found "perfect crystallographic structure" in HgI_2 crystals grown in Spacelab 3
- Soviet successes in PVT, CVD (Tatarchenko)
- Wiedemeier finds much larger mass flux rates in low-g relative to earth (up to 300%) for GeSe-GeI_4, but not for Ge-Se in Xe
- 3M has unusual growth morphologies for PVTOS experiment

BUT results are yet to be explained ...

AEROSPACE TECHNOLOGY DIRECTORATE COMPUTATIONAL MATERIALS LABORATORY NASA Lewis Research Center

Float Zone

- Microgravity Disturbances Experiment (Dunbar and Thomas, 1990)
  - 3D mid-deck acceleration data
  - Unique in attempt to correlate acceleration environment to expt.
  - Couldn't cause instability in In melt, even at 4-5x Rayleigh limit
  - Underscores need for increased understanding of oxide layers

AEROSPACE TECHNOLOGY DIRECTORATE COMPUTATIONAL MATERIALS LABORATORY NASA Lewis Research Center

Liquid bridges

- Stability limits and type of instability altered in space (Langbein)
- Most sensitive to g-jitter effects at resonant frequencies
- Tricky to model numerically
- Application to float zone?

Conclusions

- G-jitter:
  - Will dominate the acceleration environment
  - Is a *3D multifrequency* phenomenon
  - Varies dramatically in orientation
- Realistically, we should expect some surprises in the acceleration environment on Station vis à vis the specs
- We don't even know if the specs are adequate without additional research
- Space processing is still very much in the research stage
  - Heat and mass transport is still not well understood in low-g environments
  - No evidence to indicate that we can do all of the materials processes we would like (although we should be able to do some of them)
  - Not ready for near-term commercialization of space
  - Desperately need a well-coordinated numerical/experimental database

AEROSPACE TECHNOLOGY DIRECTORATE COMPUTATIONAL MATERIALS LABORATORY NASA Lewis Research Center

Recommendations

- Critical need for a well-designed coordinated experimental/numerical effort with fully characterized conditions
  - Directly applicable to specific processes/materials of critical interest
  - High-risk, high-payoff, unique endeavors
  - Fundamental research
    - Steady streaming
    - Mixing
    - Stability
- Stress flexibility on any hardware created for materials processing
- Consideration of alternate/supplementary environment for more sensitive processes (esp. dominated by surface phenomena), e.g., free flyer

N92-28439

THE MICROGRAVITY ISOLATION MOUNT (MGIM) - A COLUMBUS FACILITY FOR IMPROVING THE MICROGRAVITY QUALITY OF PAYLOADS

R.G. Owen, D.I. Jones, A.R. Owens, G. Roberts, P. Hadfield
University College of North Wales

ABSTRACT

Results from past microgravity experiments flown on Spacelab missions indicate that the vibration environment often does not satisfy the stringent requirements of many of the experiments. Such experiments would derive substantial benefit if isolated from the main sources of vibration.

The Microgravity Isolation Mount (MGIM) is a facility for providing active vibration isolation for sensitive experiments on the Columbus Attached Laboratory and the Columbus Free-Flying Laboratory. The facility is designed to be accommodated in a standard Columbus rack and interfaces with existing rack utility services.

The design is based on a non-contact strategy, whereby the payload "floats" inside the rack and its position is controlled by a number of magnetic actuators. The main advantage of using this non-contact strategy is the improved microgravity quality obtainable. Payload acceleration levels approaching 1μg have been recorded during vibration tests on an early 3 d.o.f. prototype test rig at very low frequencies [1].

The MGIM Facility has been designed to be accommodated in a single sub-unit payload rack. The various elements of the facility are as follows :-

(i) A Platform unit, which is the isolated element onto which the payload is attached.

(ii) A Liner unit, which accommodates the Platform and payload and interfaces mechanically with a standard Columbus rack.

(iii) A Cage unit, which encloses the Platform, and accommodates a pair of locking mechanisms which secure the Platform to the Cage during non-microgravity periods. These locking mechanisms operate automatically to release and re-lock the Platform. They are also designed to withstand launch and re-entry stresses.

(iv) A Platform Supervisor, which holds the Platform control electronics. This unit also contains an electrical interface which supplies all electrical services to the Platform.

(v) A Payload Supervisor, located above the Platform, which is responsible for monitoring and controlling the payload.

Electrical power is supplied to the Platform by means of a power transformer with a loosely coupled secondary coil. Up to 1 kW of power can be supplied to the payload in this way without any mechanical contact. A series of three infra-red optical links are used to transmit

control and data signals to the payload. Heat energy is dissipated from the Platform to the Liner by thermal radiation using pairs of cooling panels arranged on the outer and inner surfaces of the Platform and Liner respectively.

A full scale mock-up of the facility has been constructed, and is currently being adapted to enable vibration isolation tests to be performed. The tests will be confined to a single translational axis, but will include both sinusoidal and stochastic vibration inputs. In addition, tests on Platform release in the presence of rack vibration will be performed.

[1] Microgravity Isolation Mount: Final Report on ESTEC Contract No. 6380/85, May 1987.

THE MICROGRAVITY ISOLATION MOUNT
- a Columbus facility for improving the microgravity quality of payloads

R. G. Owen, D. I. Jones, A. R. Owens
G. Roberts, P. Hadfield

School of Electronic Engineering Science
University College of North Wales
Bangor, Gwynedd, Wales, U.K.

International Workshop on Vibration Isolation Technology for Microgravity Science Applications

NASA Lewis Research Center
Cleveland, Ohio, U.S.A.
April 23-25, 1991

The Microgravity Isolation Mount (MGIM) - a Columbus facility for improving the microgravity quality of payloads

Abstract

The Microgravity Isolation Mount (MGIM) is a Columbus rack-based facility designed to provide active vibration isolation for sensitive experimental payloads. This paper is intended to give an overview of the MGIM project currently being undertaken at the University College of North Wales. The paper describes the overall design of the facility and a description of its individual elements. Details of a preliminary study of a MGIM design to be accommodated in a Spacelab double rack are also described.

1. Introduction

Results from past microgravity experiments flown on Spacelab missions indicate that the vibration environment inside the laboratory often does not satisfy the stringent requirements of many of the experiments. Such experiments would derive substantial benefit if isolated in some way from the various sources of vibration.

The Microgravity Isolation Mount is a facility for providing vibration isolation for experiments in the Columbus Attached Laboratory and the Columbus Free-Flying Laboratory. The Facility is designed to be accommodated in a standard single rack and interfaces with existing rack utility services.

2. Mechanical Design

The design is based on a non-contact concept, whereby the payload floats inside the rack and its position is controlled by magnetic actuators. The main advantage of using this non-contact strategy is the improved microgravity quality obtainable.

The various elements of the MGIM Facility are illustrated in Fig. 1. The main elements and their functions are as follows :-

(i) A Platform unit, which is the main structural support for the payload, and which constitutes the isolated element.

(ii) A Liner unit, which accommodates the Platform and payload and interfaces mechanically and electrically with a standard Columbus rack.

(iii) A Cage unit, which encloses the Platform, and accommodates a pair of locking mechanisms which secures the Platform to the Cage during non-microgravity periods. These locking mechanisms operate automatically to release and re-lock the Platform. They are also designed to withstand launch and re-entry stresses. The Cage is attached to a slide system inside the Liner in order to enable easy withdrawal of the Platform in-orbit.

(iv) A Platform Supervisor, which holds the Platform control electronics. This unit also contains an electrical interface which supplies all electrical services to the Platform.

(v) A Payload Supervisor, located above the Platform Supervisor, which is responsible for monitoring and controlling the payload.

The above design was proposed after investigating the characteristics and requirements of several microgravity experiments [1]. It was concluded that for the majority of the experiments an input power supply of 1 kW would be adequate. However, the MGIM design described above requires that this power should be supplied in a non-contact manner. This is achieved by means of a special power transformer desribed in section 4. Control and data signals are transmitted between the Platform and Supervisor unit by means of a number of infra-red optical links, also described in section 4. Any heat produced by the experiment must also be dissipated in a non-contact manner, i.e. by thermal radiation. This influences the size of the Platform and Liner units as they must be large enough to dissipate the heat energy produced by the experiment.

A drawback of the non-contact strategy is that a considerable percentage of the available mass (and volume) budget may be consumed by the cooling panels required for thermal heat radiation. This will depend on individual microgravity experiments. However, in an extreme case, it has been estimated that almost 50 Kg of cooling panels would be required in order to provide a continuous heat dissipation of 1 kW [2]. Conversely, some experiments, notably fluid science experiments, might not require any cooling panels.

The MGIM design shown in Fig. 1 is based on a sub-unit type payload rack [3]. This type of rack has now been superceded by the Facility rack. Unlike the former, the Facility rack is not divided into individual payload sub-units (each with its own utility connections), and consequently has about 50% more payload volume. A Facility rack is therefore a more suitable rack for MGIM accommodation, and further development of the MGIM (for Columbus) should be based on this type of rack.

3. Platform Control

The Control Electronics Module, located in the Platform Supervisor, provides the control functions for the Platform. It accepts input signals derived from capacitance sensors, which define the position of the Platform in 6 degrees-of-freedom. It then executes the Platform control algorithm, and generates actuator drive signals. The control algorithm is based on position measurement using essentially a PID controller [4].

Position measurement using capacitance sensors requires the positioning of a pair of sensor plates at opposite faces of the Platform. The measurement of air-gap is performed by a 68 m.m. diameter capacitance sensor plate which is placed over the pole-pieces of the actuator. A guard ring is formed around the edges of the capacitance plate using printed circuit techniques in order to ensure the accuracy of the position measurement. The bridge circuits for the sensor are incorporated within the design by use of surface-mounted components on the underside of the PCB which is used to support the actuators. Each bridge circuit is driven with a precision square wave.

The output signals from a pair of these sensors are routed to a combining unit located on the top of the Cage. This produces a signal representing Platform position which is then passed to the control computer inside the Platform Supervisor.

The magnetic actuators are attraction-type devices, and each is made up of several pre-formed 0.65 m.m. annealed mild steel laminations. Each of the two coils consists of 300 turns of 26 s.w.g. enamelled copper wire. The force is generated by attracting a plate of soft magnetic material (the target plate) positioned on the Platform. Due to the inverse relationship between actuator current and size of air gap, prior compensation of the drive signal is carried out by the control computer in the Control Electronics Module. The compensated drive signals are then fed to the corresponding SAU.

Each measured displacement and corresponding restoring force thus needs the placement of two sensor units and two attraction actuators. To save space, a sensor and actuator are combined into a single Sensor/Actuator Unit (SAU). The use of a combined SAU yields the additional advantage that Platform position is now sensed at the actuator air gap, enabling precise linearistaion of the actuator law. The completed unit is compact, measuring 85 m.m. in width and 40 m.m. in height (Fig. 2). A minimum of 12 such SAUs is necessary to control a six degree of freedom Platform (3 translational, 3 rotational), i.e. 2 per degree of freedom).

The Control Electronics Module is also responsible for providing means of communication with on-Platform systems by means of optical links (section 4.2). It allows the user to adjust controller parameters via a front panel keyboard; provides the user with a real-time display of controller parameter values, system temperatures, alarm and status indicators; and controls the Platform locking mechanism.

4. Power and Data Transfer

In order to preserve the non-contact strategy, electrical power for the experiments on the Platform is supplied by a non-contact power transformer, whilst data and control signals are transmitted by a series of optical links.

4.1 Non-contact power transformer

The power transformer consists of a ferrite core with a tightly wound primary coil and a loosely coupled secondary coil. The primary winding, consisting of 14 turns of 14 s.w.g copper wire, is attached to the Cage, whilst the secondary, with 18 turns of 14 s.w.g has a clearance of 7 m.m. around the core in all directions and is attached to the Platform.

The transformer design is based on an input dc supply of 150 Volts and is capable of supplying 1 kW of power to the Platform at an identical output voltage. The primary winding is driven with a sinusoidal-wave derived from a bridge of power MOS transistors at a switching frequency of 90 kHz. The secondary is connected directly to a bridge rectifier and smoothing capacitor, which then feeds the load.

The control system provides a bias force at the nominal and all off-nominal Platform positions in order to counteract the residual forces produced between the transformer's primary and secondary members during periods of electrical load. Measurements of the magnitude of these forces have been carried out in order to verify theoretical predictions based on inductance measurements (Fig. 3). Experimental measurements and theoretical predictions are in good agreement, and it can be concluded that the stiffness values produced at full electrical load are within the limits which may be accommodated by the position-only method of control [5].

4.2 Non-contact data transfer

Non-contact data transfer is provided by a number of infra-red optical links. A single link consists of an emitter/receiver pair as shown in Fig. 4. Interference from ambient light, and from other data links, is provided by a pair of concentric tubes, one attached to the transmitter mounting, and the other to the receiver mounting. The spacing between the inner and outer tubes is sufficient to accommodate the sideways (radial) movement of the emitter relative to the receiver, and the length of the tubes is such that axial variations in the spacing can also be accommodated.

Previous data links, which were designed for transmitting instrumentaion data off-Platform during vibration isolation tests, are limited to a maximum bit rate of 100 kbit/s [6]. Work is in progress to update this specification and currently a 25 Mbit/s rate has been achieved.

5. Heat Dissipation

Heat energy produced on the Platform is dissipated to the Liner by means of radiative cooling panels. The cooling fins on each panel have a thickness of 2 m.m. and allow the Platform a 10 m.m. freedom of movement along each translational axis. It is predicted that a continuous heat dissipation if 1 kW is possible if the MGIM is accommodated in a Facility rack [5].

A thermal test rig has been assembled in order to test the heat transfer properties of a representative cooling panel. The hot and cold fins are set apart at the required distance of 10 m.m., as shown in Fig. 5, and thermocouples are attached along the lengths of both fins in order to measure the heat transfer characteristics. The fins are enclosed in a large evacuated bell jar in order to simulate heat transfer in the gravity-free environment of space.

An example of some of the experimental and theoretical results is shown in Fig. 6. In this case, the hot and cold fins have been given a matt black finish. The theoretical results therefore assume 100% emmissivity. A good comparison between theoretical and experimental results is observed.

6. Vibration Testing

A full-scale mock-up of the MGIM Facility has been constructed, and is currently being adapted to enable vibration isolation tests to be performed. The vibration isolation characteristics of the facility will be tested with the Platform installed inside the rack. The rack itself is mounted on a pair of precision linear slides and connected, via an A-frame linkage, to an electromagnetic vibrator. The MGIM can thus be subjected to a prescribed vibration specification along a single axis.

The Platform is supported on an air table and is free to move, with three degrees of freedom, within the moving confines of the rack. The tests will be confined to a single translational axis, but will include both sinusoidal and stochastic inputs. In addition, tests on Platform release in the presence of rack vibration will be performed.

The Platform Supervisor and Liner are shown installed in the rack in Fig. 7, with the Platform and Cage in the foreground.

7. Double Rack MGIM Facility

During the development of the MGIM, it soon became apparent that the narrow width of a 19 inch (483 m.m.) rack was a major design constraint limiting useful payload width. The information presented in Fig. 8 shows that the available payload width is further reduced once all the MGIM's sub-systems have been accommodated.

A preliminary study of a double rack version of the MGIM was therefore initiated and has recently been completed [7]. This study, however, is based on accommodating the MGIM in a double Spacelab rack, with a view to a possible flight on one of the Columbus precursor missions during the mid to late nineties.

The MGIM is accommodated as shown in Fig. 9. The Platform and Liner the occupy the main volume of the rack, whilst the Plaform and Payload Supervisors occupy half the "neck" volume. The Platform and Liner units are shown in Fig. 10.

The Platform has been designed in order to utilise the maximum possible volume available inside the rack, thereby ensuring maximum experiment volume on the Platform. It consists of two horizontal plates, set at a distance of 70 m.m. from each other, and enclosed in a framework of extruded aluminium square sections.

The upper and lower surfaces of the Platform have been reserved for cooling panels. In the event that these panels are not required, then the Platform height can be increased accordingly. Alternatively, if the heat dissipation capacity provided by the present sized panels is not sufficient, then the Platform height may be reduced, thereby allowing increased fin lengths.

The Liner is constructed from an aluminium framework and is designed to slide out of the double rack in order to provide complete access to the Platform and payload. Installation of the Platform and its payload is undertaken through the top of the Liner after removing the upper cooling panel and its support structure. As the Platform is lowered into its correct position, it is secured at its corners to the Liner by a number of high tensile steel bolts.

During launch and re-entry the Platform will be secured to the Liner at its corners by means of several high tensile steel bolts. Prior to the start of the microgravity phase of the mission, the Liner will be withdrawn from the rack. The automatic locking mechanism will then be operated so that it engages with the Platform. The high tensile bolts are then withdrawn manually and the Liner returned to its original position inside the rack. During this time the Platform will be secured to the Liner by the automatic locking mechanisms. These automatic locking mechanisms will be unlocked at the required time.

All SAUs, apart from the y units, are located in the gap around the central periphery of the Platform. The power transformer, optical links and their associated drive electronics could be similarily located.

As a consequence of U.C.N.W. work, ESA is about to place "Phase B" studies for the definition of a double rack MGIM for flight on a Spacelab SSF precursor mission. These studies are planned for completion in February 1992.

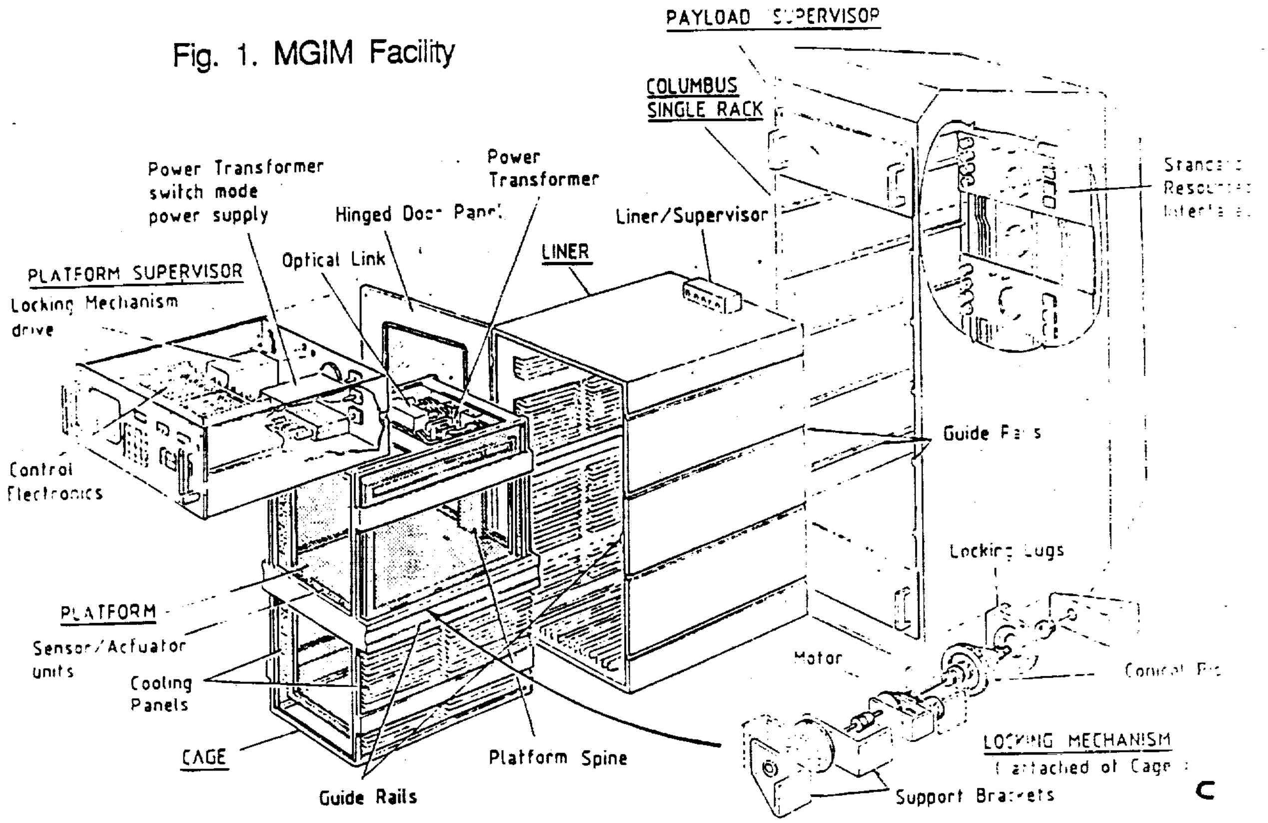
Acknowledgement

The work presented here was performed as part of ESTEC contract No. 7637/88.

References

1. Owen R.G., Jones D.I., Owens A.R.
"Columbus Applications Study", Report BTN 001, University of Wales, September 1988,
2. Owen R.G., Roberts G.
"Detailed Definition of Concept", Report BTN 005/B, University of Wales, January 1990.
3. Owen R.G., Jones D.I., Owens A.R.
"Integration of a Microgravity Isolation Mount within a Columbus Single Rack", Acta Astronautica, Vol. 22, 1990.
4. Jones D.I., Owens A.R., Owen R.G.
"A Microgravity Facility for in-orbit experiments", Active Noise and Vibration Control Symposium of the 111th ASME Winter Annual Meeting, Dallas, Texas, November 1190.
5. Jones D.I., Owens A.R., Owen R.G., Roberts G.
"Microgravity Isolation Mount, Design Report" for ESTEC Contract No. 7637/88, September 1989.
6. Jones D.I., Owens A.R., Owen R.G., Roberts G.
"Microgravity Isolation Mount, Final Report", for ESTEC Contract No. 6380/85, May 1987.
7. Owen R.G., Jones D.I., Owens A.R.
"MGIM Accommodation in a Spacelab Double Rack", Report BTN 012, University of Wales, Bangor, January 1991.

Fig. 1. MGIM Facility



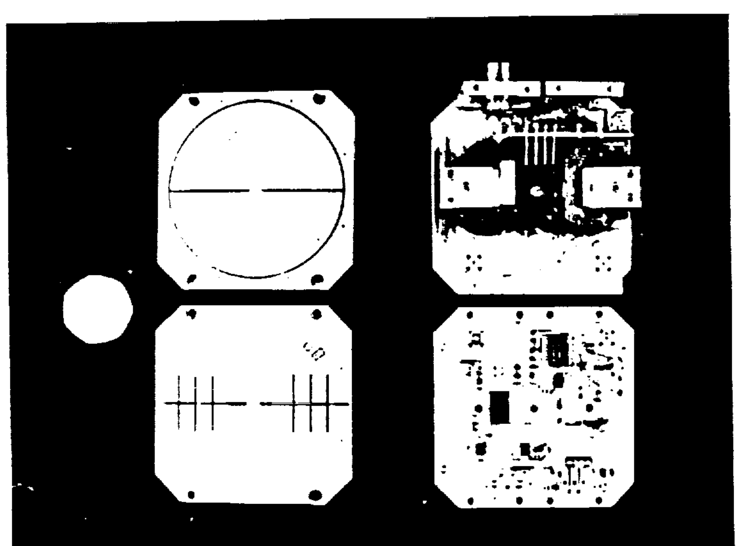
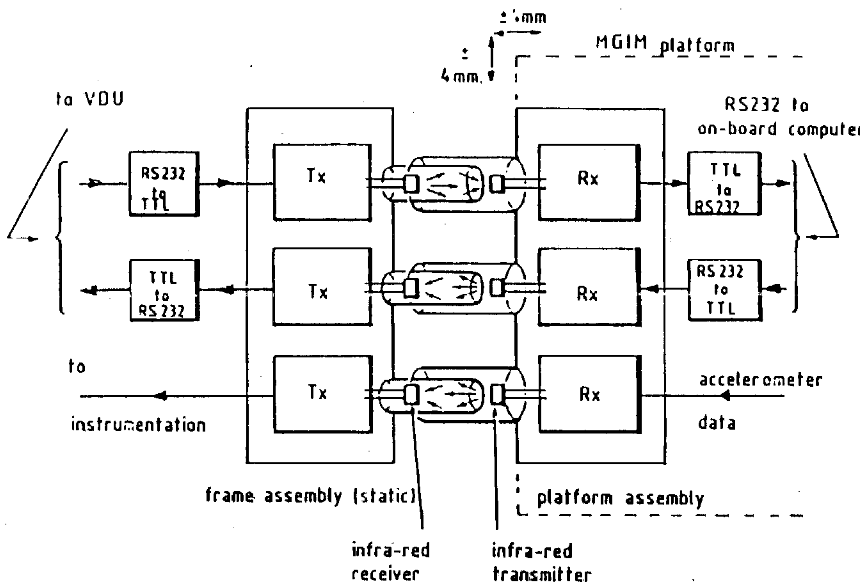


Fig. 2. Sensor Actuator Unit (SAU)



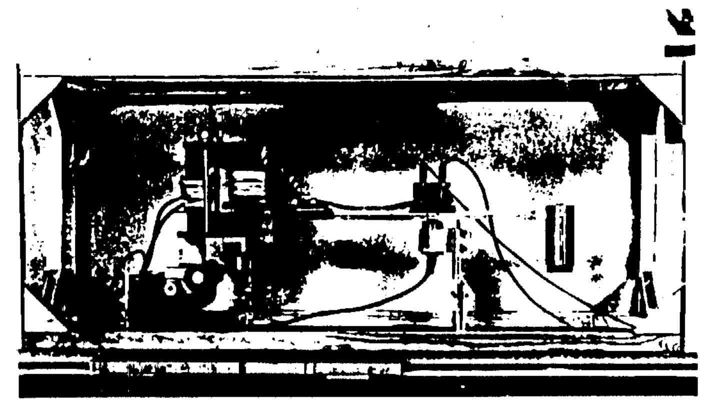


Fig. 3. Test Rig for measurement of power transformer residual forces

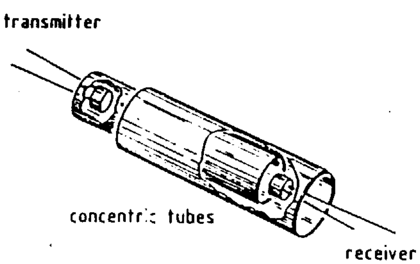


Fig. 4. Optical Link

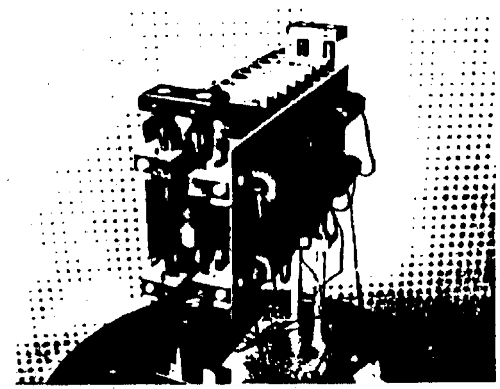


Fig. 5. Cooling Fins

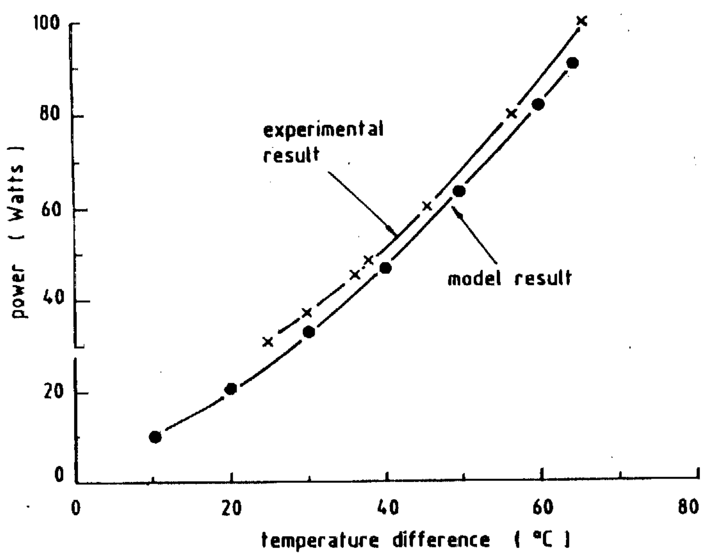


Fig. 6. A comparison of theoretical and experimental heat transfer results

Rack type		Platform Dimensions (mm)			Platform volume (m^3)
		Width	Depth	Height	
Columbus	Single rack				
Columbus	Sub - unit	311	500	667	.104
Columbus	Facility	311	759	667	.157
Columbus	Double rack				
Columbus	Facility	909	759	405	.278
Spacelab	Double rack	882	407	829	.298

Fig. 8. Platform dimensions and volumes inside various payload racks

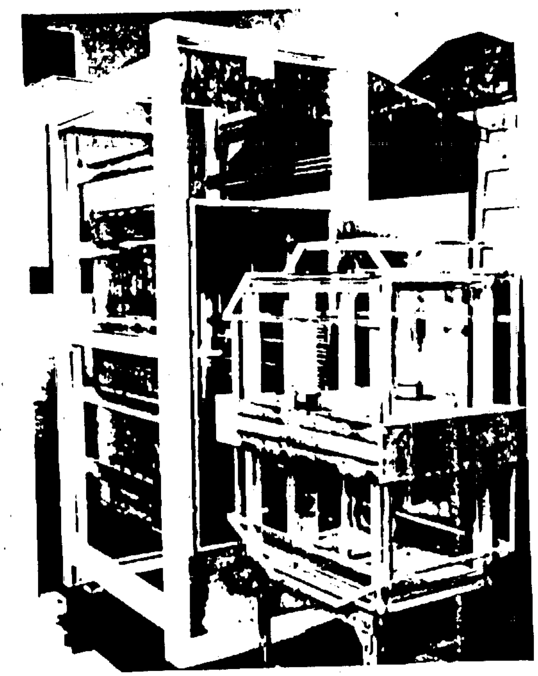


Fig. 7. A view of the MGIM Facility

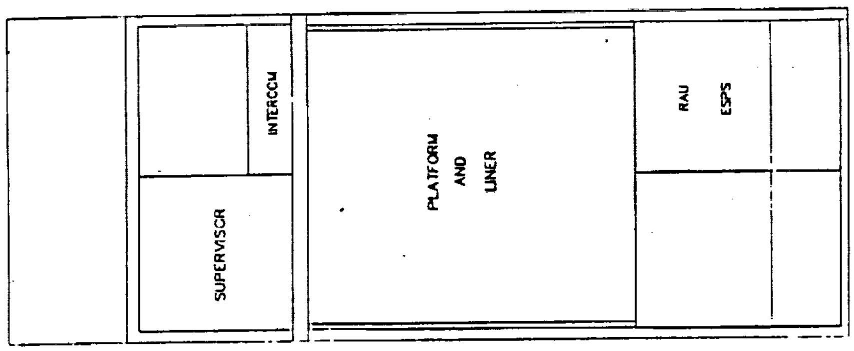


Fig. 9. MGIM Accommodation in a Spacelab Double Rack

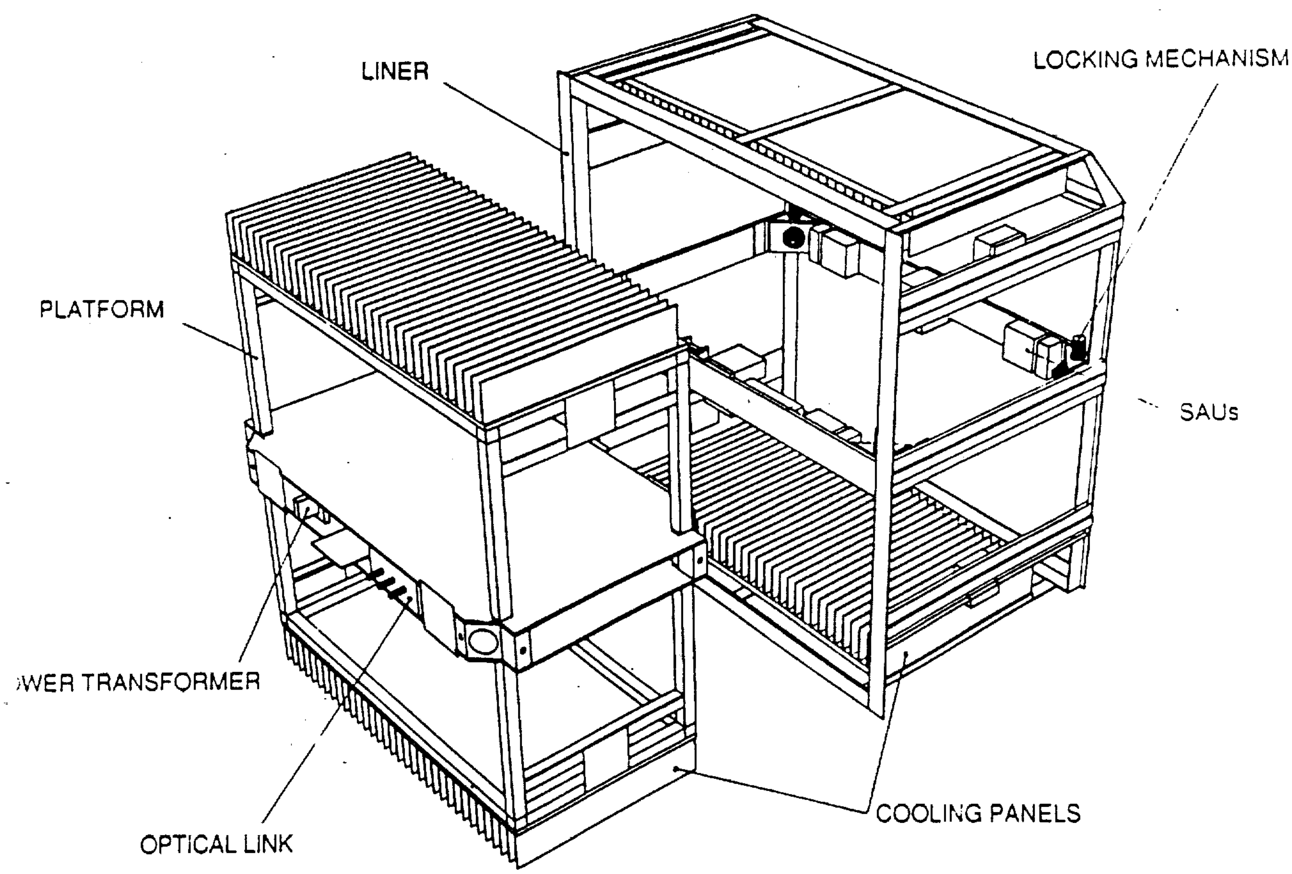


Fig. 10. Double Rack Concept

Coleg Prifysgol Gogledd Cymru
University College of North Wales

Ysgol Gwyddor Peirianneg Electronaidd
Stryd y Deon Bangor, Gwynedd LL57 1UT

School of Electronic Engineering Science
Dean Street Bangor, Gwynedd. LL57 1UT
Ffôn/Tel Bangor (0248) 351151 ext
Telex: 61100 Fax No. 0248 361429

THE MICROGRAVITY ISOLATION MOUNT

- a Columbus facility for improving the microgravity quality of payloads

R. G. Owen
D. I. Jones
A. R. Owens
G. Roberts
P. Hadfield

School of Electronic Engineering Science
University College of North Wales
Bangor, Gwynedd, Wales, U.K.

CONTENTS

A. BACKGROUND TO MGIM DEVELOPMENT AT BANGOR

B. DETAILED DESCRIPTION OF MGIM FACILITY

C. DOUBLE RACK MGIM

Feasibility Study

Title: Definition and Experimental Study of a Microgravity Isolation Mount

Contract Details

Date: October 1985 - February 1987

Contract No. ESA 6380/85

Price: approx. 100 kAU

A. BACKGROUND TO MGIM DEVELOPMENT AT BANGOR

Feasibility Study

- Contract Details
- Contract Outline
- Technical Specification

Present Contract

- Contract Details
- Contract Outline
- Technical Specification

Feasibility Study

Technical Specification:

Dimensions: 1m x 1m x 1m max.

Mass: 50 - 200 Kg

Mass centre: up to 0.2 m displacement from geometric centre

Power and data transfer

Launch/landing lock

Vibration isolation performance

Feasibility Study

Contract Outline:

Phase 1 : Design

Phase 2 : Design Verification

Present Contract

Contract Outline

Phase 1: Design

Phase 2: Hardware Manufacture and Test

Present Contract

Title: Microgravity Isolation Mount Finalisation of 6 d.o.f. design and breadboarding of major sub-systems

Contract Details

Date: May 1988 - August 1991

Contract No. ESA 7637/88

Price: 400 kAU

B. DETAILED DESCRIPTION OF MGIM FACILITY

Columbus Applications Study
Columbus Racks
Overview of MGIM
- Main Elements
- Sub-system Elements

Sub-system Elements
- Locking Mechanism
- Sensors and Actuators
- Power and Data Transfer
- Heat Dissipation

Vibration Isolation Testing

Present Contract

Technical Specification

Dimensions: to be accommodated in a standard Columbus payload rack

Mass: 200 Kg. (single)
400 Kg. (double)

Power and data transfer

Detailed design and manufacture of launch/landing lock

Heat dissipation - up to 1 kW

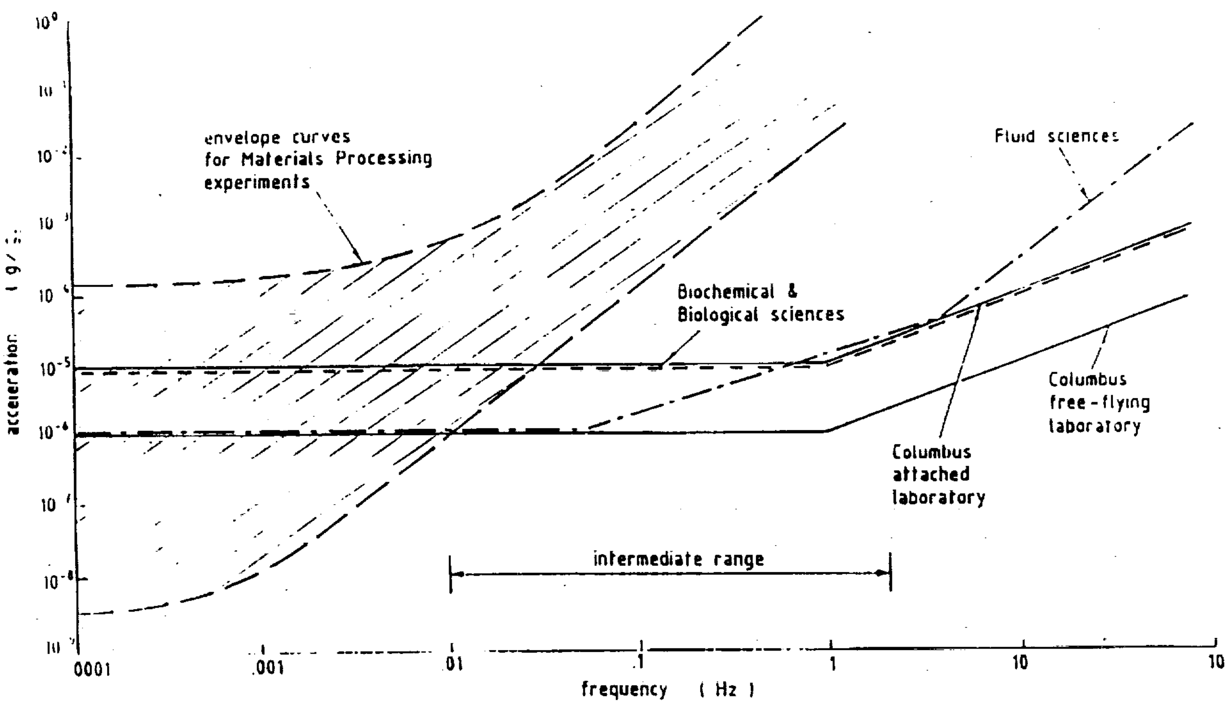
Electrical (wiring specification and routing)

Vibration isolation performance

Columbus Applications Study

Microgravity Experiments

Acceleration Sensitivity
Mass, Volume
Power
Cooling
Data
Vacuum/Venting

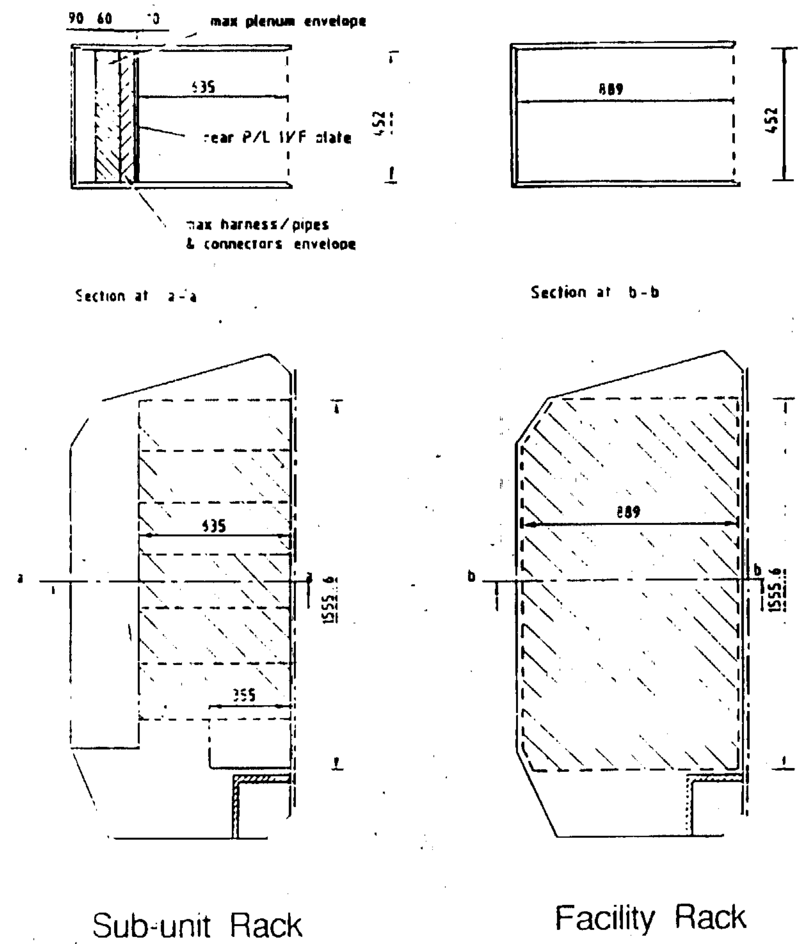


Acceleration Sensitivity of Microgravity Experiments

Overview of MGIM

Main Elements

Platform
Liner
Cage
Platform Supervisor
Payload Supervisor



Columbus Racks

Overview of MGIM

Sub-system Elements

Locking Mechanism

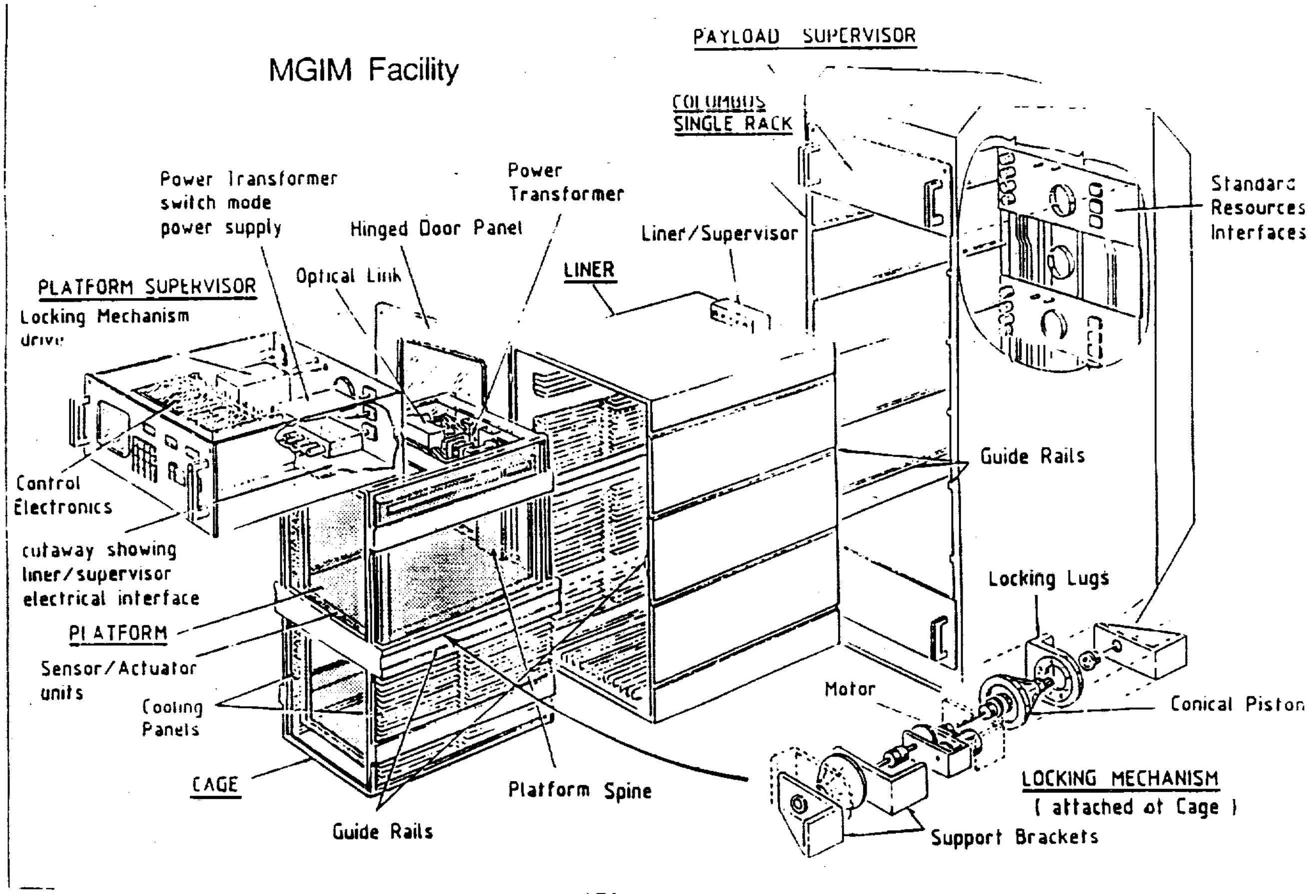
Sensors and Actuators

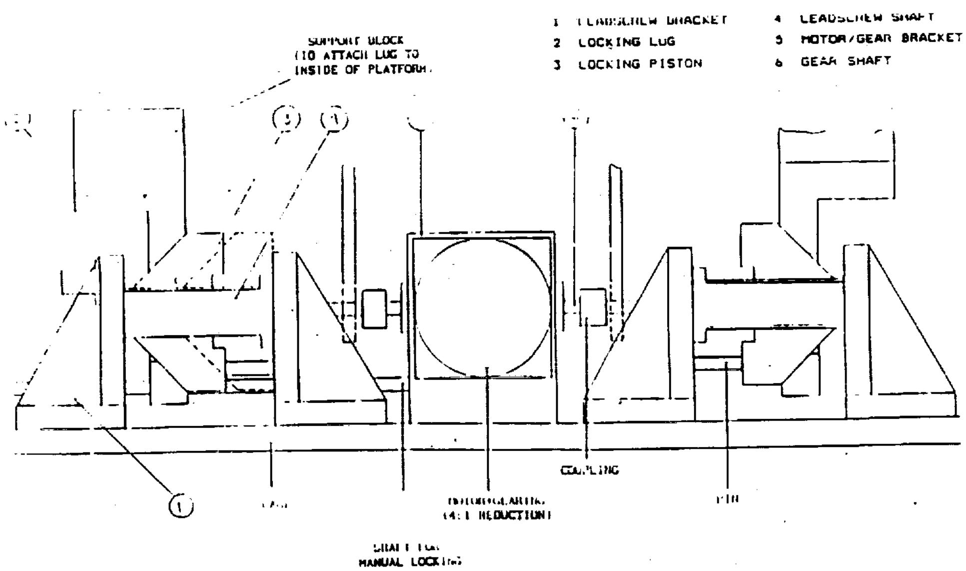
Cooling

Power Transformer

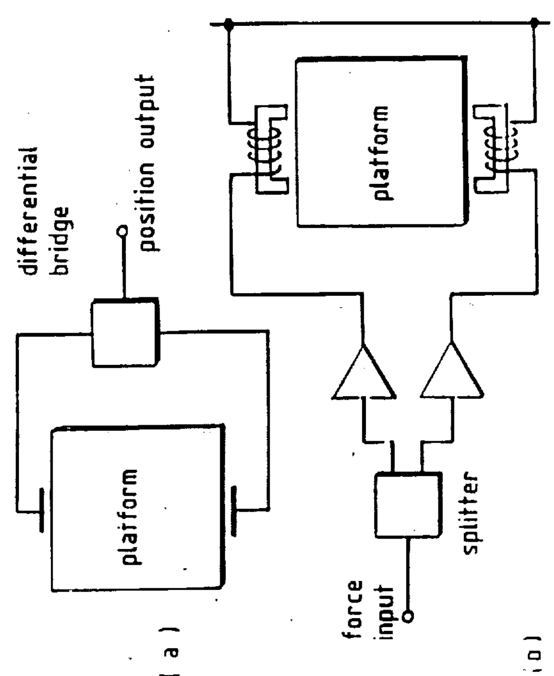
Optical Link

MGIM Facility

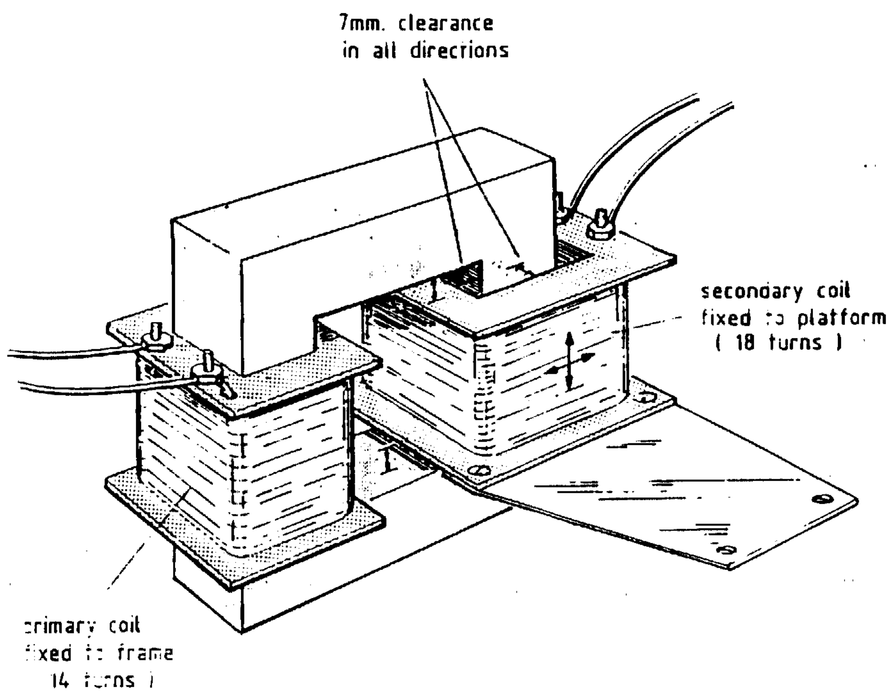




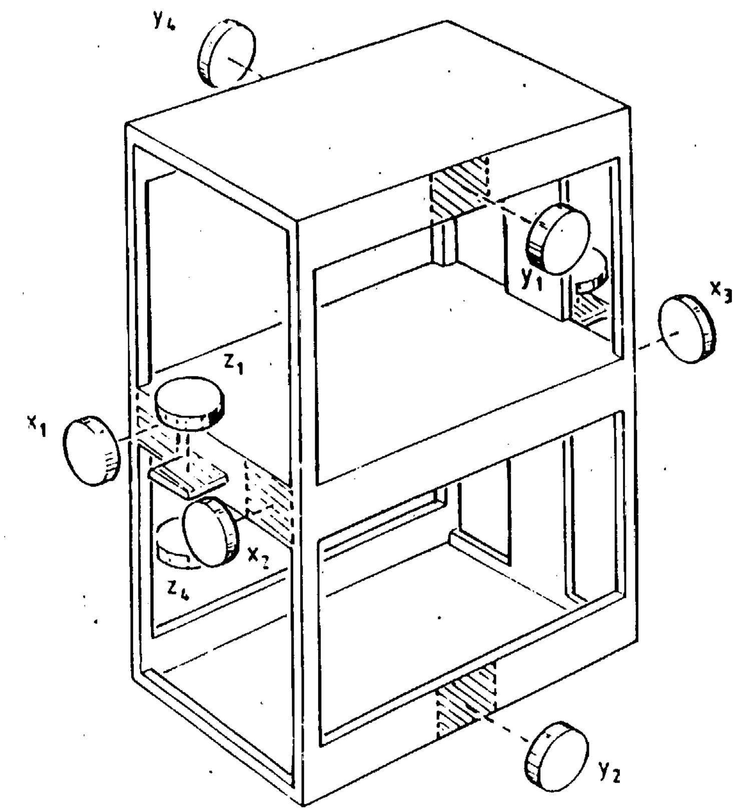
Locking Mechanism



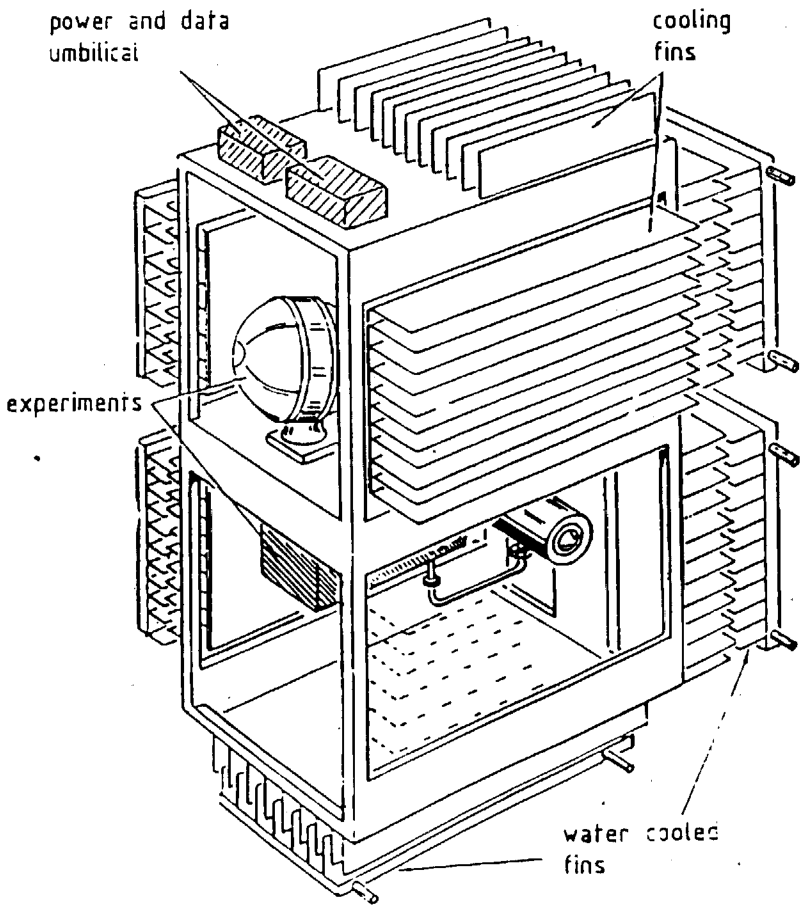
Platform Control
- using Sensors and Actuators



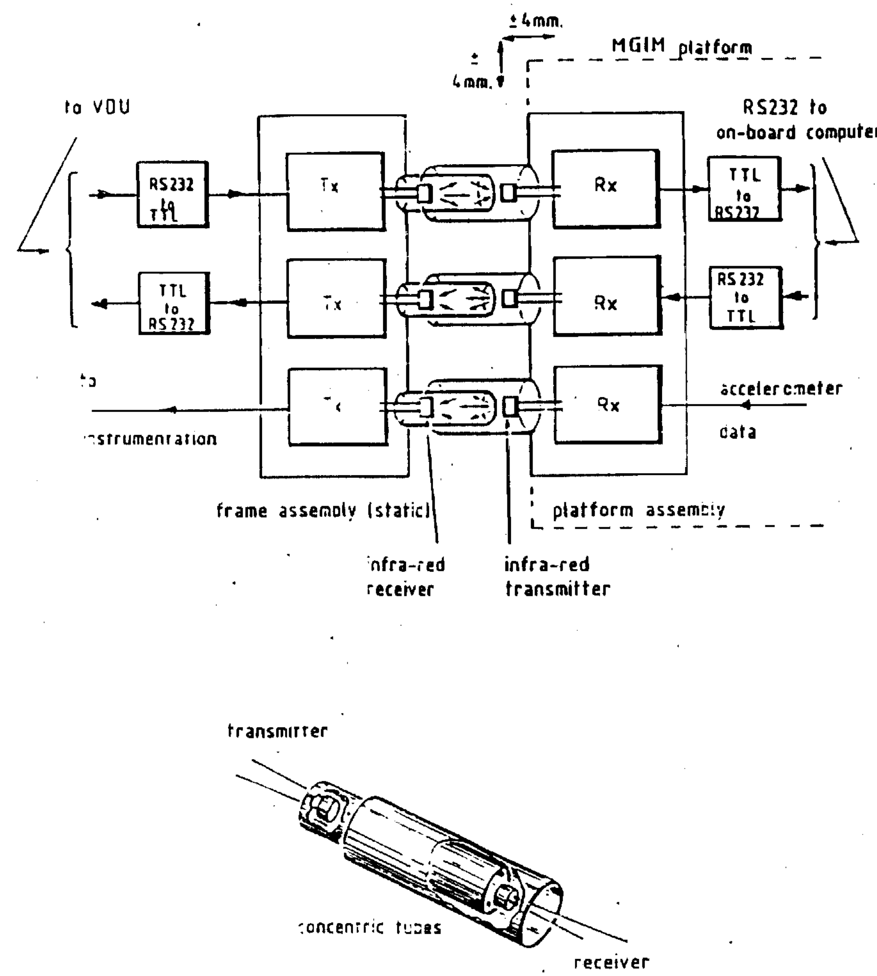
Non-contact Power Transformer



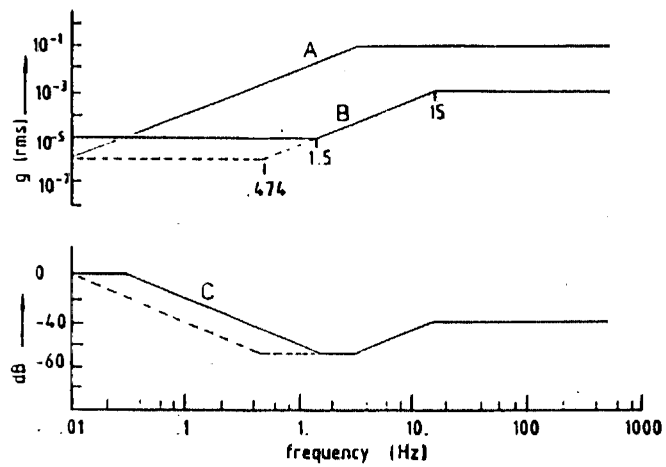
Location of Sensors and Actuators



Cooling Fins



Optical Link



Curve A - Estimated spacecraft vibration at payload interface.
Curve B - Allowable payload microgravity level.
Curve C - Transmissibility function for MGIM

Vibration Specification

Unit	Mass (Kg)
Payload Supervisor	15
Platform Supervisor	15
Platform: Central Plate (16) Frame (4) Payload (60) Cooling Panels (22) Sensors, Actuators, Power Transformer, Optical Link (6)	108
Liner: Frame (20) Cooling Panels (27) (Inc. Cold Plates) Cage and Locking Mechanism (15)	62
Total	200

Mass Distribution of MGIM Rack

Unit	Mass (Kg)
Payload on Platform	60
Central Plate	16
Payload Supervisor	15
Total	91

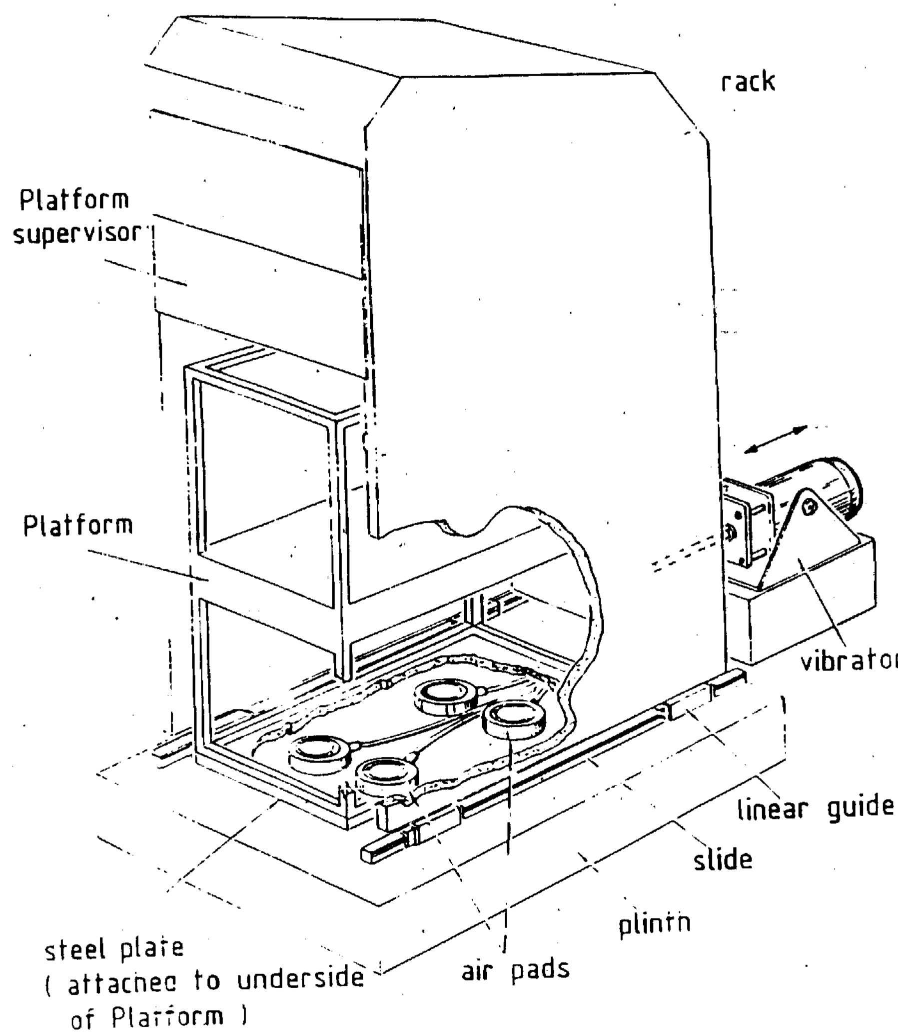
Total Payload Mass

C. DOUBLE RACK MGIM

Platform Dimensions and Volumes inside single and double Columbus racks

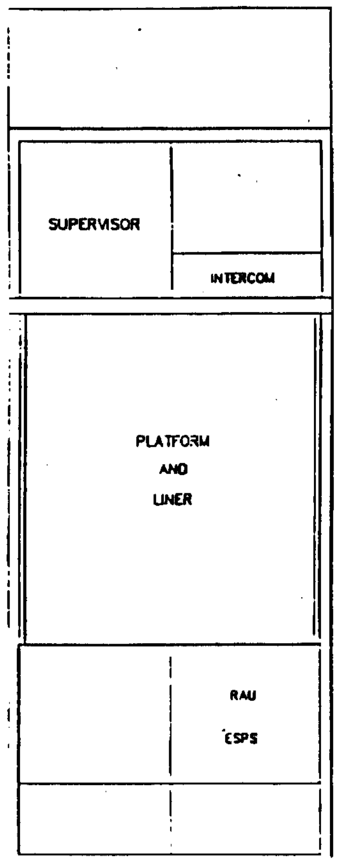
MGIM Accommodation in a Spacelab Double Rack

Double Rack Concept



Cage and Liner omitted for clarity.

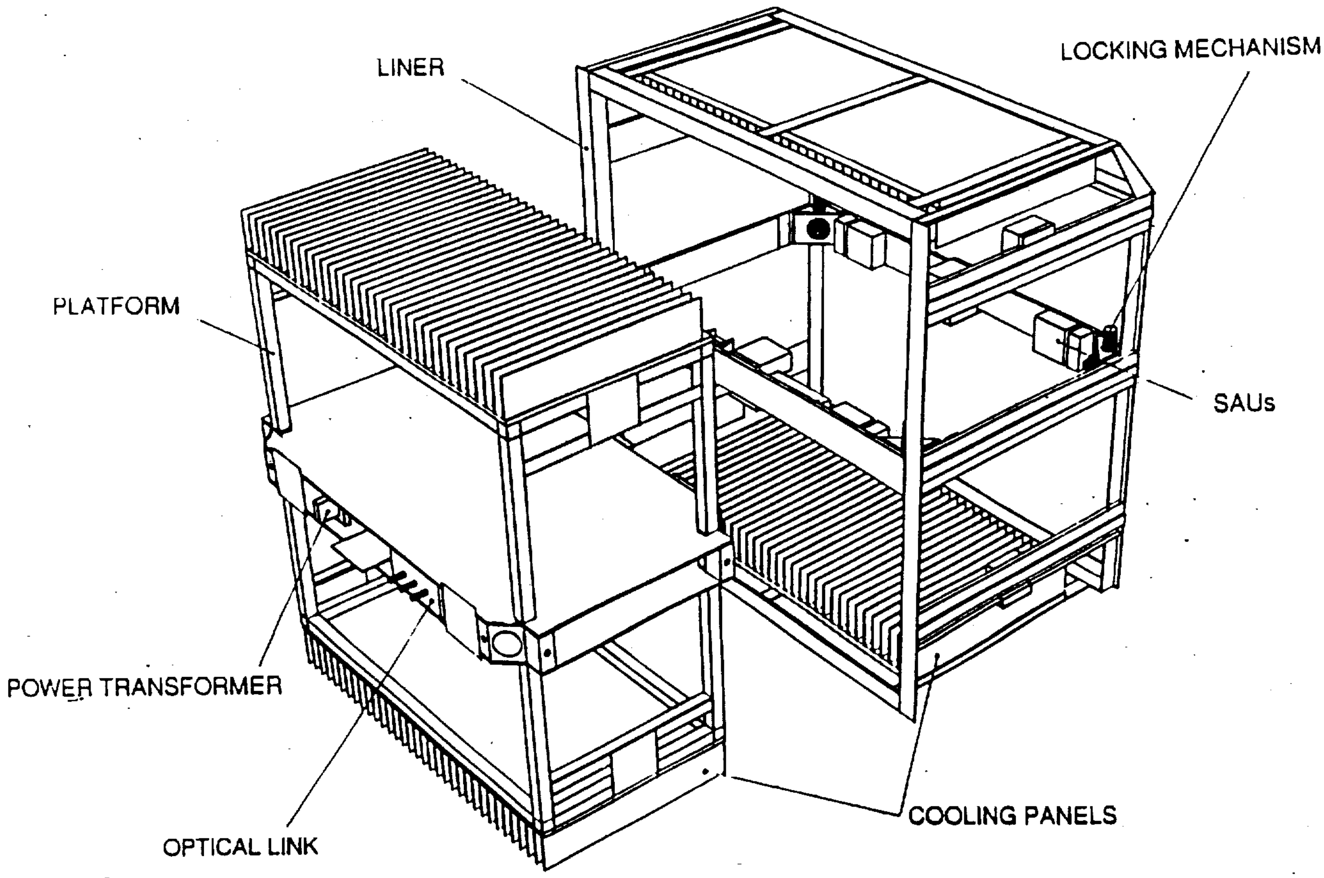
Vibration Test Rig



MGIM Accommodation in
a Spacelab Double Rack

	Rack type	Platform Dimensions (mm)			Platform volume (m^3)
		Width	Depth	Height	
Columbus	Single rack				
	Sub-unit	311	500	667	.104
	Facility	311	759	667	.157
	Double rack				
	Facility	909	759	405	.278
Spacelab	Double rack	882	407	829	.298

Platform Dimensions and Volumes
inside various payload racks



Double Rack Concept

N92-28440

KC-135

LARGE MOTION ISOLATION MOUNT

(LMIM)

Bjarni V. Tryggvason

Canadian Astronaut Program Office

Canadian Space Agency

Ottawa, Canada

Canadian Space Agency
Canadian Astronaut Program
April 22, 1991
Bjarni V. Tryggvason

Sponsoring Agencies:

Canadian Space Agency

NASA

Participants:

CSA: User Development Program

Canadian Astronaut Program Office

University of British Columbia:

Engineering Physics Project Laboratory

Department of Electrical Engineering

NASA: Microgravity Science

MSFC

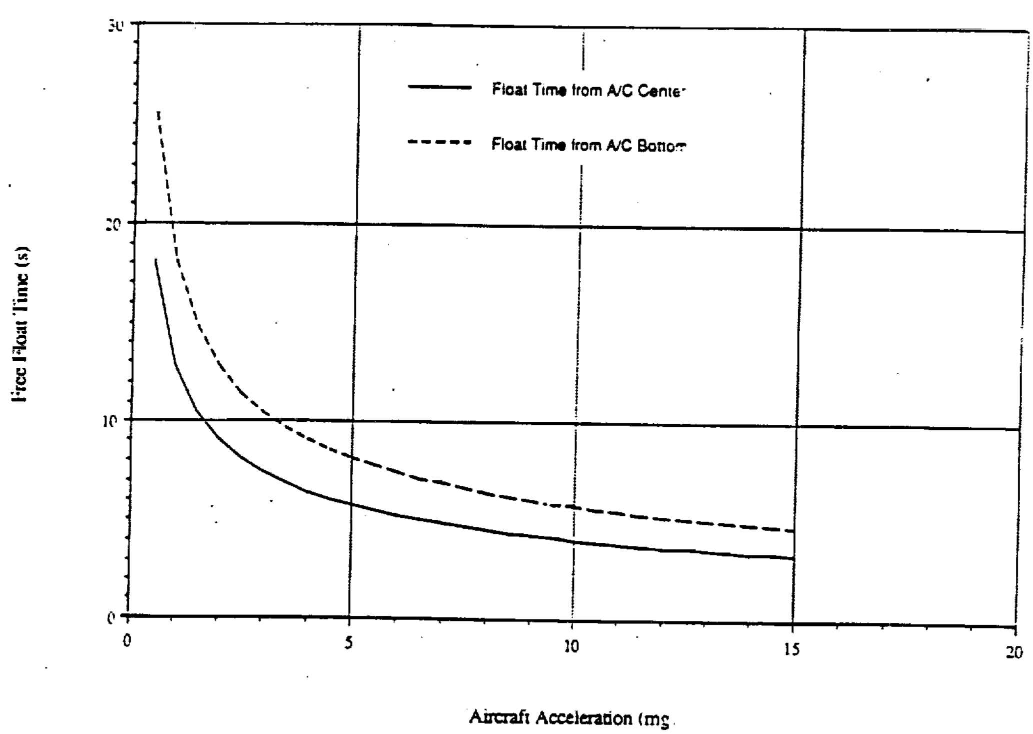
JSC Zero-g Office

Cost of Low-G

Vehicle	Time Available	Quality	Cost per Kg-sec	Experiment cost
KC-135	20 s	2×10^{-2}	$ 0.10	$ 50K
Drop Tower	5 s	10^{-5}	$ 1.00	$ 100K+
Rocket	7 min	10^{-5}	$ 40.00	$ 500K+
STS	hours	10^{-3}	$ 0.10	$ 2.5 M+
SSF	days	10^{-4}	$ 0.10	$ 5.0 M+

Canadian Space Agency
Canadian Astronaut Program April 22, 1991 Bjarni V. Tryggvason

Effect of Aircraft Acceleration Level on Free Float Time



PROGRAM ELEMENTS:

Studies of acceleration levels on the KC-135

Computer simulations of KC-135 flights and operation of the LMIM

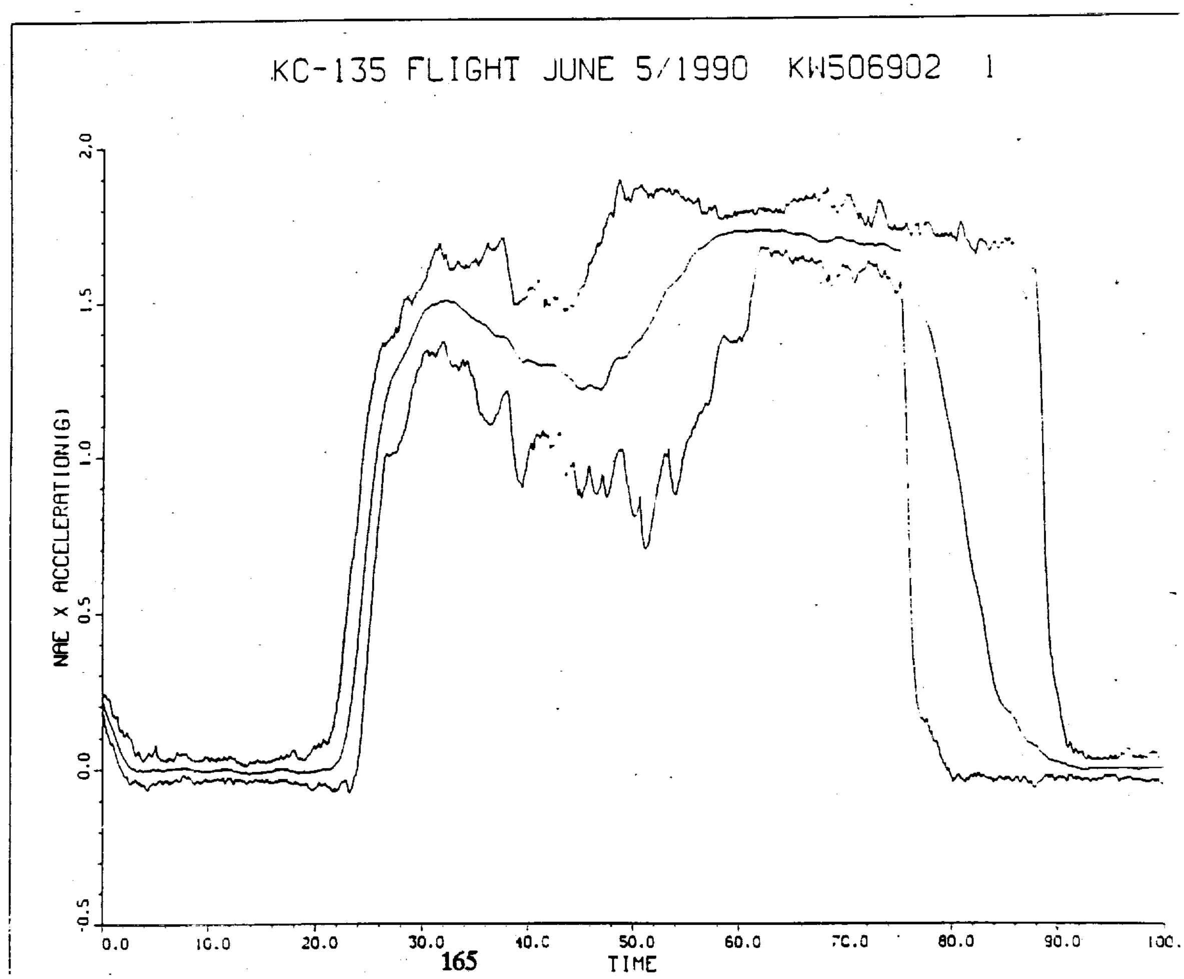
Simulation of control strategies for the LMIM

Development of proof of concept hardware

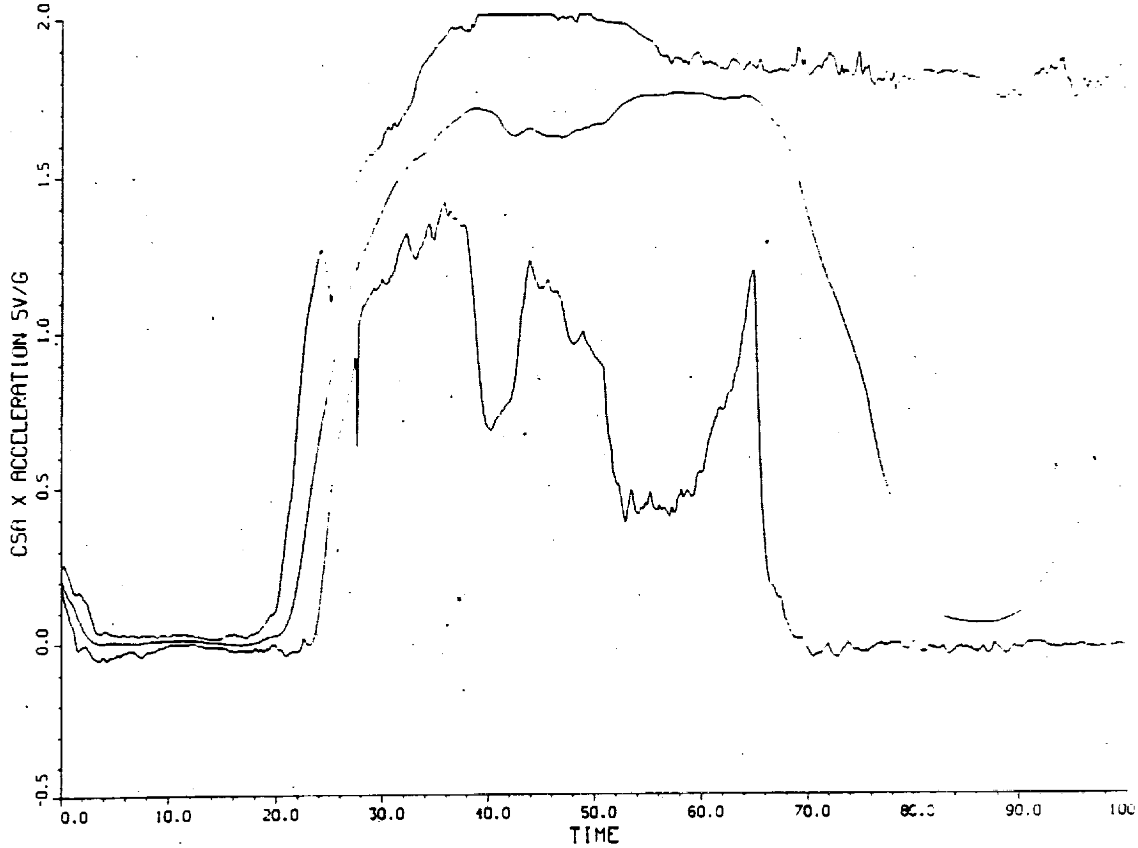
- 1-DOF system
- Control system
- 3-DOF system
- 6-DOF system

KC-135 flight tests

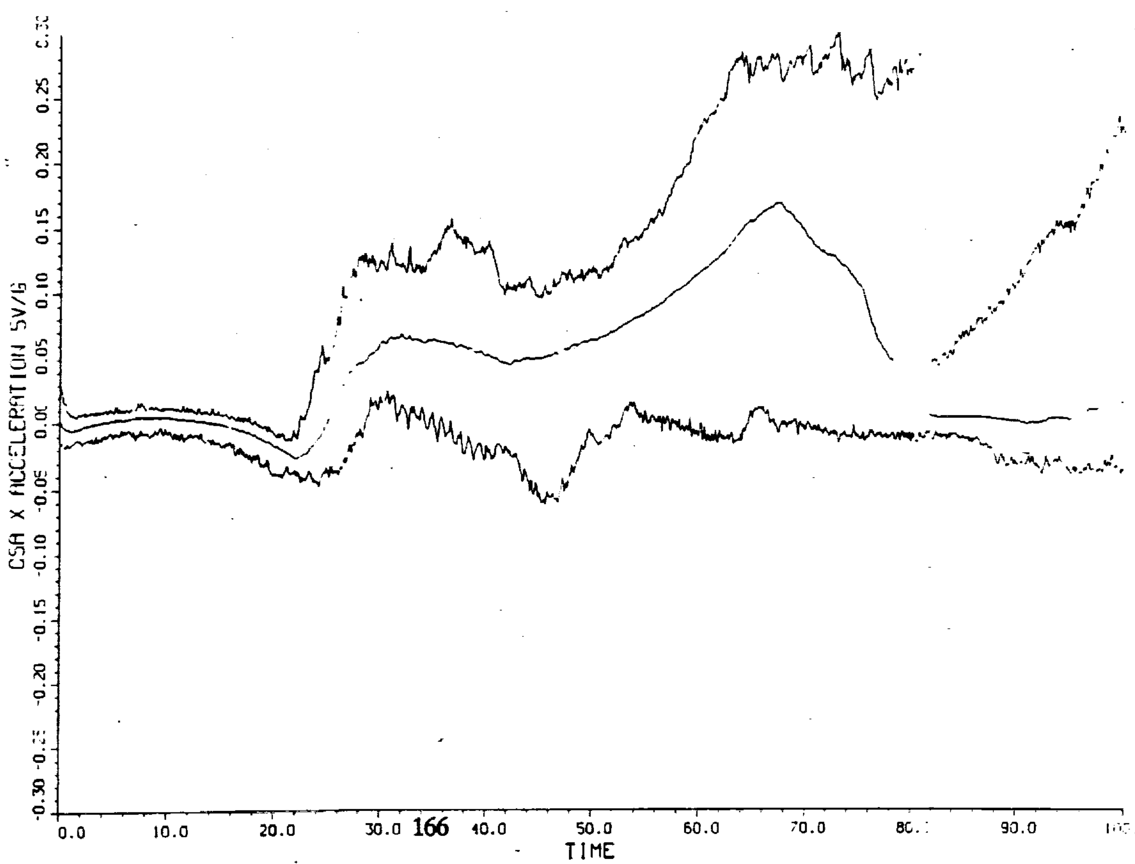
Development of working hardware

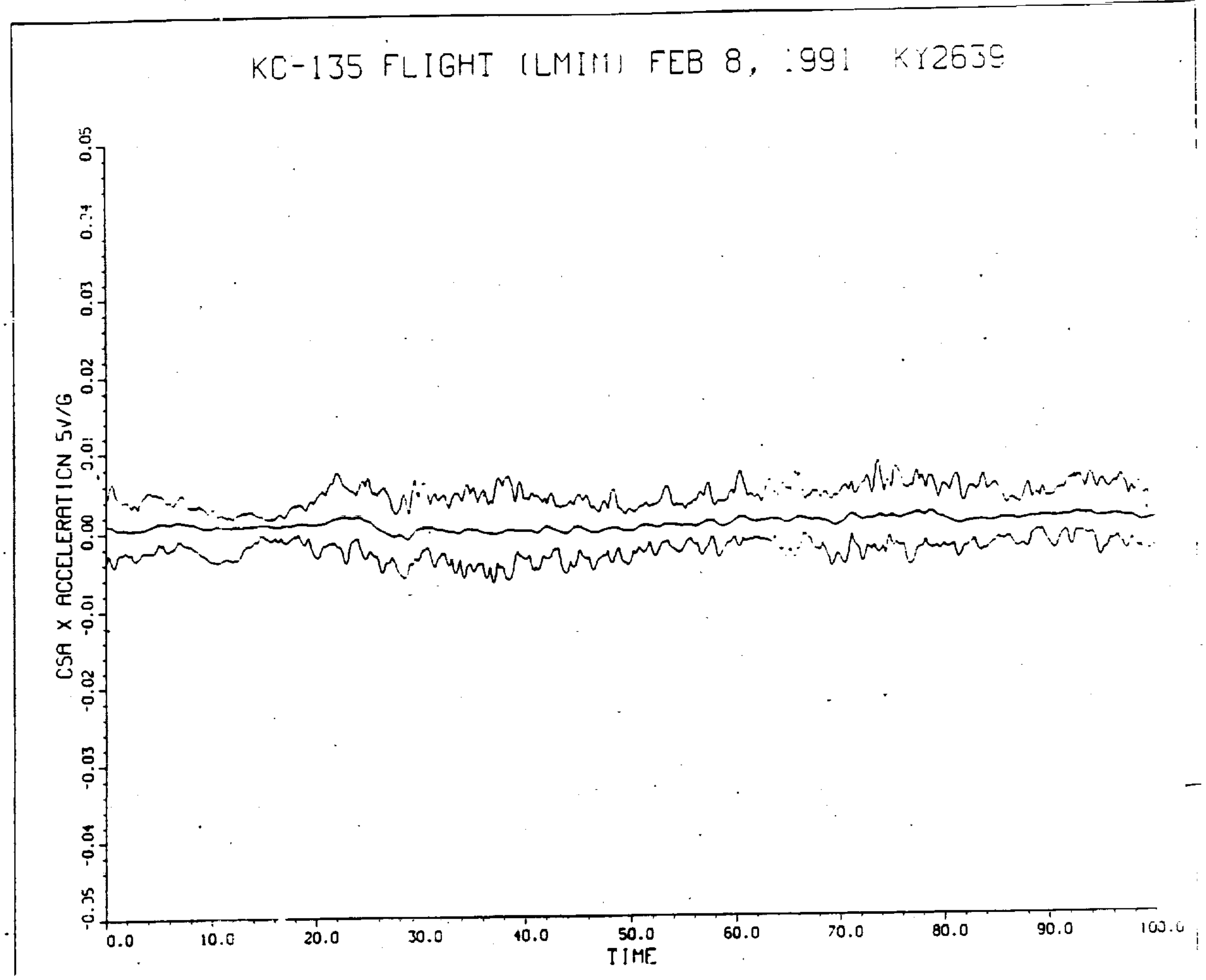


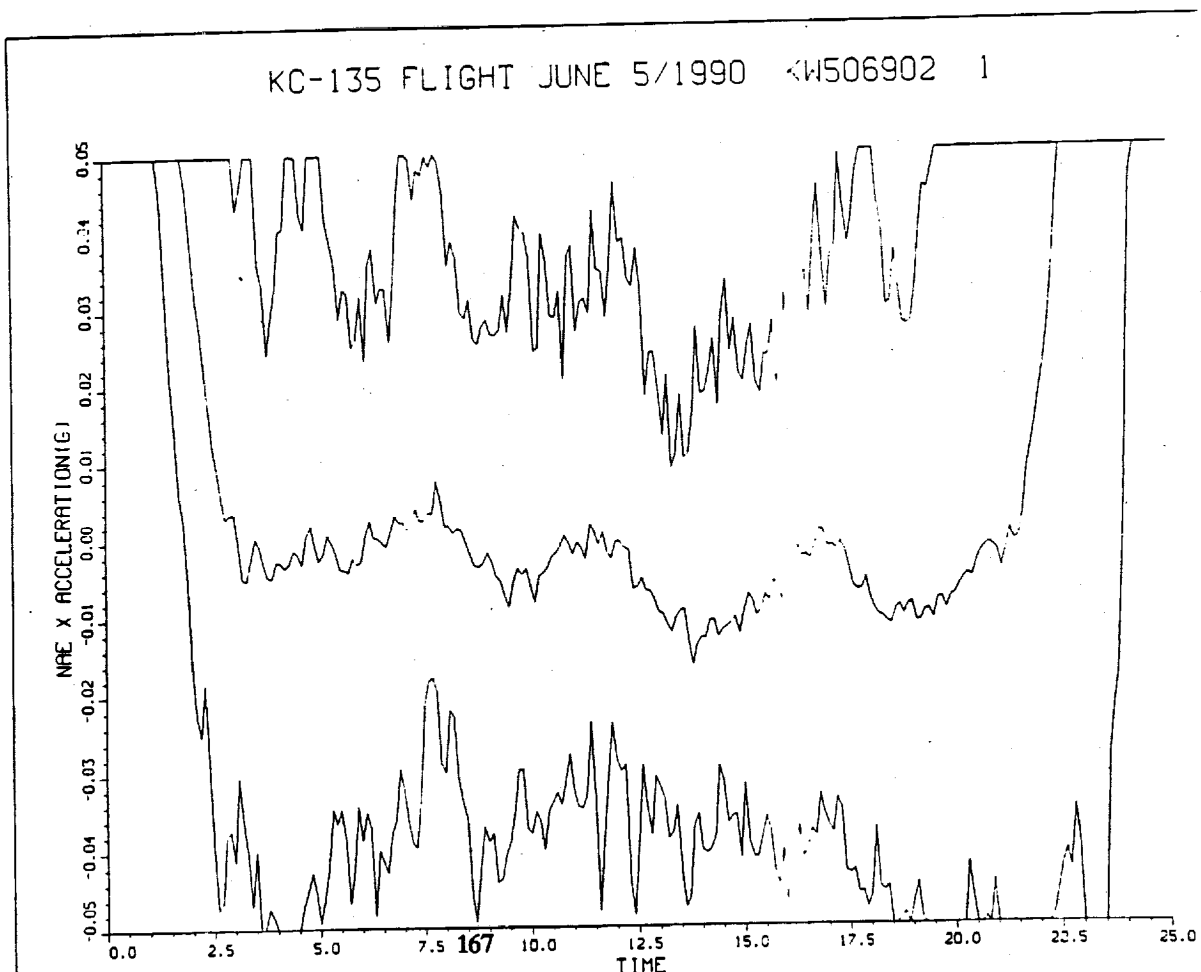
KC-135 FLIGHT (LMIM) FEB 8, 1991 KW2639

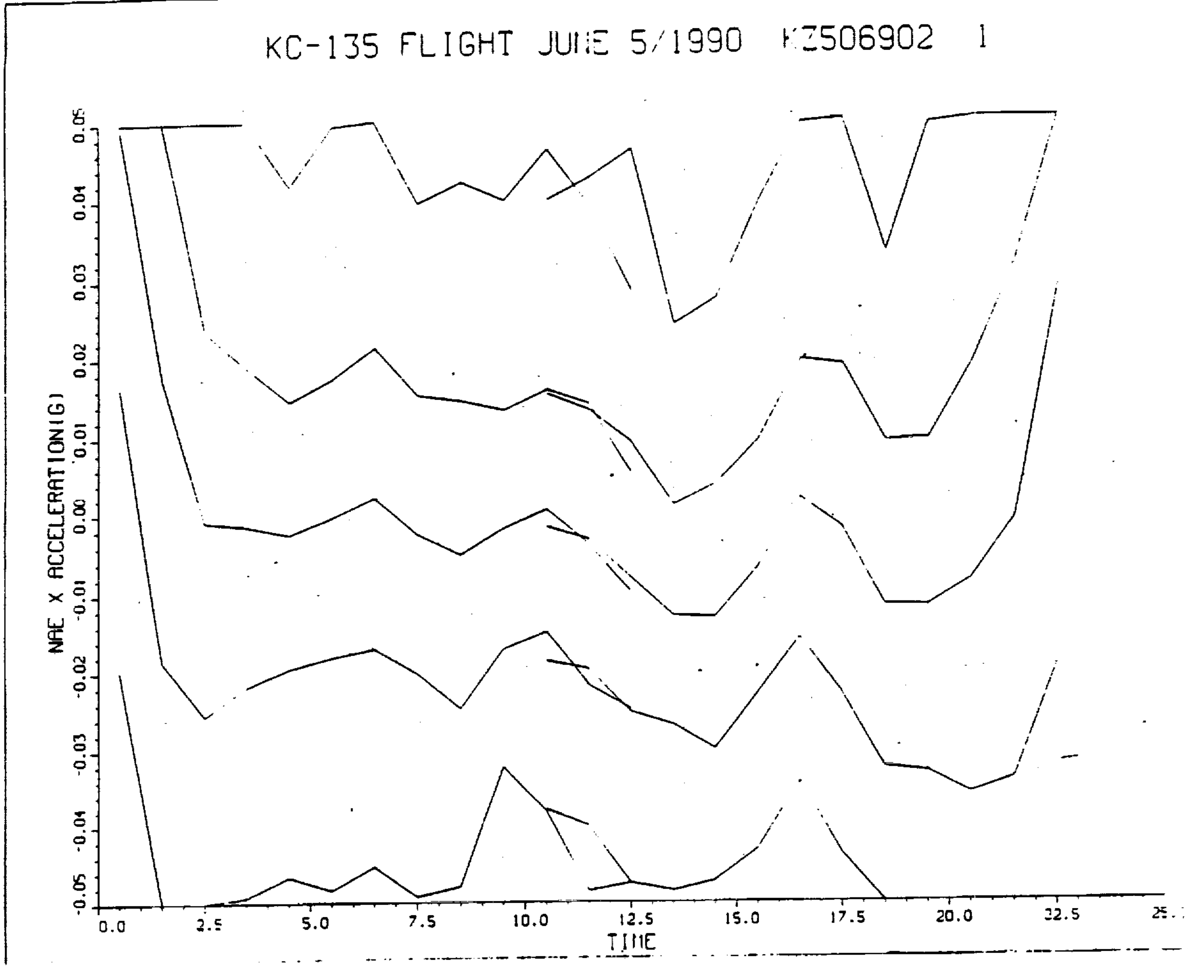


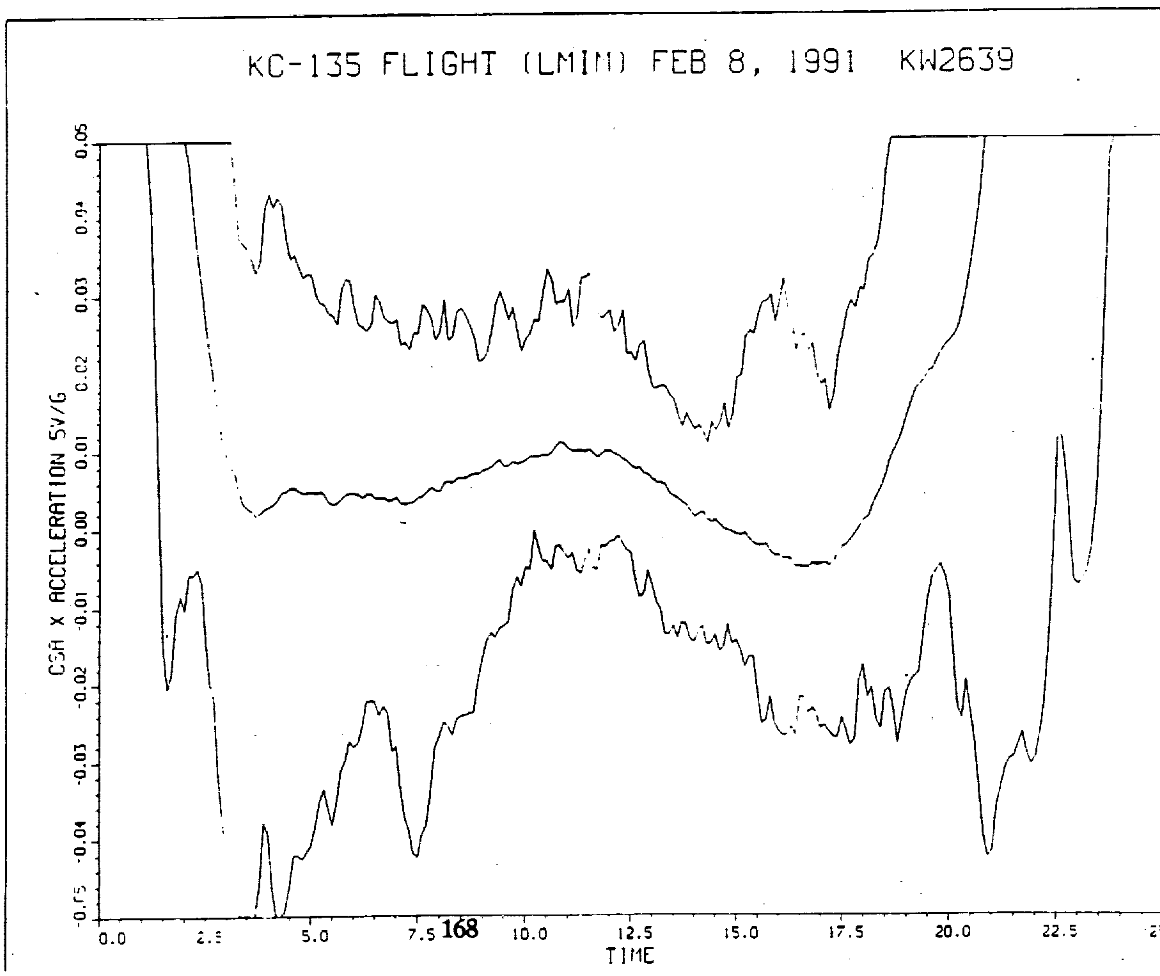
KC-135 FLIGHT (LMIM) FEB 8, 1991 KW2639

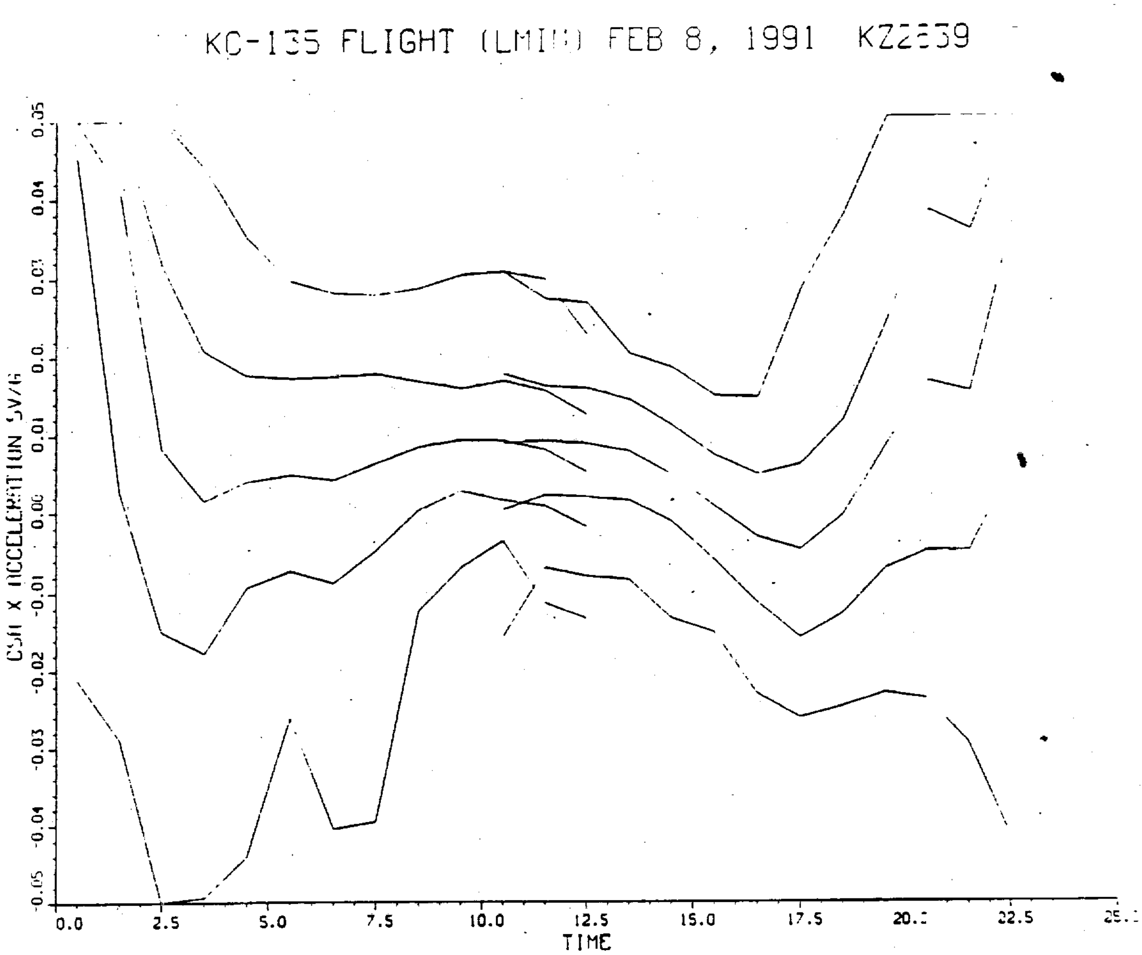


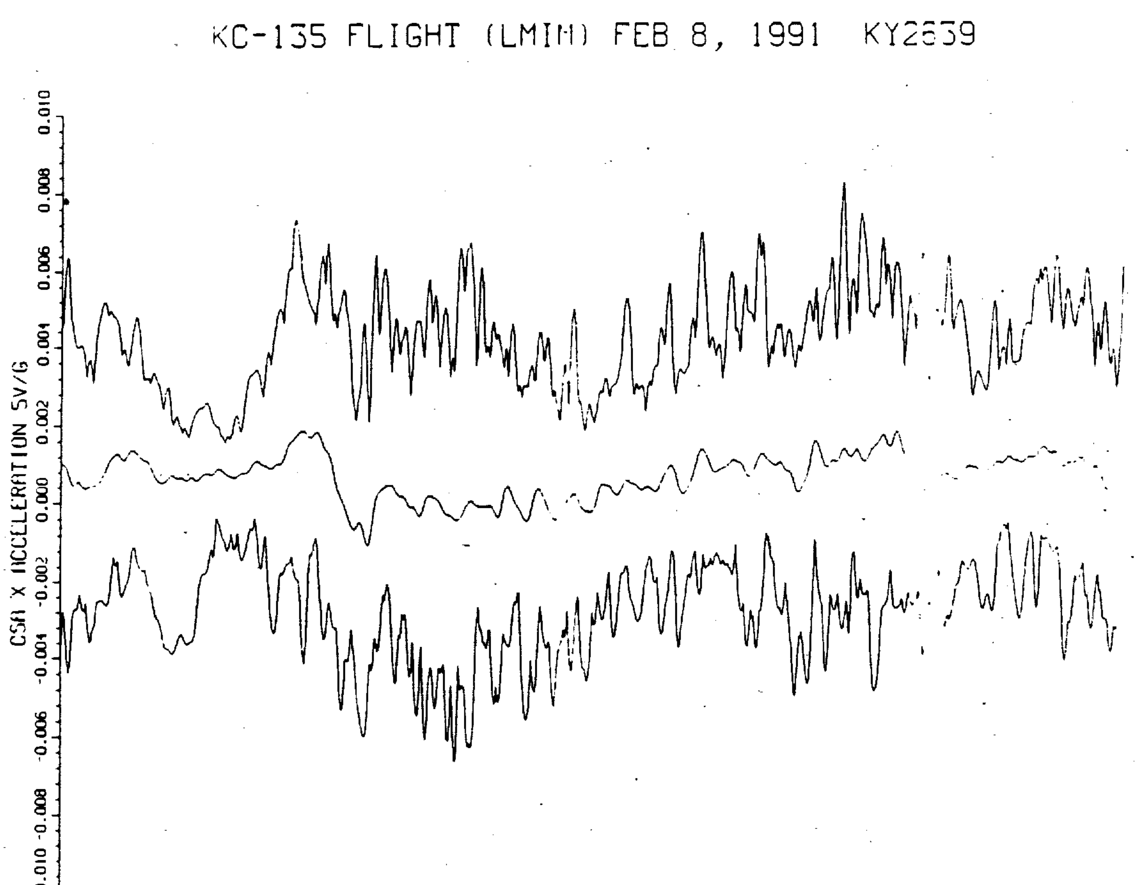

KC-135 FLIGHT (LMIM) FEB 8, 1991 KY2639
CSA X ACCELERATION 5V/G
0.05
0.04
0.03
0.02
0.01
0.00
-0.01
-0.02
-0.03
-0.04
-0.05
0.0
10.0
20.0
30.0
40.0
50.0
60.0
70.0
80.0
90.0
100.0
TIME

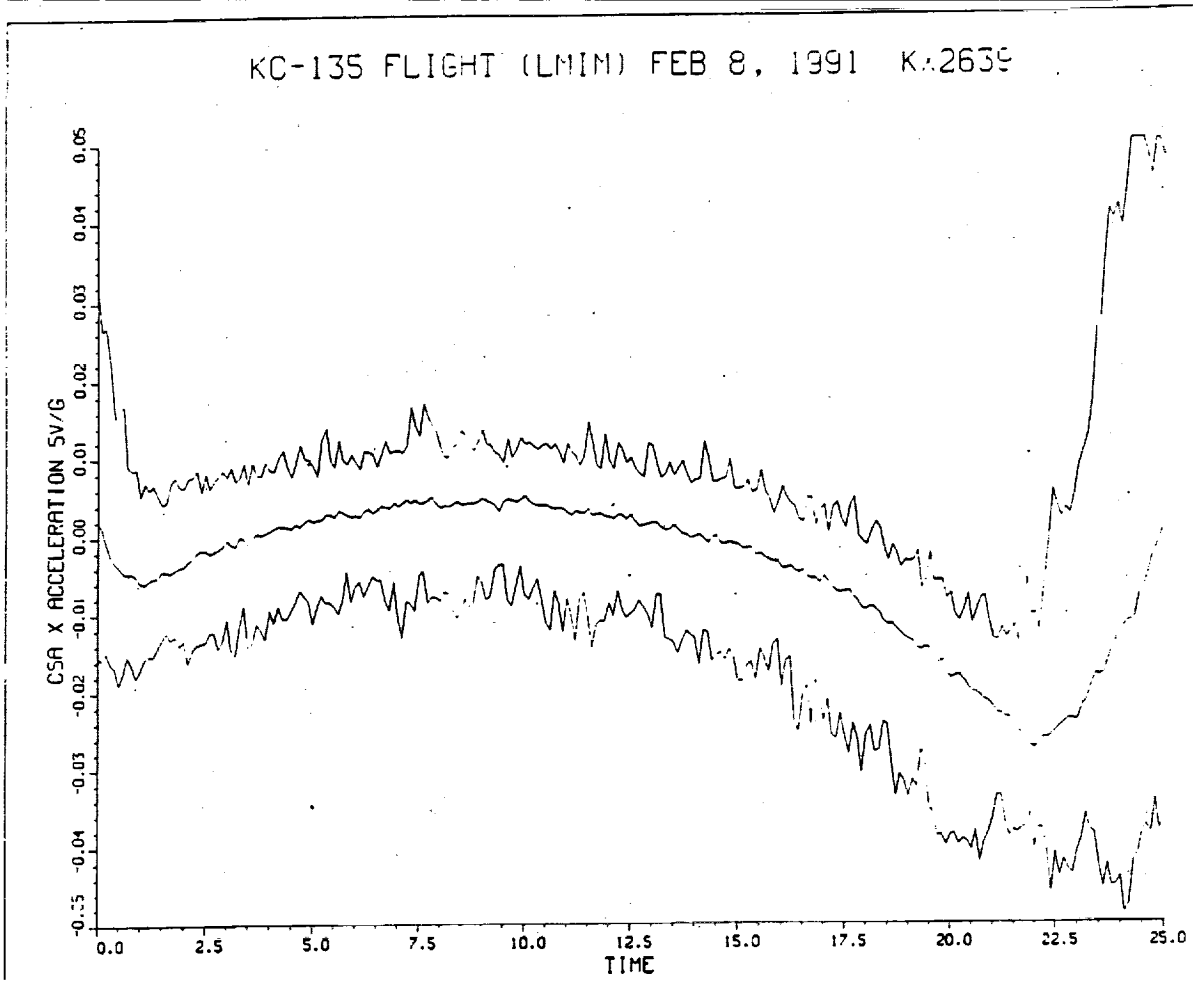

KC-135 FLIGHT JUNE 5/1990 KW506902 1
NAE X ACCELERATION(G)
0.05
0.04
0.03
0.02
0.01
0.00
-0.01
-0.02
-0.03
-0.04
-0.05
0.0
2.5
5.0
7.5
10.0
12.5
15.0
17.5
20.0
22.5
25.0
TIME


KC-135 FLIGHT JUNE 5/1990 KZ506902 1
NAE X ACCELERATION(G)
0.05
0.04
0.03
0.02
0.01
0.00
-0.01
-0.02
-0.03
-0.04
-0.05
0.0
2.5
5.0
7.5
10.0
12.5
15.0
17.5
20.0
22.5
TIME


KC-135 FLIGHT (LMIM) FEB 8, 1991 KW2639
CSA X ACCELERATION 5V/G
0.05
0.04
0.03
0.02
0.01
0.00
-0.01
-0.02
-0.03
-0.04
-0.05
0.0
2.5
5.0
7.5
10.0
12.5
15.0
17.5
20.0
22.5
TIME


KC-135 FLIGHT (LMIM) FEB 8, 1991
CSA X ACCELERATION 5V/G
0.05
0.04
0.03
0.02
0.01
0.00
-0.01
-0.02
-0.03
-0.04
-0.05
0.0
2.5
5.0
7.5
10.0
12.5
15.0
17.5
22.5
TIME


KC-135 FLIGHT (LMIM) FEB 8, 1991
CSA X ACCELERATION 5V/G
0.010
0.008
0.006
0.004
0.002
0.000
-0.002
-0.004
-0.006
-0.008
-0.010
0.0
10.0
20.0
30.0
40.0
50.0
60.0
70.0
90.0
TIME



Canadian Space Agency
Canadian Astronaut Program

April 22, 1991

Bjarni V. Tryggvason

Work to Date

Simulation routines developed and run to estimate performance

1-DOF hardware built in late 1989

First KC-135 flights in June 1990 with no control system

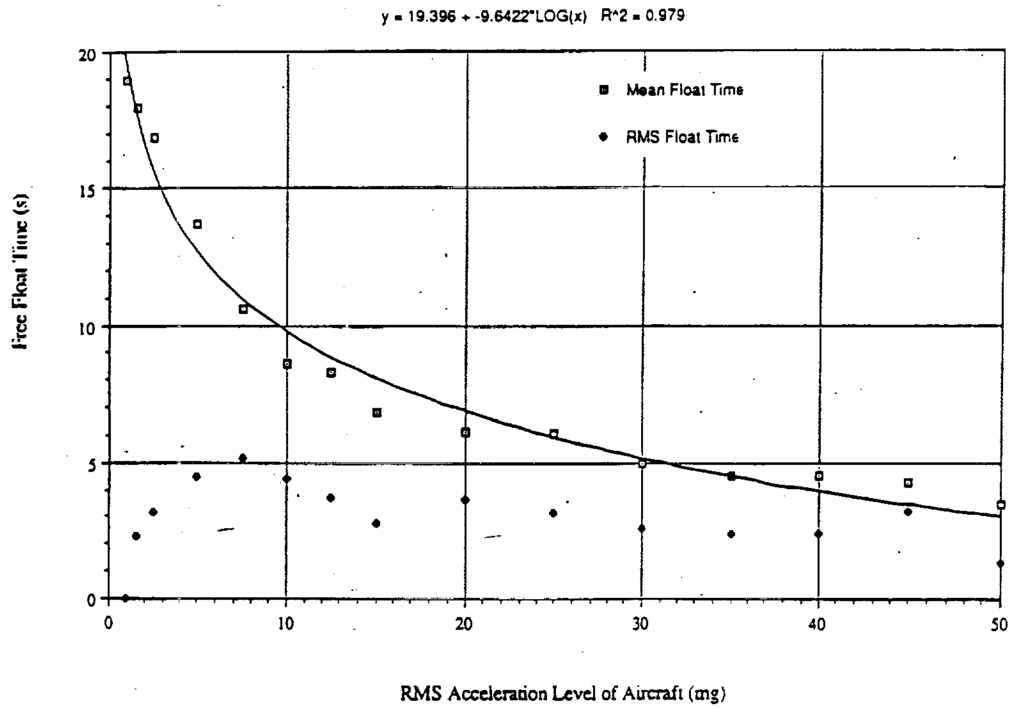
Control system hardware developed in late 1990

Second set of KC-135 flights Feb 4-8, 1991

Third set of KC-135 flights March 4-8, 1991

Fourth flight test set for March 24-28, 1991

Effect of Aircraft Acceleration Level on Free Float Time (Based on Simulation)



Planned Work

- Develop linear bearing passive 3DOF-LMIM	Summer 1991
- Develop 3-DOF simulation program	Summer 1991
- Research use of 6-DOF magnetic wrist joint for fine isolation	Summer 1991
- Research coarse-fine control strategy	Summer 1991
- KC-135 aircraft control dynamics study	Summer 1991
- Flight test of 3-DOF passive LMIM	Fall 1991
- Develop 6-DOF simulation program	Fall 1991
- Develop controlled 3-DOF LMIM	Spring 1992

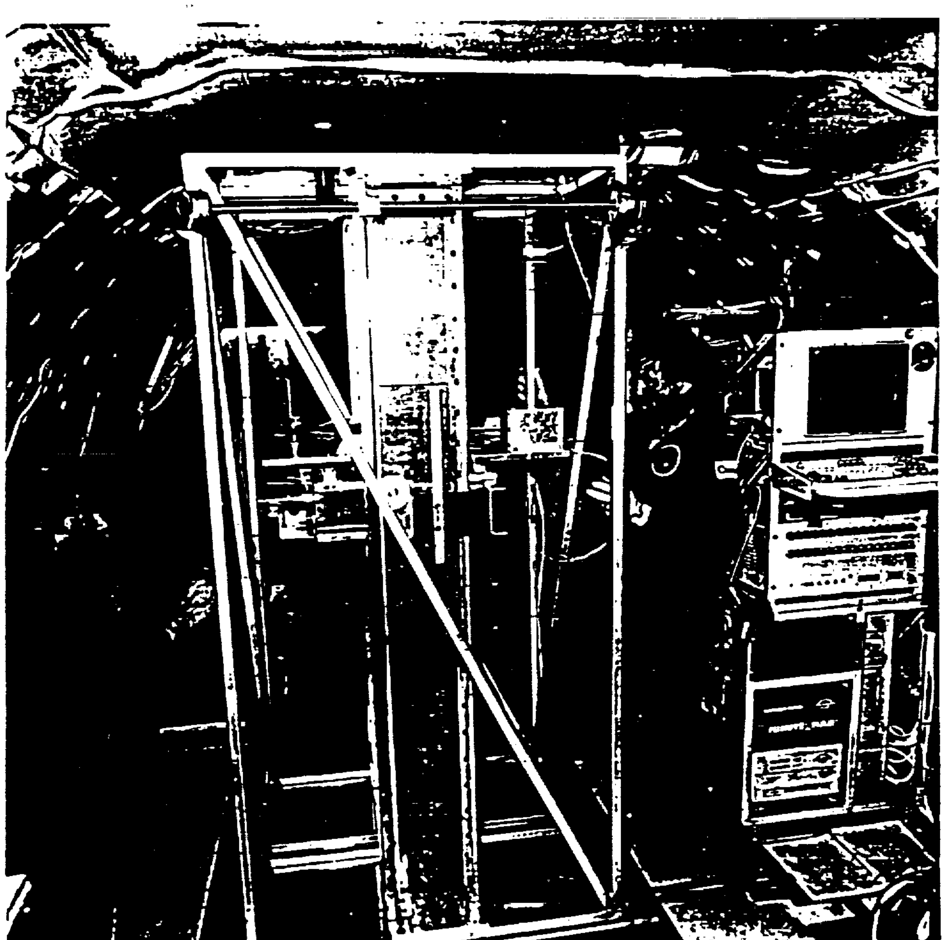
Program Objectives

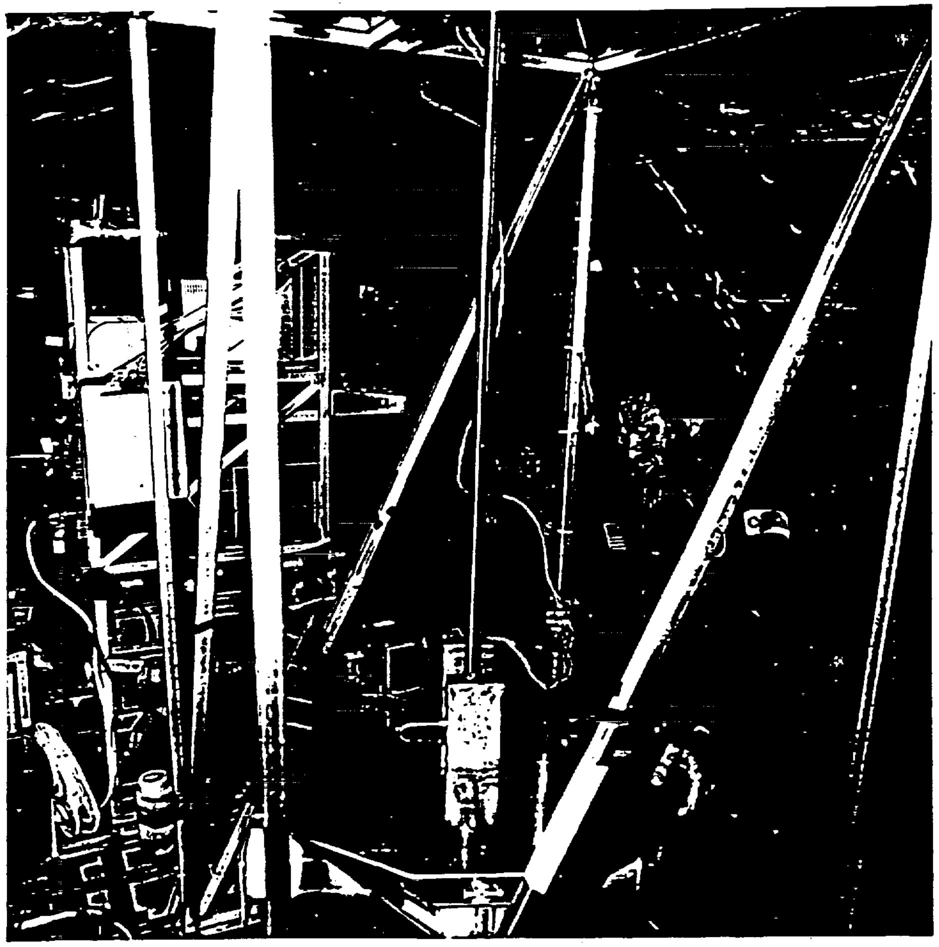
-To attain acceleration levels of 10^{-4} g for periods of 5 seconds to 15 seconds on the KC-135 aircraft

Advantages

- Longer time periods than are available in drop towers
- Experiments need not be fully automated
- Experimentor can fly with his experiment
- Easy and frequent access time
- Experiments can be repeated easily and often
- Comparitively low cost

NASA National Aeronautics and Space Administration   S91-28327   Lyndon B. Johnson Space Center Houston, Texas 77058





NASA National Aeronautics and Space Administration

S90 40155

Lyndon B. Johnson Space Center
Houston, Texas 77058

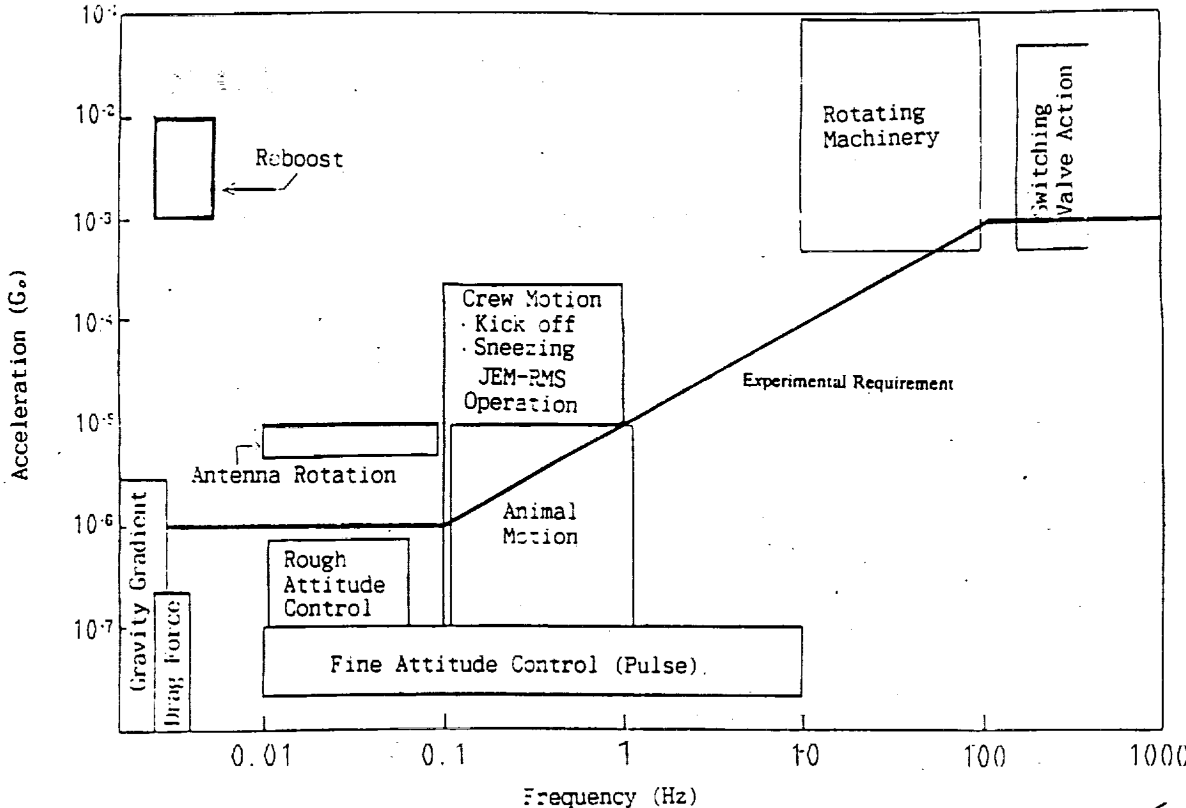
N92-28441

INTERNATIONAL WORKSHOP ON VIBRATION ISOLATION TECHNOLOGY FOR MICROGRAVITY SCIENCE APPLICATIONS

APRIL 23–25

NASDA
JSUP
IHI, NEC

CONTENTS



MICROGRAVITY REQUIREMENT

NASDA'S ACTIVITIES ON VIBRATION ISOLATION TECHNOLOGY

One of the Generic Experiment Technology
- Provide Technical Support to Users
- Provide Necessary Technology and Facilities to Users

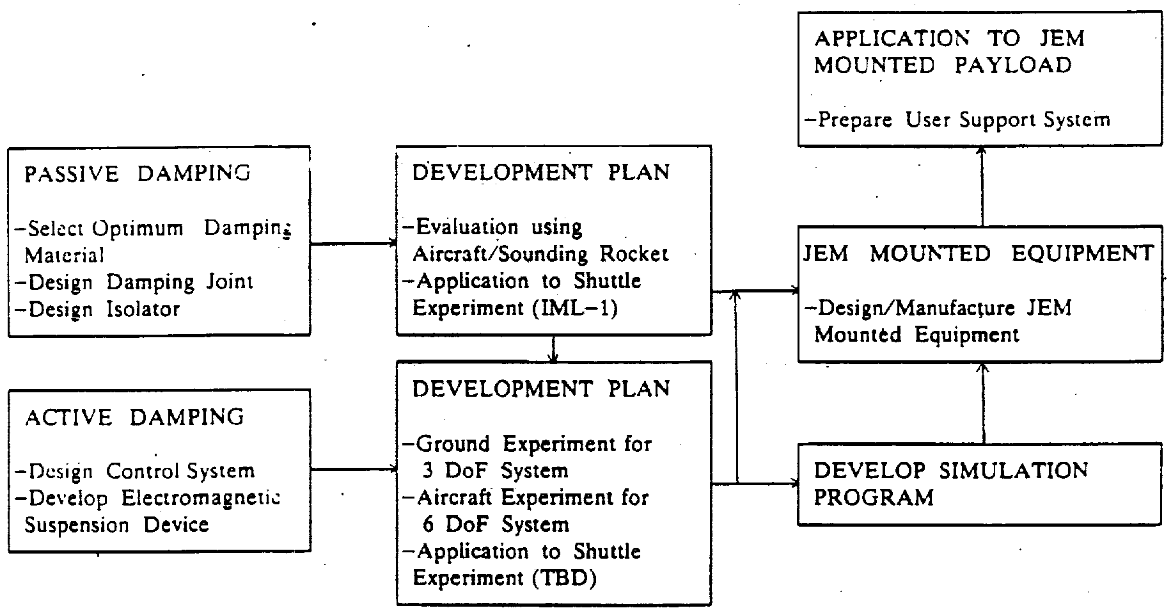
Active Damping System

Active Position Control – less than 0.02Hz
Active Damping – over 0.02Hz

Passive Damping System

Passive Damping – over 10Hz, 1/2 – 1/10 Reduction Ratio
Isolator – over 0.1Hz, 1/100 – 1/1000 Reduction Ratio

DEVELOPMENT FLOW OF VIBRATION ISOLATION TECHNOLOGY



ACTIVE DAMPING SYTEM

ACTIVE VIBRATION ISOLATION CONCEPT

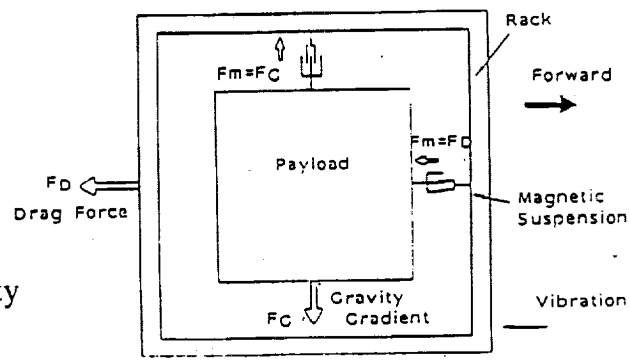
- Position Control by Electromagnetic Suspension for Steady Residual Acceleration (Atmospheric Drag, Gravity Gradient)

- Reduction/Isolation by Low Spring Constant Damping Element for Low Frequency/Oscillating Acceleration (Crew Motion, Rotating Machinery)

- Minimum Electric Power

- Small and Light Weight

- High Reliability and Safety



VOICE COIL TYPE ELECTROMAGNETIC SUSPENSION

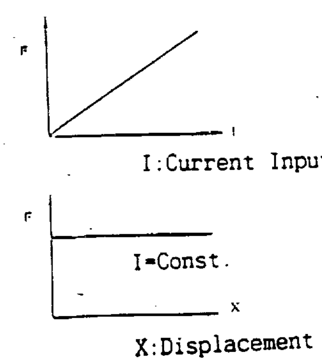
Support without Direct Contact

Low Spring Constant Damping Element

Generating Force
F ∝ I (Coil Current)
- Applicable to Any Force

F : Independent of Displacement
- Easy to Control

Wide Stroke : ± 5mm



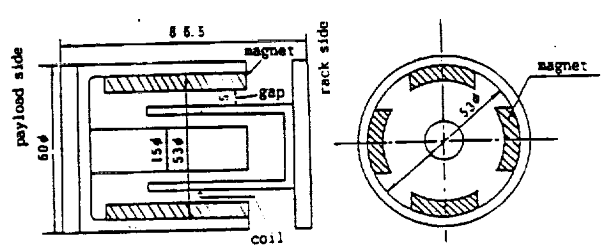
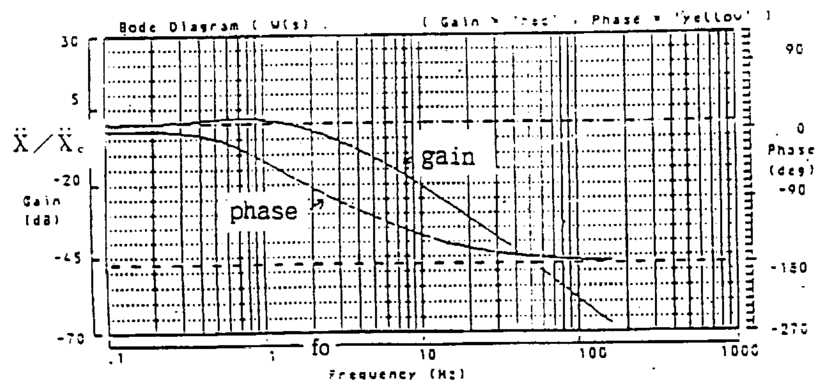


Fig. Profile of Electromagnetic Suspension and Its Force Characteristics

VIBRATION ISOLATION ABILITY

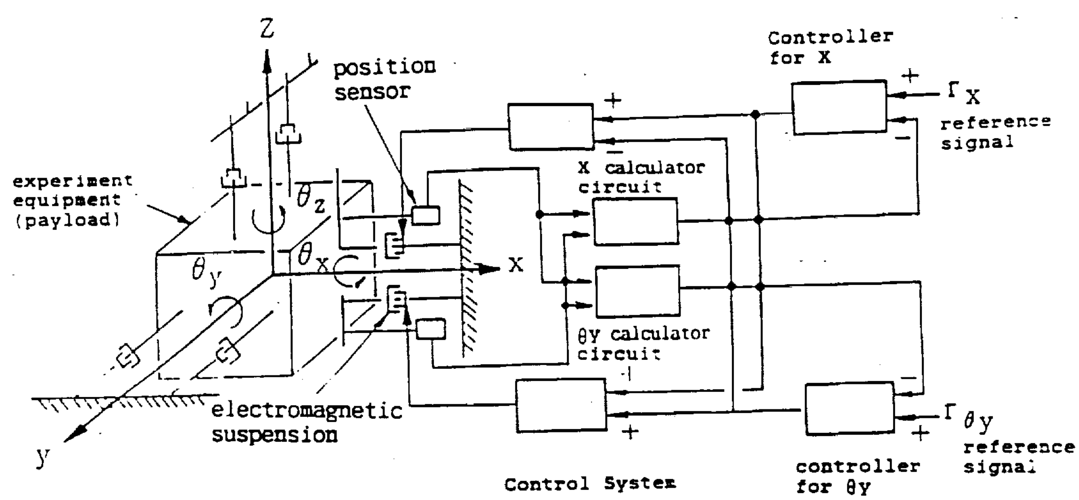


fo : Cut-Off Frequency
X : Acceleration of Payload
Xe : Acceleration of Rack

Above Cut-Off Frequency: $\ddot{X}/\ddot{X}e=1$
- Payload Follows Motion of Rack.

Below Cut-Off Frequency: $\ddot{X}/\ddot{X}e<1$
- Oscillating Vibration Is Isolated.

PROFILE OF ACTIVE VIBRATION ISOLATION SYSTEM



- 2 Pairs of Electromagnetic Suspension and Position Sensor for Each Axis
- Accelerometers for Payload and for Rack
- 2 DoF Is Controllable for Each Axis : Translation and Rotaion

STAGE OF DEVELOPMENT

PHASE 1
- Design Control System
- Evaluate Characteristics of Electromagnetic Suspension

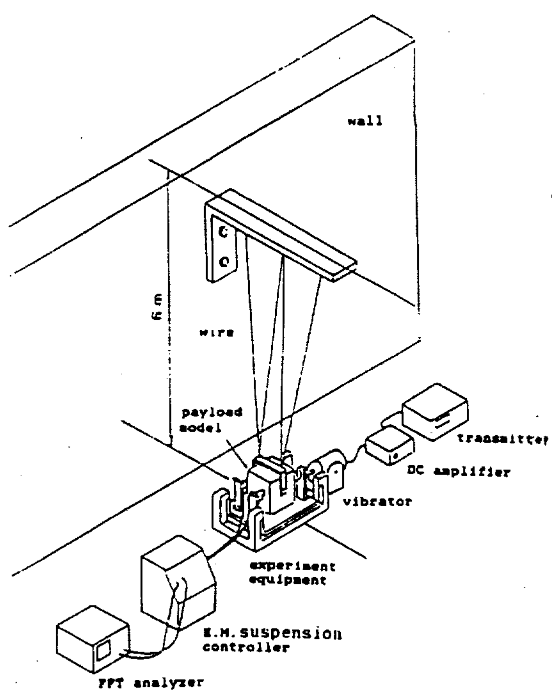
PHASE 2
- Ground Experiment
  Evaluate Vibration Reduction Ability of 3 DoF System
- Numerical Simulation Analysis

PHASE 3
- Aircraft Experiment
  Evaluate Vibration Reduction Ability of 6 DoF System
  Evaluate Vibration Isolation Effect for Mission

PHASE 4
- Shuttle Experiment
  Develop/Evaluate Active Vibration Isolation System
  for Space Experiment

Phase 5
- Application to JEM Missions

GROUND EXPERIMENT ON 3 DoF SYSTEM



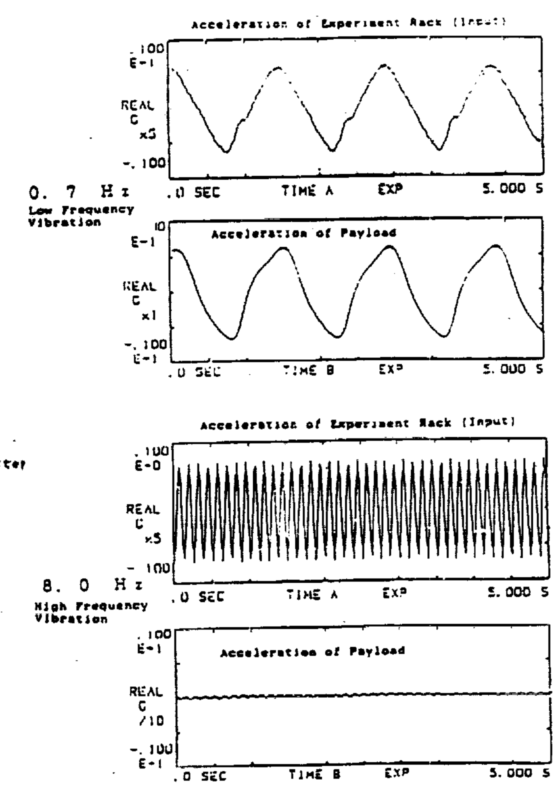
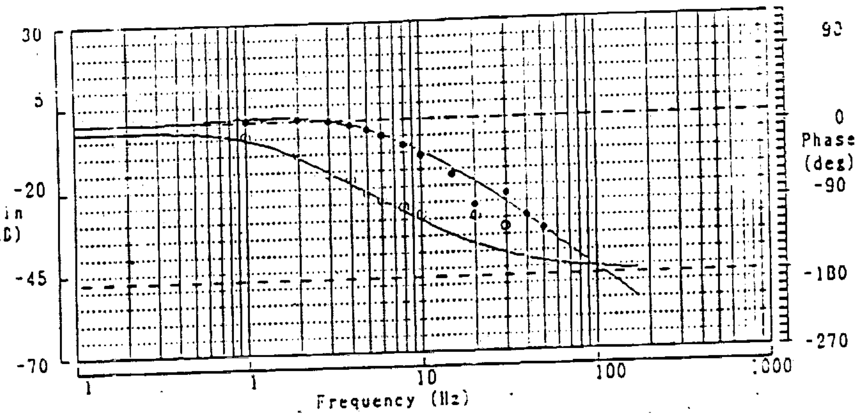


Fig. Profile of Ground Experiment Model
and Experimental Results

COMPARISON BETWEEN THEORETICAL VALUE AND EXPERIMENTAL RESULT



Solid Line : Teoretical Value
● : Experimental (Gain)
○ : Experimental (Phase)

Experimental Results Showed Very Good Agreement with Theoretical Value.

ABSTRACT OF AIRCRAFT EXPERIMENT SYSTEM

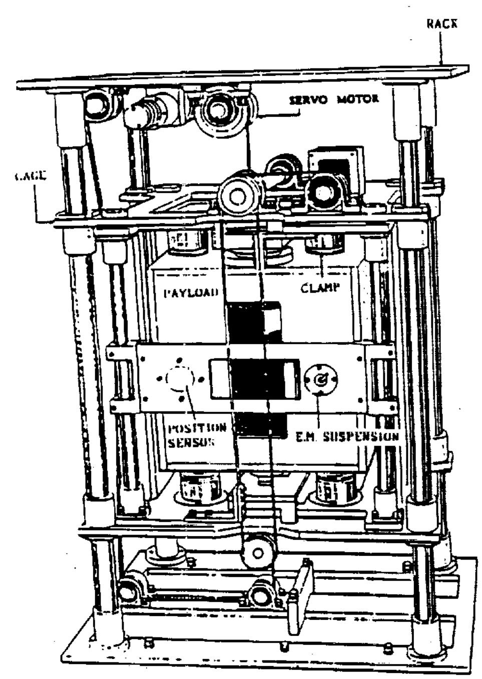
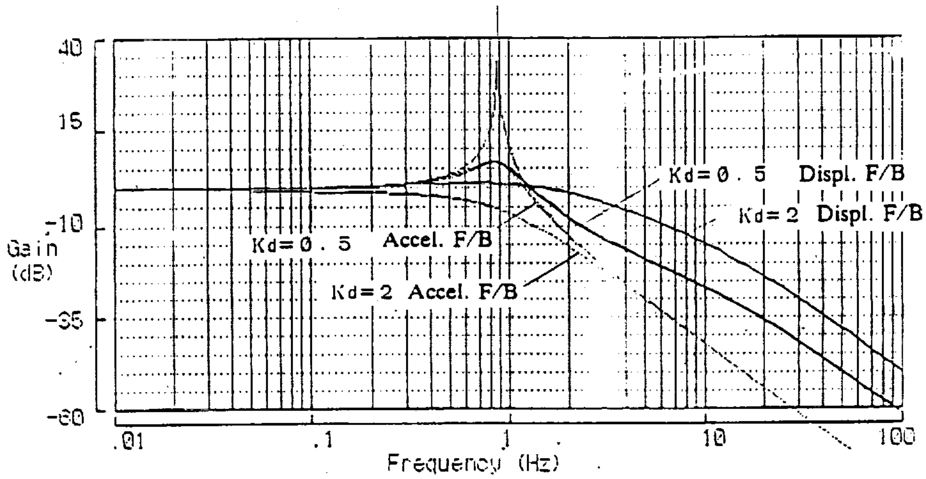


Fig. Profile of Aircraft Experiment System

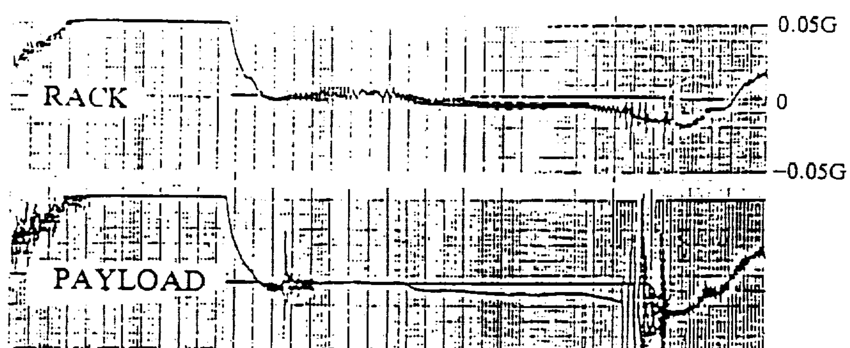
- Control 6 Degrees of Freedom
- Double Control for Z Axis
- Payload 20kg, 300mm cube
- Suspension Force
  X,Y Axis Max. 200gf
  Z Axis Max. 500gf
- Stroke Suspension: ±5mm
  Cage :±100mm
- Allowable Acceleratrion
  X,Y Axis Max. 0.02g
  Z Axis Max. 0.05g

INTRODUCTION OF ACCELERATION FEEDBACK

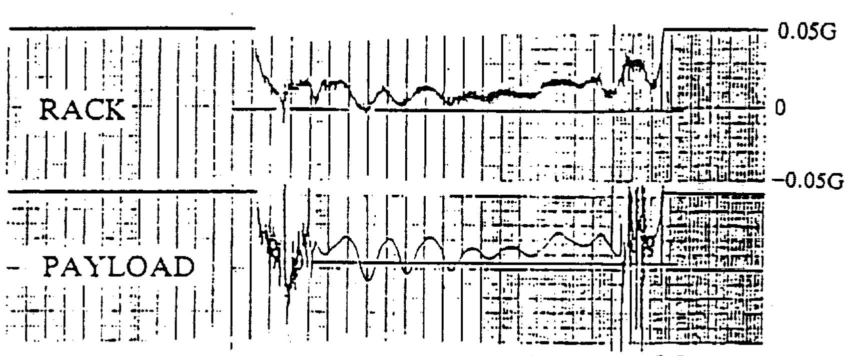


Acceleration F/B Shows Better Isolation Ability in High Frequency Range.

EXPERIMENTAL RESULT
- RELATIVE DISPLACEMENT FEEDBACK -

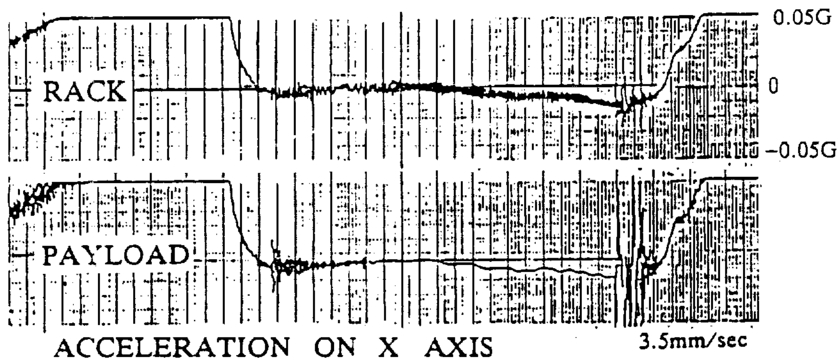


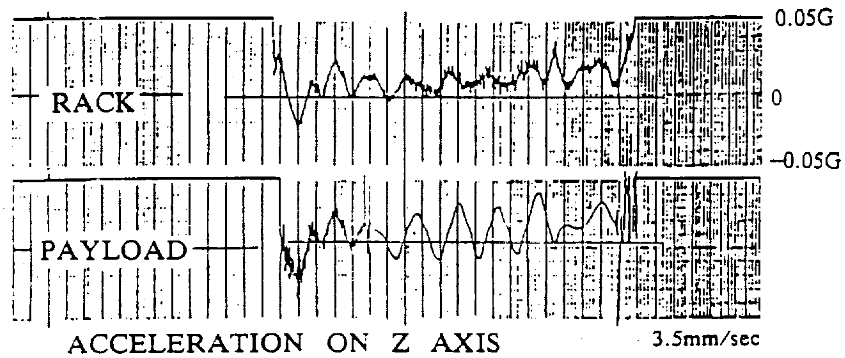
ACCELERATION ON X AXIS



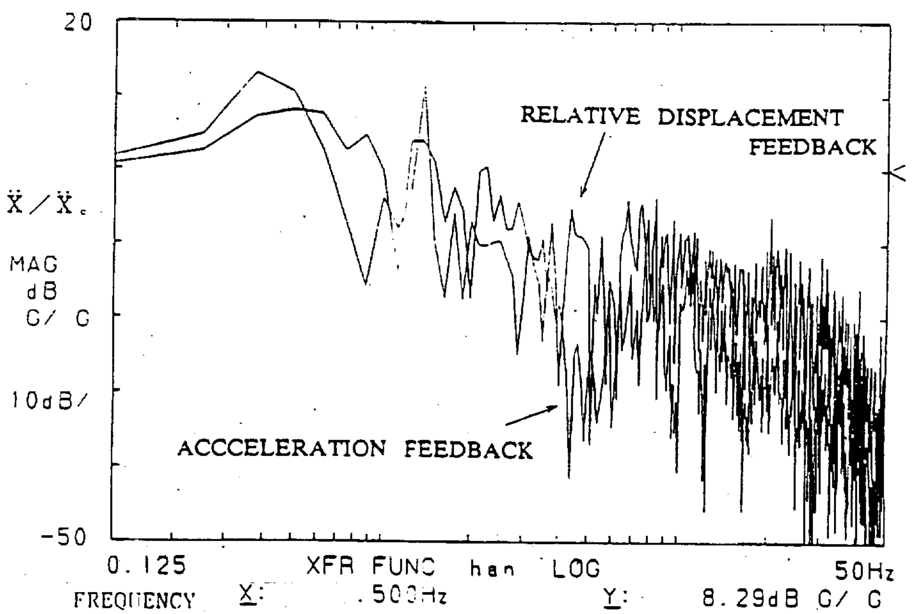
ACCELERATION ON Z AXIS

EXPERIMENTAL RESULT
- ACCELERATION FEEDBACK -

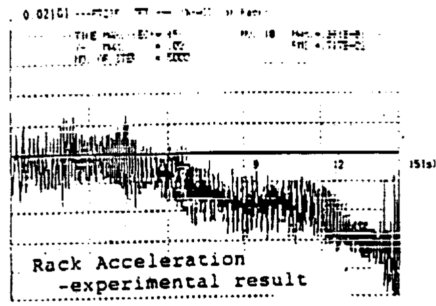


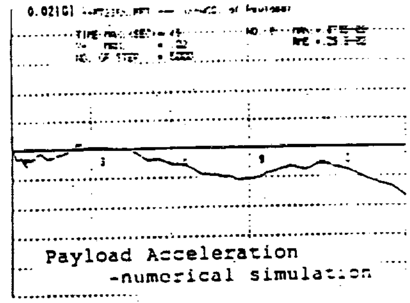


EXPERIMENTAL RESULTS
- FREQUENCY ANALYSIS (ON Z AXIS) -

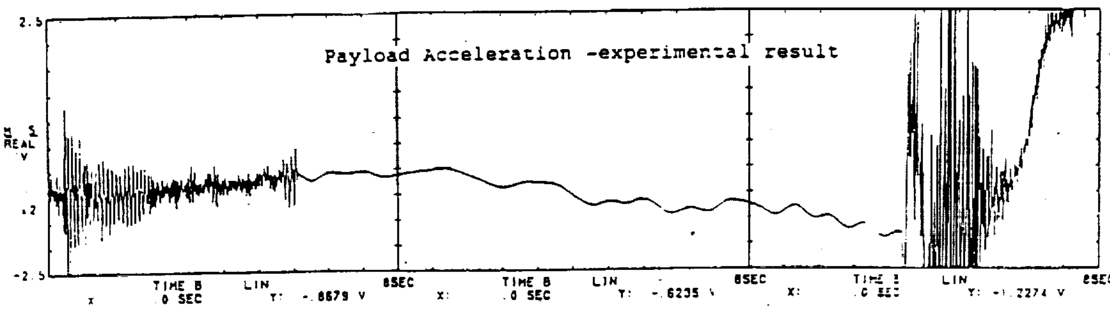


$\ddot{X}$: ACCELERATION OF THE PAYLOAD
$\ddot{X}_o$: ACCELERATION OF THE RACK

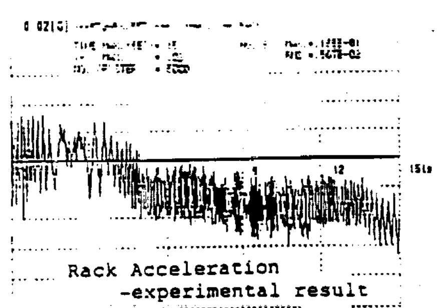


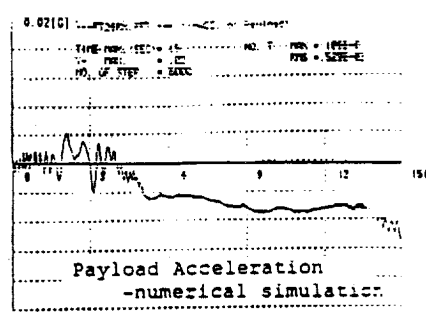


ACELERATION F/B X AXIS

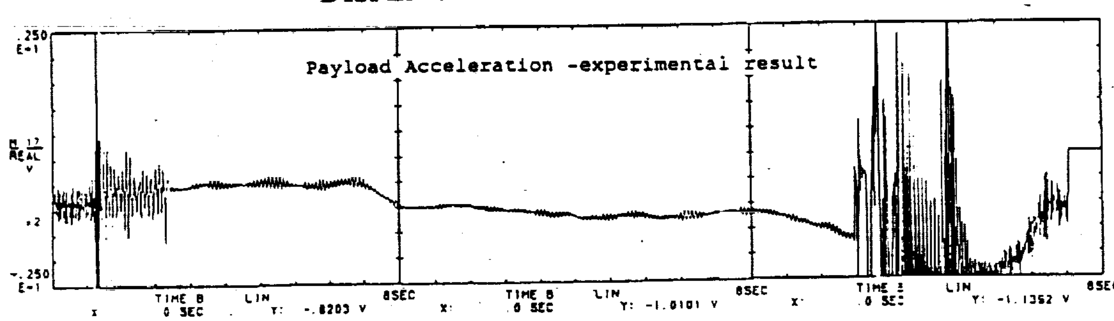


COMPARISON BETWEEN SIMULATION AND EXPERIMENTAL





DISPLACEMENT F/B X AXIS



EXPERIMENTAL RESULTS SUMMARY

- Each Control Axis of the System Was Independently Controlled.

- Acceleration Disturbance in High Frequency Range Was Reduced less than 1/10 – 1/100 in the Payload.

- Against Low Frequency Disturbance, Position of the Payload Was Well Controlled to Follow Displacement of the Rack.

- Microgravity Environment in the Aircraft Was Effectively Improved over 15 – 18 Seconds.

PASSIVE DAMPING SYSTEM

PASSIVE DAMPING SYSTEM

REQUIREMENT

To provide a high damping effect.

- — wide vibration range ($10^{-5} \sim 10^{-2}$G)
- — wide frequency range (0.1~100Hz)

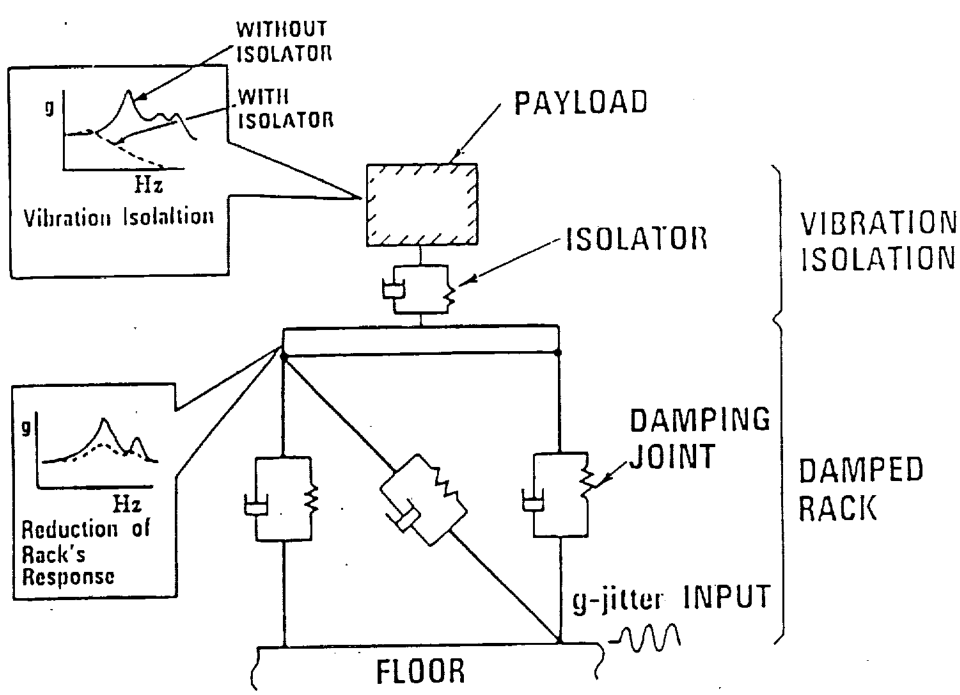
METHOD

with the use of Isolator & Damping joint.
(made of viscoelastic damping material.)

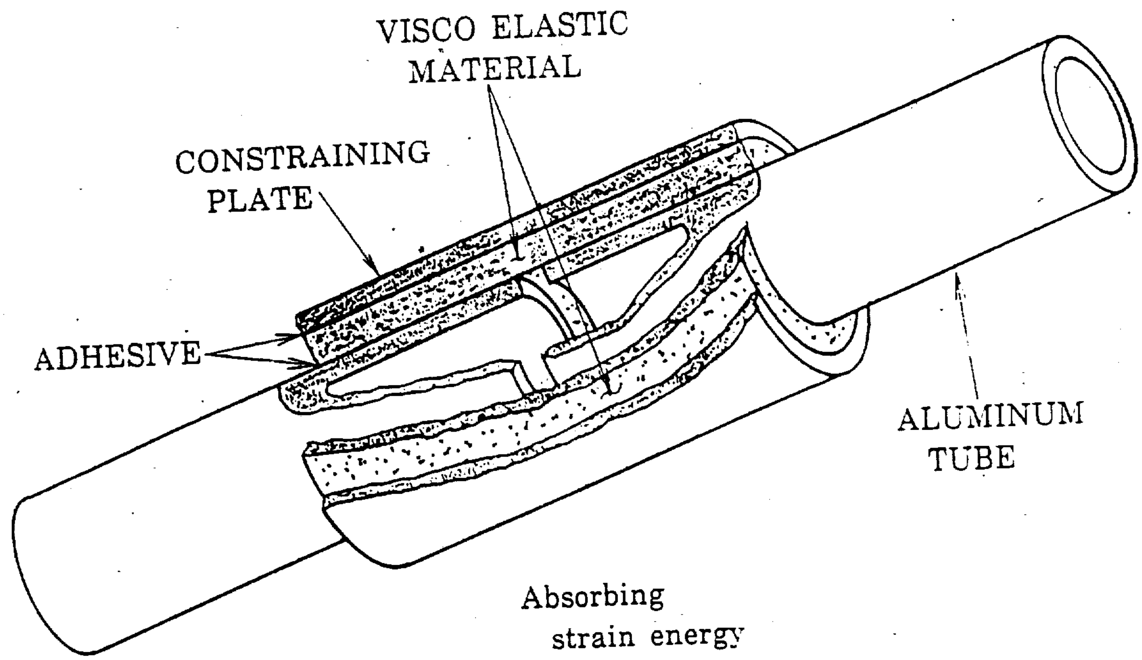
EVALUATION

In aircraft experiment,
PASSIVE DAMPING SYSTEM was evaluated.

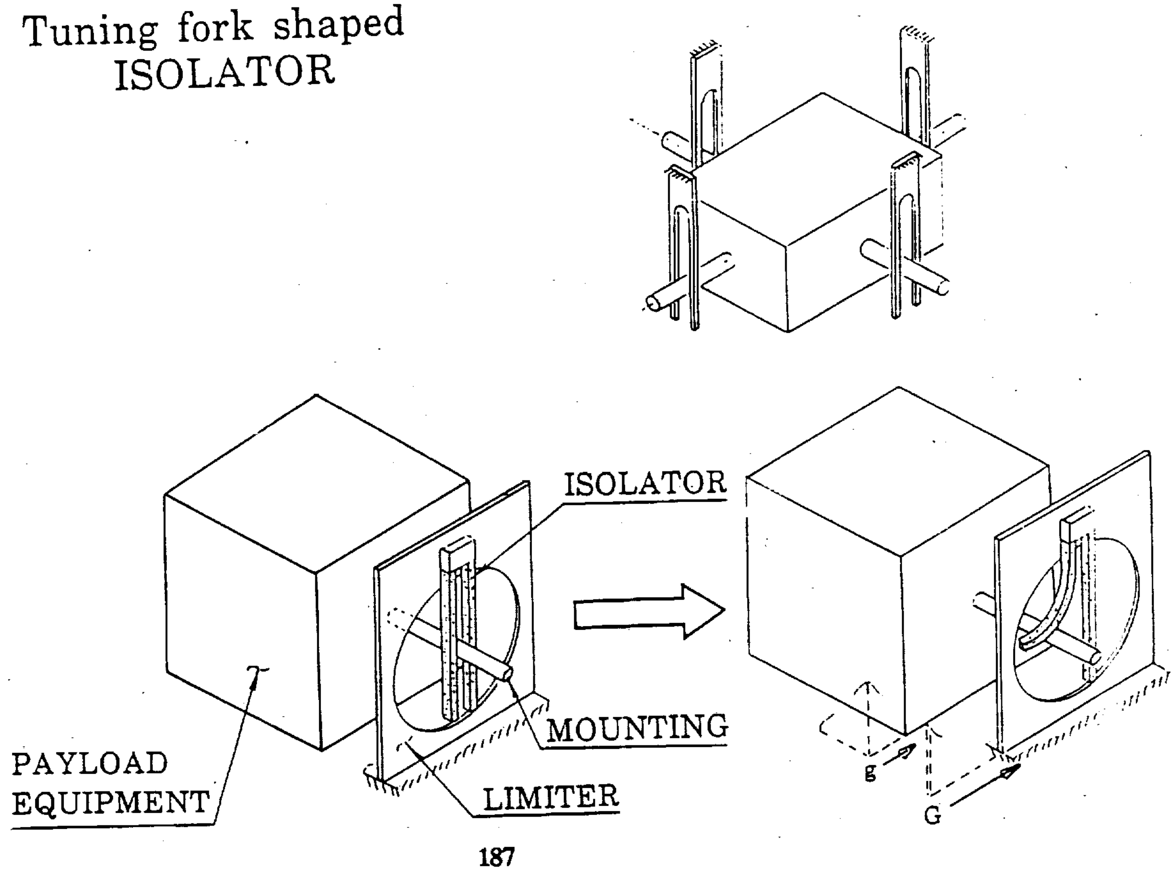
DESIGN CONCEPT of passive damping joint



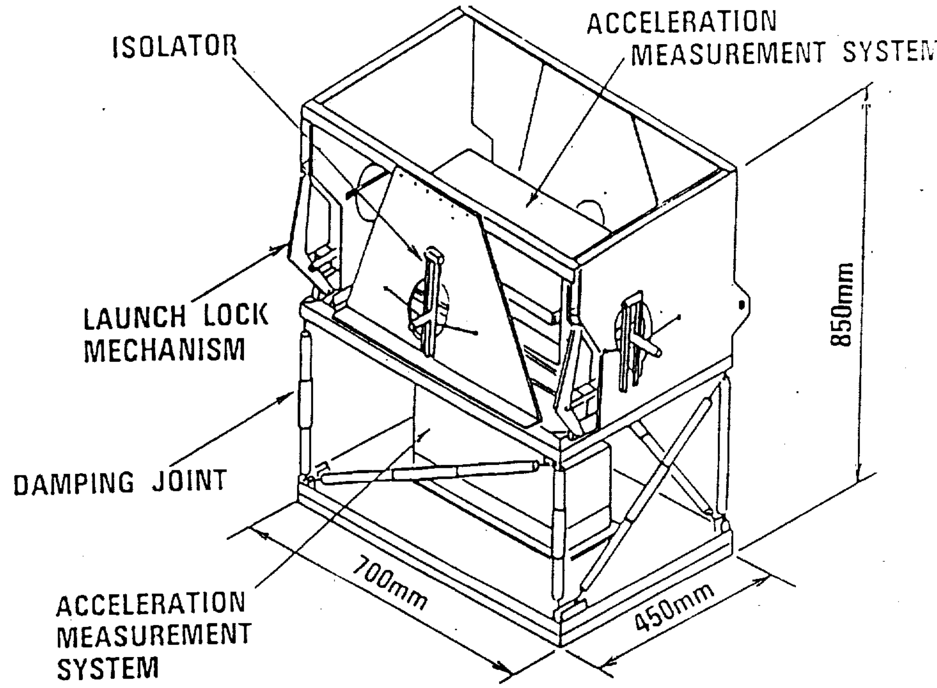
DAMPING JOINT



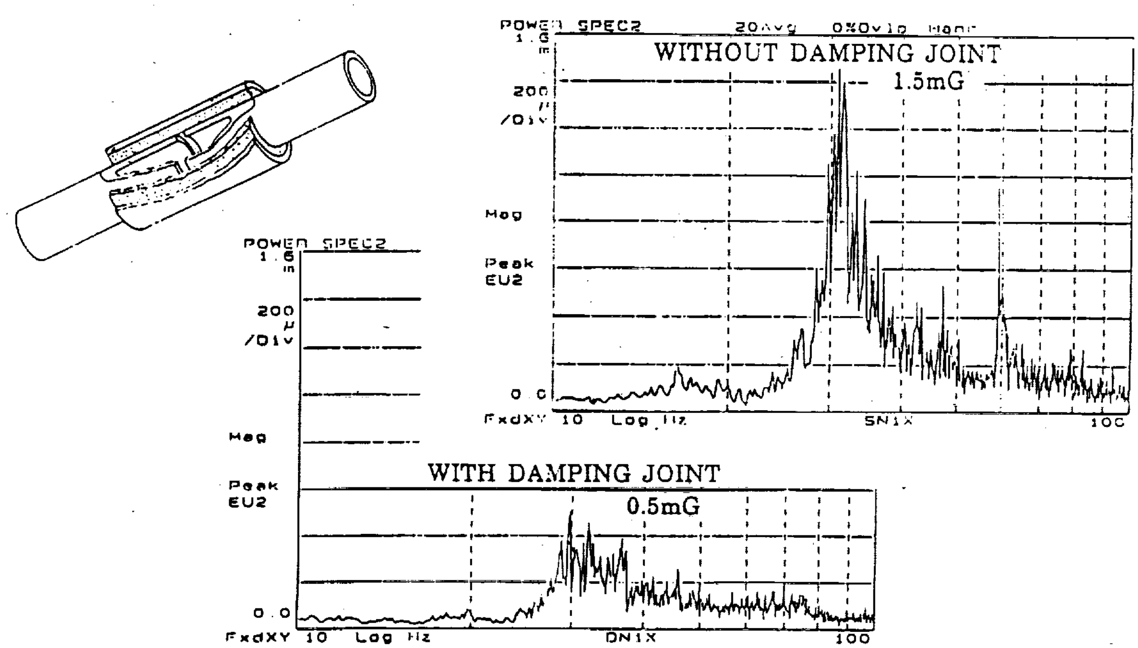
Tuning fork shaped ISOLATOR



PASSIVE DAMPING SYSTEM RACK
for aircraft experiment

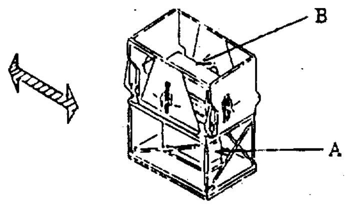


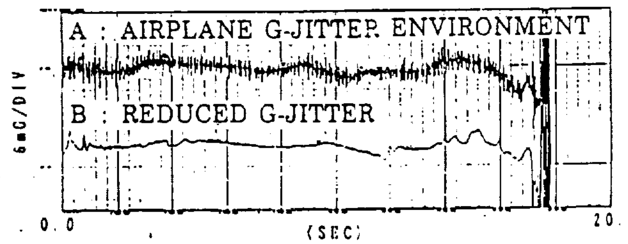
EXPERIMENTAL RESULTS
Damping joint

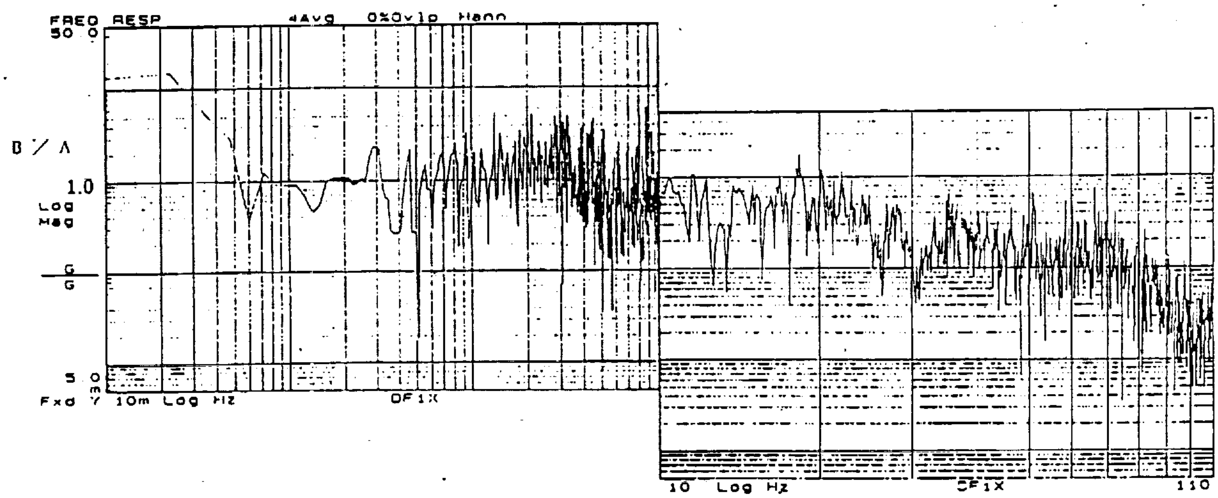


C-3

EXPERIMENTAL RESULTS
Isolator







EVALUATION
of aircraft experiment

— Maximum steady state acceleration measured during experiment was 0.02G.

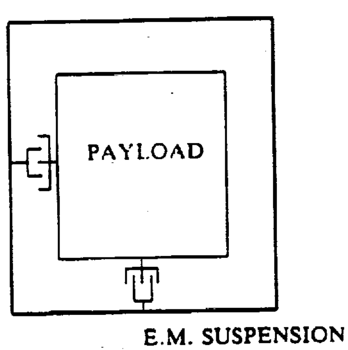
— With the use of the damping joint, resonant amplification factor was reduced to 1/2 — 1/4.

— With the use of the newly developed isolator, vibration from low frequency of 0.1Hz was isolated.

— Both damping joint and isolator can applied to g-jitter reduction in the space.

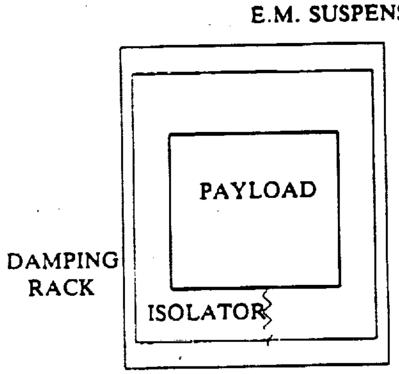
MISSION PROPOSAL

PROVIDING OPTIMUM DAMPING SYSTEM FOR MISSION



ACTIVE DAMPING

Isolation over 0.02Hz



PASSIVE DAMPING

Damping Rack
  Reduce Rack Resonance Amplification

Isolator
  Isolation over 0.1Hz

MISSION PROPOSAL OF ACTIVE VIBRATION ISOLATION SYSTEM

OBJECTIVE

To evaluate vibration isolation effect

Acceleration Data

Payload Experiment Data

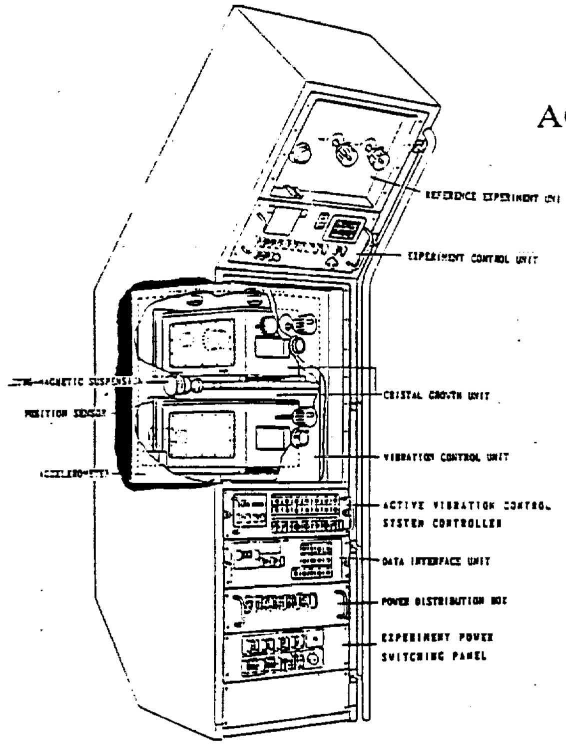
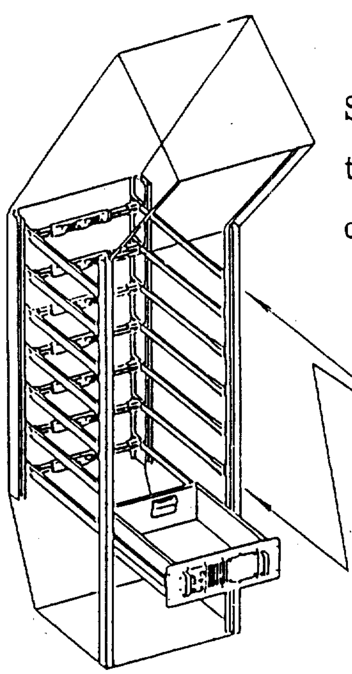
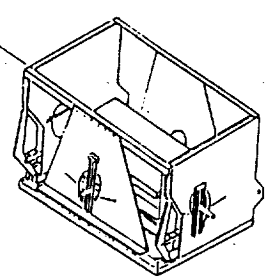


Fig. Profile of Rack Accommodated Active Vibration Isolation System

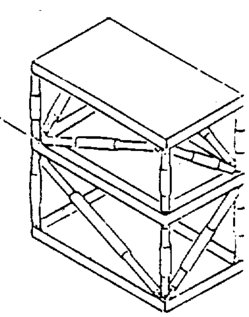
MISSION PROPOSAL OF PASSIVE DAMPING SYSTEM in a space station

STANDARDIZATION of damped rack and isolator to correspond to each experimental theme of a space station.





Isolator mechanism



Damped rack

SUMMARY

- NASDA Is Providing Various Vibration Isolation Technology for Space Station Mission.

  Active Vibration Isolation System
  for Extra Sensitive Mission in Low Frequency Range

  Passive Damping System
  Damping Rack
  for Reduction of Resonance Amplification
  Isolator
  for Vibration Isolation from Low Frequency

- for both Active and Passive Damping System, Vibration Isolation Ability Was Verified by Aircraft Experiment.

N92-28442

VIBRATION ISOLATION TECHNOLOGY DEVELOPMENT TO DEMONSTRATION

Carlos Grodsinsky

NASA Lewis Research Center
21000 Brookpark Road
Cleveland, Ohio 44135

ABSTRACT

The low gravity environment provided by space flight has afforded the science community a unique arena for the study of fundamental and technological sciences. However, the dynamic environment observed on space shuttle flights and predicted for Space Station Freedom has complicated the analysis of prior "microgravity" experiments and prompted concern for the viability of proposed space experiments requiring long–term, low gravity environments.

Due to these present concerns and the need to have enabling technology for the use of future manned and unmanned "microgravity" platforms, an Advanced Technology Development, ATD, project was established by the Microgravity Sciences and Applications Division, Code SN, in Vibration Isolation Technology. NASA Lewis Research Center began research in the field of active vibration isolation, specifically for "microgravity" experiments, in mid–1987. This ATD project was organized into three phases of development, namely a requirements, development and demonstration phase.

The requirements phase of the project has been instrumental in providing the impetus for educating the microgravity science community as to what environment is actually accessible and what needs to be addressed in order to more reliably predict an experiment's reaction to shuttle acceleration inputs. The next major step for the requirements phase of the project is to bridge the gap between flight hardware developers and realistic scientific "microgravity" requirements to design stable instrument platforms for low gravity experimentation.

In accordance with this organizational plan, the development of certain active isolation approaches have been studied. The main thrust of these studies has resulted in an active inertial feedforward/feedback isolation system. This prototype magnetic suspension system has been demonstrated in a laboratory setting in six degrees–of–freedom and has been preliminarily characterized in its isolation performance with favorable results. This isolation system consists of a closed loop digital control system referencing a platform around six relative and six inertial sensors. These sensors control the isolated mass through nine attractive electromagnetic actuators with a system capability of ± three–tenths of an inch travel in three dimensions.

The development of a prototype system from design to fabrication leads directly into the demonstration phase of the project which will attempt a low gravity environmental demonstration of engineering hardware for the isolation of a scientific payload. The demonstration phase of the project will use an aircraft low gravity maneuver to establish a research testbed for the study of isolation hardware and control strategies in an off–loaded environment. In developing this demonstration capability the Lewis Learjet aircraft has been characterized through its parabolic flight maneuvers and a trunnioned experimental volume has been designed for the test of both active and passive isolation packages. This vibration isolation testbed is operational and has two data acquisition systems available for both autonomous and interactive operation, with a combined input capability of 32 channels.

VIBRATION ISOLATION TECHNOLOGY

Publication List

1. NASA–TM 101448, "Low Frequency Vibration Isolation Technology for Microgravity Space Experiments", C. M. Grodsinsky, and G. V. Brown. (*)

2. NASA–TM 102386, "Nonintrusive Inertial Vibration Isolation Technology for Microgravity Space Experiments", C. M. Grodsinsky and G. V. Brown. (ξ,**)

3. NASA–TM 102470, "A New Approach to Active Vibration Isolation for Microgravity Space Experiments", A. Sinha, C. Kao, and C. M. Grodsinsky. (***)

4. NASA–TM in preparation, "Active Vibration Isolation of an Experiment on a Space Platform", R. D. Hampton, C. M. Grodsinsky, P. E. Allaire, and D. W. Lewis.

5. "Limitations on Vibration Isolation for Microgravity Space Experiments", C. Knospe, and P. Allaire. (**)

6. NASA–TM in preparation, "Development of a Vibration Isolation Proto–type System for Microgravity Space Experiments", K. A. Logsdon, C. M. Grodsinsky and G. V. Brown. (§)

7. NASA–TM 103103, "The Vibro–Acoustic Mapping of Low Gravity Trajectories on A Learjet Aircraft", C. M. Grodsinsky and T. J. Sutliff. (ϵ)

8. NASA–TP 2984, "Development and Approach to Low Frequency Microgravity Isolation Systems", C. M. Grodsinsky.

Student Publication

1. "Active Control of Vibration Isolation with Discrete Frequency Agitation for Space Experiments", H. M. Harris. (ζ)

* – Presented at 12th Biennial Conference on Mechanical Vibration and Noise sponsored by ASME, Montreal, Canada, Sept. 17–20, 1989

ξ – Presented at 28th Aerospace Sciences Meeting sponsored by AIAA, Reno, Nevada, Jan. 8–11, 1990

ζ – Presentation at the 31st Annual Structures, Structural Dynamics and Materials Conference sponsored by AIAA/ASME, Long Beach, California, April, 1990

§ – Presentation at the COSPAR 28 Conference, Third International Symposium on Experimental Methods for Microgravity Materials Science Research, The Hague, Netherlands, June 25 to July 2, 1990

ϵ – Presentation at the Second Workshop on Microgravity Experimentation, Ottawa, Ont. Canada, May 8–9, 1990

**– Submitted to the Journal of Spacecraft and Rockets sponsored by AIAA

***– ACTA Astronautica Vol. 21, No. 11/12, pp. 771–775, 1990

NASA
Lewis Research Center

SPACE EXPERIMENTS DIVISION

SFSD
Space Flight Systems Directorate

VIBRATION ISOLATION TECHNOLOGY DEVELOPMENT TO DEMONSTRATION

Carlos M. Grodsinsky
NASA Lewis Research Center
Cleveland, OH.

International Workshop on Vibration Isolation Technology
for
Microgravity Science Applications

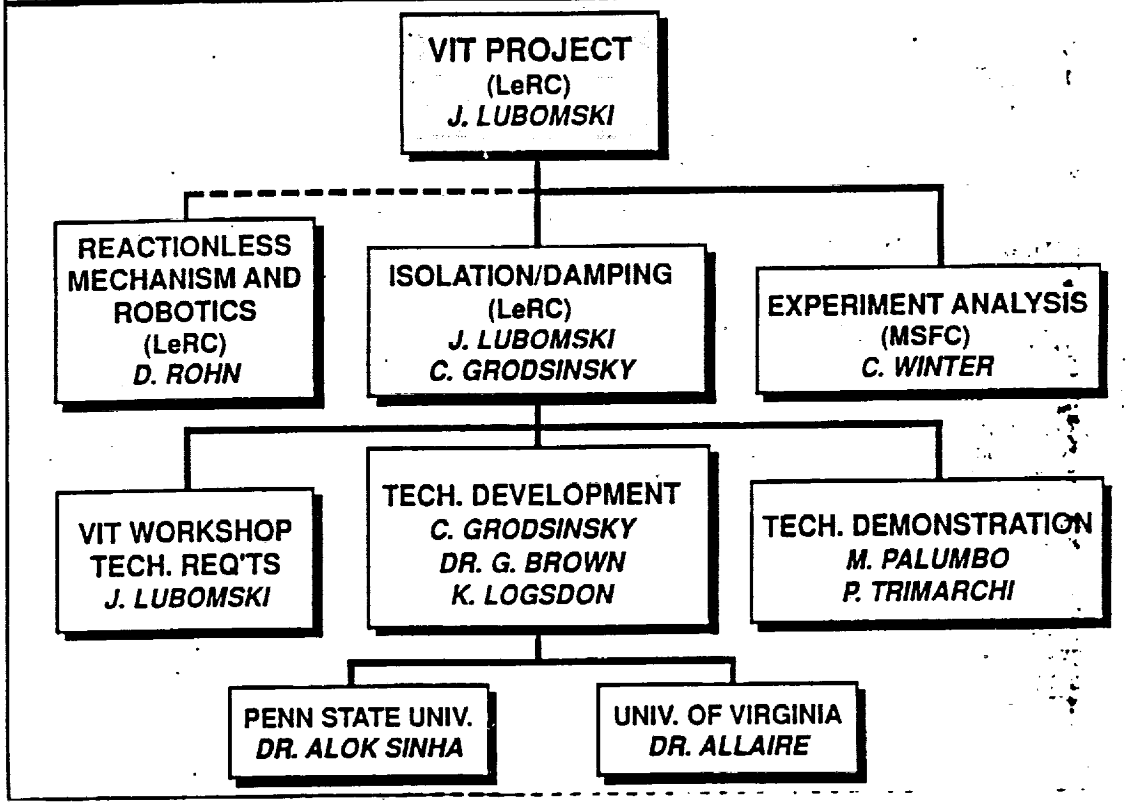
Nasa Lewis Research Center
Cleveland, OH. U.S.A.
April 23-25, 1991

NASA
Lewis Research Center

SPACE EXPERIMENTS DIVISION

SFSD
Space Flight Systems Directorate

- Background
  - Vibration Isolation Technology Project
  - Environment Definition
  - Space Station Freedom "Microgravity" Truths and Predictions
- Requirements
  - Space Station Freedom Microgravity Requirement vs. Selected Acceleration Measurements
- Development of Isolation Concepts
  - Theoretical Isolation Approach
  - Prototype Development and Proof-of-Concept
- Demonstration
  - Learjet Characterization
  - Learjet Passive Isolation Testbed
  - Learjet Active Isolation Testbed



NASA Lewis Research Center | SPACE EXPERIMENTS DIVISION | SFSD Space Flight Systems Directorate

Vibration Isolation Technology Development to Demonstration

Background

- Actual shuttle dynamic environment is not microgravity, irrespective of its title, namely "Microgravity".
- Space Station Freedom will be susceptible to structural mode excitation at lower frequencies from numerous random energy inputs.
- The Space Station Freedom Program Requirements Document, PDRD, has a microgravity requirement which was signed on March 26, 1990.
- The need for stabilized platforms or controlled "microgravity" experiment volumes is self-evident.

NASA Lewis Research Center | SPACE EXPERIMENTS DIVISION | SFSD Space Flight Systems Directorate

Vibration Isolation Technology Development to Demonstration

Background (Cont.)

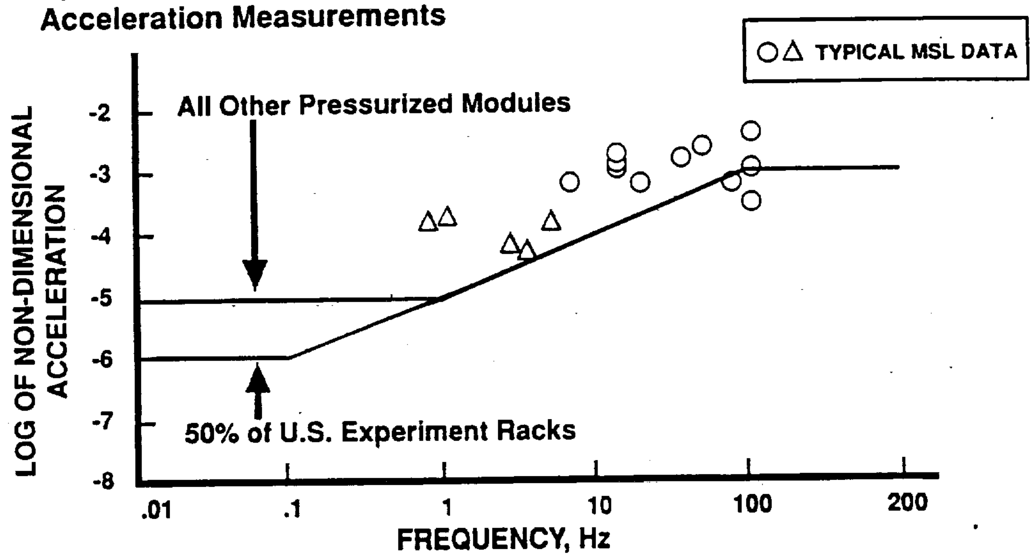
- Lewis Research Center began work on vibration isolation technology for "microgravity" experimentation in 1987.
- This technology development project was organized into three phases, requirements, development, and demonstration.

NASA Lewis Research Center | SPACE EXPERIMENTS DIVISION | SFSD Space Flight Systems Directorate

Vibration Isolation Technology Development to Demonstration

Requirements

- Space Station Microgravity Requirements and Selected Acceleration Measurements



NASA
Lewis Research Center

SPACE EXPERIMENTS DIVISION

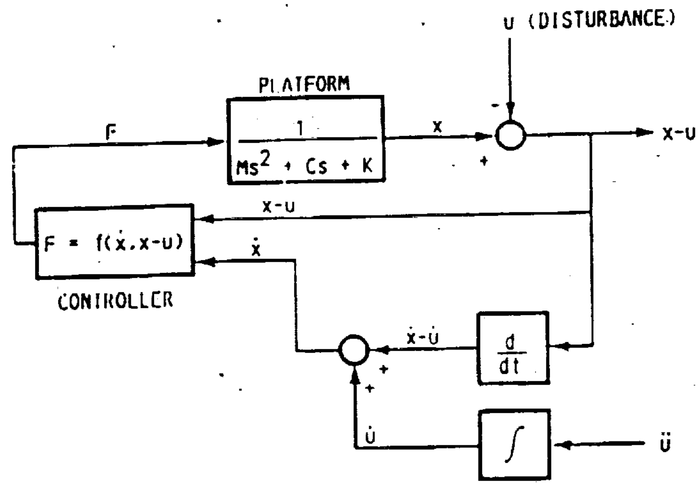
SFSD
Space Flight Systems Directorate

Vibration Isolation Technology Development to Demonstration

Development

- Active feedforward/feedback inertially referenced mass.

One Dimensional Control Block Diagram



NASA
Lewis Research Center

SPACE EXPERIMENTS DIVISION

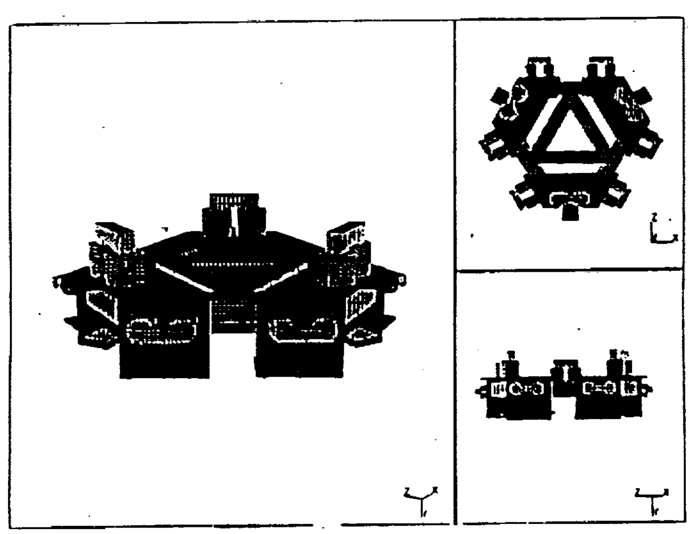
SFSD
Space Flight Systems Directorate

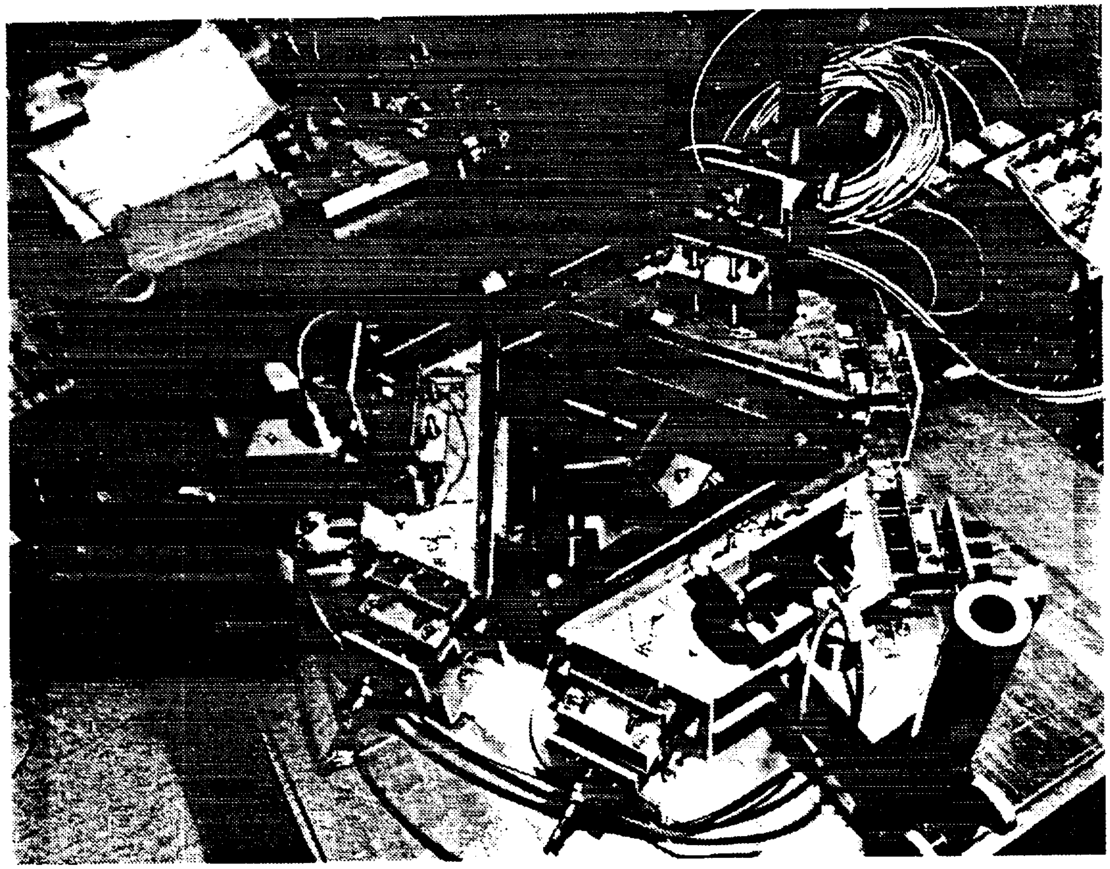
Vibration Isolation Technology Development to Demonstration

Demonstration (Cont.)

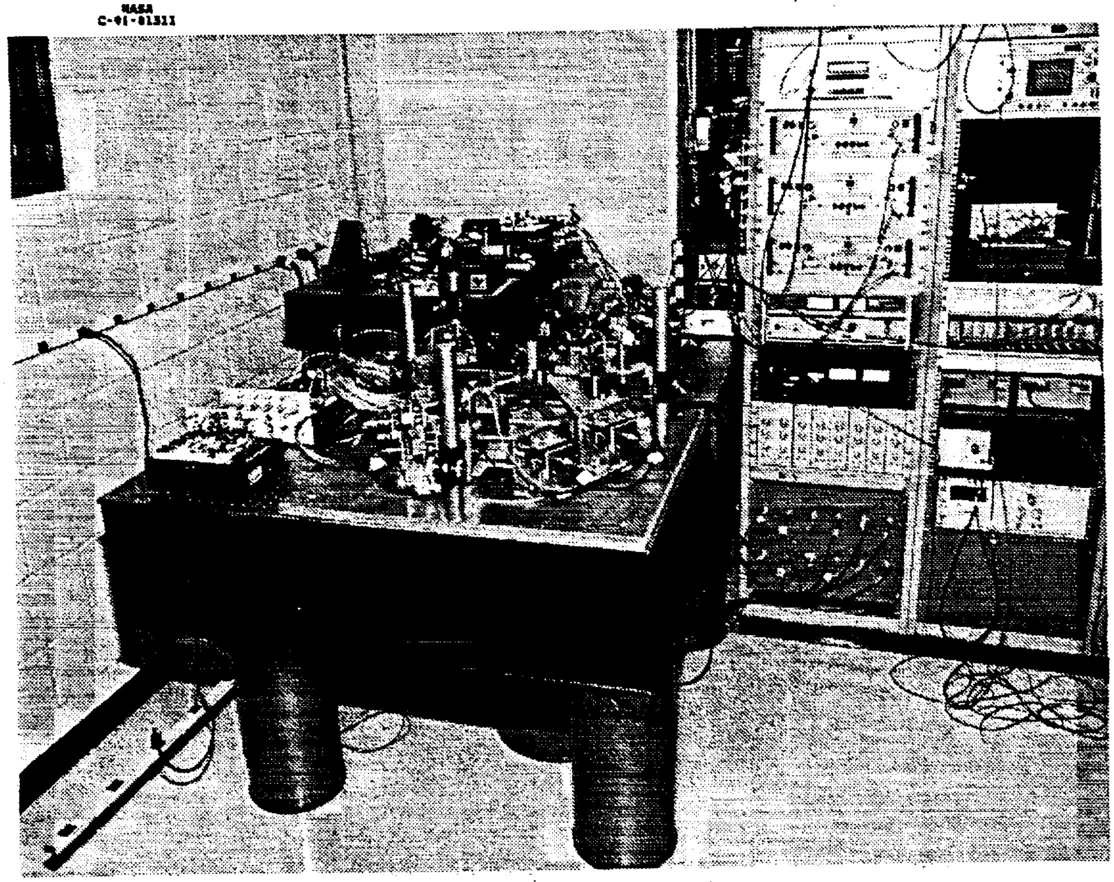
- Prototype Six Degree-of-Freedom System Design

SDRC I-DEAS 4.1: Object Modeling





NASA
C-91-01311



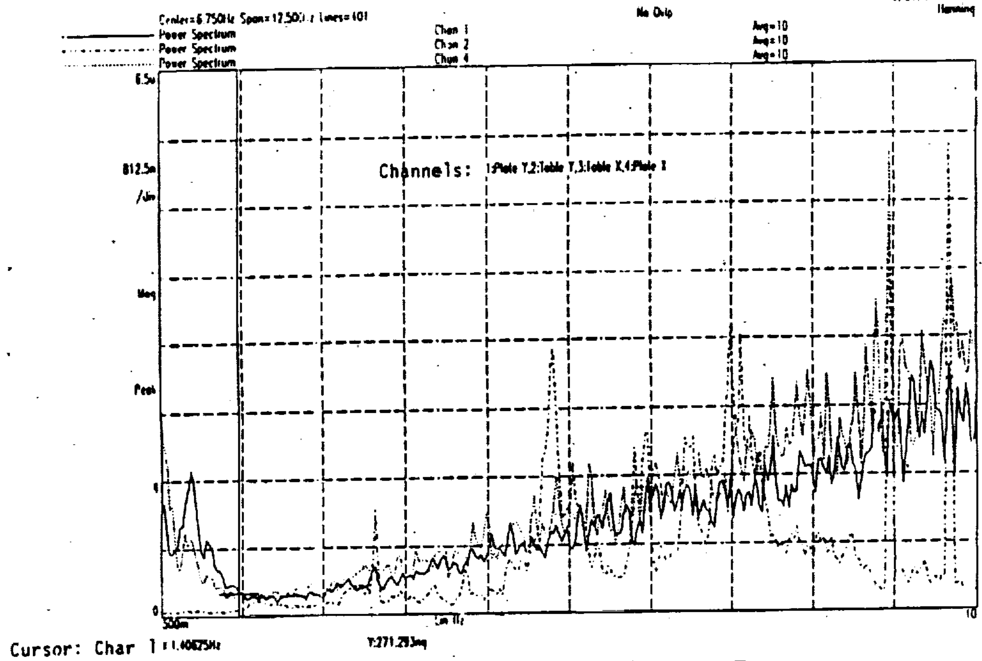
ORIGINAL PAGE
BLACK AND WHITE PHOTOGRAPH

NASA
Lewis Research Center

SPACE EXPERIMENTS DIVISION

SFSD
Space Flight Systems Directorate

Vibration Isolation Technology Development to Demonstration



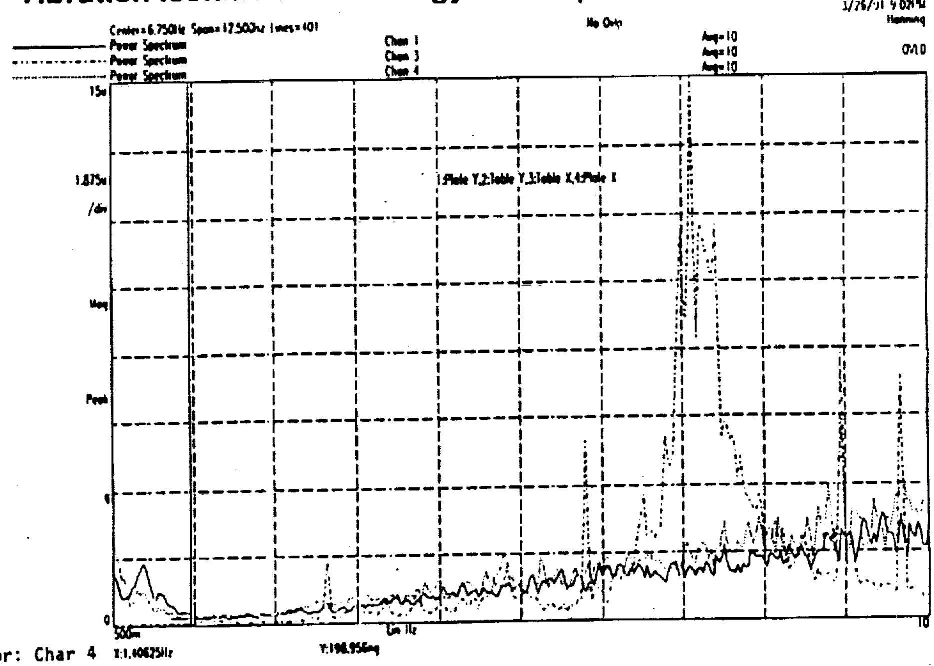
System Horizontal Noise Floor Power Spectrum

NASA
Lewis Research Center

SPACE EXPERIMENTS DIVISION

SFSD
Space Flight Systems Directorate

Vibration Isolation Technology Development to Demonstration



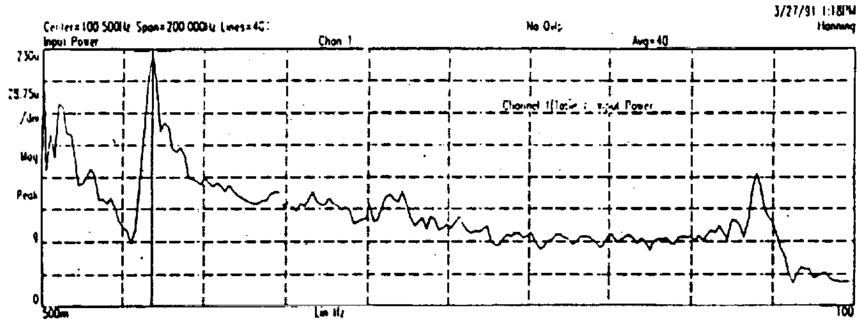
System Horizontal Noise Floor Power Spectrum

NASA
Lewis Research Center

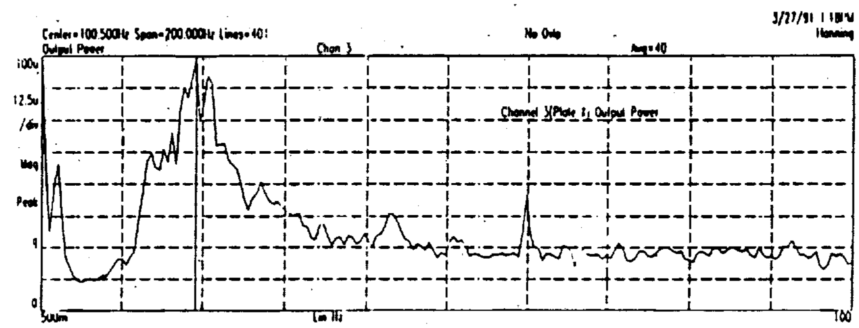
SPACE EXPERIMENTS DIVISION

SFSD
Space Flight Systems Directorate

Vibration Isolation Technology Development to Demonstration



Cursor: X:14Hz Y:278.294ueq



Cursor: X:19.5Hz Y:99.5806ueq

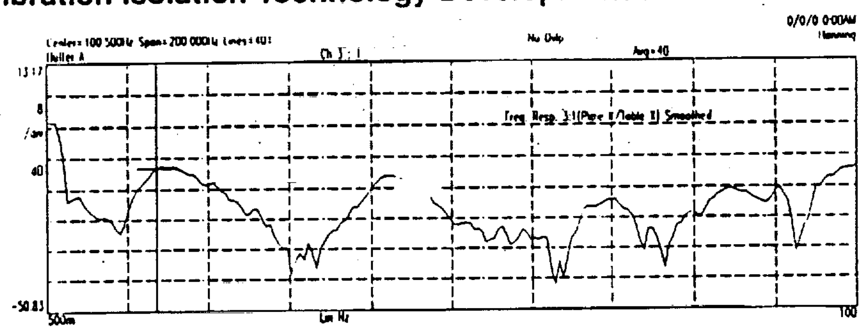
Input and Output Power for Frequency Response

NASA
Lewis Research Center

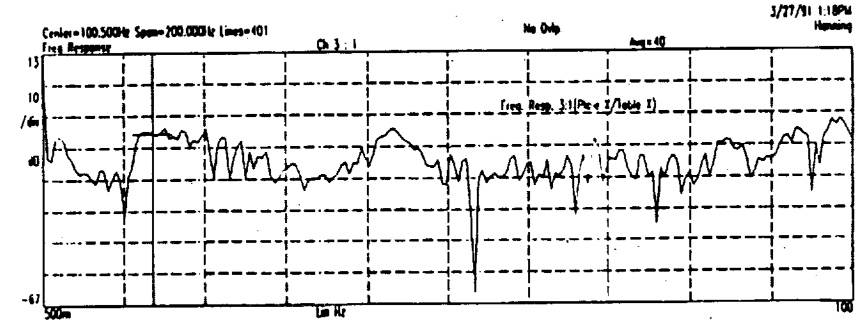
SPACE EXPERIMENTS DIVISION

SFSD
Space Flight Systems Directorate

Vibration Isolation Technology Development to Demonstration



Cursor: X:14Hz Y:-13.5335dB



Cursor: X:14Hz Y:-12.62dB

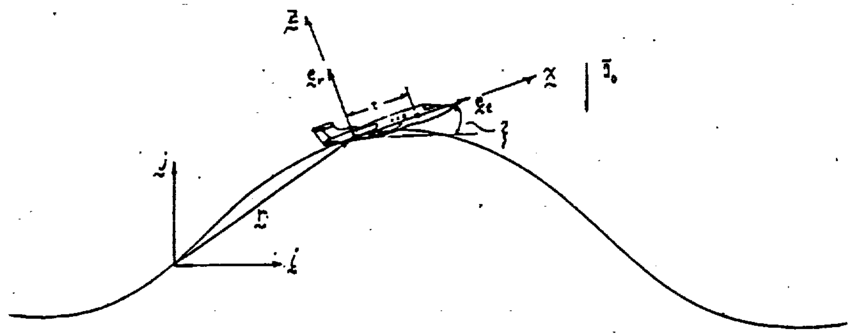
System Horizontal Transmissibility

NASA Lewis Research Center | SPACE EXPERIMENTS DIVISION | SFSD Space Flight Systems Directorate

Vibration Isolation Technology Development to Demonstration

Demonstration

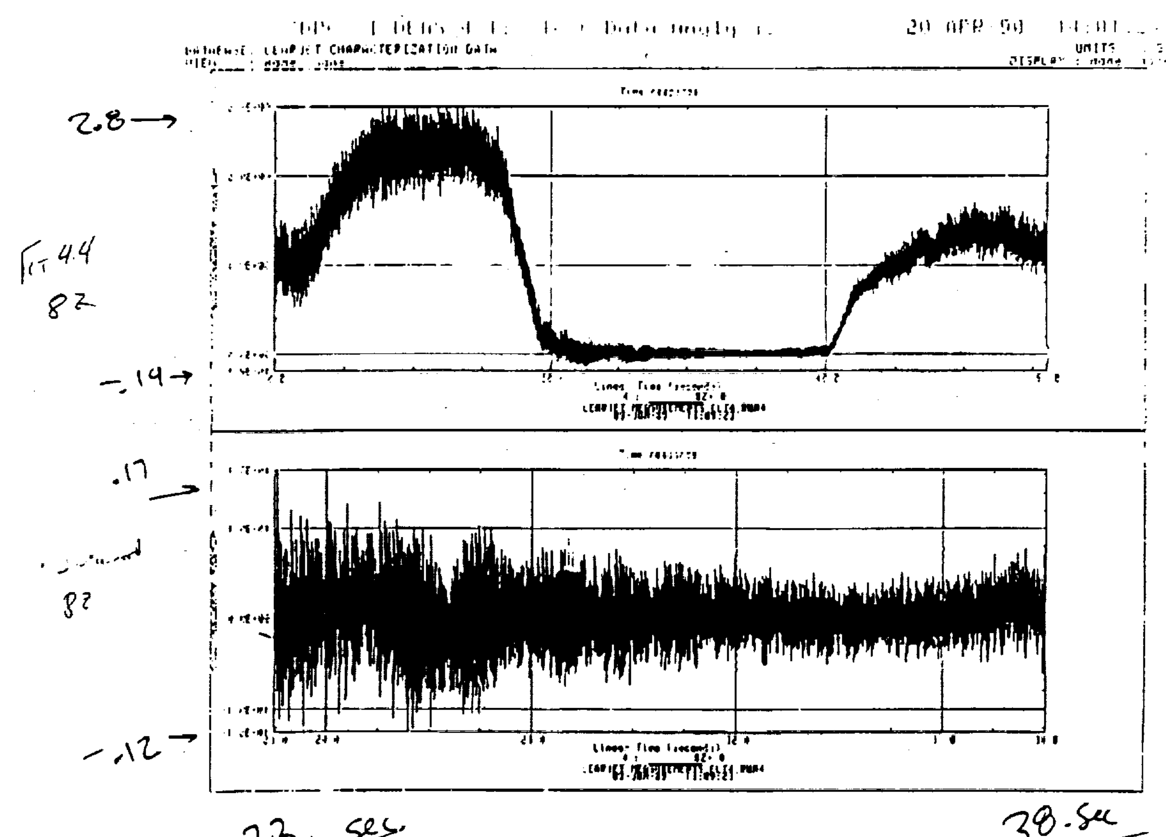
- **Low Gravity Parabolic Flight Maneuver**

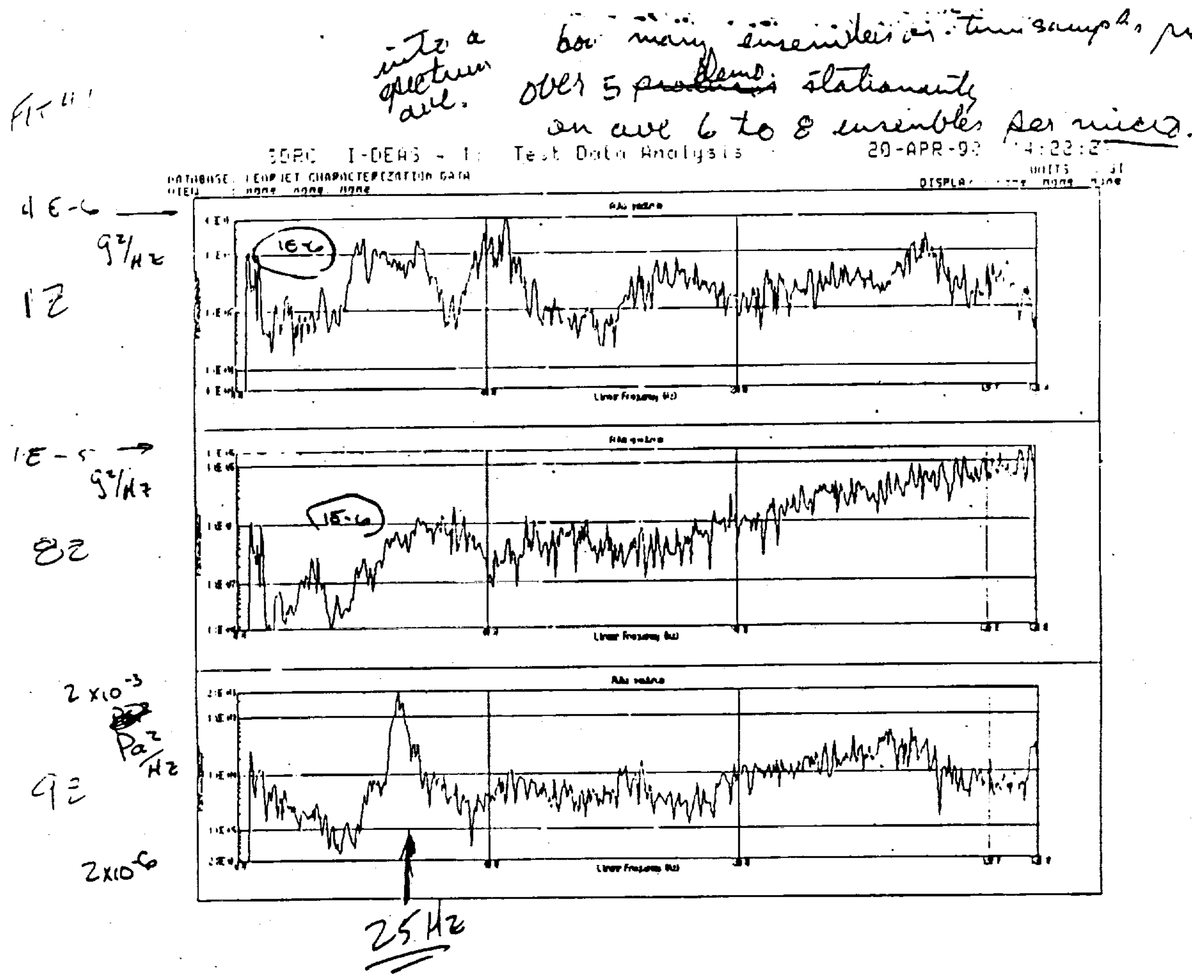


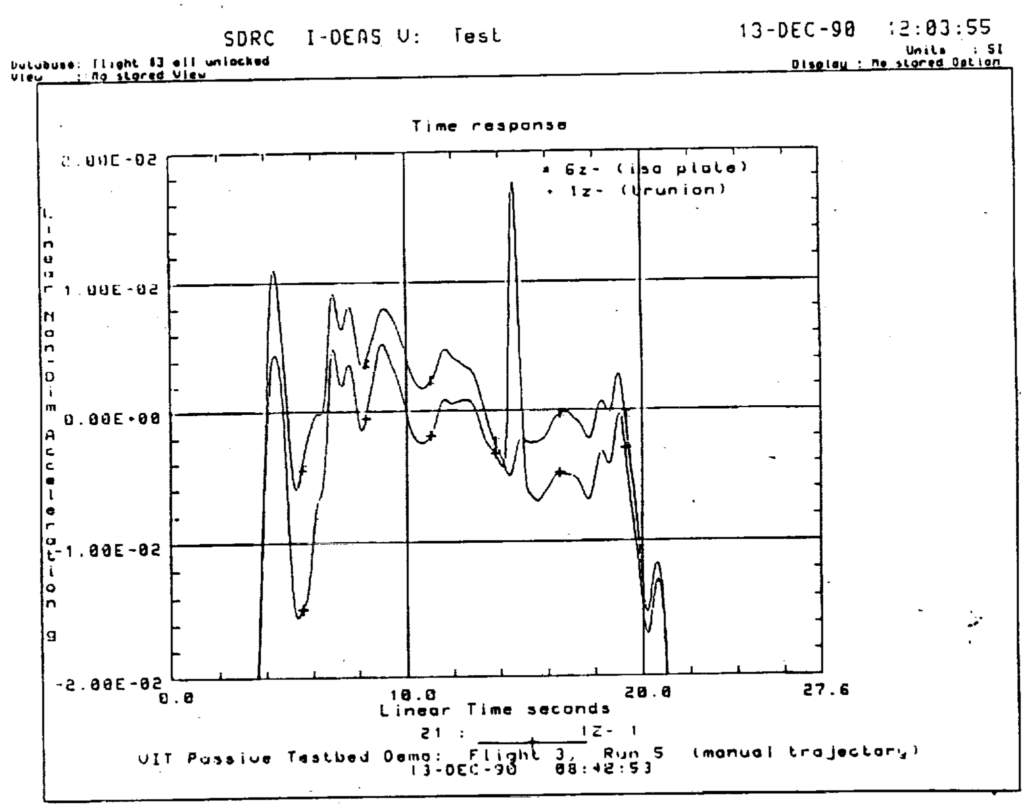
Acceleration of Location t:

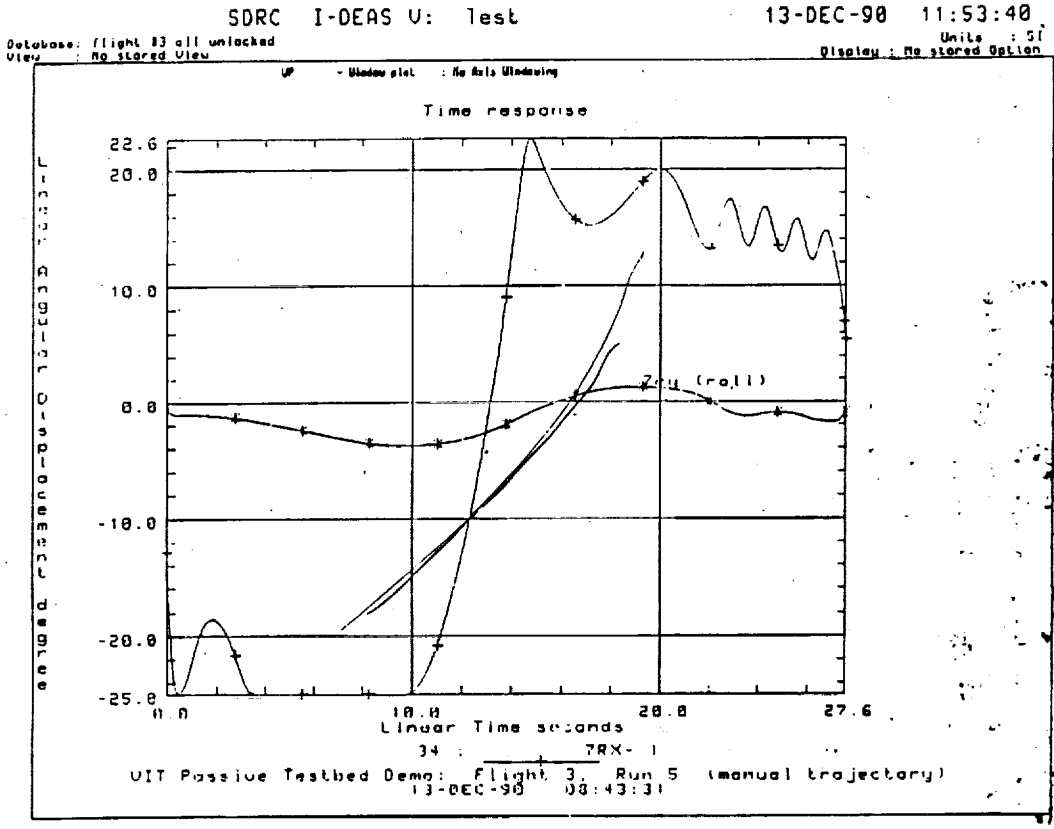
$$\ddot{r}_t = (v\dot{\xi} - t\ddot{\xi} - g_o\cos\xi)e_r + (\dot{v} - t\dot{\xi}^2 - g_o\sin\xi)e_t.$$

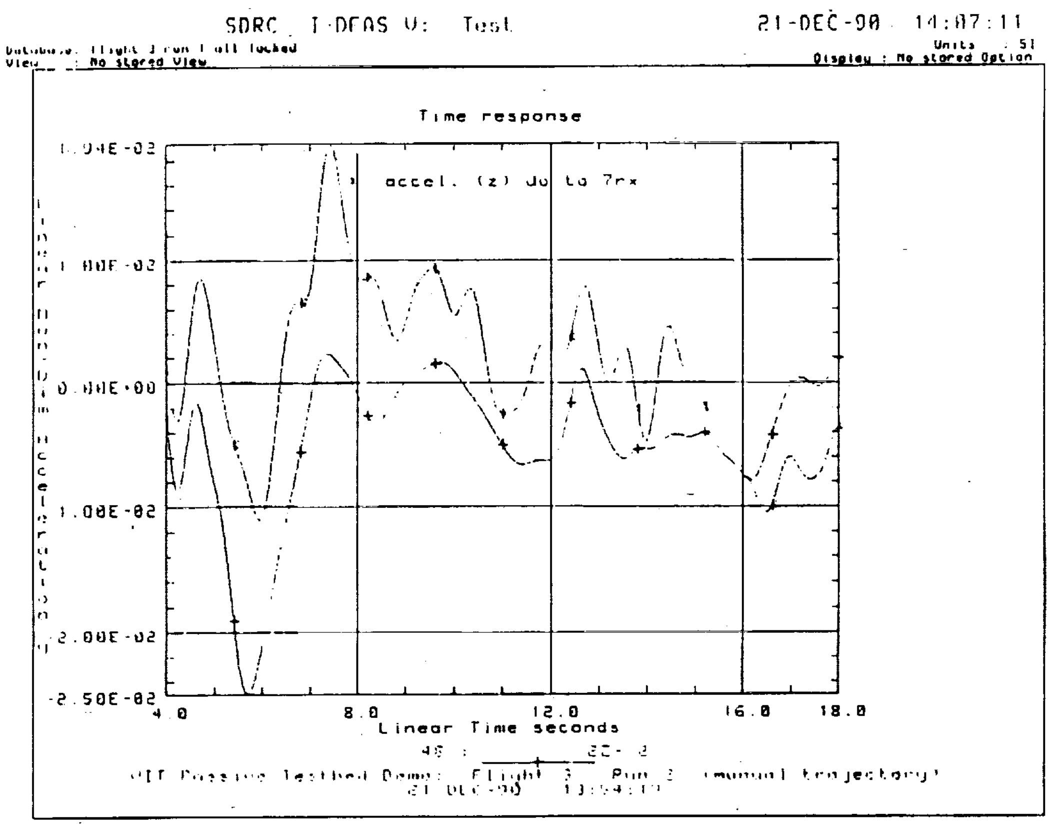
Coordinate Systems Definitions

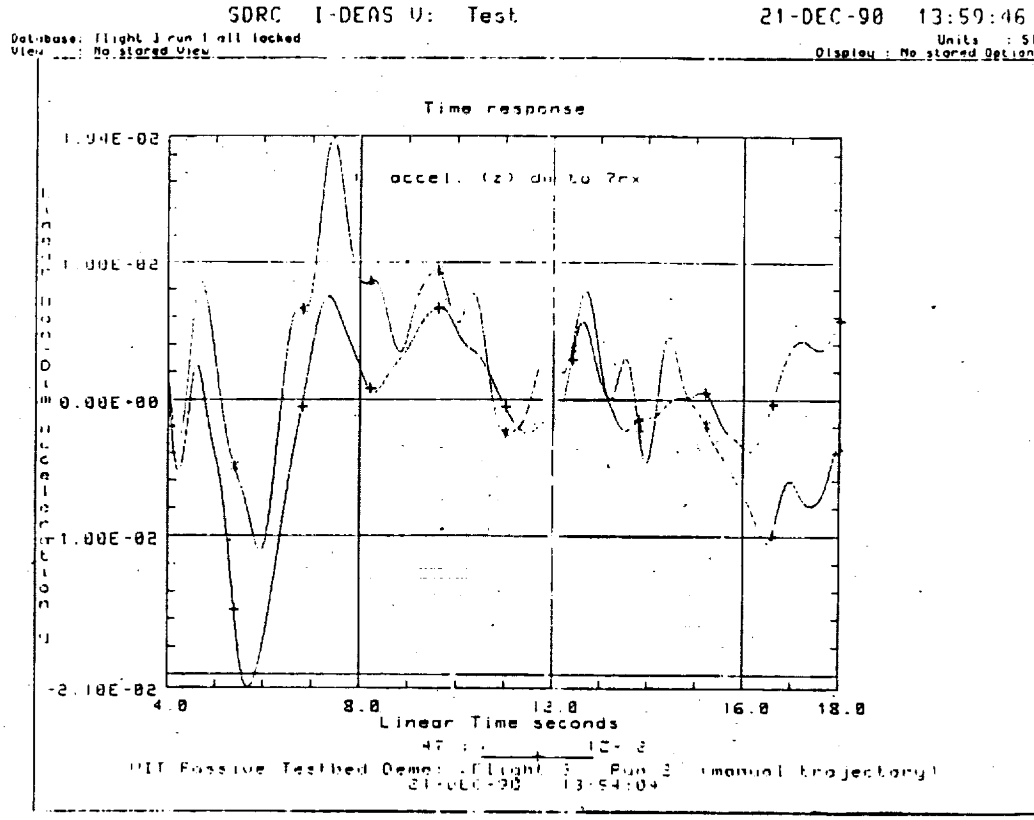



over 5 stationarity
on ave 6 to 8 ensembles per micro.
SDRC I-DEAS - I: Test Data Analysis
4E-6
g^2/Hz
1Z
1E-5
g^2/Hz
8Z
2x10-3
Pa2/Hz
9Z
2x10-6
25 Hz


SDRC I-DEAS V: Test
13-DEC-90 12:03:55
Units : SI
Database: Flight #3 all unlocked
View : No stored View
Display : No stored Option
Time response
* 6z- (iso plate)
+ 1z- (trunion)
2.00E-02
1.00E-02
0.00E+00
-1.00E-02
-2.00E-02
Linear Non-Dim Acceleration g
0.0
10.0
20.0
27.6
Linear Time seconds
VIT Passive Testbed Demo: Flight 3, Run 5 (manual trajectory)
13-DEC-90 08:42:53


SDRC I-DEAS V: Test
13-DEC-90 11:53:40
Units : SI
Display : No stored Option
View : No stored View
Time response
22.6
20.0
10.0
0.0
-10.0
-20.0
-25.0
0.0
10.0
20.0
27.6
Linear Time seconds
Linear Angular Displacement degree
7RX- 1
VIT Passive Testbed Demo: Flight 3, Run 5 (manual trajectory)
13-DEC-90 08:43:31


SDRC I-DEAS V: Test
21-DEC-90 14:07:11
Units : SI
Display : No stored Option
View : No stored View
Time response
accel. (z) du to 7rx
1.00E-02
0.00E+00
-1.00E-02
-2.00E-02
-2.50E-02
4.0
8.0
12.0
16.0
18.0
Linear Time seconds



NASA
Lewis Research Center

SPACE EXPERIMENTS DIVISION

SFSD
Space Flight Systems Directorate

Vibration Isolation Technology Development to Demonstration

Demonstration (Cont.)

- **Learjet Passive Testbed**

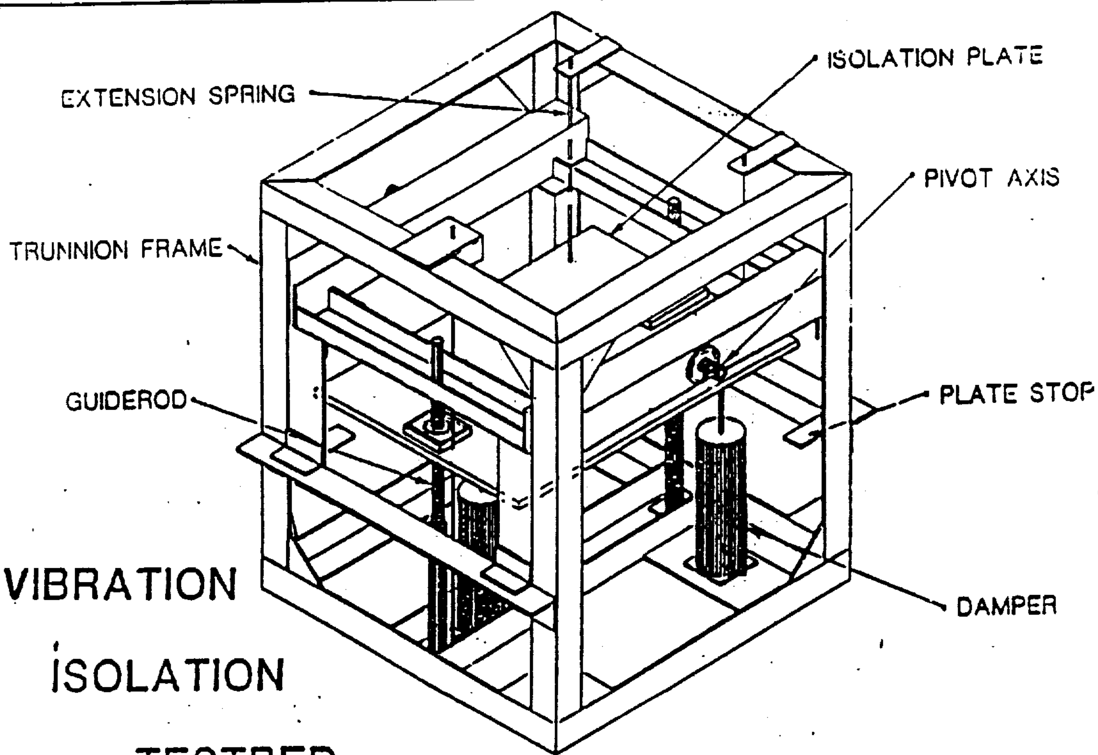
  - Trunnion Support Structure
  - Simple Passive Spring-Dashpot Test Article
  - Preliminary Low Gravity Passive Isolation Data

- **Learjet Active Testbed**

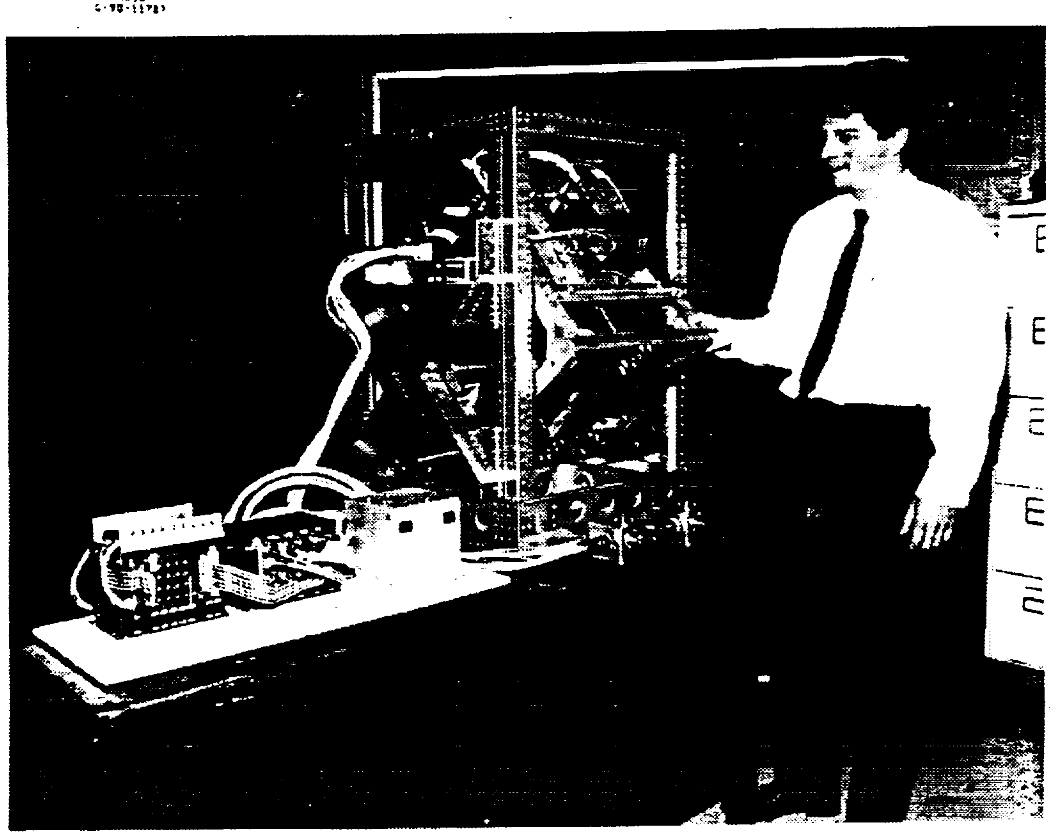
  - Design

ENGINEERING DIRECTORATE

NASA
LEWIS RESEARCH CENTER

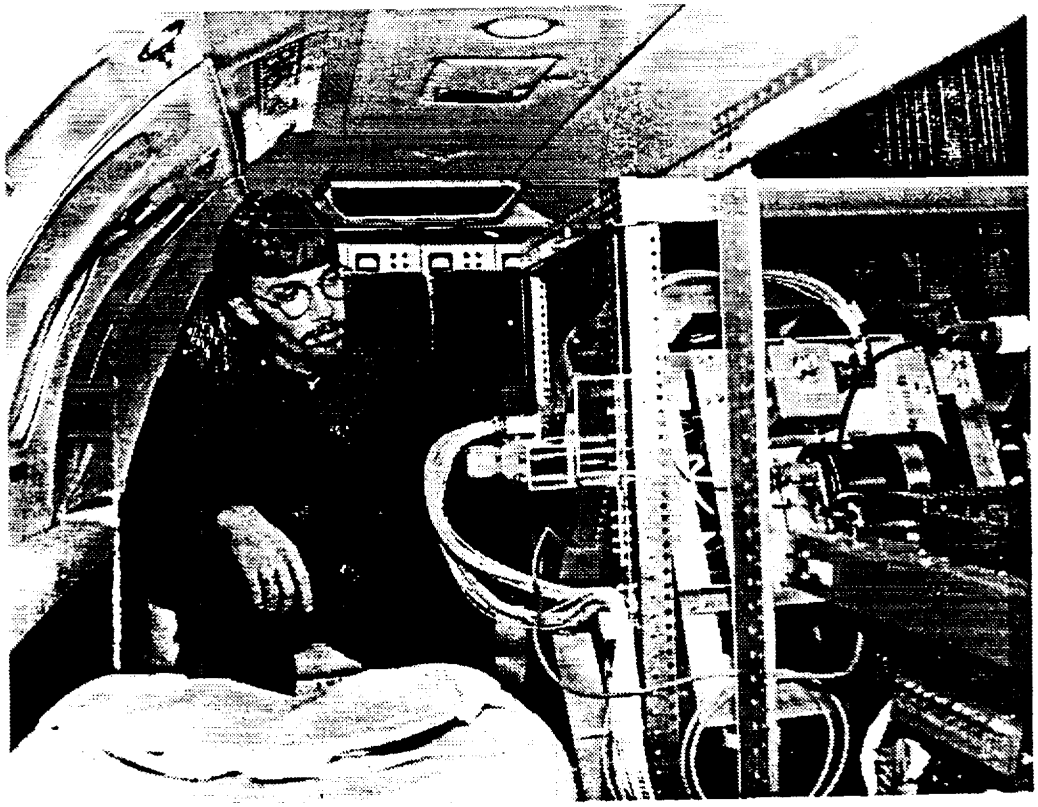


VIBRATION
ISOLATION
TESTBED

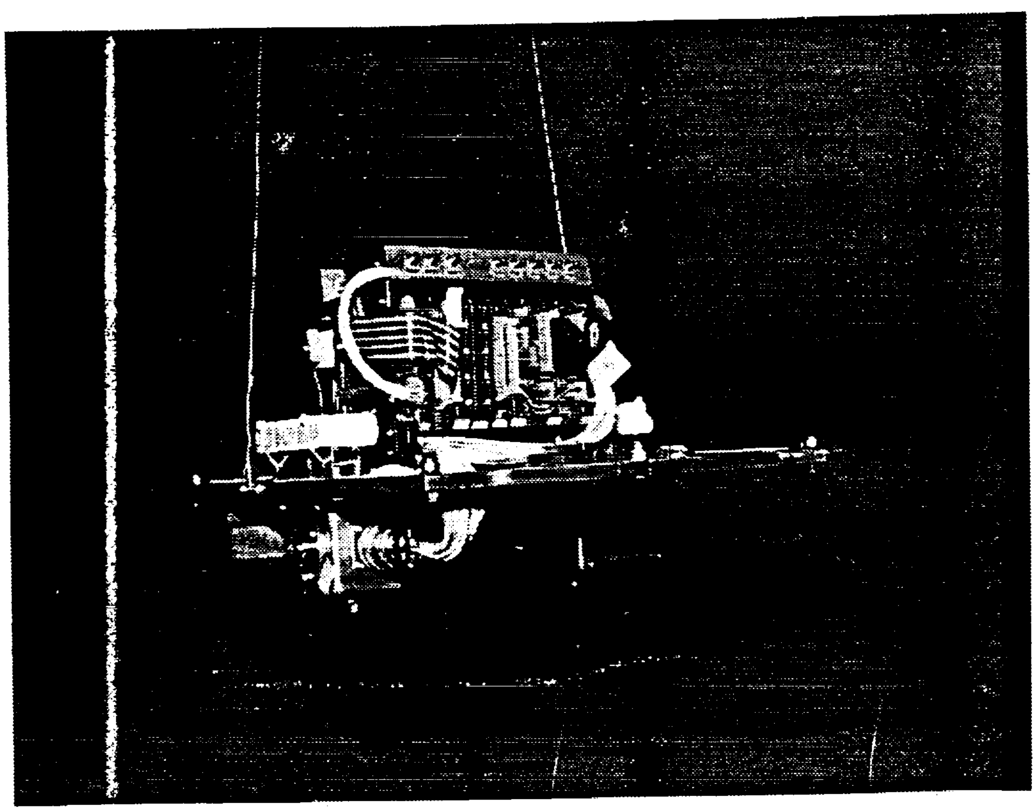


ORIGINAL PAGE
BLACK AND WHITE PHOTOGRAPH

NASA
C-90-12982



NASA
C-90-11784



ORIGINAL PAGE
BLACK AND WHITE PHOTOGRAPH

NEW INERTIAL ACTUATOR PROVIDES ISOLATION AND STABILIZATION IN MICROGRAVITY CONDITIONS

John Blackburn
Applied Technology Associates, Inc.

N92-28443

ABSTRACT

Experiments in materials and fluids processing have been conducted, or are planned, that take advantage of the low-g environment offered by space-based platforms. While the specific goals of the experiments vary, a common objective is to see improved results from the processes over those that would be obtained from similar ground-based experiments.

While the space-based processing environment does offer a low-g environment, it is not disturbance free. Results of experiments already conducted, particularly those in manned vehicles, show that spacecraft induced disturbances still limit what can be achieved in materials processing. The duration of actual micro-g level environments is shorter than desired and periods of milli-g activity levels are not uncommon. While small scale experiments can be configured to overcome some of the vehicle disturbance sources, larger scale processing devices and commercial activities will require alternative methods to reduce the influences of spacecraft vibrations.

Applied Technology Associates, Inc., (ATA), in cooperation with NASA Lewis Research Center, is developing hardware to provide a sustained micro-g experiment environment. This work is being sponsored under a Small Business Innovation Research (SBIR) program, which is currently in the Phase II development stage. ATA's approach is based on an inertial actuator, which when used as part of a closed-loop stabilization system, rejects unwanted disturbances to the experiment package. A prototype of the actuator has been fabricated and used in a laboratory demonstration to prove the principle of operation.

ATA's presentation will emphasize the development and testing of the Digital Materials Processing Experiment (DAMPER) inertial actuator. Physical and performance characteristics of the device will also be presented. Technical issues, including further optimization of the actuator's performance and plans for additional laboratory experiments, will also be covered.

ATA Applied Technology Associates, Inc.

NEW INERTIAL ACTUATOR PROVIDES ISOLATION AND STABILIZATION IN MICROGRAVITY CONDITIONS

Presented to:
International Workshop on Vibration Isolation
Technology for Microgravity Science Applications
Cleveland, Ohio

Presented by:
John Blackburn

Applied Technology Associates, Inc.
1900 Randolph Road, SE
Albuquerque, NM 87106
(505) 247-8371

April 22, 1991

ATA

BACKGROUND

- Actuator Development Sponsored Under the Small Business Innovation Research (SBIR) Program
- Digital Active Materials Processing Experiment (DAMPER) Employs Inertial Actuators to Counteract Vibration Disturbances Acting on Host Materials Processing Platform
- Inertial Actuator is an Innovative Means of Performing Closed-Loop Vibration Control Through Momentum Exchange

G

ATA

DAMPER EXPERIMENT OBJECTIVES

(1) Quantify in a Spectral Sense, the Disturbances which Act on the System

(2) Assess the Requirements of the Isolation System, i.e., the Maximum Allowable Residual Motion of the Platform While Under Active Control

(3) Develop Inertial Isolator, Sizing Its Components and Tailoring Its Frequency Response to Reject the Predicted Disturbances

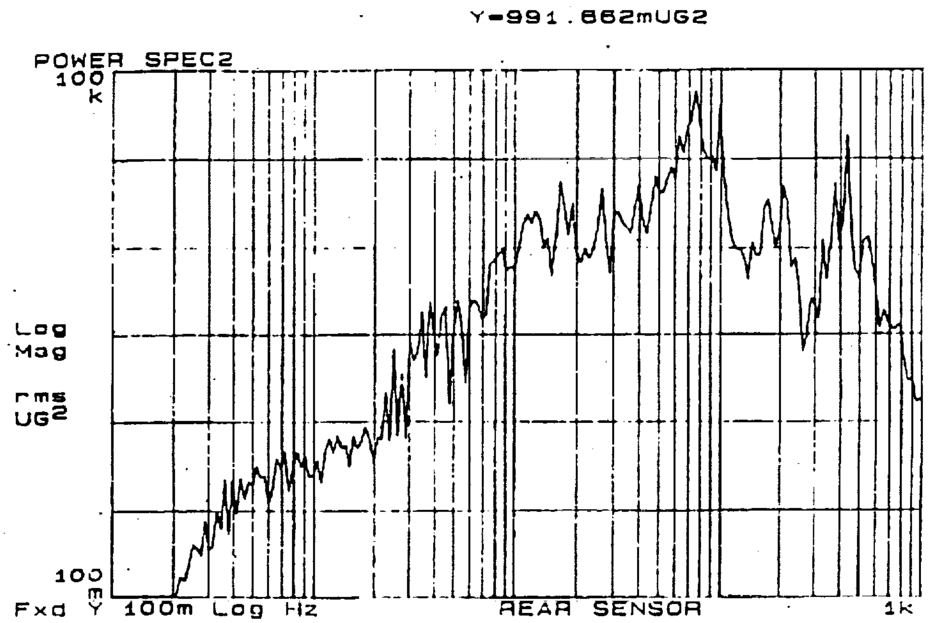
(4) Demonstrate in One Dimension, the Performance of the Actuator in a Closed-Loop Disturbance Rejection Control System

(5) Demonstrate the Use of Inertial Actuators in a System Which Actively Controls Vibration in Three Axes Simultaneously

(6) Demonstrate "Non-Tethered" Operation

ATA

DISTURBANCE ENVIRONMENT ON AIR BEARING



- This Environment is Expected to be More Severe than that Encountered in Space-Based Microgravity Experiments

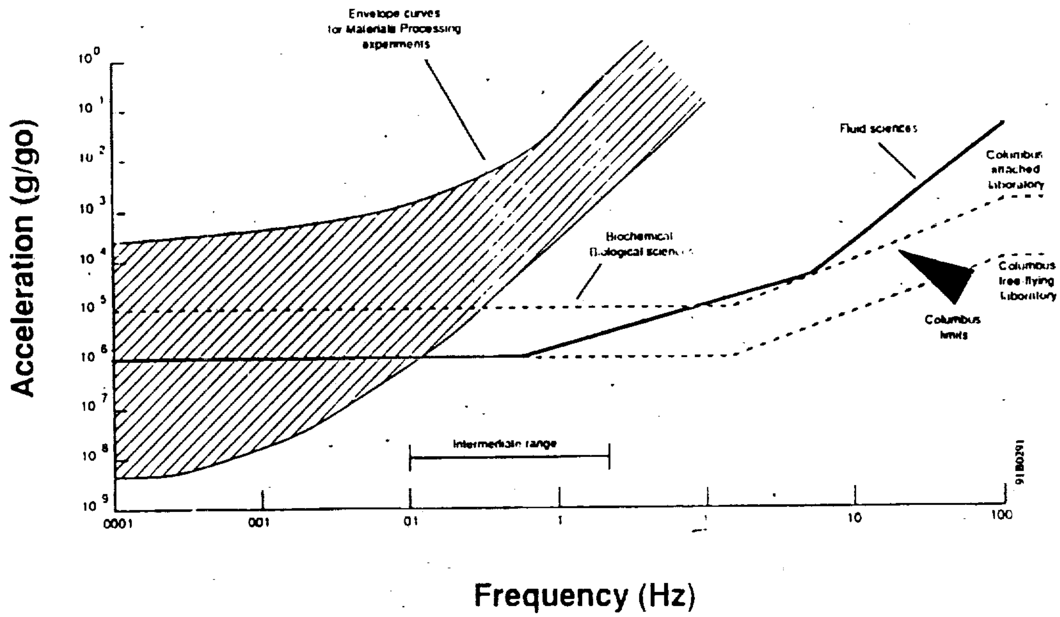
ATA

ISOLATION SYSTEM REQUIREMENTS

- Published Sensitivities of Microgravity Experiments to Vibration Dictate Allowable Residual Vibration

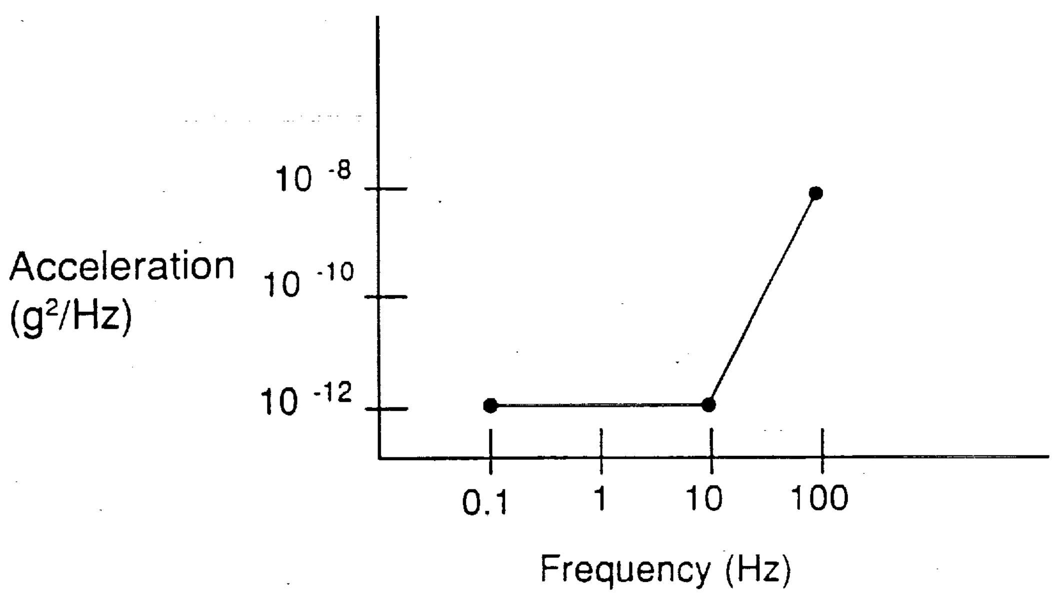
- Isolation System Requirements Expressed as an "Envelope" Based on Published Sensitivities

ATA — ACCELERATION SENSITIVITY OF MICROGRAVITY EXPERIMENTS



(From International Astronautical Federation)

ATA — REQUIREMENTS ENVELOPE



ATA

INERTIAL ACTUATOR SPECIFICATIONS

Size:	7.20" (18.3 cm) Long x 3.43 (8.70 cm) High
Weight:	8.1 lb (3.7 kg)
Force Constant:	Composite Coil Form 0.5 N/A Air Bearing 0.5 - 1 N/A
Stroke:	±2.0" (±5.1 cm)
Frequency Response:	0 - 500 Hz
Peak Force:	Composite Coil Form 2 N Air Bearing Coil Form > 2 N
Peak Acceleration:	For 100 lb Payload ≈ 4.5 mg

ATA

ACTUATOR CONFIGURATION FEATURES

- Simple Drive Electronics can be Adapted to Incorporate:
  - Dither Signal
  - Analog Control Utilizing Position Sensing Coils
  - Supports Either Acceleration or Position Control

- Simple Operating Principle Means Design can be Readily Adapted to Accommodate Longer Configurations/Larger Moving Mass with Predictable Performance

- High Bandwidth DC > 500 Hz

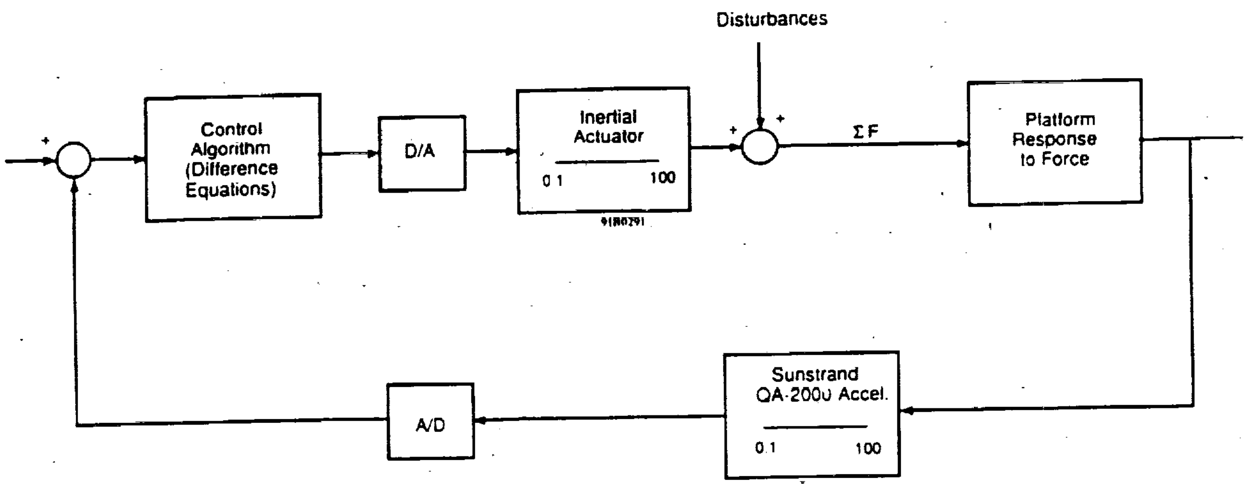
ATA

Color VG of Inertial Actuator

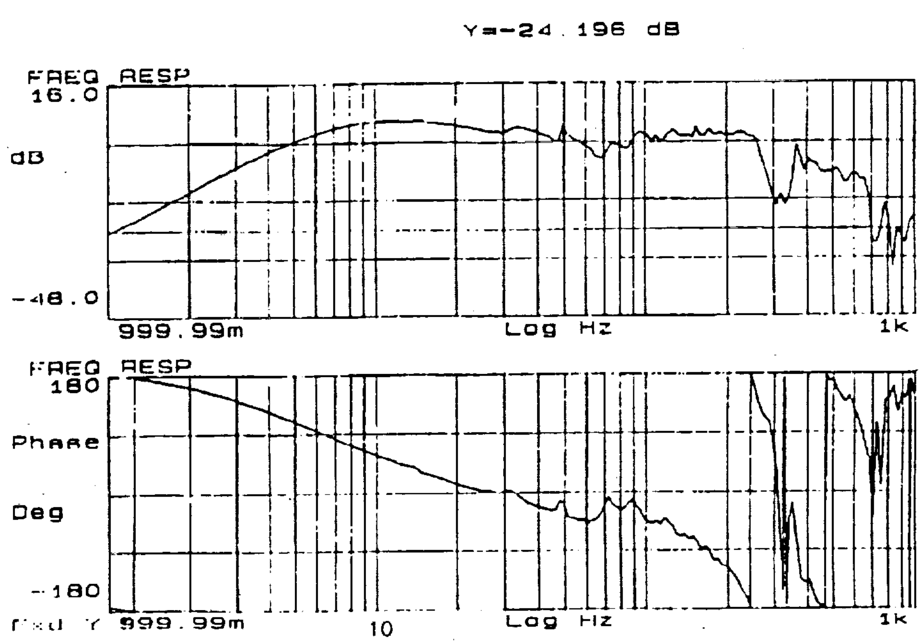
ATA

Color VG of DAMPER 1-DOF Platform

1-DOF CONTROLLER FORM



OPEN-LOOP RESPONSE OF UNCOMPENSATED 1-DOF CONTROL SYSTEM



CONTROL ALGORITHM

- 8th Order Low Pass Filter Algorithm Designed for Minimal Phase Loss

$$C(s) = \left[K_1 \frac{(s+\omega_1)(s+\omega_2)}{(s+\omega_3)(s+\omega_4)}\right]^2 \left[K_2 \frac{(s+\omega_5)(s+\omega_6)}{(s+\omega_7)(s+\omega_8)}\right]^2$$

$\omega_1 = 2\pi(500)$ $\omega_5 = 2\pi(2000)$

$\omega_2 = 2\pi(800)$ $\omega_6 = 2\pi(2500)$

$\omega_3 = 2\pi(150)$ $\omega_7 = 2\pi(600)$

$\omega_4 = 2\pi(1000)$ $\omega_8 = 2\pi(3000)$

$$K_1 = \frac{\omega_3 \omega_4}{\omega_1 \omega_2}$$

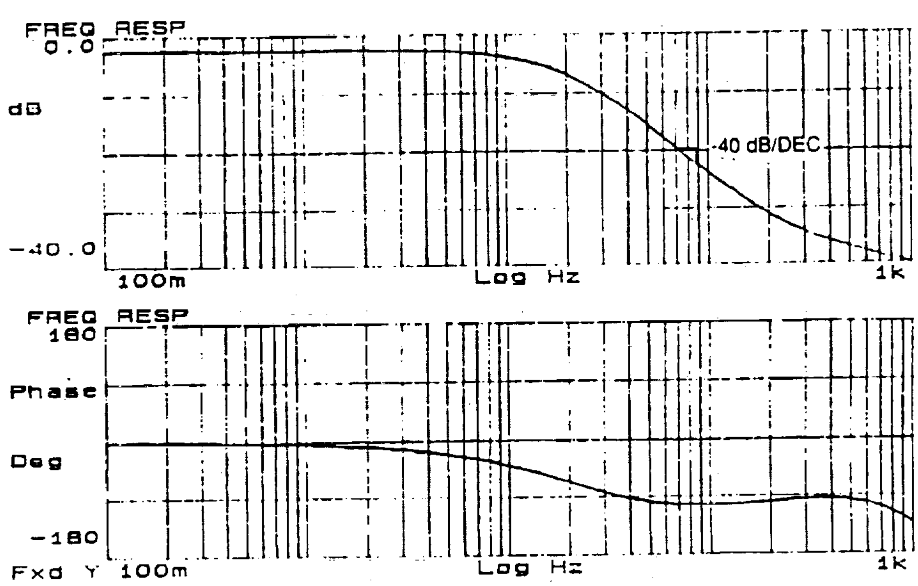
$$K_2 = \frac{\omega_7 \omega_8}{\omega_5 \omega_6}$$

- Implemented as 4 2nd-Order "Blocks" in DSP Software

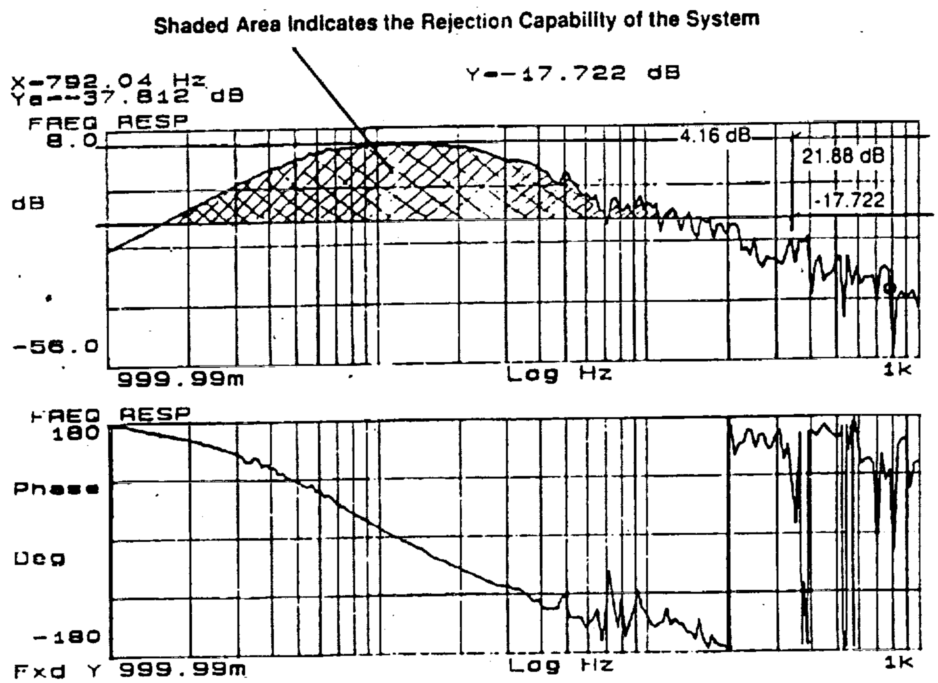
- "Blocks" are Translated to Difference Equations

ATA

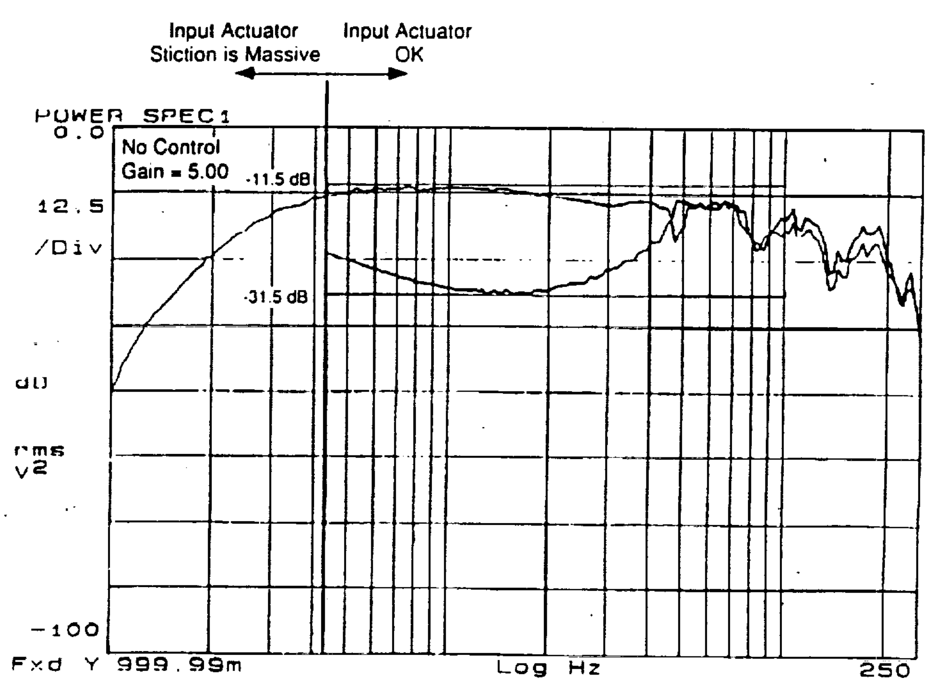
BODE PLOTS OF CONTROL ALGORITHM



OPEN-LOOP RESPONSE OF COMPENSATED 1-DOF CONTROL SYSTEM

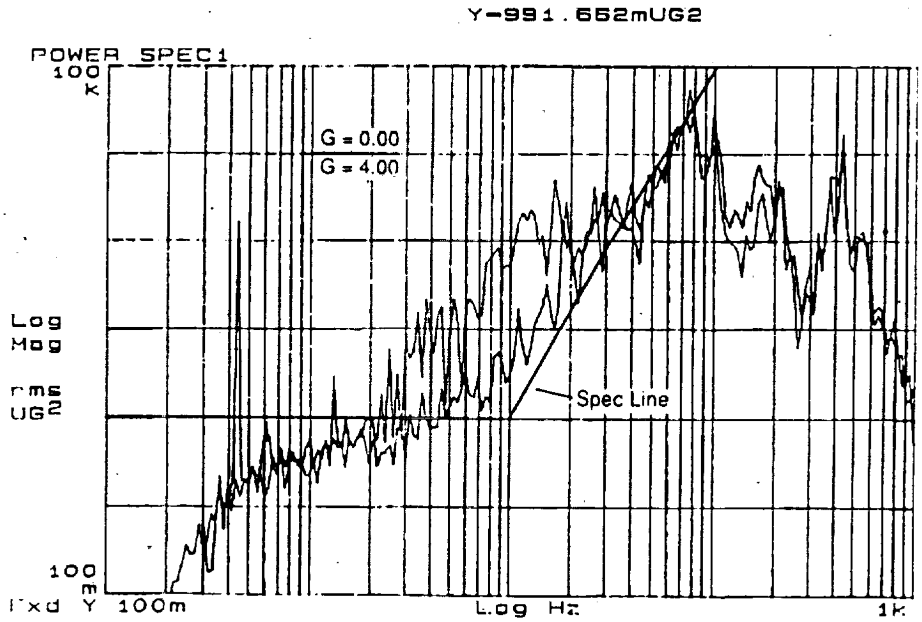


ACCELERATION PSDs INJECTING DISTURBANCE INTO SYSTEM WITH AND WITHOUT CONTROL



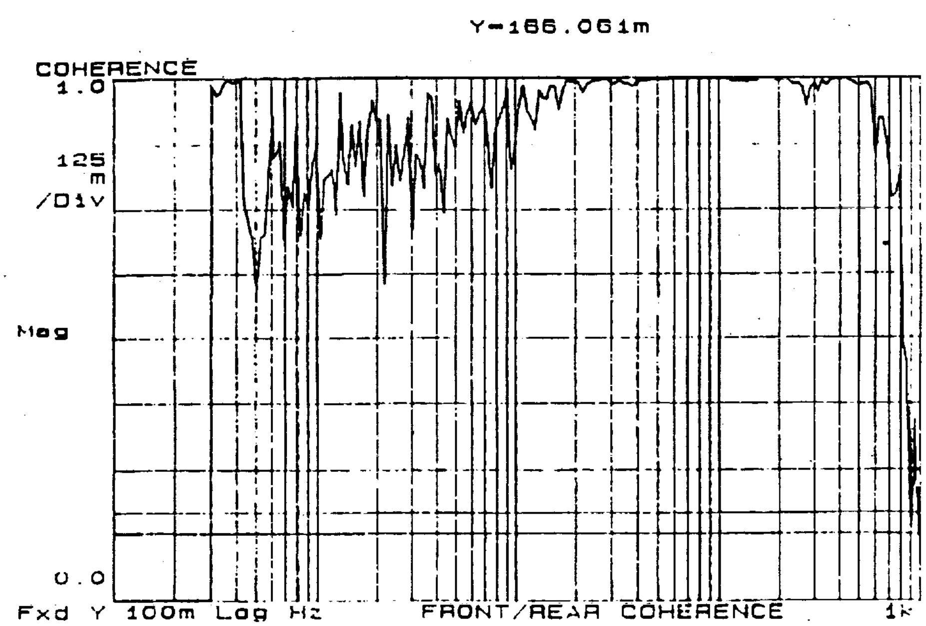
ACCELERATION PSDs: AMBIENT ACCELERATION OF PLATFORM AND PLATFORM UNDER CONTROL

- Noise Floor of QA-2000 Prohibits Improved Performance



POOR COHERENCE BETWEEN PARALLEL SENSORS INDICATES POOR NOISE PERFORMANCE

- Performance of Isolation System Can Be Enhanced by Employing a Sensor Having a Lower Noise Floor



- **Isolation Technology Developed for 1-DOF Experiment is Being Used to Control Vibration in 3 Axes Simultaneously**

Color VG of 3-DOF DAMPER Platform

- SBIR Program Phase I Work Has Been Completed
- Phase II Work to be Completed 9/91
- Applied Technology Associates, Inc., is Currently Addressing Phase III Work by Performing Marketing Research into Potential "Spinoff" Applications of Inertial Actuator Technology
- Phase III Could Involve the Participation of Other Firms Interested in Commercial Applications of the Actuator, Either Ground- or Space-Based

N92-28444

A SIX DEGREE OF FREEDOM LORENTZ FORCE VIBRATION ISOLATOR WITH NONLINEAR CONTROLLER

Ralph Fenn and Bruce Johnson
SatCon Technology Corporation

ABSTRACT

Many of the proposed uses of Space Station are predicated on its capability to provide a low acceleration environment across a broad spectrum of frequencies. Vibration isolation technology to attenuate Space Station accelerations will be an enabling technology for many space-based experiments. These experiments' stringent acceleration requirements are lower than the quiescent Space Station acceleration levels, necessitating the use of vibration isolation.

This program demonstrated the technical feasibility of constructing large-stroke magnetic suspensions that can meet the active vibration isolation requirements of Space Station. These requirements include: (1) strokes over 1 cm in all directions, (2) actuator bandwidths over 100 Hz, (3) isolator roll-off frequencies below 10^{-2} Hertz, and (4) force capability over 1 Newton in all axes. The 100 Hz actuator bandwidth allows the suspension to reject any direct force disturbances that act on the microgravity experiment, for example forces created by cable connections. The low isolator roll-off frequency and large stroke allow the magnetic suspension to isolate the microgravity experiment from Space Station vibrations above the roll-off frequency. The capability to meet these requirements was demonstrated by designing, constructing and testing a six-degree-of-freedom, prototype magnetic suspension system that featured high-performance, Lorentz-force actuators and full multi-input, multi-output control. This prototype suspension is designed to isolate large orbiter locker experiments under typical spacecraft constraints of size, weight, and power. Suspension in the full six-degrees-of-freedom was successfully demonstrated in this program while using a gravity-force unload mechanism to simulate a space environment. The prototype isolator is capable of space-based isolation service with relatively minor modification.

The use of advanced, nonlinear control algorithms were investigated on a specially designed single-degree-of freedom testbed. This low acceleration test facility simulates the Space Station vibration isolation problem in a single horizontal axis with low-friction, air-slide support. This allowed testing at the desired microgravity levels, without the gravity bias effects that are seen in a full six-degree-of-freedom suspension. Precision components were used to reduce residual accelerations to microgravity levels so that the effects of sensor, actuator, and electronic noise could be evaluated. During the Phase II program, this testbed was used to demonstrate the advantages of nonlinear control algorithms to provide increased vibration isolation performance.

PRECEDING PAGE BLANK NOT FILMED

A SIX DEGREE OF FREEDOM LORENTZ FORCE VIBRATION ISOLATOR WITH NONLINEAR CONTROLLER

International Workshop on
VIBRATION ISOLATION TECHNOLOGY

NASA LEWIS RESEARCH CENTER

April 23, 1991

Presentation by:

Ralph Fenn
Bruce Johnson

SatCon Technology Corporation
12 Emily Street
Cambridge, MA 02139
(617) 661-0540

PRESENTATION OVERVIEW

MICROGRAVITY EXPERIMENT ISOLATION REQUIREMENTS

SIX DEGREE OF FREEDOM SUSPENSION SPECIFICATIONS

SATCON SIX DEGREE OF FREEDOM LORENTZ FORCE ISOLATOR

PROTCTYPE SPACE BASED SYSTEM
- DESIGN
- HARDWARE
- TEST RESULTS

SATCON SINGLE DEGREE OF FREEDOM TESTBED

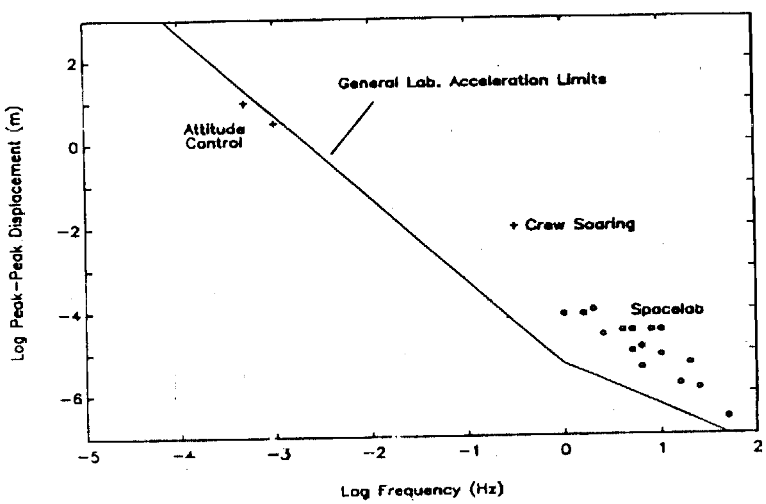
LOW ACCELERATION PRECISION TEST FACILITY
- DESIGN
- HARDWARE
- TEST RESULTS

NONLINEAR CONTROL

SIMULATIONS

TEST RESULTS

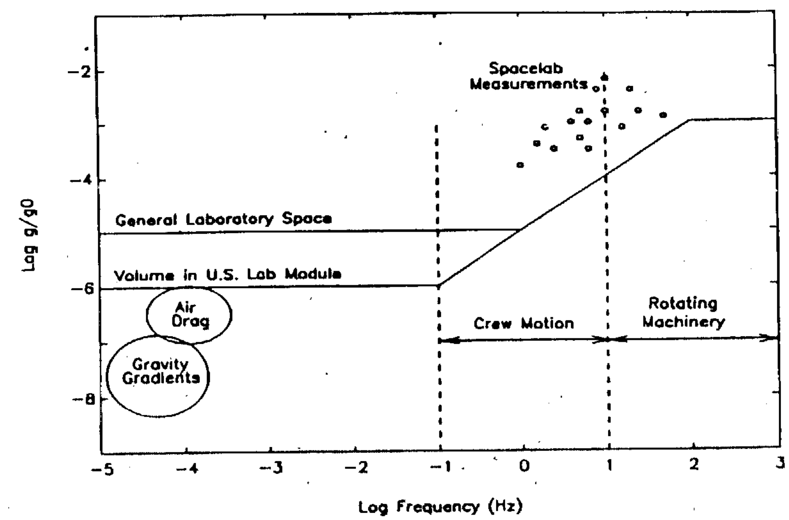
VIBRATION AMPLITUDE MEASUREMENTS AND LIMITS



SOLID LINE IS THE VIBRATION POSITION AMPLITUDE LIMIT

± 1 CM STROKE IS REQUIRED FOR ISOLATION DOWN TO 0.04 Hz

EXPERIMENT REQUIREMENTS VS THE ENVIRONMENT



CHARACTERISTIC FREQUENCY RANGES

LOW FREQUENCIES DON'T REQUIRE ISOLATION

HIGH FREQUENCIES (10-100Hz) REQUIRE ISOLATION BUT EASY
MECHANICAL SUSPENSIONS

MIDDLE FREQUENCIES (0.04-10Hz) HARDEST; CREW MOTION
- LOW CROSSOVER
- LARGE STROKE

INTERSECTION OF ENVIRONMENT AND REQUIREMENT PLOTS AT .04 Hz

THE ADVANTAGES OF LORENTZ FORCE ACTUATORS

(FORCE α CURRENT TIMES FLUX DENSITY)

ADVANTAGES:

- OPEN-LOOP STABILITY (FACILITATES NON-LINEAR CONTROLLERS)
- INHERENT ZERO GRAVITY ISOLATION AT ZERO CURRENT
- CAN ISOLATE TO LOWER FREQUENCIES THAN FERRO-ATTRACTIVE ACTUATORS.
- MECHANICAL SIMPLICITY FACILITATING SIX DOF DESIGNS
- EASY INTEGRATION WITH STANDARD ELECTRONICS

DISADVANTAGES:

- HIGH MASS PER UNIT FORCE - HOWEVER ONLY SMALL FORCES REQUIRED HERE, ESPECIALLY IF "FREE FLYING" AT LOW FREQUENCIES.
- HIGHER FORCES AVAILABLE FOR INTERMITTENT USE.

THE ADVANTAGES OF ACTIVE SUSPENSIONS

- ISOLATE TO LOWER FREQUENCIES
- COUNTERBALANCE DIRECTLY APPLIED FORCES
- FLEXIBILITY MATCHES EXPERIMENT AND ENVIRONMENT
- CREATE SPECIAL SUSPENSION CHARACTERISTICS, E.G., NONLINEARITY.

SIX DOF ISOLATOR DESIGN

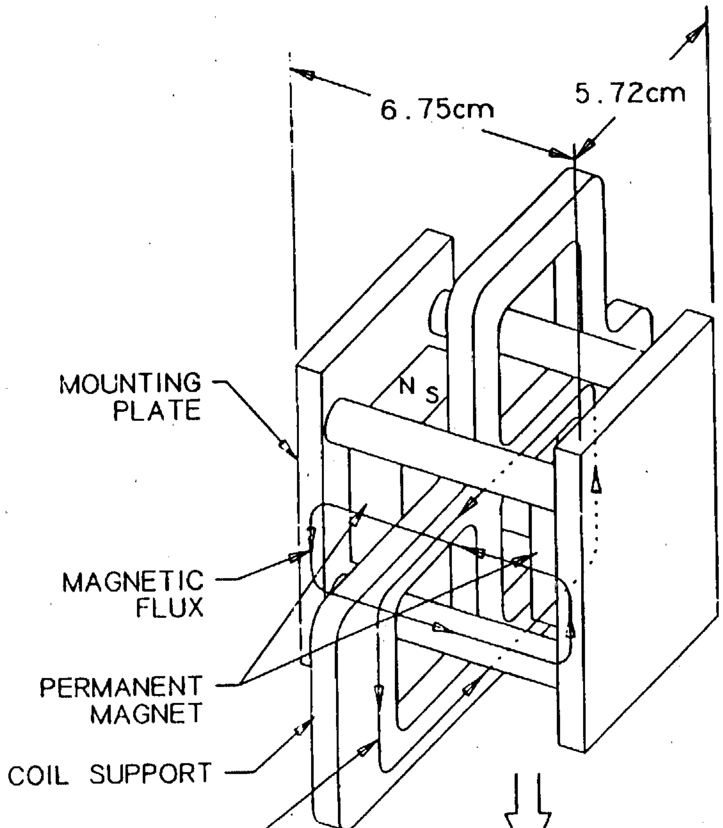
FORCE	4 N	(EXCEEDS REQUIREMENTS)
DIMENSIONS:		
STROKE	+/- 1 CM	
WIDTH AND DEPTH	40 CM	(ORBITER LOCKER SIZE)
HEIGHT - EDGE - CENTER	11.5 CM 8 CM	
WEIGHT:		
4 ACTUATORS	2.9 KG	
ALUMINUM STRUCTURE	2.0 KG	
TOTAL	4.9 KG	
ISOLATOR POWER:		
1 N Z AXIS	5 W	
1 N X OR Y AXIS (EXPER CG 15 CM UP)	20 W	
OPEN-LOOP ACTUATOR BW:		
	> 100 HZ	

SatCon Technology Corporation V023-8-91

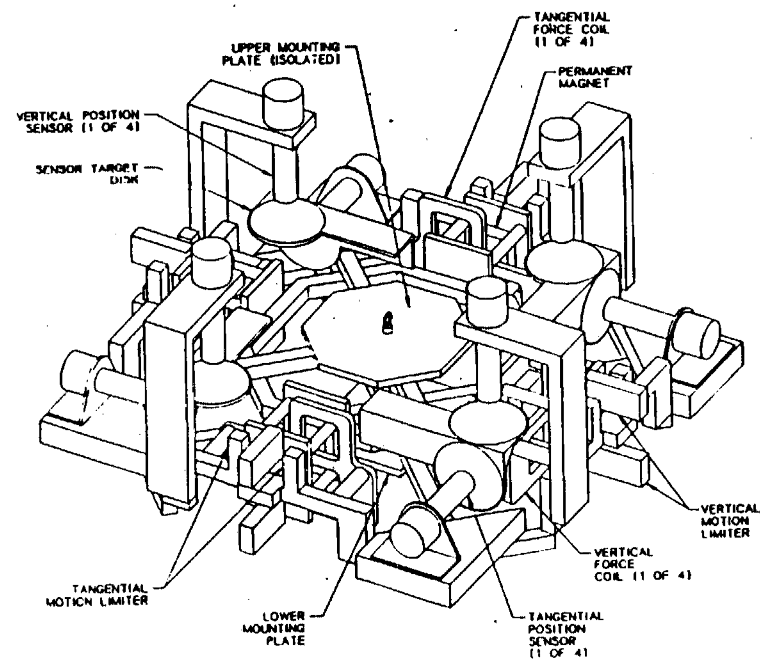
ISOLATOR FUNCTIONAL REQUIREMENTS

STROKE (X, Y, AND Z)	+/- 1 CM
MAXIMUM BASE-FOLLOWING FREQUENCY (MAXIMIZE "FREE FLYING")	4×10^{-2} Hz
MINIMUM ACTUATOR BANDWIDTH (COUNTERBALANCE DIRECT FORCES)	10^2 Hz
FORCE FOR 500 KG EXPERIMENT	1 N

SatCon Technology Corporation V023-7-91

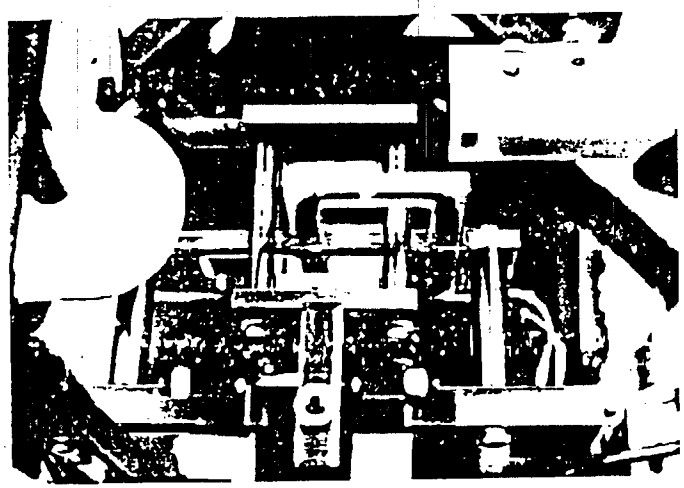


ONE OF THE SIX DOF ISOLATOR ACTUATORS

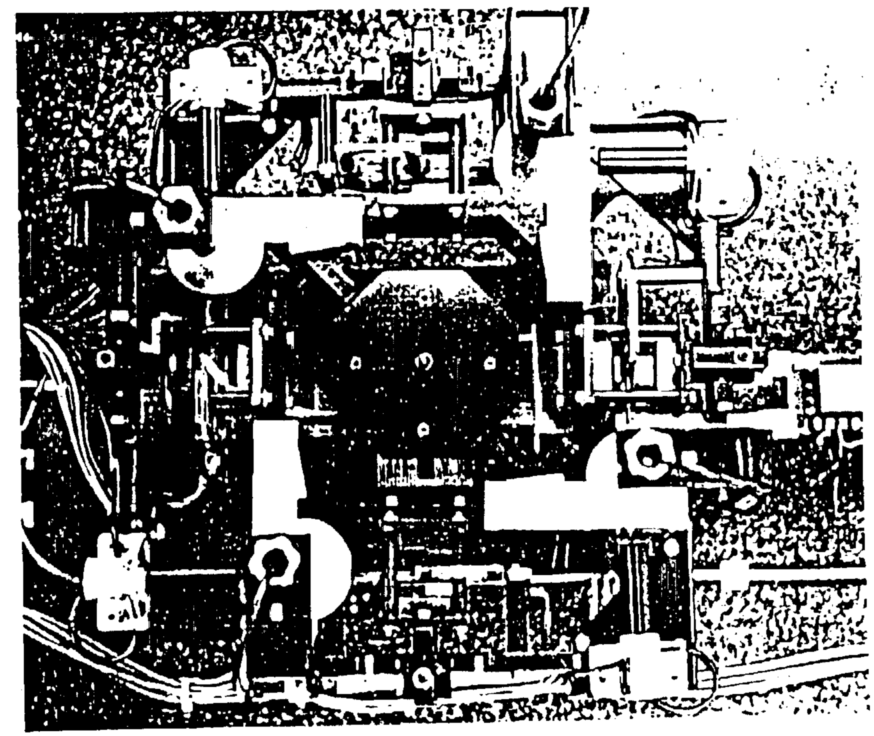


SATCON SIX-DOF MAGNETIC SUSPENSION

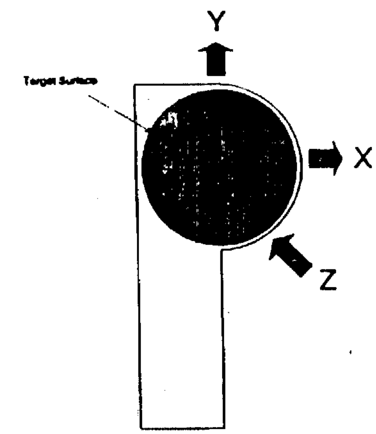
AN ACTUATOR INSTALLED ON THE SIX-DOF ISOLATOR



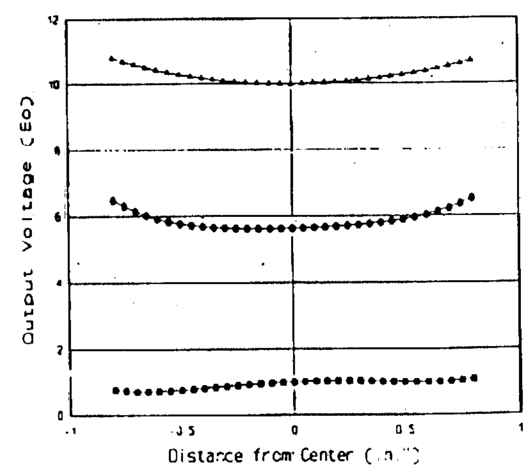
THE COMPLETED SIX-DOF ISOLATOR



INSTALLED VERTICAL AND TANGENTIAL POSITION SENSORS

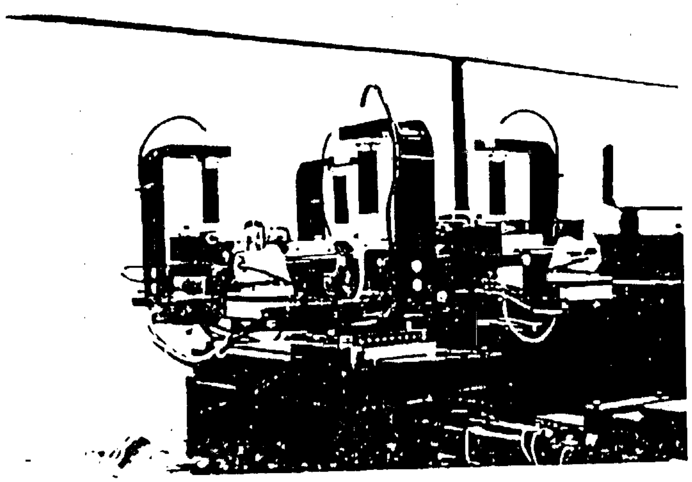


DETAIL OF SENSOR TARGET AXIS CONVENTION.

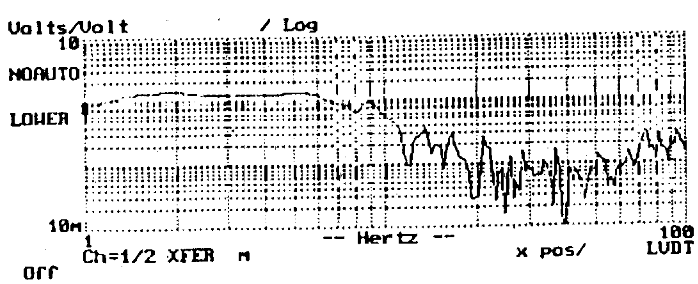


POSITION SENSOR RESPONSE FOR RADIAL AND NORMAL MOTIONS.

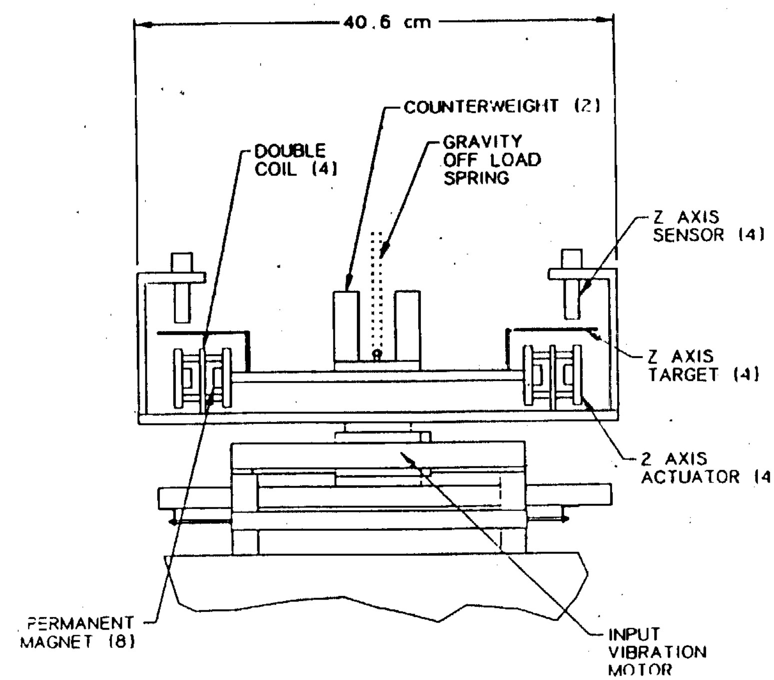
CONSTRUCTED SIX-DOF ISOLATOR ON THE TESTBED



TRANSFER FUNCTION OF BASE TO ISOLATOR MOTION



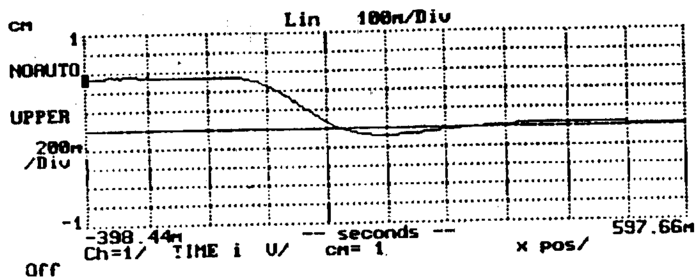
SatCon Technology Corporation V023-16-91



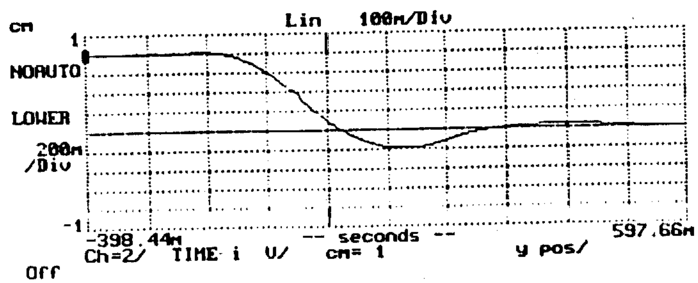
THE SIX DOF ISOLATOR PROTOTYPE MOUNTED ON THE TESTBED.

SatCon Technology Corporation V023-15-91

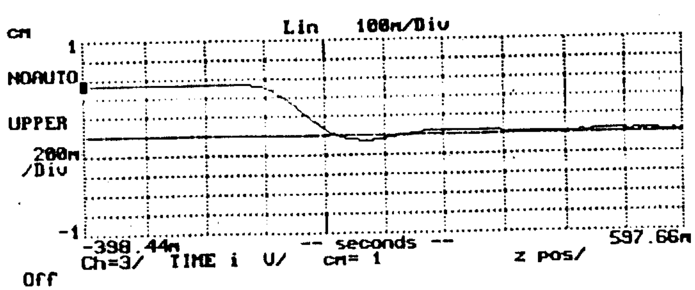
INITIAL CONDITION RESPONSES OF TRANSLATION AXES;
(a) x axis, (b) y axis, (c) z axis

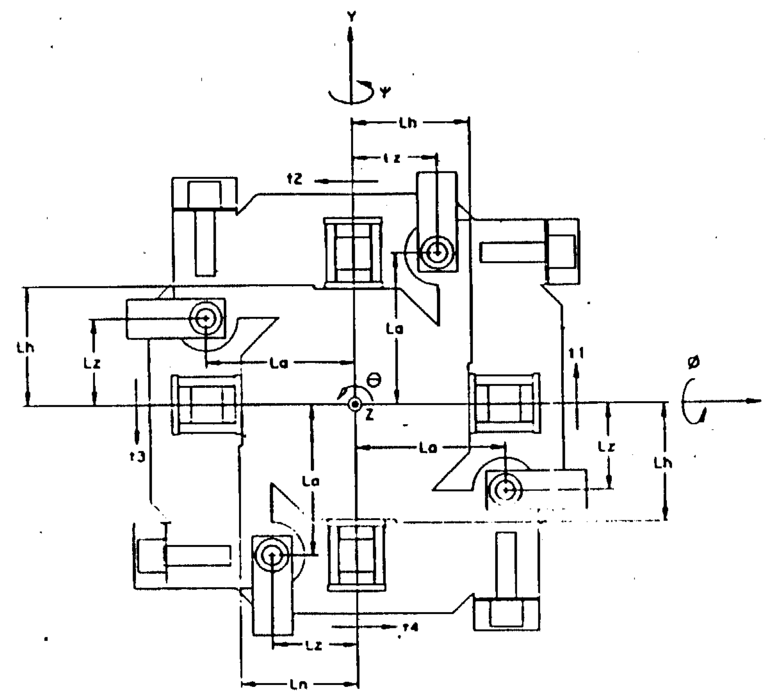


(a)



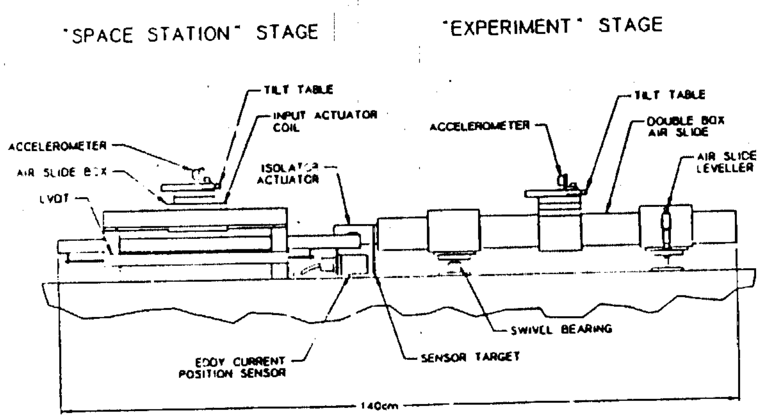
(b)





ISOLATOR TOP VIEW SHOWING COORDINATE CONVENTIONS.

ONE DEGREE OF FREEDOM TESTBED

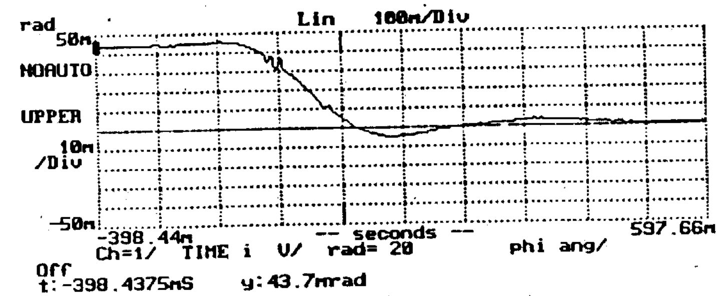


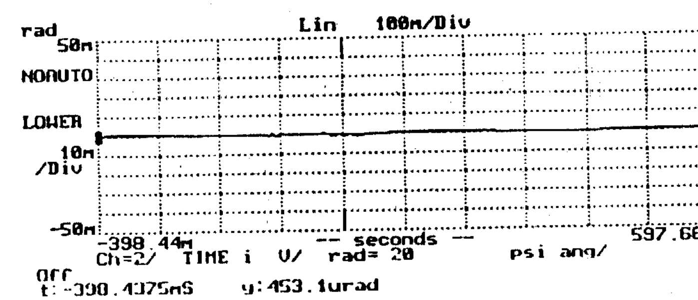
PRECISION LOW ACCELERATION TEST FACILITY

MICROGRAVITY LEVEL ACCELERATIONS

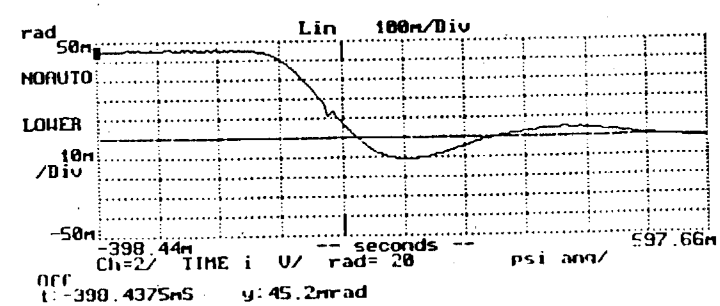
INITIAL CONDITION RESPONSE OF ROTATIONAL AXES;

(a) phi

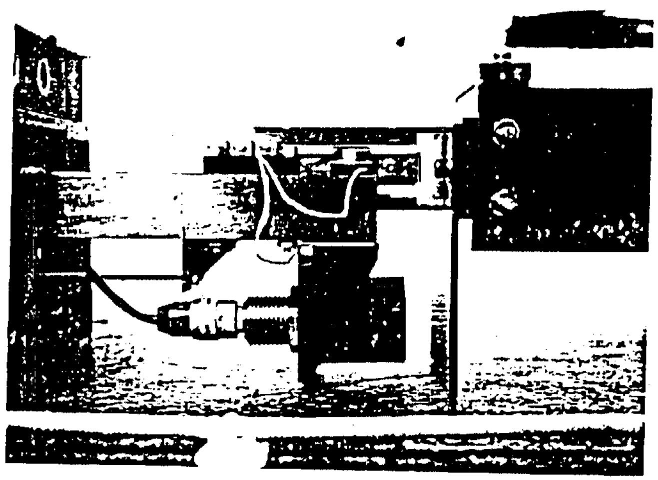




(b) psi



PHOTOGRAPH SHOWING ONE-DOF ISOLATION ACTUATOR AND EDDY CURRENT SENSOR



ONE DOF TESTBED

INPUT STAGE SIMULATES SPACE STATION VIBRATIONS.

EXPERIMENT STAGE IS ISOLATED FROM BY LINEAR ACTUATOR IN CENTER.

EACH STAGE HAS POSITION AND ACCELERATION FEEDBACK.

SIMULATE TYPICAL SPACE ACCELERATIONS AND SIGNAL LEVELS

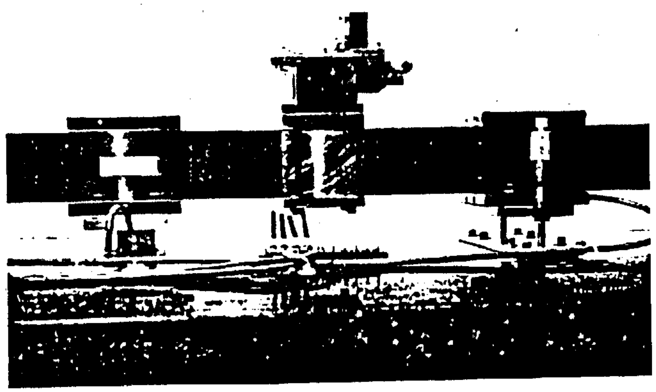
IMP. FOR NONLINEAR CONTROL TESTS, SENSOR AND ACTUATOR TESTS.

NONCONTACTING HARDWARE:

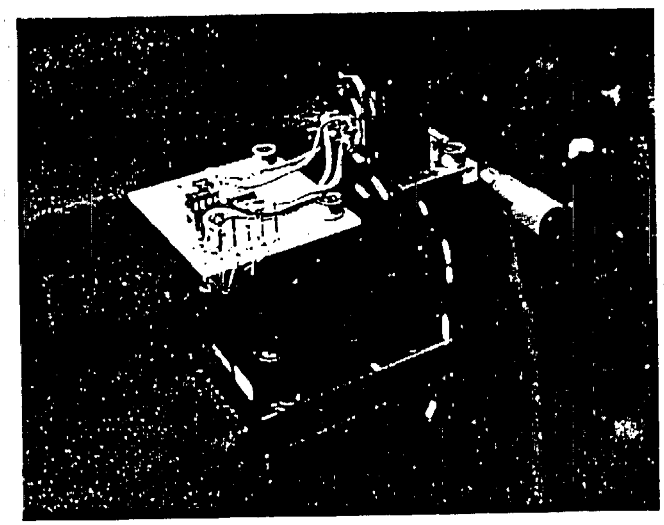
- AIR SLIDES - ACCURATE SPACE STATION ACCELERATION REPLICATION W/O STICTION AND SENSITIVE ISOLATION EVALUATION.
- NONCONTACTING POSITION SENSORS
- NONCONTACTING INPUT MOTOR AND ISOLATION ACTUATOR.
- AIR LINES ON NONMOVING PART
- ONLY ACCELEROMETER LEADS CONNECT TO EXPERIMENT.

VERY STIFF STRUCTURE

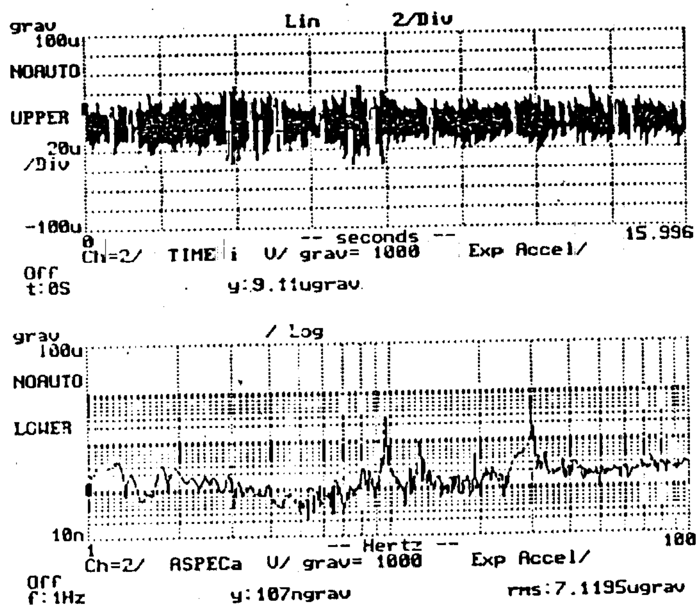
SIDE VIEW OF EXPERIMENT STAGE ASSEMBLY



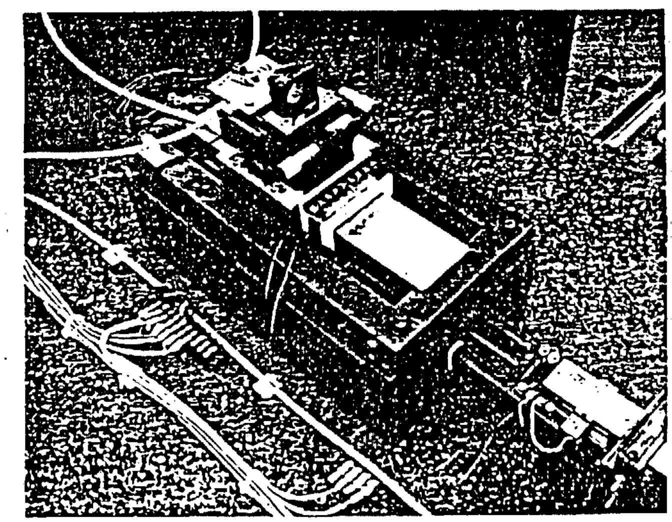
ACCELEROMETER AND PREAMPLIFIER MOUNTED ON AIR SLIDE



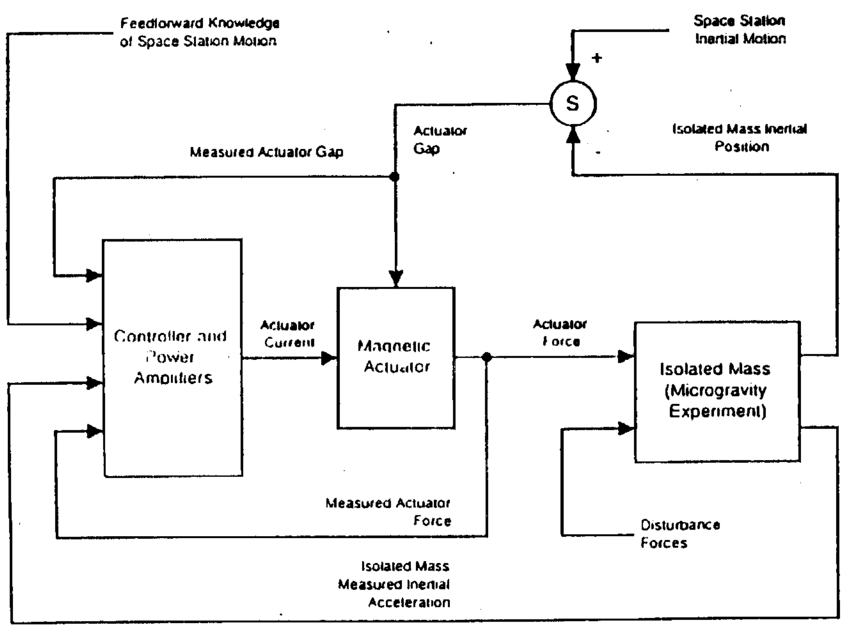
ACCELERATION WITH AIR SLIDE OPERATING



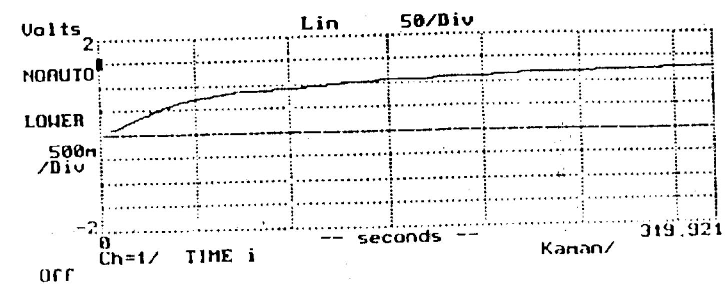
LORENTZ FORCE INPUT MOTOR AND INSTRUMENTATION



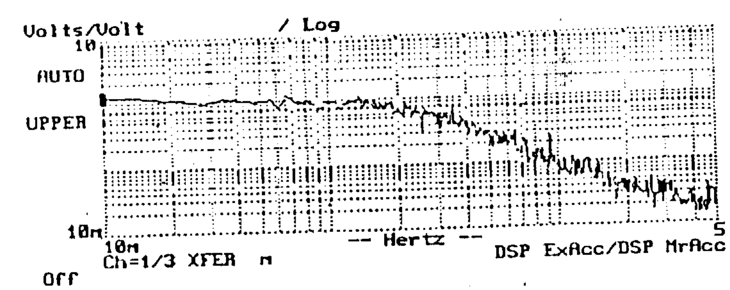
GENERAL ISOLATOR CONTROL SYSTEM



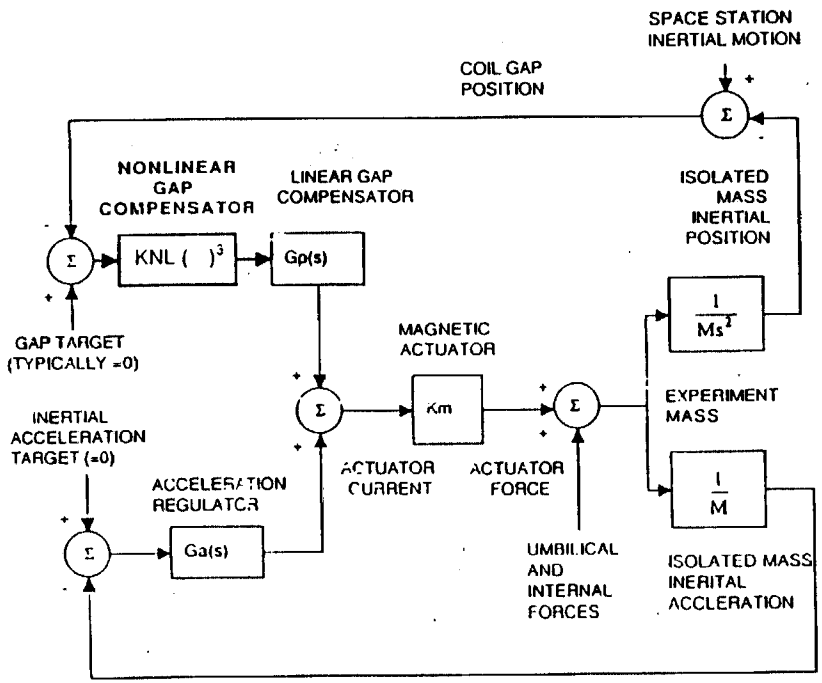
STEP RESPONSE OF THE 0.04 Hz BANDWIDTH POSITION CONTROL



TRANSFER FUNCTION OF 0.04 Hz BANDWIDTH POSITION CONTROL



CONTROL SYSTEM EXAMPLE WITH CUBIC NONLINEARITY



NONLINEAR CONTROLLER ADVANTAGES

ALL ACTUATORS HAVE SATURATION NONLINEARITY

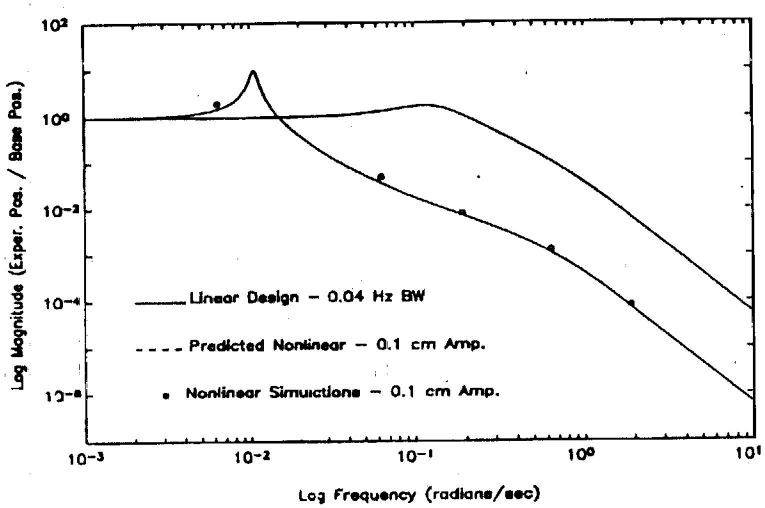
A LINEAR CONTROLLER APPLIES EXCESSIVE FORCES

A NONLINEAR CONTROLLER CAN FREE FLY AT ALL FREQUENCIES (AT SMALL AMPLITUDES)

THE SUSPENSION COULD BE STIFFENED BY GAIN SCHEDULING ANTICIPATING LARGE BASE MOTIONS

ADVANCED CONTROLLERS COULD MINIMIZE A COST FUNCTION OF STROKE AND FORCE USING A STOCHASTIC MODEL OF EXPERIMENT SENSITIVITY AND THE ENVIRONMENT

CONTROL SYSTEM EXAMPLE WITH NONLINEAR SIMULATION DATA



1) CUBIC NONLINEARITY IS AN ADJUSTABLE GAIN - DOWN BY A FACTOR OF 90

2) ISOLATION FREQUENCY REDUCED BY A FACTOR OF 17

3) NONLINEAR CONTROLLER PRODUCES LOWER EXPERIMENT ACCELERATIONS IF BASE RMS VIBRATION IS MUCH SMALLER THAN PEAK VALUES

A CUBIC GAP ERROR NONLINEAR CONTROLLER

GAP LOOP IS LOW PASS (0.04 HZ) BASE FOLLOWING.

ACCELERATION LOOP IS BANDPASS REGULATOR (TARGET = 0) REJECTS DIRECT FORCES.

THE CONTROLLER REDUCES FORCES FOR LOW AMPLITUDE MOTIONS.

THE NONLINEAR GAIN GIVES THE SAME LOOP GAIN AT MAXIMUM DISTURBANCE AMPLITUDE (AND THE SAME BANDWIDTH).

THE DESCRIBING FUNCTION ALLOWS LINEAR ANALYSIS WHEN THE INPUT FUNDAMENTAL PREDOMINATES.

THE DESCRIBING FUNCTION OF $(A*SIN(WT))^3$ IS 3/4 A^2.

THE GAP LOOP GAIN BECOMES PROPORTIONAL TO THE $ERROR^2$.

A STOCHASTIC DESCRIBING FUNCTION FOR $(RANDOM)^3$ IS $3*\sigma^2$. USE FOR GAIN SELECTION FOR STOCHASTIC NONLINEAR CONTROLLER.

SUMMARY

MICROGRAVITY ISOLATORS ARE REQUIRED.

0.04 HZ ISOLATION AND ±1 CM STROKE ARE REQUIRED.

BOTH BASE ISOLATION AND DIRECT FORCE REJECTION ARE DESIRABLE.

LORENTZ FORCE ACTUATORS ARE WELL SUITED FOR THIS APPLICATION.

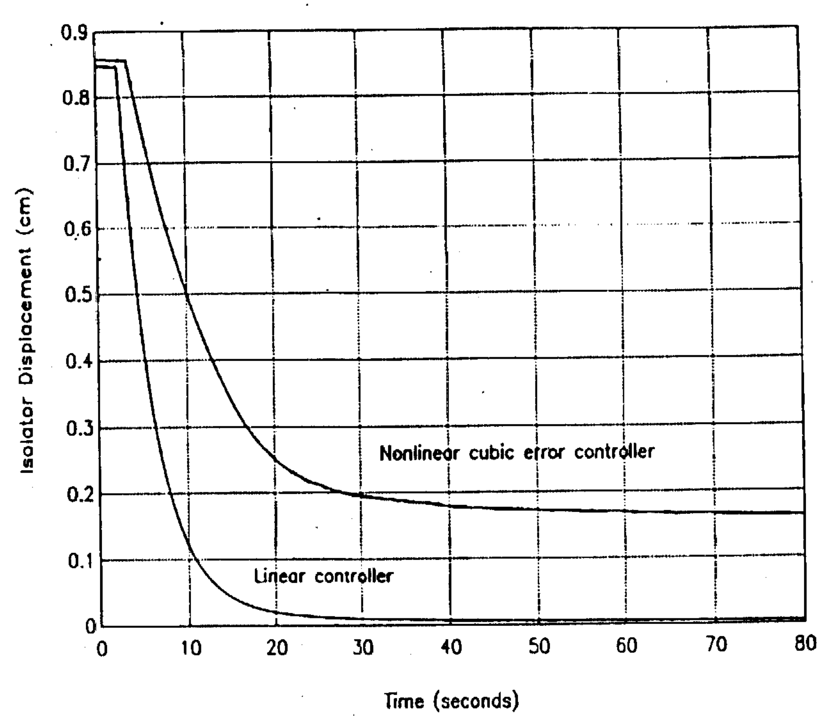
A NEW TWO DEGREE OF FREEDOM ACTUATOR WAS DESIGNED.

A SIX DEGREE OF FREEDOM SUSPENSION WAS DESIGNED, CONSTRUCTED AND TESTED
- PROTOTYPE SPACE BASED SUSPENSION

NONLINEAR POSITION CONTROLLERS CAN REDUCE EXPERIMENT ACCELERATIONS.

A ONE DOF MICROGRAVITY ACCELERATION TESTBED WAS BUILT
- INVESTIGATE ACCELERATION DISTURBANCES PRODUCED BY CONNECTIONS TO THE EXPERIMENT
  - POWER
  - SIGNAL
  - COOLING

CUBIC ERROR AND LINEAR INITIAL CONDITION RESPONSES



MEASURED DATA FROM SINGLE-DEGREE-OF-FREEDOM TESTBED

DIGITAL CONTROL ALGORITHMS FOR MICROGRAVITY ISOLATION SYSTEMS

Alok Sinha and Yung-Peng Wang
The Pennsylvania State University

N92-28445

ABSTRACT

New digital control algorithms have been developed to achieve the desired acceleration transmissibility function. The attractive electromagnets have been taken as actuators. The relative displacement and the acceleration of the mass have been used as feedback signals. Two approaches have been developed to find that controller transfer function in Z-domain, which yields the desired transmissibility at each frequency.

In the first approach, the controller transfer function is obtained by assuming that the desired transmissibility is known in Z-domain. Since the desired transmissibility $H_d(S) = 1/(\tau S+1)^2$ is given in S-domain, the first task is to obtain the desired transmissibility in Z-domain. There are three methods to perform this task: bilinear transformation, backward and forward rectangular rules. The bilinear transformation and backward rectangular rule lead to improper controller transfer functions, which are physically not realizable. The forward rectangular rule does lead to a physically realizable controller. However, this controller is found to be marginally stable because of a pole at Z=1. In order to eliminate this pole, a hybrid control structure is proposed. Here the control input is composed of two parts: analog and digital. The analog input simply represents the velocity (or the integral of acceleration) feedback; and the digital controller which uses only relative displacement signal, is then obtained to achieve the desired closed-loop transfer function. The stability analysis indicates that the controller transfer function is stable for typical values of sampling period.

In the second approach, the aforementioned hybrid control structure is again used. First, an analog controller transfer function corresponding to relative displacement feedback is obtained to achieve the transmissibility as $1/(\tau S+1)^2$. Then the transfer function for the digital control input is obtained by discretizing this analog controller transfer function via bilinear transformation. The stability of the resulting Z-domain closed loop system is analyzed. Also, the frequency response of the Z-domain closed-loop transfer function is determined to evaluate the performance of the control system in terms of transmissibility.

First, the performance of the digital control system is presented for a single degree of freedom system. It has been found that the both approaches of controller design lead to the desired transmissibility function. The digital phase lead/lag compensator leads to a transmissibility function which exceeds desired values at certain frequencies. Also, the maximum current required by the phase lead/lag compenstor is greater than currents required by new controllers.

Lastly, the controller design methodologies for a multi-degree of freedom system are presented. Numerical results are discussed in the context of a three-degree of freedom system for which parameters pertain to the experimental set-up at the NASA Lewis Research Center.

DIGITAL CONTROL ALGORITHMS FOR MICROGRAVITY ISOLATION SYSTEMS

by

ALOK SINHA
Department of Mechanical Engineering
The Pennsylvania State University
University Park, PA 16802

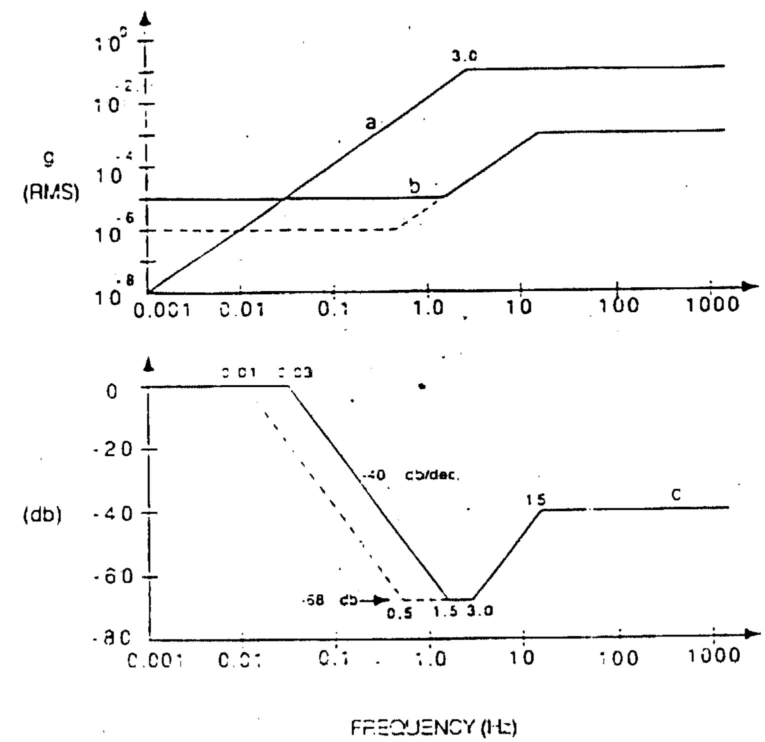


Figure 1

NEW APPROACH TO THE CONTROLLER DESIGN

It is a straightforward approach to guarantee that the transmissibility is below its upper bound at each frequency.

- Uses the relative displacement and the acceleration of the mass as feedback signals.
- The controller transfer function is determined to achieve the desired transmissibility.

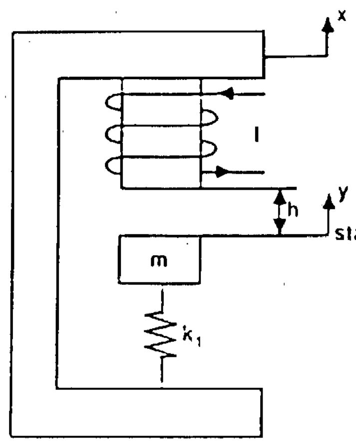
$$\text{Desired Transmissibility} = \frac{1}{(\tau S+1)^2}$$

where

$$\frac{1}{\tau}=2\pi\times 0.03\,\text{rad/sec}$$

$$\text{or}\left(2\pi\times 0.01\,\text{rad/sec}\right)$$

DIGITAL ALGORITHMS: SDOF CASE



Dynamics

$$m\ddot{y} + K_1(y-x) = v(t)$$

where

$$v(t) = f - mg$$

$$f = \mu_0 A N^2 \frac{I^2}{h^2} = G\frac{I^2}{h^2}$$

Generic Model for the Controller Design

$$m\ddot{y} + K(y-x) = u(t)$$

where

$$k = \begin{cases} K_1 \text{ without linearization} \\ K_1 - 2G\frac{I_S^2}{h_0^3} \text{ for linearization approach} \end{cases}$$

and

$$u(t) = \begin{cases} v(t) \text{ without linearization} \\ 2G\frac{I_S}{h_0^2} i \text{ with linearization} \end{cases}$$

$mg = G\frac{I_s^2}{h_0^2}$ where I_s: bias current

COIL CURRENT

a) LINEARIZATION

$$I = I_S + i$$

b) Without LINEARIZATION

$$I = G^{-1/2}\,(h_o - (y-x))\,(v+mg)^{1/2}$$

TWO METHODS FOR CONTROLLER DESIGN

- Method I
  H_d (Z) is known and determine the corresponding H_1 (Z)

- Method II
  Using H_d (S), first find H_1 (S). Then, obtain H_1 (Z) using bilinear transformation

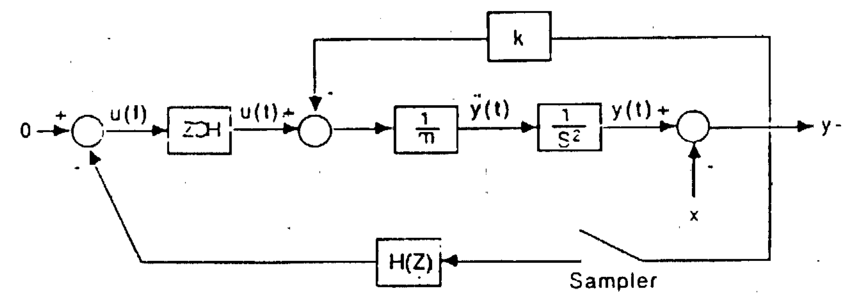


Figure 4

OPEN LOOP DYNAMICS IN Z-DOMAIN (C = 0)

$$Y(Z) = G(Z)\,U(Z) + K\,G(Z)\,X(Z)$$

where

$$G(Z)=\begin{cases} \dfrac{T^2(Z+1)}{2m(Z-1)^2} & \text{when } K=0 \\ \dfrac{1}{K}\,\dfrac{(Z+1)(1-\cos\omega_n T)}{(Z^2-2Z\cos\omega_n T+1)} & \text{when } k>0 \\ \dfrac{1}{K}\,\dfrac{(Z+1)(1-\cosh(w_o T))}{(Z^2-2Z\cosh(\omega_o T)+1)} & \text{when} K<0 \end{cases}$$

$$\omega_n=\sqrt{\frac{k}{m}} \text{ when } k>0$$

$$\omega_o=\sqrt{-\frac{k}{m}} \text{ when } k<0$$

CONTROL ALGORITHM

$$U(Z)=-H(Z)\left[Y(Z)-X(Z)\right]$$

The controller transfer function H(Z) is to be obtained such that

$$\frac{Y(Z)}{X(Z)}=H_d(Z)=\frac{N_d(Z)}{D_d(Z)}$$

where $H_d(Z)$ is the desired transmissibility function.

Denoting

$$G(Z)=\frac{N(Z)}{D(Z)}$$

$$H(Z)=\frac{N_d(Z)D(Z)-KN(Z)D_d(Z)}{N(Z)\left[D_d(Z)-N_d(Z)\right]}$$

Note that the zero of N(Z) = 0 is located at Z=-1.

Finding $H_d(Z)$ from $H_d(S)$

Given

$$H_d(S)=\frac{1}{(\tau S+1)^2}$$

- Bilinear Transformation

  $S=\frac{2}{T}\frac{Z-1}{Z+1}$ IMPROPER $H(Z)$

- Backward Rectangular Rule

  $S=\frac{1}{T}\frac{Z-1}{Z}$ IMPROPER $H(Z)$

- Forward Rectangular Rule

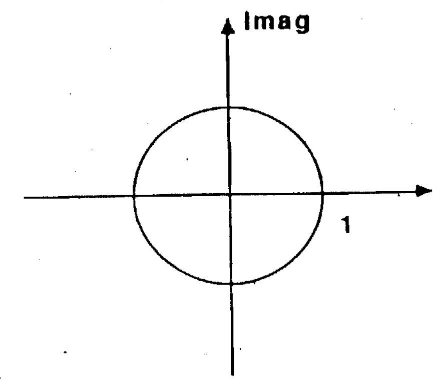
  $S=\frac{Z-1}{T}$ PROPER $H(Z)$

  $$H_d(Z)=\frac{\alpha^2}{[Z+(\alpha-1)]^2} \text{ where } \frac{T}{\tau}=\alpha$$

  However, $H(Z)$ has a pole at $Z=-1$

Basic Requirements for H(Z)

- H(Z) has to be stable; i.e., all poles of H(Z) should be located inside the unit circle of Z-plane.



- the order of the numerator of H(Z) ≤ the order of the denominator of H(Z).

  (PROPER H(Z))

CLOSED-LOOP DYNAMICS IN Z-DOMAIN

Let

$$G_1(Z)=\frac{N_c(Z)}{D_c(Z)} \text{ and } H_1(Z)=\frac{N_1(Z)}{D_1(Z)}$$

$$\left(D_c(Z)D_1(Z)+N_c(Z)N_1(Z)\right)Y(Z)$$
$$=\left(N_c(Z)N_1(Z)+KN_c(Z)D_1(Z)\right)X(Z)$$

Since

$$\frac{Y(Z)}{X(Z)}=H_d(Z)$$

there are COMMON FACTORS on the left and the right hand sides.

For closed-loop stability, all common factors must have roots inside the unit circle in Z-plane.

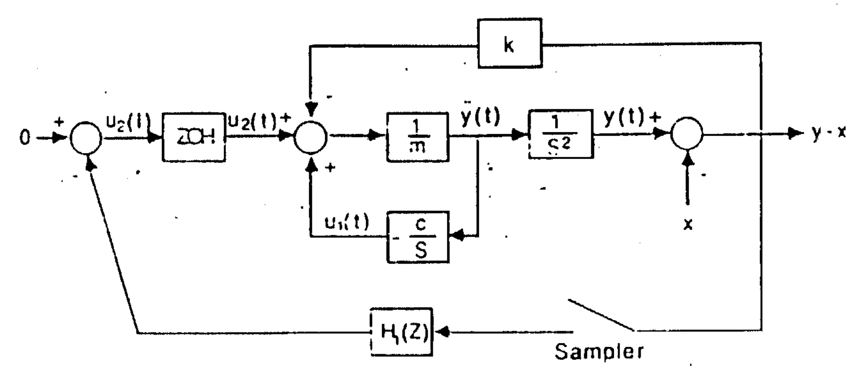


Figure 3

HYBRID CONTROL STRUCTURE

$$u(t) = u_1(t) + u_2(t)$$

↑ analogpart ↑ digital part

$$u_1(t) = -c\dot{y}$$

Therefore, the system dynamics is given by

$$m\ddot{y}+c\dot{y}+K(y-x)=u_2(t)$$

In Z-domain,

$$Y(Z)=G_1(Z)U_2(Z)+KG_1(Z)\times(Z)$$

CONTROL ALGORITHM

$$U_2(Z)=-H_1(Z)\left[Y(Z)-X(Z)\right]$$

SECOND APPROACH

- Obtain controller transfer function $H_1(S)$ to achieve $H_d(S)$
- Obtain $H_1(Z)$ using bilinear transformation

$$H_1(S)=\frac{\left(m-K\tau^2\right)\left(S+\frac{c-2\tau K}{m-K\tau^2}\right)}{\tau^2\left(S+\frac{2}{\tau}\right)}$$

$$H_1(Z)=\frac{\left(m-K\tau^2\right)\left[Z\left(2+\frac{c-2K\tau}{m-K\tau^2}T\right)-\left(2-\frac{c-2K\tau}{m-K\tau^2}\right)\right]}{2\tau^2\left[(1+\alpha)Z+(\alpha-1)\right]}$$

The resulting transfer function

$$H_a(Z)=\frac{Y(Z)}{X(Z)}=\frac{KG_1(Z)+G_1(Z)H_1(Z)}{1+G_1(Z)H_1(Z)}$$

The frequency response of $H_a(Z)$ is to be compared to that of $H_d(Z)$.

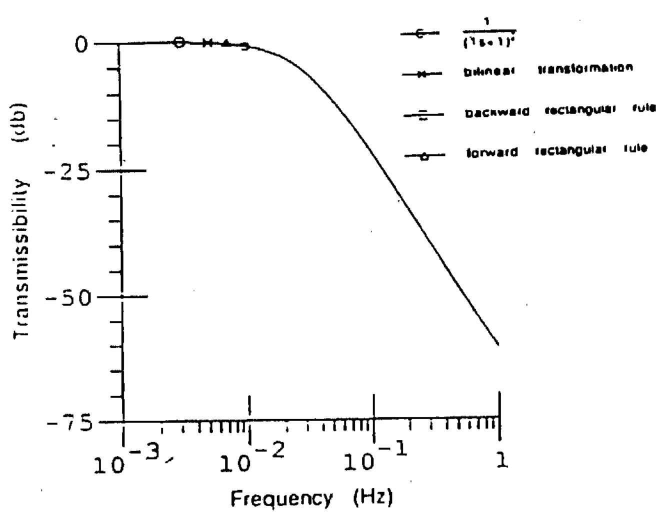


Figure 5

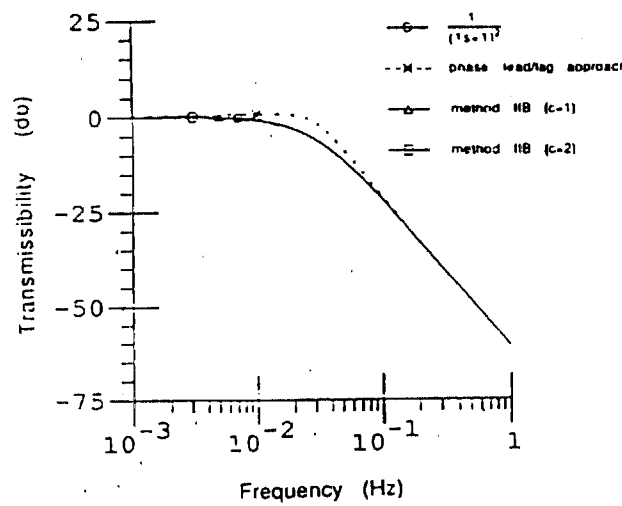


Figure 6

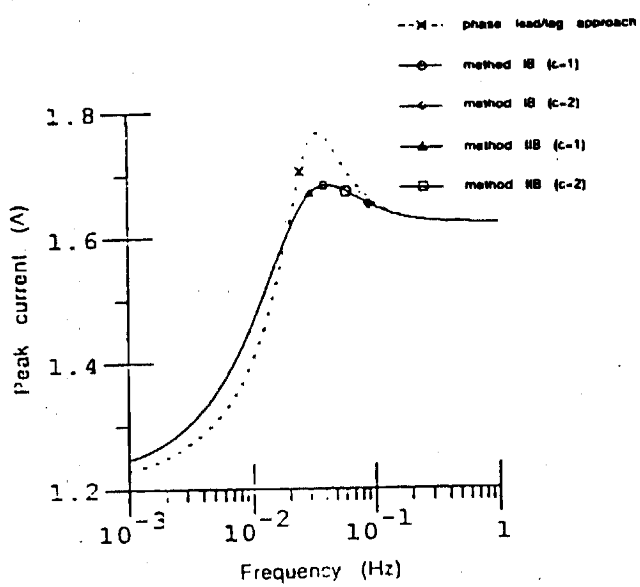


Figure 7

Controller Design for MDOF System: General Theory

Equations of Motion

$$M\ddot{\underline{q}}(t)+K_{eff}\,\underline{q}(t)=B_1\underline{f}(t)+D_1\underline{v}(t)$$

where

$\underline{q}\in R^n$, $\underline{f}\in R^n$ and $\underline{v}(t)\in R^n$

control law:

$$\underline{f}(t)=\underline{f}_a(t)+\underline{f}_d(t)$$
$$\underline{f}_a(t)=-C_1\dot{\underline{q}}(t)$$

Dynamical Equations for Digital Controller Design

$$M\ddot{\underline{q}}(t)+C_{eff}\,\dot{\underline{q}}(t)+K_{eff}\,\underline{q}(t)=B_1\underline{f}d(t)+D_1\underline{V}(t)$$

where

$$C_{eff}=\alpha M+\beta K_{eff}$$

OPEN LOOP DYNAMICS IN Z-DOMAIN

$$Q(Z)=G_1(Z)\underline{F}_d(Z)+G_2(Z)\underline{V}(Z)$$

where

$$G_1(Z)=PG_0(Z)\bar{B}$$

$$G_2(Z)=PG_0(Z)\bar{D}$$

where

$$\bar{B}=P^{-1}M^{-1}B_1$$

$$\bar{D}=P^{-1}M^{-1}D_1$$

$$G_0(Z)=\text{diag}\left(g_{01}(Z),g_{02}(Z),\ldots,g_{0n}(Z)\right)$$

where $g_{0i}(Z)$ are second-order transfer function in Z-Domain

CONTROLLER TRANSFER FUNCTION: METHOD I

$$F_d(Z)=-H(Z)\underline{R}(Z)$$

where

$$\underline{R}(Z)=L\underline{Q}(Z)-\underline{V}(Z)$$

H(Z) is determined to achieve the following desired transfer function matrix:

$$L\underline{Q}(Z)=G_d(Z)\underline{V}(Z)$$

Solution

$$H(Z)=G_1(Z)^{-1}\left[L^{-1}G_d(Z)-G_2(Z)\right]\left[I_n-G_d(Z)\right]^{-1}$$

where

$$G_d(Z)=g_d(Z)I_n$$

$$g_d(Z)=\frac{T^2}{\left(\tau Z+(T-\tau)\right)^2}$$

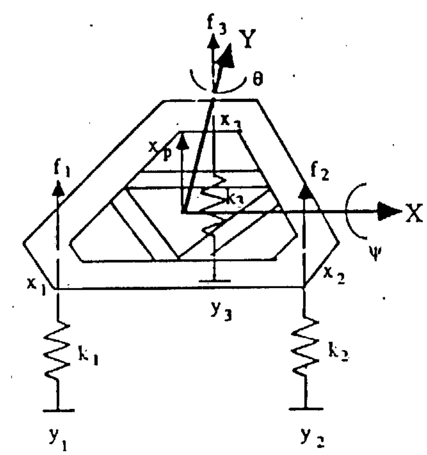
CONTROLLER TRANSFER FUNCTION: METHOD II

Define

$$F_d(S) = -H(S)\underline{Q}(S)$$

Determine $H_d(S)$ to achieve the following transfer function:

$$L\underline{Q}(S) = G_d(S)\underline{V}(S)$$



Solution:

$$H(S) = B_1^{-1}\left\{\left[MS^2 + \left(\alpha M + \beta K_{eff}\right)S + K_{eff}\right]L^{-1}G_d(S) - D_1\right\}$$

$$\left[I_n - G_d(S)\right]^{-1}$$

where

$$G_d(S) = \frac{1}{(\tau S + 1)^2} I_n$$

$$H(Z) = H(S)\Big|_{S = \frac{2}{T}\frac{Z-1}{Z+1}}$$

Controller Transfer Function: Method I

$$h_{ij}(Z)=\left\{\sum_{n=1}^{3} r_{in}\bar{r}_{nj}\, g_{on}^{-1}(Z)\, g_d(Z)-K_i\delta_{ij}\right\}\frac{1}{\left(1-g_d(Z)\right)}$$

where

$$(\bar{R})_{ij}=\bar{r}_{ij}=\left(P^{-1}L^{-1}\right)_{ij}$$

$$(R)_{ij}=r_{ij}=\left[\left(L^T\right)^{-1}MP\right]_{ij}$$

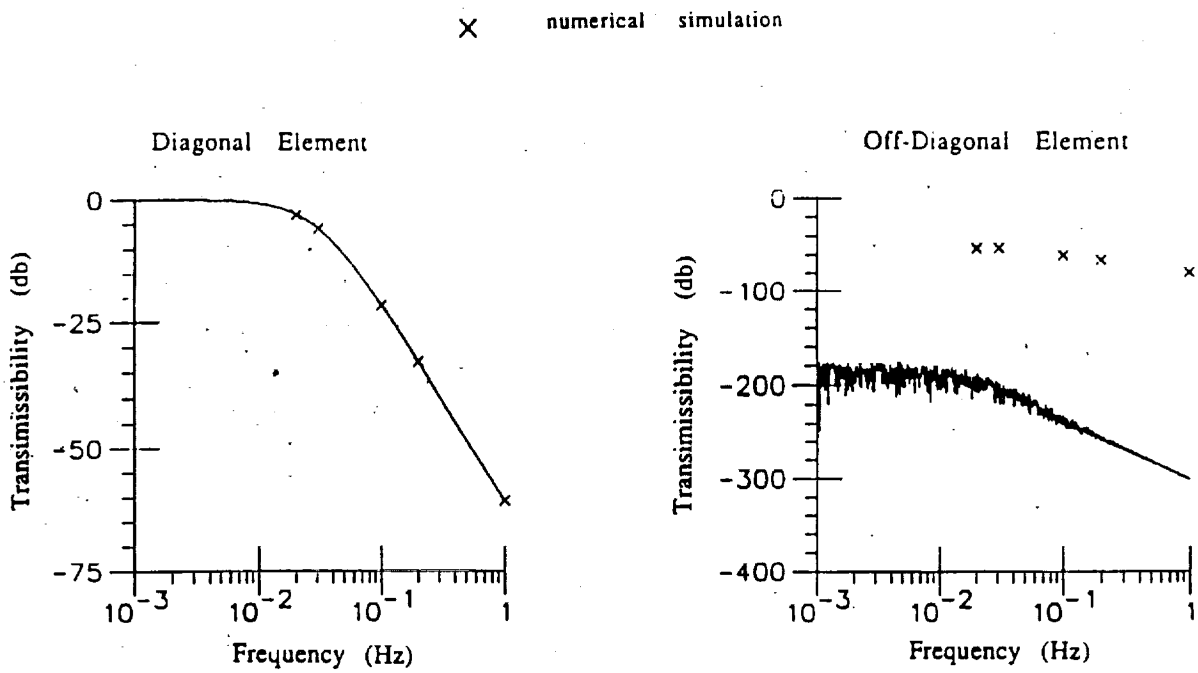
$$g_d(Z)=\frac{T^2}{\left(\tau Z+(T-\tau)\right)^2}$$

Controller Transfer Function: Method II

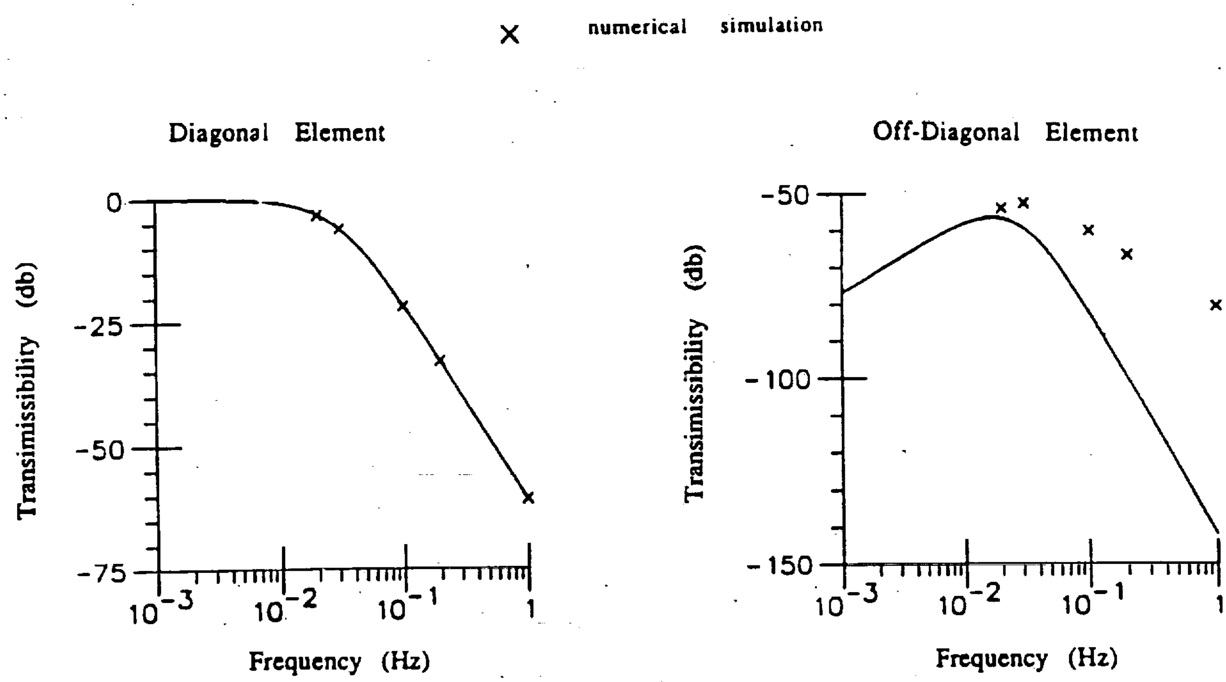
$$h_{ij}(Z)=\frac{\left\{\left[2\left(n_{1ij}+\tau^2 n_{3ij}\right)+T\left(n_{2ij}+2\tau n_{3ij}\right)\right]Z+\left[-2\left(n_{1ij}+\tau^2 n_{3ij}\right)+T\left(n_{2ij}+2\tau n_{3ij}\right)\right]\right\}}{\left\{\left(2\tau^2+2\tau T\right)Z+\left(-2\tau^2+2\tau T\right)\right\}}$$

where

n_{1ij}, n_{2ij} and n_{3ij} dependonsystemparameters.



Approach 1



Approach 2

CONCLUSIONS

- A new approach has been developed for the design of a digital control system for the microgravity isolation system.

- A general theory for a MDOF system has been developed.

- The performance of the control system designed on the basis of this new approach is superior to that of the phase lead/lag compensator.

N92-28446

MICROGRAVITY VIBRATION ISOLATION RESEARCH AT THE UNIVERSITY OF VIRGINIA

Carl R. Knospe Paul E. Allaire
David W. Lewis Robert R. Humphris
A. Peter Allan Bibhuti B. Banerjee
R. David Hampton
Center for Magnetic Bearings, University of Virginia

ABSTRACT

Research at the University of Virginia on microgravity vibration isolation is reviewed. This work falls into three areas: (1) the one degree of freedom isolation test rig and Lorentz actuator design, (2) multiple degree of freedom active isolation system control, and (3) innovative actuators for long stroke, non–contacting six degree of freedom isolation. Theoretical and design issues of multiple degree of freedom active isolation are discussed.

A Six Degree-of-Freedom Actuator Design for Microgravity Vibration Isolation

A. P. Allan and C. R. Knospe
Department of Mechanical and Aerospace Engineering
University of Virginia

April 5, 1991

1 Introduction

It is generally accepted that microgravity space experiments will need to be isolated from the vibrations inherent on spacecraft in earth orbit[3]. The fundamental constraint on any isolation system's capability is the available working envelope[4]. Figure 1 shows the relationship between the envelope (peak-to-peak displacement) and frequency for several sustainable RMS acceleration levels. The graph is for a one degree-of-freedom case and assumes sinusoidal vibrations, but the relationships are acceptable for order of magnitude estimates even if these assumptions are relaxed.

No definitive specification of the required isolation levels or frequency range exists. The proposed US Space Station usable specification[3] is also shown in Figure 1. It is claimed that vibrations below this curve will not adversely affect microgravity experiments. We have pursued the design of an active isolation system with a 'reasonable' envelope of 4 inches of travel, and a sustained 1 μg RMS acceleration. It can be seen from the figure that this will offer isolation down to 0.002 Hz. The amplitude to which vibrations can be attenuated is constrained only by controller design and available instrumentation. Operation at lower frequencies, however requires a larger envelope, which becomes prohibitive in terms of available spacecraft space. We have also required that the system be active in all six degrees-of-freedom, with a rotational range of 40 degrees.

Redundant coarse-fine schemes with magnetic levitation for vibration isolation are discussed in the robotics literature[2]. This approach is particularly attractive in the microgravity application since it allows the use of magnetic levitation while overcoming range of motion limitations. We have chosen the Stewart platform for our coarse stage and a novel magnetic bearing for the fine stage. The approximate regions of activity in the frequency-displacement plane of these two devices are shown in the figure. Both stages act to attenuate spacecraft vibrations, effectively reducing vibration amplitudes below their

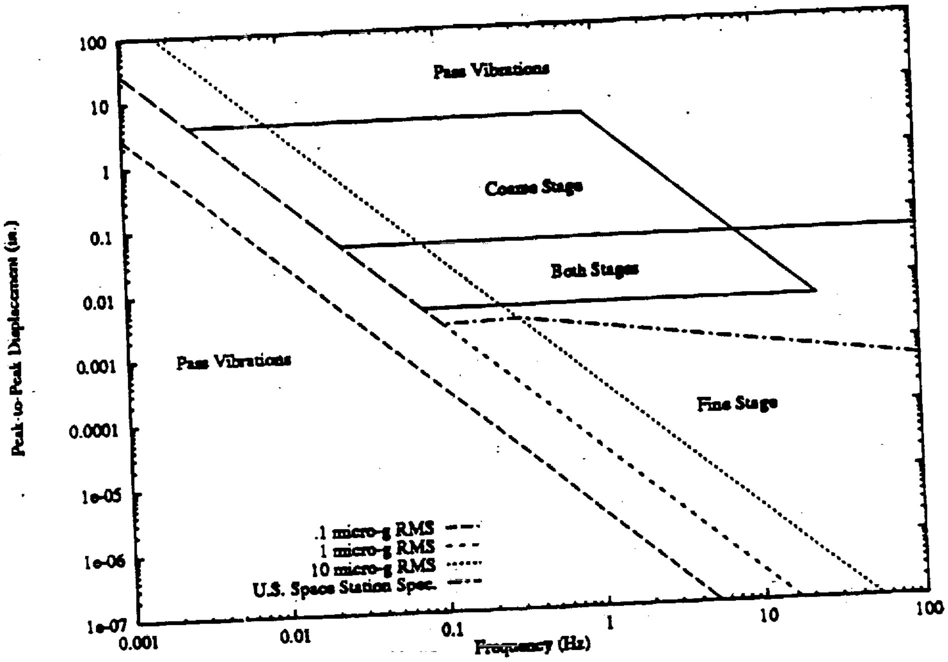


Figure 1: Peak-to-Peak Displacement vs. Frequency for Various RMS Accelerations, U. S. Space Station Usable Specification, and Activity Regions of the Two Actuator Stages

active regions on the plane. As an example, it can be seen in the figure that a vibration of the spacecraft with 10 inches of displacement at a frequency of 1 Hz falls outside the active region and could only be partially attenuated. It should be noted that such a large vibration is unlikely. If the displacement was only 1 inch, however, the coarse stage would absorb all of it except about 0.005 inches, and the remainder would be reduced down to the micro-g level by the fine stage.

The combination of the Stewart platform and a magnetic bearing allows continuous isolation at frequencies above 0.002 Hz, and a compact, reliable package suitable for the application. These choices and some preliminary design concepts will be discussed in detail.

2 Stewart Platform

The Stewart platform is a six degree-of-freedom parallel manipulator first proposed by Stewart[5]. It has been extensively used in aircraft cockpit simulator applications, and substantial design information is available in the literature[1]. Figure 2 shows the mechanism in our proposed configuration. Six linear actuators (legs) connect a base (bottom) to a platform (top). The base will be mounted in the spacecraft and move with it, while the platform tracks an inertial reference frame. We propose the use of stepper motor driven ball lead-screws as actuators.

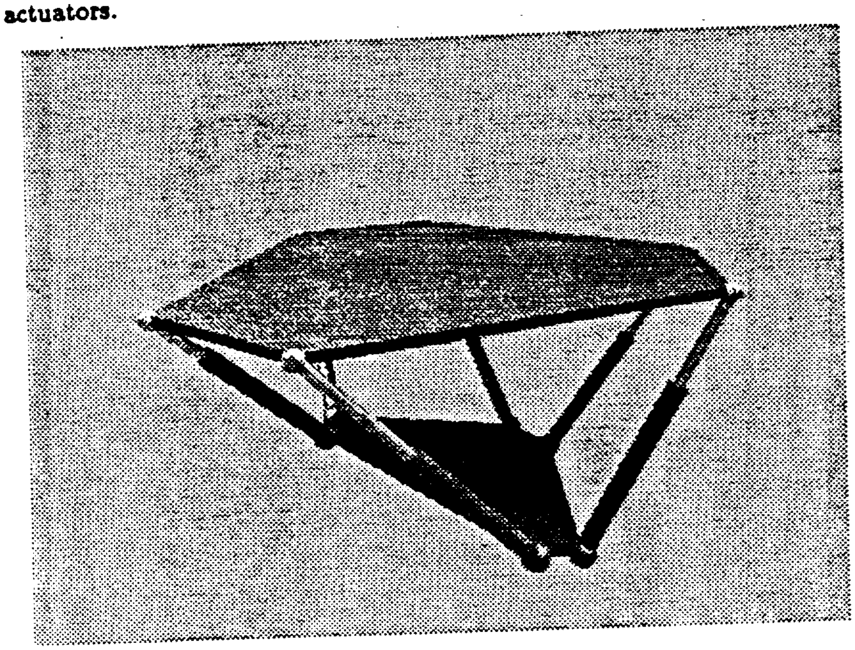


Figure 2: The Stewart Platform

This mechanism was chosen over other candidates such as a carriage/gimbal approach, or a serial linkage mechanism because it has the following features:

- *Inherent rigidity.* The parallel connection of the actuators gives the mechanism rigidity on the order of the extensional rigidity of the actuators. For the proposed actuators, this will allow controller design to ignore the dynamics of the mechanism. The effects of 'umbilical' connection to the platform will also be negligible.
- *Determinate inverse kinematics.* The actuator lengths required to achieve a prescribed orientation are found directly from a coordinate transformation from the base to the platform frame. This is seldom the case for a serial linkage. This will also simplify control.
- *Compactness.* The configuration proposed here places the fine stage on top of the platform for convenience in testing. A fully developed imple-

mentation could locate the fine system and microgravity experiment in the space between the base and platform, resulting in a compact package.

The Stewart platform has some disadvantages that must be considered. It is nonlinear in its response to actuator lengths, its general direct kinematics have not been discovered in closed form, and it has singularities in its operational space. The fist two problems can be overcome with digital controls. The singularities, which are points or loci where the mechanism gains a degree of freedom and the actuators can lose control of it, must be addressed in design.

A simulation code has been written to allow exploration the design alternatives. Figure 2 is an example of its output. Preliminary results indicate that our specification (4 inches translation, 40 degrees rotation) will be achievable with actuators 10.5 inches long in the retracted position, and with 9 inches of stroke. The simulation can confirm that singularities are safely outside the working envelope. Commercial actuators with the required range, load capacity, speed and acceleration have been identified.

3 Magnetic Bearing

Two axes of a six axis magnetic bearing are shown in Figure 3, mounted atop the Stewart platform. A ferromagnetic cube is at the center of the bearing. Two pole pieces protrude from each of its faces (four shown) and each pole piece is surrounded by a coil. This part of the structure is called the core and is mounted to the platform with four posts. Three ferromagnetic bands surround the core (one shown) forming three independent magnetic flux paths. The core is capable of exerting three orthogonal forces, and three orthogonal torques on the bands. For the axes shown, equal currents in each pair of adjacent coils will cause magnetic flux to flow in a local circuit, causing an attractive force to the band. By controlling these currents a prescribed force can be exerted on the band along the axis that crosses the page form left to right. If the currents in adjacent coils are not equal, some flux will flow around the outside of the band and through the center of the cube. This will create a controllable torque on the band around the vertical axis.

Similar pole pieces and coils will protrude from the other faces of the cube, and corresponding bands will surround them. These have been omitted so that all parts can be seen. Also, the size of the bearing and the gaps have been exaggerated for clarity. Flux sensors will be mounted in the pole pieces and this will allow the position of the bands relative to the core to be calculated for control. The microgravity experiment will occupy the space surrounding the bearing, and be attached to the bands.

This configuration was chosen over other levitation approaches such as Lorentz actuators or magnetic actuators located on the periphery of the experiment package because it has the following advantages:

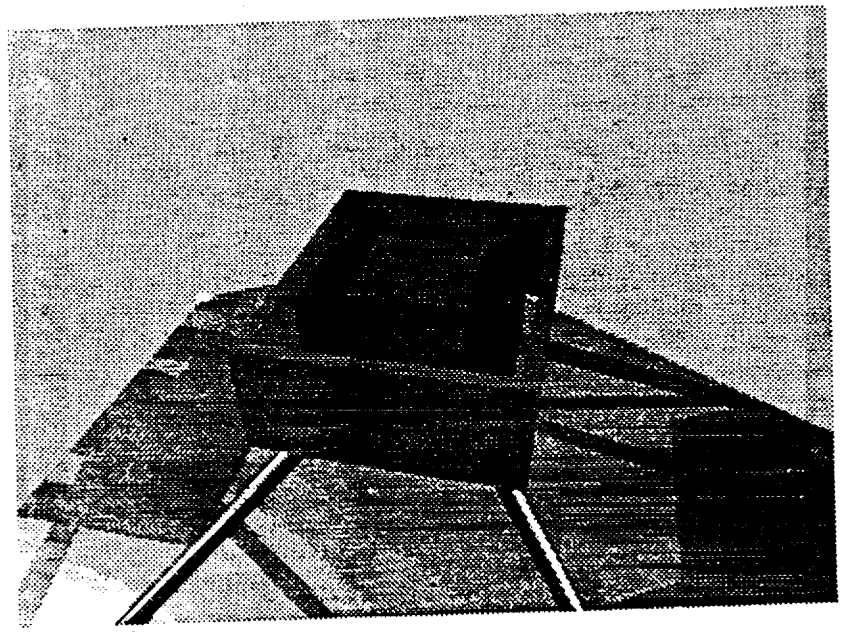


Figure 3: Magnetic Bearing

- *Compactness*. The high force capability of the magnetic bearing relative to a Lorentz actuator of similar size and power consumption suits the application. Testing in earth gravity will be facilitated, and levitation during launch to protect sensitive instrumentation may be feasible. Also, the rigid structure required to mount actuators around the periphery is avoided.
- *Force/torque balance and rotational range*. Actuators capable of the required forces mounted on the periphery of the experiment are capable of torques far greater than is required, and they limit the rotational range of the experiment. The proposed design approach brings the relative force/torque magnitudes closer to the requirement, and allows substantial rotational range.
- *Integral sensor capability*. Compact semiconductor magnetic flux sensors (hall effect or magneto-resistive) can be utilized to both stabilize the system and infer relative position. No elegant integrated approach is known for Lorentz actuators.

Magnetic bearings have typically been avoided in 'large gap' applications because of their nonlinearity (force is proportional to the square of flux). We feel that emerging Digital Signal Processor technology and control work will allow us to overcome these limitations. Finite element tools will be employed to develop a design that is both capable of high forces and torques, and avoids nonlinearities associated with saturation and flux path variations.

4 Conclusion

A conceptual design is proposed for a coarse-fine actuator pair that synergistically combines two dissimilar six degree-of-freedom actuators. This design is particularly suited to the microgravity isolation application because of the way it spans the useful portion of the displacement-acceleration plane. The combination is shown together in Figure 4.

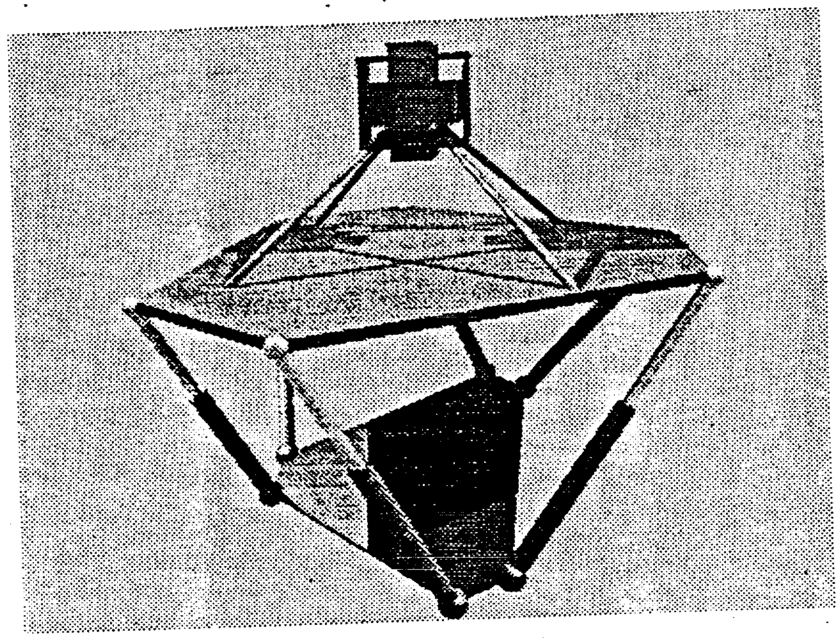


Figure 4: Coarse-Fine Actuator

Ongoing work will more precisely define the exact geometries, materials, and components to be used. Simulation will allow the specification of a Stewart platform that meets the specification, and uses commercially available components. Finite element methods will be used to optimize the magnetic bearing design. A simultaneous effort in controller design will be undertaken. A test rig will then be constructed to verify the design and quantify the performance of the actuators and controller together.

We look forward to and welcome any input that can be worked into our design effort.

5 Acknowledgements

This work was supported in part by the NASA Lewis Research Center and the Commonwealth of Virginia's Center for Innovative Technology.

References

[1] E. F. Fichter. A Stewart platform based manipulator: General theory and practical construction. *International Journal of Robotics Research*, 5(2):157–182, 1986.

[2] R. L. Hollis, A. P. Allan, and S. Salcudean. A six degree-of-freedom magnetically levitated variable compliance fine motion wrist. *Robotics Research*, 4:65–73, 1987.

[3] D. I. Jones, A. R. Owens, and R. G. Owen. A microgravity isolation mount. *Acta Astronautica*, 15(6/7):441–448, 1987.

[4] C. R. Knospe and P. E. Allaire. Limits on the isolation of stochastic vibration for microgravity space experiments. To be published in the *Journal of Spacecraft and Rockets*.

[5] D. Stewart. A platform with six degrees of freedom. *Proceedings of the Institute of Mechanical Engineers*, 180(1):371–386, 1965.

N92-28447

EXTENDED H_2 SYNTHESIS FOR MICROGRAVITY VIBRATION ISOLATION

R. David Hampton
Carl R. Knospe

Center for Magnetic Bearings
University of Virginia

April 8, 1991

Introduction

The vibration environment onboard current and planned manned orbiters requires isolation for microgravity science experiments. The disturbance frequencies are sufficiently low and the attenuation requirements sufficiently great so as to preclude a passive isolation system [1]. This paper describes a design procedure, known as extended H_2 synthesis, for active isolation system controllers currently being developed at the University of Virginia.

MDOF Isolation

To isolate an experiment platform from the orbiter vibration environment requires a large–stroke actuator capable of acting over six degrees of freedom as well as having great precision and large bandwidth. These conflicting requirements necessitate the use of a coarse/fine actuator system. The fine isolation system, as described in [2], will require a higher bandwidth controller than the coarse isolation system, but will be substantially more linear. The design of controllers for a multiple–degree–of–freedom (MDOF) fine isolation system is the topic of this paper.

MDOF controller design is much more difficult than single–degree–of–freedom (SDOF) design because the resulting system has many inputs (actuator forces) and outputs (measured displacements and accelerations). Multiple–input–multiple–output (MIMO) designs can be very susceptible to unmodeled cross–coupling between channels of input or output [3], a problem not encountered in SDOF design. The control forces used must therefore be properly coordinated. If a controller's performance is not very sensitive to unmodeled dynamics the controller is said to be *robust*. The design of a robust MIMO control system requires the iterative use of synthesis and analysis tools. The synthesis tools are needed to design the controller and the analysis tools are required for evaluation of system performance and stability.

Optimal Control

A particular vibration isolation problem may involve different kinds of undesirable outputs, such as excessive absolute accelerations and unacceptable relative displacements. Some of these undesired outputs may be more important than others, and the degree of undesirability may be greater in certain directions or in a certain frequency range. For example, rattlespace constraints may be more restrictive in one direction than in others. Or a crystal–growth experiment may be particularly sensitive to accelerations at certain frequencies or in certain directions. One of the goals, then, should be to design a controller that is capable of minimizing the plant outputs as dictated by these considerations.

However. control energy consumed in achieving acceptable outputs has power and thermal costs, both of which are of concern in a space environment. Consequently, the control effort used should not be excessive. Since the controller bandwidth must be limited in order to increase robustness, the control effort should be minimized at higher frequencies.

H_2 Synthesis

Figure 1 shows a block diagram of the H_2 synthesis problem. The dynamics of the isolation platform, its actuators, and sensors are described by the block transfer function G(s). Inputs to this system are disturbance forces (from umbilicals) D(s) and actuator control currents U(s). The outputs of G(s) are measured outputs (accelerations and relative positions) Z(s) and performance outputs (positions and velocities) Y(s) which may not be measured. The goal of H_2 synthesis is:

> Given a mathematical description of the system's dynamics G(s), find a feedback block transfer function controller C(s) which minimizes the performance index
>
> $$J = \int_{-\infty}^{\infty} \| Y(j\omega) \|_2 \, d\omega + \int_{-\infty}^{\infty} \| U(j\omega) \|_2 \, d\omega$$

Here the first term is the total "power" in the performance output signals while the second term is the total "power" in the control input signals [4].

Extended H_2 Synthesis

In the first extension of the H_2 design procedure, the control input U(s) and the performance output Y(s) are re–defined as in Figure 2 with the introduction of matrix transfer functions W(s) and V(s). This allows one to weight some performance outputs and control inputs more highly than others, with this weighting being frequency–dependent [5]. Note from the diagram that this merely requires defining $\tilde{G}(s)$ and $\tilde{C}(s)$ to include these transfer functions. The same procedure as employed in H_2 synthesis can then be used to solve for $\tilde{C}(s)$ and, from this, C(s). This extension allows the standard mathematical machinery of H_2 synthesis to include accelerations in the performance index to be minimized. It also permits the isolation system to pass low frequency vibrations for which insufficient rattlespace exists.

The second extension of H_2 synthesis allows the anticipated frequency content of the disturbances to be taken into account during the design procedure. This requires the introduction of a shaping filter transfer function matrix S(s) into the dynamical description of the system, as shown in Figure 3. With this addition, the standard mathematical machinery of H_2 synthesis can once again be employed [6]. A recent extension allows the incorporation of sensed disturbances (preview control) into the H_2 design procedure [7,8].

Design Procedure

The control determined by these synthesis procedures is only optimal with respect to the chosen performance index. Since the performance outputs and control inputs to be used in the procedure are selected by the designer, the resultant controller C(s), its performance, and its robustness are direct products of these choices. Thus, the synthesis procedure is a tool available for controller design, but its machinery cannot replace the knowledge and insight of the designer. Several researchers have explored methods to incorporate various design goals into this framework. Straightforward analysis techniques employing matrix singular values have been used successfully in this aspect of H_2 design [9].

Conclusion

H_2 synthesis techniques are well understood and readily applicable to the MIMO disturbance rejection problem. Extensions exist in the literature, and research continues at the University of Virginia in this area. Extended H_2 synthesis techniques are being adapted and applied to the special demands of the microgravity vibration isolation problem.

Acknowledgments

This work was supported in part by the NASA Lewis Research Center and by the Center for Innovative Technology of the Commonwealth of Virginia.

References

[1] C. M. Grodsinsky and G. V. Brown, "Non–intrusive Inertial Vibration Isolation Technology for Microgravity Space Experiments," NASA TM–201386, January 1990.

[2] A. P. Allan and C. R. Knospe, "A Six Degree–of–Freedom Actuator Design for Microgravity Vibration Isolation," International Workshop on Vibration Isolation Technology for Microgravity Science Applications, Cleveland, OH, April 23–25, 1991.

[3] C. R. Knospe, R. D. Hampton, and P. E. Allaire, "Control Issues of Microgravity Vibration Isolation," Acta Astronautica, in review.

[4] S. J. Citron, Elements of Optimal Control, Holt, Rinehart and Winston, Inc., New York, 1969, pp. 235–242.

[5] N. K. Gupta, "Frequency Shaped Cost Functionals: Extension of Linear–Quadratic–Gaussian Design Methods," AIAA Journal of Guidance and Control, November/December 1980, pp. 529–535.

[6] C. D. Johnson, "Further Study of the Linear Regulator with Disturbances — The Case of Vector Disturbances Satisfying a Linear Differential Equation," IEEE Trans. on Automatic Control (Short Papers), Vol. AC–15, April 1970, pp. 222–228.

[7] M. E. Salukvadze, "Analytic Design of Regulators (Constant Disturbances)," translated in Automation and Remote Control, Vol. 22, No. 10, October 1961, pp. 1147–1155. Originally published in Avtomatika i Telemakhanika, Vol. 22, No. 10, February 1961, pp. 1279–1287.

[8] R. D. Hampton, C. R. Knospe, P. E. Allaire, D. W. Lewis, and C. M. Grodsinsky, "An Optimal Control Law for Microgravity Vibration Isolation," Workshop on Aerospace Applications of Magnetic Suspension, NASA Langley Research Center, Hampton, VA, September 25–27, 1990.

[9] M. G. Safonov, A. J. Laub, and G. L. Hartmann, "Feedback Properties of Multivariable Systems: The Role and Use of the Return Difference Matrix," IEEE Trans. on Automatic Controls, Vol. AC–26, No. 1, February 1981, pp. 47–65.

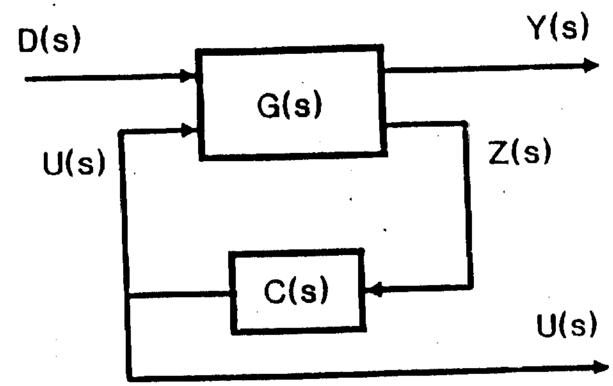


Figure 1: H_2 Synthesis Problem

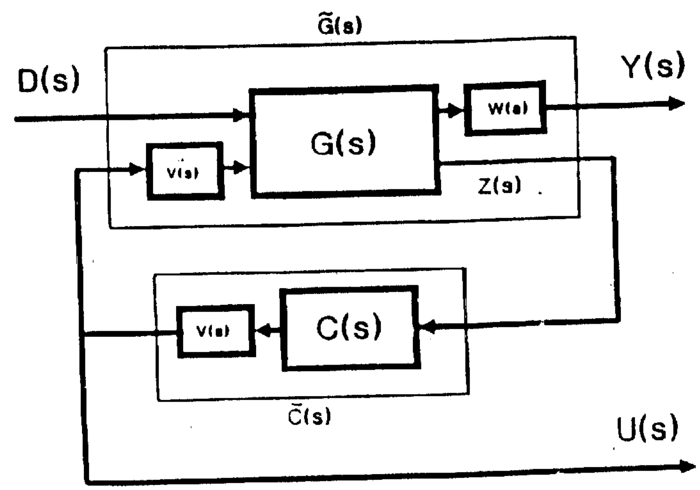


Figure 2: First Extension of H_2 Synthesis

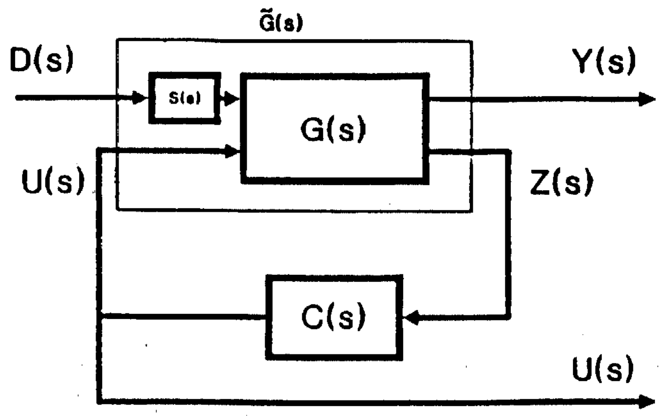


Figure 3: Second Extension of H_2 Synthesis

COMPACT LORENTZ ACTUATOR — FINAL DESIGN

Bibhuti B. Banerjee

Carl R. Knospe

Paul E. Allaire

Center for Magnetic Bearings

University of Virginia

Introduction

This report describes the final design of a compact long stroke Lorentz Actuator for a microgravity vibration isolation research project at the University of Virginia sponsored by the NASA Lewis Research Center. An earlier version was presented at the NASA Langley Workshop on Aerospace Applications of Magnetic Suspension Technology in September. 1990. The final design described here incorporates many of the same features, but is much more linear with coil position. This was accomplished through modification of the flux distribution.

A schematic of a typical Lorentz Actuator, along with the terminology used, is shown in Fig. 1. The current–carrying coil moves in and out along the core. A strong permanent magnet in the shell maintains a constant magnetic flux in the cylindrical air gap across the pole faces. irrespective of the current in the coil (within design limits). The Lorentz force generated. therefore. can be linearly varied with coil current.

Design Goals

The primary requirement was to design a non–contact actuator with a stroke of two inches and enough force capability to isolate a mass of 75 lbs connected by an umbilical (air dashpot) to a source generating very low frequency vibrations. Force linearity with position and with current were also required. Moreover, in view of the ultimate goal of deployment in space, such a device had to be compact and lightweight. Low power consumption and low heat generation during operation were also important.

Design Method

A number of designs, incorporating various features, were analyzed using the finite element analysis package MAGGIE. The finite element model was generated so as to achieve as much accuracy as possible, within hardware limitations. The mesh consists predominantly of quad elements. Infinite air elements, used earlier, were found to cause severe restrictions on mesh fineness. An air thickness of an inch on three sides of the axisymmetric model was specified instead. This was determined to be as accurate as having infinite air elements on all three sides for a model of this size, while a fine mesh could be used without encountering core memory limitations. Moreover, the finest mesh allowed by the configuration of our 386–based personal computer was used for the analysis.

Final Design

Position linearity was improved by increasing the length of the magnet, imparting a lip to it by reducing the shell outer diameter, and reducing the core diameter. The gap ratio resulting from the last change mentioned above is still only 1.47:1 — much smaller than a typically specified value of 5:1. The use of such an unconventionally low gap ratio enabled the design of a compact and lightweight actuator. Use of a large ratio would also have required a large diameter magnet that could not be made in one piece, thus increasing costs. The decrease in flux, and therefore force, caused by the increase in the length of the magnet was compensated, to some extent, by a reduction in the inner diameter of the magnet and a doubling of the pole piece thickness. Fig. 2 shows the design. The overall length of the actuator is 4 in., while the outer diameter is only 1.95 in..

The salient features of the final design of the compact Lorentz Actuator are described below:

• Long Stroke — The requirement of two inches of total stroke is satisfied.

• Position Linearity — Over the whole two inches of stroke, the actuator exhibits a high degree of linearity. For a constant coil current, this means that the actuator force is the same irrespective of the axial position of the coil, within the stroke bounds. Figures 3 and 4 depict this relationship for positive and coil currents respectively. This may also be inferred from the values of flux density from 0.3 in. to 2.3 in. (Fig. 5) for both extremes of coil current. Thus, flux leakage has been reduced to almost zero over the shell–to–core gap.

• Current Linearity — This requires that the average flux density in the effective air gap remain constant with variations in the coil current between the upper and the lower limits. This is indeed the case, resulting in a remarkable force vs. current linearity, Fig. 6.

• Force — A maximum force of 1.25 lbs is produced by this actuator, which is sufficient for our needs. This peak force requires a coil current of 2.5 A.

• Weight — At 2.28 lb, this actuator is only a tenth of a pound heavier than the previous design.

• Current Density — A value of 1000 A/sq.in. in continuous use ensures cool operation. For peak loads, a fivefold increase in current density is possible.

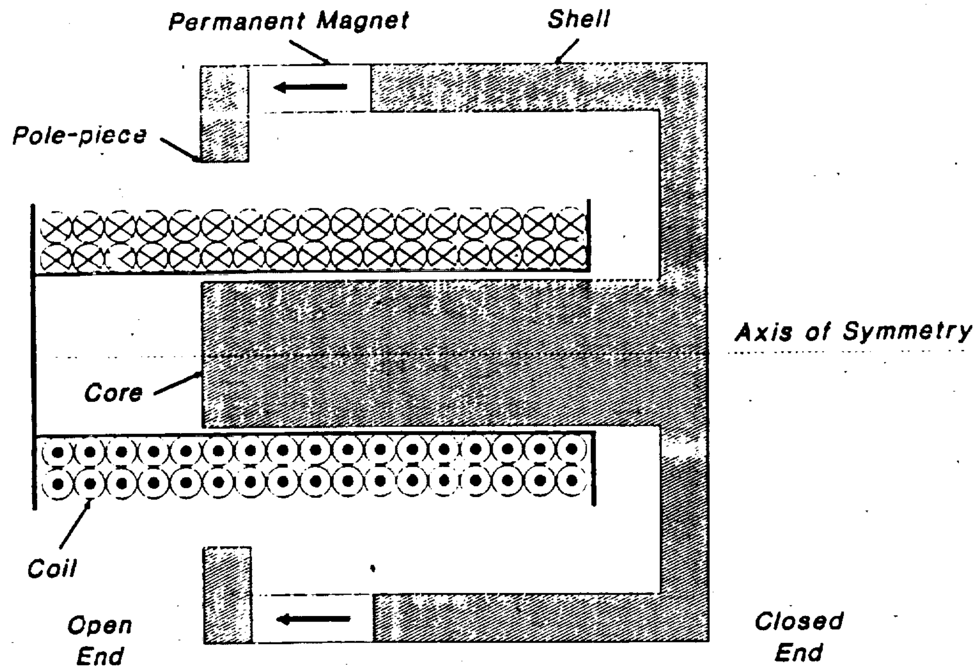
• Materials — The magnet is made of Crumax 355, which has a very high maximum energy density product of 35 MGOe (mega–Gauss–Oersted). Selection of such a material helped make the design compact. The circuit material is High Permeability "49", which is a 48% nickel–iron alloy. The B–H curve for this material, provided by the manufacturer, was input to MAGGIE as a table of a large number of points on the curve. This was necessary because a nonlinear material characteristic was being modelled.

This actuator has been built. and will be tested in our laboratory in the near future before being used on the vibration isolation rig being assembled here.

Acknowledgements

This work was supported by the NASA Lewis Research Center and the Center for Innovative Technology of the Commonwealth of Virginia.

Fig. 1: A Typical Lorentz Actuator



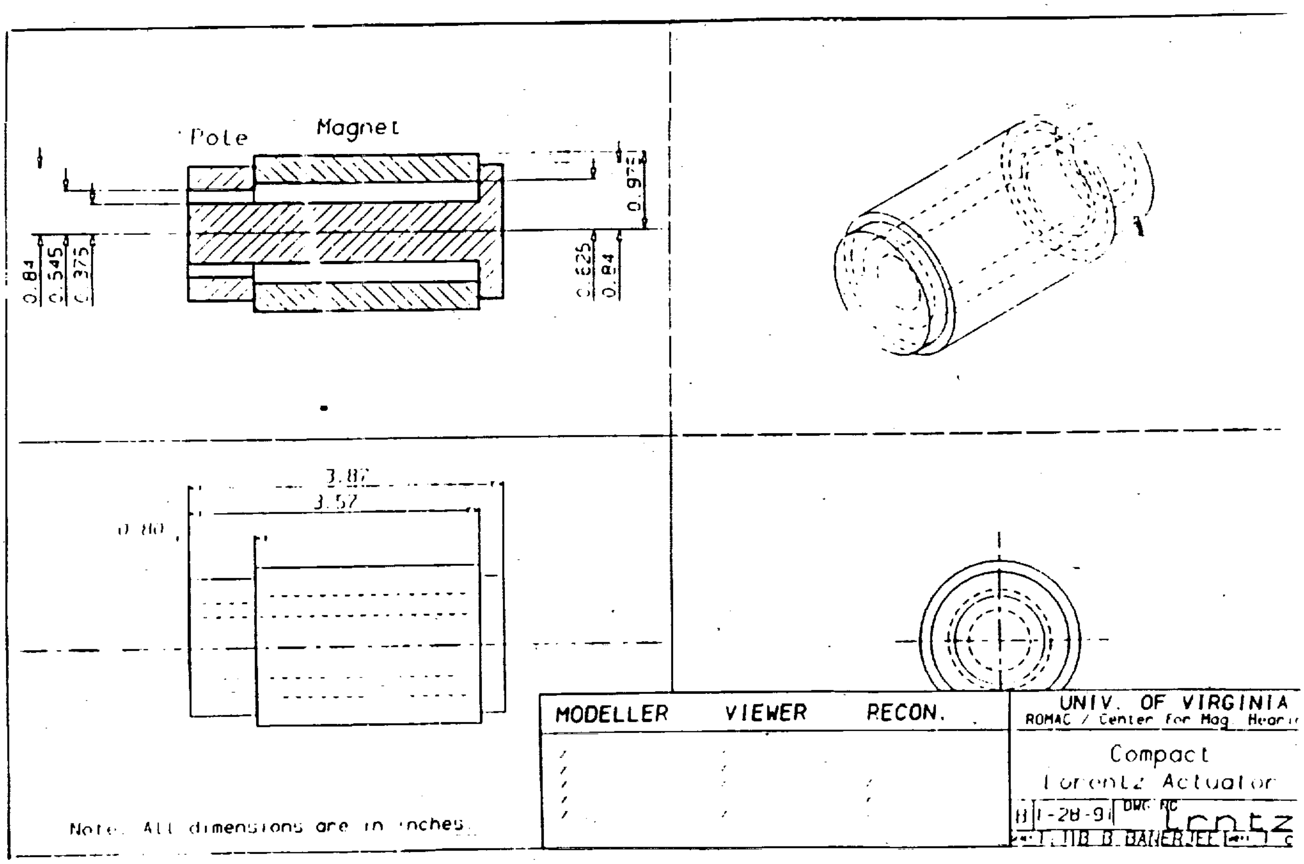


Fig. 2: Compact Lorentz Actuator

Fig. 3: Compact Lorentz Actuator - Force
Coil Currents Positive (as Shown)

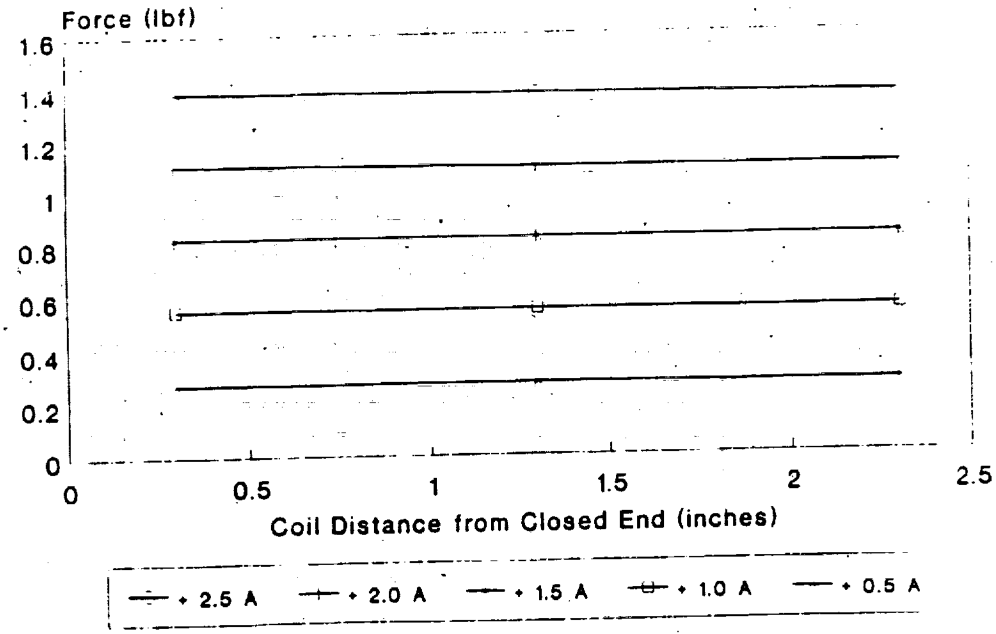


Fig. 4: Compact Lorentz Actuator - Force
Coil Currents Negative (as Shown)

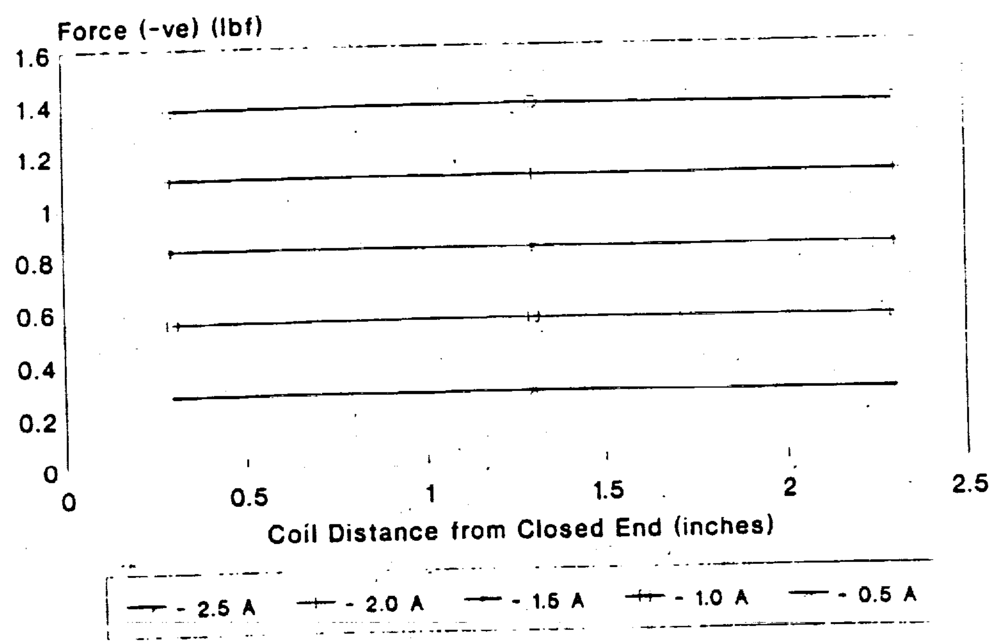


Fig. 5: Flux Density in Actuator Gaps
Current in Coil as noted

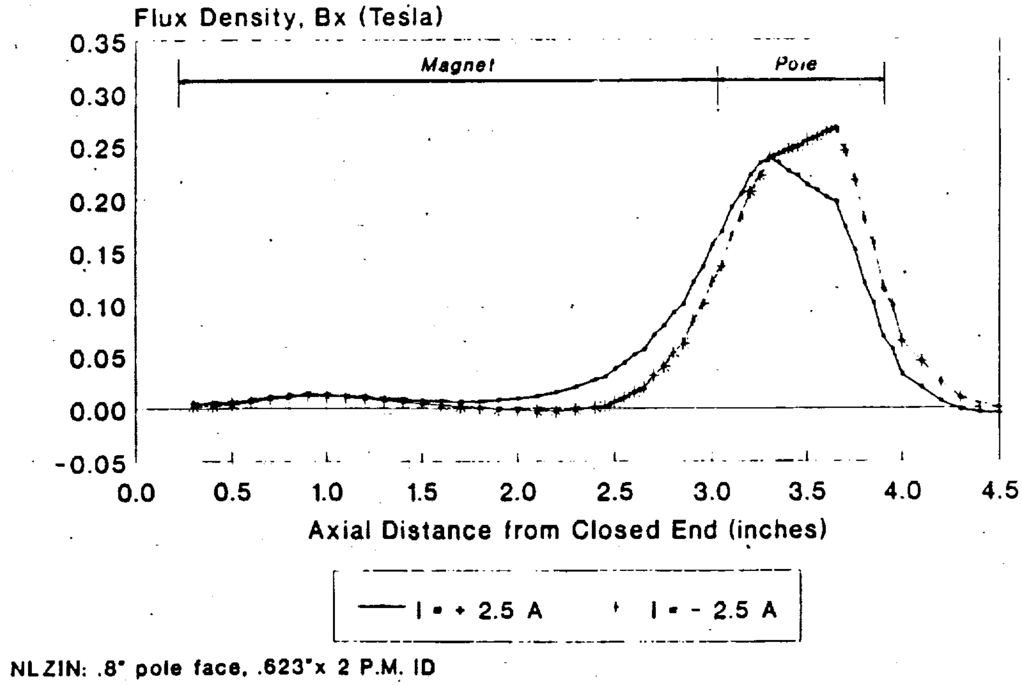
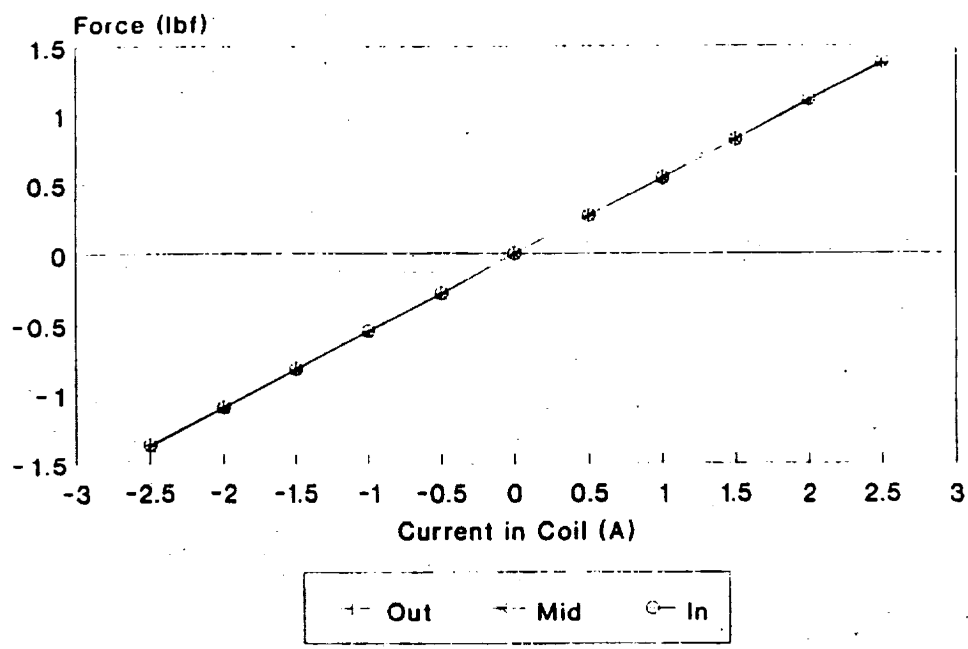


Fig. 6: Compact Lorentz Actuator - Force
Legend Indicates Coil Position



MICROGRAVITY VIBRATION ISOLATION RESEARCH AT UVA

CARL R. KNOSPE
PAUL E. ALLAIRE
ROBERT R. HUMPHRIS
DAVID W. LEWIS
A. PETER ALLAN
BIBHUTI B. BANERJEE
R. DAVID HAMPTON

CENTER FOR MAGNETIC BEARINGS
UNIVERSITY OF VIRGINIA

PERSONNEL

FACULTY:

Carl R. Knospe
Paul E. Allaire
Robert R. Humphris
David W. Lewis

GRADUATE STUDENTS:

A. Peter Allen
Bibhuti Banerjee
R. David Hampton

NASA LRC CONTACTS:

Joseph F. Lubomski
Gerald V. Brown
David P. Fleming
Carlos M. Grodsinsky

UNDERGRADUATES:

Shelly Spencer
William Wakefield

OVERVIEW

1) ONE DOF ISOLATION RIG

2) COMPACT LORENTZ ACTUATOR

3) ISOLATION CONTROL DESIGN

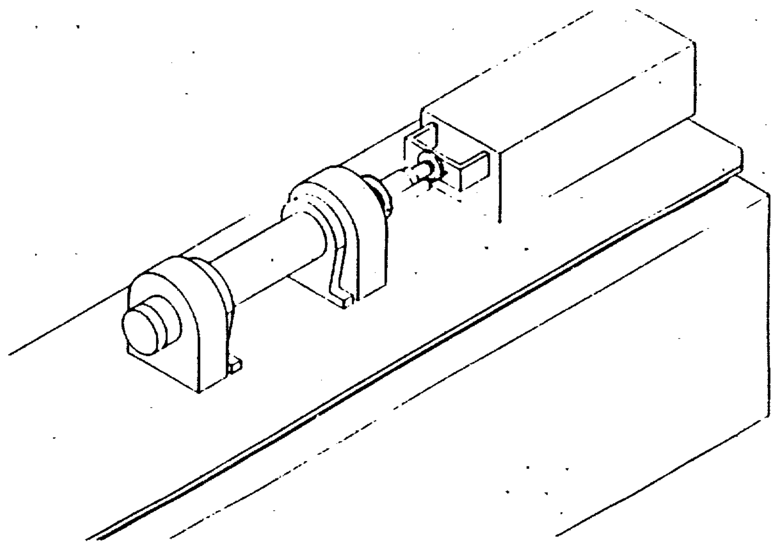
4) SIX DOF ISOLATION RIG

ONE DOF ISOLATION RIG

GOALS:

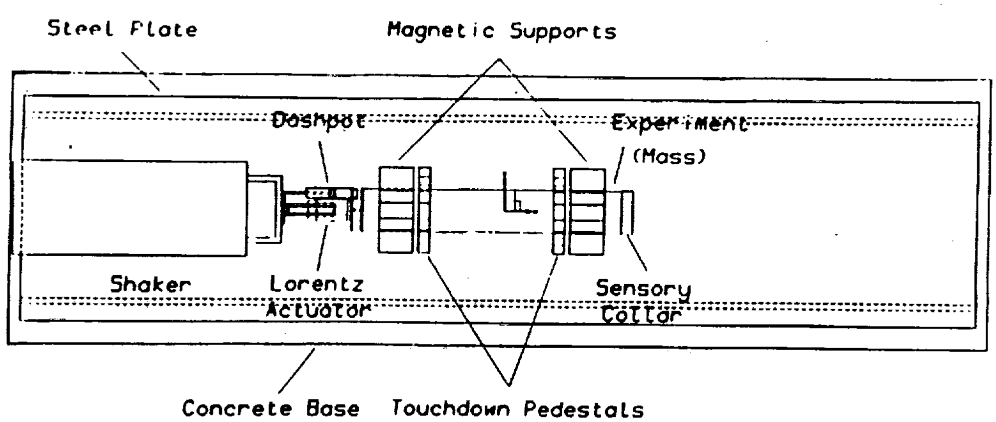
- To demonstrate that isolation to the micro-g level is acheivable with non-contacting electromagnetic actuators.
- To develop the technologies required for microgravity vibration isolation.

ONE DOF ISOLATION RIG



NASA LRC • CENTER FOR MAGNETIC BEARINGS • UVA

ONE DOF ISOLATION RIG



NASA LRC • CENTER FOR MAGNETIC BEARINGS • UVA

ONE DOF ISOLATION RIG

PLANS:

- Linear power amplifier and controller construction
- Instrumentation
- Testing

NASA LRC • CENTER FOR MAGNETIC BEARINGS • UVA

COMPACT LORENTZ ACTUATOR

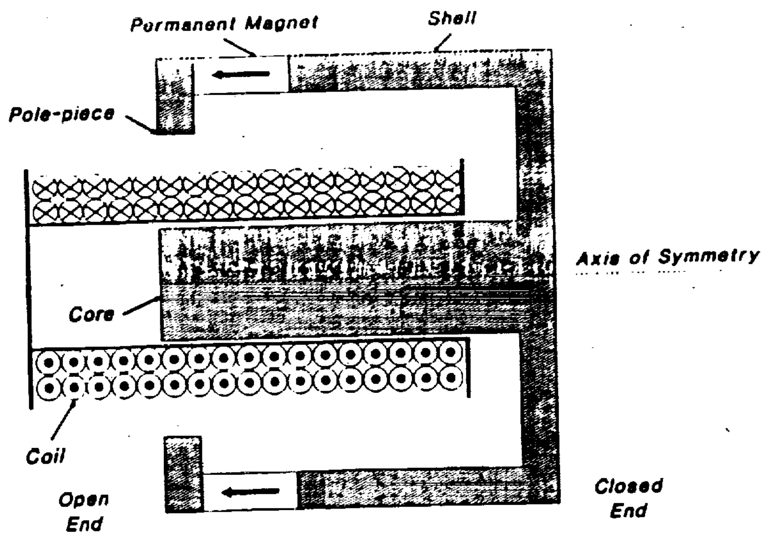
GOAL:

To design a compact, long stroke, very linear actuator for the one DOF isolation rig.

- Linearity over a long stroke dictates a Lorentz actuator design.

NASA LRC • CENTER FOR MAGNETIC BEARINGS • UVA

LORENTZ ACTUATORS



NASA LRC • CENTER FOR MAGNETIC BEARINGS • UVA

DESIGN METHODS

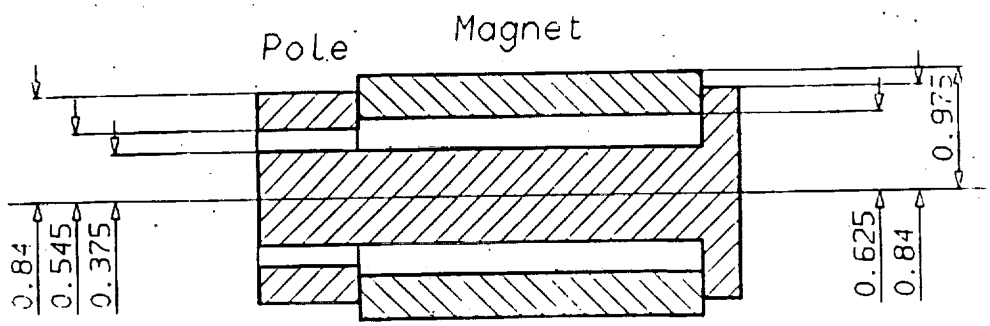
1) Electrical circuit analogy, spreadsheet method, - S. Spencer

2) Electrical circuit analogy, iterative design program, - D. Hampton

3) Finite element methods, MAGGIE program, iterative design, - B. Banerjee

NASA LRC • CENTER FOR MAGNETIC BEARINGS • UVA

FINAL DESIGN



NASA LRC • CENTER FOR MAGNETIC BEARINGS • UVA

DESIGN FEATURES

- High Linearity, both position and current, via core saturation.
- Small gap ratio yielding compact, economical design.
- Low coil current density to prevent thermal problems.

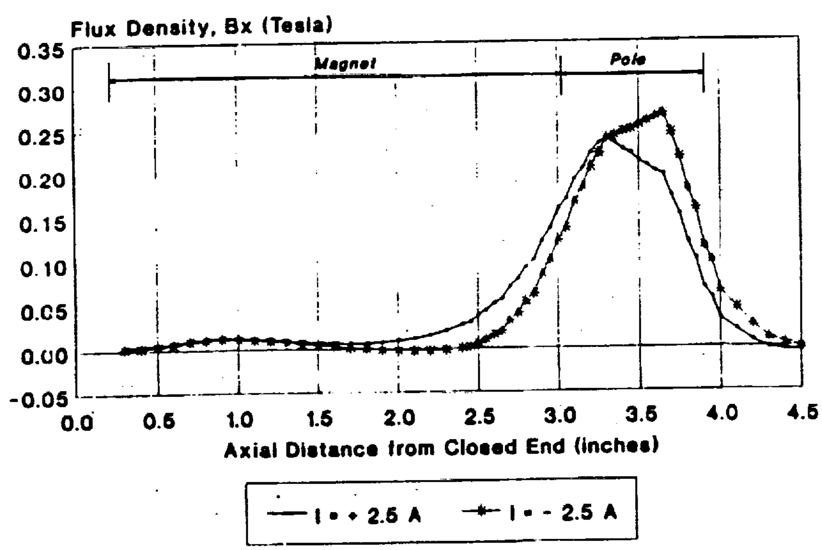
NASA LRC • CENTER FOR MAGNETIC BEARINGS • UVA

FINAL DESIGN

- Stroke: 2.0 in
- Weight: 2.3 lbs.
- Length: 4 in.
- Diameter: 1.95 in.
- Pole gap: 0.17 in.
- Core gap: 0.25 in.
- Magnet: Crumax 355
- Poles, core: High Perm. 49
- Rated force: 1.25 lbf.
- Rated current: 2.5 Amp.

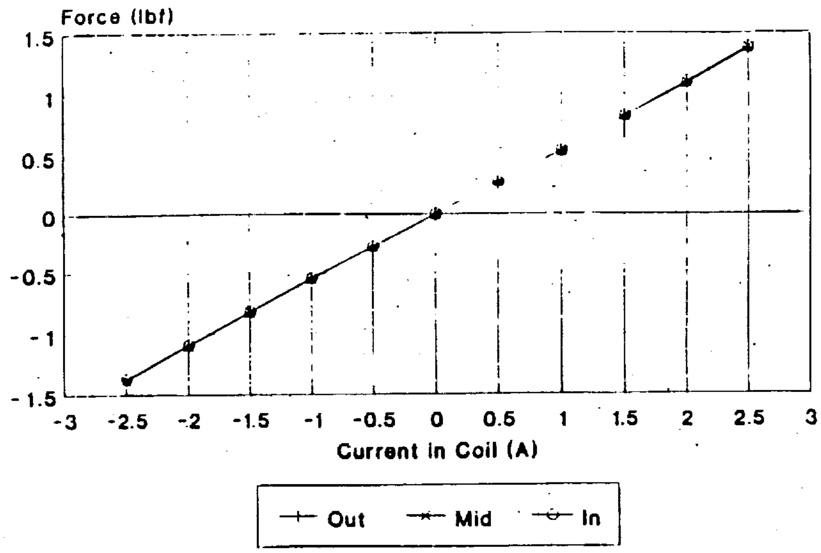
NASA LRC • CENTER FOR MAGNETIC BEARINGS • UVA

PREDICTED PERFORMANCE



NASA LRC • CENTER FOR MAGNETIC BEARINGS • UVA

PREDICTED PERFORMANCE



NASA LRC • CENTER FOR MAGNETIC BEARINGS • UVA

ISOLATION CONTROL DESIGN

GOALS:

- To design practical control algorithms to implement on the one DOF rig to achieve micro-g level isolation.
- To examine different strategies for vibration isolation.
- To develop tools to design multiple DOF micro-g isolation controllers.

NASA LRC • CENTER FOR MAGNETIC BEARINGS • UVA

ISOLATION SPECIFICATIONS

(1) Unity transmissibility from DC to 0.001 Hz.

(2) At least 40 dB attenuation above 0.1 Hz.

(3) Stability and performance robustness with respect to changes in umbilical/experiment properties, sensor/actuator misalignment, and center of mass uncertainties.

NASA LRC • CENTER FOR MAGNETIC BEARINGS • UVA

PASSIVE ISOLATION ANALOGIES

Analogies to passive isolation techniques were explored as a paradigm for active control.

- Relative stiffness
- Inertial stiffness
- Inertial damping

The analogies were examined for a one DOF benchmark problem. A control design technique known as loop shaping was also investigated.

NASA LRC • CENTER FOR MAGNETIC BEARINGS • UVA

PASSIVE ISOLATION ANALOGIES

Results:

Passive isolation analogies, while useful for understanding the isolation problem, are not an effective design tool.

High gain acceleration feedback can be employed to meet the micro-g acceleration specifications; the isolation achievable is the level of accelerometer noise.

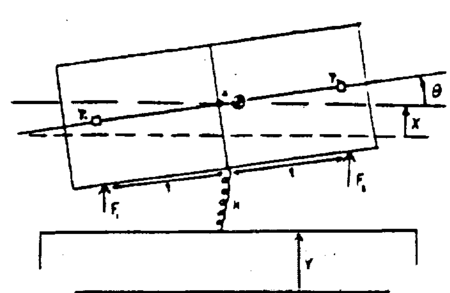
Summarized in: C. Knospe, D. Hampton, "Control Issues of Microgravity Vibration Isolation," submitted to Acta Astronautica, Sept. 90.

NASA LRC • CENTER FOR MAGNETIC BEARINGS • UVA

MULTIPLE DOF ISOLATION

Design of multiple DOF isolation control systems is much more complex than one DOF design.

MDOF example problem:



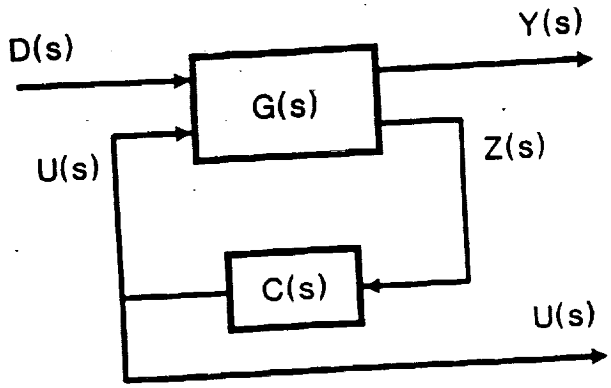
Decoupled, single axis controllers designed; as little as 6 mm center of mass shift can destabilize.

NASA LRC • CENTER FOR MAGNETIC BEARINGS • UVA

EXTENDED H2-SYNTHESIS DESIGN

Find 'optimal' controller C(s) which minimizes

$$J = \int_{-\infty}^{\infty} \| Y(j\omega) \|_2 \, d\omega + \int_{-\infty}^{\infty} \| U(j\omega) \|_2 \, d\omega$$



EXTENDED H2-SYNTHESIS DESIGN

- State space model of plant.
- Frequency shaped cost function.
- Disturbance modeled using spectral factorization of power spectral density.
- Robustness checked with singular value methods.
- Resulting algorithm must be implemented on a digital controller.

EXTENDED H2-SYNTHESIS DESIGN

PLANS:

- A six DOF benchmark problem will be selected. A linear model will be constructed.
- The design procedure will be carried out. This will require several iterations.
- The controller design will be verified via linear analysis and nonlinear simulation.

NASA LRC • CENTER FOR MAGNETIC BEARINGS • UVA

INTEGRATED DIGITAL CONTROLLER

The Center for Magnetic Bearings is currently constructing an integrated digital controller/ power amplifier for control of magnetic actuators.

- Digital signal processor based
- Capable of coordinated multiple axis control using complex algorithms
- 90 KHz sampling parallel A/D converters

A controller of this kind will be required for sucessful implementation of algorithms for large stroke, non-contacting isolation systems.

NASA LRC • CENTER FOR MAGNETIC BEARINGS • UVA

SIX DOF ISOLATION RIG

GOALS:

- To demonstrate 1 micro-g six DOF isolation using digital control of magnetic actuators.

- To develop the required electronic, control, and actuator technologies.

NASA LRC • CENTER FOR MAGNETIC BEARINGS • UVA

SIX DOF CONCEPT

Non-contacting magnetic actuators, while capable of yielding a very high degree of isolation, are in practice stroke limited.

Solution: Coarse/fine isolation

- Coarse control is achieved by large stroke contacting actuators (e.g. lead-screws).

- Fine control is achieved through non-contacting magnetic thrust actuators.

NASA LRC • CENTER FOR MAGNETIC BEARINGS • UVA

COARSE/FINE ISOLATION

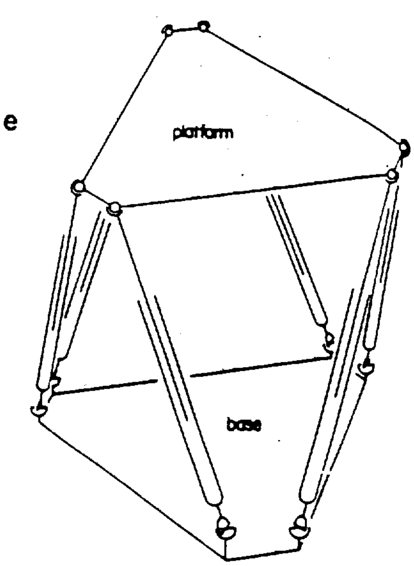
- The coarse control maintains the "inertial" position of the fine thrust actuators to within 10 mils and isolates from 0.002 to 1 Hz vibrations. Total stroke: 4+ inches.

- The fine control isolates the microgravity (fine) platform from vibrations above 0.02 Hz including vibrations induced by the coarse control actuators. Total stroke: 50 mils.

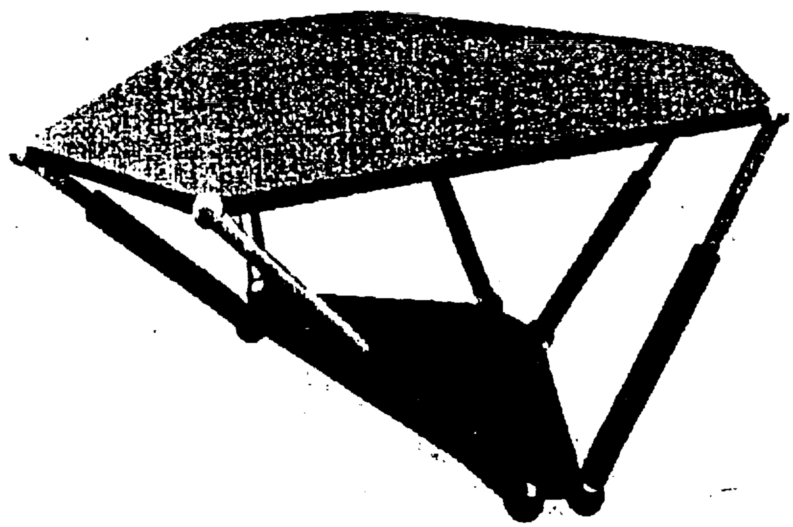
NASA LRC • CENTER FOR MAGNETIC BEARINGS • UVA

COARSE ACTUATOR - STEWART PLATFORM

Stewart platforms is a parallel connection robot manipulator. Six leg actuators are attached between the base and the coarse platform. Changing the lengths of the legs yields six DOF control of the coarse platform.



NASA LRC • CENTER FOR MAGNETIC BEARINGS • UVA



STEWART PLATFORM

ADVANTAGES:
- Very rigid
- Large stroke
- Mechanically simple

DISADVANTAGES:
- Kinematic indeterminancy
- Greater play in mechanism
- Direct kinematics unsolved

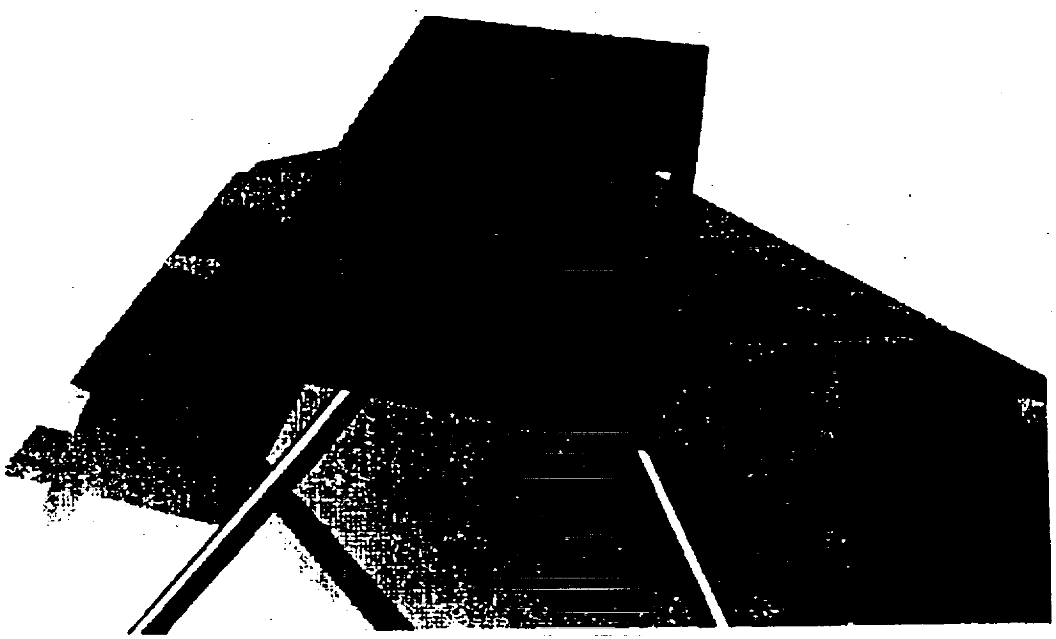
Stewart platforms have been used in aircraft simulators, vibration testing, and robotics. Researchers are currently investigating these manipulators.

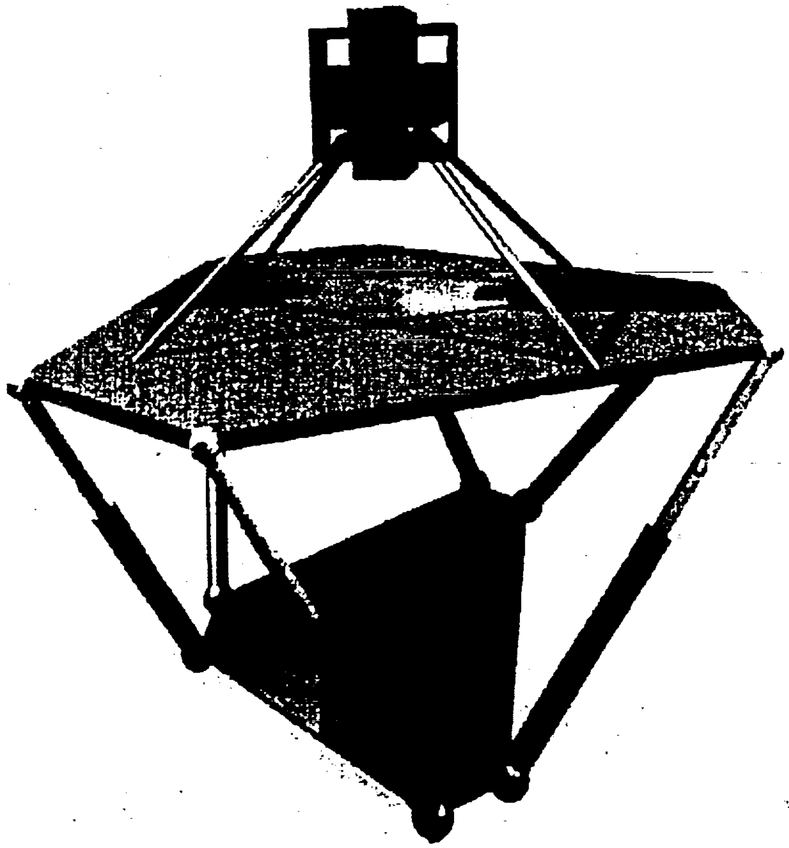
FINE PLATFORM

Several alternate concepts for six DOF electromagnetic control of the fine (microgravity) platform are being considered.

- Twelve individually controlled electromagnets provide suspension and isolation for the fine platform.
- Coordination of coil currents permits any combination of force and moment to be exerted on the fine platform.

NASA LRC • CENTER FOR MAGNETIC BEARINGS • UVA





SIX DOF ISOLATION

PLANS:

- Complete design of coarse and fine isolation systems.
- Coarse platform controller design
  - (a) linearized, look-up table based
  - (b) neural network controller
- Fine platform controller design via extended H2 synthesis.
- Construction and testing?

NASA LRC • CENTER FOR MAGNETIC BEARINGS • UVA

CONCLUSIONS

Research at the University of Virginia is progressing on both single and multiple DOF microgravity vibration isolation technology.

Key enabling technologies in which great progress is being made are:

- Finite element modeling of electro-magnetic actuators.
- Advanced control algorithms for six DOF isolation.
- Innovative isolation achitectures for long stroke, non-contacting suspension.
- Advanced digital controllers

NASA LRC • CENTER FOR MAGNETIC BEARINGS • UVA

ACAP N92-28448

ACCELERATION CHARACTERIZATION and ANALYSIS PROJECT

presentation to the

VIBRATION ISOLATION WORKSHOP

LEWIS RESEARCH CENTER
CLEVELAND, OHIO

APRIL 24, 1990

Charles R. Baugher
Space Science Laboratory, ES7
Marshall Space Flight Center
205/544-7417

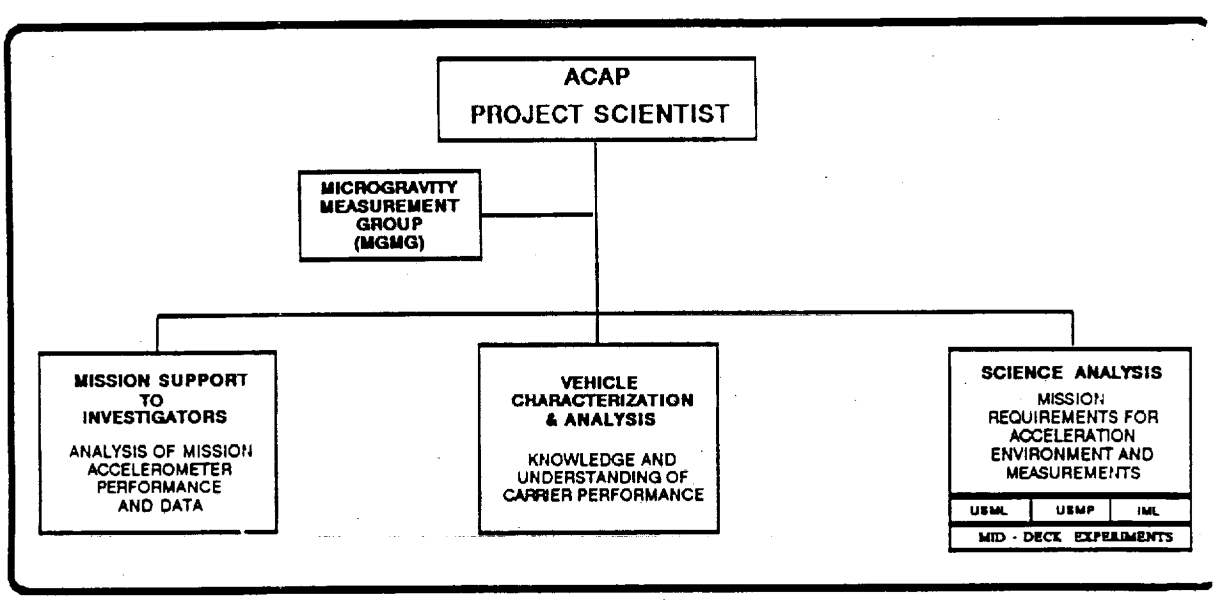
Headquarters Code SN Manage
G. L. Martin

ACCELERATION CHARACTERIZATION & ANALYSIS PROGRAM (ACAP)

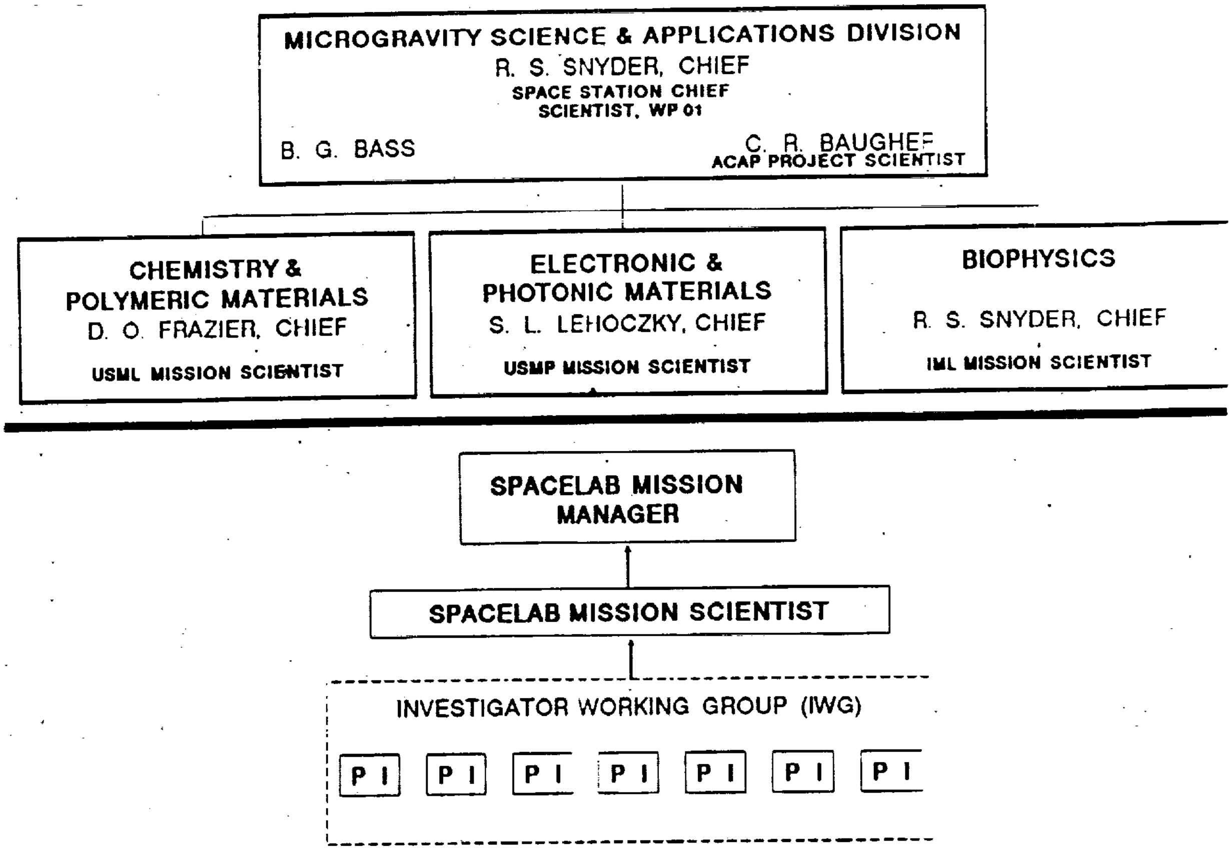
ACAP ESTABLISHED TO ASSIST INVESTIGATORS AND MISSION SCIENTISTS IN UNDERSTANDING AND EVALUATING MICROGRAVITY ENVIRONMENT OF EXPERIMENT CARRIERS

ACAP ACTS AS PROJECT SCIENTIST FOR FLIGHT ACCELEROMENTERS AND PERFORMS OR COORDINATES DATA ANALYSIS

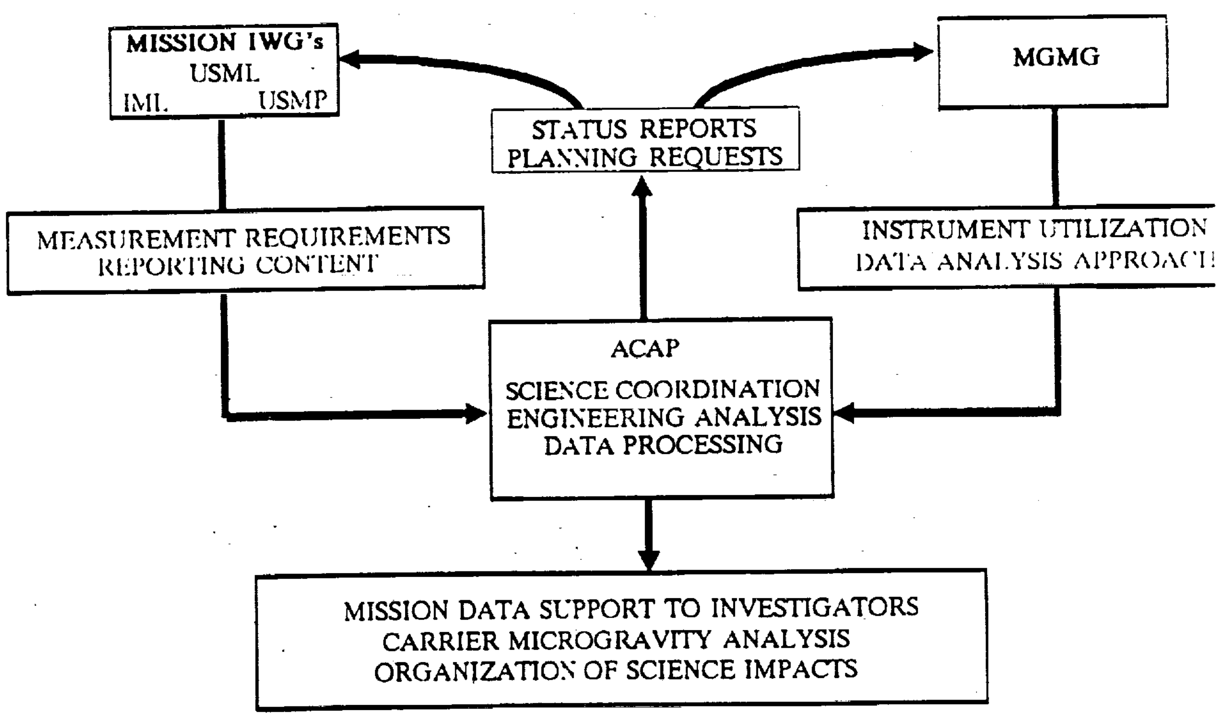
ACAP RESPONSIBLE TO OSSA FOR ORGANIZING SCIENTIFIC ANALYSIS OF EFFECTS OF MISSION ENVIRONMENT ON MICROGRAVITY SCIENCE OBJECTIVES



GEORGE C. MARSHALL SPACE FLIGHT CENTER
SPACE SCIENCE LABORATORY

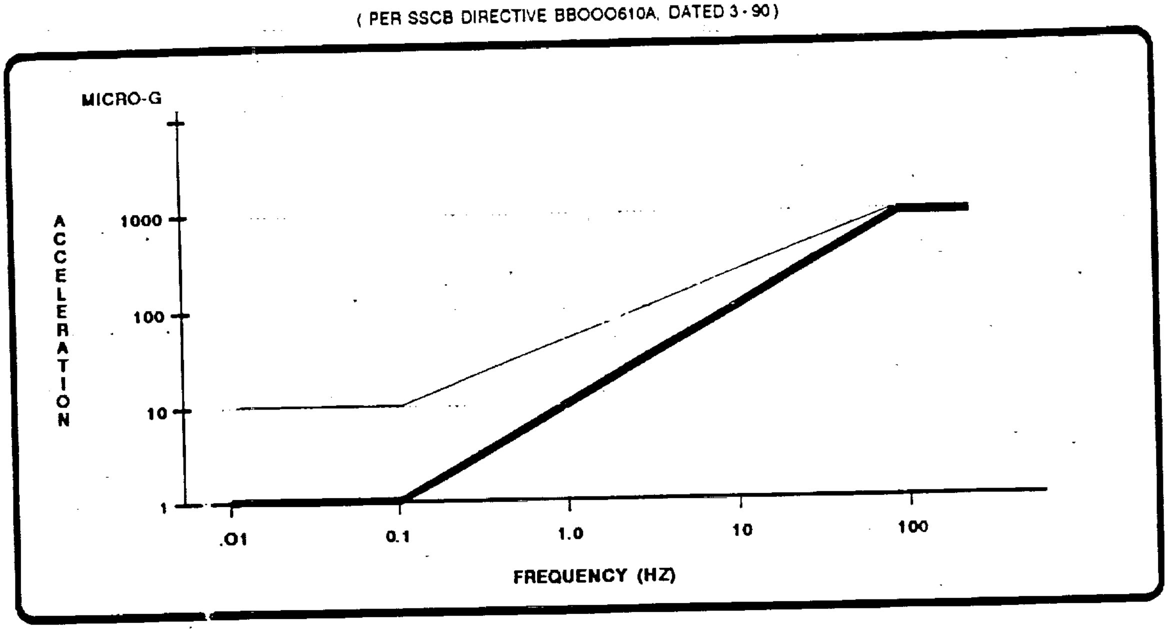


ACAP RELATION TO MISSIONS, MEASUREMENTS, & ANALYSIS



MICROGRAVITY ACCELERATION REQUIREMENTS CURVE

SPACE STATION
OSCILLATORY & TRANSIENT DISTURBANCE LIMITS
(PER SSCB DIRECTIVE BB000610A, DATED 3-90)

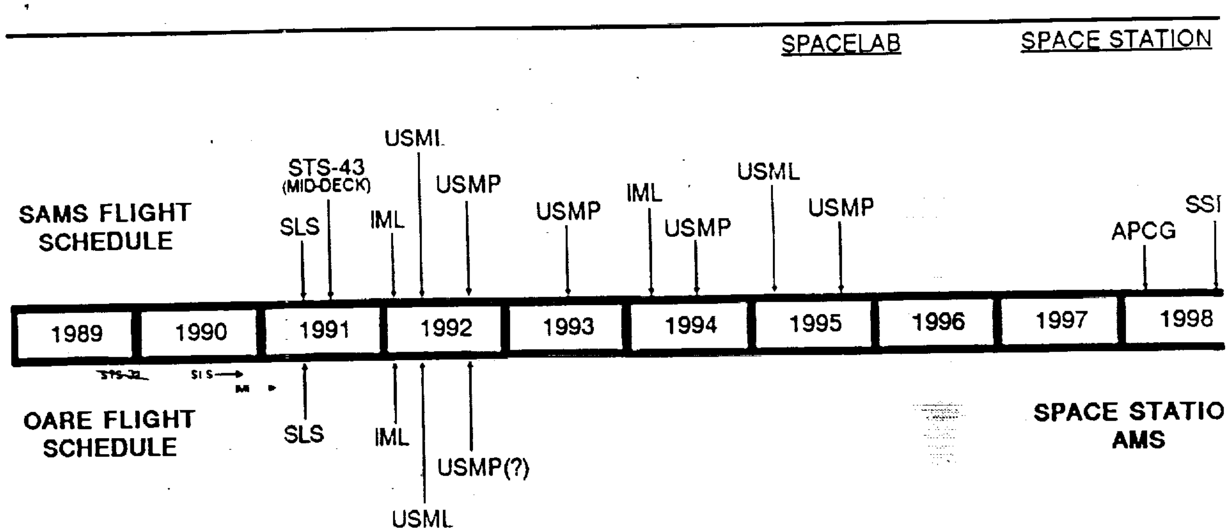


LOWER CURVE APPLIES TO ALL ROUTINE DISTURBANCES - INCLUDING CREW EXERCISE AND PAYLOAD HARDWARE

UPPER CURVE APPLIES TO EVENTS WHICH ARE CONSIDERED TO BE SCHEDULABLE

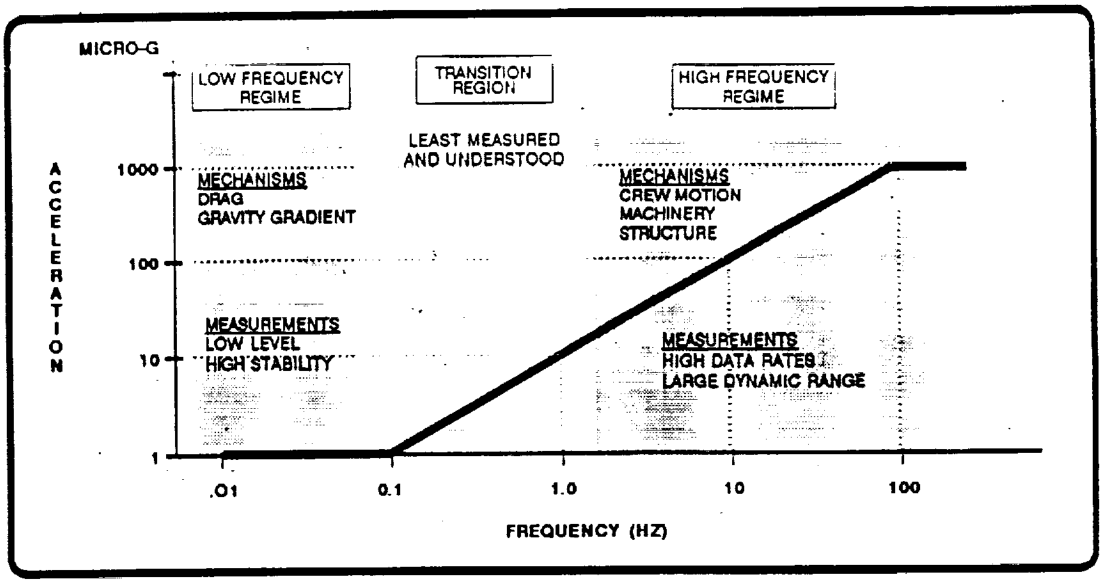
REQUIREMENT IS IN EFFECT FOR 50% OF THE USER RACKS FOR INTERVALS OF AT LEAST 30 DAYS

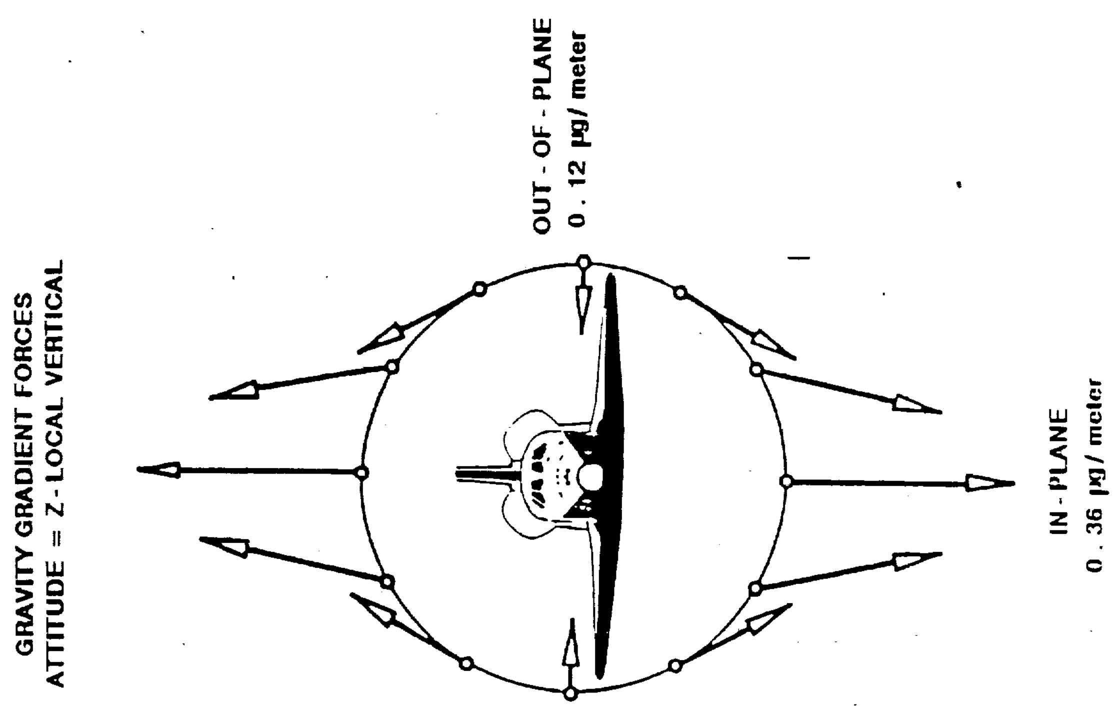
MICROGRAVITY PAYLOADS SCHEDULE OF MAJOR FLIGHTS



MICROGRAVITY ACCELERATION REQUIREMENTS CURVE

MEASUREMENT, CONTROL AND ISOLATION CHALLENGE





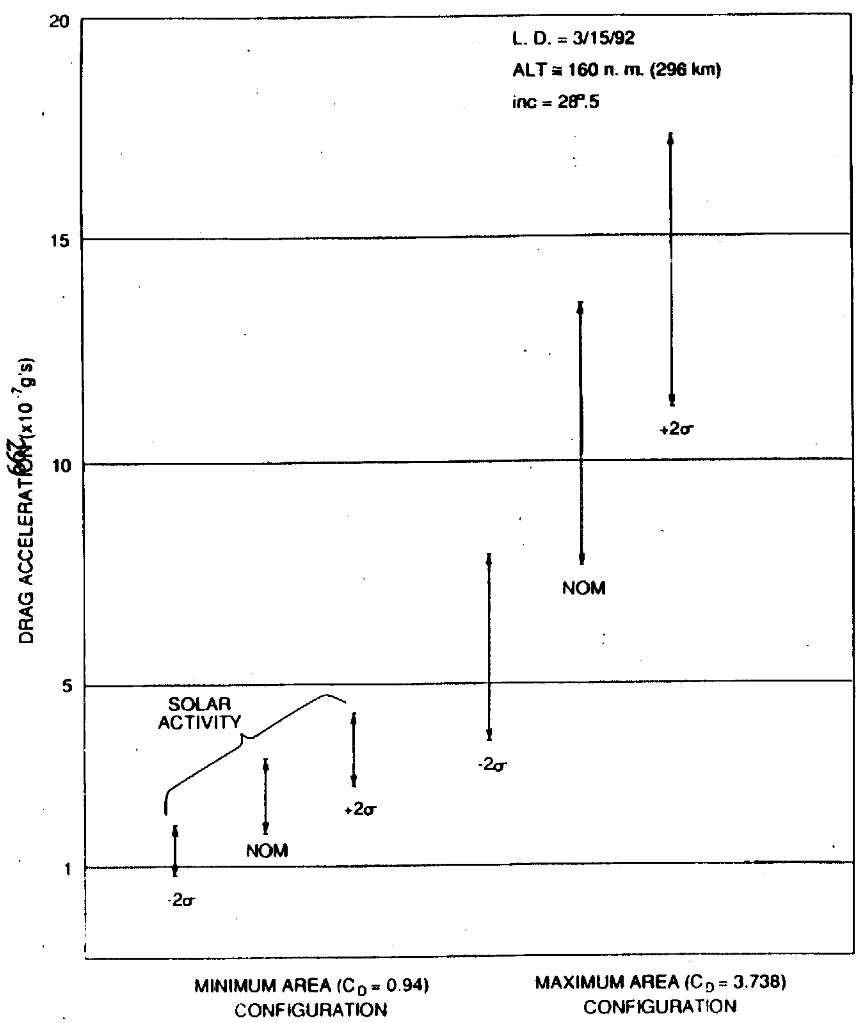
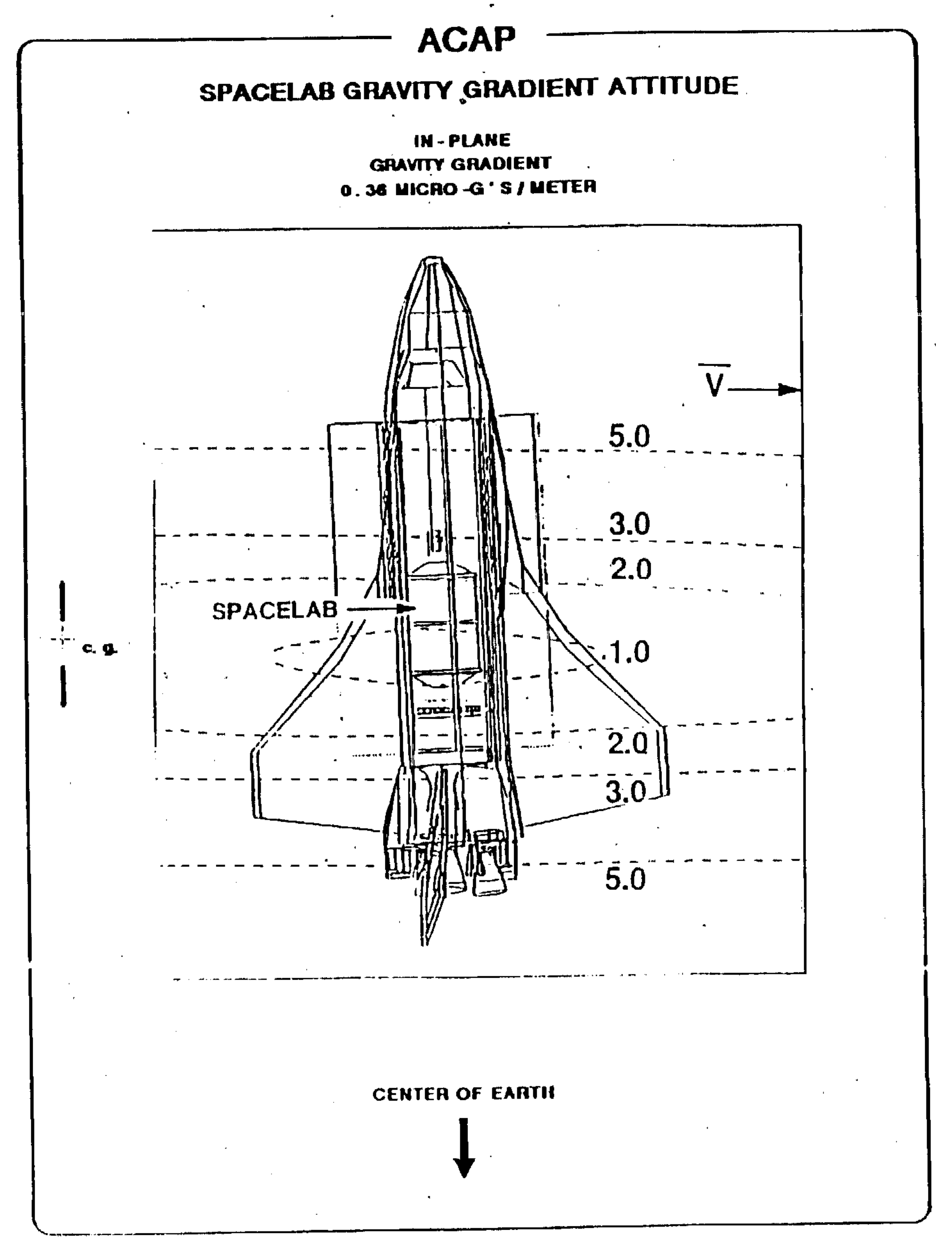
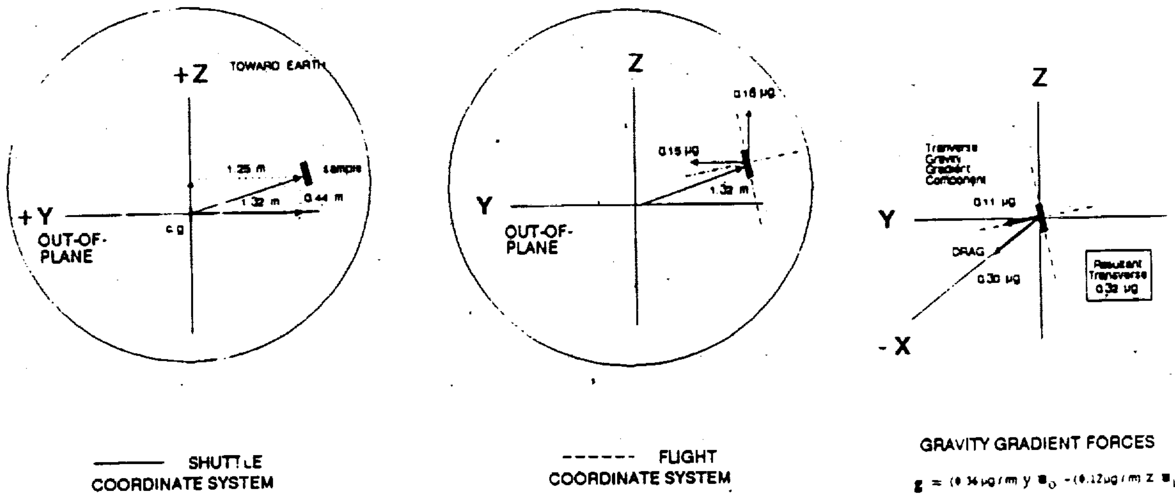


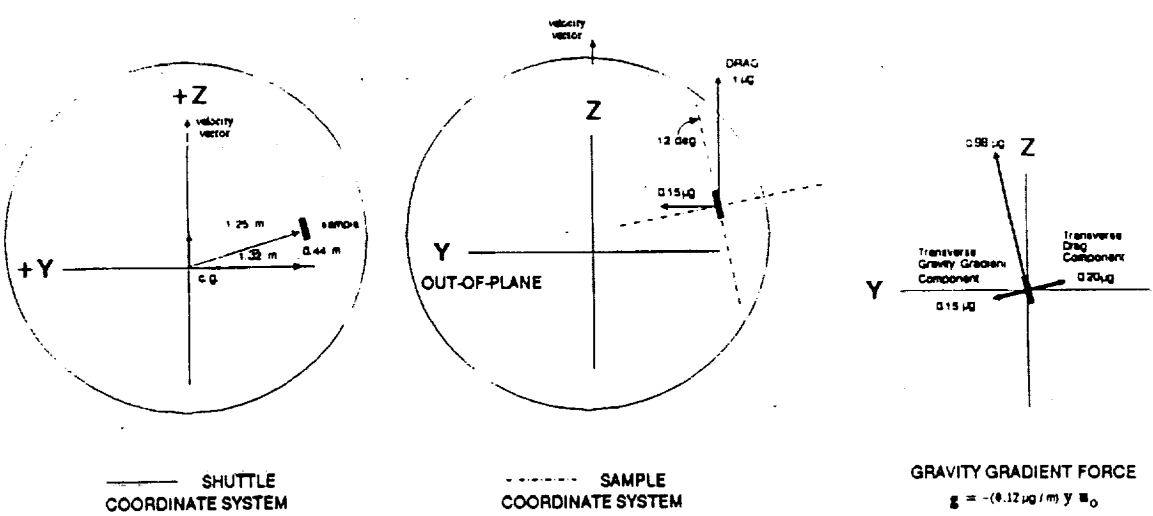
FIGURE 4. RANGE OF DRAG ACCELERATION LEVELS vs. SOLAR ACTIVITY AND SHUTTLE ATTITUDE



Z-LOCAL VERTICAL
X = 1087 inches



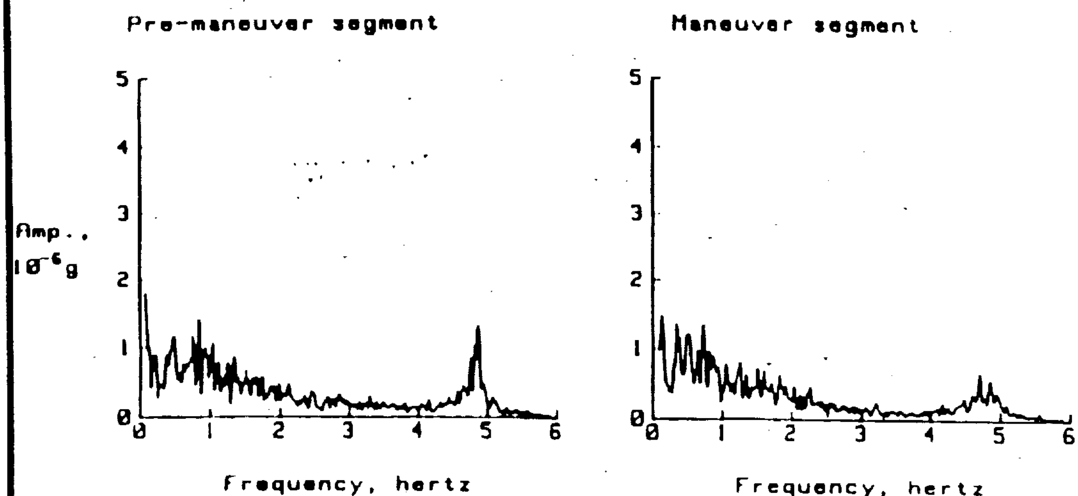
GRAVITY GRADIENT ATTITUDE - LOOKING AFT
X = 1087 inches



ACAP

DETERMINATION OF SHUTTLE ORBITER CENTER - OF - GRAVITY
FROM FLIGHT MEASUREMENTS, BLANCHARD, R. C., E. W. HINSON, J. Y. NICHOLSON.
FIFTH MEETING OF THE MGMG, MARCH 13 & 14, 1990, WASHINGTON, D. C.

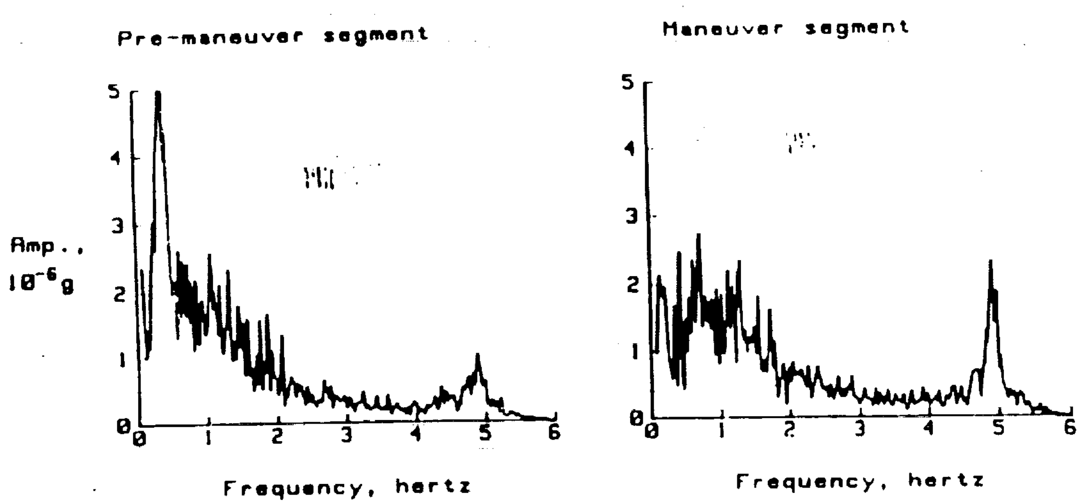
COMPONENT SPECTRA, MANEUVER 2, Z-AXIS



ACAP

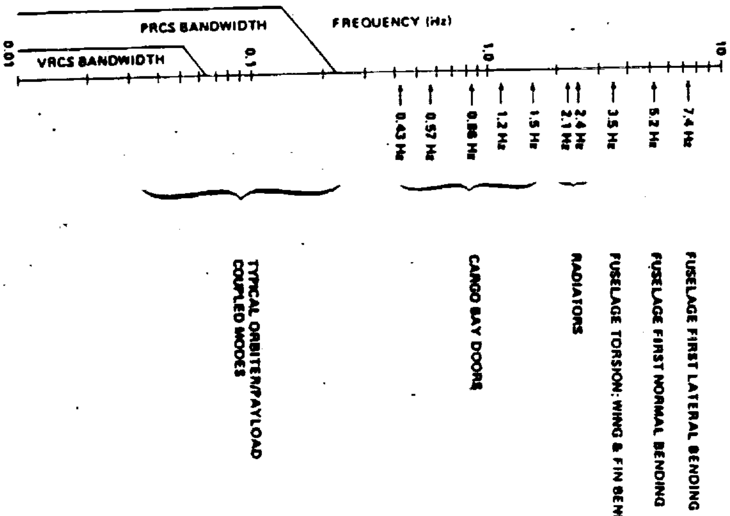
DETERMINATION OF SHUTTLE ORBITER CENTER - OF - GRAVITY
FROM FLIGHT MEASUREMENTS, BLANCHARD, R. C., E. W. HINSON, J. Y. NICHOLSON.
FIFTH MEETING OF THE MGMG, MARCH 13 & 14, 1990, WASHINGTON, D. C.

COMPONENT SPECTRA, MANEUVER 1, Z-AXIS



HIRAP Data for Micrograviy

Typical Structural Frequencies

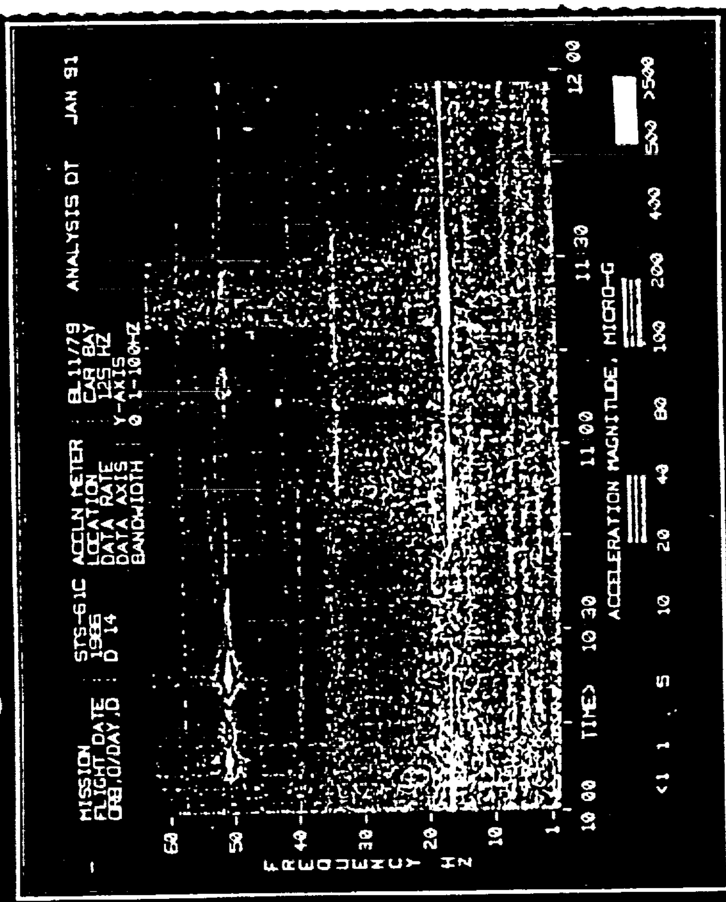


CSDL

EVB - 4 - 9/90

Marshall Space Flight Center

Space Science Laboratory



STS Microgravity Environment

Charles R. Baugher, Project Scientist
Acceleration Characterization & Analysis Project

Narayanan Ramachandran
USRA/Space Science Laboratory

Fred H. Henderson
Teledyne-Brown Engineering

This chart provides a frequency vs. time view of the low-level acceleration environment for microgravity payloads on the MSL carrier during the twenty-fourth flight of the space shuttle in December 1986. The dominant disturbance seen at 17 hertz is driven by the dither of the STS TDRESS antenna. This disturbance appears variable and saturates the accelerometer at about 11:20. Vehicle structural modes are visible below 1 hertz and at about 3, 5, 7, and 35 hertz. The origin of the strong vibration above 50 hertz is unknown. This display was prepared for use with data from the OSSA Space Acceleration Monitoring System to be flown on all future microgravity Spacelab missions. The data plotted here is from the Linear Triaxial Accelerometer assembled by the Sensory Branch of the MSFC Information & Electronic System

ACAP

MEASUREMENTS OF THE LOW - LEVEL ACCELERATION ENVIRONMENT OF THE IML - 1 MODULE

at the

KSC MISSION SEQUENCE TEST

QUICK - LOOK

IML MISSION SCIENTIST: R. S. SNYDER
ACAP PROJECT SCIENTIST: C. R. BAUGHER
LEAD ENGINEER: F. H. HENDERSON (TBE)

ACAP

ACAP IML MISSION SEQUENCE TEST

OBJECTIVES OF TEST

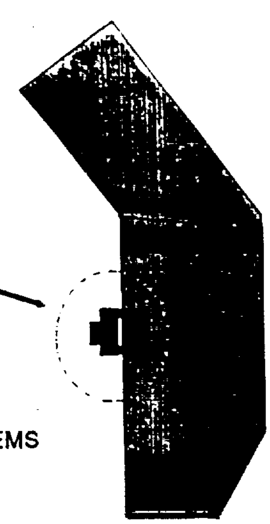
ASSESS DYNAMIC INFLUENCE OF LSLE REFRIGERATOR ON OTHER PAYLOAD LOCATIONS

COMPARE GROUND MEASUREMENTS TO SPACE MEASUREMENTS VIA SAMS

MEASURE OTHER "TARGETS OF OPPORTUNITY"

APPROACH

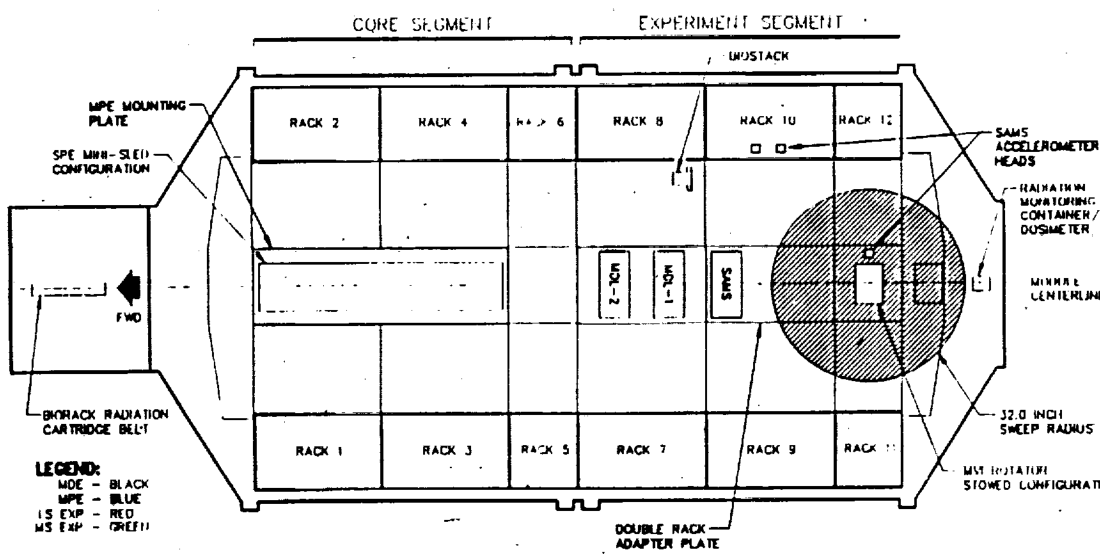
ACAP CONSTRUCTED ACCELEROMETERS (THREE - AXIS) WERE MOUNTED ON THREE PAYLOADS:



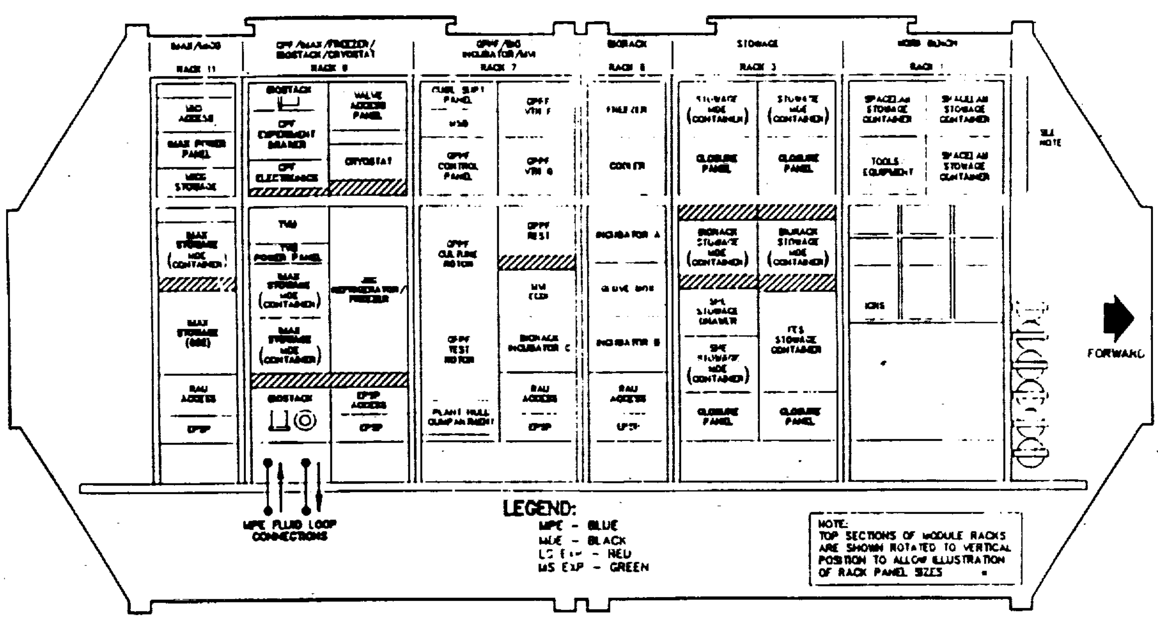
LSLE REFIRGERATOR
GPPF (ADJACENT RACK)
FES (ACROSS AISLE)

ACCELERATION LEVELS RECORDED ON MACINTOSH & IBM - AT SYSTEMS WITH LIMITED REAL - TIME ANALYSIS

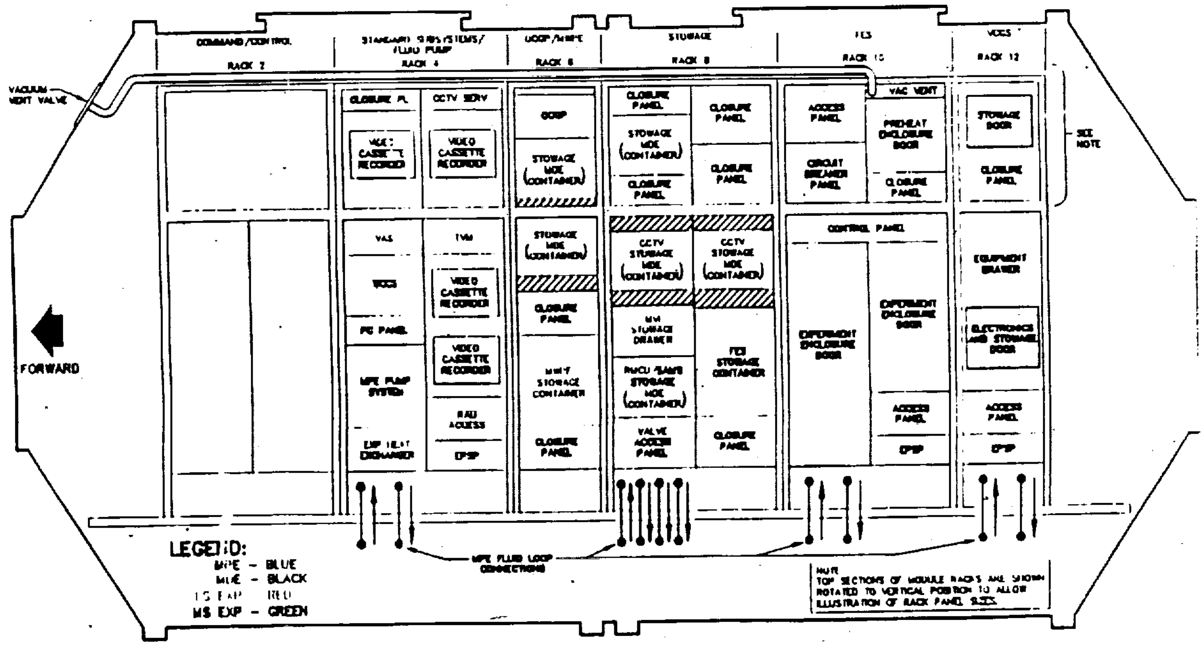
IML-1 MODULE FLOOR CONFIGURATION



IML-1 MODULE PORT SIDE



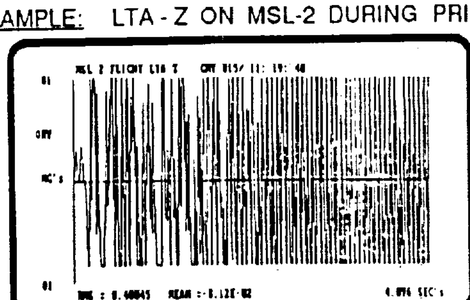
IML-1 MODULE STARBOARD SIDE

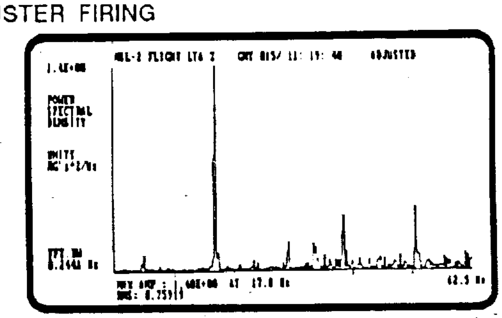


ACAP
DATA ACQUSITION FILTERS

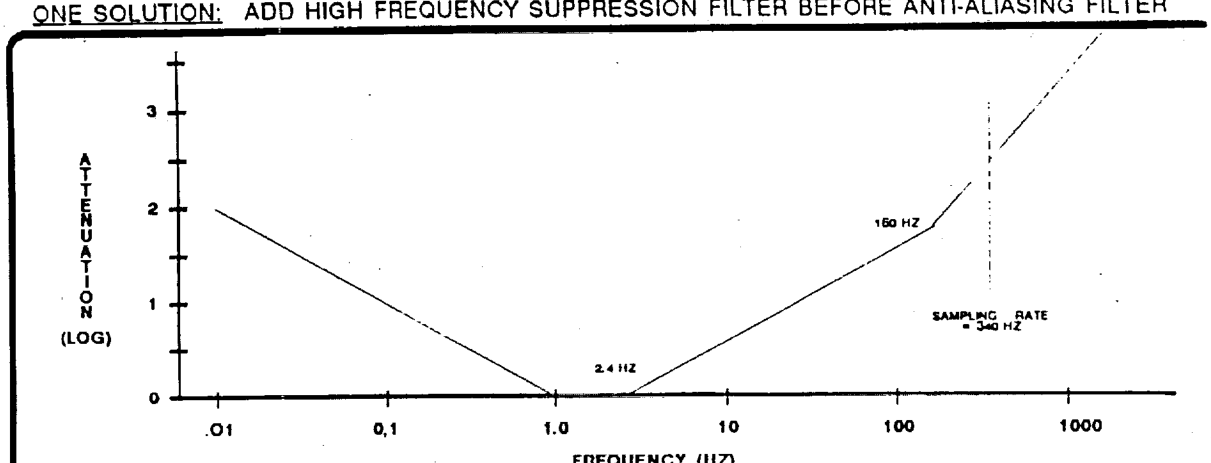
PROBLEM: DYNAMIC RANGE OF MEASUREMENTS AND SATURATION OF MEASUREMENT SYSTEM

EXAMPLE: LTA - Z ON MSL-2 DURING PRIMARY THRUSTER FIRING



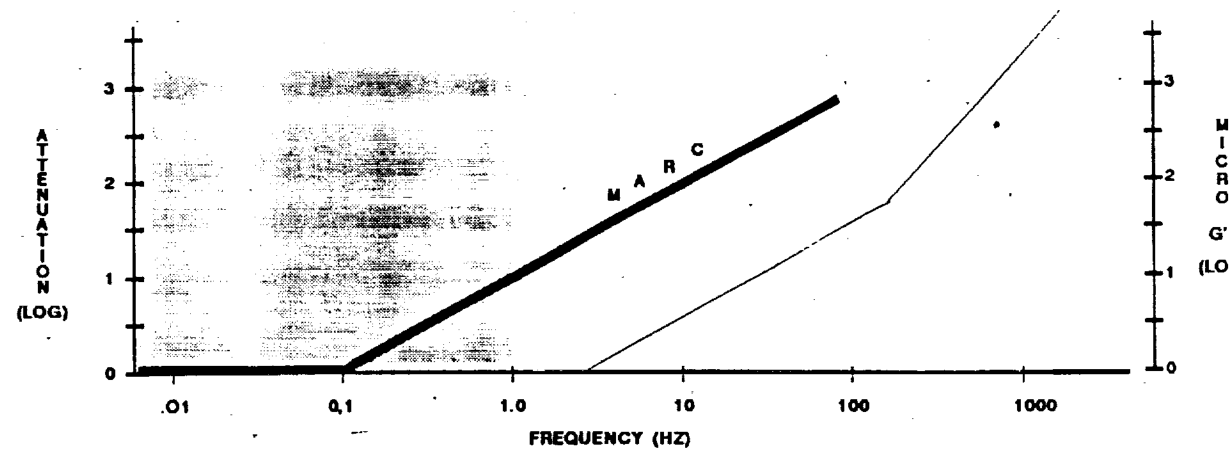


ONE SOLUTION: ADD HIGH FREQUENCY SUPPRESSION FILTER BEFORE ANTI-ALIASING FILTER



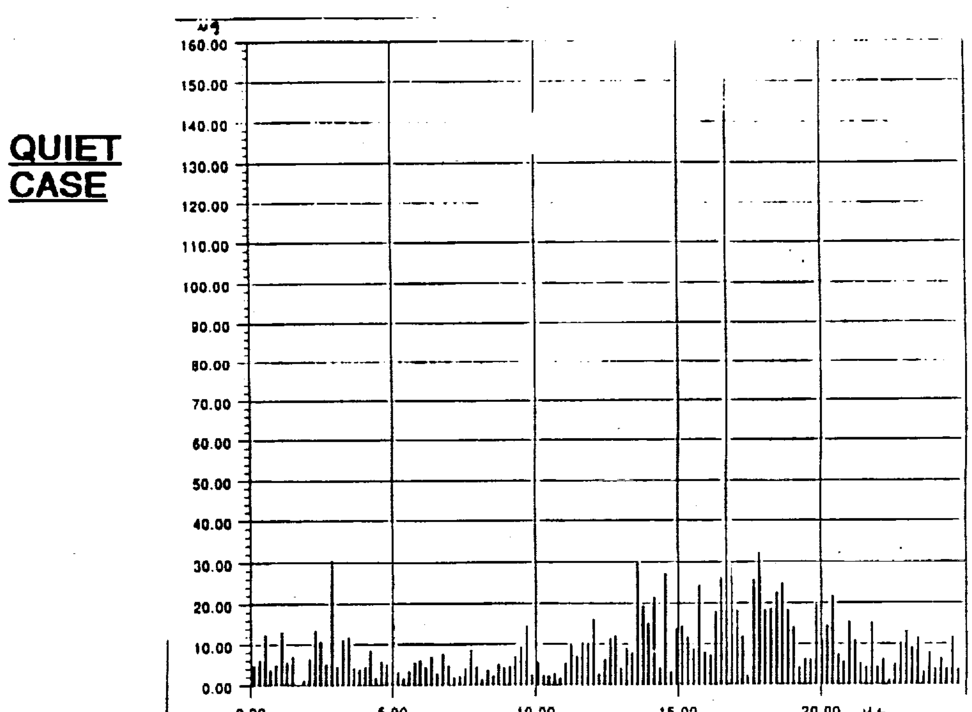
ACAP
DATA ACQUSITION FILTERS

COMPARISON TO MICROGRAVITY ACCELERATION REQUIREMENTS CURVE



ACAP

ANALYSIS OF THE STS-32 HONEYWELL IN SPACE ACCELEROMETER DATA FOR THE MATERIALS DISTURBANCE EXPERIMENT

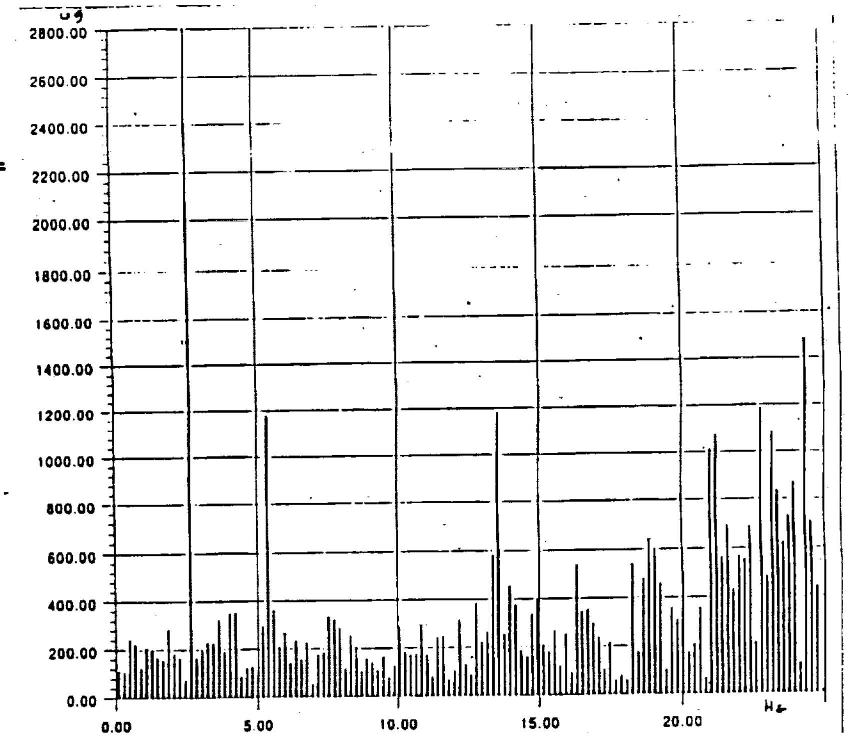


ACAP

ANALYSIS OF THE STS-32 HONEYWELL IN SPACE ACCELEROMETER DATA FOR THE MATERIALS DISTURBANCE EXPERIMENT

TREADMILL ACTIVE

RUNNING CASE



ACAP

QUESTIONS TO ANSWER FROM THE FLIGHT DATA

WHEN WE STARTED:

WHAT IS THE MICROGRAVITY ENVIRONMENT FOR EXPERIMENTS ?

WHERE WE ARE:

CAN WE MODEL AND PREDICT THE LOW FREQUENCY REGIME ?
ARE THE CHARACTERISTIC MODAL VIBRATION PATERNS A DOMINATE FEATURE ?
HOW DO THE MODAL VIBRATION PATTERNS VARY FROM LOCATION TO LOCATION ?
HOW ARE DISTURBANCES OBSERVED TO PROPAGATE ?
WHAT IS THE IDENTITY AND MAGNITUDE OF TRANSIENT DISTURBANCES ?

N92-28449

Shock and Vibration Isolation for Cyclic Exercise in Spacecraft

W. Thornton, M.D., Scientist Astronaut, NASA JSC

Presented at an International Workshop on Vibration Isolation for Microgravity Science Applications, Cleveland, OH 1991

A unique feature of undisturbed space flight is a vibration- and weight-free environment not available on Earth. Such an environment is of particular value to materials science, which is currently one of two primary activities planned for Space Station (SS). Very stringent G limits, 1 microgravity at low frequencies, have been accordingly imposed in response to experimenter's requirements.[1] These missions are manned and crew activity is also an essential mission component. On long flights, exercise and especially locomotor exercise will be required if the crew is to function in relatively normal fashion and avoid lengthy rehabilitation on return to Earth.[2,3] Exercise forces in the low-frequency range can amount to 2-3 times crew body weight in the frequency range most critical to material science.[4,5]

The relation between forces from a variety of in-flight activities and resulting accelerations for a series of rigid masses were plotted in figure 1 to illustrate the magnitude of the problem. It is obvious to those who have dealt with the problem that orders of magnitude of isolation beyond those available from current techniques will be required.

The following is a very brief rationale of the need for these exercises and a description of exercise forces, a subject not widely appreciated. Current isolation means and their deficiencies will then be briefly described, a method capable of providing the isolation proposed and work to date with it mentioned.

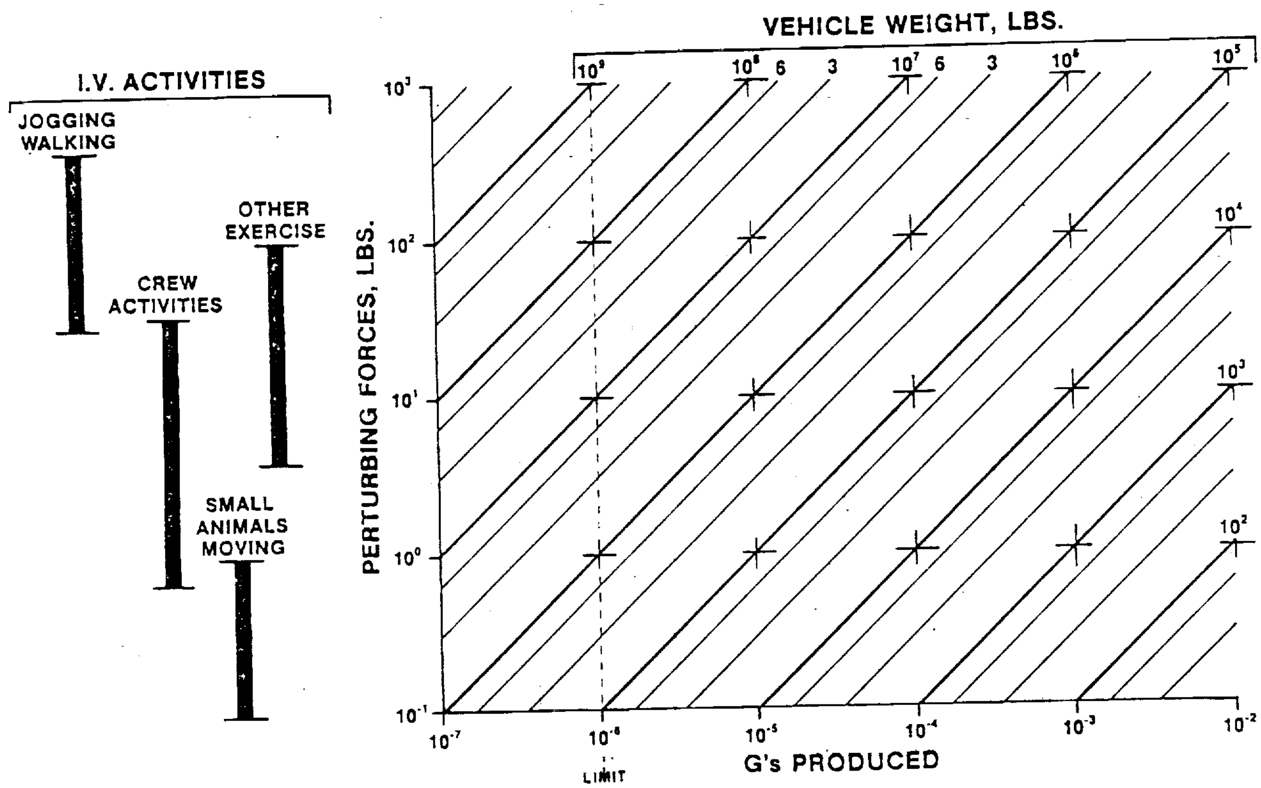


Fig. 1 - Plot of rigid single-body accelerations produced by single-axis forces for a range of masses. Range of forces for some typical activities are shown. There is a great discrepancy between specified limits and expected activities.

PRECEDING PAGE BLANK NOT FILMED

Human Exercise in Space:

All human movement is provided by forces generated by muscle and transmitted by bone. Force capacity or strength is a function of the usual maximum loads experienced by this combination of bone and muscle (e.g. the arms of a weight lifter versus those of a distance runner). Not only are the muscles of the weight lifter much larger but so too are the associated bones — they have to be able to support the muscle load. In quiet standing on Earth, one leg may support a maximum of one body weight (1 BW) and usually 1/2 BW. The BW is carried by bone with little muscle force required. (See fig. 2.) In walking, each foot must alternately support slightly more than 1 BW by muscle force. In jogging/running, the body is thrown clear of all ground contact by muscle forces each step, and the inertial loading now increases foot ground forces (FGF) to 2-3 BW. Bone and muscle forces are several times higher than FGF through anatomical leverage, fig. 3. Without such large forces, such as lack of work and exercise on Earth, both muscle[6] and bone[7,8] will atrophy — muscle in weeks and bone in months. The resulting reduction in metabolic loads will also reduce cardiorespiratory capacity.[9]

In weightlessness, locomotion is impossible; legs are virtually unused; and muscle and bone strength and mass are lost at a near maximum rate. Strength loss from Skylab missions are shown in fig. 4.[6] A bicycle ergometer was used as shown on all these missions. Had the loss continued at the rate of the first two missions, it is unlikely that the 84-day crew could have walked off, but a crude locomotor exercise was added to the last mission, and while forces were inadequate, ~1 BW, there was a sharp reduction in loss. This unplanned experiment demonstrates the answer to another question: Why not use other forms of exercise? Unless other exercises provide loads which approximate those usually seen with locomotion on Earth, bone and muscle will atrophy; i.e., any effective locomotor exercise will produce large disturbing forces.

Force loads were never adequate to completely prevent leg muscle and, especially, bone loss on Skylab; but we can now reasonably estimate force levels required to maintain muscle and bone. Required duration of locomotor exercise in space is unknown but will almost certainly require a minimum time of an hour/day. While peak force loads on Skylab were never adequate to maintain strength, the mean workload provided by the cycle ergometer was adequate to maintain cardiorespiratory condition (ability to provide blood and respiration as needed). Since this atrophy is even more rapid than muscle and bone, such "aerobic" devices[1] might provide a stopgap on missions where strength loss is acceptable (e.g., up to 20 days).

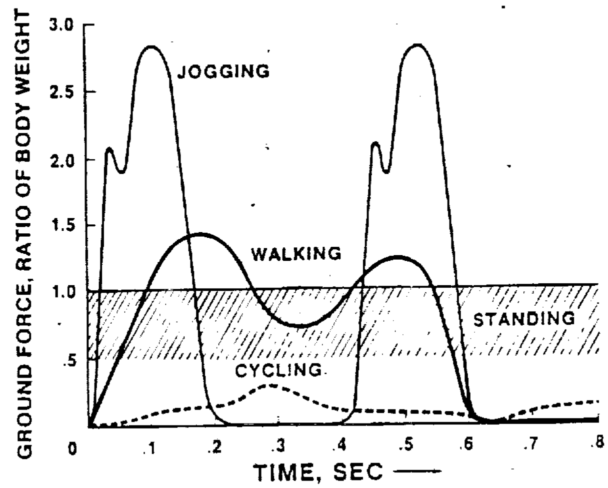


Fig. 2 - Comparison of foot ground forces from several activities. Static or "weight-bearing force" as in standing is small compared to dynamic locomotor forces. Forces from bicycle ergometry are even smaller.

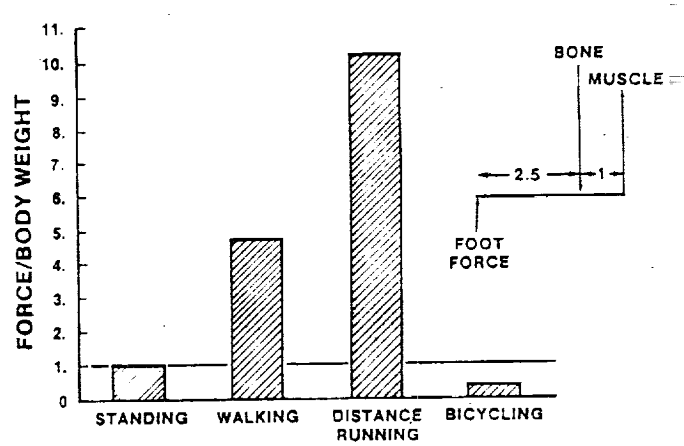


Fig. 3 - Calculated bone forces (leg) from several activities. This lever-arm ratio of the foot and ankle is typical throughout the leg. Note that weight loads are trivial (i.e., it is dynamic muscle forces, not weight, that determine leg muscle and bone strength).

1 Such devices include rowing machine, steppers, and ski machin

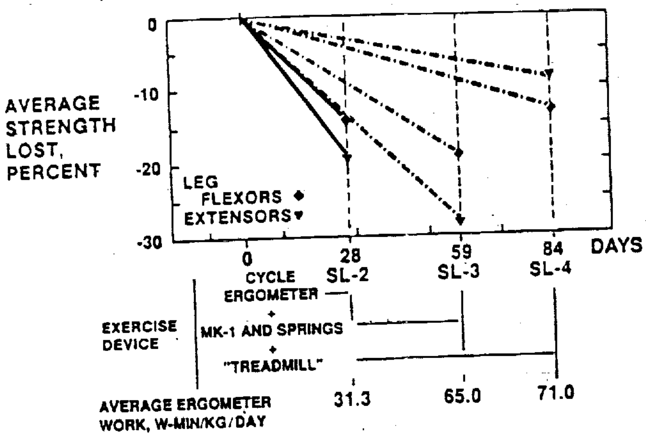


Fig. 4 - Mean postflight leg strength loss on Skylab missions. A cycle ergometer was used on all missions, and the work on it is shown. On the last mission, a "walking" exercise was added and used ~12 mins/day by the crew.

Upper-body exercise will also be required, but the forces are much lower; external forces are small and duration is short.[1]

1 By external forces are meant those transmitted to supporting structure.

Major External Forces and Torques From Exercise:

Vehicle acceleration forces are of primary concern here, and special emphasis will be placed on locomotor forces.

Linear forces: Step rate or fundamental frequency varies with individual, load, grade, mode (walk or run), jogging, and velocity but typically range from ~1 - 2.2 Hz in normal walking to ~2-3 + Hz jogging/running.

FGF are complex and their vector components for moderate walk and fast running are shown in figure 5.[10,11,12,13] Horizontal FGF $(F_{x,y})$[2] are usually small or trivial in relation to vertical forces (Fz), except in running, and Fz will be emphasized here. For a single walking step, it is a double-humped plateau with a maximum of 1-1.3 + BW figure 5A. There is an overlap of foot-ground

2 The conventional biomechanical reference system has been used here: Z = long axis of body, X = anterior - posterior axis, and Y = lateral axis.

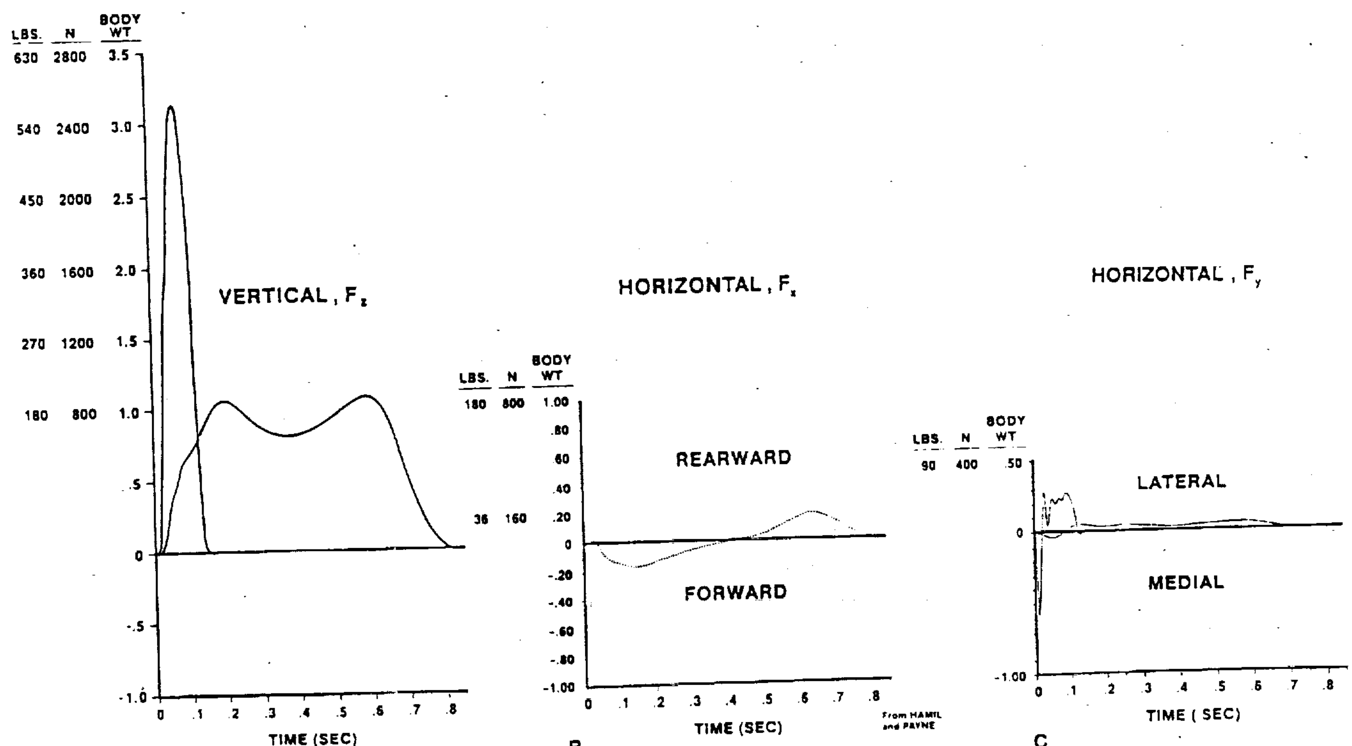


Fig. 5 - Locomotor foot ground force components produced by a moderate walk and fast run simultaneously plotted to an equivalent time and amplitude scale.

contact and FGF in walking (fig. 6) which produces an "M"-shaped complex with peaks that may exceed 1.3 BW. Peak amplitude of these forces vary directly with speed and subject weight. In jogging/running, all foot ground contact is broken each step and FGF goes to zero. The resulting waveform is pulselike and approximates a half sinusoid[1] (fig. 7). Pulse width (ground contact) decreases with overground velocity and may be as short as 100 msec. Again Fz

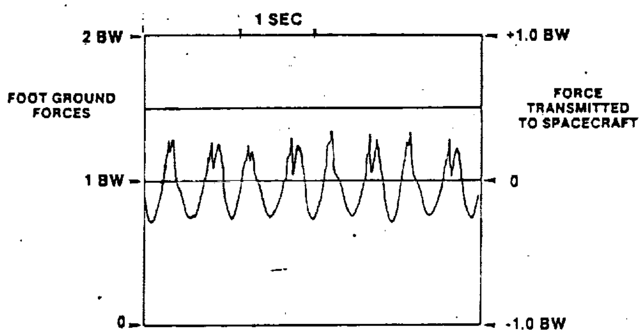


Fig. 6 - Vertical foot ground forces during walking at 3.5 mph (5.6 km-1) by a 203 lb (93 kg) subject recorded from a TM instrumented by the author. In weightlessness, forces transmitted to the spacecraft from a treadmill (as in fig. 8) will be those shown on the right scale albeit with the waveform altered by the system's structural frequency response.

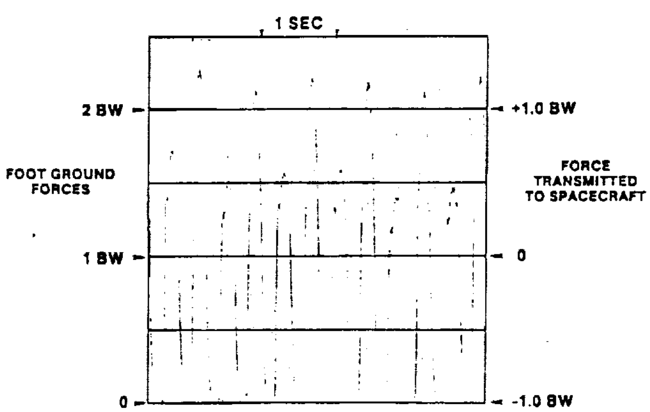


Fig. 7 - Foot ground forces recorded as in figure 6, except same subject is jogging at 6 mph (9.7 km-1). Step asymmetry is common but not to the extent shown in this case.

1 In some "heel-strike" runners, there may be a brief transient at the beginning of each step with a magnitude as large or larger than the usual peak.

amplitudes vary with velocity and subject weight and grade from ~2 to 3 BW in jogging and moderate running.

It has been repeatedly demonstrated that overground and treadmill locomotion are essentially equivalent.[14, 15] To use a treadmill in space, weight must be replaced by approximately constant force and this is currently accomplished by a harness and extended elastic bungee (figure 8). If this force is equal to the subject's one-g BW, then the FGF characteristics described should closely approximate treadmill forces in weightlessness. If the bungees are attached to the treadmill (TM) frame as in figure 8, 1 BW constant force is removed from FGF

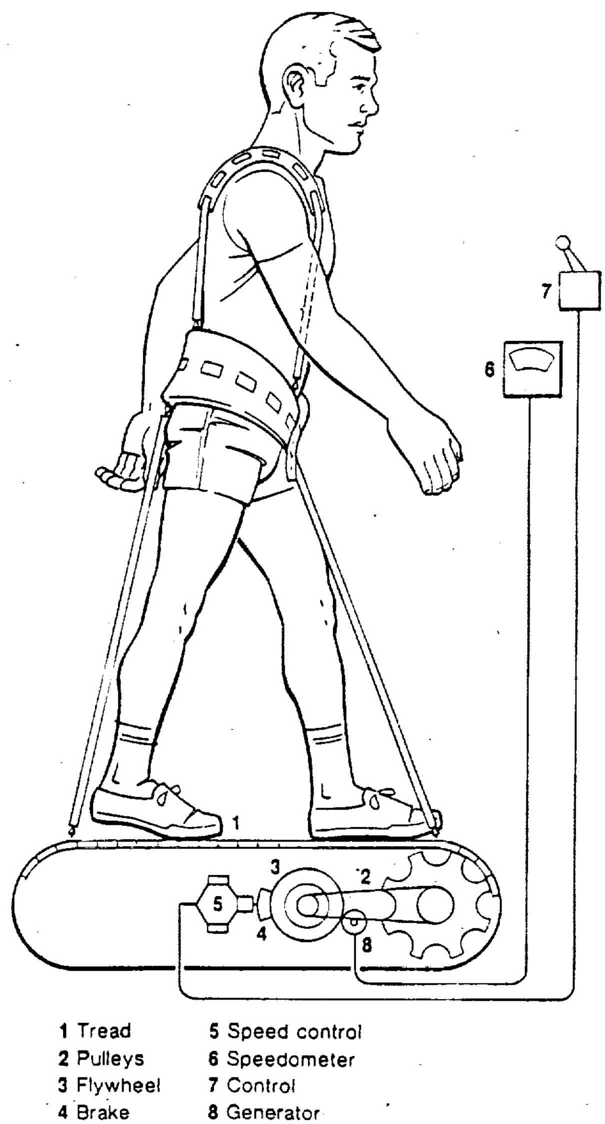


Fig. 8 - Schematic of early Shuttle treadmill. Current unit uses a longer "folded" bungee to reduce force variation with motion.

transmitted to the spacecraft (figs. 4, 5) right side scale; i.e., only inertial components remain.

Torques: Torques are complex, unmeasured and poorly analyzed. There is only room for comment here, not analysis. Kinematics of locomotion produce unbalanced moment arms about all three axes which, with the forces described, produce cyclic torques too complex to describe here. Figure 9 sketches the geometry of this, and table 1 lists some characteristics and first-order approximations of peak torque in one-g. They will be appreciably less in weightlessness.

Forces in cycle ergometry: In seated cycling, pedal forces are typically distorted half sine waves for each foot[1] with a period which is approximately 1/2 that of crank revolution and maximum forces of 60-80 pounds (267-356 N).[16] External ergometer forces and torques have seldom been studied and the data shown here were generously provided by Damon Smith, LMSC. The vertical forces transmitted to ground by the ergometer are shown in figure 10 and their spectra in figure 11. Such forces will be a function of mean ergometer load, pedal technique, and crank speed.

1 There is still controversy over "pull up" forces in the last half of crank revolution but they are small in any event

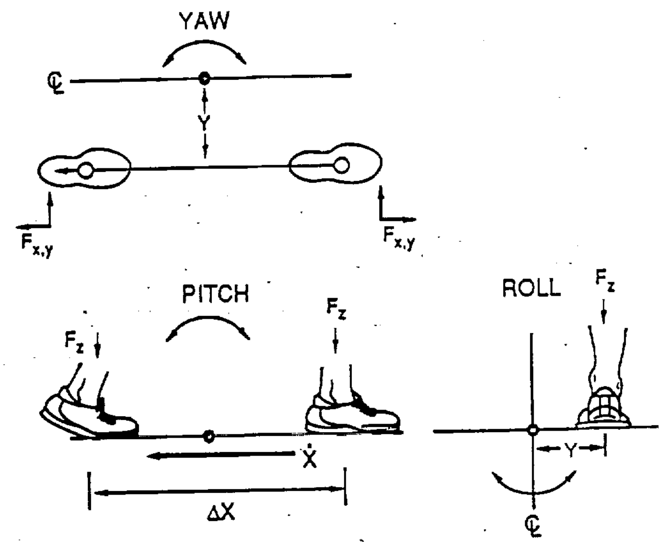


Fig. 9 - Generation of torques on treadmill. Moment arm Y is relatively* fixed, and component forces vary as in figure 5. In the pitch axis, the arm ΔX varies continuously and reverses direction in addition to bearing the force Fz.

* There is some variation in point of force application over the footprint during each step, but this moment arm variation is small.

Table 1. Some Characteristics of Treadmill Locomotor Torques in One g

Axis	Mode	Waveform Approximation	Fundamental Frequency	Estimated Torque Body Wt. X Ins(cm)
Pitch	Walk	sine	SR	12 (30.5)
	Run	1/2 sine pulse*	SR	6 (15.2)
Roll	Walk	bilphasic trapezoid	1/2 SR	5.2 (13.2)
	Run	bilphasic 1/2 sine pulse*	1/2 SR	10 (25.9)
Yaw	Walk	complex	SR	< 1. (2.5)
	Run	1/2 sine pulse of alternating phases	SR	4 (10.2)

Walk = 3 mph, Run = 6 mph, SR = Step Rate
* Pulse width = foot tread contact time

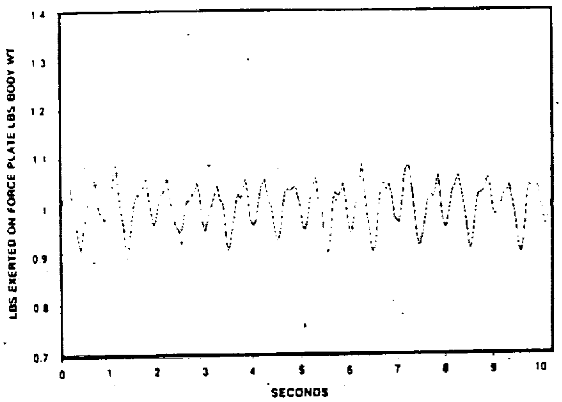


Fig. 10A. Seated, 100 W mean load

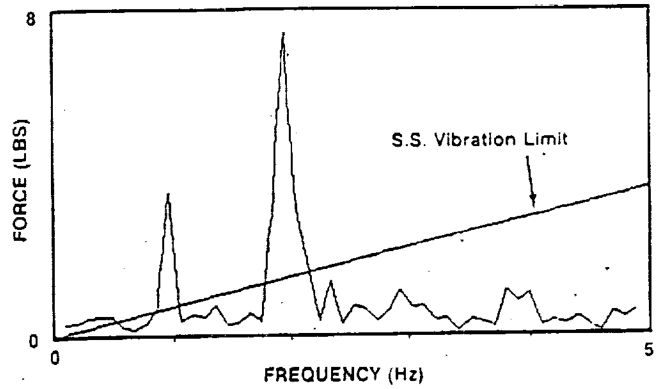


Fig. 11A. Seated, 100 W mean load

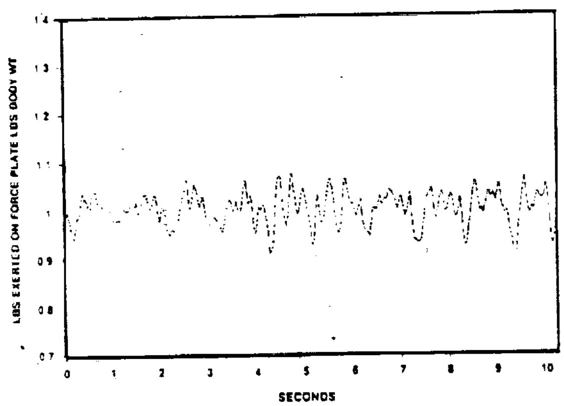


Fig. 10B. Seated, 200 W mean load

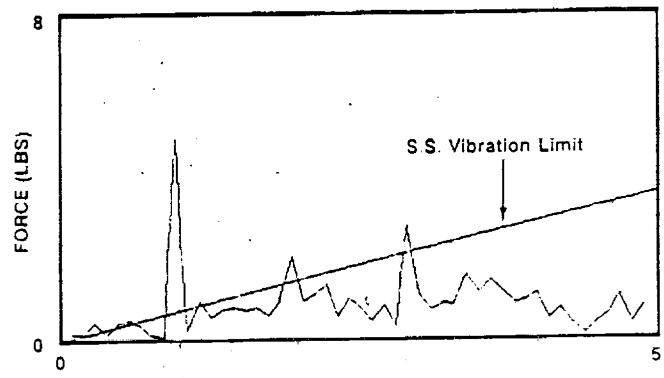


Fig. 11B. Seated, 200 W mean load

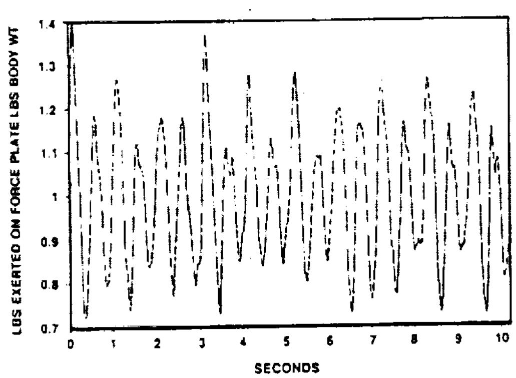


Fig. 10C. Pedal supported, 200 W mean load

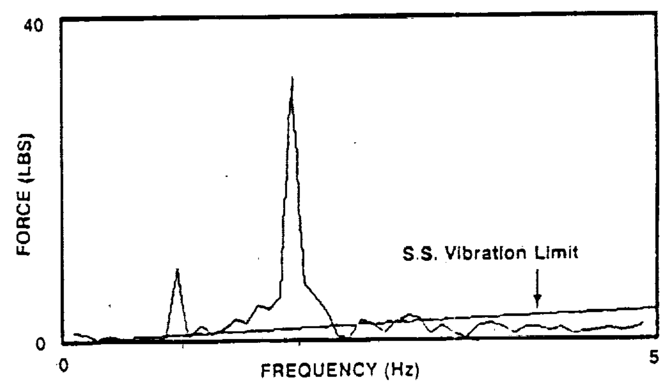


Fig. 11C. Pedal supported, 200 W mean load

Fig. 10 A, B, C - Vertical cycle ergometer-to-support forces. One-hundred-and-eighty pound (81.8 kg) subject in one g at 60 rpm pedal rate. There are large differences in seated versus unseated mode, probably resulting from shifting center of mass. In weightlessness, only dynamic forces would appear external to ergometer.

Fig. 11 A, B, C - Spectral analysis of forces in figure 10. Shift in dominant frequency from A to B probably results from asymmetry in pedal forces, equivalent to step asymmetry seen in figure 7 albeit with different subjects.

If the subject is pedal supported (i.e., "walks the pedals"), then a large weight component will also be present at the pedals, and considerable shifting of center of mass occurs. This is reflected in increased external forces (figure 10C). An equivalent effect can be expected in weightlessness where coupling the rider to the seat will reduce external forces by stabilizing body position. The usual mode in weightlessness is to ride free of the seat. External torques are complex and unmeasured, but estimated values are small. Dynamic external forces in weightlessness should approximate these (i.e., the weight component will be absent).

Other devices that may be used in exercise include weight equivalents, rowing ergometers, climbers, or steppers. Space here does not allow a description or analysis. While large internal forces may be generated, it is the unbalanced inertial loads that will be transmitted to supporting structure. In the rowing machine, which has the largest body mass displacement of these exercises with up to 2 ft translation of body center of gravity (CG), the cyclic rate is relatively low, typically 1/2 Hz, and acceleration is also limited such that cyclic forces of a fraction of BW might be expected.

The foregoing illustrates how little basic measured data we have on shock and vibration from exercise on Earth, and there is essentially none from space. It is or should be an almost trivial matter to attach force elements between machines and structure and document the forces with an accompanying miniature recorder. A word of warning is prompted by in-flight vibration recordings made of exercise to date. To be quantitatively meaningful, exercise conditions must be known or measured. In the case of the treadmill, this would include tread speed, mode, subject mass, equivalent grade, and subject equivalent weight (e.g., bungee force). The latter is particularly important (and yet to be measured) for FGF are directly proportional to it. Without accurate knowledge of exercise conditions, its effects must be treated as qualitative observation.

Current Isolation Means:

A wide variety of shock and vibration isolation systems using springs and mechanical resistance are available. They all function in a manner equivalent to either a resonant circuit, whose frequency is below that of the vibration, or else as a low pass filter. In either case, a compliant connection is involved; and, in the case of the treadmill, this would allow excessive motion without a counterpoise mass since the treadmill weighs less than 100 pounds and would undergo excessively large excursions under the forces of walking and especially running. Active "throw mass" systems could be used theoretically to cancel forces, but they are heavy, complex, and power hungry. Simple calculations show requirements for kilowatts of power for this application.

Proposed Isolation Concept:

In 1989 no solution to this problem was in sight, and there was a clamor against locomotor exercise because of vibration. The author proposed the following system and disclosed it fully in a patent application[17] and partially described it in a NASA TM[18].

If a sufficient counterpoise mass is attached to the TM, it may be used normally[1] with the unit totally isolated from the spacecraft. Such a floating system would eventually contact structure and must be restrained; however, the restraint cannot exceed accelerating force limits imposed on the vehicle.

A schematic for such a system is shown for one axis in figure 12. Mass M_2 is made large enough to be an effective counterpoise to M_1 the moving mass (TM). This allows the subject to run on the TM and produce acceptably small oscillations of it.

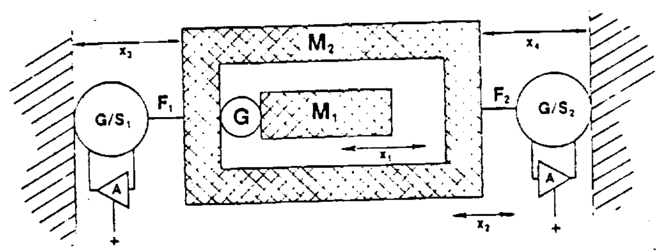


Fig. 12 - Schematic of single-axis isolation system. M_1 is subject and treadmill mass coupled to M_2, a counterpoise mass selected to limit system oscillations induced by the generator, G. This system "floats" between two tethers F_1, F_2 in which their tension or force may be varied by generators sensors $G/S_{1,2}$ which detect any slow alteration in the distances X_1, X_2 and apply limited corrective forces. Small rapid excursion of M_2 caused by generator forces are ignored.

1 Other than running barefoot on concrete, there is always some movement of supporting structure; however, excessive motion can seriously disturb or distort locomotion.

In practical terms, this mass would not simply be ballast, but relatively fixed portions of the vehicle (not primary structure) such as storage lockers, etc., which will provide the mass "for free". It will be calculated to limit TM movement to, say, a few tenths of an inch. Rigid body calculations show that counterpoise masses of ~2000 lbs weight will limit peak displacement to ±.5 in. for a 225-weight equivalent subject over a practical range of walking/jogging. The combined TM and counterpoise mass will be floated in the vehicle and provide an acceptable, totally isolated system; however, air currents and other small pertubations will cause it to drift ultimately into contact with structure. To avoid this, limited counterforces must be applied. There are many ways to do this, ranging from noncontact sensors controlling airjets to magnetic or other fields. A simple scheme is a combined sensor/force generator (SFG) attached to a filament tether. In the example shown, two such SFGs ($G/S_{1,2}$) are used. They operate as follows. A small drift may be detected, and a force F_1 or F_2 will be developed to offset it. This force will have two characteristics; it is relatively constant regardless of shortterm displacements of M_2, and it is limited such that it can never exceed the allowable spacecraft shock and vibration g limits.

There are several simple approaches to such a tether, and two are shown in figures 13A and B. Both methods use a filament and reel that contain an optical position encoder to detect drift. When a drift or slow change in mean position is detected by the OE (which could be analog instead of digital as shown), it will cause an increase in DC motor current, I, which produces a torque increase by the motor and reel and an applied force, F, in a direction to correct the drift and restore the mass to its usual neutral position. The forces required are compatible with available miniature DC motors. An alternative is shown in figure 13B in which a small brushless AC motor runs continuously and, after torque multiplication by a gear train, is coupled to the reel and filament through a variable torque clutch. In the same way as above, errors in position produce changes in DC current to the clutch and corrective increases in torque and force F. The error detector/current generator will contain frequency selective components such that only slow drifts are responded to and short-term position changes or oscillations are ignored. If desired or necessary, error rate damping may be incorporated. If multiple units are interconnected, it may be desirable to coordinate their outputs through the computers shown.

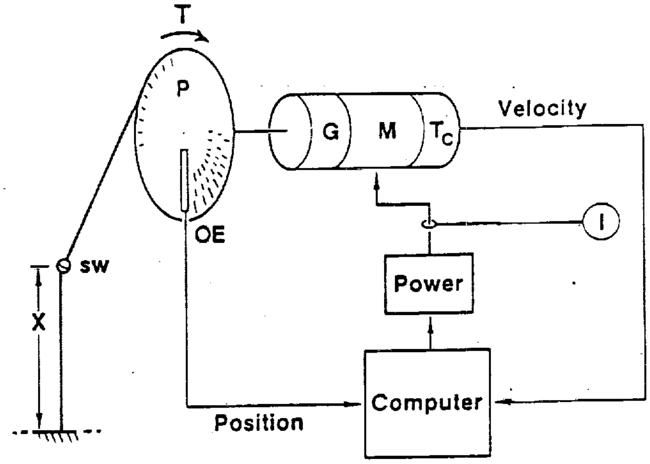


Fig. 13A - A generator sensor tether to maintain a fixed position as determined by tether length, X. SW is a swivel feeding tether onto a reel with a digital position scale, P. An optical encoder OE picks up the position and applies it to the computer which controls a current generator. Any error will produce a current, I, to a motor, M, which through a gear train, G, generates a torque through P, applying a corrective force to the tether. A tachometer, T_c, provides rate feedback to the computer.

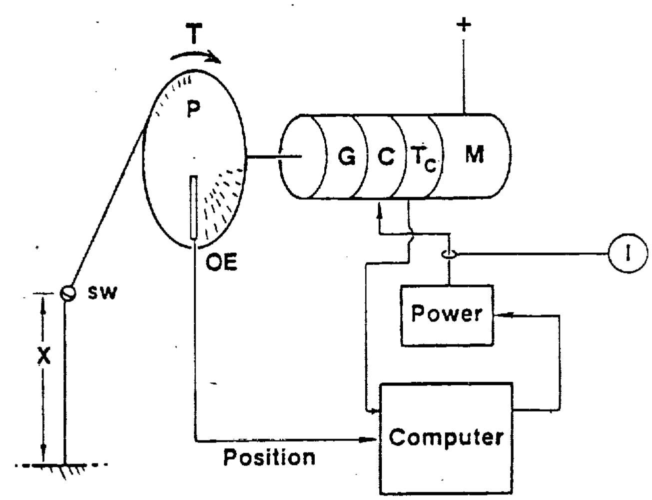


Fig. 13B - As A, except the motor runs M continuously. Corrective forces are applied through the clutch, c, whose torque is a function of input current, I, and is limited.

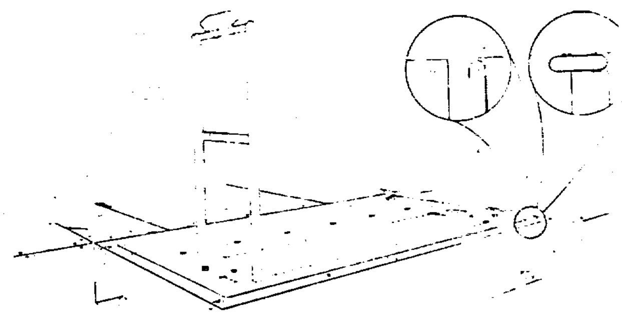
A member of these active tethers will be required to maintain position, and the number and arrangement will be a function of the designer's ingenuity. Figure 14 is a sketch of a space station treadmill isolated in this fashion.

Implementation:

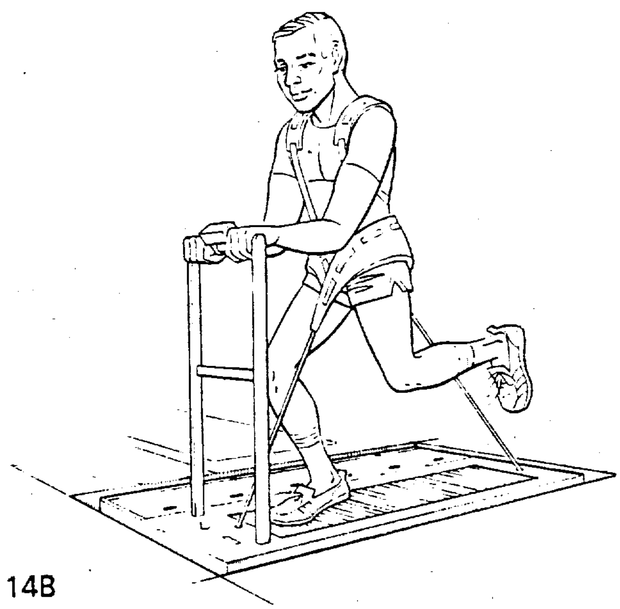
A single tether was fabricated per figure 13, and its force/displacement characteristics are shown in figures 15 and 16.[19] These tethers could be very small, a few cubic inches, and consume a few watts of power. No further pursuit of this particular embodiment has been pursued. Armentrout[20] proposed a variant of passive tethers described in the patent disclosure which provides a sharper force attenuation with frequency while still providing adequate drift restoration. This is being supported by the Johnson Space Center (JSC). Smith[21] proposed several methods of increasing the effective counterpoise mass by more efficient active means, and this is also being supported by JSC with a cycle ergometer isolated by this means scheduled for IML-1.

Conclusion:

There is little doubt that the method proposed, or some variant of it, will be adequate to allow exercise without disturbing a micro-g environment. This is far from solving the entire problem, for, looking at figure 1, other crew activity or even an animal in a cage will cause disturbances exceeding current limits. Rather than trying to quiet the entire vehicle, a virtually impossible task, the sensitive facilities could be isolated — possibly by some of the means described and others which were disclosed. Even if the attenuation were only partial, it would make isolation of sources much simpler. Such approaches can make it possible to have material sciences undisturbed on manned missions.

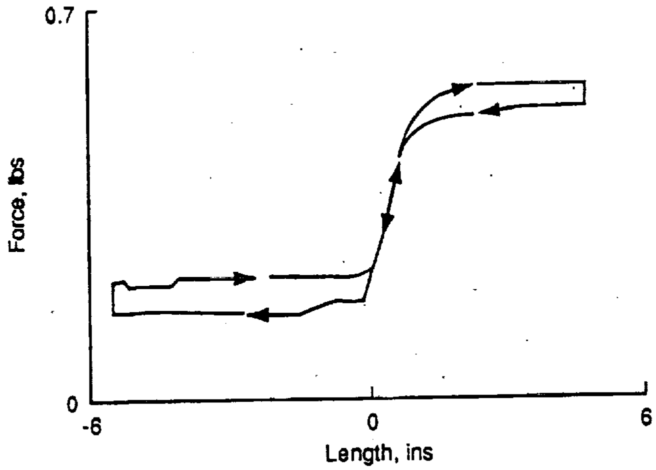


14A

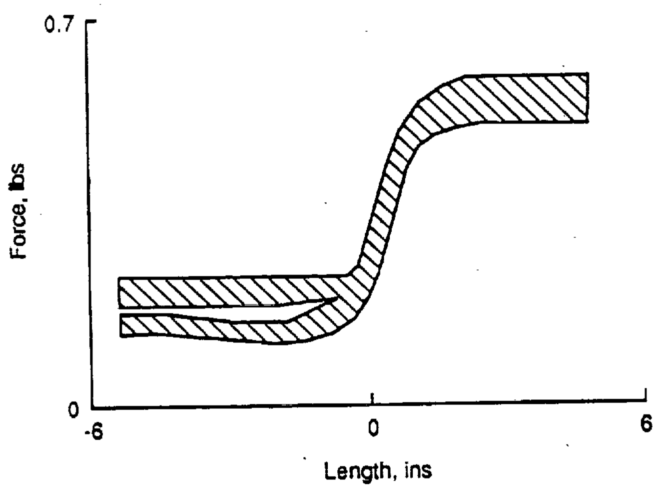


14B

Fig. 14A and B - Sketch of a space station treadmill designed by the author using the isolation concept described. Tethers are not shown. Conceptual latches are shown in 14A and would be used when rigidity of the system is desired. The TM has been attached to a series of heavy lockers to provide a counterpoise mass.



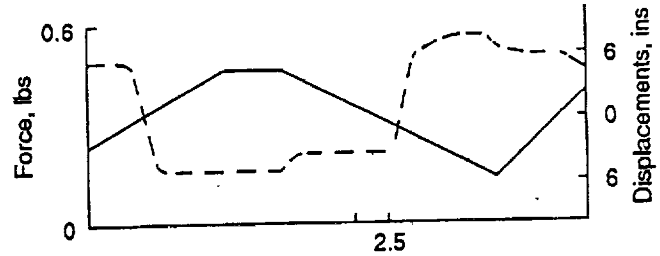
A. Linear Extension Retraction



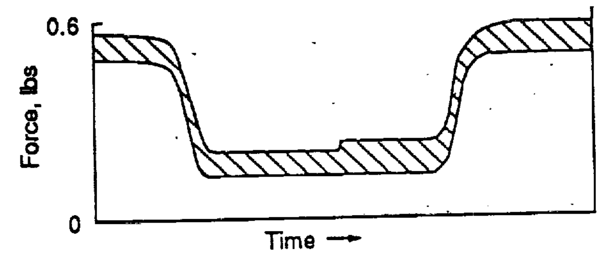
B. With 0.3 in superimposed motion

X101170M

Fig. 15 - Performance of active tether design shown in figure 13B. In A, simple linear, reciprocal displacement was used. Hysteresis is caused by clutch characteristic. Force was limited to 0.5 lb (2.2 N). In B, the displacement had a superimposed oscillation. An advantage over passive elastic elements is that full restoring force is developed with small errors resulting in more rapid corrections.



A. Force with linear motion



B. With 0.3 in superimposed motion

X101171M

Fig. 16 - As in figure 15, except displacement was a trapezoidal function of time shown by solid line in A with its scale on the right.

BIBLIOGRAPHY

1. Owens, R.G., et al., Integration of a Microgravity Isolation Mount Within a Columbus Single Rack, I AF Paper IAF 89-428, 1989

2. Thornton, W., Rationale for Exercise in Spacecraft, Conf. Proc., Spaceflight Deconditioning and Physical Fitness, pp. 13-81, NASA HQ, 1981

3. Thornton, W., Work, Exercise and Spaceflight: I. Operations, Environment and Effects of Spaceflight, III. Exercise Devices and Protocols, Workshop on Exercise Prescription for Long Duration Spaceflight, 1989, pp - 19-31, NASA HQ, NASA-CP 3051, 1989

4. Cavanaugh, P.R., and LaFortune, M.A., Ground Reaction Forces in Distance Running, Jour. Biomechanics, 13, pp.397-406, 1980

5. Mann, R.A., In Biomechanics, W.B. Saunders, U.S., 1982

6. Thornton, W.E. and J.A. Rummel, Muscular Deconditioning and Its Prevention in Spaceflight in Biomedical Results from Skylab, NASA-SP-377, U.S. GPO, 1976

7. Vogel, J.M., Bone Mineral Measurement: Skylab Experiment MO-78, Acta Astronautica, 2; pp. 129-39, 1975

8. Whedon, G.D., et al., Mineral and Nitrogen Metabolic Studies: Experiment MO-71 in Biomedical Results from Skylab, NASA SP-377, U.S. GPO, 1976

9. Saltin, et al., Response to Exercise after Bed Rest and Training, Circulation 38, Suppl 7, pp. 1-78, 1968

10. Okumura, S., et al., *Analysis of the Resultant Floor Reaction Forces in Normal Gait in Biomechanics VIIIB*. Proc. of Eighth Int.. Congr. of Biomechanics, Human Kinetics Publishers, pps. 1198-1204.

11. Lanshammar, H., and Strandberg, L., Horizontal Floor Reaction Forces and Heel Movements During the Initial Stance Phase, Ibid., pp. 1123-1128.

12. Payne, A.H., Foot to Ground Contact Forces of Elite Runners. Ibid., 746-753

13. Bates, B.T., et al., Identification of Critical Variables Describing Ground Reaction Forces During Running, Ibid., pp. 635-640.

14. McMiken, D.F. and J.T. Daniels, Aerobic Requirements and Maximum Aerobic Power in Treadmill and Track Running. Med Sci Sports 8, pp. 14 -, 1976

15. Thornton, W., Comparison of Energy Costs of Active and Passive Treadmills, JSC Internal Report, 1981

16. Cavanaugh, P.R. and D.J. Sanderson, The Biomechanics of Cycling, in Science of Cycling, Human Kinetics Pubs., U.S., 1980

17. Thornton, W., Means for Isolation of Shock and Vibration in Spacecraft, Disclosure MSC-21541-1 to JSC Office of Patent Counsel, 1989

18. Thornton, W.E., A Method of Isolating Treadmill Shock and Vibration on Spacecraft, NASA TM 100474, April 1989

19. Silvas, H.S., Feasibility Demonstration of Force-Limiting Active Tether, Final Report by Southwest Research Institute., SWRI Proj. 15-3143, Jan. 1990

20. Armentrout, R.W, Dynamic Analysis of Space Station Exercise Treadmill on Passive Low-Frequency Isolator, McDonnell Douglas S.S.C. - S.S.D. Rept A9G-J749-STN-TR-RWA-9000242, Jan. 1991

21. Smith, D., Isolated Shuttle Ergometer, Lockheed Missile & Space Co. Proposal, Jan. 1991

N92-28450

Space Station Freedom Microgravity Environment Requirements and Assessment Methods

International Workshop on Vibration Isolation Technology
Cleveland, Ohio
April 23-25, 1991

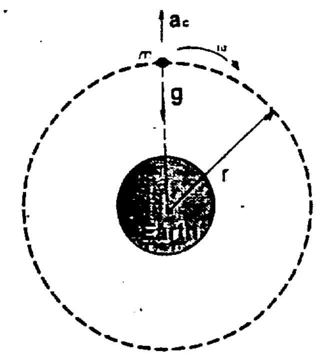
Philip Bogert
NASA SSFPO, Reston, VA

	Overview of Presentation	Microgravity Requirements Clarification	
		Kevin Schaefer/MSU-1 (703) 487-7088	Date: 4/2/91 Page: 9

- Introduction and Program Status
- Space Station Freedom Microgravity Requirements
- Quasi-Steady Assessment Techniques
- Low Frequency Vibration Assessment Techniques
- Vibroacoustic Assessment Techniques

Introduction

- **Gravitational Force**
  - The gravitational force on a body orbiting at 200 miles is only 10% less than the gravitational force it would experience on the surface of the Earth



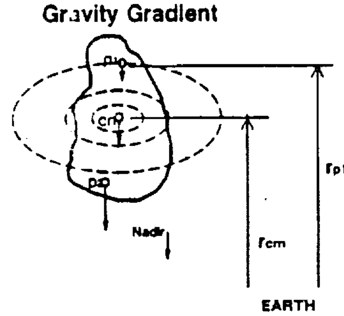
- **"Zero" Gravity**
  - The net acceleration on a *point* in a circular orbit is zero

- **Additional External Forces**
  - External forces add to the free body diagram and result in extremely small accelerations known as "microgravity"

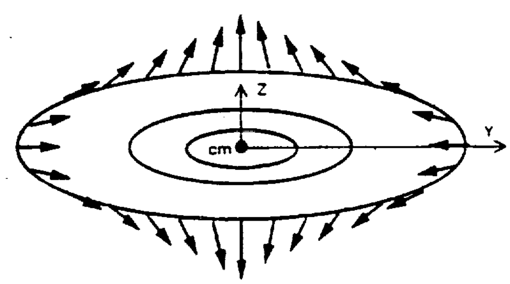
3

Quasi-Steady Accelerations

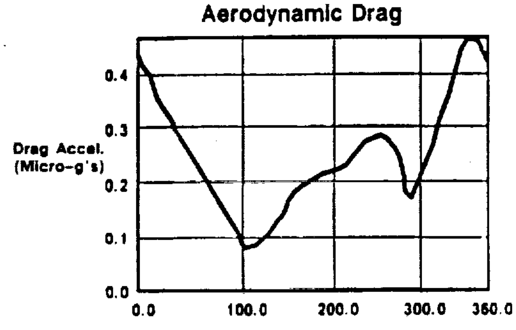
Gravity Gradient



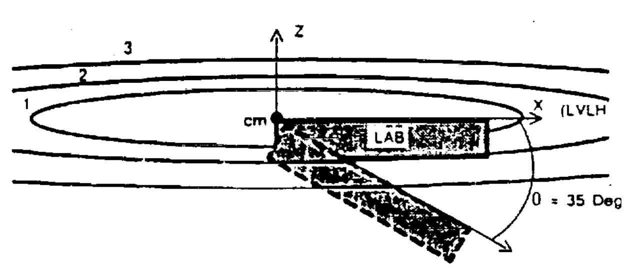
Gravity Gradient with Angular Rotation



Aerodynamic Drag



Spacecraft Torque Equilibrium Attitude



5

Microgravity Environment Definition

- Quasi-Steady Accelerations
  - Gravity Gradient
  - Rotational Forces
  - Aerodynamic Drag
  - Spacecraft Torque Equilibrium Attitude
- Transient Accelerations Determined By
  - Structural modes and frequencies of vibration
  - Applied forcing functions

Program Status	Microgravity Requirements Clarification	
	Kevin Schaefer/MSU-1 (703) 487-7088	Date: 4/2/91 Page: 9

Program Status

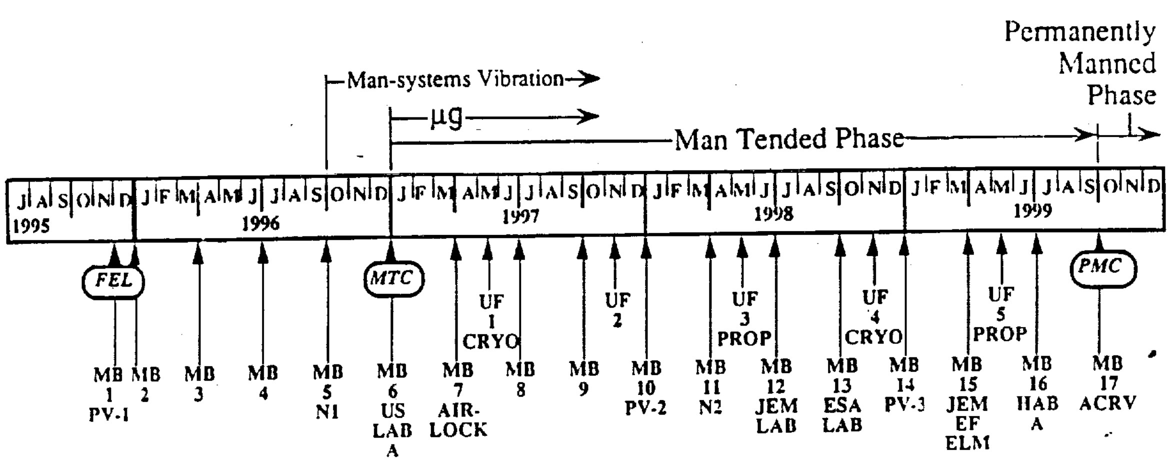
- Updating requirements to reflect restructuring
- Calculating quasi-steady environment for restructured station at various utilization stages
- Assembling catalog of all dynamic disturbing functions
- Starting dynamic analysis of restructured Station
- Sub-allocation of requirement to WPs /Partners by fall 1991
- Allocation basis for isolation strategy

Space Station Freedom Microgravity Requirements Clarification

SSFP Microgravity Workshop
NASA SSFPO, Reston, Virginia
April 2-3, 1991

Kevin Schaefer
Space Station freedom
Program Office /MSU-1
(703) 487-7088

- Requirements apply when function needed
- Man-systems requirements apply after Node 1 arrives
- μg requirements apply after Lab A arrives



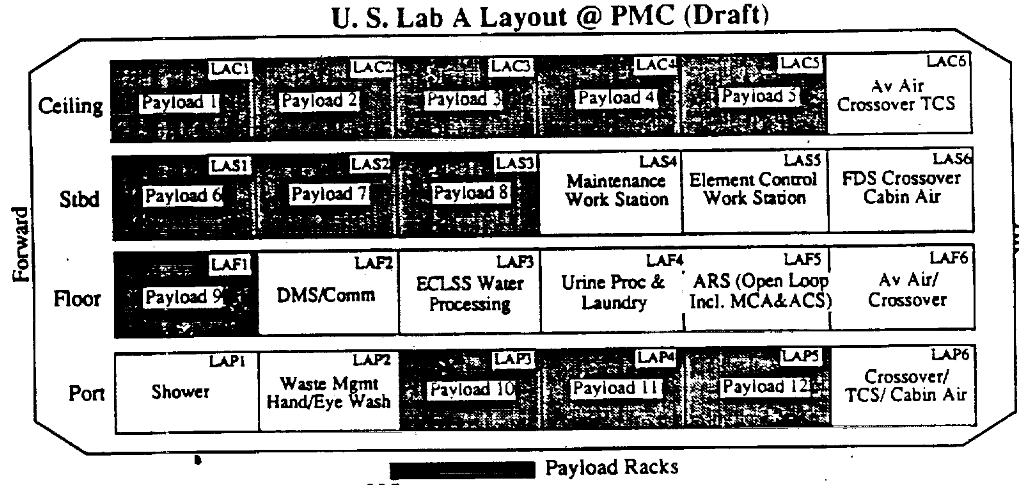
	µg Quiescent Periods	Microgravity Requirements Clarification
		Kevin Schaefer/MSU-1 (703) 487-7088 — Date: 4/2/91 Page: 6

- At least 30 continuous days; 180 days/year total
- Quiescent periods start after reboost
- As the orbit decays, atmospheric drag increases
- Quiescent periods end when drag shrinks µg envelope
- Minimal µg operations during Shuttle visits

	µg Locations	Microgravity Requirements Clarification
		Kevin Schaefer/MSU-1 (703) 487-7088 — Date: 4/2/91 Page: 7

- 50% of payload racks in each pressurized module
  - •• 6/12 racks at MTC; ≈22/44 racks at PMC with International Partners
- 50% of JEM Exposed Facility payload locations (5)

U. S. Lab A Layout @ PMC (Draft)



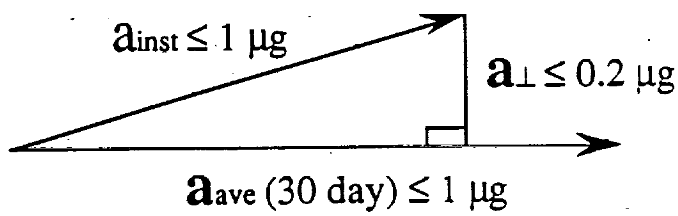
Quasi-steady Acceleration

Microgravity Requirements Clarification

Kevin Schaefer/MSU-1
(703) 487-7088

Date: 4/2/91
Page: 8

- Magnitude of residual acceleration $\leq 1\ \mu g$
- Acceleration must be stable relative to crystal growth plane
- a_{inst} cannot vary significantly from 30 day a_{ave}
- Perpendicular acceleration $\leq 0.2\ \mu g$



- Delete conflicting requirement that $a_{drag} \leq 0.3\ \mu g$

Vibration

Microgravity Requirements Clarification

Kevin Schaefer/MSU-1
(703) 487-7088

Date: 4/2/91
Page: 9

- **2 sets of requirement curves: broadband and narrowband**
  - Format and techniques similar to EMI
- **Lower curve: mechanically induced vibrations and nominal crew activity**
  - Fans, pumps, exercise equipment, latches , and vents
- **Upper curve: off nominal crew motion induced vibrations**
  - Push-offs, EVA, IVA, servicing operations
- **Man-systems requirements:**
  - Outside quiescent period
  - Entire internal pressurized volume
  - Orbiter docking, thruster firings, planned maintenance

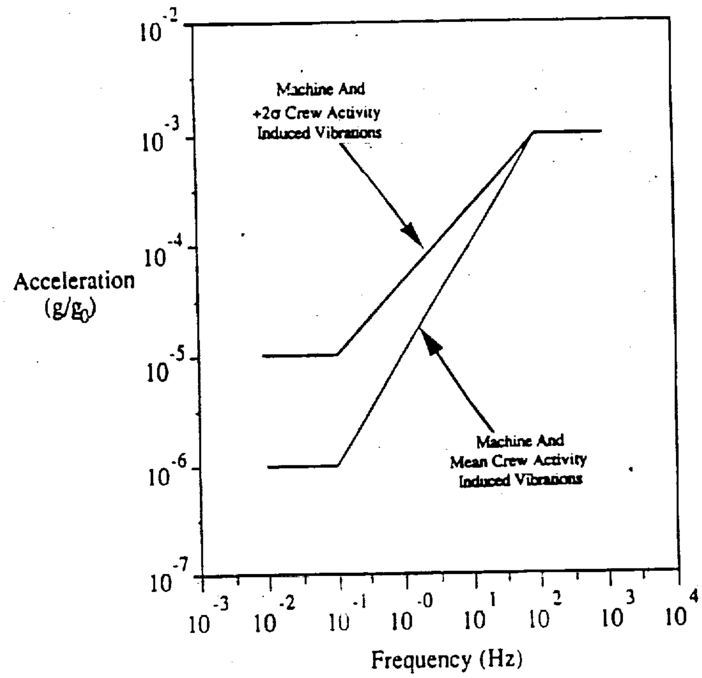
Narrowband Vibration

Microgravity Requirements Clarification

Kevin Schaefer/MSU-1
(703) 487-7088

Date: 4/2/91
Page: 10

- Limits amplitude of acceleration at different frequencies



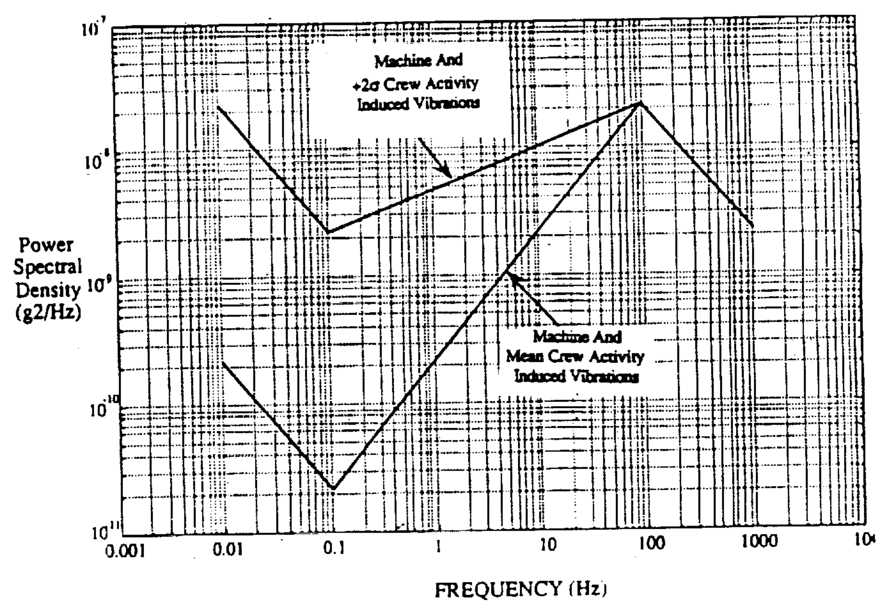
Broadband Vibration

Microgravity Requirements Clarification

Kevin Schaefer/MSU-1
(703) 487-7088

Date: 4/2/91
Page: 11

- Also known as "power spectral density"
- Limits total acceleration in each 3rd octave frequency band



	Vibroacoustic Control Plan (VCP)	Microgravity Requirements Clarification	
		Kevin Schaefer/MSU-1 (703) 487-7088	Date: 4/2/91 Page: 12

- Introduce the VCP into the program
- VCP will provide a mechanism for controlling SSF-wide vibration
- Vibroacoustic Control Plan will include:
  - •• Major allocations for systems and elements
  - •• Standard sub-allocation methods
  - •• Verification and testing techniques
  - •• Standard analysis techniques
  - •• Generic equipment design techniques
- Baseline VCP with a separate CR

SPACE STATION FREEDOM QUASI-STEADY MICROGRAVITY ASSESSMENT METHODS

SSFP - MICROGRAVITY WORKSHOP
NASA SSFPO, RESTON, VIRGINIA
APRIL 2, 1991

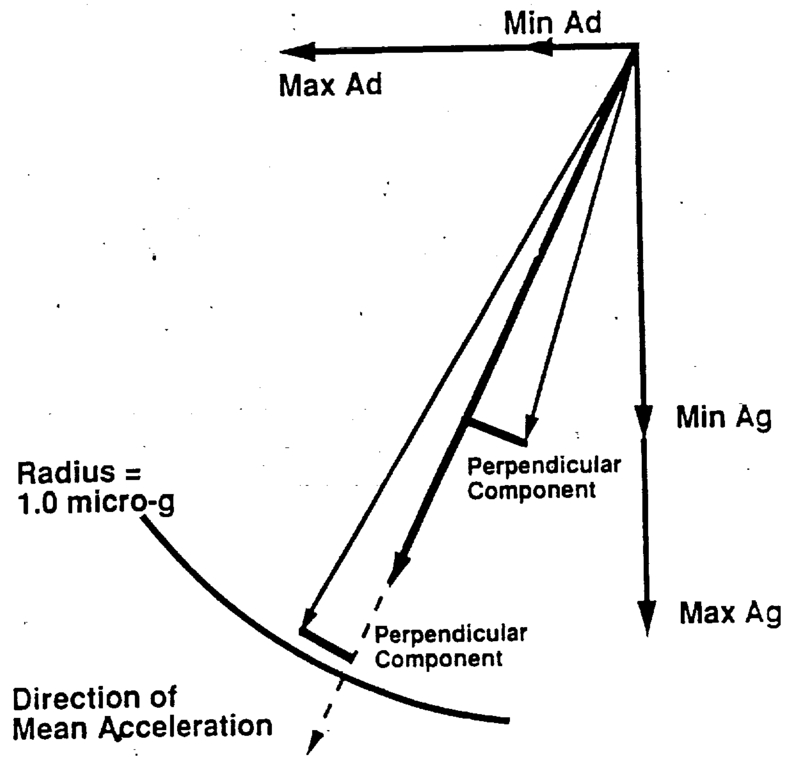
Richard Chipman
Grumman SSEIC
Flight Mechanics Performance
(703) 438-5706

The Effects of Drag on Perpendicular Component

Quasi-steady Micro-gravity

Richard Chipman / SSEIC Flight Mechanics
(703) 438-5706

Date: 4/2/91
Page: 6

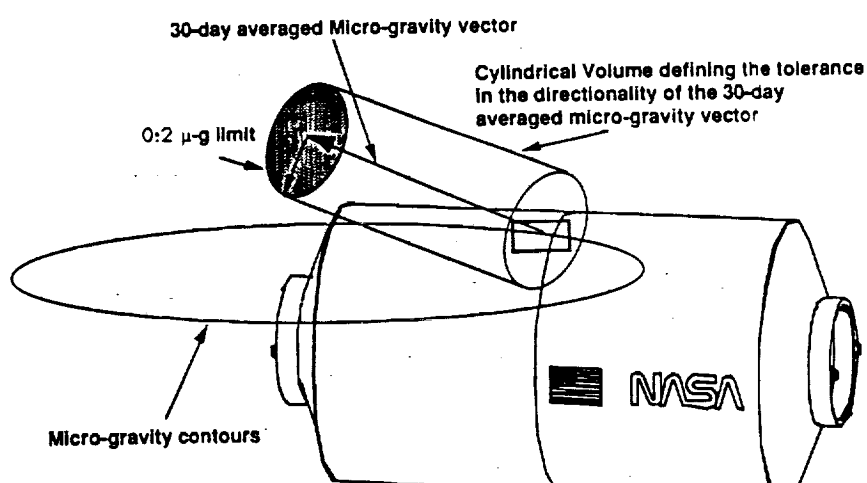


Perpendicular μ-g Component

Quasi-steady Micro-gravity

Richard Chipman / SSEIC Flight Mechanics
(703) 438-5706

Date: 4/2/91
Page: 5



- Gravity-Gradient
- Aerodynamics
  - Solar Activity Cycle
  - Seasonal & Diurnal Variations
  - Altitude
  - Orbit Nodal regression
  - Solar Flux Dynamics (daily)
  - Geomagnetic Index Dynamics (hourly)
  - Thermospheric winds
- Station Attitude Drift Rates
- Magnetic Torque
- Non-circular orbit
- Thermal Flexure
- Continuous venting (resistojet)
- Articulation Dynamics

Sources of Disturbances

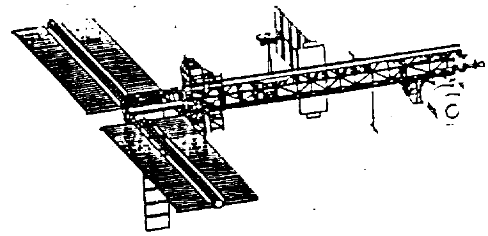
- Gravity-gradient accelerations:
  - due to LAB center of mass being offset from total SSF center of mass
  - TEA effect causing misalignment of LAB axis of symmetry from the major axis of the gravity gradient ellipsoid

- Aerodynamic drag on the station surface area

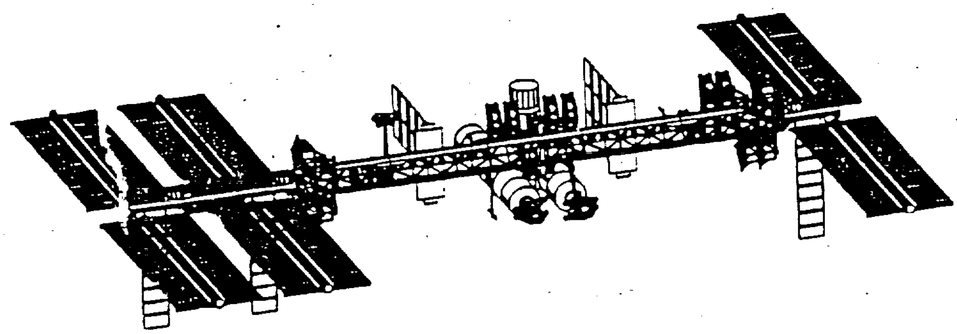
- Rotational accelerations due to deviations in the station attitude rate from orbital rate

Representative Stages of Restructured Baseline

Richard Chipman / SSEIC Flight Mechanics
(703) 438-5706



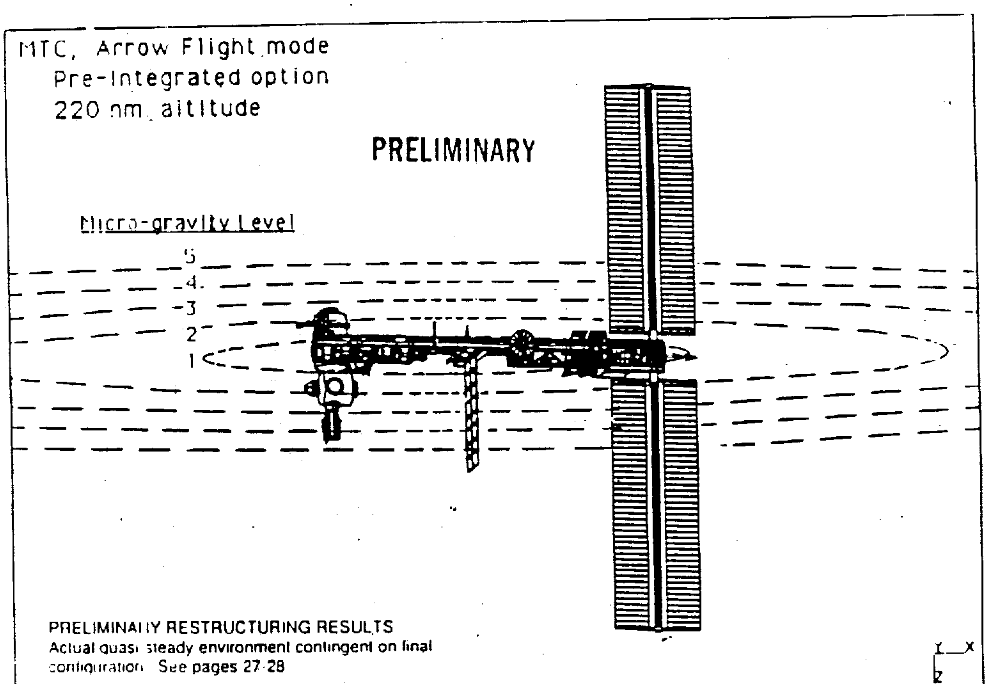
MTC



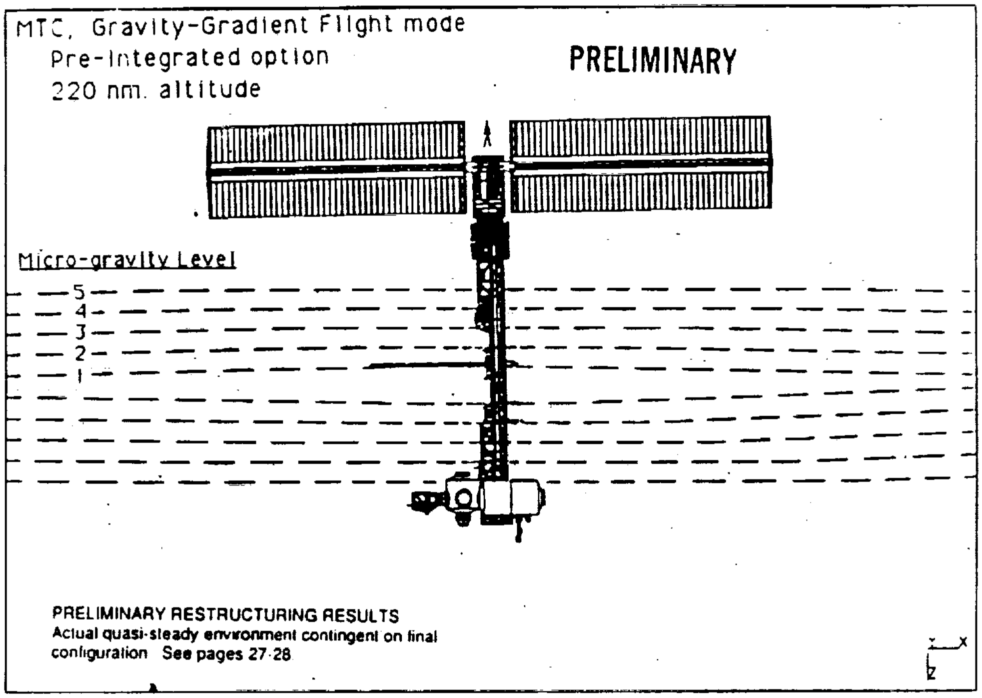
PMC

MTC Arrow Flight Mode

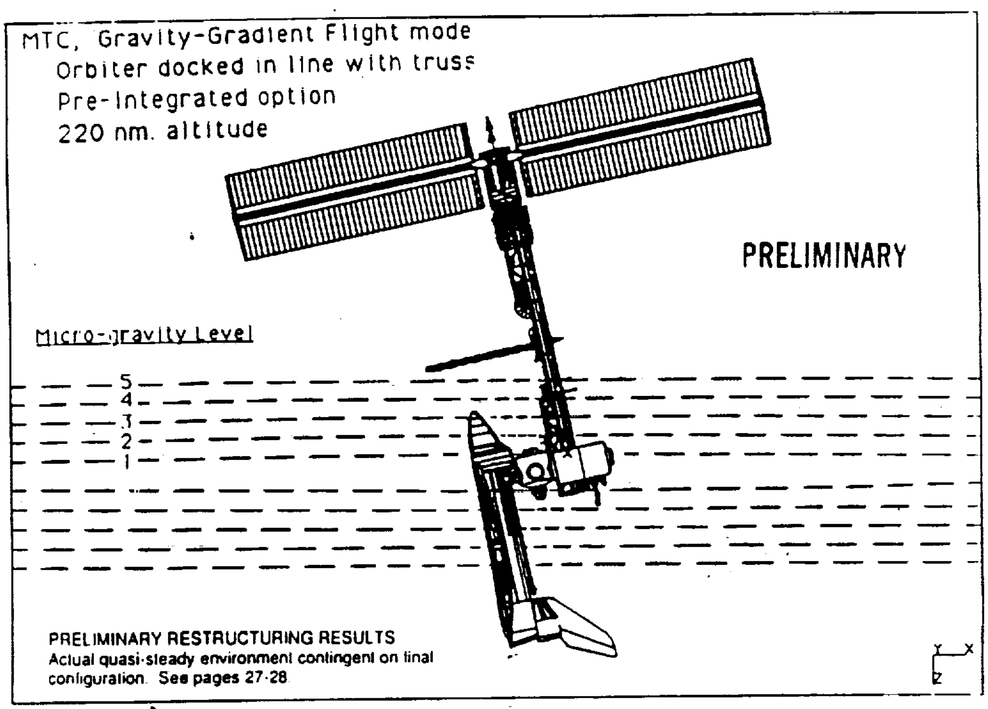
Richard Chipman / SSEIC Flight Mechanics
(703) 438-5706



	MTC Gravity Gradient	Quasi-steady Micro-gravity	
		Richard Chipman / SSEIC Flight Mechanics (703) 438-5706	Date: 4/2/91 Page: 19



	MTC Gravity Gradient with Orbiter Docked	Quasi-steady Micro-gravity	
		Richard Chipman / SSEIC Flight Mechanics (703) 438-5706	Date: 4/2/91 Page: 20



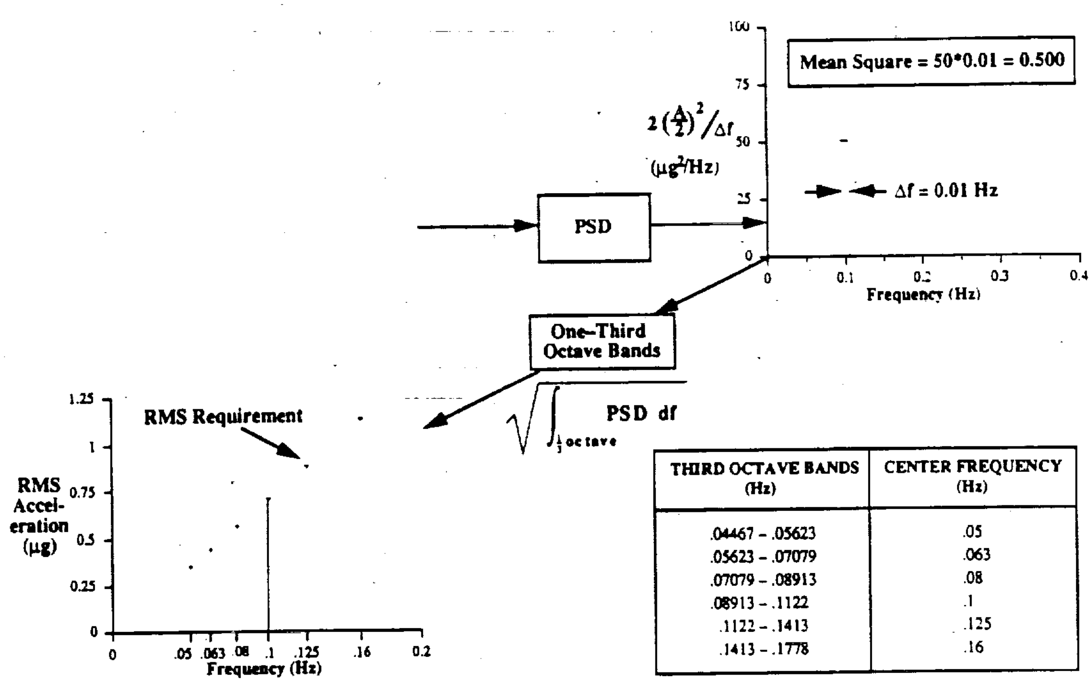
Suggested Improvements for PMC

Richard Chipman / SSEIC Flight Mechanics
(703) 438-5706

- Static performance at PMC will be improved to meet current specifications
- Goal is to meet requirement micro-g levels in at least 50% of user racks in all Labs
- Minor modifications to configuration are being explored to vertically align CP, CG and Lab centers
  - System approach being taken
  - Study objectives being formulated
- Study to determine best system solution to be concluded by 31 July, 1991

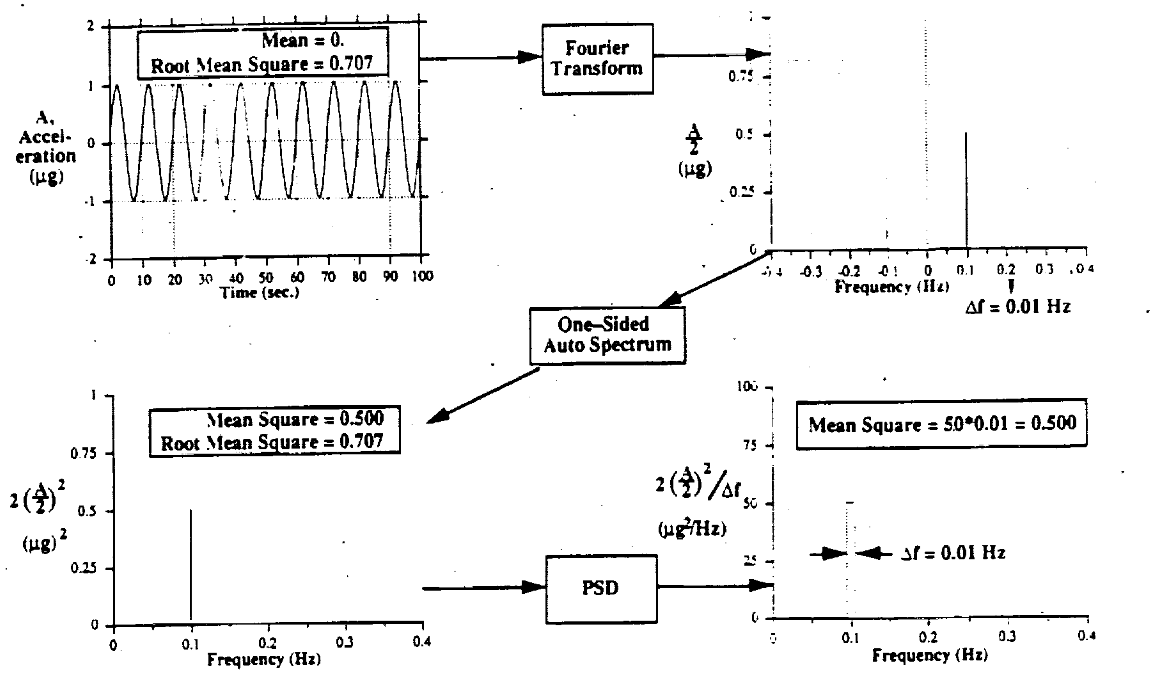
TERMINOLOGY

Steve Del Basso/Grumman
(703) 438-5674



THIRD OCTAVE BANDS (Hz)	CENTER FREQUENCY (Hz)
.04467 – .05623	.05
.05623 – .07079	.063
.07079 – .08913	.08
.08913 – .1122	.1
.1122 – .1413	.125
.1413 – .1778	.16

SSEIC	**TERMINOLOGY**	Dynamic Assessment Methods	
		Steve Del Basso/Grumman (703) 438-5674	Date: 4/2/91 Page:



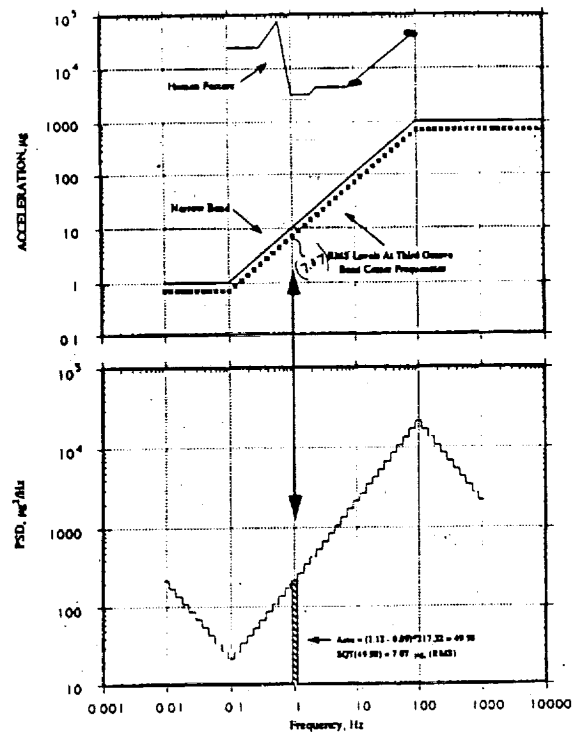
SSEIC	**DYNAMIC REQUIREMENTS**	Dynamic Assessment Methods	
		Steve Del Basso/Grumman (703) 438-5674	Date: 4/2/91 Page: 7

BROAD BAND REQUIREMENT DERIVATION

- Environment is a combination of periodic, transient, and random disturbance sources.
- Root-mean-square acceleration levels are an appropriate way of characterizing such an environment.
- Define allowable RMS levels at a finite set of discrete frequencies from the narrow band requirement.
- Define this finite set of frequencies as the center frequencies of the one-third octave bands.
- Formulate a Power Spectral Density requirement on this basis.

	Dynamic Assessment Methods	
DYNAMIC REQUIREMENTS	Steve Del Basso/Grumman (703) 438-5674	Date: 4/2/91 Page: 8

MICROGRAVITY ACCELERATION REQUIREMENTS



	Dynamic Assessment Methods	
DYNAMIC REQUIREMENTS	Steve Del Basso/Grumman (703) 438-5674	Date: 4/2/91 Page: 9

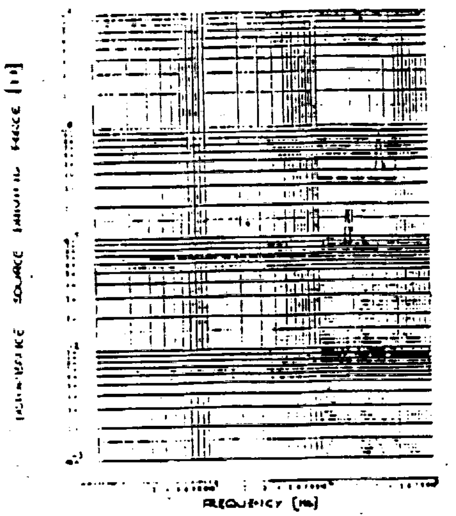
BROAD BAND REQUIREMENT DERIVATION

- **Acceleration Bounds:**

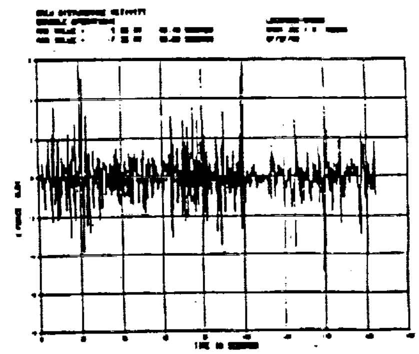
11220.0000 -	1122.0000 Hz	2235.7 μg (rms)
1122.0000 -	112.2000 Hz	2235.7 μg (rms)
112.2000 -	11.2200 Hz	1159.5 μg (rms)
11.2200 -	1.1220 Hz	115.9 μg (rms)
1.1220 -	0.1122 Hz	11.6 μg (rms)
0.1122 -	0.0089 Hz	2.3 μg (rms)

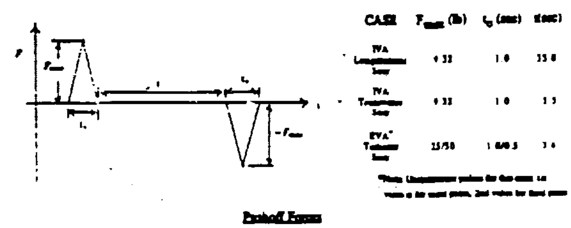
- **One-Third Octave Bands:**
  - One-Third octave bandwidths are defined as: $f_u=2^{1/3} * f_l$
  - Gives sufficient resolution to spread fundamental modes response into different bands.
  - Shorter bandwidths, e.g. one-tenth octave bands, would yield higher bounds because of the increase in the number of center frequencies at which assessments would be made.
  - Provides commonality with human factor requirements.

	ASSESSMENT METHODS <15 Hz	**Dynamic Assessment Methods**
		Steve Del Basso/Grumman (703) 438-5674 — Date: 4/2/91 Page: 14



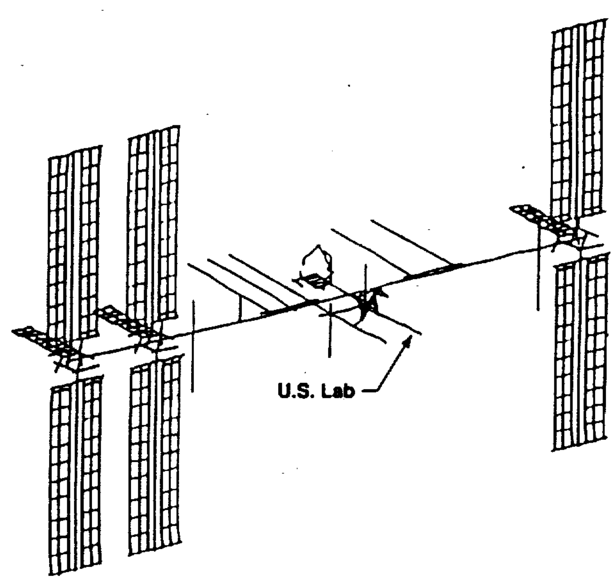
Freon Pump Package - Interface Driving Force Spectrum



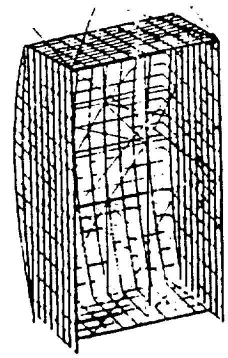


	ASSESSMENT METHODS <15 Hz	**Dynamic Assessment Methods**
		Steve Del Basso/Grumman (703) 438-5674 — Date: 4/2/91 Page: 17

Preliminary Loads Finite Element Model

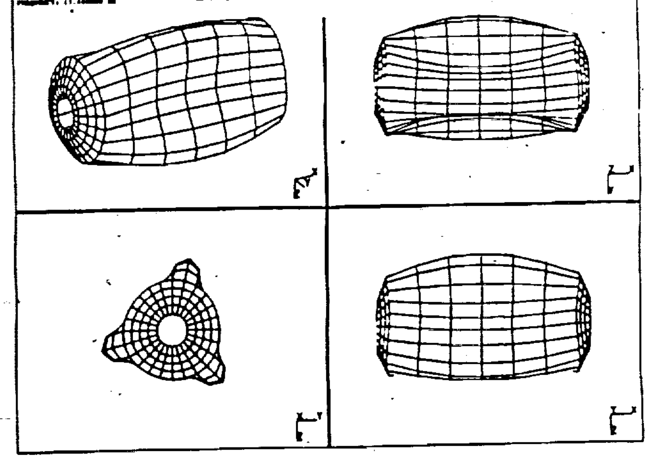


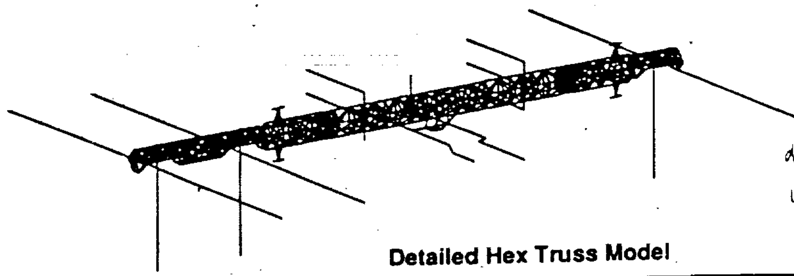
SSEIC	ASSESSMENT METHODS <15 Hz	Dynamic Assessment Methods	
		Steve Del Basso/Grumman (703) 438-5674	Date: 4/2/91 Page: 18



Detailed Rack Model

Shell Model of Module

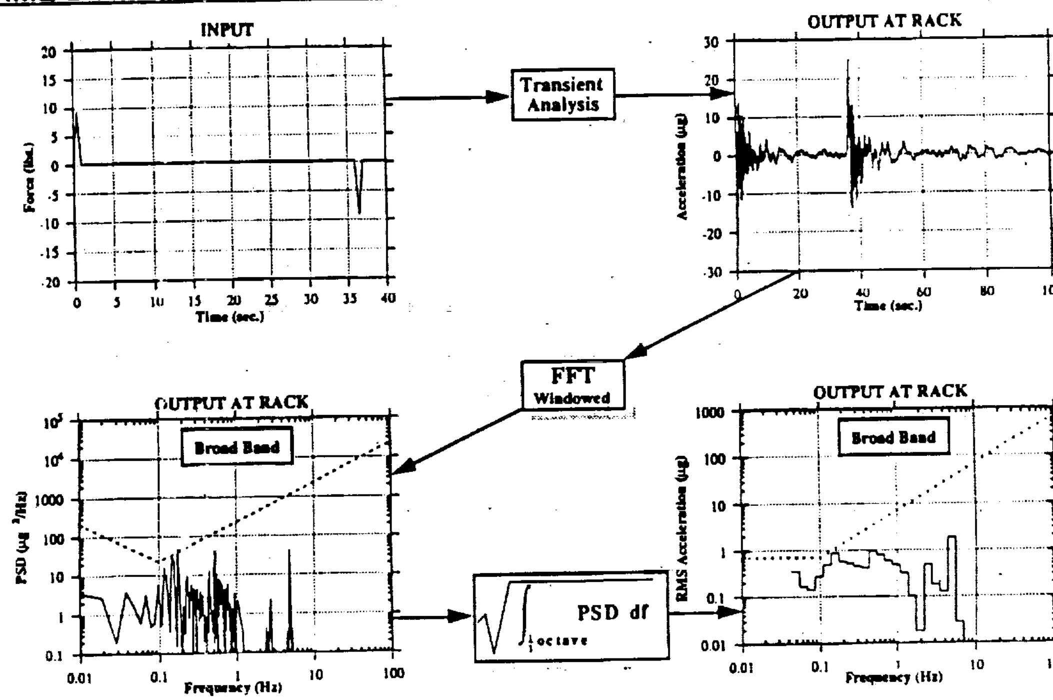




Detailed Hex Truss Model

SSEIC	ASSESSMENT METHODS <15 Hz	Dynamic Assessment Methods	
		Steve Del Basso/Grumman (703) 438-5674	Date: 4/2/91 Page: 19

TIME DOMAIN RESPONSE ANALYSIS



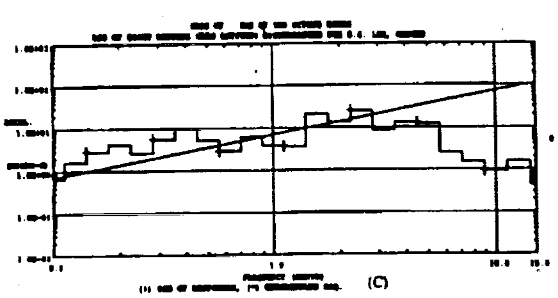
Steve Del Basso/Grumman
(703) 438-5674

MACHINE ALLOCATIONS

- Define typical environments with the aid of Design Reference Mission (DRM) documentation and system engineering personnel.
- Compare the combined acceleration response one-third octave band Grms levels at the various microgravity payload accommodation locations to the broad band requirement and check the steady-state sinusoidal disturbances against the narrow band requirement.
- Identify the worst case quiescence factor (ratio of allowable to response acceleration) in each one-third octave band.
- Identify the contributing disturbances.
- Scale the input disturbance levels by the quiescence factor and a derived weighting function to account for dominant sources, physical limitations, et cetera ...

Steve Del Basso/Grumman
(703) 438-5674

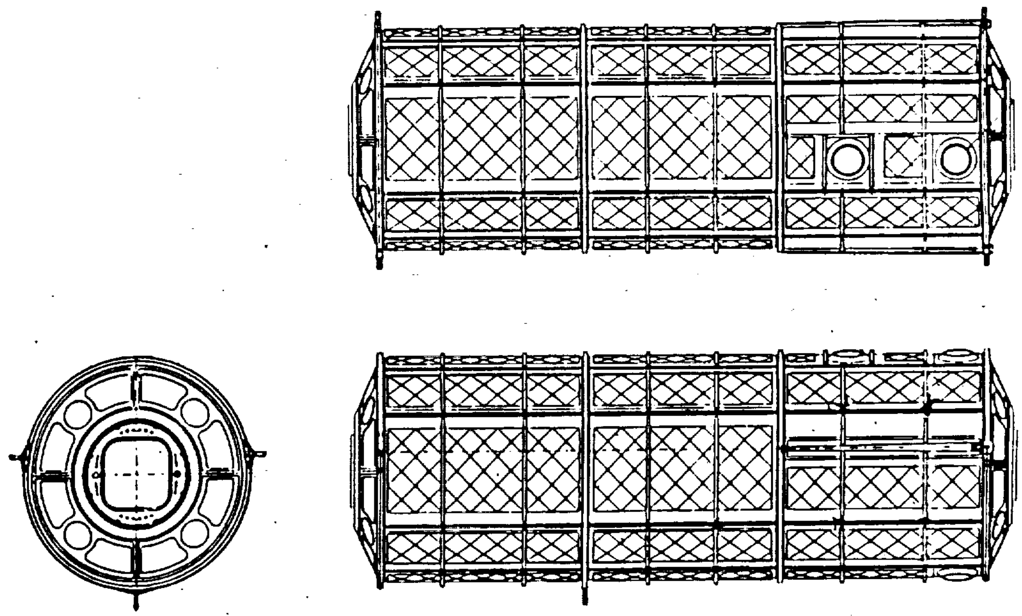
PDR RESULTS



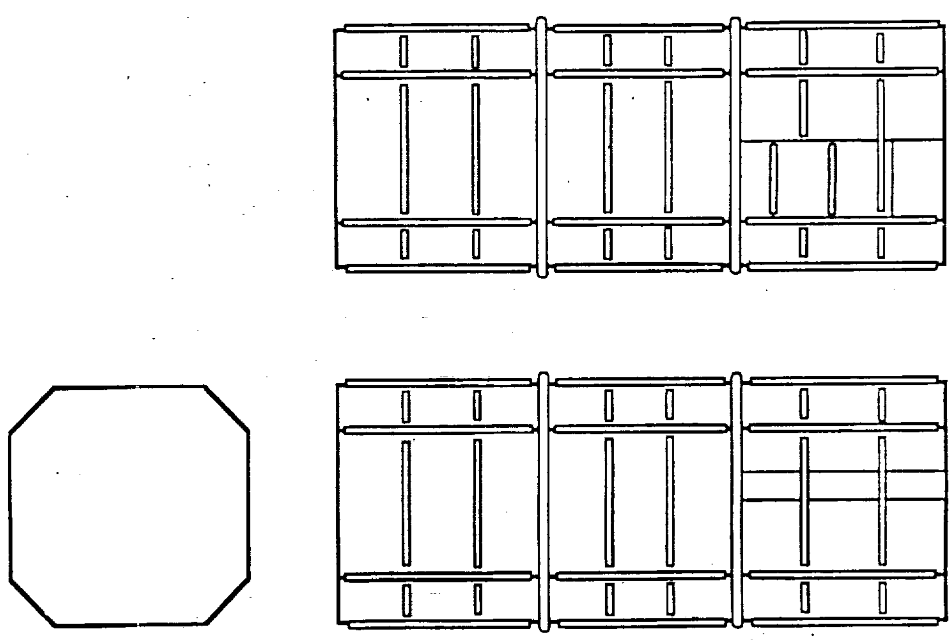
(C) RESPONSE AT U.S. LAB CENTER

Table 6-13. ROUTINE DISTURBANCE ENVIRONMENT ASSESSMENT

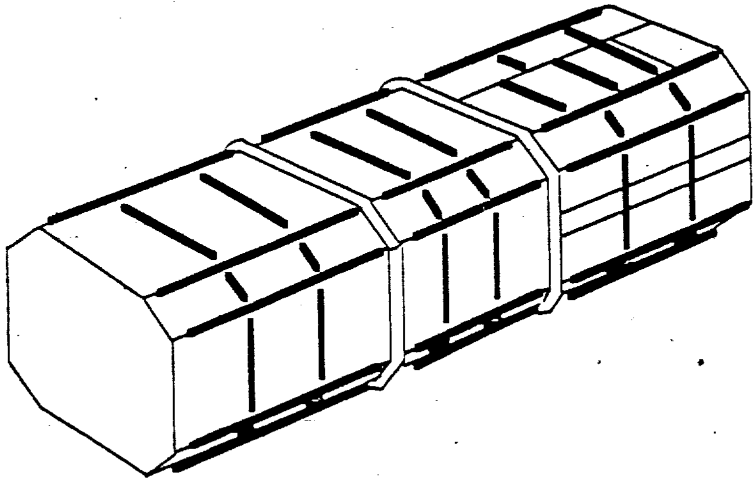
Center Frequency	Quiescence Factor JEM Center	ESA Center	LAB Center
0.10	0.97	0.94	0.97
0.13	0.41	0.41	0.47
0.16	0.23	0.21	0.32
0.20	0.15	0.12	0.26
0.25	0.49	0.31	0.56
0.32	0.26	0.20	0.33
0.40	0.07	0.06	0.20
0.50	0.44	0.49	0.59
0.63	0.90	1.21	1.29
0.79	0.53	0.71	0.76
1.00	0.87	0.86	0.98
1.26	1.62	1.71	1.50
1.59	0.78	0.86	0.45
2.00	0.77	0.99	0.74
2.51	0.69	0.66	0.55
3.16	2.31	2.33	1.80
3.98	1.82	1.76	1.39
5.01	0.42	0.42	0.42
6.31	1.44	1.44	1.44
7.94	1.95	1.95	1.95
10.00	0.66	0.66	0.66
12.59	1.27	1.27	1.27
Logarithmic Average	0.64	0.64	0.71
Number of Exceedences	16	15	15



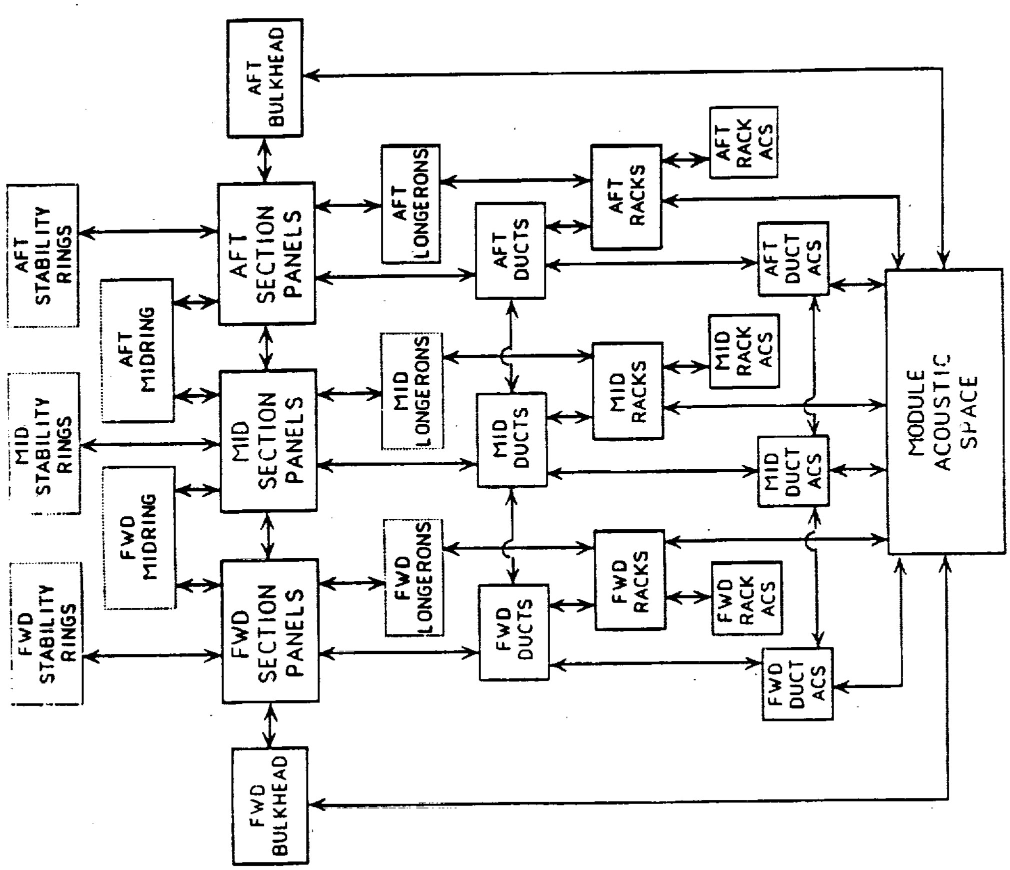
ENGINEERING DRAWING OF SSF HABITATION MODULE



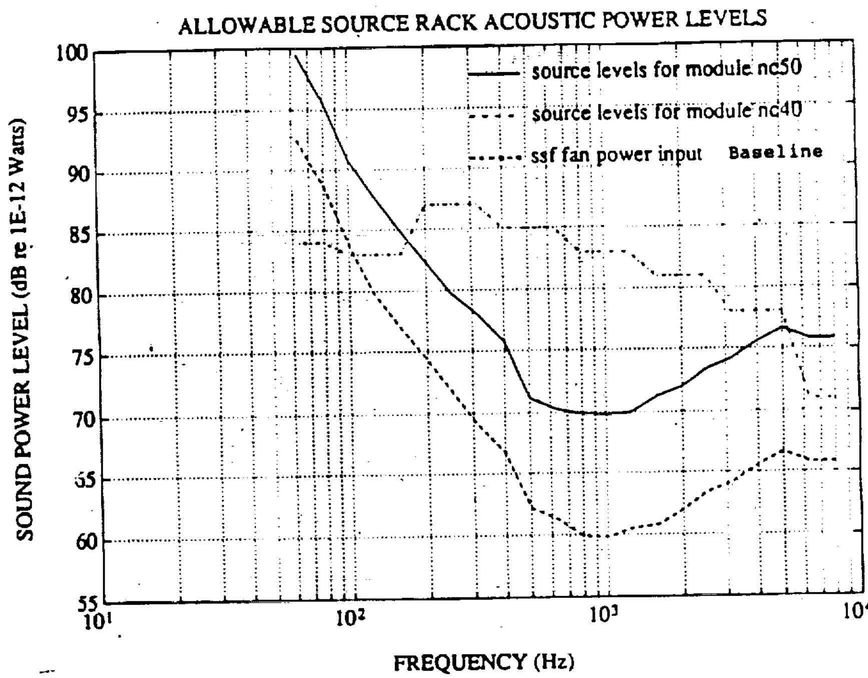
VAPEPS MODEL OF SPACESTATION FREEDOM
HABITATION MODULE OUTER STRUCTURE



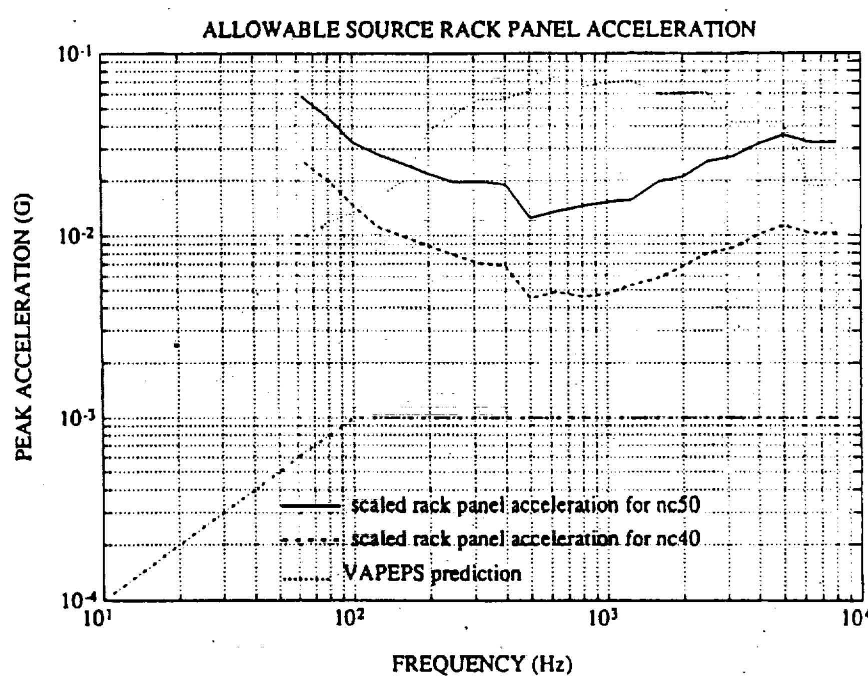
OBLIQUE VIEW OF VAPEPS
SSF MODEL



BASELINE VAPEPS MODEL OF
SPACESTATION FREEDOM HABITATION MODULE



Predicted Acoustic Source Levels in Rack Corresponding
NC-40 and NC-50 Acoustic Levels Inside the Module Acoustic Space.



Prediction of Rack Vibration Levels Created when NC-40 and NC-50
Acoustic Levels Occur Inside the Module Acoustic Space.

WP-2 Micro-G Disturbance Sources

Fred Trueman

3 April 1991

Space Station Freedom McDonnell Douglas • GE • Honeywell • IBM • Lockheed

3/30/91 -000-1 Author Filename

	A	B	C	D	E	F	G	H	I	J	K	L	M
1													
2					WP-2 Potential Vibration Sources								
3													
4				Time of	Duration of	Noise Generating	Sound			Amp. Force,			
5	System	Component	Location	Operation	Operation	Mechanism	Power Level	Freq. Character	Vibration	[illegible]	[illegible]	Part No.	Supplier
6													
85	C&T	TV Camera Pan-Tilt Motor	Node 1 (external)		Intermittent	Mech vib							
86	ECLSS	PCA equalization valve	Node 1	Opening	Intermittent	Mech vib							
87		Control logic	Node 2	After PMC									
88	ECLSS	Node to element IMV Fan(2)	Node 2 & 3		Continuous	Aerodyn, spring modes, mech vib							
89	C&T	TV Camera Pan-Tilt Motor	Node 2 (external)		Intermittent	Mech vib							
90	EPS	Fluorescent Lights	Node Airlock Cupola		Continuous	Mech vib		60 Hz					
91	[illegible]	[illegible]	Node/Airlock		Intermittent								
92	[illegible]	[illegible]	Node/Airlock		Intermittent								
93	[illegible]	Audio terminal (Crewmember)	Node/Airlock		Intermittent								
94	EVA	Zero-g [illegible]	Node/Airlock		Intermittent								
95	ECLSS	[illegible] Fan	Nodes		Continuous	Aerodyn, spring modes, mech vib							
96	ECLSS	IMV return air ISO valves (4-5)	Nodes										
97	ECLSS	THC Temp control valve	Nodes	Continuous	Continuous	Mech vib							
98	ECLSS	THC [illegible] fan	Nodes	Continuous	Continuous	Mech vib							
99	ECLSS	THC Water separator	Nodes	Continuous	Continuous	Mech vib							
100	ECLSS	THC Condensate heat exchanger	Nodes	Continuous	Continuous	Aerodyn, hydrodynamics							
101	ECLSS	CO2 Control Assy	Nodes	Continuous	Continuous	Aerodyn, mech vib							
102	ECLSS	Condensate Dump Assy	Nodes		Intermittent	Aerodyn, hydrodynamics							
103	ECLSS	O2 + N2 Control valve Assy	Nodes		Intermittent	Aerodynamics							
104	ECLSS	[illegible] valve assy	Nodes		Intermittent	Mech vib							
105	ECLSS	[illegible] valves (5-6)	Nodes		Intermittent	Mech vib							
106	ECLSS	O2 ISO valves (4-5)	Nodes		Intermittent	Mech vib							
107	ECLSS	N2 ISO valve	Nodes		Intermittent	Mech vib							
108	ECLSS	Water ISO valve	Nodes		Intermittent	Mech vib							
109	ECLSS	Sample line ISO valves (7-10)	Nodes		Intermittent	Mech vib							
110	ECLSS	[illegible]	Nodes		Continuous	Mech vib							
111	ECLSS	[illegible] ISO valves	Nodes										
112	ECLSS	[illegible] valves	Nodes										
113	ECLSS	[illegible] valves	Nodes										
114	ECLSS	[illegible] ISO valves	Nodes										
115	[illegible]	[illegible]	Nodes	[illegible]	Intermittent	Mech vib							
116	ATCS	Water Lines	Nodes		Continuous	Mech vib							
117	ATCS	[illegible] valves	Nodes		Continuous	Mech vib							
118	ATCS	Water Pump Package	Nodes 2 & 3		Continuous	Mech vib							
119	ATCS	[illegible]	Nodes 2 & 3 [illegible]		Continuous	Mech vib							
120	ECLSS	IMV supply air ISO valves (4-7)	[illegible]										
121	EPS	[illegible] Lights	[illegible]		Continuous	Mech vib		60 Hz					
122	[illegible]	[illegible]	Seg 1	Assembly only	Intermittent								
123	[illegible]	[illegible] Berth Adapter	Seg 1	Assembly only	Intermittent								
124	[illegible]	[illegible]	Seg 1	Assembly only	Intermittent								
125	[illegible]	[illegible] (4)	Seg 1	Assembly only	Intermittent								
126	[illegible]	Prop. Module Att. Sys Mnting(2)	Seg 1 & 10	Resupply	all Seg resupply	Mech. Vibration							Allied Signal
127	[illegible]	Prop. Module Att. Sys Mnting(2)	Seg 1 & 10	Resupply	all Seg resupply	Mech. Vibration							Allied Signal
128	[illegible]	Prop. Module Att. Sys Mnting(2)	Seg 1 & 10	Resupply	all Seg resupply	Mech. Vibration							Allied Signal
129	[illegible]	Prop. Module Att. Sys Mnting(2)	Seg 1 & 10	Resupply	all Seg resupply	Mech. Vibration							Allied Signal
130	[illegible]	[illegible]	Seg 1 & 10		Intermittent	Mech vib							
131	[illegible]	[illegible] Module	Seg 1 & 10	[illegible]	Intermittent	Mech vib							
132	[illegible]	Valves	Seg 1 & 10	[illegible]	Intermittent	Mech vib							LMSC
133	[illegible]	Solar Alpha Rotary Joint	Seg 1 & 10		Continuous	Mech vib							[illegible]
134	[illegible]	1 bearing assy	Seg 1 & 10		Continuous	Mech vib							Honeywell [illegible]
135	[illegible]	2 [illegible] assy	Seg 1 & 10		Continuous	Mech vib							Honeywell [illegible]
136	[illegible]	3 drive assy	Seg 1 & 10		Continuous	Mech vib							Honeywell [illegible]
137	[illegible]	4 motors	Seg 1 & 10		Continuous	Mech vib							Honeywell [illegible]
138	[illegible]	5 gears	Seg 1 & 10		Continuous	Mech vib							Honeywell [illegible]
139	[illegible]	6 [illegible] unit	Seg 1 & 10		Intermittent	Mech vib							LMSC
140	[illegible]	7 [illegible]	Seg 1 & 10		Intermittent	Mech vib							
141	[illegible]	[illegible]	Seg 1 & 10		Continuous							[illegible]	
142	[illegible]	TV Camera Pan-Tilt Mnting(2)	Seg 1 & 10	Viewing	Intermittent	Mech vib							
143	[illegible]	ACS Thrusters	Seg 1 Prop module	Docking	Intermittent	Mech vib							
144	[illegible]	Valves	Seg 1 Prop module	Docking	Intermittent	Mech vib							
145	[illegible]	[illegible]	Seg 1 Prop module	[illegible]	Intermittent	Mech vib							
146	[illegible]	Valves	Seg 1 Prop module	[illegible]	Intermittent	Mech vib							
147	EPS	[illegible] lights ([illegible] lights)	Seg 1, 2, 3, 6		Continuous	Mech vib							
148	EPS	DDCU	Seg 1, 2, 3, 6		Continuous	Mech vib		[illegible]	[illegible]		[illegible]	[illegible]	Honeywell
149	[illegible]	MDM Transformers	Seg 1, 2, 3, 6		Continuous	Mech vib		[illegible]					
150	[illegible]	Mass Memory Unit (2)	Seg 2		Continuous	Mech vib		[illegible]		[illegible]	[illegible]	[illegible]	[illegible]
151	[illegible]	CMG (4)	Seg 2		Continuous	Mech vib							[illegible]
152	[illegible]	[illegible] Assy	Seg 2		Continuous	Mech vib		[illegible]		[illegible]			[illegible]
153	[illegible]	[illegible]	Seg 2		Continuous	Mech vib		[illegible]		[illegible]			[illegible]
154	[illegible]	[illegible]	Seg 2		Continuous	Mech vib		[illegible]		[illegible]			[illegible]
155	[illegible]	[illegible]	Seg 2		Continuous	Mech vib						[illegible]	[illegible]
156	[illegible]	[illegible] Assy	Seg 2		Continuous	Mech vib		[illegible]		[illegible]			[illegible]
157	[illegible]	[illegible]	Seg 2		Continuous	Mech vib		[illegible]					[illegible]
158	[illegible]	Transformers	Seg 2		Continuous	Mech vib							
159	[illegible]	KU band antenna [illegible] (2)	Seg 2	[illegible]	Continuous	Mech vib							
160	[illegible]	KU band antenna [illegible]	Seg 2	[illegible]	Intermittent	Mech vib							

	A	B	C	D	E	F	G	H	I	J	K	L	M
1													
2					WP-2 Potential Vibration Sources								
3													
4				Time of	Duration of	Noise Generating	Sound			Amp. Force,			
5	System	Component	Location	Operation	Operation	Mechanism	Power Level	Freq. Character	Vibration	[illegible]	[illegible]	Part No.	Supplier
6													
161	[illegible]	KU band antenna [illegible]	Seg 2	[illegible]	Intermittent	Mech vib							
162	[illegible]	TV Camera Pan-Tilt Motor	Seg 2	[illegible]	Intermittent	Mech vib							
163	[illegible]	ACS control motor [illegible]	Seg 2 [illegible] CMG	[illegible]	Intermittent	Mech vib							GE
164	[illegible]	ACS control motor [illegible]	Seg 2 [illegible] CMG	Continuous	Intermittent	Mech vib							
165	[illegible]	TV Camera Pan-Tilt Motor	Seg 3 & 8		Intermittent	Mech vib							
166	[illegible]	[illegible] Rotary Joint	Seg 3 & 8		Continuous	Mech vib							Honeywell [illegible]
167	ATCS	1 bearing assy	Seg 3 & 8		Continuous	Mech vib							Honeywell [illegible]
168	ATCS	2 drive assy	Seg 3 & 8		Continuous	Mech vib							Honeywell [illegible]
169	ATCS	3 motors	Seg 3 & 8		Continuous	Mech vib							Honeywell [illegible]
170	ATCS	4 gears	Seg 3 & 8		Continuous	Mech vib							Honeywell [illegible]
171	ATCS	5 [illegible] unit	Seg 3 & 8		Intermittent	Mech vib							Honeywell [illegible]
172	ATCS	6 [illegible]	Seg 3 & 8		Intermittent	Mech vib							Honeywell [illegible]
173	ATCS	7 [illegible] transfer assy	Seg 3 & 8		Continuous	Mech vib							Honeywell [illegible]
174	ATCS	8 [illegible]	Seg 3 & 8		Continuous	Mech vib							LMSC
175	ECLSS	O2 Gas [illegible] assy	Seg 4										
176	ECLSS	N2 Gas [illegible] assy	Seg 4										
177	EPS	MBSU	Seg 4		Intermittent	Mech vib		[illegible]					
178	[illegible]	[illegible]	Seg 4		Intermittent	Mech vib							
179	[illegible]	ULC Cryo Carrier Att. Sys Mnting(2)	Seg 4	Resupply	all Seg resupply	Mech. Vibration							Allied Signal
180	[illegible]	ULC Cryo Carrier Att. Sys Mnting(2)	Seg 4	Resupply	all Seg resupply	Mech. Vibration							Allied Signal
181	[illegible]	[illegible] (2)	Seg 2 NAV [illegible]	Continuous	Continuous	Mech vib		[illegible] Hz			[illegible]	[illegible]	Honeywell
182	[illegible]	[illegible]	Seg 2 NAV [illegible]	Continuous	Continuous	Mech vib		[illegible]			[illegible]		Honeywell
183	[illegible]	[illegible]	Seg 2 NAV [illegible]	Continuous	Continuous	Mech vib		[illegible]		[illegible]	[illegible]	[illegible]	Honeywell
184	[illegible]	[illegible]	Seg 2 NAV [illegible]	Continuous	Continuous	Mech vib		[illegible]		TBD		TBD	Honeywell
185	[illegible]	[illegible] (2)	Seg 2 NAV [illegible]	Continuous	Continuous	Mech vib		TBC		TBD	TBD	TBD	[illegible]
186	[illegible]	[illegible] (2)	Seg 2 NAV [illegible]	Continuous	Continuous	Mech vib		TBD			TBD	TBD	[illegible]
187	[illegible]	[illegible] (2)	Seg 2 NAV [illegible]	Continuous	Continuous	Mech vib		TBD			TBD	TBD	[illegible]
188	[illegible]	Transformers	Seg 2 NAV [illegible]	Continuous	Continuous	Mech vib		TBD		TBD	TBD	TBD	[illegible]
189	ATCS	[illegible]	Seg 2,3, [illegible]	Continuous	Continuous	Mech vib							
190	ATCS	[illegible]	Seg [illegible]	Continuous	Continuous	Mech vib							
191	ATCS	[illegible] (pump)	Seg [illegible]	Continuous	Continuous	Mech vib							
192	ATCS	[illegible] valve	Seg [illegible]	Continuous	Continuous	Mech vib							
193	ATCS	[illegible]	Seg [illegible]	Continuous	Continuous	Mech vib							
194	[illegible]	[illegible] Docking [illegible] Mnting(2)		Docking	[illegible]	Mech vib		[illegible]					
195	[illegible]	[illegible]		Docking		Mech vib		[illegible]					
196	[illegible]	[illegible] (2)		Docking		Mech vib		[illegible]					
197	[illegible]	[illegible] (2)		Docking	[illegible]	Mech vib		[illegible]					
198	[illegible]	[illegible]		Docking		Mech vib							
199	[illegible]	[illegible] (2)		Docking		Mech vib							
200	[illegible]	[illegible] (2)		Docking		Mech vib							
201	[illegible]	[illegible] (2)		Docking		Mech vib							
202	[illegible]	[illegible] (4)		Docking		Mech vib							
203	[illegible]	[illegible]		Docking	[illegible]	Mech vib		[illegible]					
204	[illegible]	ULC Dry Cargo Att. Sys Mnting(2)		Resupply	all Seg resupply	Mech. Vibration							Allied Signal
205	[illegible]	ULC Dry Cargo Att. Sys Mnting(2)		Resupply	all Seg resupply	Mech. Vibration							Allied Signal
206	[illegible]	ULC Dry Cargo Att. Sys Mnting(2)		Resupply	all Seg resupply	Mech. Vibration							Allied Signal
207													

Key Technical Challenges

- Quasi-steady environment
- Fidelity of forcing function data base
- Format of requirements curve / analysis output
- Allocation scheme
- Restructuring effects on requirements
- Man system requirements linkage
- Analysis of broad frequency range with multiple forcing functions
- Quasi steady normal component / GN&C linkage
- Upper vs. lower vibration allowable curves

N92-28451

SPACE ACCELERATION MEASUREMENT SYSTEM

SAMS

NASA Lewis Research Center
Cleveland, Ohio

EARLY MISSION SCIENCE SUPPORT

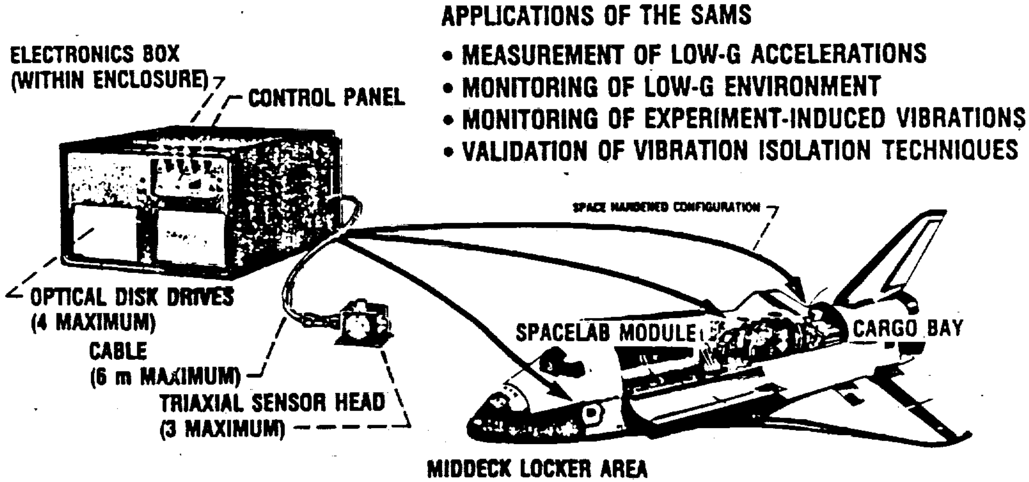
INTERNATIONAL WORKSHOP ON VIT
APRIL 23-25, 1991

RICHARD DeLOMBARD
SAMS PROJECT MANAGER

NASA Lewis Research Center	SPACE EXPERIMENTS DIVISION	SFSD Space Flight Systems Directorate

SPACE ACCELERATION MEASUREMENT SYSTEM (SAMS)

APPLICATIONS OF THE SAMS
- MEASUREMENT OF LOW-G ACCELERATIONS
- MONITORING OF LOW-G ENVIRONMENT
- MONITORING OF EXPERIMENT-INDUCED VIBRATIONS
- VALIDATION OF VIBRATION ISOLATION TECHNIQUES



TYPICAL LOCATIONS FOR THE SAMS

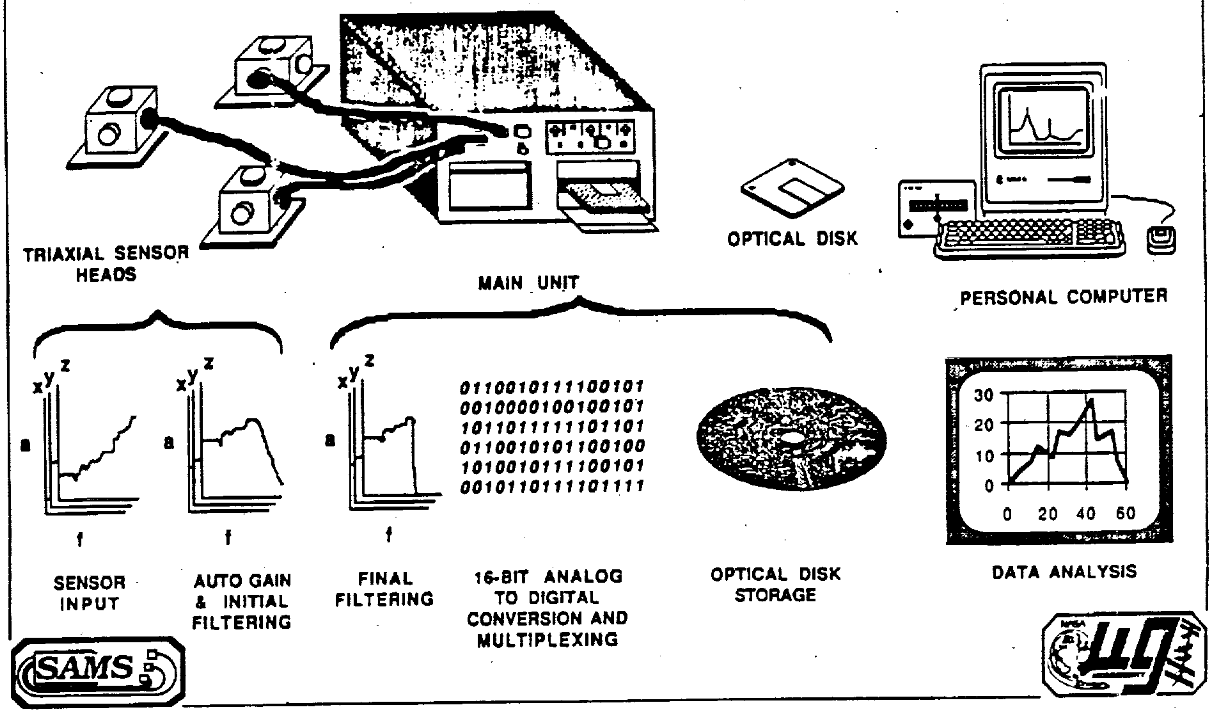
LeRC CONTACT:
P.M.: R. DeLOMBARD

CD-88-38074

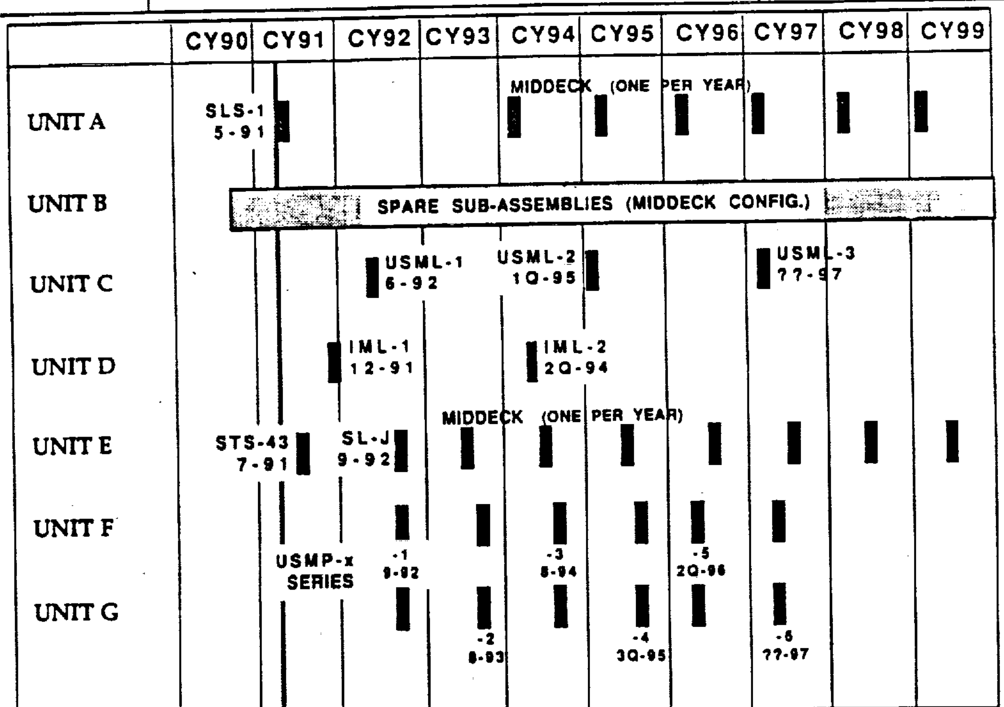
5

NASA Lewis Research Center | SPACE EXPERIMENTS DIVISION | SFSD Space Flight Systems Directorate

SAMS DATA FLOW



NASA Lewis Research Center | SPACE EXPERIMENTS DIVISION | SFSD Space Flight Systems Directorate



SAMS FLIGHT SCHEDULE

Rev.N 4/11/91

NASA Lewis Research Center | SPACE EXPERIMENTS DIVISION | SFSD

MISSION SUPPORT

SLS-1:
TSH-A: SSCE Rack #7, 5 hertz, 25 s/s
TSH-B: SMIDEX Rack #5, 5 hertz, 25 s/s
TSH-C: BRS Chair Frame (in center aisle), 5 hertz, 25 s/s

STS-43:
SSCE;MF57H&K, 2.5 Hz, TSH on SSCE baseplate
PCG; MF14M, 50 Hz; TSH location: MF14K
Treadmill; middeck floor, 50 hertz

IML-1:
MVI Rotating Chair (under floor), Sundstrand TSH, 100 hertz, 500 s/s
Rack #10 (bottom), Bell XI-79 TSH, 2.5 hertz, 12.5 s/s
FES Bench (rack #10), Sundstrand TSH, 100 hertz, 500 s/s

SAMS

NASA Lewis Research Center | SPACE EXPERIMENTS DIVISION | SFSD

MISSION SUPPORT

USML-1:
TSH-A: STDCE Rack 3, 5 Hz,
TSH-B: CGF Support Structure, Rack 9, 2.5 Hz
TSH-C: Glovebox Rack 12, 25 Hz

SL-J:
TSH-A: Rack #10, FMPT-MEL, 50 Hz
TSH-B: Rack #7, FMPT-LS, 50 Hz
TSH-C: Rack #9, Next to SAMS, 2.5 Hz

USMP-1:
Lambda Point Experiment: #1TSH, 100 hertz, 250 s/s (Downlinked)
#2TSH, 100 hertz, 250 s/s (Recorded)
MEPHISTO: MPESS-A Carrier #1TSH, 10 hertz, 50 s/s (Downlinked)
MPESS-A Carrier #2TSH, 25 hertz, 125 s/s (Recorded)

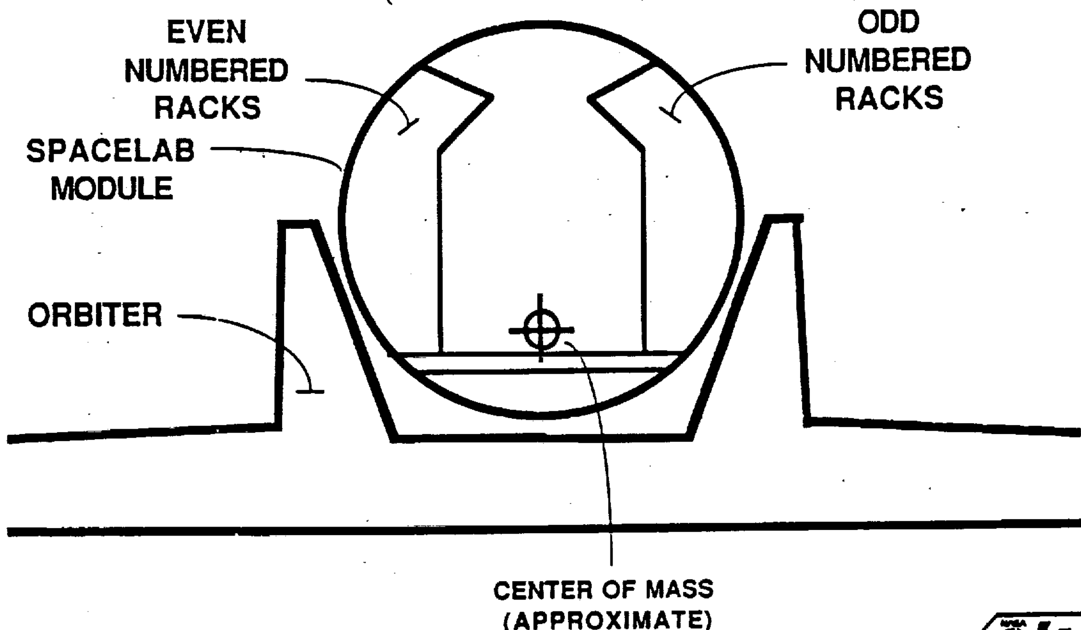
SAMS

NASA
Lewis Research Center

SPACE EXPERIMENTS DIVISION

SFSD
Space Flight Systems Directorate

SHUTTLE/SPACELAB CROSS SECTION
(LOOKING AFT)



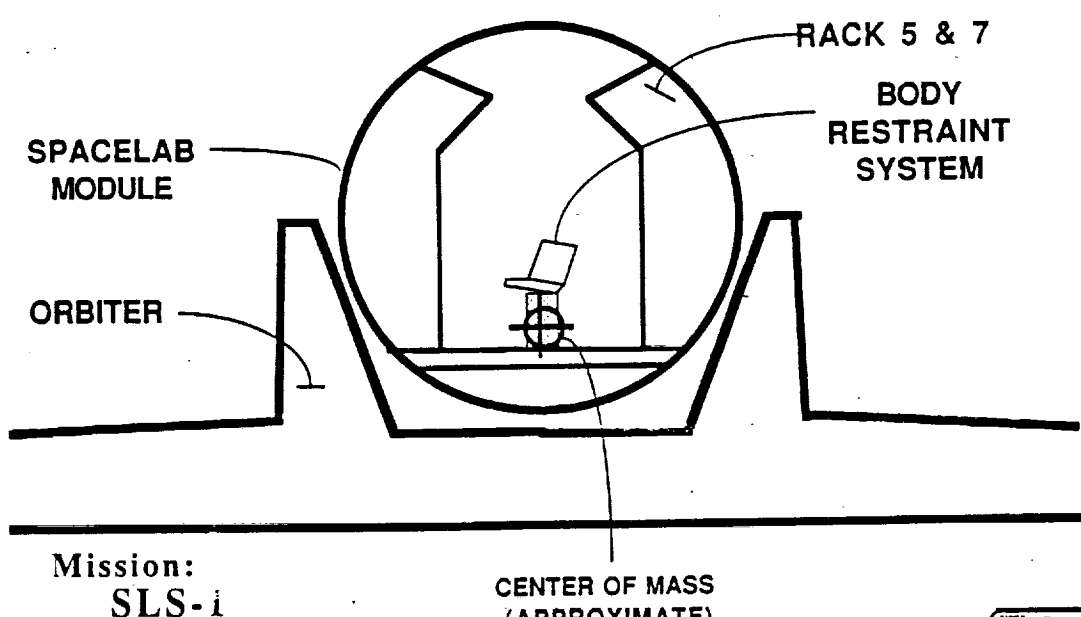
SAMS

NASA
Lewis Research Center

SPACE EXPERIMENTS DIVISION

SFSD

Spacelab Cross Section (Looking Aft)



Mission:
SLS-1

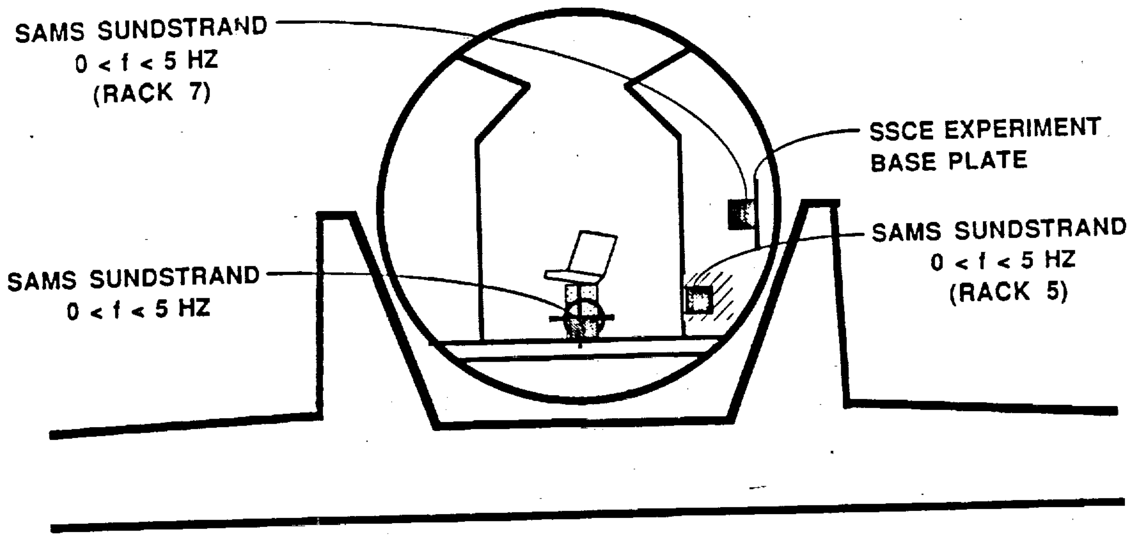
SAMS

NASA
Lewis Research Center

SPACE EXPERIMENTS DIVISION

SFSD

Location of SAMS Sensor Heads, SLS-1



Mission:
SLS-1

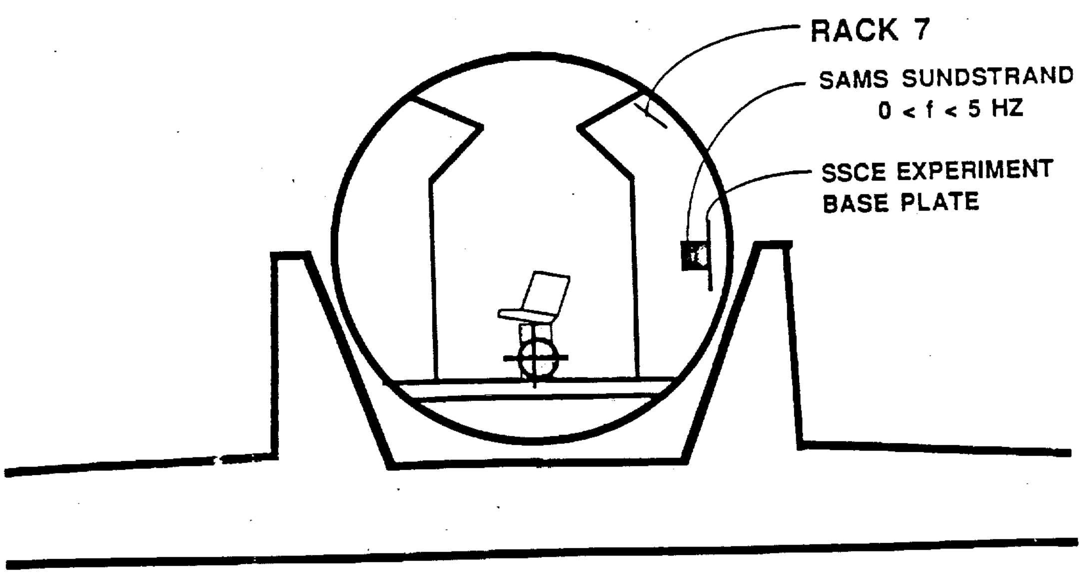
SAMS

NASA
Lewis Research Center

SPACE EXPERIMENTS DIVISION

SFSD

SSCE Low-Frequency Environment, SLS-1



Mission:
SLS-1

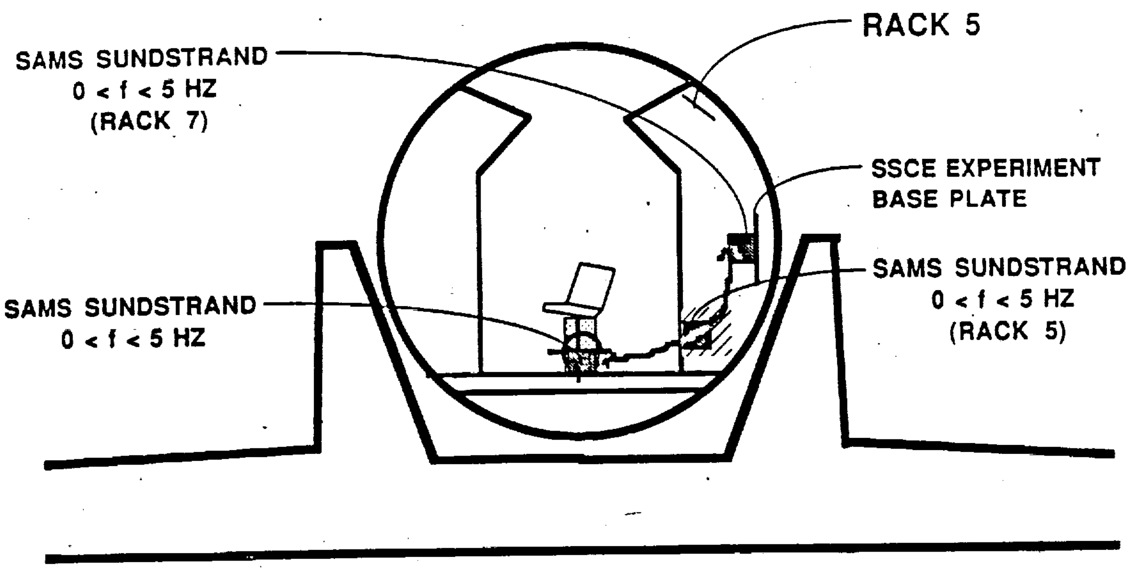
SAMS

NASA
Lewis Research Center

SPACE EXPERIMENTS DIVISION

SFSD

Acceleration Transfer Function, SLS-1



Mission:
SLS-1

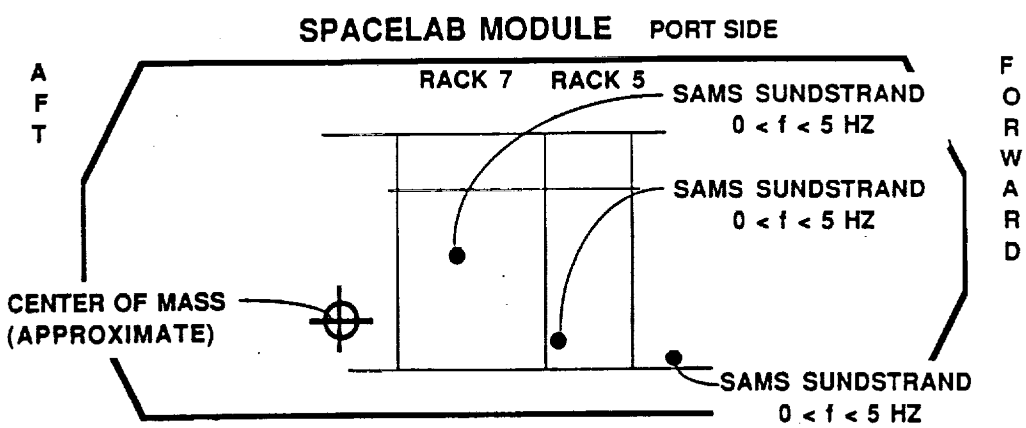
SAMS

NASA
Lewis Research Center

SPACE EXPERIMENTS DIVISION

SFSD

Spacelab Side View and Sensor Head Locations



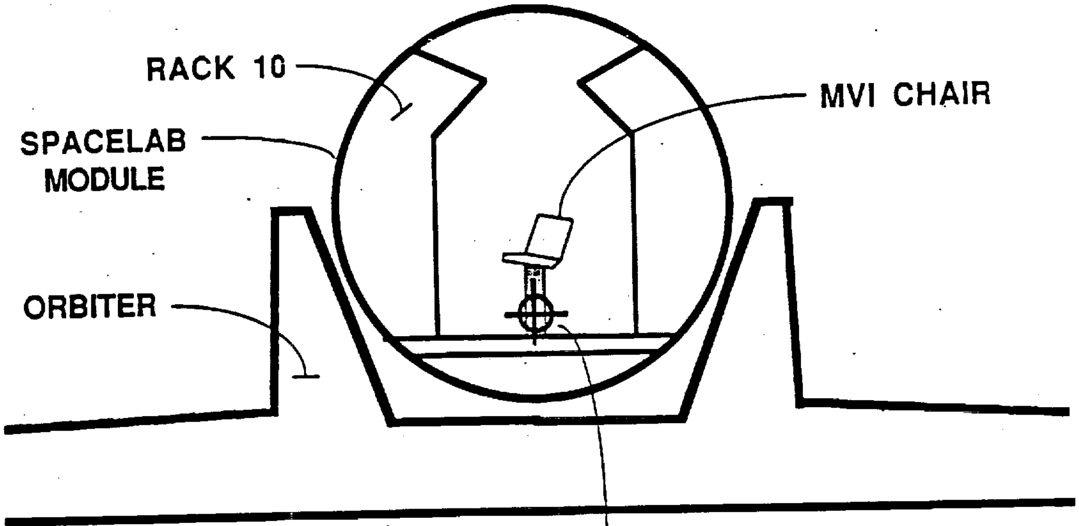
SAMS

NASA
Lewis Research Center

SPACE EXPERIMENTS DIVISION

SFSD
Space Flight Systems Directorate

SHUTTLE/SPACELAB CROSS SECTION
(LOOKING AFT)



Mission:
IML-1

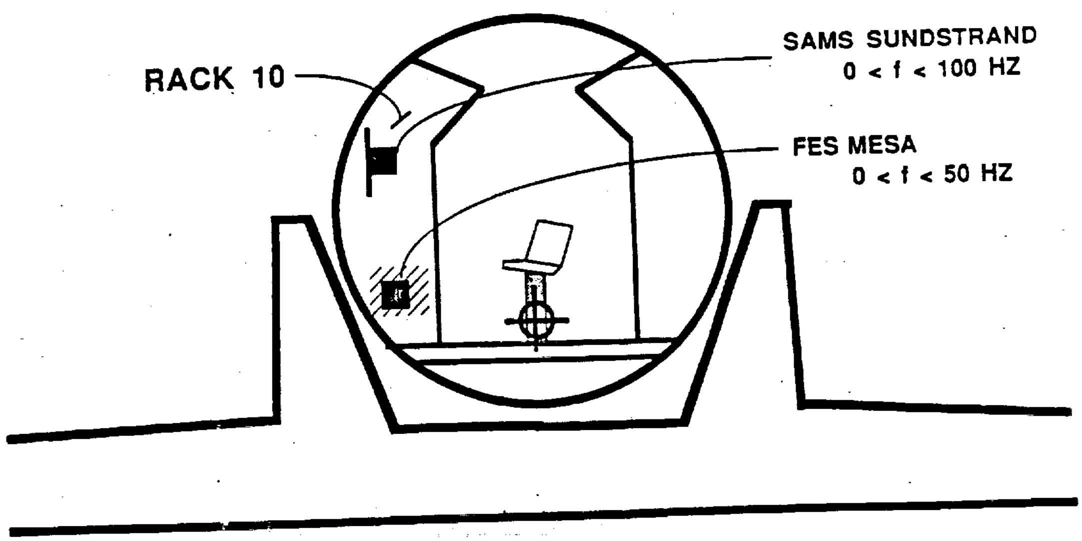
SAMS

NASA
Lewis Research Center

SPACE EXPERIMENTS DIVISION

SFSD
Space Flight Systems Directorate

FES / VCGS HIGH FREQUENCY ENVIRONMENT

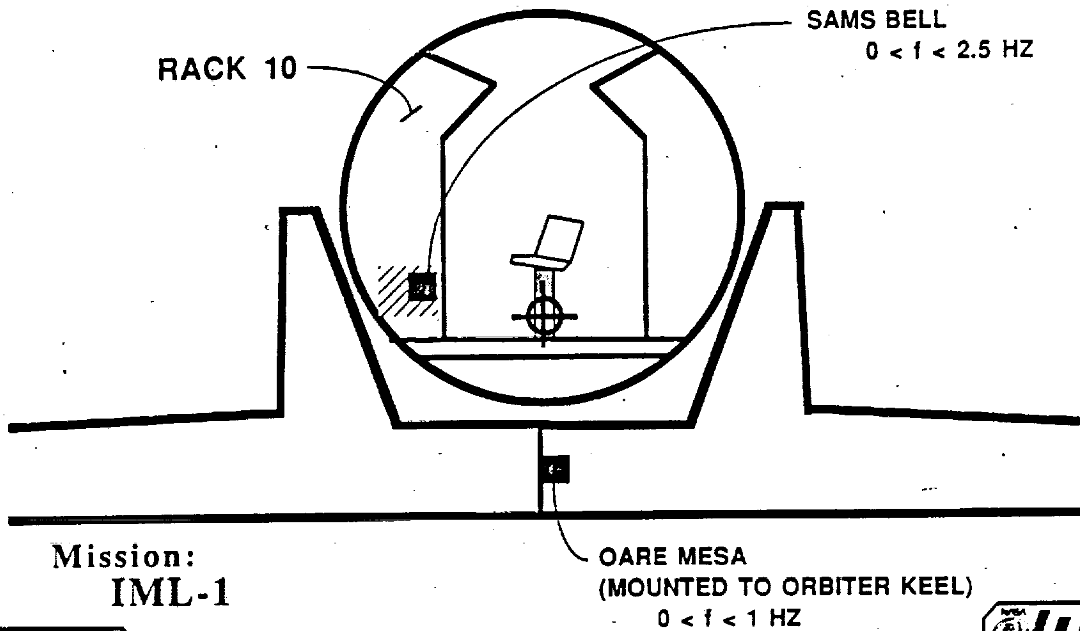


Mission:
IML-1

SAMS

NASA Lewis Research Center | SPACE EXPERIMENTS DIVISION | SFSD Space Flight Systems Directorate

FES / VCGS LOW FREQUENCY ENVIRONMENT

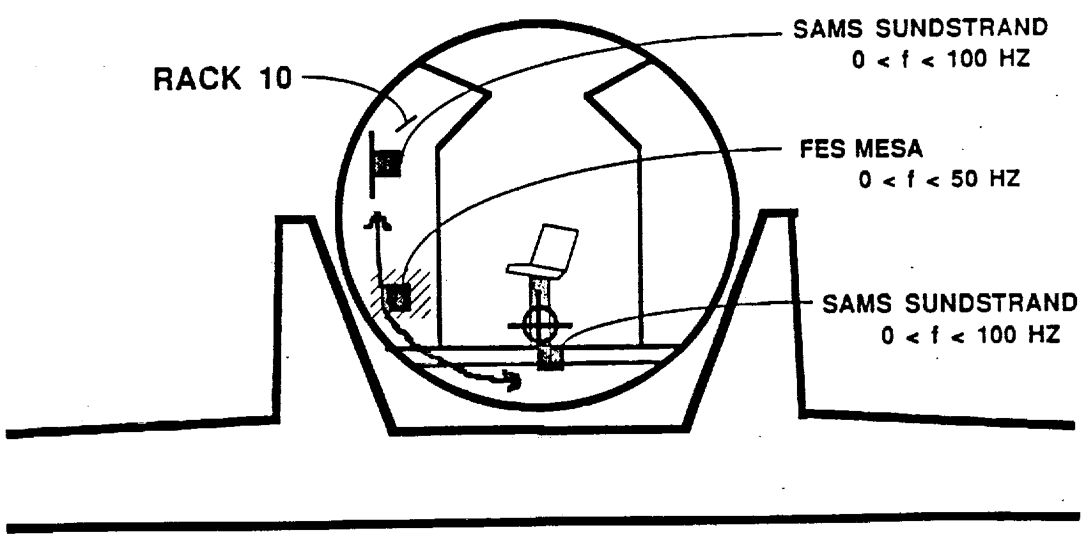


Mission:
IML-1

SAMS

NASA Lewis Research Center | SPACE EXPERIMENTS DIVISION | SFSD Space Flight Systems Directorate

ACCELERATION TRANSFER FUNCTION



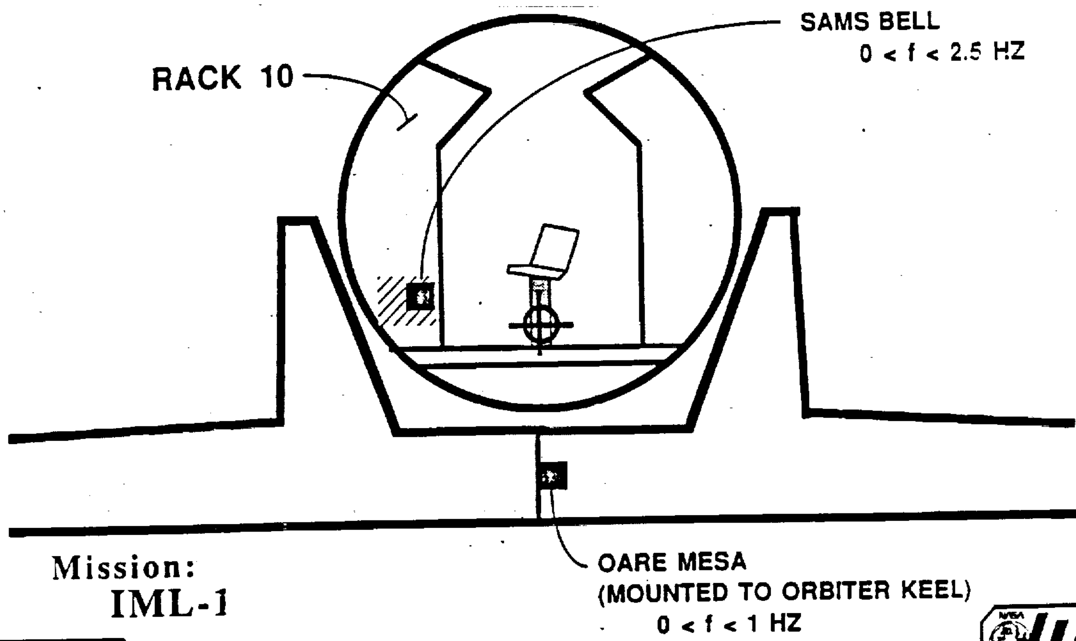
Mission:
IML-1

NOTE: SIMILAR INVESTIGATION TO BE CONDUCTED ON SLS-1 MISSION

SAMS

NASA Lewis Research Center | SPACE EXPERIMENTS DIVISION | SFSD Space Flight Systems Directorate

LOW-FREQUENCY, RIGID BODY CORRELATION

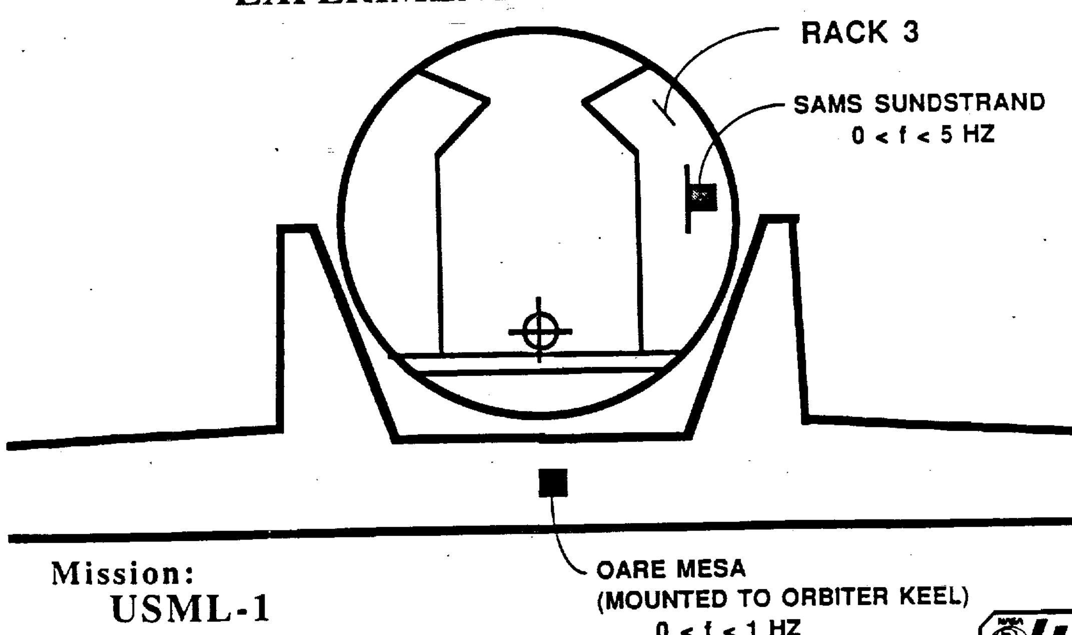


Mission:
IML-1

SAMS

NASA Lewis Research Center | SPACE EXPERIMENTS DIVISION | SFSD Space Flight Systems Directorate

SURFACE TENSION DRIVEN CONVECTION EXPERIMENT ENVIRONMENT



Mission:
USML-1

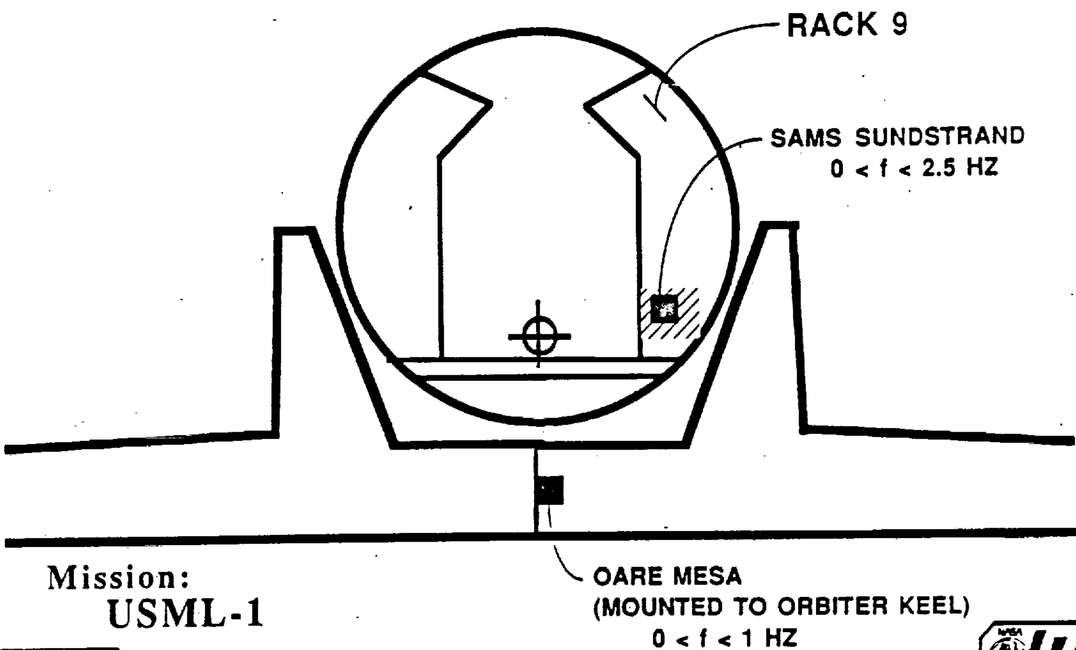
SAMS

NASA
Lewis Research Center

SPACE EXPERIMENTS DIVISION

SFSD
Space Flight Systems Directorate

CRYSTAL GROWTH FURNACE ENVIRONMENT



Mission:
USML-1

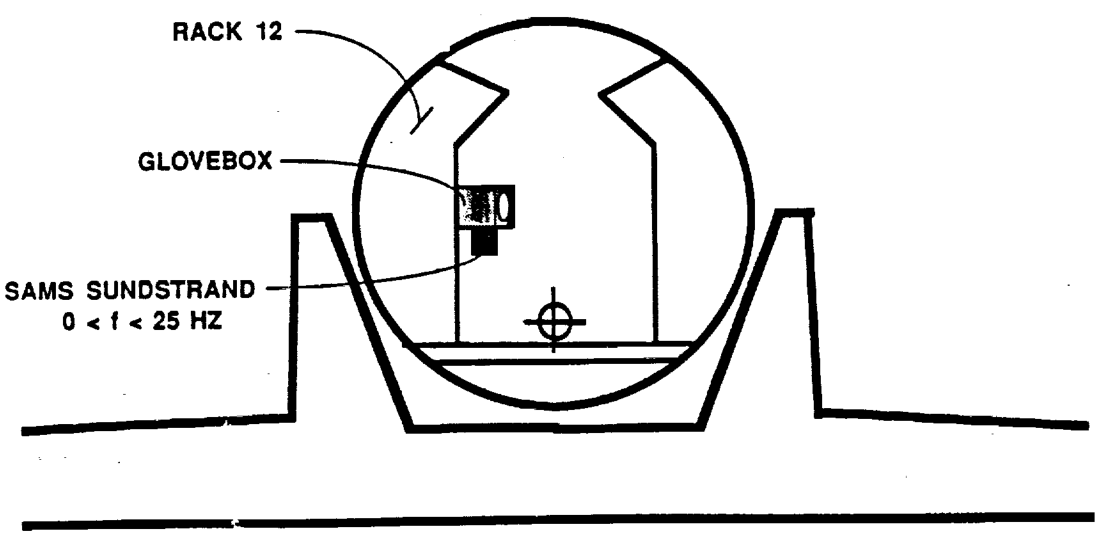
SAMS

NASA
Lewis Research Center

SPACE EXPERIMENTS DIVISION

SFSD
Space Flight Systems Directorate

GLOVEBOX EXPERIMENTS' ENVIRONMENT



Mission:
USML-1

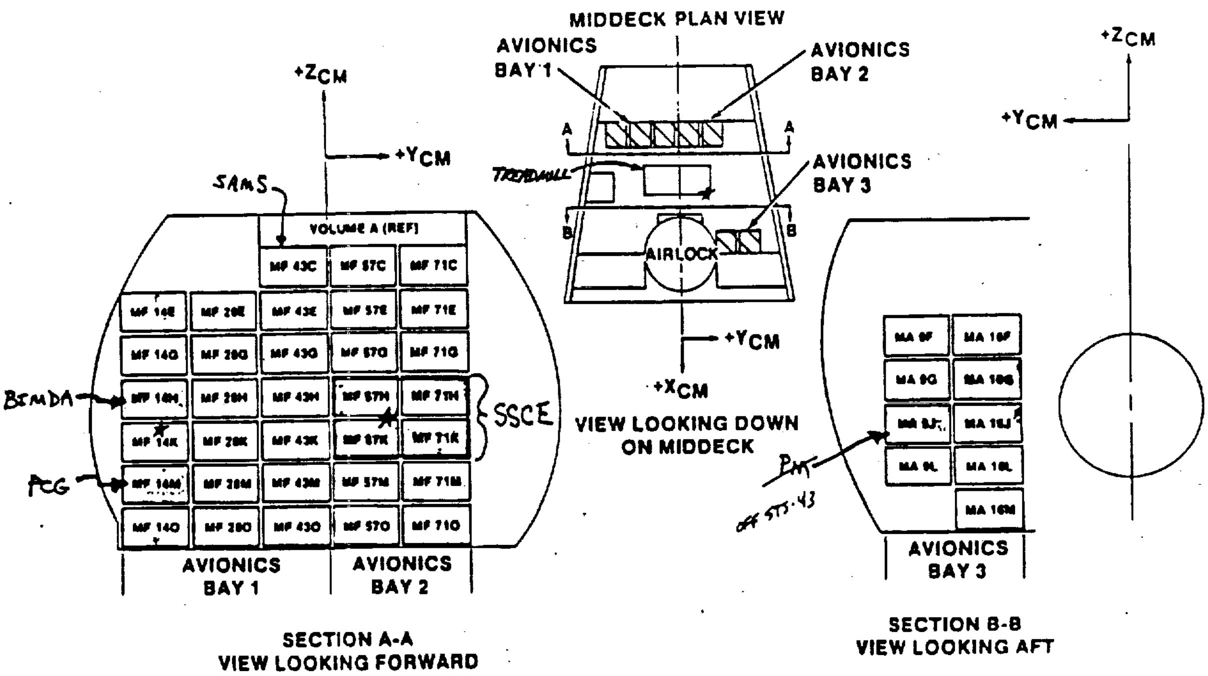
SAMS

SPACE SHUTTLE PAYLOAD
DESIGN AND DEVELOPMENT

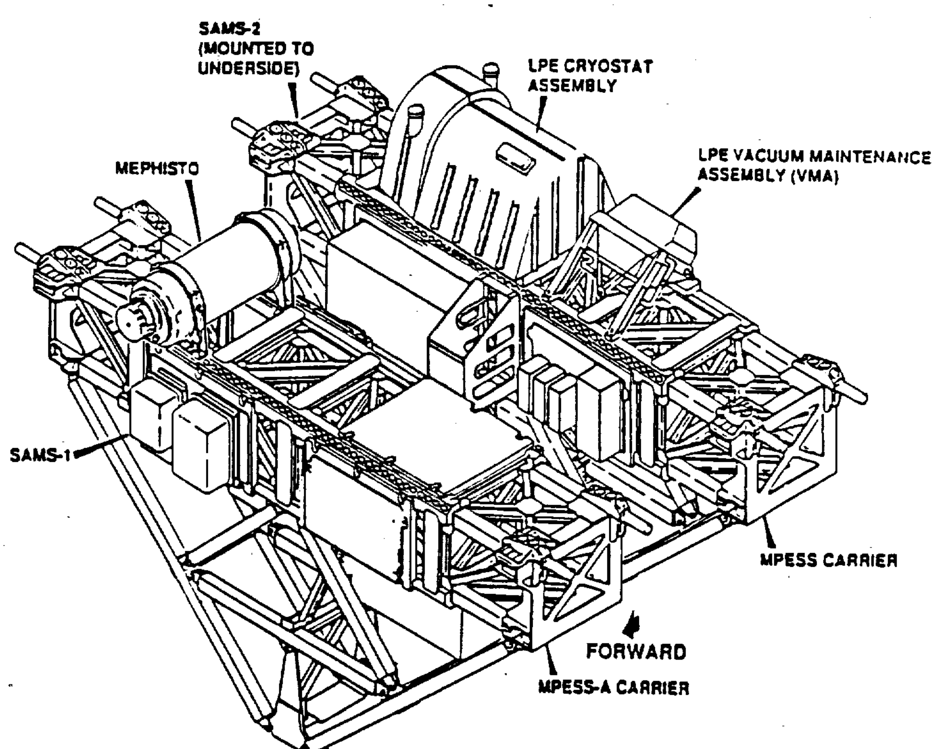
NASA

MIDDECK MODULAR LOCKER LAYOUT

S-88-00349



28



1-2

JA-1031

FIGURE 1-1. USMP-1 CONFIGURATION LAYOUT

MICROGRAVITY ACCELEROMETER CHARACTERIZATION ON COLUMBIA STS-32 MISSION

Jeff Schoess
Honeywell Systems & Research Center

Don Thomas & Bonnie Dunbar
NASA Johnson Space Center

ABSTRACT

The Honeywell In-Space Accelerometer (HISA) is a three-axis microgravity accelerometer instrument package recently developed by Honeywell Systems and Research Center (SRC) to monitor oscillatory and transient accelerations onboard spacecraft and spaceborne structures. The HISA was designed to be co-located with materials and life sciences experiments to record real-time accelerometer event data, sampling time, and temperature.

The HISA was originally developed to monitor the microgravity disturbances associated with a polymer morphology experiment developed by 3M Company in Minneapolis, Minnesota. The HISA was first flight tested with the 3M experiment on the Space Shuttle Atlantis STS-34 in October 1989. The HISA was successfully flown on a second shuttle mission (Columbia STS-32 in January 1990) in support of the NASA JSC-sponsored Microgravity Disturbances Experiment (MDE), which focused on the effects of microgravity disturbances on the growth of high-quality Indium crystals.

The primary objective of the STS-32 MDE experiment was to investigate the effects of crew-induced gravity disturbances on the microstructure (crystal defects and uniformity of impurity distribution) of float-zone-grown crystals. The float-zone technique involves establishing a suspended molten zone between two cylindrical samples a pure, single-crystal sample and an impure, polycrystalline sample.

Microgravity disturbances due to crew treadmill activity and orbiter maneuvering system thruster firings were sensed and recorded by the HISA to understand their effects on the stability of the float zone.

This paper summarizes the principle of operation of the HISA, the flight configuration of the HISA supporting the MDE experiment, and the characterization of STS-32 treadmill disturbance data.

Microgravity Accelerometer Characterization on Columbia STS-32 Mission

by Jeff Schoess
Honeywell Systems and Research Center

Don Thomas and Bonnie Dunbar
NASA Johnson Space Center

STS-32 Accelerometer Briefing Agenda

Honeywell

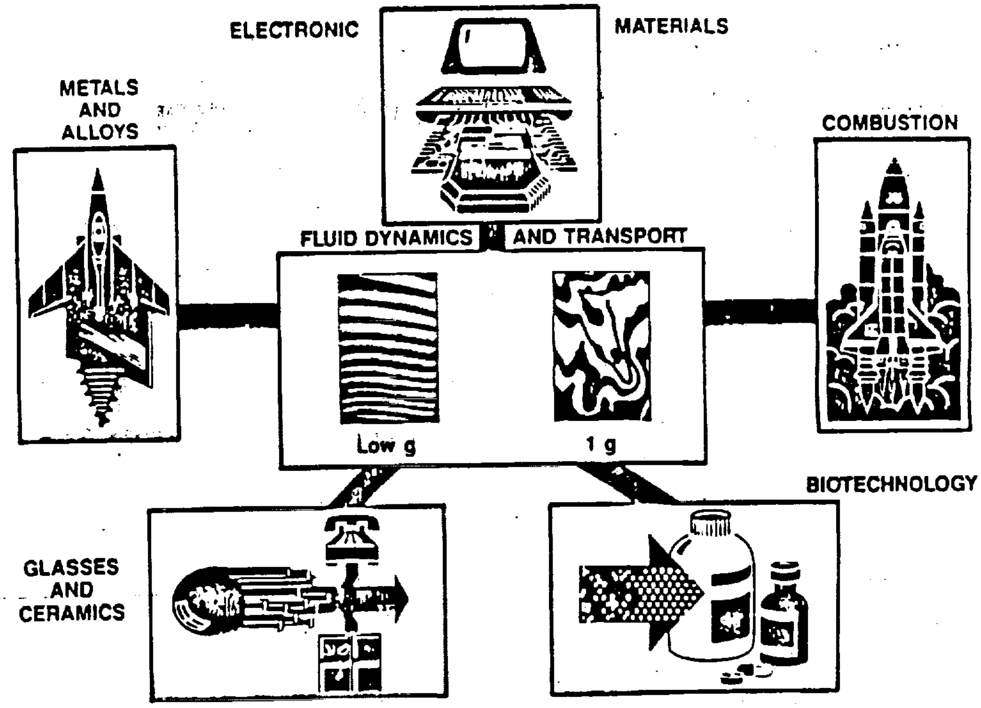
NASA

- Introduction—Honeywell's role in Microgravity Disturbance Experiment (MDE)
- Highlights of STS-32 Mission—MDE experiment and LDEF
- Description of Honeywell In-Space Accelerometer (HISA)
  - Principle of operation
  - Performance specifications
- MDE Treadmill Disturbance Measurement data
- Future plans for HISA
- Summary

Microgravity Science Applications

Honeywell

NASA



Microgravity Disturbances Experiment Processing with Dr. Bonnie Dunbar

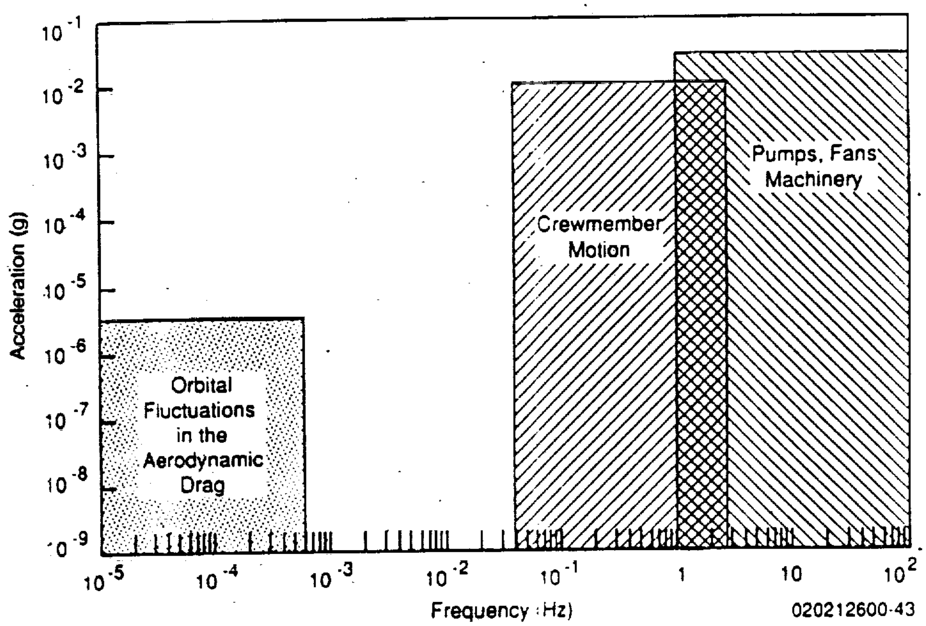
Honeywell

NASA

Microgravity Disturbance Effects in the Space Shuttle

Honeywell

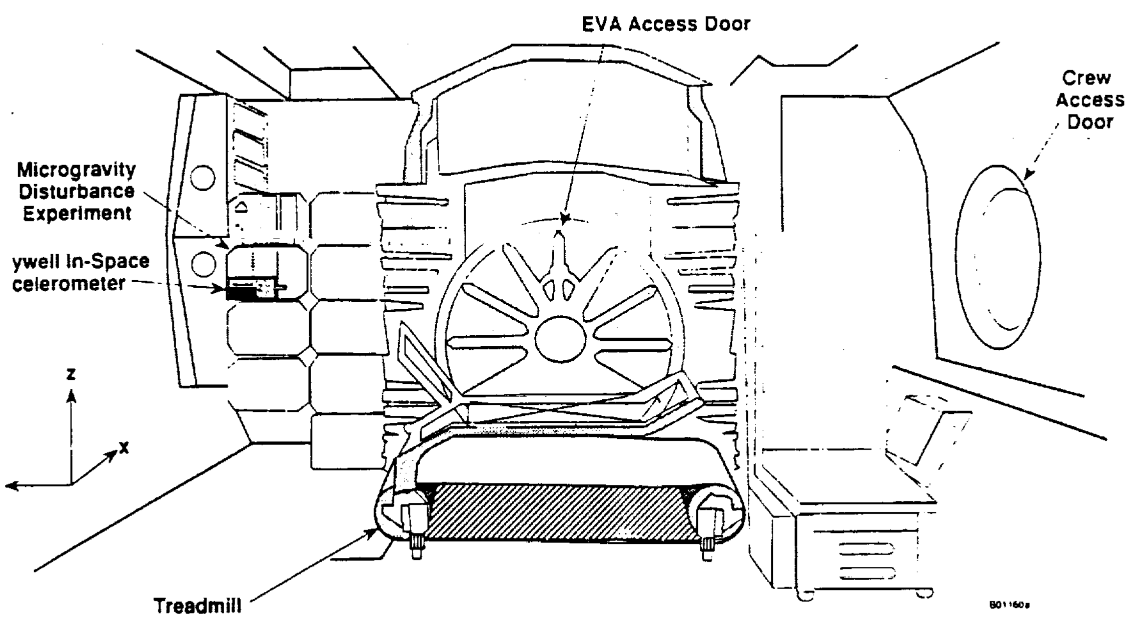
NASA

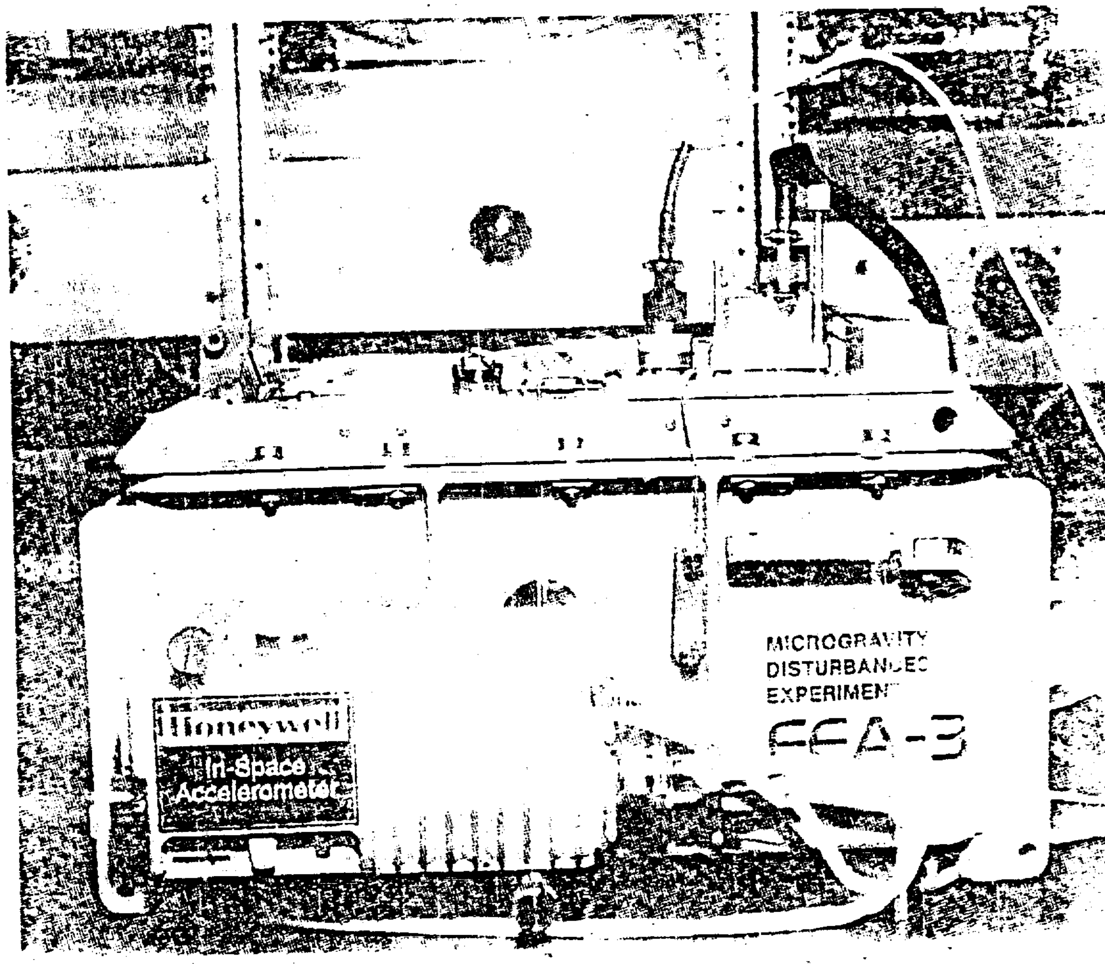


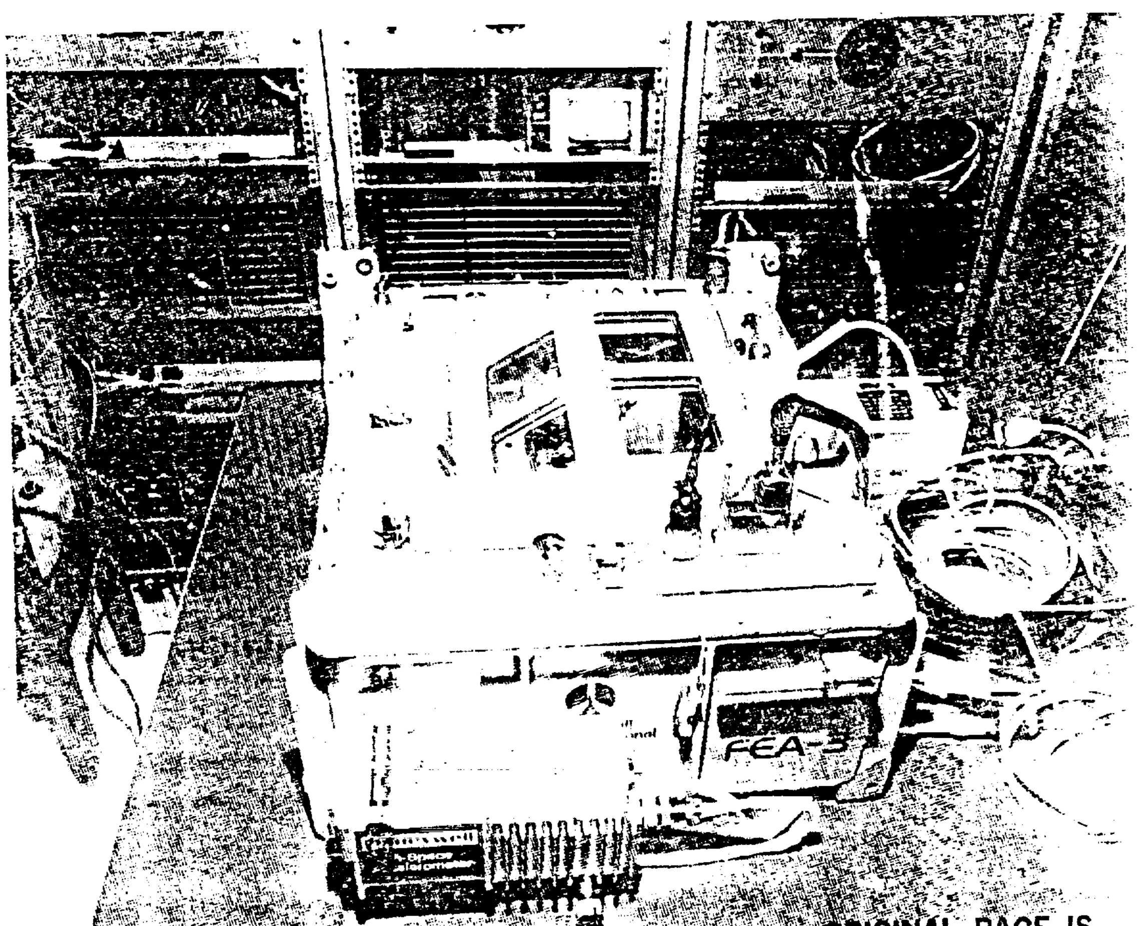
MDE Flight Configuration

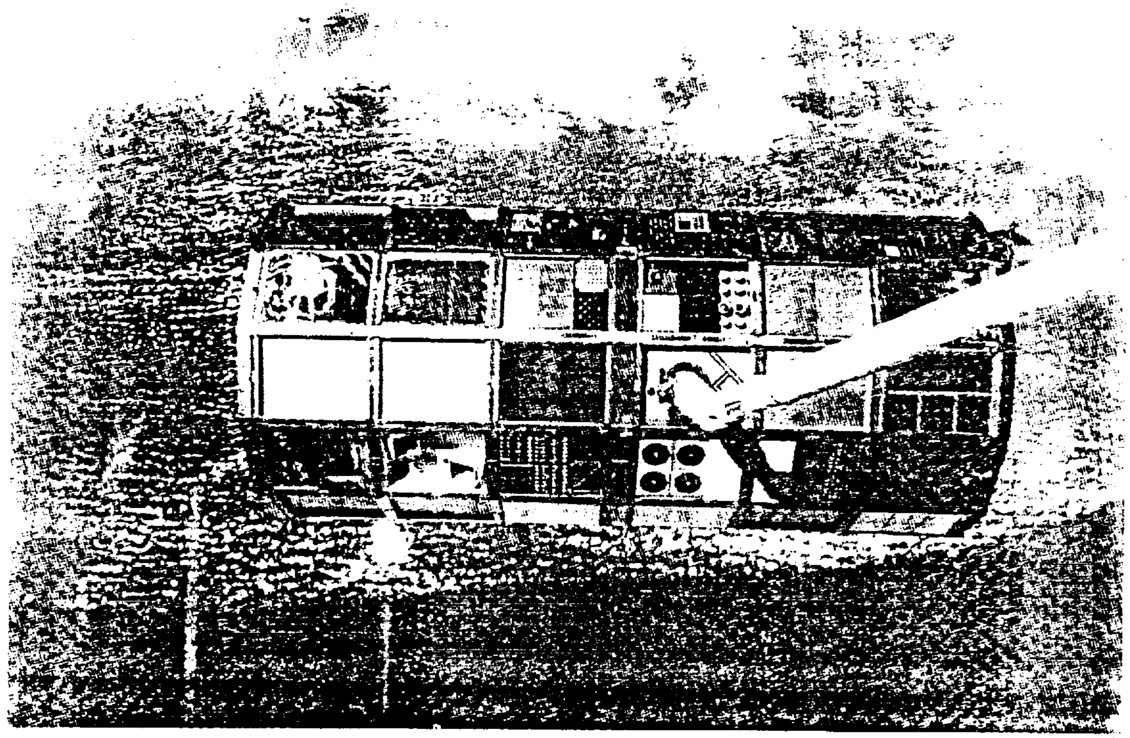
Honeywell

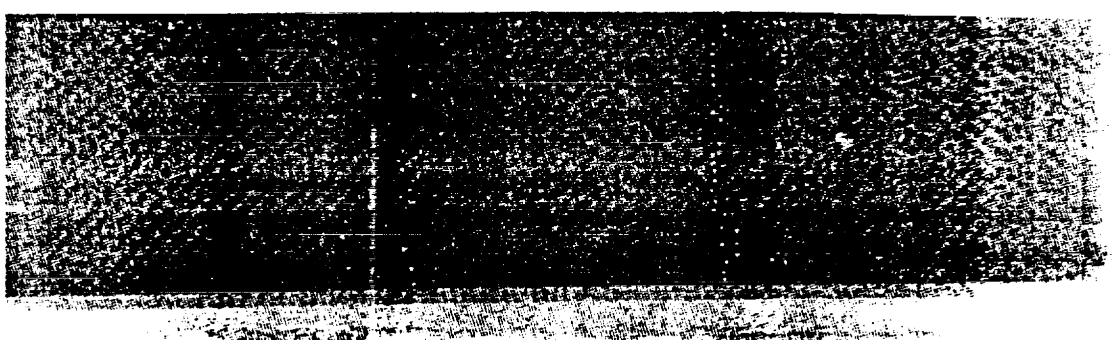
NASA

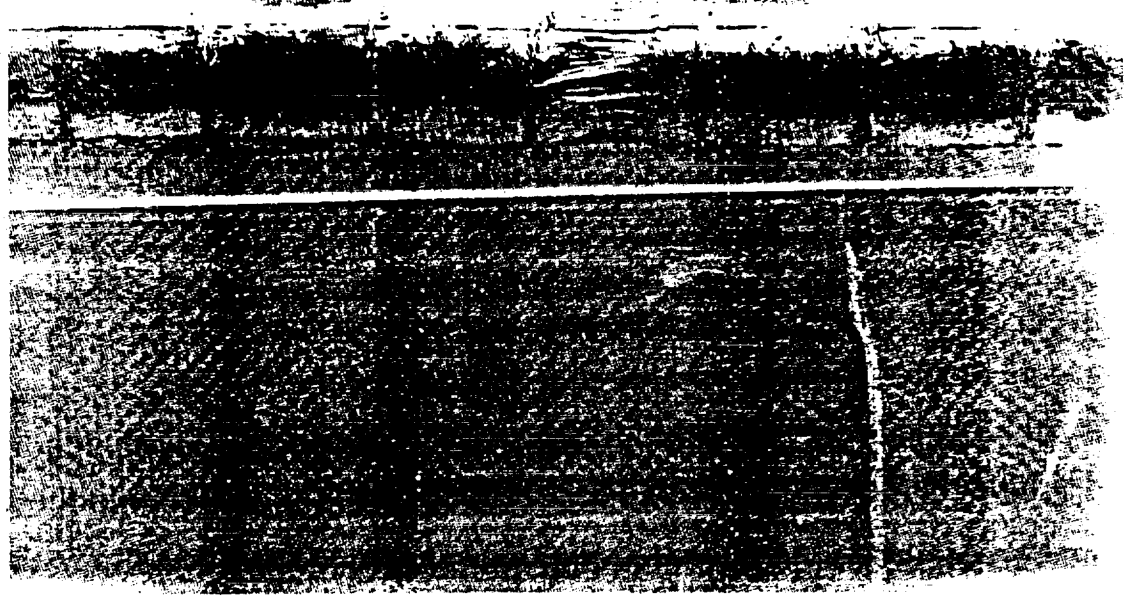


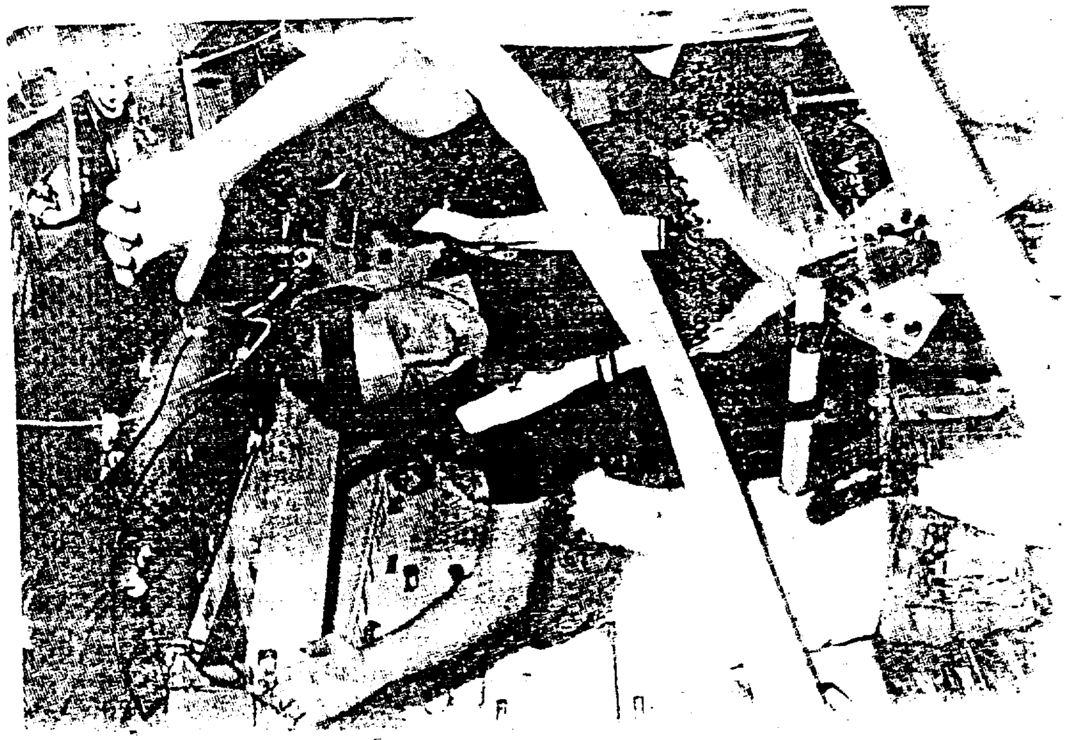

Honeywell
In-Space Accelerometer
MICROGRAV
DISTURBAN
EXPERIMEN
FEA-3


FEA-3





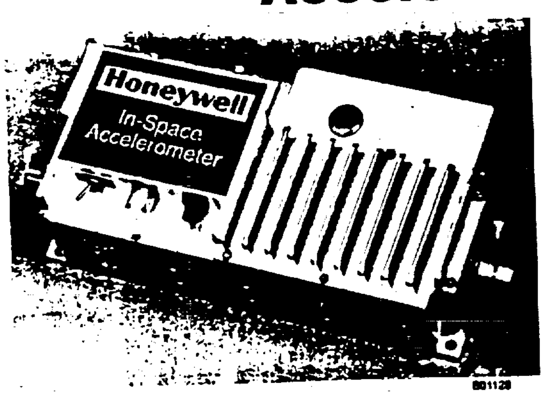




Honeywell In-Space Accelerometer

Honeywell

NASA

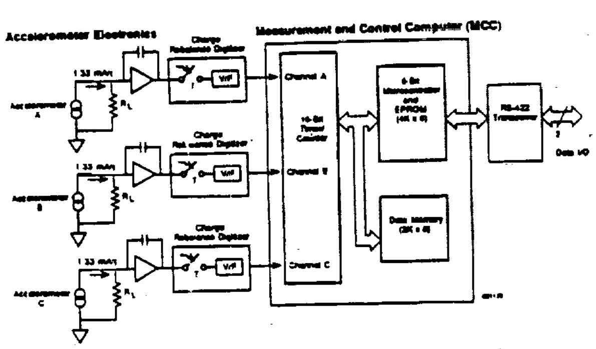


801128

In R&D since 1986

R&D Activities

- Fabricated 3-axis microgravity accelerometer with 1-μg resolution
- Flew on Atlantis STS-34 in Oct. '89 supporting polymer morphology experiment
- Flew on Columbia STS-32 in Jan. '90 supporting NASA JSC microgravity disturbances experiment



Applications

- Materials processing/life sciences experiments
- Structural truss monitoring
- Magnetic isolation and pointing systems

B910206

Honeywell In-Space Accelerometer Performance Specifications

Honeywell

NASA

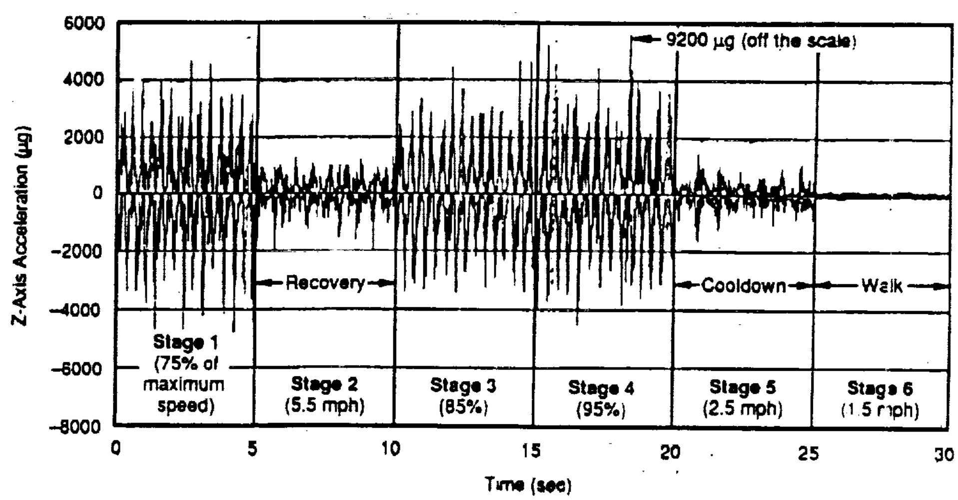
Parameter	Performance
Orientation	Three-axis orthogonal
Range	10^{-6} to 10^{-2} g (increments of 1.0×10^{-6} g) at 1 Hz; 10^{-5} to 10^{-2} g (increments of 9.0×10^{-6} g) at 50 Hz
Accuracy	±(1% |reading| + 0.00002) g
Resolution	<1.0 micro-g at 1 Hz 8.7 micro-g at 50 Hz
Frequency Response (±5%)	0.025 to 19.500 Hz
DC Bias	None (AC output)
Sample Data Rate	50 Hz, 1 Hz
Communications	RS-422/ASCII format
Size	8.0 x 3.8 x 2.1 in. (64 in.^{3})
Weight	4.0 lb.
Power	5.6W (@ 28V)

R910206 10

Middeck Treadmill Disturbance Data Recorded on Columbia STS-32 Flight

Honeywell

NASA

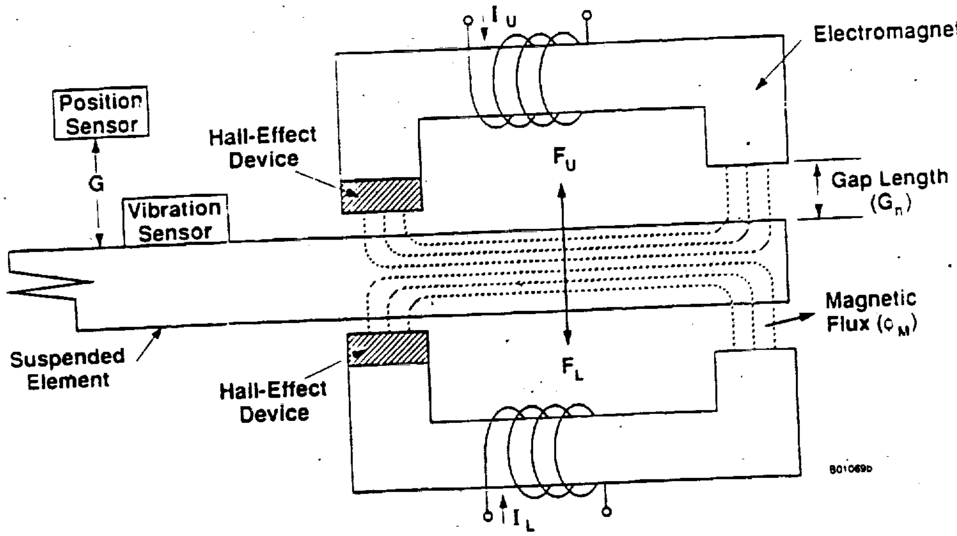


8910206 04

Flux-Feedback Magnetic Suspension Actuator

Honeywell

NASA



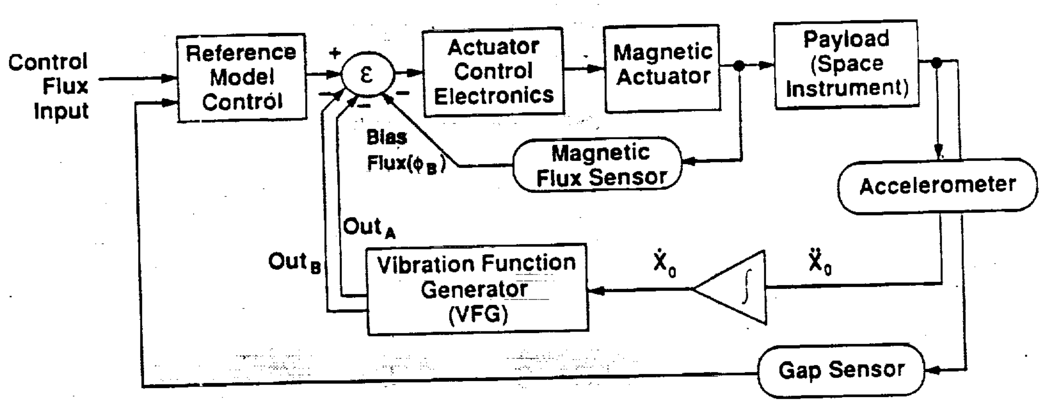
Highlights

- DC Isolator Capability– Using Honeywell In-Space Accelerometer (HISA) as inertial (vibration) feedback provides mechanical isolation down to DC
- Hall-Effect Sensing– Linear Output Hall-Effect Transducers (LOHETs) are used in electronic feedback control circuit to control suspension of ferromagnetic element between two magnets
- Flux-Feedback Control– Hall-Effect transducers measure magnetic force in upper magnetic (F_U) and lower magnet (F_L) to maintain constant flux independent of changes in gap between magnets

Adaptive Vibration Control System Concept

Honeywell

NASA



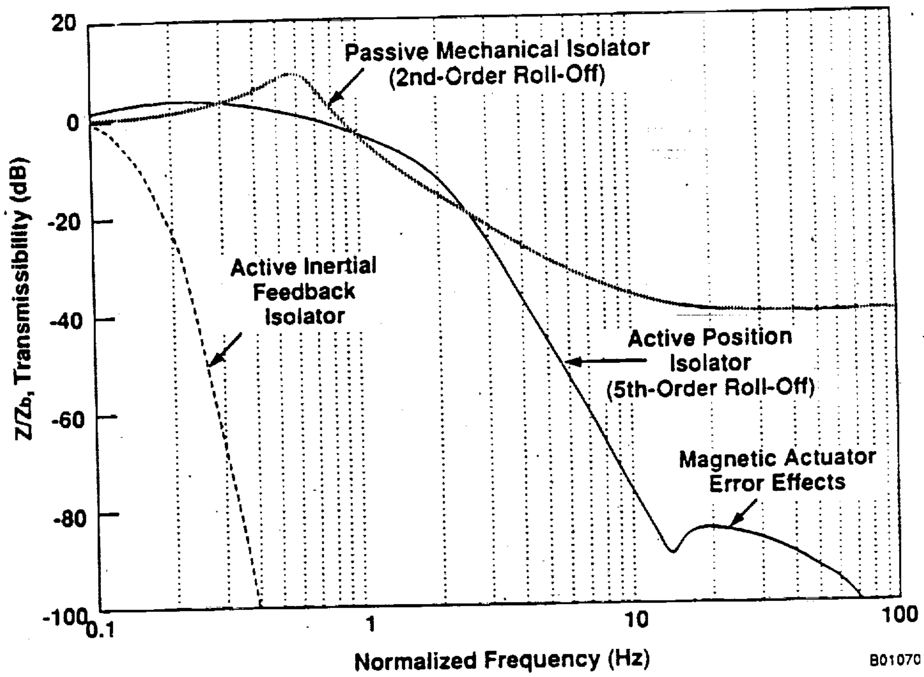
Key Features

- Three-Loop Response– Vibration effects on payload are eliminated by closed-loop response of actuator force (flux), vibration and gap displacement measurements
- DC Isolation Capability– Using inertial (vibration) feedback provides mechanical isolation down to DC
- Flux-Feedback– This concept uses a flux-feedback principle of operation to magnetically suspend and isolate payload

A Comparison of Passive and Active Isolator Technology

Honeywell

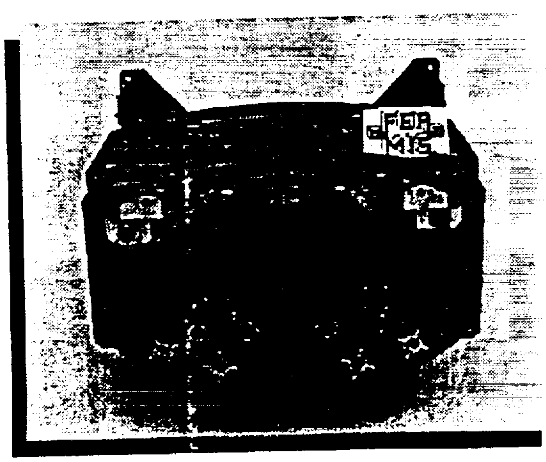
NASA



Fluid Experiment Apparatus Magnetic Isolation System (FEAMIS)

SPACE SYSTEMS GROUP

SATELLITE SYSTEMS OPERATIONS



Honeywell

STS-32 Accelerometer Characterization Summary

Honeywell

NASA

STS-32 Mission Results

- More than four hours (6 million bytes) of microgravity disturbances data was successfully recorded by HISA
- Orbiter microgravity disturbances due to crew background activity, treadmill exercise activity, and orbiter engine burns were recorded and analyzed
- Microgravity acceleration levels of 25 μg (quiescent period) to 9200 μg (peak treadmill event) were acquired

Future Activities

- Honeywell is considering the production of a low-cost version of the HISA electronics for microgravity investigators
- Honeywell is investigating the incorporation of delta-velocity sensor data into magnetic isolation systems to provide adaptive vibration isolation capability (<1 Hz frequency response)

B910206-22

STS-32 Accelerometer Characterization Summary

Honeywell

NASA

STS-32 Mission Results

- More than four hours (6 million bytes) of microgravity disturbances data was successfully recorded by HISA
- Orbiter microgravity disturbances due to crew background activity, treadmill exercise activity, and orbiter engine burns were recorded and analyzed
- Microgravity acceleration levels of 25 μg (quiescent period) to 9200 μg (peak treadmill event) were acquired

Future Activities

- Honeywell is considering the production of a low-cost version of the HISA electronics for microgravity investigators
- Honeywell is investigating the incorporation of delta-velocity sensor data into magnetic isolation systems to provide adaptive vibration isolation capability (<1 Hz frequency response)

R910206-72

DEVELOPMENT OF A RESIDUAL ACCELERATION DATA REDUCTION AND DISSEMINATION PLAN

N92-28453

Melissa J. B. Rogers
Center for Microgravity and Materials Research
University of Alabama in Huntsville
Huntsville, Alabama 35899

ABSTRACT

A major obstacle in evaluating the residual acceleration environment in an orbiting space laboratory is the amount of data collected during a given mission: gigabytes of data will be available as SAMS units begin to fly regularly. Investigators taking advantage of the reduced gravity conditions of space should not be overwhelmed by the accelerometer data which describe these conditions. We are therefore developing a data reduction and analysis plan that will allow principal investigators of low-g experiments to create experiment specific residual acceleration data bases for post-flight analysis. The basic aspects of the plan can also be used to characterize the acceleration environment of earth orbiting laboratories.

Our development of the reduction plan is based on the following program of research:

- The identification of experiment sensitivities by order of magnitude estimates and numerical modelling [1],
- Evaluation of various signal processing techniques appropriate for the reduction, supplementation, and dissemination of residual acceleration data, and
- Testing and implementation of the plan on existing acceleration data bases.

Discussions of the basic analysis techniques we are using and of the results of our analysis of the Spacelab 3 data base can be found in references [2-5]. Three initial aspects of residual acceleration data that can be analyzed are the acceleration vector magnitude and orientation and the relative strengths of the frequency components that make up the data window of interest. The acceleration time history can be subjected to a variety of statistical analyses and can be manipulated into a range of data presentation styles aimed at the identification of potentially intolerable acceleration events while reducing the number of data points plotted.

The orientation of the residual acceleration vector with respect to some set of coordinate axes is important for experiments with known directional sensitivity. Orientation information can be obtained from the evaluation of direction cosines.

Fourier analysis is commonly used to transform time history data into the frequency domain. Common spectral representations are the amplitude spectrum which gives the average of the components of the time series at each frequency and the power spectral density which indicates the power or energy present in the series per unit frequency interval.

The data reduction and analysis scheme developed involves a two tiered structure to 1) identify experiment characteristics and mission events that can be used to limit the amount of accelerometer data an investigator should be interested in and 2) process the data in a way that will be meaningful to the experiment objectives. A general outline of the plan follows:

LEVEL ONE

1. Pre-flight identification of acceleration sensitivity to determine frequency and magnitude ranges of interest and experiment tolerance limits.

2. Pre-flight identification of times at which the experiment is liable to be vulnerable, i.e., some experiments may be more sensitive at specific stages (e.g. protein crystal growth during the nucleation stage).

3. Preliminary post-flight analysis of experimental results to identify times when unexpected results occurred that may be related to perturbations in the residual acceleration environment of the laboratory.

LEVEL TWO

1. Selection of time windows of interest using a threshold detection routine based on sensitivities identified in Level One, Step 1 above.

2. Use of data decimation techniques, when appropriate, to reduce the number of data points needed to evaluate lengthy windows of data.

3. Specific analysis of windows of data identified in Level One and the first step of Level Two, including estimation of mean and mean squared values, determination of the acceleration vector orientation, and spectral analysis to investigate the magnitude of the frequency components for the specific time window of interest.

4. Evaluation of accelerometer data in conjunction with experimental results to identify causal relationships and revise sensitivity limits.

Cross-correlation analysis of accelerometer data and experimental output is suggested as a viable means of identifying causal relationships between specific acceleration events and noticeable experiment perturbations [4].

We have devised a contact sheet for IML1 principal investigators that gives an overview of the basic types of residual acceleration data processing that can be useful, including example plots. In order to make this more meaningful to the investigators, we have suggested specific data windows that should be of interest to them, based on the current mission timeline and our evaluation of their experiment sensitivity to acceleration. The use of such a plan will make the evaluation of the residual acceleration environment during a particular experiment considerably less time consuming than processing the entire accelerometer data base.

REFERENCES

[1] Alexander, J. I. D., Low-gravity Experiment Sensitivity to Residual Acceleration: A Review, **Microgravity Sci. Technol. III** (1990) 52.

[2] Rogers, M. J. B. and Alexander, J. I. D., Analysis of Spacelab 3 Residual Acceleration Data, **J. Spacecraft and Rockets** (1991), to be published May/June 1991.

[3] Rogers, M. J. B. and Alexander, J. I. D., A Strategy for Residual Acceleration Data Reduction and Dissemination, **Proceedings of the 28th COSPAR Plenary Meeting, Advances in Space Research** (1991) to be published.

[4] Rogers, M. J. B. and Alexander, J. I. D., Cross-correlation Analysis of On-orbit Residual Accelerations in Spacelab, **AIAA Paper 91-0348**, presented at the AIAA 29th Aerospace Sciences Meeting, 7-10 January 1991, Reno, Nevada.

[5] Rogers, M. J. B., Alexander, J. I. D., and Snyder, R. S., Analysis Techniques for Residual Acceleration Data, NASA **TM-103507**, July 1990.

Development of a Residual Acceleration Data Reduction and Dissemination Plan

Melissa J. B. Rogers

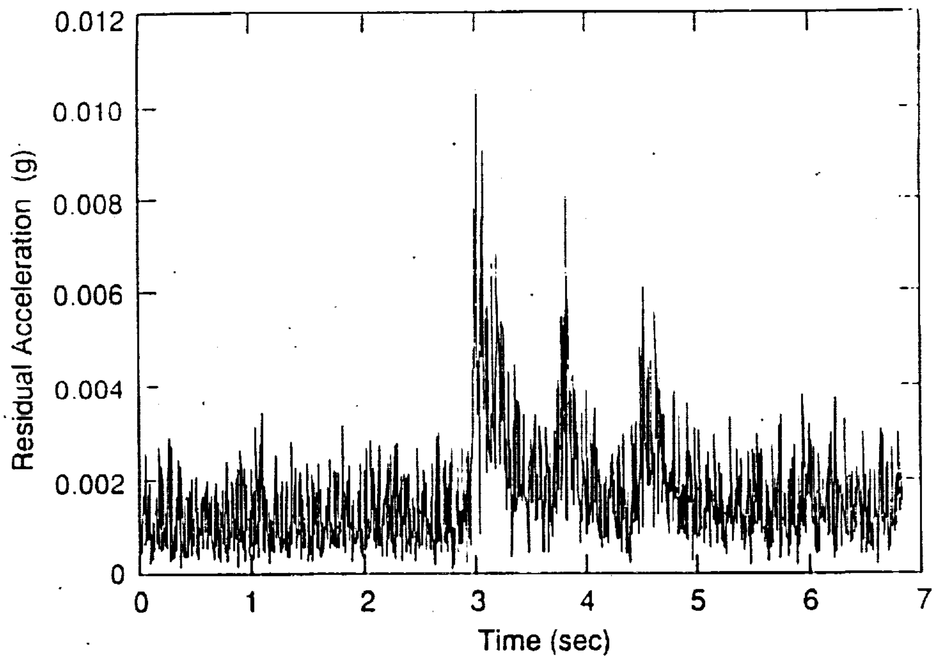
23 - 25 April 1991
Int'l. Workshop on Vibration Isolation Technology
NASA Lewis Research Center
Cleveland, Ohio

The University
Of Alabama
In Huntsville

Center for Microgravity and Materials Research

PROJECT GOALS

- create user specific accelerometer data base for post-flight analysis of experiments
- assist in characterization of low-gravity environment of orbiting space laboratories
- diminish the size of record while maintaining desired temporal coverage and fidelity
- provide the ability to rapidly identify time periods of interest
- make extraction of detailed information from raw data base an easier task



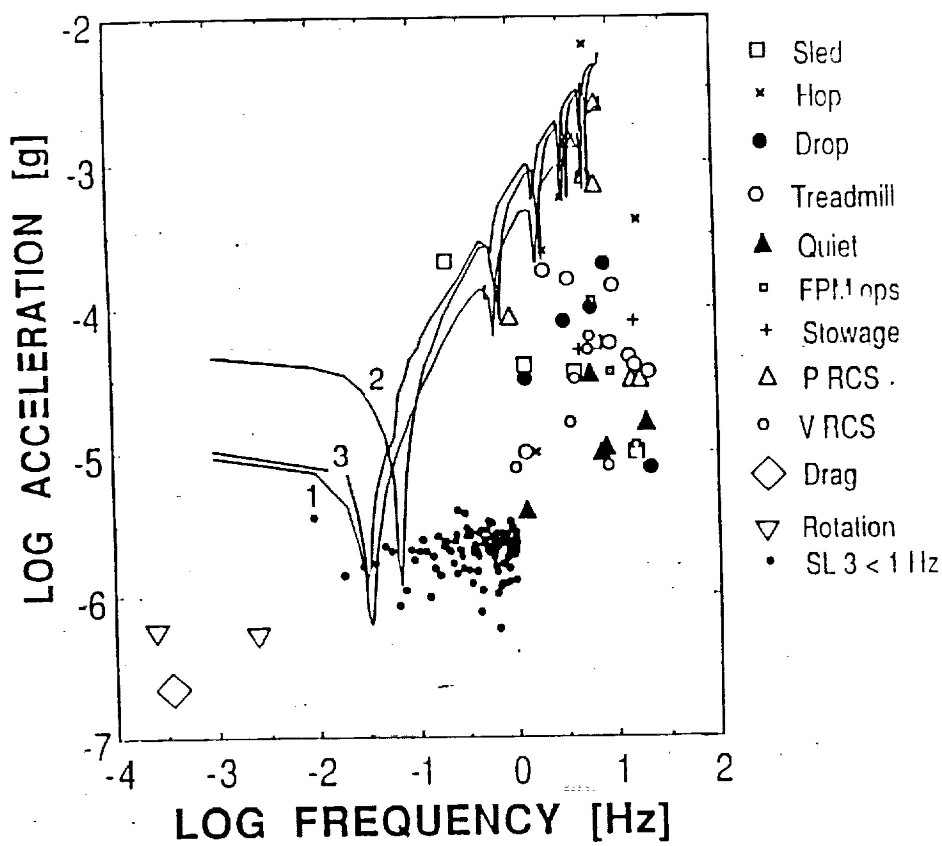
2048 Points out of 1.5×10^8 Data Points from Spacelab 3.
Up to 2 Gigabytes of Raw Data Expected from SAMS.

DEVELOPMENT OF THE DATA REDUCTION PLAN

The development stages of the data reduction plan are focused on the SL3 acceleration data base. The TGS crystal growth experiment was flown on SL3 in conjunction with the accelerometer. We therefore initially centered our attention on tolerance limits of the TGS crystal growth experiment.

We will look at 3 basic aspects of residual acceleration using 3 different techniques.

Acceleration Magnitude	Peak Detection
Frequency Components	Fourier Analysis
Acceleration Orientation	Direction Cosines



TGS SENSITIVITY*

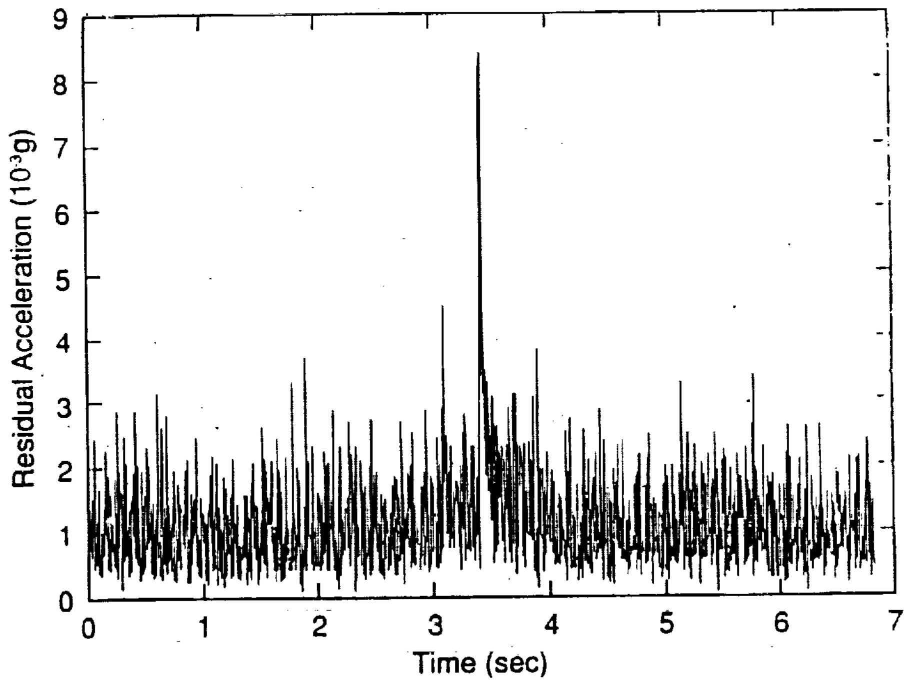
Maximum tolerable acceleration magnitude: 1×10^{-3} g

Maximum tolerable steady acceleration: 1×10^{-4} g

Maximum tolerable acceleration levels for given frequencies

Frequency (Hz)	Magnitude of Component (g)
$<10^{-2}$	10^{-4}
10^{-2} - 1.0	10^{-3}
>1.0	10^{-2}

* Nadarajah et al, J. Crystal Growth 104 (1990) 218.



Peak Detection Routine: Identify $|g| \geq 6 \times 10^{-3}$g

The University Of Alabama In Huntsville

Center for Microgravity and Materials Research

WHAT DOES A TIME WINDOW TELL US?

PEAK DETECTION

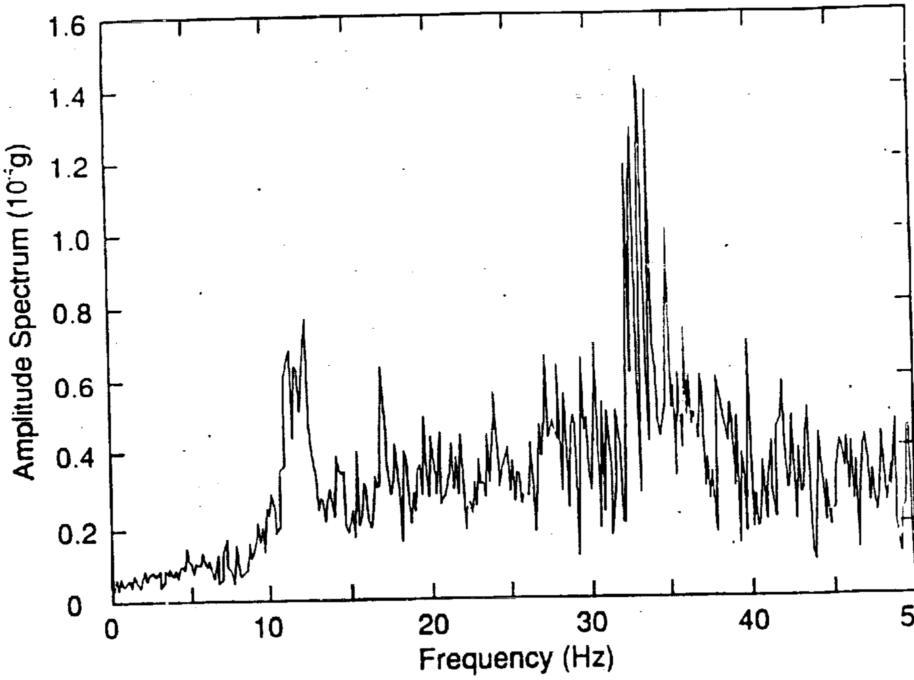
- maximum accelerations experienced
- identification of intolerable accelerations
- general character of background acceleration level and response to specific stimuli

INTEGRATION TECHNIQUES

- average characterization over specified time window
- limited amount of data to peruse for initial look at the acceleration environment

The University Of Alabama In Huntsville

Center for Microgravity and Materials Research



Fourier Transform Time Window to Identify Frequency Components

WHAT DO AMPLITUDE SPECTRA AND POWER SPECTRA TELL US?

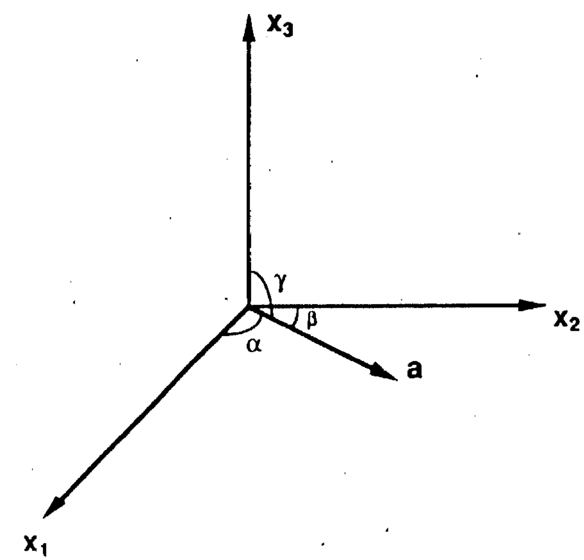
- Presence of particular frequency components in signal
- Identification of intolerable accelerations
- Acceleration environment associated with particular sources
- Identification of noisy equipment and activity to be avoided
- Indication of the power/energy of the time window

Orientation of the Residual Acceleration Vector

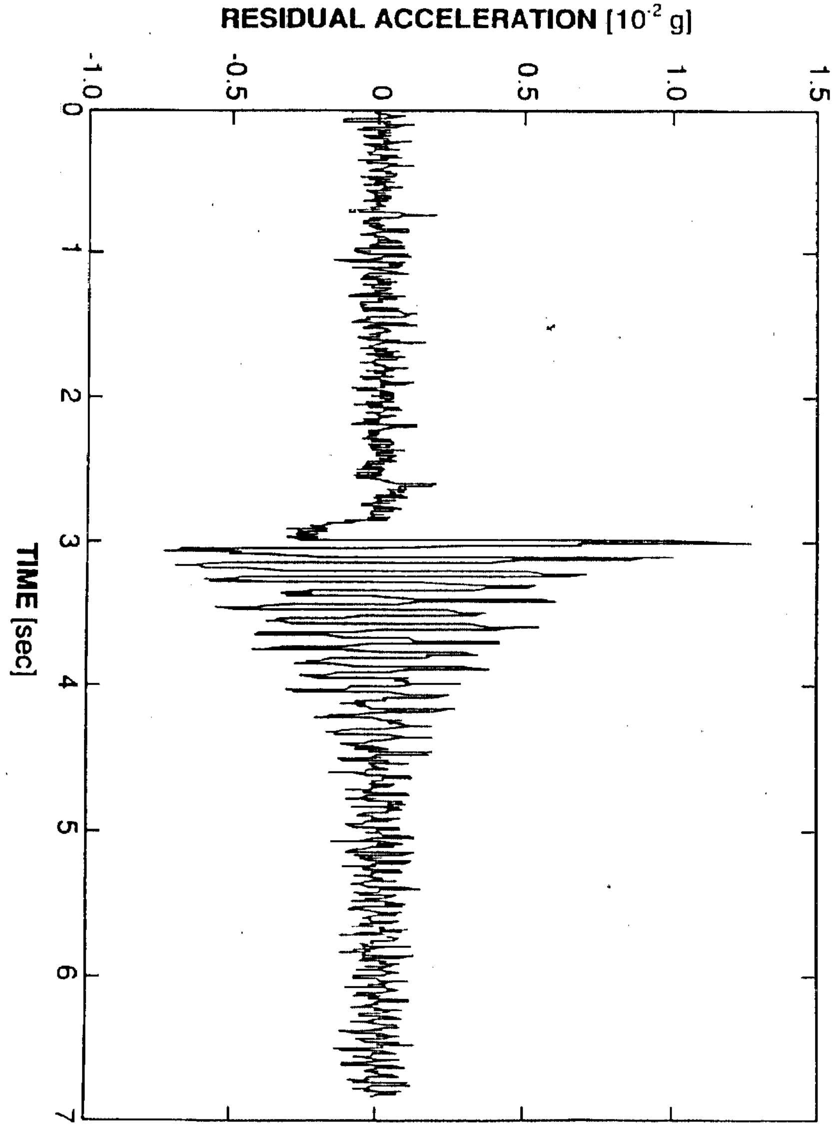
Direction cosines are used to determine orientation of measured accelerations with respect to the recording axes.

$\cos \alpha = a_1/a$ $\qquad$ $a = [a_1^2 + a_2^2 + a_3^2]^{1/2}$

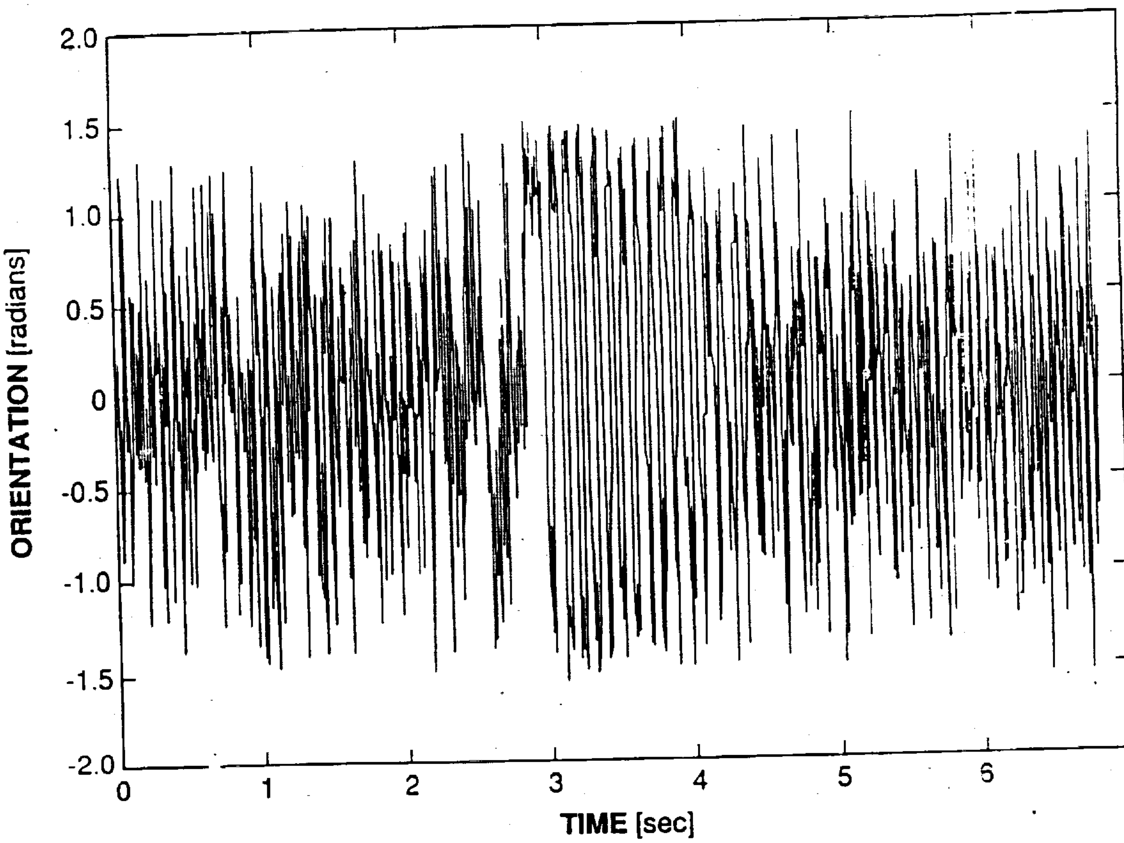
α is the angle between **a** and the x_1-axis.



The University Of Alabama In Huntsville $\quad$ Center for Microgravity and Materials Research



Q-5

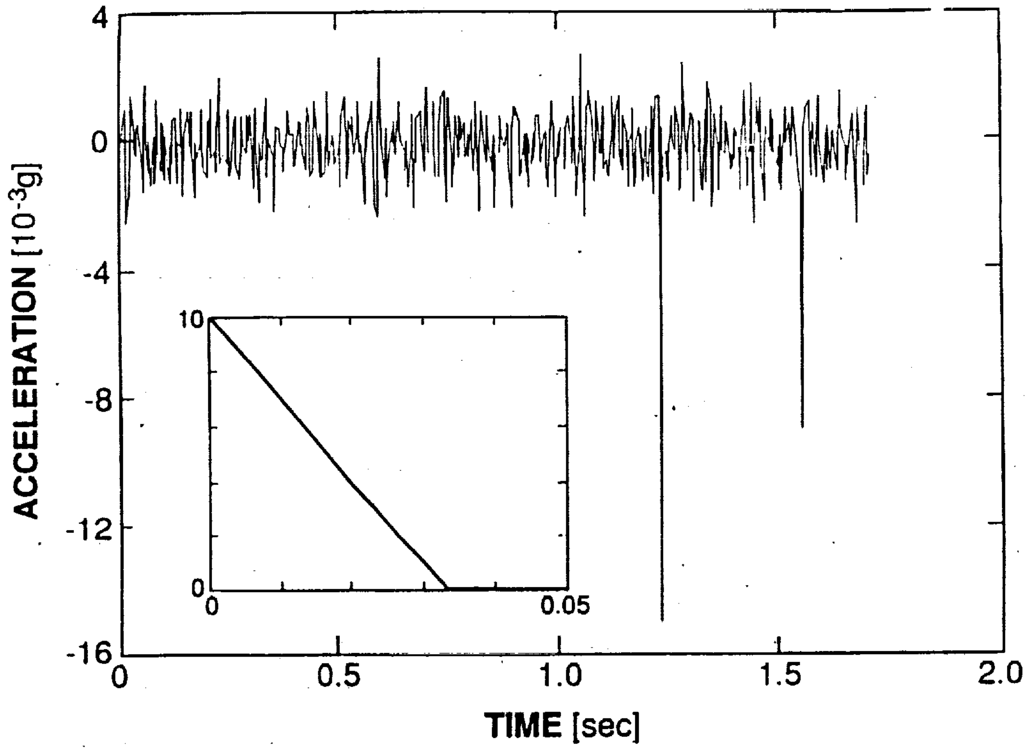


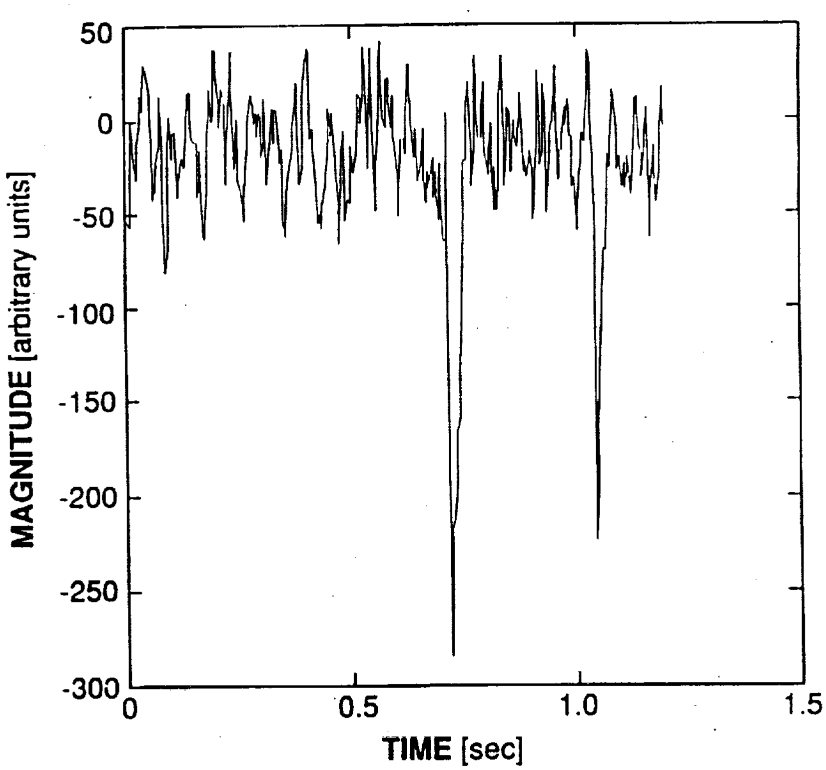
Cross-correlation Function

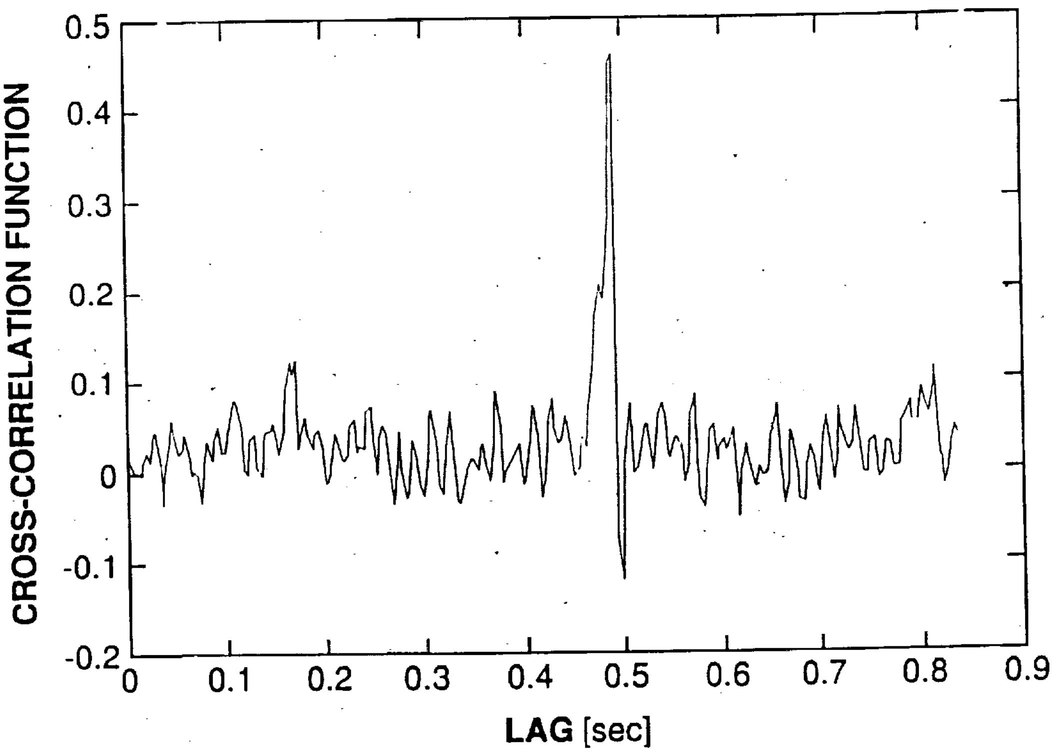
$$\phi_{12}(\tau) = \frac{1}{N}\sum_{t=1}^{N} f_1(t)\ f_2(t+\tau)$$

Normalized Cross-correlation Function

$$\rho_{12}(\tau) = \frac{\phi_{12}(\tau)}{\left[\phi_{11}(0)\ \phi_{22}(0)\right]^{1/2}}$$


4
0
-4
-8
-12
-16
ACCELERATION [10-3g]
10
0
0
0.05
0
0.5
1.0
1.5
2.0
TIME [sec]


50
0
-50
-100
-150
-200
-250
-300
MAGNITUDE [arbitrary units]
0
0.5
1.0
1.5
TIME [sec]



Application to Residual Acceleration Data

- Experiment must lend itself to cross-correlation analysis
- Experiment can be made appropriate by identification of key parameters
- Experiment time series can be created from qualitative results
- Modelling can be used to identify experiment response patterns (both form and time delay)

OUTLINE OF DATA REDUCTION PLAN

LEVEL ONE

1. Pre-flight identification of acceleration sensitivity to determine frequency and magnitude ranges of interest and experiment tolerance limits.
2. Pre-flight identification of times at which the experiment is liable to be most vulnerable, i.e., some experiments may be most sensitive at specific stages (e.g. protein crystal growth during the nucleation stage).
3. Preliminary post-flight analysis of experimental results to identify times when unexpected results occurred that may be related to perturbations in the residual acceleration environment of the laboratory.

OUTLINE OF DATA REDUCTION PLAN

LEVEL TWO

1. Selection of time windows of interest using a threshold detection routine based on sensitivities identified in Level One, Step 1 above.
2. Use of data decimation techniques, when appropriate, to reduce the number of data points needed to evaluate lengthy windows of data.
3. Specific analysis of windows of data identified in Level One and first step of Level Two, including estimation of mean and mean squared values, determination of the acceleration vector orientation, and spectral analysis to investigate the magnitude of the frequency components for the specific time window of interest.
4. Evaluation of accelerometer data in conjunction with experimental results to identify causal relationships and revise sensitivity limits.

ACCELERATION DATA ANALYSIS PLAN

(SUGGESTED APPROACH TO ACCELERATION DATA ANALYSIS)

EXPERIMENT:

PRINCIPAL INVESTIGATOR:

ADDRESS:

TELEPHONE:
FACSIMILE:

PRE-FLIGHT PLAN - Answers to the following questions will be used to select short windows for an overview of the low-gravity environment.

TIMESPAN OF EXPERIMENT:

LENGTH OF EXPERIMENT:

EXPERIMENT LOCATION:

SAMS SENSOR HEAD OF INTEREST, IF KNOWN:

NUMBER OF ACCELEROMETER DATA POINTS PER AXIS TO BE COLLECTED DURING EXPERIMENT

TIMES/REASONS OF KNOWN INCREASED EXPERIMENT SENSITIVITY:

DO YOU KNOW OF ANY POTENTIAL ACCELERATION SOURCES DURING YOUR EXPERIMENT? (eg, scheduled orbiter attitude adjustment)

POST-FLIGHT PLAN - For your experiment, we suggest analysis of data windows as indicated.

1. FREQUENCY AND MAGNITUDE RANGES OF INTEREST:

2. OVERALL MAXIMUM TOLERABLE ACCELERATION MAGNITUDE

   a) MAXIMUM CONTINUOUS (STEADY) TOLERABLE ACCELERATION

   b) MAXIMUM TOLERABLE ACCELERATION (PEAK DETECTION THRESHOLD)

3. EXPERIMENT SENSITIVITY TO CHANGES IN ACCELERATION ORIENTATION.

SUMMARY

- Data reduction plan developed based on experiment tolerance limits
- Use of the plan will allow creation of user specific accelerometer data bases for post-flight experiment analysis and orbiter characterization
- General data analysis scheme introduced involves:
  - threshold detection
  - Fourier analysis
  - evaluation of orientation
- Cross-correlation analysis is a viable means of assessing causal relationships
- Interested principal investigators to be contacted regarding experiment sensitivities and data requirements

SECTION IV

LIST OF ATTENDEES

International Workshop On

VIBRATION ISOLATION TECHNOLOGY for Microgravity Science Applications

April 23-25, 1991

Holiday Inn/Hopkins International Airport -- I-71 & Bagley Rd.
Middleburg Heights, Ohio

ATTENDEE LIST

Paul E. Allaire
University of Virginia
Mechanical Engineering Department
Thornton Hall, McCormick Rd.
Charlottesville, VA 22901
804-924-3292

Peter Allen
University of Virginia
Mech. Engr. Building, Thornton Hall
McCormick Rd.
Charlottesville, VA 22901

Gloria Badilla
JPL
M.S. 301-456
4800 Oak Grove Drive
Pasadena, CA 91109
818-249-5252

Bibhuti Banerjee
University of Virginia
Mech. Engr. Building Thornton Hall
McCormick Road
Charlottesville, VA 22901
804-924-6233

Charles Baugher
NASA Marshall Space Flight Center
ES71
MSFC, AL 35812
205-544-7417

David Beals
NASA Langley Research Center
M.S. 434
Hampton, VA 23665
804-864-7179

John Blackburn
Applied Technology Assoc.
1900 Randolph SE
Albuqerque, NM 87106
505-247-8371

Philip Bogert
NASA Headquaters
Code MSS-21
Washington, DC 20546
703-487-7679

Gerald Brown
NASA Lewis Research Center
M/S 23-3
21000 Brookpark Rd.
Cleveland, OH 44135
216-433-6047

Jim Brucher
Booz-Allen & Hamilton
NASA Headquarters/M8
4330 East-West Highway
Bethesda, MD 20814
301-951-4607

James E. Cake
NASA Lewis Research Center
M/S 500-205
21000 Brookpark Rd.
Cleveland, OH 44135
216-433-3565

Arnon Chait
NASA Lewis Research Center
M/S 105-1
21000 Brookpark Rd.
Cleveland, OH 44135

Benjamin Choi
NASA Lewis Research Center
M.S. 23-3
21000 Brookpark Rd.
Cleveland, OH 44135
216-433-6040

Michael Cork
ESA
Postbus 299
2200 AG Noordwijk
THE NETHERLANDS

Dan Curtis
Grumman
M/S 1313
1760 Business Center Dr.
Reston, VA 22090
703-438-5674

Richard DeLombard
NASA Lewis Research Center
M/S 500-217
21000 Brookpark Rd.
Cleveland, OH 44135
216-433-5285

Steve Del Basso
SSEIC/Grumman
MS-1313
1760 Business Center Dr.
Reston, VA 22090
703-438-5674

Ann Delleur
Sverdrup Technology, Inc.
2001 Aerospace Parkway
Brook Park, OH 44142
216-891-2901

Thierry Dewandre
European Space Agency
Noordwick
THE NETHERLANDS

Dirk Eilers
MBB ERNO
P.O. Box 105909
Hunefeldstr. 1-5
D-2800 Bremen 1, GERMANY
04215394390

David B. Eisenhaure
SatCon Technology Corporation
12 Emily Street
Cambridge, MA 02142
617-661-0540

Ralph Fenn
SatCon Technology
12 Emily Street
Cambridge, MA 02139
617-661-0540

Robert D. Galletly
JPL
M.S. 125-112
4800 Oak Grove Drive
Pasadena, CA 91109
818-354-3733

Thomas K. Glasgow
NASA Lewis Research Center
M.S. 105-1
21000 Brookpark Rd.
Cleveland, OH 44135
216-433-5013

Stuart Glazer
NASA Headquarters
Code SN
Washington, DC 20546
202-863-1223

Randy Goucher
Applied Technology, Assoc.
1900 Randolph Rd. SE
Albuquerque, NM 87106
505-247-8371

Robert Grant
Sverdrup Technology, Inc.
2001 Aerospace Parkway
Brook Park, OH 44142
216-891-2299

Carlos Grodsinsky
NASA Lewis Research Center
M/S 500-205
21000 Brookpark Rd.
Cleveland, OH 44135
216-433-2664

Nelson J. Groom
NASA Langley Research Center
M.S. 161
Hampton, VA 23665
804-864-6613

Michael Guerrero
G.E. Government Service
NASA Ames Research Center
M.S. 244-2A
Moffett Field, CA 94035
415-604-6048

David Hampton
University of Virginia
ROMAC Labs, Mech & Aero Engr
Thornton Hall, McCormick Rd.
Charlottesville, VA 22901

Doug Havenhill
Honeywell, Inc.
Space Systems Group
19019 North 59th Ave.
Glendale, AZ 85027
602-561-3208

David Haydu
Sverdrup Technology, Inc.
2001 Aerospace Parkway
Brook Park, OH 44142
216-826-6772

John Heese
Sverdrup Technology, Inc.
2001 Aerospace Parkway
Brook Park, OH 44142
216-891-2977

Steven Hegg
Sverdrup Technology, Inc.
2001 Aerospace Parkway
Brook Park, OH 44142
216-891-2976

Maurice Hiles
Technology 2000
234 Silver Valley Blvd.
Munroe Falls, OH 44262
216-688-9775

Myron Hill
NASA Lewis Research Center
M.S. 500-217
21000 Brookpark Rd.
Cleveland, OH 44135
216-433-5279

Ralph Hollis
IBM
Thomas J. Watson Research Center
P.O. Box 218
Yorktown Hts.,, NY 10598
914-945-2894

Michael Horkachuck
NASA Ames Research Center
M.S. 244-19
Moffett Field, CA 94035
415-604-3823

Seiko Hoshi
IHI
Tokyo-Choo Bldg. 6-2
Marunouchi 1-chome Chiyoda-ku
Tokyo 100, JAPAN
0332862049

Bruce G. Johnson
SatCon Technology Corporation
12 Emily Street
Cambridge, MA 02139
617-661-0540

Charlie Jones
NASA Marshall Space Flight Center
JA61
MSFC, AL 35812
FTS-824-1881

Mohammad Kassemi
NASA Lewis Research Center
M.S. 105-1
21000 Brookpark Rd.
Cleveland, OH 44135
216-433-5031

Dennis Kern
JPL
M/S 301-456
4800 Oak Grove Dr.
Pasadena, CA 91109
818-354-3158

Walter Knabe
MBB/ERNO
Honefeldstrasse 1
D-2800 Bremen
GERMANY,
011494215390

Prof. Carl Knospe
University of Virginia
Dept. of Mech. & Aerosp. Engr.
Thornton Hall, McCormick Rd.
Charlottesville, VA 22901
804-924-3292

Jean Koster
University of Colorado
Dept. of Aerospace Engineering Sciences
Campus Box 429
Boulder, CO 80309
303-492-6945

Kevin Kral
Honeywell Satellite Sys.
19019 N. 59th Ave.
Glendale, AZ 85308
602-561-3194

Charles Lawrence
NASA Lewis Research Center
M/S 23-3
21000 Brookpark Rd.
Cleveland, OH 44135
216-433-6048

Kirk Logsdon
NASA Lewis Research Center
M/S 500-205
21000 Brookpark Rd.
Cleveland, OH 44135
216-433-2836

Joseph F. Lubomski
NASA Lewis Research Center
M/S 500-205
21000 Brookpark Rd.
Cleveland, OH 44135
216-433-3907

Michael Lyban
Teledyne Brown Engineering
300 Sparkman Dr.
Huntsville, AL 35807-7007
205-726-6349

Gary Martin
NASA Headquarters
Code SN
Washington, DC 20024
202-453-1517

William Masica
NASA Lewis Research Center
M/S 500-205
21000 Brookpark Rd.
Cleveland, OH 44135
216-433-2864

Joseph Matus
McDonnell Douglas Space Systems
689 Discovery Dr.
MS 23A1
Huntsville, AL 35812
205-922-7731

George McCanless
NASA Marshall Space Flight Center
PS05
MSFC, AL 35812
205-544-3545

Jeffrey H. Miller
Sverdrup Technology, Inc.
NASA Lewis Research Center, M.S. 23-3
21000 Brookpark Rd.
Cleveland, OH 44135
216-433-6012

R. Gareth Owen
University of Wales
School of Electronic Engineering Sci.
Dean Street
Bangor, Gwynedd LL57 1UT, U.K.
0248135115

Michelle Oziomek
NASA Lewis Research Center
M.S. 105-1
21000 Brookpark Rd.
Cleveland, OH 44135
216-433-3785

Mary Palumbo
NASA Lewis Research Center
M/S 86-12
21000 Brookpark Rd.
Cleveland, OH 44135
216-433-2043

Ray Pavlik
Sverdrup Technology, Inc.
2001 Aerospace Parkway
Brook Park, OH 44142
216-891-2203

Andrew Peddie
Sverdrup Technology, Inc.
NASA Lewis Research Center, M.S. 500-217
21000 Brookpark Rd.
Cleveland, OH 44135
216-433-2873

N. Ramachandran
USRA
NASA Marshall Space Center
Mail Code ES42
MSFC, AL 35816
205-544-8308

Melissa Rogers
CMMR, UAH
Huntsville, AL 35899

Douglas A. Rohn
NASA Lewis Research Center
M/S 23-3
21000 Brookpark Rd.
Cleveland, OH 44135
216-433-3325

Kevin Romine
Sverdrup Technology, Inc.
2001 Aerospace Parkway
Brook Park, OH 44142
216-891-2979

Gil Santoro
Sverdrup Technology, Inc.
NASA Lewis Research Center, M.S. 500-205
21000 Brookpark Rd.
Cleveland, OH 44135
216-433-3598

Kevin Schaefer
Reston SSPO HQ
MSU-1
Reston, VA
703-487-7088

Jeff Schoess
Honeywell Corp.
Mail Stop MN09-D200
10701 Lyndale Ave. S
Minneapolis, MN 55420
612-887-4435

Nancy Shaw
NASA Lewis Research Center
M.S. 500-205
21000 Brookpark Rd.
Cleveland, OH 44135
216-433-3285

Peggy Shiflett
Applied Technology Associates
1900 Randolph Rd. SE
Albuquerque, NM 87106
505-247-8371

Prof. Alok Sinha
Pennsylvania State University
Mechanical Engineering Department
University Park, PA 16802
814-863-3079

Damon C. Smith
Lockheed Missiles and Space Company
6N40, Building 580
1111 Lockheed Way
Sunnyvale, CA 94089-3504
408-756-7632

Michael Szabo
Sverdrup Technology, Inc.
2001 Aerospace Parkway
Brook Park, OH 44142
216-891-2214

William E. Thornton
NASA Johnson Space Center
Mail Code CB
Houston, TX 77058

Paul A. Trimarchi
NASA Lewis Research Center
M/S 86-12
21000 Brookpark Rd.
Cleveland, OH 44135
216-433-3824

Bjarni Tryggvason
Canadian Space Agency
Canadian Astronaut Program Office
Building R-92 Rockcliffe Bass
Ottawa, Ontario K1A 0R6, CANADA
613-993-4106

Bob Williams
NASA Johnson Space Center
EK6
Houton, TX 77058
713-483-5772

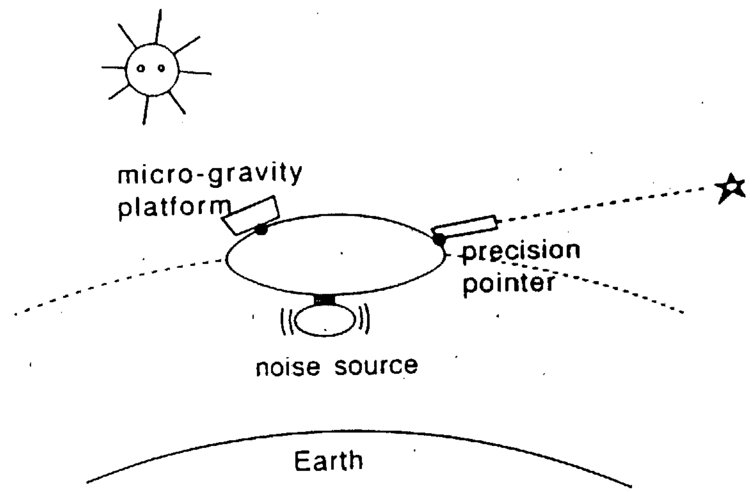
Andreas von Flotow
MIT
37-335
Cambridge, MA 02139
617-253-4865

SECTION V

APPENDIXES
ADDITIONAL PRESENTATIONS

N92-28454

MICROGRAVITY ISOLATION MOUNTS BASED UPON PIEZOELECTRIC FILM



Andreas von Flotow
Department of Aeronautics and Astronautics
Massachusetts Institute of Technology
Cambridge, MA 02139

(SPONSOR: McDonnell Douglas Space Systems Co.)

ACTIVE MICROGRAVITY VIBRATION ISOLATION USING PVDF POLYMER PIEZOELECTRIC ACTUATORS

by

DAVID SCOTT STAMPLEMAN

B.S., Massachusetts Institute of Technology (1986)

SUBMITTED IN PARTIAL FULFILLMENT OF THE REQUIREMENTS FOR THE DEGREE OF

MASTER OF SCIENCE IN AERONAUTICS AND ASTRONAUTICS

at the

MASSACHUSETTS INSTITUTE OF TECHNOLOGY

February 1991

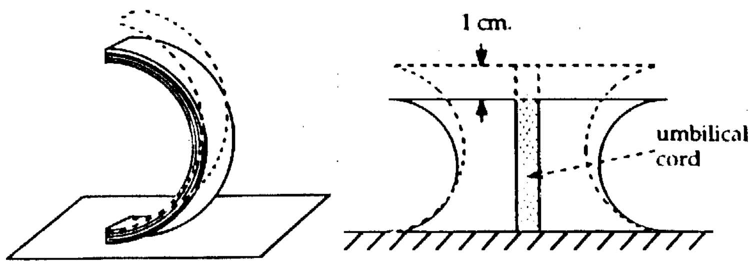
Signature of Author ____________________
Department of Aeronautics and Astronautics
January 22, 1991

Certified by ____________________
Professor Andreas H. von Flotow
Thesis Supervisor, Department of Aeronautics and Astronautics

Accepted by ____________________
Professor Harold Y Wachman
Chairman, Departmental Graduate Committee

Flotow, May 90 (12)

The PVDF Actuator



- Layered PVDF piezoelectric film used in a "bimorph" format
- Arcs can be used in various configurations as semi-active mounts

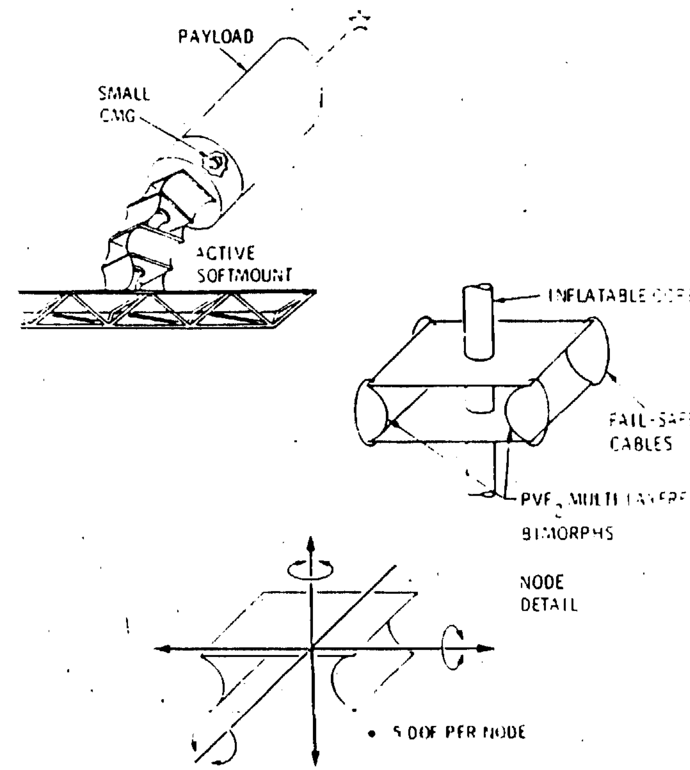
Each Arc:

- internal modes > 40 Hz
- can overpower itself for 1 cm. travel
- workable example:

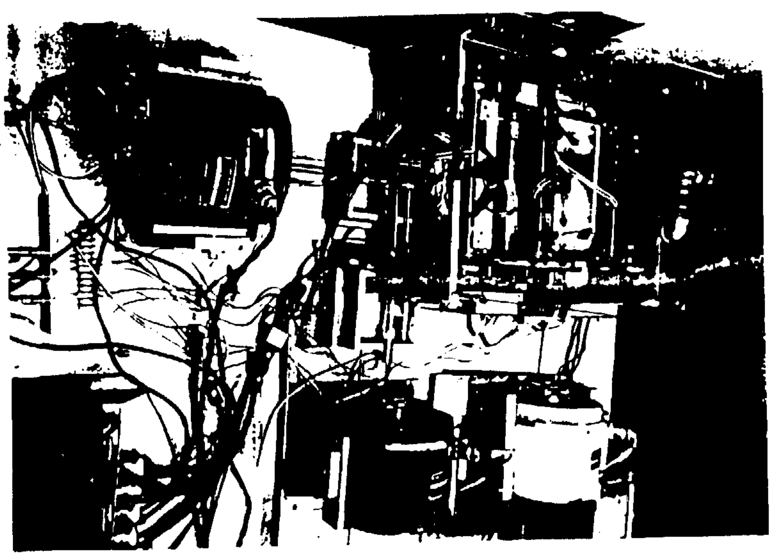
width	=	0.05 m
radius	=	0.04 m
layer thickness	=	28 µm
number of layers	=	6
force @ 1 cm. travel	=	0.12 N
piezoelectric induced force	=	0.13 N

8

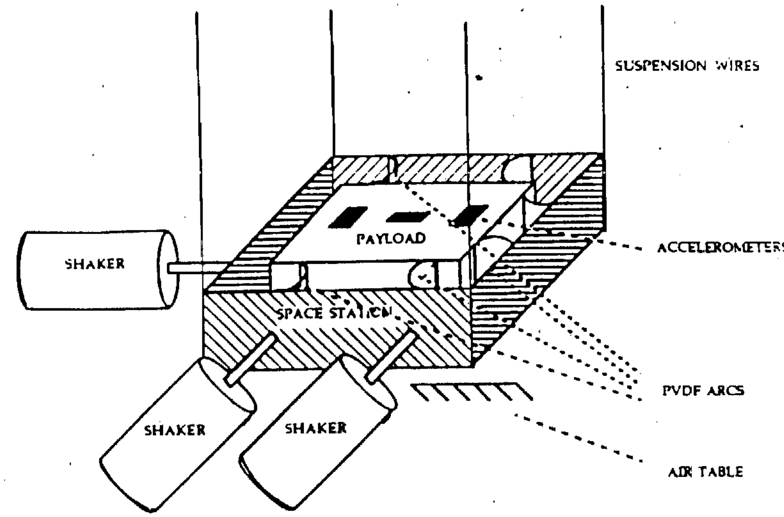
A SEMI - ACTIVE SOFT MOUNT



ref: S.W. Sirlin, "Piezoelectric Polymer-Based Isolation Mount for Articulated Pointing Systems on Large Flexible Spacecraft," AAS/AIAA Astrodynamics Conf., Kalispell, MT, Aug 1987



3-Axis Experiment



- Shaker simulates Space Station motion (3-axes)
- Payload rests on an air table .
- Accelerometers can resolve below 1 μg
- Control System minimizes the resulting acceleration of the payload (10 kg.)

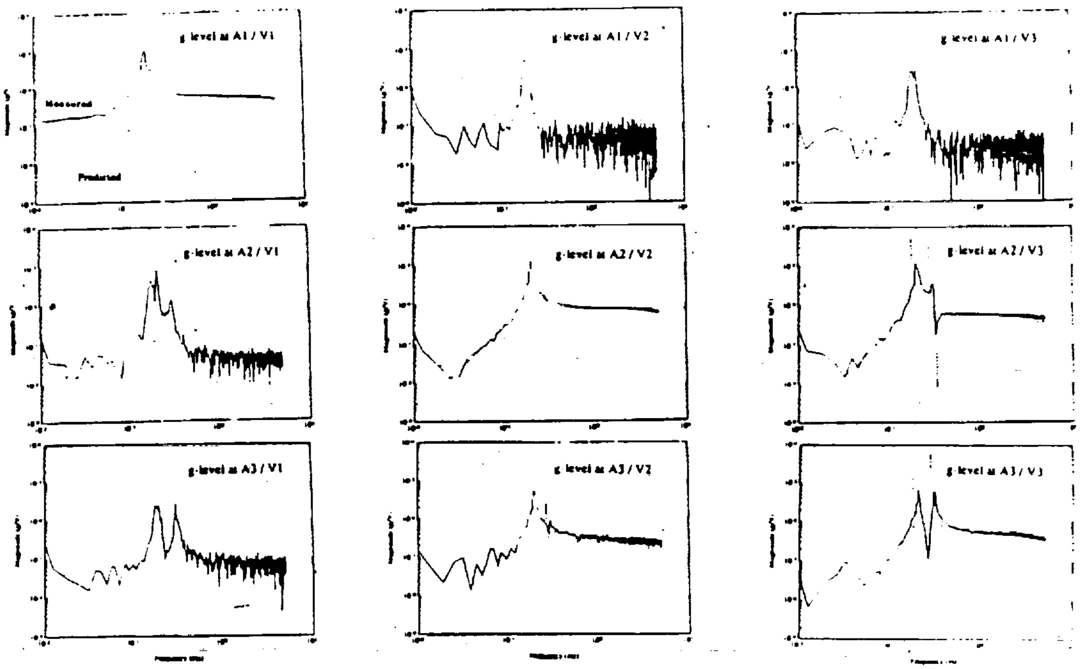


Figure 5.7: Matrix of transfer functions: Actuator group applied voltages to accelerometer acceleration in g's

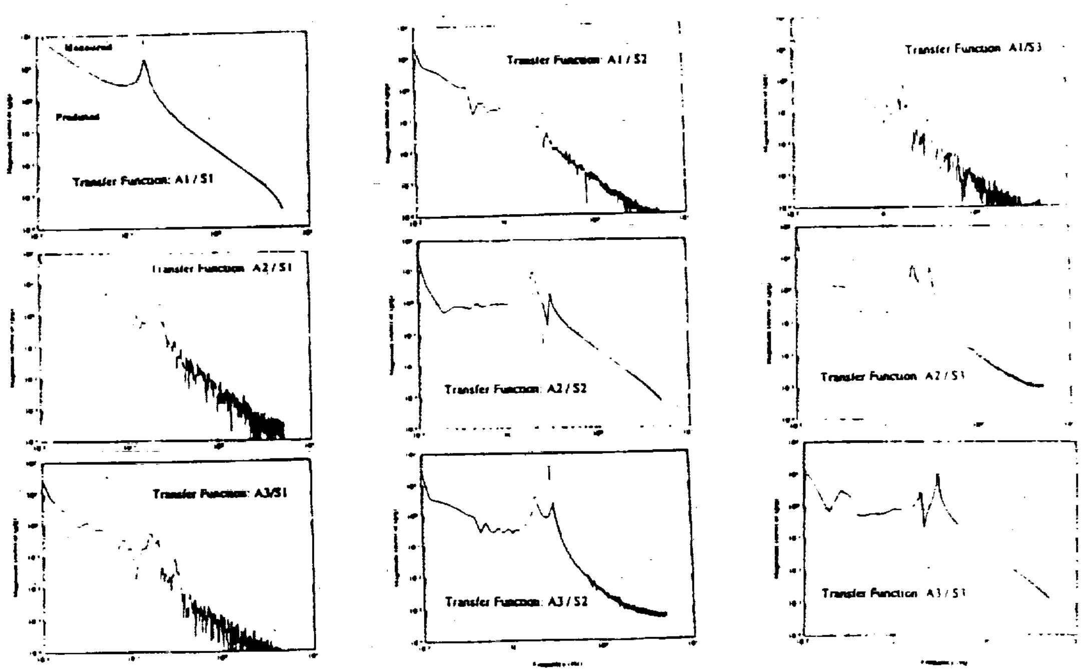
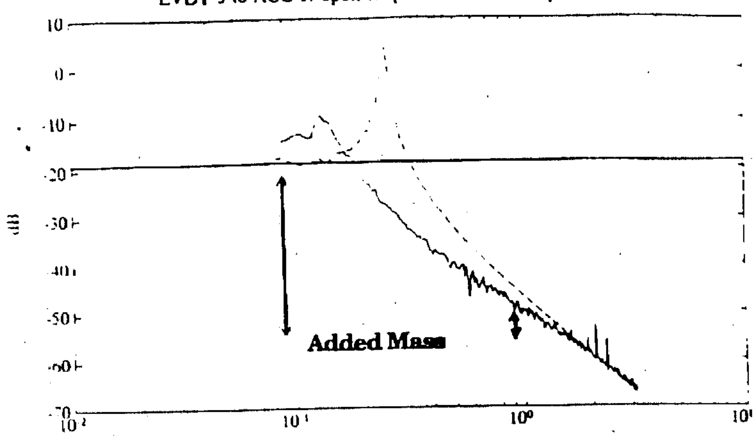
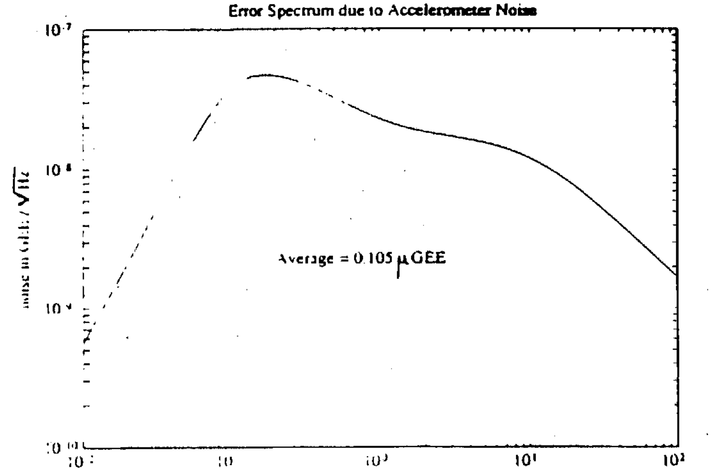


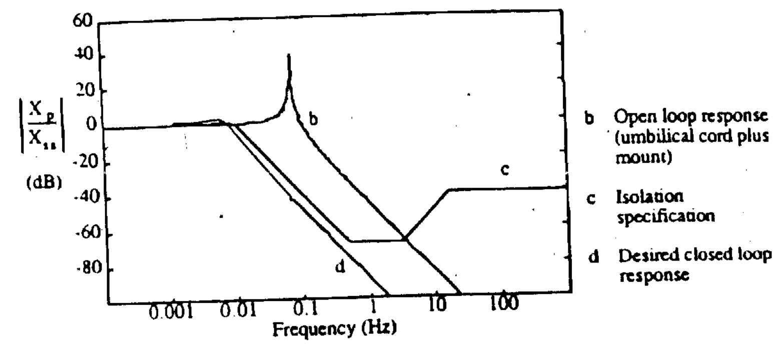
Figure 5.6: Matrix of transmissibilty transfer functions: Shaker head motion to accelerometer motion.

Low Frequency Disturbance Attenuation Using Sunstrand QA-2000 Accelerometers

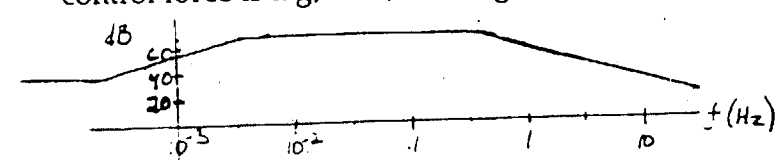




Control Problem (Each axis)



- Km ≈ 40 N/m to bury the uncertain umbilical cord dynamics
- Axes decoupled by using modal analysis
- At least a factor of 10 active enhancement of isolation is necessary between 0.01 Hz and 10 Hz
- Compensator (each axis) from accelerometer to control force is lag/lead,lead/lag



- Mount is essentially passive outside the active range of 0.01 Hz ≤ f ≤ 1 Hz

Summary

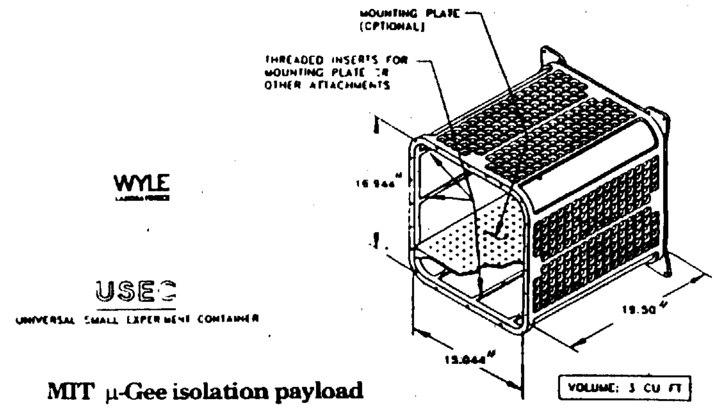
- Microgravity isolation is easily achievable
- Active isolation required by stiff umbilical, and for damping
- MIT's mount is passive with active enhancement at low frequency

Needed Development

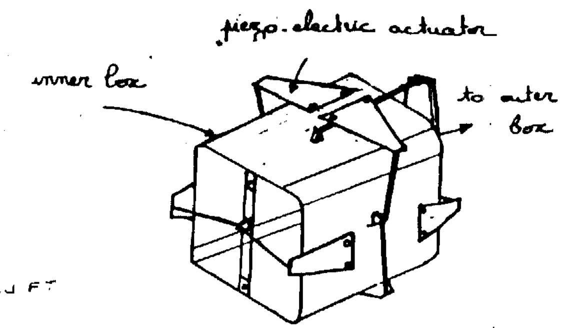
- Enthusiastic customer with payload specs
- Lower range accelerometers
- μ Gee umbilicals

A 6 Axis Laboratory Prototype

WYLE's Containment Vessel



MIT μ-Gee isolation payload



Preliminary laboratory testing complete in Sept 1991, 3 tests, 3 axis each

GAT SENSORS ?

N92-28455

PREDICTING MICROGRAVITY LEVELS FOR SPACE STATION USING VAPEPS

(VIBROACOUSTIC PAYLOAD ENVIRONMENT PREDICTION SYSTEM)

INTERNATIONAL WORKSHOP ON VIBRATION ISOLATION TECHNOLOGY

APRIL 23-25, 1991

NASA LEWIS RESEARCH CENTER

BY

G. BADILLA/T. BERGEN/D. KERN/T. SCHARTON
JET PROPULSION LABORATORY
PASADENA, CA

OUTLINE OF TOPICS COVERED

VAPEPS AND SEA DESCRIPTION

SPACE STATION MODEL

MICROGRAVITY AND ACOUSTIC RESULTS

CONCLUSIONS

VAPEPS DESCRIPTION

VibroAcoustic Payload Environment Prediction System

Computer Program and Database for vibroacoustic predictions

Statistical Energy Analysis techniques are used for vibracoustic predictions.

Maintained by The Jet Propulsion Laboratory

Under sponsorship of United States Air Force Space Division and NASA/Lewis Research Center

Code is free, existing, validated, comprehensive, aerospace oriented SEA code.

Support for start-ups, training, consulting, and updates is provided.

Code is available to all: non-proprietory; ESA and CSA have requested and received copies of VAPEPS.

OBJECTIVES OF JPL SPACE STATION ANALYSIS EFFORT

To develop a computer model for assessing and controlling the acoustic and microgravity environment of space station.

To provide the model and technical assistance to other NASA centers and space station contractors

STATISTICAL ENERGY ANALYSIS (SEA)

Developed by R. H. Lyon and colleagues at BBN in 1960's

Vibroacoustic analysis tool to support design of complex systems

Particularly useful during preliminary design when structural details are not yet available

Wide spread applications to aerospace vehicles, ships, automobiles, and room acoustics

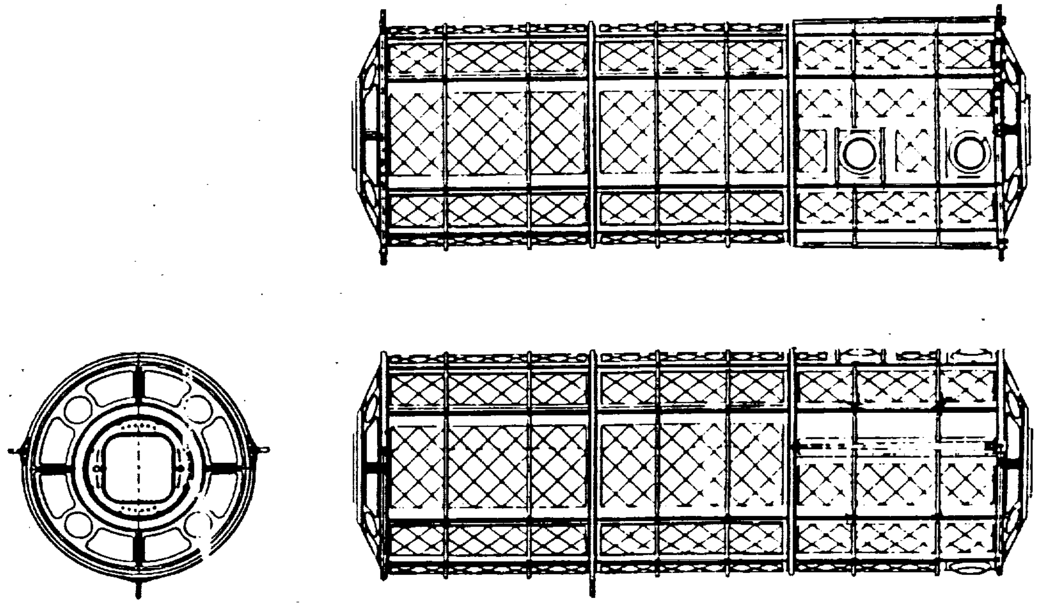
---

VAPEPS SPACE STATION FREEDOM MODEL DESCRIPTION

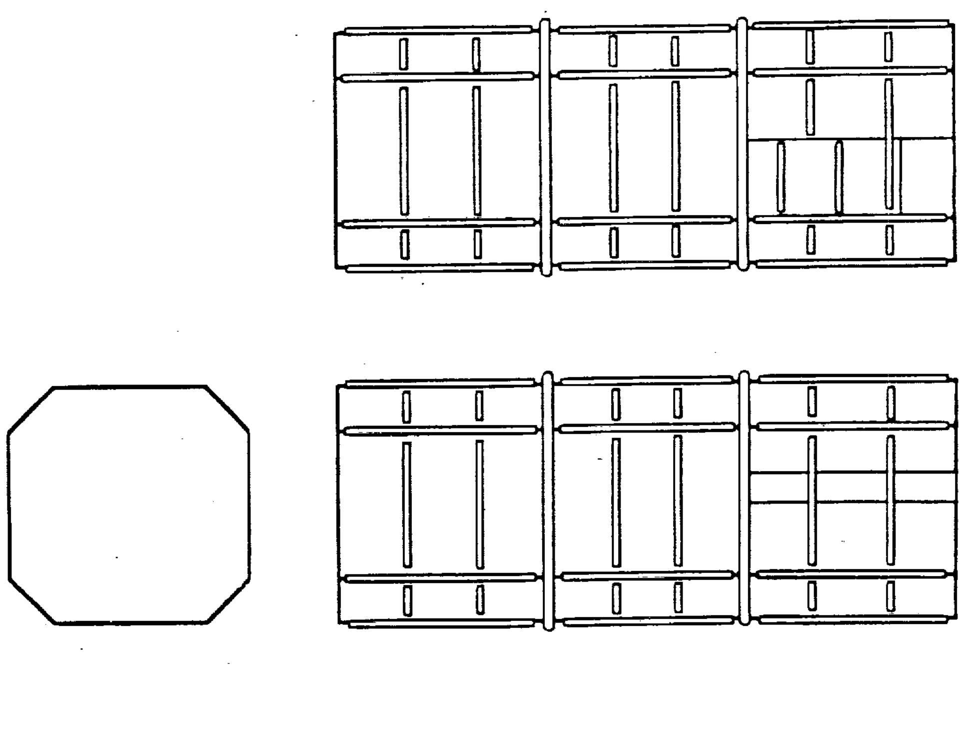
BASELINE MODEL

SOURCES

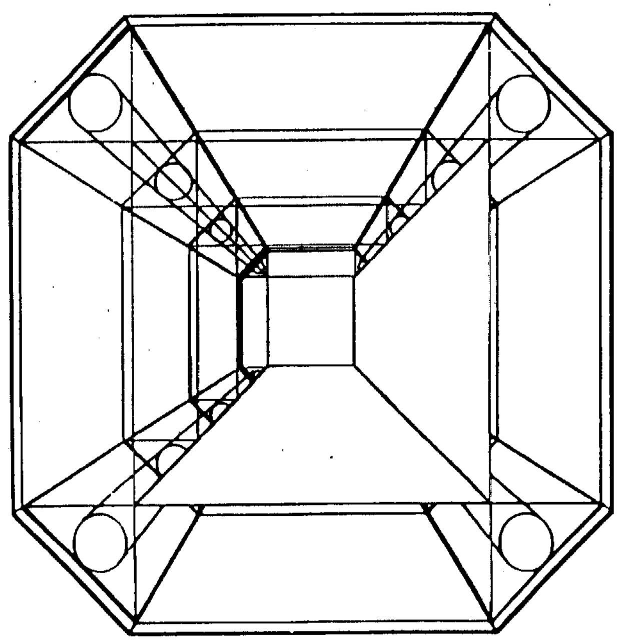
TREATMENTS



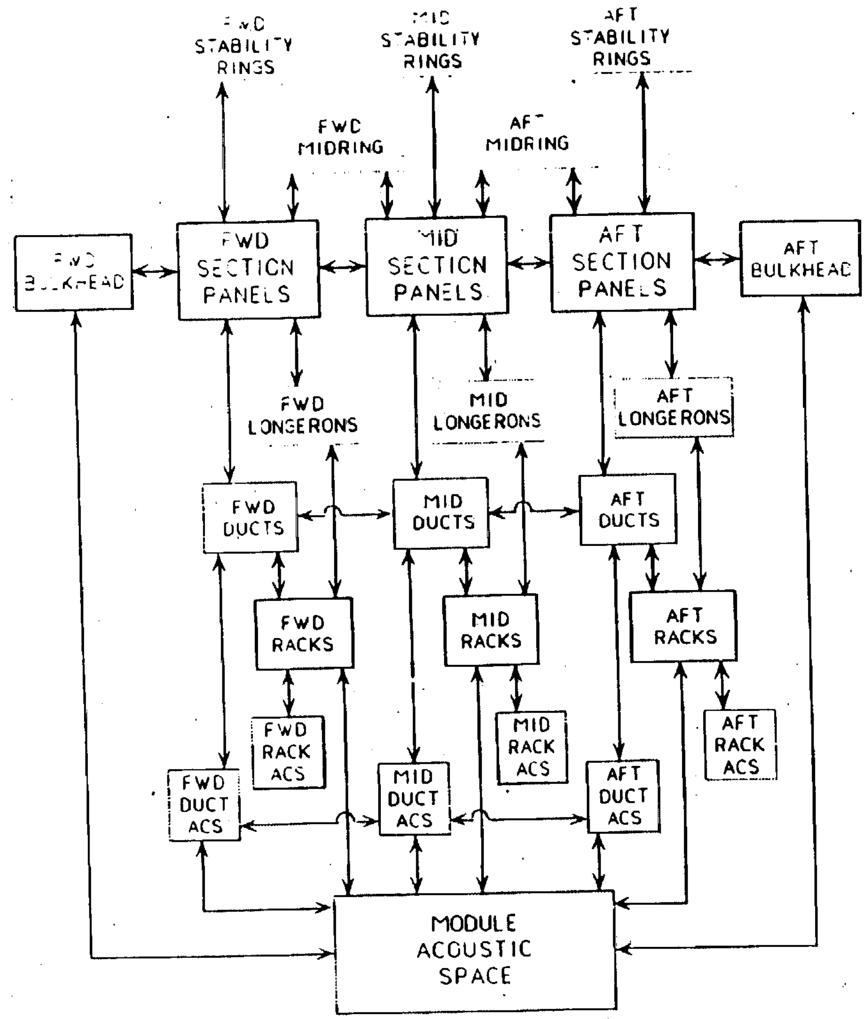
ENGINEERING DRAWING OF SSF HABITATION MODULE



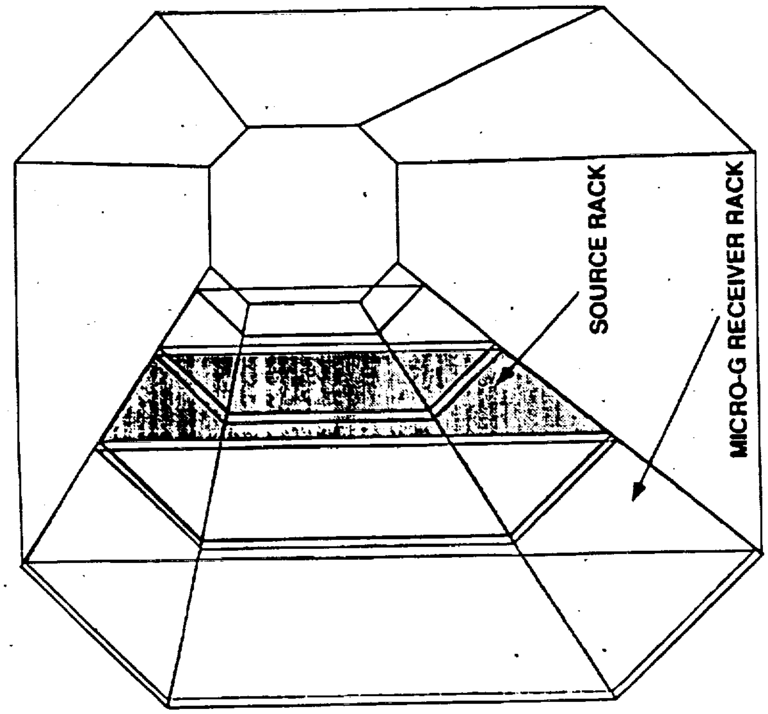
VAPEPS MODEL OF SPACESTATION FREEDOM
HABITATION MODULE OUTER STRUCTURE



END VIEW OF SSF MODULE SECTION
SHOWING RACK AND DUCT CONNECTION



BASELINE VAPEPS MODEL OF
SPACESTATION FREEDOM HABITATION MODULE



VAPEPS MODEL OF SSF MODULE MID-SECTION

LOCATION OF SOURCE AND RECEIVER RACKS FOR BASELINE MODEL

SOURCES

SSF FAN

Acoustic Power

Mechanical power*

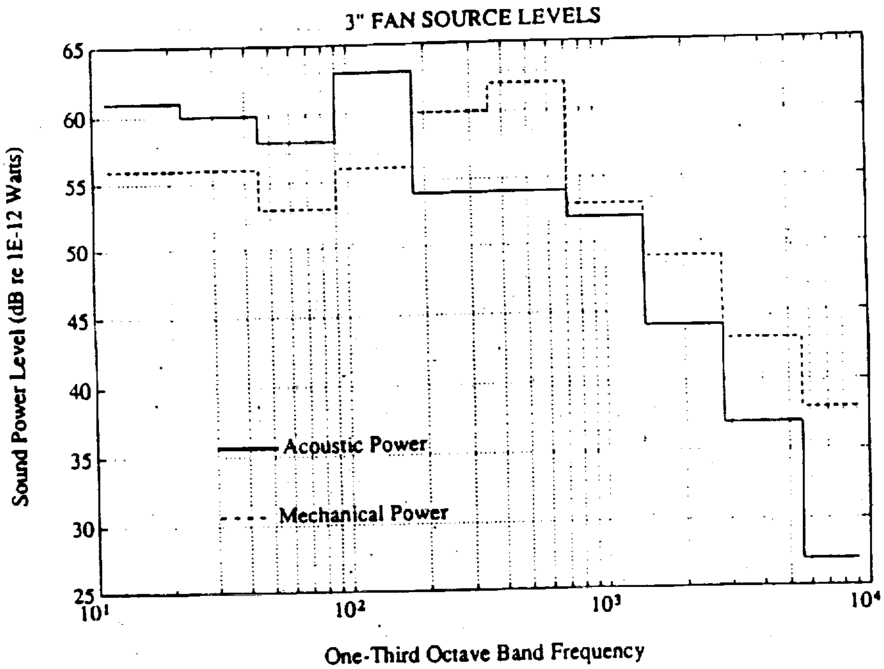
Turbulent boundary layer excitation in ducts

SHUTTLE SOURCES

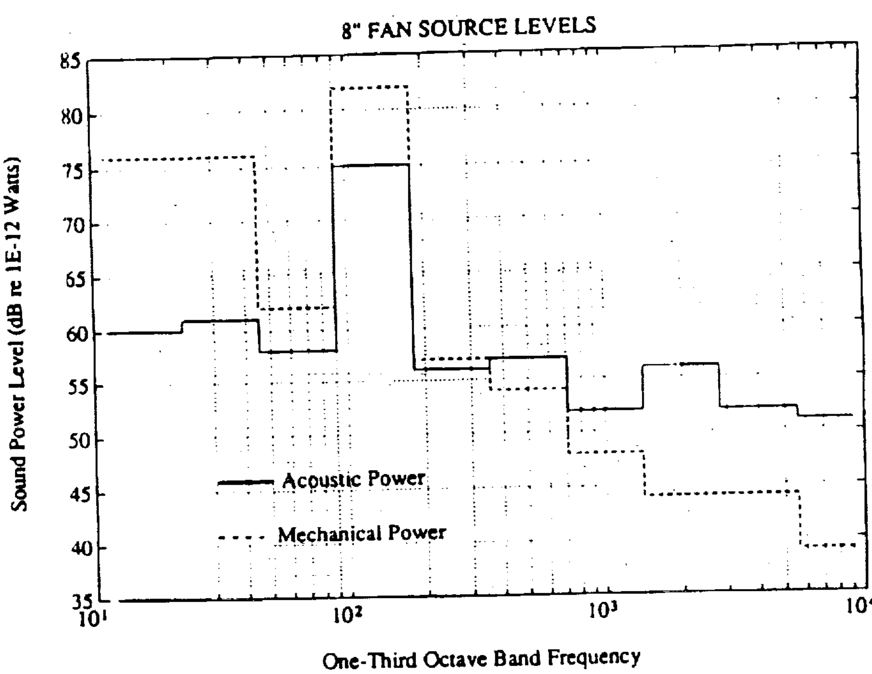
Avionics and equipment bay acoustic and mechanical* sources

ECLSS water pump, mechanical and acoustic

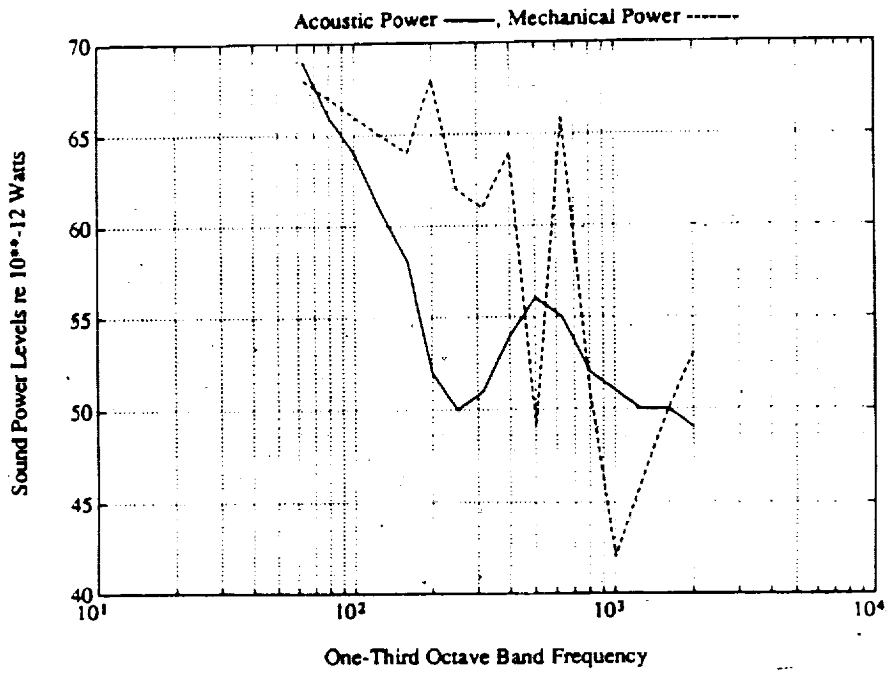
* Mechanical power inputs are assumed equal to the acoustic power inputs



Comparison of Acoustic and Mechanical Power Generated by 3" Fan in Rack Mounted Electronic Instrument in Vibration Test Laboratory



Comparison of Acoustic and Mechanical Power Generated by 8" Fan Mounted in Rear of Electronic Rack in Vibration Test Laboratory



Comparison of Acoustic and Mechanical Power Generated by ECLSS Water Pump in Shuttle Orbiter

NOISE CONTROL TREATMENTS

INCREASE ACOUSTIC ABSORPTION CO-EFFICIENT

- IN DUCTS

INCREASE DAMPING LOSS FACTOR

- IN DUCTS
- IN RACKS

ISOLATE SOURCES MECHANICALLY

- USING TYPICAL VIBRATION ISOLATOR

ISOLATE SOURCES ACOUSTICALLY

SPACE STATION FREEDOM MICRO-G PRELIMINARY ANALYSIS RESULTS

ANALYSIS RANGE 50 - 100 Hz

BASELINE WITH SSF FAN IN DUCT

EFFECTS OF SOURCE/RECEIVER DISTANCE, DAMPING, ABSORPTION, AND ISOLATION

SHUTTLE ACOUSTIC & MECHANICAL SOURCES

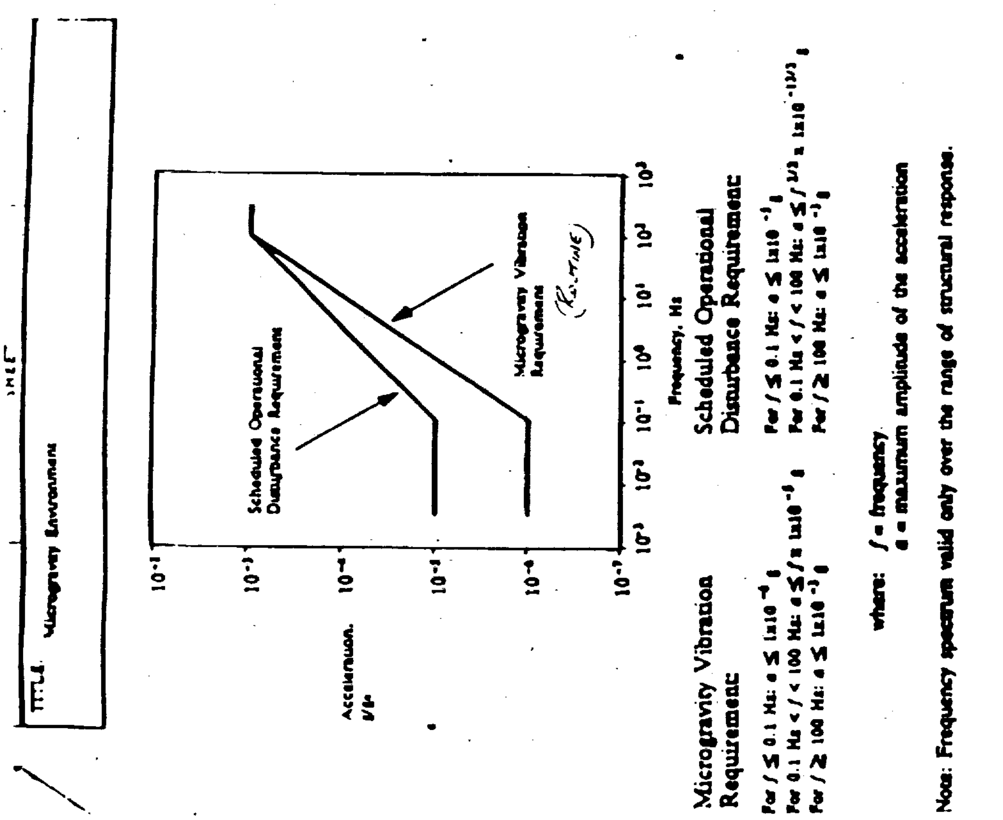
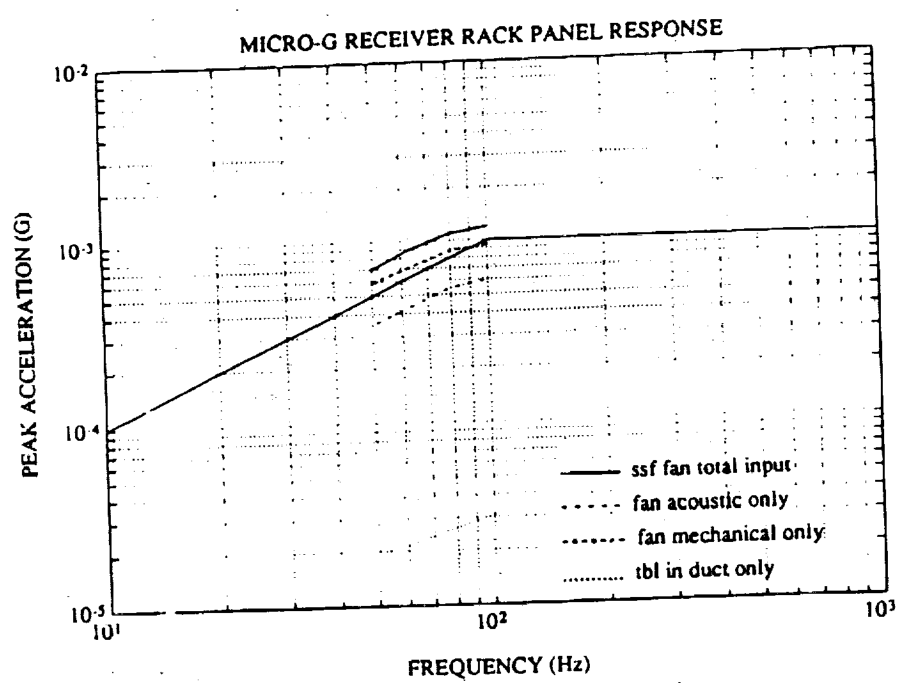


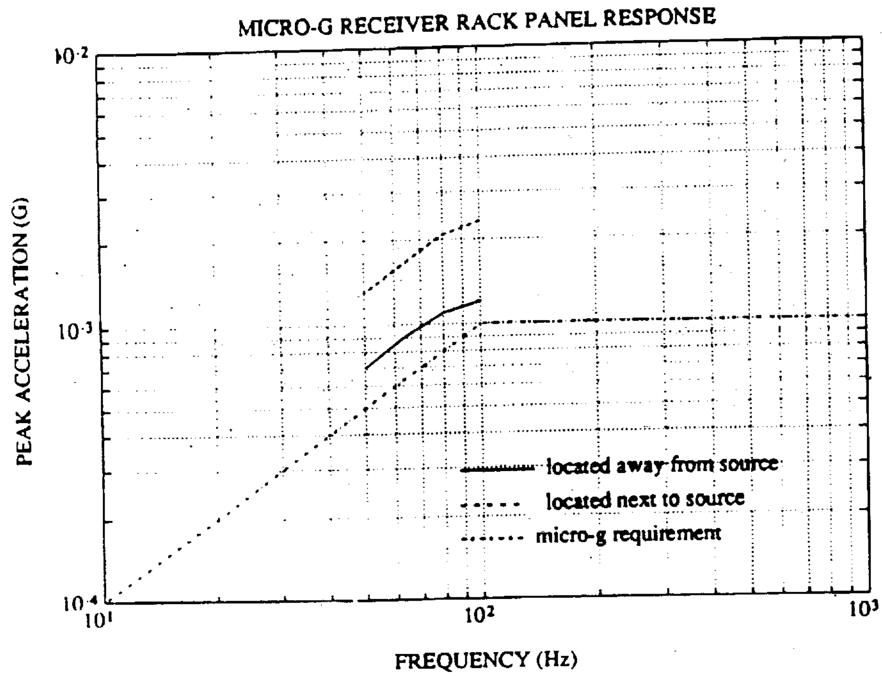
FIGURE 3-23 MICROGRAVITY ENVIRONMENT OSCILLATORY/TRANSIENT DISTURBANCE ACCELERATION LIMITS

SUMMARY OF RESULTS OF VAPEPS ANALYSIS OF SSF MODULE

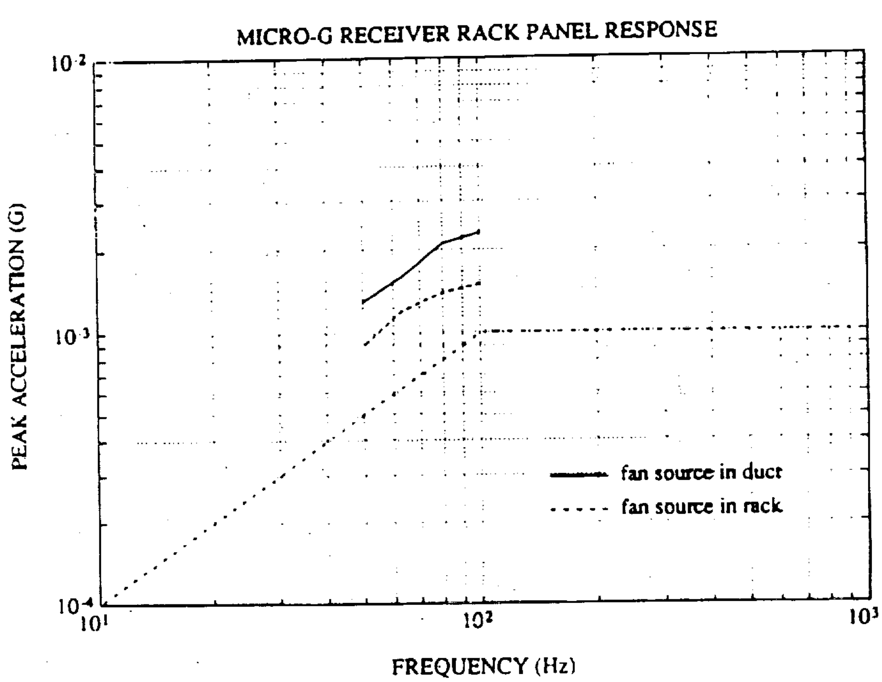
MODEL NUMBER	MICRO-G RECEIVER LOCATION	DUCT TO MODULE ACOUSTIC COUPLING	DUCT TO RACK NON-RESONANT COUPLING	RACK TO MODULE NON-RES. COUPLING	AAC	DLF	SOURCE(S)	OVERALL SPL IN MODULE (dBA)	AVG. RACK RESPONSE @50 Hz (g)	AVG. RACK RESPONSE @100 Hz (g)
BASELINE	NEXT TO SOURCE	YES	YES	YES	0.15 ducts 0.11 mod.	0.05	SSF FAN IN DUCT	86.0	1.3E-3	2.3E-3
6	AWAY FROM SOURCE		NO	NO			FAN ACOUS ONLY	86.6	5.8E-4	9.4E-4
8	AWAY						SSF FAN TOTAL	86.9	7.0E-4	1.2E-3
9	AWAY						TBL IN DUCT ONLY	37.7	1.9E-5	3.1E-5
10	AWAY						FAN MECH ONLY	76.0	3.5E-4	6.0E-4
12	NEXT					↓	SSF FAN IN DUCT	86.9	1.3E-3	2.3E-3
13	NEXT			↓		0.1 DUCTS 0.1 RACKS	SSF FAN IN DUCT	86.0	8.0E-4	1.5E-3
14	NEXT			YES	↓	0.05	SSF FAN IN DUCT	86.0	1.3E-3	2.3E-3
16	NEXT	↓	↓		AAC=0.9 8 DUCTS		SSF FAN IN DUCT	83.6	4.8E-4	8.5E-4
18	NEXT	NO	YES		AAC=0.9 ALL DUCTS		SSF FAN IN DUCT	76.0	1.2E-3	2.1E-3
19	NEXT	YES			0.15 ducts 0.11 mod.		SSF FAN IN RACK	73.0	9.0E-4	1.5E-3
20	NEXT						SHUTTLE SOURCES	72.3	—	6.7E-4
21	NEXT	↓	↓	↓	↓	↓	ECLSS PUMP	48.9	—	3.1E-4



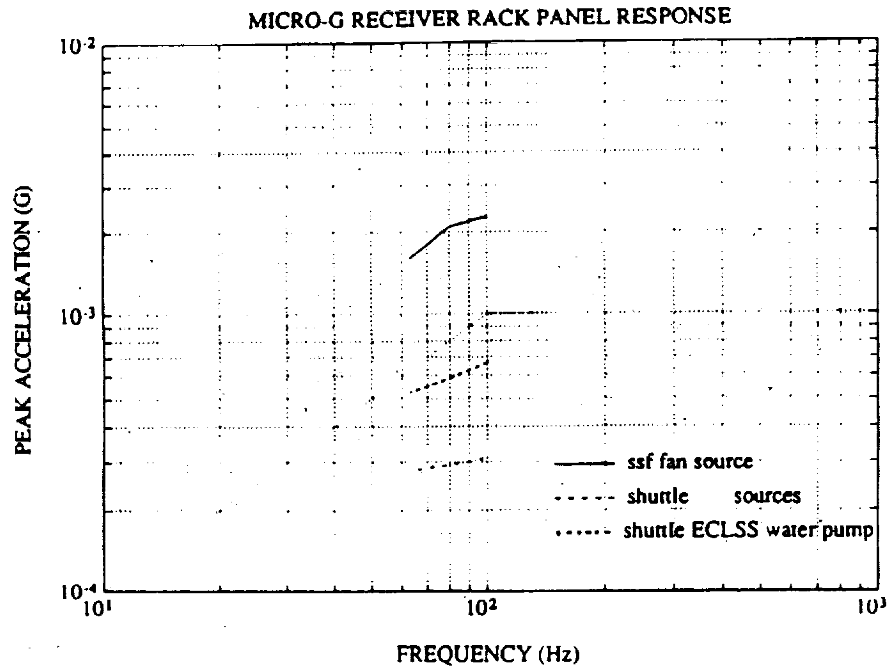
Contributions to the Total Micro-gravity Receiver Response by the SSF Fan Acoustic Power and Mechanical Power Components, and the TBL Excitation in the Ducts. Receiver Located Away from Source.



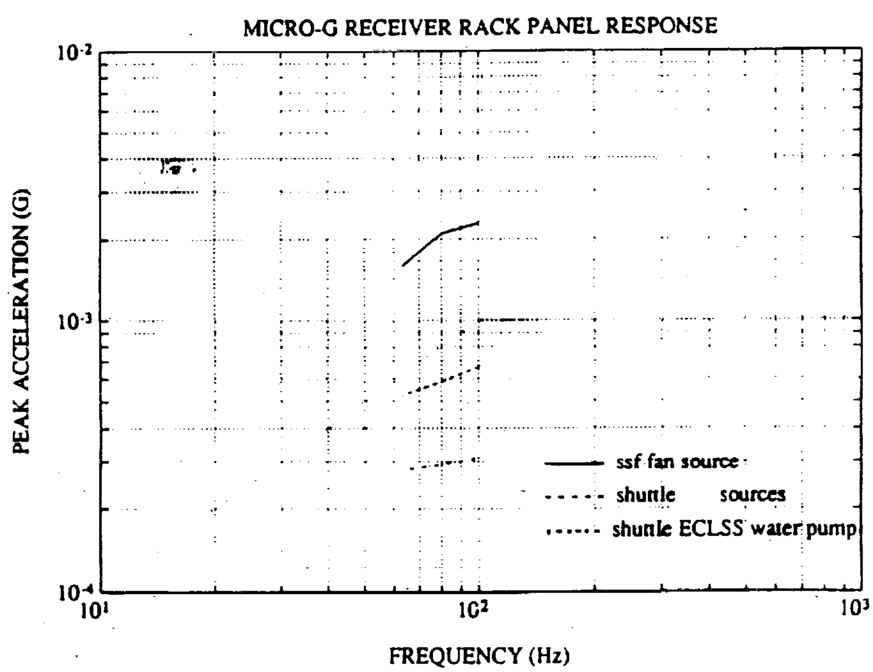
Comparison of Receiver Rack Response for the Receiver Located Away from the Source Rack and Next to the Source Rack.



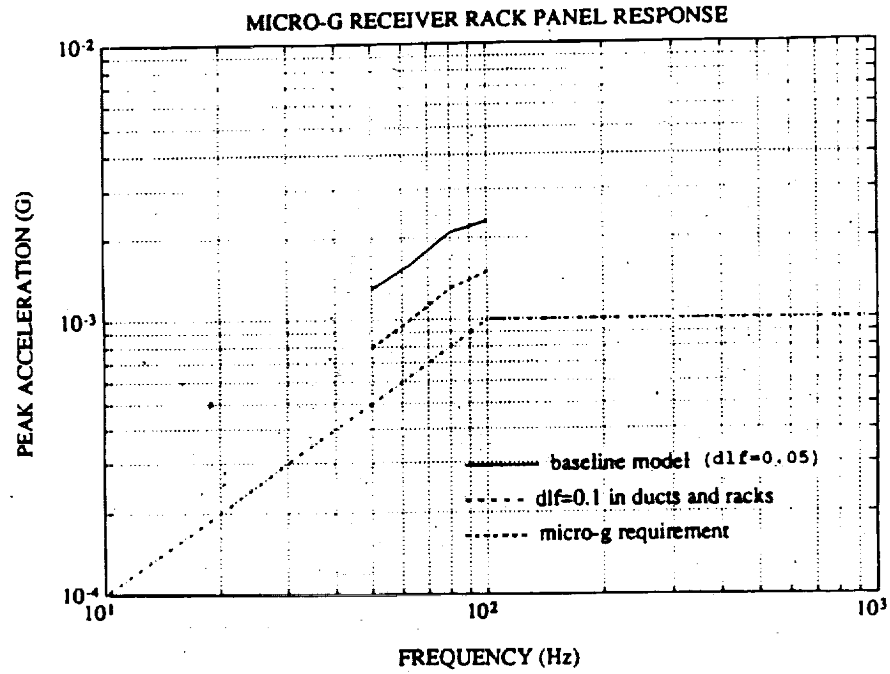
Comparison of the Microgravity Receiver Rack Response for Fan Acoustic Power Injection into the Duct vs. the Rack.
Receiver Located Next to Source.



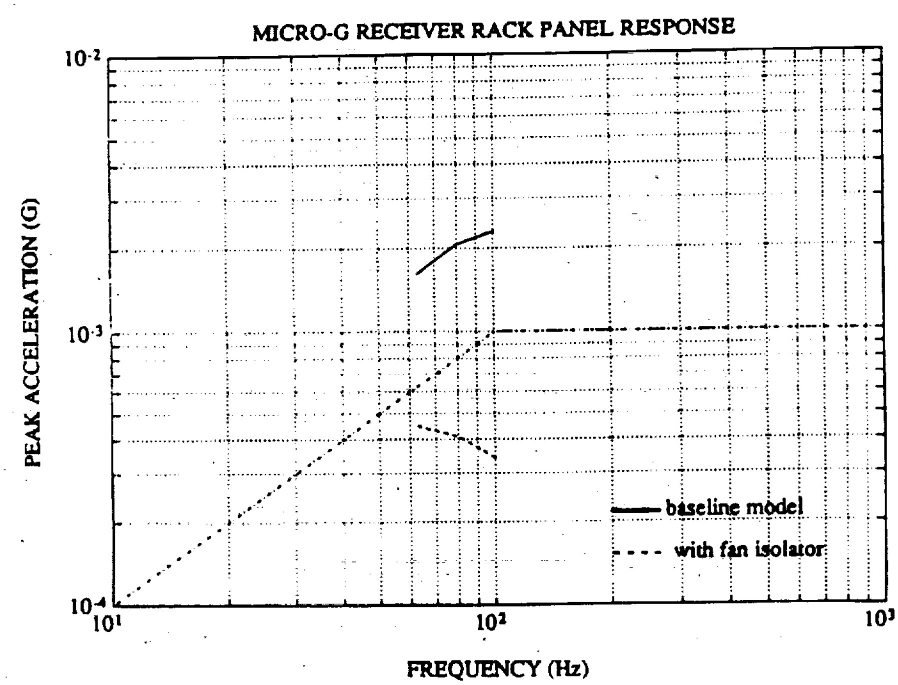
Comparison of Micro-gravity Receiver Rack Panel Response to Three Individual Inputs.



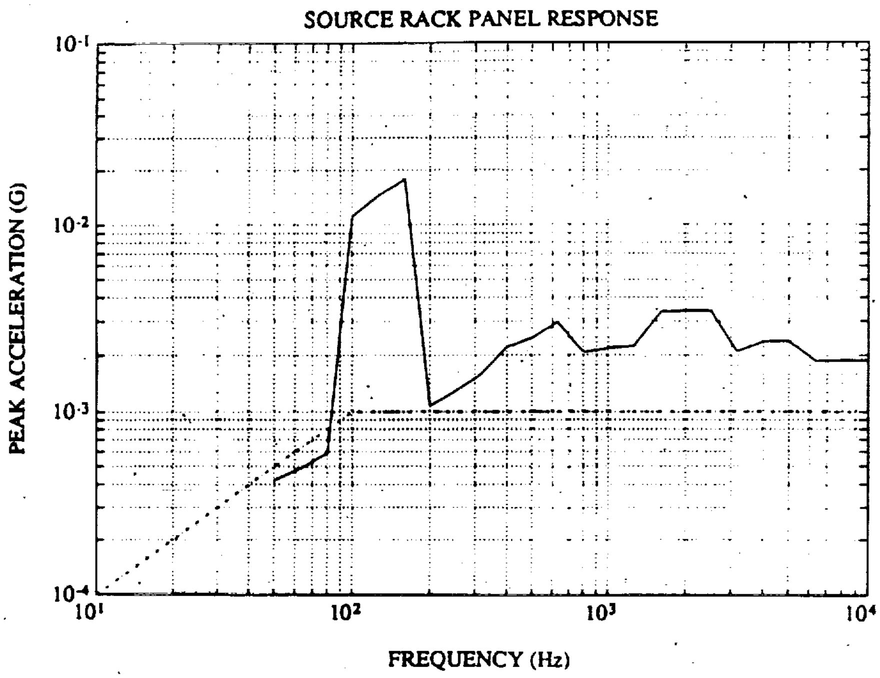
Comparison of Micro-gravity Receiver Rack Panel Response to Three Individual Inputs.



Effect of Doubling the Damping Loss Factor of the Rack Panels and Duct Walls. Simulates the Application of Structural Damping Treatments.



Effect of Typical Vibration Isolation of Fan on Micro-gravity Receiver Rack Response.



Prediction of Rack Vibration Levels Created when 3" Fan is Inside of Rack

CONCLUSIONS

MEETING SPACE STATION MICROGRAVITY REQUIREMENTS WILL BE DIFFICULT WITH CURRENT SCENARIO OF EQUIPMENT.

VIBRATION CONTROL NEEDS TO BE CONSIDERED IN THE DESIGN PHASES OF SPACE STATION.

VAPEPS IS VALUABLE FOR PREDICTING THE EFFECTS OF VARIOUS VIBRATION CONTROL TREATMENTS IN THE DESIGN PHASES OF SPACE STATION.

REPORT DOCUMENTATION PAGE

Form Approved
OMB No. 0704-0188

1. AGENCY USE ONLY (*Leave blank*)

2. REPORT DATE
May 1992

3. REPORT TYPE AND DATES COVERED
Conference Publication

4. TITLE AND SUBTITLE
International Workshop on Vibration Isolation Technology for Microgravity Science Applications

5. FUNDING NUMBERS
WU–694–03–0C

6. AUTHOR(S)

7. PERFORMING ORGANIZATION NAME(S) AND ADDRESS(ES)
National Aeronautics and Space Administration
Lewis Research Center
Cleveland, Ohio 44135–3191

8. PERFORMING ORGANIZATION REPORT NUMBER
E–7035

9. SPONSORING/MONITORING AGENCY NAMES(S) AND ADDRESS(ES)
National Aeronautics and Space Administration
Washington, D.C. 20546–0001

10. SPONSORING/MONITORING AGENCY REPORT NUMBER
NASA CP–10094

11. SUPPLEMENTARY NOTES
Responsible person, Joseph F. Lubomski, (216) 433–3907.

12a. DISTRIBUTION/AVAILABILITY STATEMENT
Unclassified - Unlimited
Subject Category 31

12b. DISTRIBUTION CODE

13. ABSTRACT *(Maximum 200 words)*

Vibration isolation technology for microgravity science applications has emerged the last several years as a critical issue. All of the major space agencies have work in progress in this area. Because of the importance of this issue and also because of the large amount of work being done throughout the community an international workshop was convened. The purpose of the workshop was to evaluate the relevance of the current work in progress and to make recommendations as to what needs must be addressed in future work. The results of the workshop discussions as recorded are included in these proceedings along with the material presented relative to the work in progress. The scope of the material presented at the workshop was:

–Sensitivity of microgravity science experiments
–Isolation technology development
–Orbital environment
–Microgravity measurements and data reduction.

The working group discussions focused on isolation technology methods and needs and on science requirements and the environment. The discussion in the working group were transcribed and are presented in detail except for some minor editing for ease of reading.

14. SUBJECT TERMS
Vibration isolation; Microgravity experiments; Space station; Space flight experiments

15. NUMBER OF PAGES
415

16. PRICE CODE
A18

17. SECURITY CLASSIFICATION OF REPORT	18. SECURITY CLASSIFICATION OF THIS PAGE	19. SECURITY CLASSIFICATION OF ABSTRACT	20. LIMITATION OF ABSTRACT
Unclassified	Unclassified	Unclassified	

NSN 7540-01-280-5500

Standard Form 298 (Rev. 2-89)
Prescribed by ANSI Std. Z39-18
298-102