Remarks on Mission to Planet Earth*

*Comments of James Hansen at the Federation of American Scientists hearing on Mission to Planet Earth held at the United States Senate September 6, 1990

First of all, I would like to stress that, for Mission to Planet Earth to be successful, it must be built on a broad solid scientific base. As attachment A to my remarks I have provided three charts which address this requirement. These charts were put together in consultation with my colleagues at the Goddard Institute, for a meeting called by Senator Gore last December.

Chart 1 addresses the brainpower issue. The scale and difficulty of Mission to Planet Earth are at least comparable to that of the space science program in the 1960s, at which time there was a large scale effort to educate and train scientists in appropriate fields. As we indicate in Chart 1, to produce a flux of 200 new scientists per year requires supporting at a given instant about 1000 graduate students, and an appropriately scaled number of undergraduates, post-docs and young scientists. We calculate the cost of a training program of that magnitude at about $100 million per year. It's encouraging that NASA and other agencies are now taking steps in the right direction, but the size of these steps does not yet approach the required magnitude.

Chart 2 addresses the issue of data sources. There are different important roles to be played by relatively large polar platforms, small satellites, operational meteorological satellites, the weather station network, ships and buoys, and ground and aircraft special studies. This full set of data sources is essential; it's fair to say that global change research would be crippled in the absence of any one of these components. Some of these data sources are in desperate need of support for upgrading, or to avoid critical degradations. I want to return to this issue of data sources in a moment.

Chart 3 addresses the need for science research support. Much data being acquired now is under-utilized. Research support for individual investigators is at an all time low, as measured by the percentage of proposals actually funded of those which are evaluated as of excellent quality and deserving of funding. Yet progress always has depended, and probably always will depend, on innovative individual and small scale research activities. Also it is important to stress that support for the disciplines critical to global change research must be maintained and enhanced; it would be counterproductive to divert such disciplinary support to a global change project.

Before discussing priorities, I would like to mention attachment B to my remarks. Attachment B is Three Short Stories with a Moral, by Freeman Dyson. These are true stories which take place in the third world, the second world, and the first world, and they describe common mistakes which occur in large projects. For example, in the third world, a committee of Boulou tribesmen prefer to pursue an unrealistic proposal for a grand water project rather than ask the assistance of a neighboring Fulani tribesman in the digging of a simple well. And so their village continues indefinitely without any nearby water supply. I have been passing Dyson's article around for the past year, because I feel that it has a good deal of potential relevance to present global change planning. I hope you will read it.

One of the points which Dyson stresses is the value of small satellite missions. Small satellites have the <u>potential</u> for quick turnaround and low cost. That capability is vital for global change studies. There are several small satellite projects in development, and the potential for many others.

As a specific example, I would like to mention the climate problem: Is global climate changing? What are the causes? My attachment C is the draft of an article describing a proposed pair of small climate satellites designed to answer those questions. Such small satellites can do the best job of providing the needed measurements of key climate forcings, climate feedbacks and climate diagnostics for this important global problem.

However, I want to stress that I am <u>not</u> arguing against the polar platforms. Even for the climate discipline alone, the polar platforms are very important. By providing simultaneous viewing by instruments with high spectral and spatial resolution, they will allow process studies which are essential for climate research. Also these exploratory measurements will provide a data base which will allow us to analyze new problems, issues, or processes which will certainly arise in the future.

What I <u>am</u> arguing for is priorities. I believe that the issues of brainpower development, science research support, small satellites, and conventional data sources deserve first priority in the division of resources. Otherwise the large hardware projects make no sense. To a farmer it is like milking the cows, feeding the horses, and eating breakfast in the morning; you do these things every day before launching into the field work. You may save a few minutes by not taking care of the horses properly, but it won't pay in the long run.

The danger which Dyson warns about, that the big project may squeeze out the small even if the small are more productive on a dollar for dollar basis, is one which we must guard against. The first priorities should be taken care of first, and those remaining adjusted to fit available resources. We need some mechanisms, some checks, to verify that this is happening.

As a climate researcher it is easy for me to give an example of a good litmus test. Precipitation has long been recognized as a key climate diagnostic, which is inadequately measured. Now there are plans for a small satellite mission, TRMM, to obtain precipitation data for much of the world. The official launch date is February 1996. Maintaining the TRMM launch date is one crucial litmus test. Supporting small satellites, like feeding the horses, should be automatic. Unless small satellites are kept on a development timetable of five years and less, they lose much of their value.

I recognize that there are considered statements about priorities in the NRC report, which I'm sure Jim Baker will mention. But I fear that they could be buried in too many words. What we may need is a half page list of priorities required to achieve and maintain a broad solid scientific base, like the brainpower and other issues I mentioned, and a set of litmus tests, which would help judge whether these priorities are being served --- a half page, or at least something of a size which can be grasped by congressmen and other interested parties.

Finally I want to stress that I strongly support EOS. The reason that I focus on the issue of priorities is that I perceive such a need in this discussion. Dyson talks about a tendency of committees to focus on the big ticket item; then when everyone agrees that the big ticket item is good, because of its size it becomes first priority, even though that wasn't intended. We have to guard against that.

Also I would like to end on an optimistic note by saying that there has been progress, I believe, compared to a year and a half ago when the article with the ambiguous title, "Bringing NASA Down to Earth", appeared in Science (244, 1248-1251, 1989). There is more of an indication of plans for support of students, small satellites, study of existing data, etc. But I still believe we need clearer statements of priorities, and ways to judge that we are following them.

Scholarship/Fellowship/Young Scientist Program

- Support for undergraduate activities (research opportunities and summer programs) and for graduate scholarships allotted to universities based on their ability to educate (but travel support should be included for graduate students to allow research interactions with another university or government laboratory).

- Post-doctoral fellowships (2 years) and young scientist partial salary support (3 years) provided directly to participants, who should be free to carry out research at location(s) specified in their proposal.

- Order of magnitude and costs:

$o(10^3)$ undergraduate	10 K × 1000
$o(10^3)$ graduate	25 K × 1000
o(500) post-doctoral	60 K × 500
o(500) young scientist	60 K × 500
	~100 M/year

- Stipend should be enough to make the award attractive and competitive

- Support needs to be incremental to current funding.

- Administration/funding should be isolated to prevent it from being swallowed by a money-hungry bureaucratic organization

Full Set of Data Sources Essential*

- Eos Polar Platforms
  - — includes large instruments with < 1 km resolution
  - — global coverage once every two days, depending on altitude

- Small Missions — Earth Probes
  - — crucial for many measurements which require diurnal coverage, e.g., rainfall, radiation budget
  - — low cost, large return, potential for quick response to science needs.

- Operational Meteorological Satellites
  - — provide many long term data sets necessary for climate analyses
  - — provide necessary high frequency data

- Weather Station Network
  - — surface air, precipitation and *in situ* atmospheric profiles (rawinsondes) via this network are essential to climate studies

- Ship and Buoy Measurement Programs
  - — essential for most ocean measurements, including heat storage which is key climate unknown

- Ground (and Aircraft) based Special Studies and Monitoring
  - — essential for many specific problem, e.g., trace gas monitoring and river runoff

*<u>Note:</u> Continuation of data collection efforts such as WOCE, TOGA, and projects of WCRP are complementary programs which are crucial for "Mission to Planet Earth."

*<u>Note:</u> Many of these data sources require increased support to improve capabilities for climate monitoring.

Need for Science Research Support

- Data being acquired now is under-utilized

- Science support up front can help optimize science return of Eos

- Need to assure end-use of data by providing adequate and protected science funding

- Must be treated as long-term data analysis and research program, not solely an instrument development and data collection project

- Present research support for individual investigators is at an all time low, as measured by the percentage of proposals actually funded of those evaluated as of excellent quality.

- Discipline support, crucial to success of global change studies, must be maintained or enhanced, not sacrificed to promote Eos.

Three Short Stories with a Moral*

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I take three cautionary tales, one from each of the three worlds into which our planet is divided. The tales will have various morals. One of the morals is that human nature is the same in all three worlds. We are the same people making the same mistakes, whether we happen to belong to the third world, the second world or the first world. The tales have other morals. But let me tell you the stories first. The stories should speak for themselves. After you hear the stories you can decide for yourselves what the morals ought to be.

1. The Third World

For the third world I choose the village of Ngon, a village in central Africa where my daughter Mia spent some time as a Peace Corps volunteer three years ago. Mia was in Ngon as an employee of the Office of Community Development of the Republic of Cameroun. Her official function was to encourage local initiatives leading to the improvement of public health and education.

The main problem in Ngon is water. The village is several kilometers away from the nearest source of drinkable water. Night and morning, the women of the village must walk to the spring and back, with heavy water-pots balanced on their heads. During the dry season the spring degenerates into a muddy swamp. In 1985 the official Committee of Village Development, composed of prominent residents of Ngon and three neighboring villages, met to consider the problem of their water supply. The meetings were conducted according to the traditional rules of African hospitality, the village chiefs presiding, their wives keeping the delegates supplied with food and drink, my daughter as an honored guest seated among the chiefs.

The villagers mostly belong to the Boulou tribe and have their own Boulou language, but they have been educated for three generations in French. The committee of Village Development in keeping with its official status, conducted its deliberations entirely in the best bureaucratic French.

Two courses were available. I will call them Plan A and Plan B. Plan A was to engage the services of a professional well-digger who happened to live nearby. The fee he charged was high by village standards, but not prohibitive. He would design and direct the construction of an adequate well, including a bath-house and laundry, using the villagers as his work-force. My daughter had made enquiries about his work in other villages and found that the results were generally satisfactory. Plan B was to write a formal proposal to the central government in Yaoundé - 400 kilometers away - for a massive water adduction system (that means plumbing) using urban technology. The chance that the proposal would be accepted was small. Many hundreds of villages would be competing for the central government's limited resources. But if Ngon should happen to be the lucky winner the rewards would be great, especially for the members of the Committee of Village Development. The decision was made unanimously to proceed with Plan B. As a result, Ngon still has no supply of water.

After the meetings were over, my daughter went back to the village and spoke privately with the villagers, trying to understand why they made what seemed to her to be a clearly wrong decision. She found that everybody, including the women who carry the water-pots, was in favor of Plan B. In the end they almost convinced my daughter that Plan B made sense. After all, as one of the women said to my daughter, nobody in Ngon ever dies of thirst. The problem of the water supply is not a matter of life and death. The problem is a matter of status.

On the one hand, the act of writing an official proposal to the government would enhance the status of the village and of the Committee of Village Development, even if nothing ever came of it. It would open a channel of communication and create contacts between the villagers and the political authorities in Yaoundé. In the long run, such contacts are more important to the life of the village than a communal bath-house. On the other hand, the act of making a deal with a back-woods well-digger would be unworthy of the dignity of an official Committee. If these arguments had not been sufficient, there was an even more cogent reason for rejecting Plan A. The well-digger is a Fulani. He belongs to the wrong tribe. The Boulous of Ngon are agricultural people. They have lived from time immemorial in villages. They consider themselves civilized. The Fulanis are Northerners, nomads and cow-herders. No self-respecting Boulou would want to take orders from a Fulani.

So I leave the villagers of Ngon. On the whole they are a happy and contented people. They were always friendly and hospitable to my daughter, even when they found her ideas a little strange. I pass on to the second world.

2. The Second World

To represent the second world I choose the great Soviet astronomical observatory at Zelenchukskaya in the Caucasus mountains. I visited the observatory in 1977. The six-meter telescope, the largest optical telescope in the world, was then brand new and just beginning to go into operation. I spent three days and nights up there on the mountain and enjoyed my stay very much. The astronomers at Zelenchukskaya were as friendly to me as the villagers of Ngon had been to my daughter. They talked frankly about the six-meter telescope and its history.

Twenty years earlier, a committee of the Soviet Academy had met to discuss with their political authorities the facilities for astronomy in the Soviet Union. The six-meter telescope was their Plan B. Plan A was to construct four or five modern observatories of modest size at good clear-sky sites in Central Asia.

One example of a Plan A observatory already existed at Byurakan in Soviet Armenia. As everybody knows, the Armenians are the Fulanis of the Soviet Union. I also visited Byurakan and saw there a two-meter telescope with a Fulani by the name of Markarian in charge. Markarian was using his telescope to great effect, taking pictures of the sky and picking out objects that have strong emission in the blue spectrum. Many of the most interesting objects in the universe were first identified by Markarian using that little telescope and still carry Markarian's catalogue numbers. Byurakan is in the hands of Fulanis who know how to do important science with limited means.

Unfortunately, there are no other observatories like Byurakan in the Soviet Union. Instead, Plan B prevailed. The assembled academicians decided to build the biggest telescope in the world. Six meters was chosen as the mirror diameter because it had to be decisively bigger than the five-meter telescope at Palomar. The manufacture of the telescope was entrusted to a heavy industrial outfit in Leningrad which had little previous experience with astronomy. The observatory was under construction for twenty years. When I visited it in 1977, one of the Soviet astronomers remarked that the structure was built out of left-over pieces from dismantled battleships. Another Soviet astronomer told me that this one instrument had set back the progress of astronomy in the Soviet Union by twenty years. It had absorbed for twenty years the major part of the funds assigned to telescope-building, and it was in many ways already obsolete before it began to operate. It deprived a whole generation of young astronomers of the opportunity to put their skills to use. Since then 11 years have gone by and the telescope has now set back optical astronomy by 31 years. No scientifically exciting discoveries have been made with it.

One of the factors which the committee planning the observatory did not worry about was the Zelenchukskaya weather. I was on the mountain for three nights and did not see the sky. Even at Palomar one may be unlucky and run into a string of cloudy nights. But at Zelenchukskaya the weather is consistently bad for eight months of each year. Besides, the site is far too close to the high Caucasus peaks which regularly stir up storms and clouds. The committee probably chose this site because it is easily accessible by rail and road. The sites with good seeing in Central Asia have no roads suitable for the transport of a super-massive structure. At Zelenchukskaya the roads are good because there is a skiing resort in the same valley. Of course, the snow which makes the area good for skiing also causes problems for the telescope. When I was there, a great mass of accumulated ice had blocked the action of the dome so that the slit could not be opened. Even if the sky had been clear, the telescope would not have been able to see it. I gave a theoretical seminar to the astronomers in a lecture-room where the temperature was minus ten Celsius. The situation was not good for anybody who wanted to do serious work in astronomy.

During my stay, I looked for clues which might explain how this scientific disaster had happened. I found the essential clue in the visitors' gallery. Some of you may have gone as tourists to visit the five meter telescope at Palomar. At Zelenchukskaya they have a visitor' gallery, like the one at Palomar, only about ten times as big. And behind the vis-

*Based on Wooster College May 9, 1988 commencement address and lectures ("On Being the Right Size: Reflections on the Ecology of Scientific Projects") given in Seattle in May 1988.

itors' gallery they have a white wall for visiting dignitaries to write their names on. Instead of a visitors' book they have a wall. They invited me to write my name on the wall. This wall is huge, about a hundred feet long. Still I had a hard time finding an empty space large enough to write my name on. Every square centimeter of the wall was tightly packed with names.

When I saw the wall, I understood for the first time what the observatory was for. The government officials who decided to build it twenty years earlier did not care much about astronomy. They did not mind keeping the astronomers waiting for twenty years while it was being built. Even when the telescope was finished, they were not in any hurry to get the dome unstuck so that the astronomers could get to work. The things that mattered were the visitors' gallery and the wall. This visitors' gallery and the wall must have been given some high priority. They were evidently in full swing for many years before the telescope was ready. For years and years before my visit, bus-loads of school teachers and factory workers and party chairmen were trooping through the visitors' gallery, admiring this latest triumph of Soviet science, and writing their names on the wall.

Plan B gave the political authorities in Moscow what they wantec a tangible symbol of Soviet greatness. Plan ' might have been better for science. Plan A might have saved a whole generation of astronomers from being frustrated. But with Plan A, the authorities would not have had the satisfaction of building the biggest telescope in the world, and there would have been no hundred-foot wall for the visitors to write their names on.

3. The First World

My third cautionary tale concerns our own world, the so-called first world. The astronomers of the United States set up a committee at the beginning of each decade to plan the telescopes to be built in the next ten years. The committees are called by the names of their chairmen. I shall talk about the Field Committee, named after its chairman George Field who happens to be a very fine astronomer, which dealt with the 1980s and published its report in 1982. I shall talk only about space-based astronomy, the launching and operation of telescopes in orbit.

While the Field Committee was meeting from 1978 to 1980, the situation of American space-based astronomy was as follows. We had two active space-telescope projects. We had one Boulou space-telescope and one Fulani space-telescope. The Boulou telescope was the Hubble Space Telescope, which I am sure you have heard of – a grand and elaborate instrument, very large and heavy, which was supposed to be launched by the shuttle, if all went well, in 1985.

The Fulani telescope was a small and cheap instrument called International Ultraviolet Explorer or IUE. IUE is a little 18-inch telescope which sits in the sky over Brazil and works all the time, 24 hours every day. It is user-friendly. Astronomers, young and old, are using it all the time. IUE was launched in January 1975, before the Field Committee started work, and has been from the beginning, like Markarian's telescope in Armenia, a brilliant scientific success. It is still going strong and still doing excellent science after ten years in space.

The Field Committee considered two programs. Their Plan A was a series of Explorer missions following the pattern of IUE. An Explorer mission means a mission small enough and cheap enough to be paid for out of the NASA space-science budget without any special exertions. If Explorer missions were given the highest priority, it would be possible for NASA to sustain a launch-rate of one astronomical Explorer per year in addition to the Explorers concerned with other things such as earth sciences and plasma physics. There are many important things for astronomical Explorers to do. If we had had one Explorer mission in X-ray astronomy, one in infrared, one in ultraviolet, one in astrometry and one in radio-interferometry, the scientific harvest would have been enormous. If Plan A had been adopted we could have had all these flying in the 1980s without any stretching of the NASA budget.

The Field Committee, however, like the committees in Ngon and Moscow, preferred Plan B. Plan B consisted of a series of space-missions known collectively as Great Observatories. The Hubble Space Telescope was supposed to be the first Great Observatory. After that would come the Gamma-Ray Observatory, also dependent on the Shuttle for its launch and scheduled to go up in 1987. Next would be the Advanced X-ray Astrophysics Facility, familiarly known as AXAF. AXAF was the highest-priority item on the Field Committee list of new missions, since the Committee assumed the first two Great Observatories, the Hubble Telescope and the Gamma-Ray Observatory, to be already in the bag. After AXAF would come the fourth Great Observatory called LDR or Large Deployable Reflector, a far-infrared telescope with mirror-diameter in the ten-diameter class. Plan B began with these four Great Observatory missions, plus a number of smaller missions left over from earlier Committee reports. To be fair, I should mention that Plan B included an Explorer mission called IRAS or Infra-red Astronomy Satellite which actually flew in 1983 and gave us our first comprehensive view of the infra-red universe. IRAS was, like the earlier Explorer mission IUE, an international venture and an enormous scientific success. But it was not high on the Field Committee's list of priorities. It was another Fulani mission.

The main emphasis in the Field Committee report was on the Great Observatory missions. Each Great Observatory costs as much as five or ten Explorers. Each requires protracted and difficult negotiations between NASA and various committees of Congress to obtain the necessary funds. Each requires about a decade to complete its engineering development and construction after its funding has been authorized. And each requires a Shuttle launch to put it into orbit. As a consequence of the Challenger disaster of January 1986, no Great Observatories have been launched. The Hubble Telescope is sitting idle in a warehouse, costing about as much to maintain on the ground as an Explorer mission would cost to build and launch. The scientific return from the entire Plan B program, apart from IRAS and some ground-based activities which I am not discussing here, has been zero. Just like Ngon. Just like in Zelenchukskaya.

It is important to understand that the debacle of the Great Observatory program is not simply a consequence of the Shuttle accident. The Great Observatories were in deep trouble long before the Challenger crashed. Their troubles were technical as well as political. The Hubble telescope, the only Great Observatory yet built, had a long history of engineering difficulties, delays and cost-overruns. AXAF, the highest-priority item in the Field Committee list, has not yet been approved by Congress. Construction of AXAF and of the LDR has not been started. Even if the Shuttle had remained alive and well, none of the missions recommended by the Field Committee and not already recommended by earlier committees could possibly have been launched in the 1980's. The Field Committee report was entitled "Astronomy and Astrophysics for the 1980's." The title shows that the members of the committee were deluding themselves. So far as the 1980's are concerned, their program is, and always was, a mirage.

The fundamental flaw in the Great Observatory program is ecological. The Great Observatories are too big and too slow and too expensive to fit comfortably into the ecology of science. They take so long to fund, to build and to launch that they are unable to keep pace with the rapid growth of science. Scientific discoveries emerge, scientific ideas change and scientific tools develop, all within a year or two. A Great Observatory which takes ten years to build is always in danger of being left behind. The ecology of science needs missions that are small, cheap and quick enough to respond to new ideas and new questions. This is true, whether or not the Shuttle crashes.

4. Moral

That is the end of my third tale. The moral of these tales is clear. Committees are not always right. The nature of committees is the same, whether it is revealed in an African village assembly or in the academic politics of Moscow and Washington.

The game of status-seeking, organized around committees, is played in roughly the same fashion in Africa, in America and in the Soviet Union. Perhaps the aptitude for this committee game is part of our genetic inheritance, like the aptitude for speech and for music. The game has had profound consequences for all of us. In our political institutions, as in the quest for a village water supply, big projects usually win, whether or not they are useful. The large and fashionable squeeze out the small and unfashionable. The Space Shuttle squeezes out the modest and scientifically more useful expendable rocket. The Great Observatory squeezes out the Explorer. The centralized adduction system squeezes out the village well.

Fortunately, the American academic system is pluralistic and chaotic enough so that first-rate small science can still be done in spite of the committees. In odd corners, in out-of-the-way universities and in obscure industrial laboratories, our Fulanis are still at work. So I hope some of you graduating students, whether you are intending to be scientists or not, will be Fulanis. We have enough Boulous in this country to staff all of our committees. We badly need a few more Fulanis.

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The Missing Data on Global Climate Change

A pair of small, inexpensive satellites could help answer pressing questions about projected warming trends.

Governments worldwide have suddenly realized that they face an issue unprecedented in the history of science and technology: the anthropogenic greenhouse effect. Will gases spewed into the atmosphere by modern civilization cause large global climate changes, threatening the natural biosphere as well as civilization itself? The issue is urgent because the long lifetime of these gases in the atmosphere and the slow response time of the climate system mean that we are committing ourselves and our children to live with whatever changes do occur as a result of our actions today.

Scientific warnings have generated rancorous political debate owing to the presumed costs of curtailing greenhouse-gas emissions. And the debate is fueled by scientific uncertainties. Are climate changes already occurring? Are they caused by the greenhouse effect? What climate impacts are likely in coming years and decades? Until those questions can be answered, legislators are unlikely to agree on a policy response. Indeed, policymakers cannot even weigh the various options rationally until the climate system is better understood, and that, in turn, depends on the availability of comprehensive observations of global climate change.

In the following discussion we summarize the measurements that are needed to characterize changes in the Earth's climate. Much of the data is already being acquired by operational satellites and ground stations. Several important parameters are not being measured, however, and these omissions will persist until the end of the decade, when the National Aeronautics and Space Administration (NASA) plans to include the necessary data-gathering instruments in the Earth Observing System (EOS), a set of large polar-orbiting platforms. The urgency to answer questions about climate change compels us to ask whether the missing data can be collected more quickly. Specifically, can the instruments be placed on smaller satellites that can be launched sooner?

Our principal conclusion is that the most crucial unmeasured climate quantities could be obtained by a pair of inexpensive small satellites. The proposed satellites would allow climate data to be obtained sooner than from the large EOS platforms, improve the

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