

# EXTRATERRESTRIAL INTELLIGENCE RESEARCH

**HEARINGS**  
**BEFORE THE**  
**SUBCOMMITTEE ON**  
**SPACE SCIENCE AND APPLICATIONS**  
**OF THE**  
**COMMITTEE ON**  
**SCIENCE AND TECHNOLOGY**  
**U.S. HOUSE OF REPRESENTATIVES**  
**NINETY-FIFTH CONGRESS**

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# EXTRATERRESTRIAL INTELLIGENCE RESEARCH

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TUESDAY, SEPTEMBER 19, 1978

U.S. HOUSE OF REPRESENTATIVES,  
COMMITTEE ON SCIENCE AND TECHNOLOGY,  
SUBCOMMITTEE ON SPACE SCIENCE  
AND APPLICATIONS,  
*Washington, D.C.*

The subcommittee met, pursuant to notice, in room 2318, Rayburn House Office Building, 10:07 a.m., Hon. Don Fuqua (chairman of the subcommittee) presiding.

Mr. FUQUA. The subcommittee will be in order. Without objection, permission will be granted for radio, television, and still photography to be taken during the course of the hearing.

During the next 2 days, the subcommittee will review space research dealing with analyzing the radiation from space for evidence of intelligent life beyond our planet Earth.

As you are aware, NASA requested funding to initiate a program designated search for extraterrestrial intelligence in order to conduct a systematic study of the radio frequency spectrum for evidence of intelligent transmissions from space. The House and Senate authorized \$2 million for fiscal year 1979 for NASA to initiate the program. However, the House and Senate Appropriations Committee have elected in conference not to provide any funds for this program.

This subcommittee supported NASA's proposal to initiate the program in fiscal year 1979 in recognition of the importance of this new field of space exploration growing out of the science of radio astronomy.

Although the prospects for making contact may, indeed, be remote, a positive or even negative outcome can have significance on society substantially in excess of resources required to conduct the search.

The whole idea of other intelligent beings existing beyond our planet is both fascinating and mind-boggling. It captures the imagination, not only of space scientists but of people throughout our Nation and the world.

As one theologian has said in pondering the extent of the known universe, God certainly made a lot of matter, why not more intelligence, more free beings, who, alone, can seek and know him.

A number of highly qualified and distinguished scientists will appear before the subcommittee today and tomorrow to give their views on the subject.

The subcommittee will have the opportunity to review the nature and scope of the program and examine the rationale, re-



quirements, and issues associated with establishing, and maintaining a viable search capability.

Our first witness this morning is Dr. Richard Berendzen, provost of American University and professor of astronomy. Following him we welcome Dr. Philip Morrison, the Institute Professor and Professor of Physics at MIT. Then Dr. Noel Hinners, NASA's Associate Administrator for Space Science will be our last or wrap-up witness this morning.

We will ask the witnesses to first present their testimony and then we will have questions after all of the testimony is completed.

The House is going in session at 11 o'clock. There will be a series of votes and a lengthy interruption shortly after that. We hope in this process the delays will be held to a minimum.

Now, Dr. Berendzen, if you will proceed, we will be happy to hear from you. You may read your statement.

#### STATEMENT OF DR. BERENDZEN

Dr. BERENDZEN. Mr. Chairman and members of the Subcommittee on Space Science and Technology, I appreciate this opportunity to testify before you on the subject of SETI—the search for extraterrestrial intelligence.

In the 19th century, Thomas Carlyle considered the stars and said: "A sad spectacle. If they be inhabited, what a scope for misery and folly. If they be not inhabited, what a waste of space." Or, as Lee DuBridge, Science Adviser to President Eisenhower, put it: "Either we are alone or we are not; either way boggles the mind."

The quest for mankind's kin invariably prompts philosophical speculations. But on the basis of modern knowledge, what actually do we know about this esoteric yet enthralling subject?

As recently as a generation ago, most scientists would have argued, often *ex cathedra*, that the likelihood is low that life exists beyond Earth. However, as Martin Rees has succinctly noted, "Absence of evidence is not evidence of absence." And accumulating evidence during the last two decades has convinced many scientists world-wide that extraterrestrial life probably does exist, possibly in enormous abundance. It must be noted, however, that incontrovertible proof has yet to be found: to date, the evidence is strictly circumstantial, but it is highly suggestive and possibly compelling. Today, the serious scientific search for extraterrestrial life commands the attention and respect of many of our most prominent, careful, and judicious scientists. SETI—in its sophisticated, modern form—is solid and sober, not tawdry or sensational.

Even for many informed skeptics, the question of the existence of extraterrestrial life has become not so much one of if as of where, and with regard to the search it has even become when, for ultimate contact may be virtually inevitable. This view was summarized recently in a report by the august U.S. National Academy of Sciences:

Each passing year has seen our estimates of the probability of life in space increase, along with our capabilities of detecting it. More and more scientists feel that contact with other civilizations is no longer something beyond our dreams, but a natural event in the history of mankind that will perhaps occur within the lifetime of many of us. . . . In the long run this may be one of science's most important and most profound contributions to mankind and to our civilization.



Life, yes—but in what forms? Extraterrestrial beings almost certainly will not be humanoids. It would be too extraordinary if this planet's conditions were duplicated precisely elsewhere. Our life resulted from a lengthy, delicate evolutionary process, which would have been permanently changed if any of a multitude of parameters had been different. Thus, although many scientists believe that life, even in advanced forms, probably is ubiquitous in the universe, they are equally convinced that there are no humans beyond Earth. In Loren Eiseley's words: " \* \* \* nowhere in all space or on a thousand worlds will there be men to share our loneliness \* \* \* Of men elsewhere and beyond, there will be none forever." Even in a fertile cosmos, life here remains unique and precious.

Biochemists believe that of the 100 plus elements known to man, only one can be the basis of life, here or elsewhere—carbon. Given that terrestrial life consists of aggregates of complex carbonaceous polymers, such an assertion may appear chauvinistic; actually it reflects nature itself. Carbon is the only known element capable of forming the intricate molecules so seemingly essential to anything approximating life as we understand it [fig. 1].



FIGURE 1.—“The Creation of Man” from the Cappella Sistina.

The insightful question about the genesis of life on Earth is not, “Was it miraculous?” but rather, “Was it unique?” In the 1920's the biochemists Haldane in England and Oparin in Russia independently suggested that organic compounds could be produced from elementary inorganic molecules. Increased knowledge about the Earth's primordial atmosphere indicated that it had contained appropriate ingredients for the origin of life—hydrogen, ammonia, methane, water vapor. Then, in the early 1950's, Miller and Urey dramatically showed that these molecules would form amino



acids if subjected to a laboratory simulation of the Earth's early conditions [fig. 2].

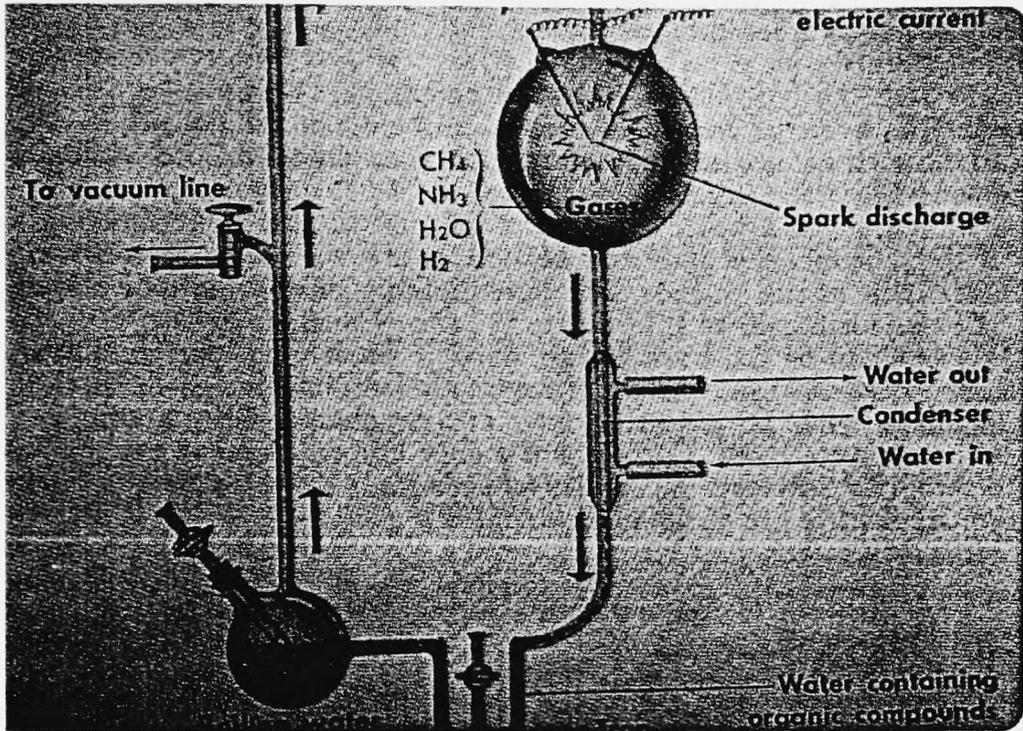


FIGURE 2.—Miller/Urey synthesis of amino acids.

Subsequent research supports the conclusion that life likely will arise spontaneously, given the right initial situation. Of course, if we are wrong on this, irrespective of the cosmos's vastness, we could be alone. Even though terrestrial life now is hearty and adaptable, any one of a multitude of possible calamities could have prevented our development, such as inadequate supply of terrestrial water, severe irregularities in solar luminosity, major dynamical perturbations by the Moon or planets, or collision with another astronomical object. Our biological beginnings may have been special, even unique; or perhaps no other suitable habitat exists. But besides the philosophical repugnance of this egocentric view, modern findings seem contrary.

Therefore, many of us believe that, under reasonable conditions, chemical and biological evolution will be inevitable. And, although the appearance of extraterrestrials may be wildly different from our own, their chemistry will be at least remotely similar. If this last assumption is false, then the following statements will constitute a conservative lower bound; that is, if noncarbon based life is possible, the overall prevalence of life will be even greater, [fig. 3].



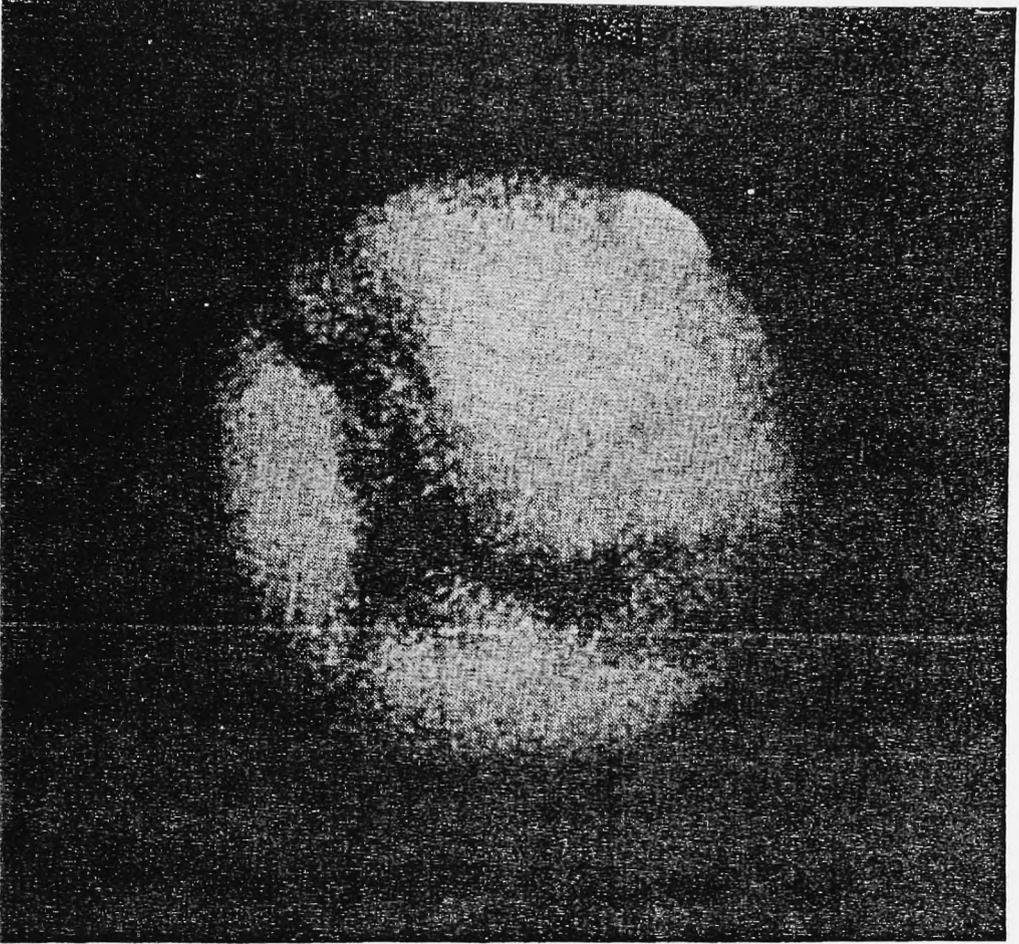


FIGURE 3.—Mars, as photographed in 1909.

Of all the celestial objects, the one that for the longest time has prompted man to wonder if he is alone is the enigmatic Mars. Like Earth, it has seasons during which its polar caps shrink in the spring, while its equatorial regions darken as if vegetation were being nourished by melting liquids [fig. 4].







our understanding of Mars changed dramatically as a NASA probe returned by far the best photographs seen of the planet until that time [fig. 5].

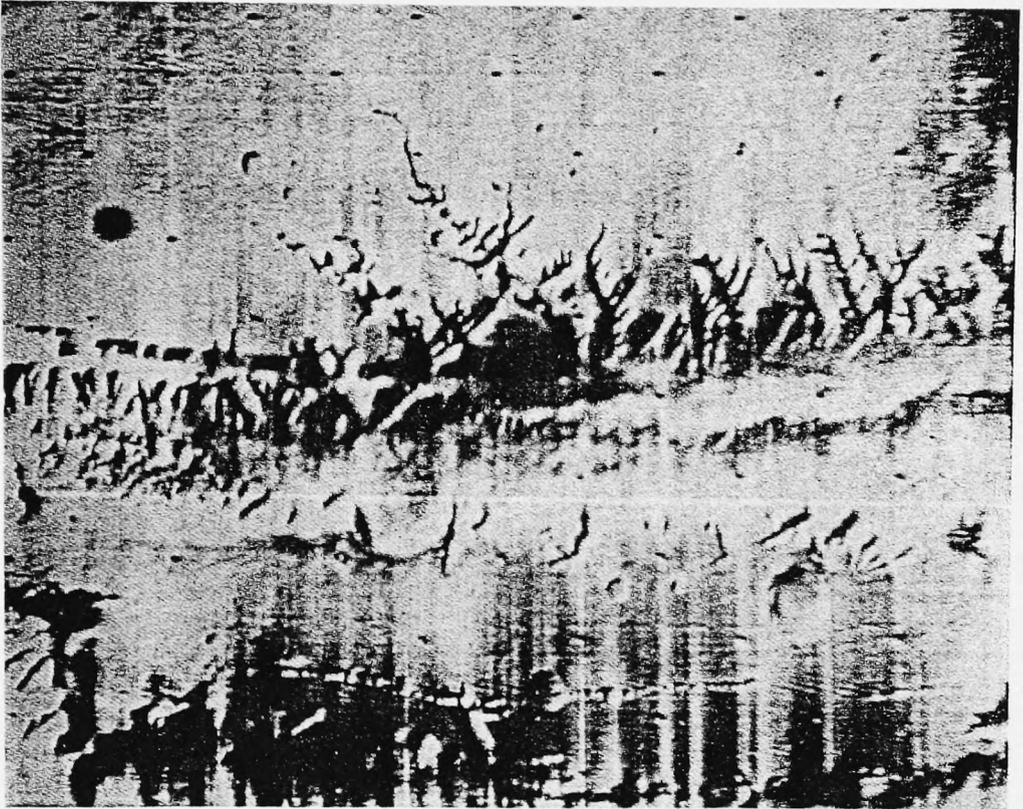


FIGURE 5.—Mariner 9 view of Martian canyon system, roughly four times the depth of our Grand Canyon and as long as from California to New York.

Although they did not settle the question of Martian life, they revealed a fascinating world, with gargantuan volcanos and colossal canyons. The fabled canals were revealed to be nothing more than undulating natural depressions, and the equatorial darkenings were explicable without life forms.

Still, we do not know about the possible presence of microscopic Martian life. The rigors of Mars would make life difficult although not insuperable. NASA's Viking mission has been stupendous but many questions remain. If even elementary life forms were discovered on Mars or elsewhere in the solar system, we would be emboldened, for the presence of two life-supporting objects about a single star would strongly imply prevalence elsewhere.

But our discussion today focuses on intelligent extraterrestrial life, and that unquestionably implies a search outside our solar system.

To proceed, let us assume that life will arise only, if not reside permanently, on planets, not in interstellar gas clouds or elsewhere. Again, if we were wrong on this, our estimates would be too conservative [fig. 6].



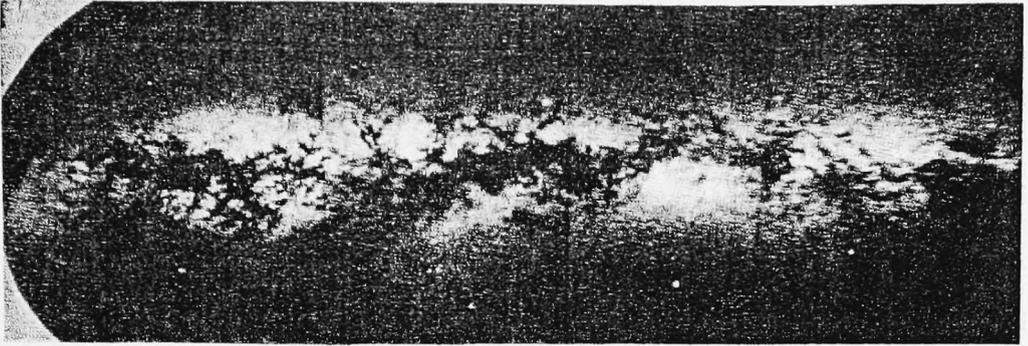


FIGURE 6.—Mosaic of the Milky Way.

Our story now must turn to the planets's parents, the stars. Our Sun is one of more than 250 billion stars composing the Milky Way galaxy, in a universe containing tens of billions of galaxies. In fact, there are more stars in the heavens than there are grains of sand on the beaches of Earth. Of these multitudes of stars, which might make suitable parents for life-supporting planets? A candidate star should be moderately luminous, roughly like the Sun. Extremely luminous stars burn nuclear material so rapidly that their lifetimes are mere fractions of the Sun's; hence, life near them probably would have insufficient time to undergo the arduous process of evolution. In contrast, extremely under-luminous stars burn slowly, permitting biological evolution; but their ecospheres, or habitable zones, would be small, requiring potentially life-supporting planets to orbit so close that they would be forced to keep one face perpetually toward the star, thereby evaporating their atmosphere on one side while freezing it on the other. A candidate star should also be temporarily stable; it should not pulse in size, or erupt, or throw off gaseous shells, or spin frantically. Therefore, we omit as prime candidates such objects as variable stars, novae, planetary nebulae, and pulsars [fig. 7].



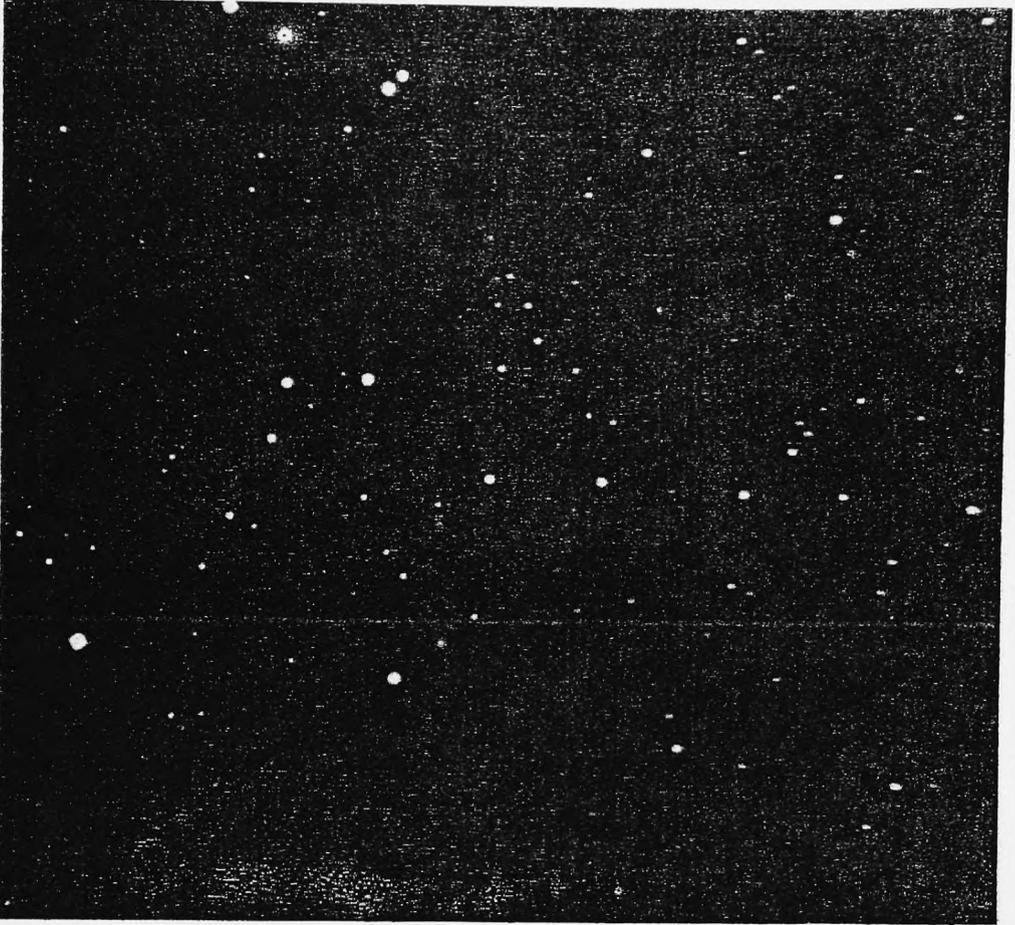


FIGURE 7.—Planetary nebula—an ultra hot central star, shedding a vast shell of gas.

The planets themselves must not undergo dynamical perturbations. Roughly half the stars in our galaxy are members of multiple systems in which two or more stars lock gravitationally and orbit each other. Planets around a member of such a system might be perturbed by the other star. To be cautious, let us discard all multiple star systems as being dangerous abodes for life [fig. 8].



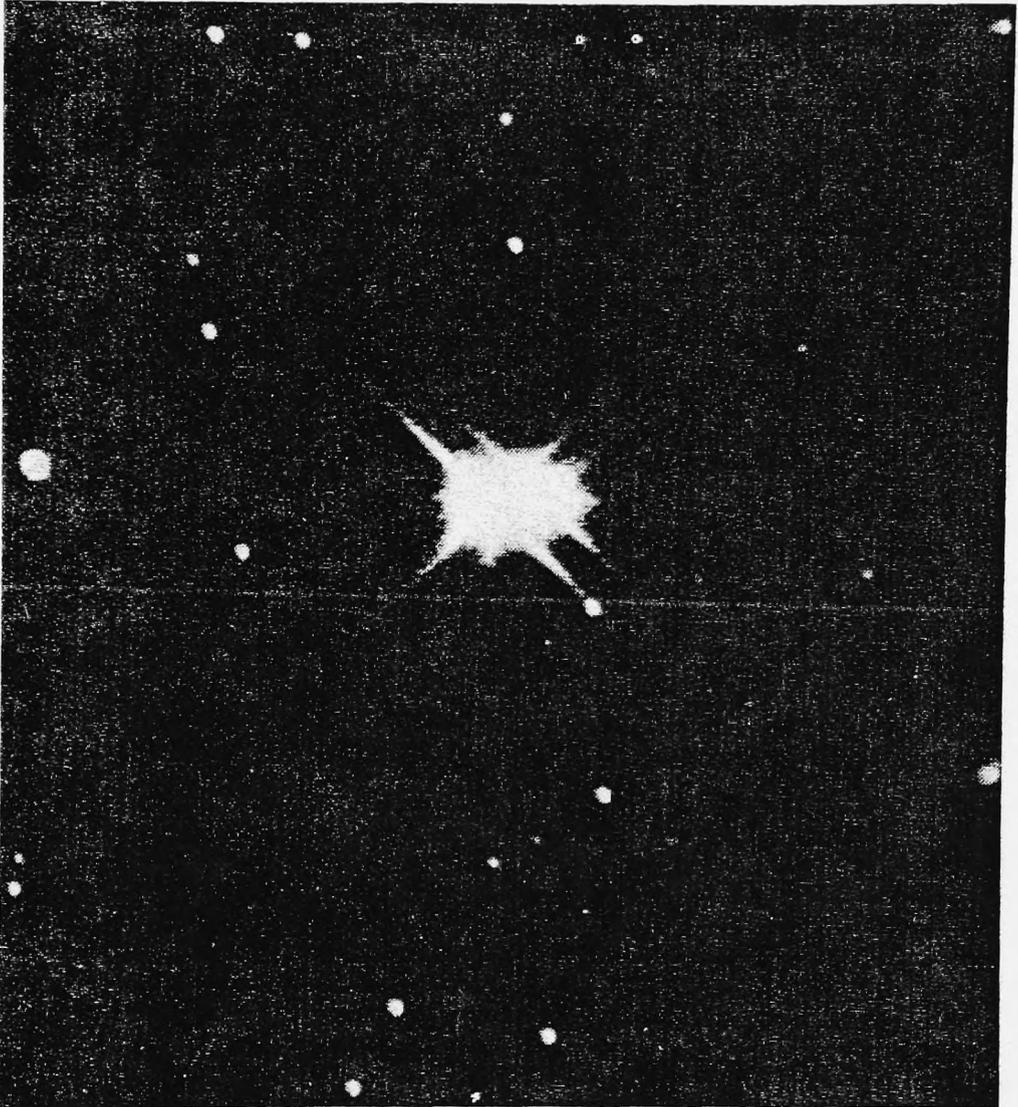


FIGURE 8.—Binary star system.

The next question is whether any of the suitable stars actually possess planets. Several independent lines of evidence suggest they do. Theory indicates that stars form by condensing gravitationally from the gas and dust that lace throughout a galaxy's spiral arms. As such a nebula contracts, apparently it can split into clumps of different mass: a large one with several small companions, leading to a single star with planets; or several of comparable size, leading to a multiple star system. In principle, a substantial fraction of the suitable stars could possess planets [fig. 9].



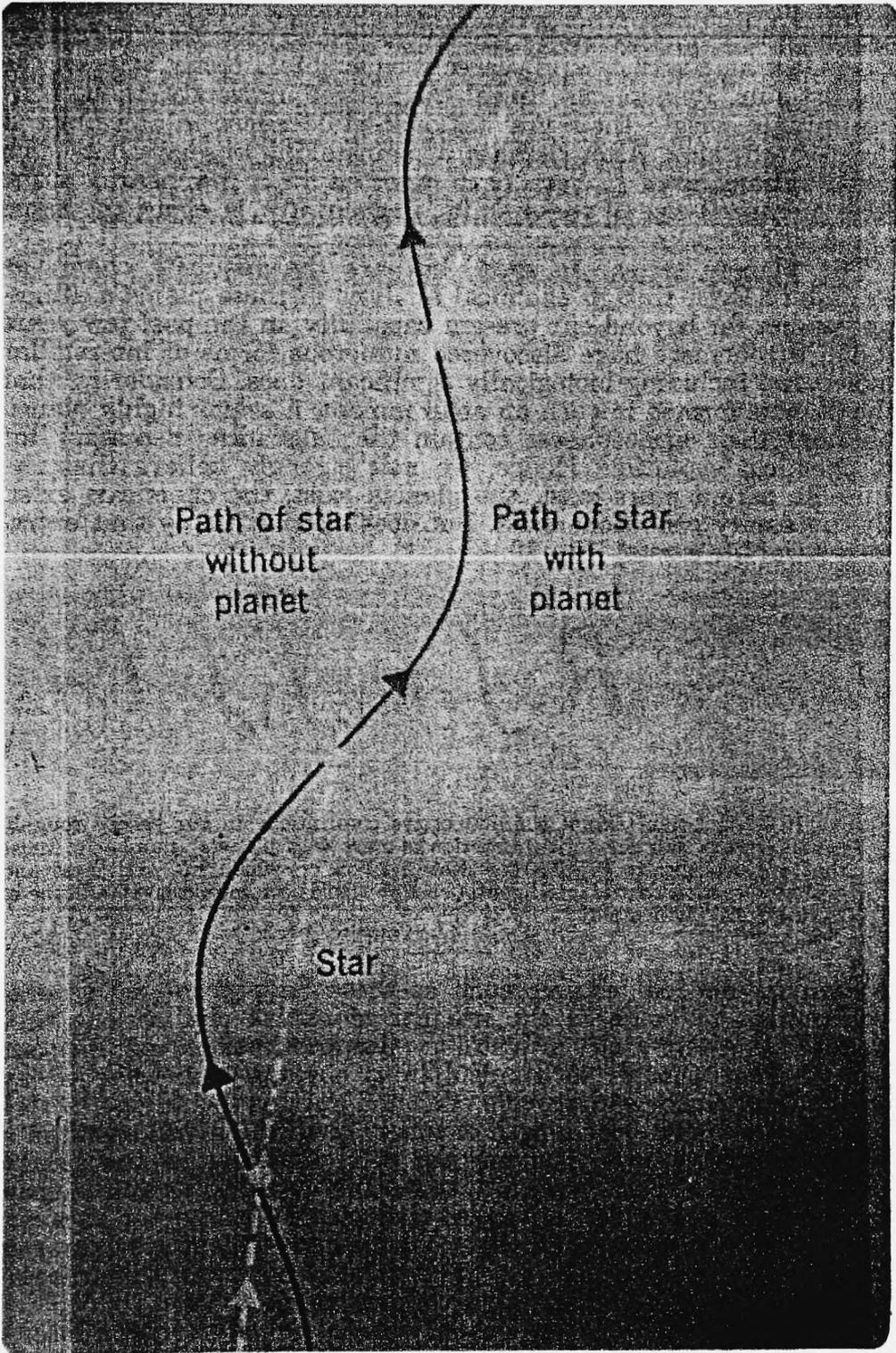


FIGURE 9.—Perturbation of a star's motion by an orbiting planet.



And planets appear to be observed orbiting some of the stars nearest the Sun. Interstellar distances are so vast that a Jupiter, much less an Earth, would scarcely be visible directly at even the nearest star. Planets can reveal themselves indirectly, though, by the minute wiggle their gravitational pull causes in the path of their parent star. After decades of painstaking observations, such perturbations seem to have been detected for a few nearby stars. (The measured size of these shifts, incidentally, is about the same as a hair's angular diameter as seen at one mile.)

The planets appear to exist, but are the requisite chemicals available? Even though chemical analysis of these planet's atmospheres lies far beyond our present capability, in the past few years radio astronomers have discovered numerous forms of interstellar molecules, including biologically significant ones. Considering that the planets formed in such an environment, it seems highly plausible that their atmospheres contain the substances necessary for biochemical evolution. Hence, we now strongly believe that the suitable parent stars exist, the planets exist, the chemicals exist, the necessary conditions exist. But does all that imply that extra-terrestrial life exists? [Fig. 10.]

$$N = R_* f_p n_e f_l f_i f_c L$$

FIGURE 10.—Number of extant communicative civilizations in our galaxy roughly equals (rate of star formation) (fraction of stars with planets) (number of those that are ecologically suitable for life) (fraction on which life originates and evolves) (fraction that is intelligent) (fraction that is communicative) (lifetime of technological civilization).

Findings and conjectures such as these have prompted several bold efforts to estimate the number of extant civilizations that might be attempting interstellar communications within our galaxy. Even though such calculations obviously are plagued by unknowns, science can provide educated guesses for all the factors involved, save one—the length of time a civilization will spend in a communicative phase. The number of civilizations in the galaxy today turns out mathematically to roughly equal to this time span in years; for example, if advanced civilizations survive for an average of a million years, then about one million of them are extant in our galaxy. Paradoxically, the greatest uncertainty in this calculation may arise not from the study of nature but of Man.

Within the solar system we can search directly for non-intelligent lower life forms like ferns or bacteria. But outside our parochial environment, the hypothetical creatures must be communicative if we are to find them. The most dramatic way to communicate would be through direct contact; however, even using wild extrapolations of contemporary technology, deep space voyages seem highly problematic. If a rocket could burn with the efficiency of the



solar interior, the energy requirements for a round trip even to a nearby star still would be enormous. Such pessimistic conclusions are based not on our ability in technology but on our understanding of nature. The distances between stars are characteristically light-years; yet, Einstein showed [figs. 11, 12, 13, 14] us that no craft can travel faster than the speed of light and even to approach that rate would require titanic expenditures of energy. Although perhaps ingenious solutions eventually can be found, most astronomers today concur that interstellar space travel will be untenable, at least for Earthlings, probably for generations to come.





FIGURE 11.—Earliest known existing photograph of Albert Einstein.



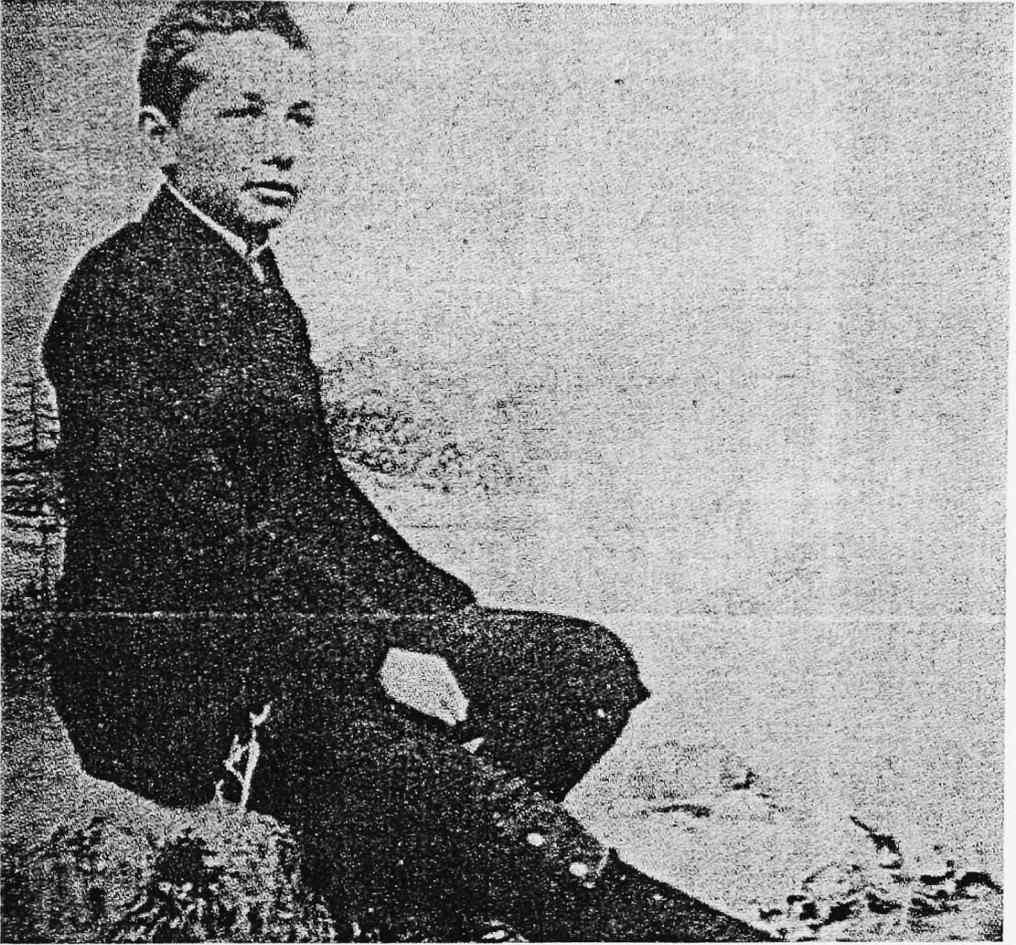


FIGURE 12.—Einstein at age 14.



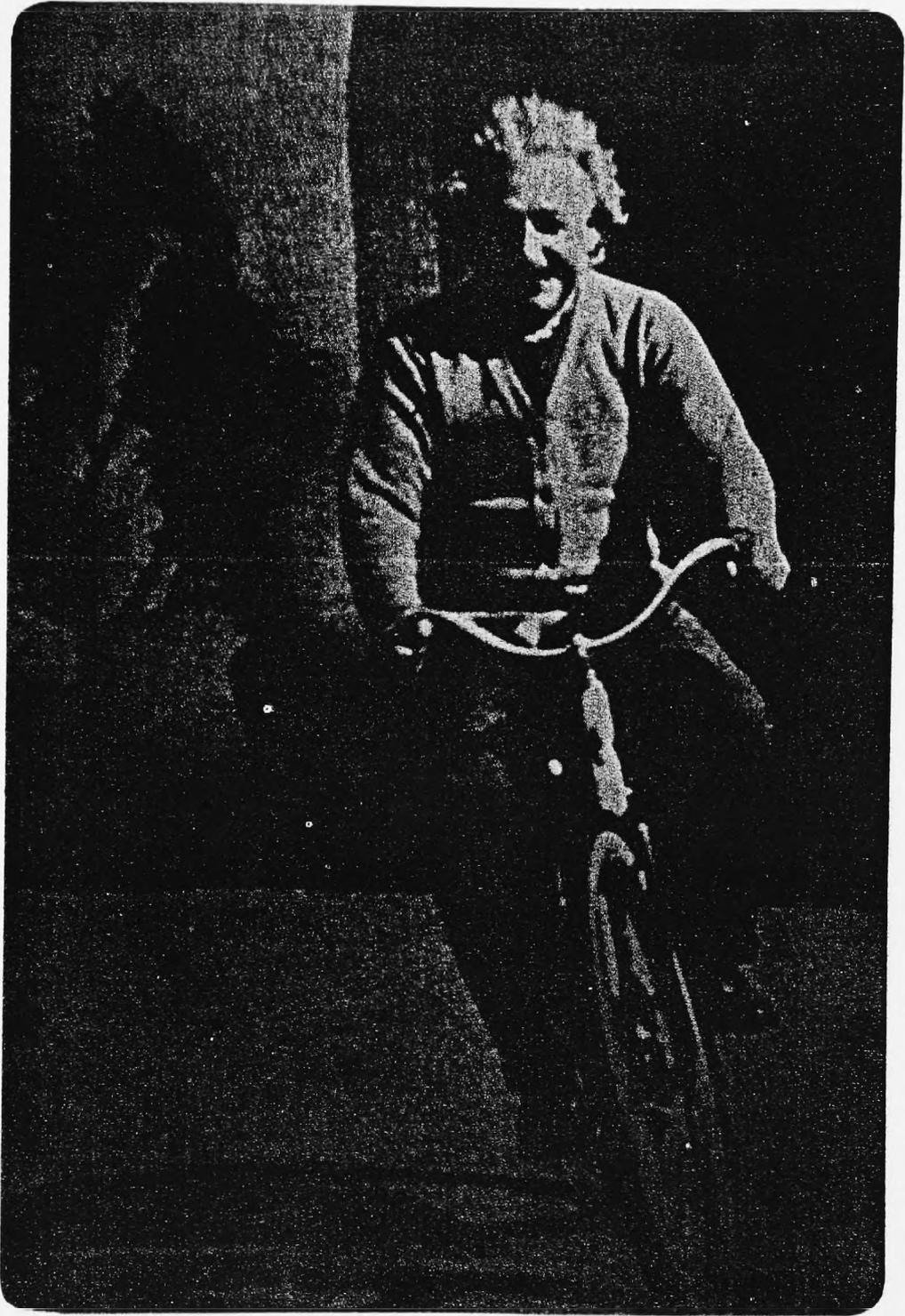


FIGURE 13.—Einstein relaxing at Caltech.



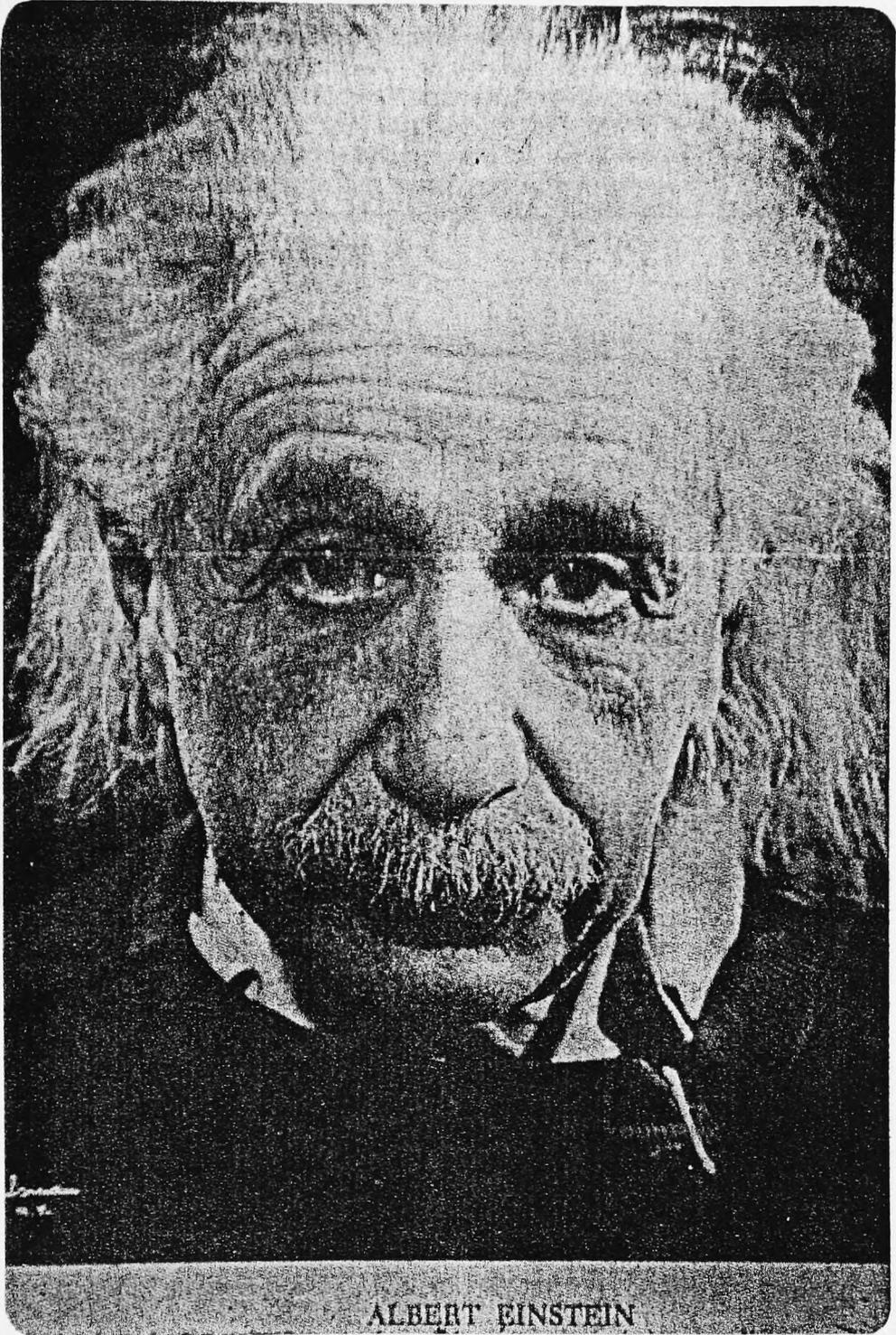


FIGURE 14.—Einstein, the classic view.



Nevertheless, interstellar communication could be achieved immediately by using the fastest, most efficient mode known—the electromagnetic spectrum, of which visible light is only a small portion. In the 19th century, several imaginative but vain proposals [fig. 15] were made for attempting visual contact. The mathematician Gauss, for instance, suggested planting a pine forest in Siberia in the shape of a Pythagorean triangle, as a detectable sign of terrestrial intelligence. And an astronomer proposed signalling by burning kerosene in a 20 mile-wide ditch to be dug in the Sahara desert [fig. 16].

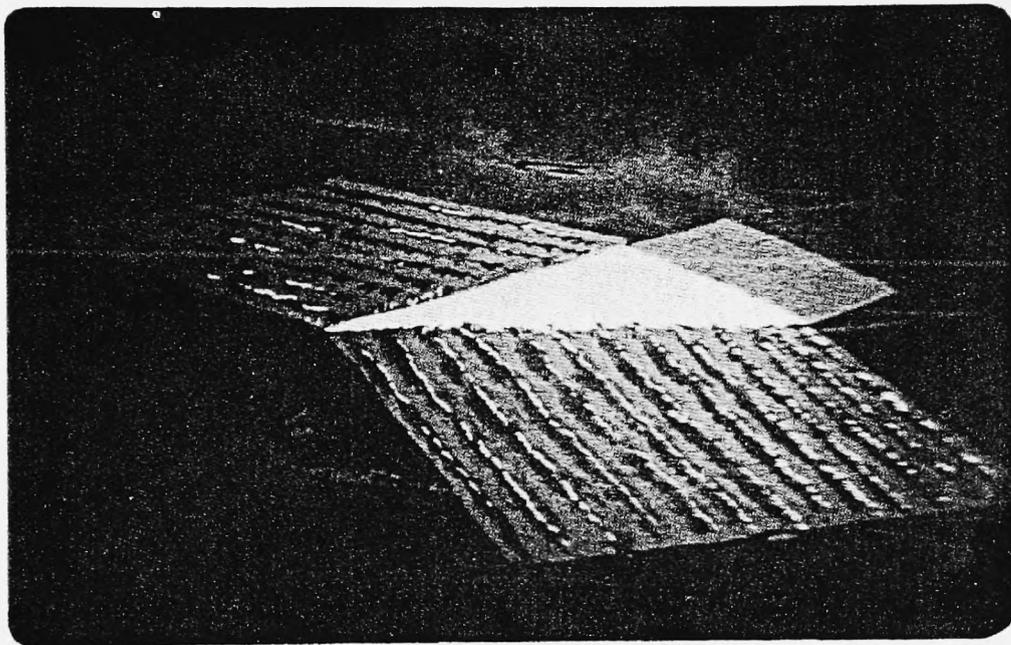


FIGURE 15.—Gauss' plan for interplanetary communication, c. 1820.





FIGURE 16.—Von Littrow's plan for interplanetary communication, *c.* 1840.

Are we equally naive today? Undoubtedly! That unprofound observation, however, should not prevent action; if it were to do so, we would ensure the blunting of mankind's ingenuity. To paraphrase William James, only by risking do we live at all.

Astronomers now study the heavens with radio waves as well as with visible light, and such bands now seem to offer the uniquely rational mode for interstellar communication: They are easily transmitted and received, propagate over interstellar distances without being absorbed, stand out against background stars, and travel at the maximum speed in nature—that of light [fig. 17].



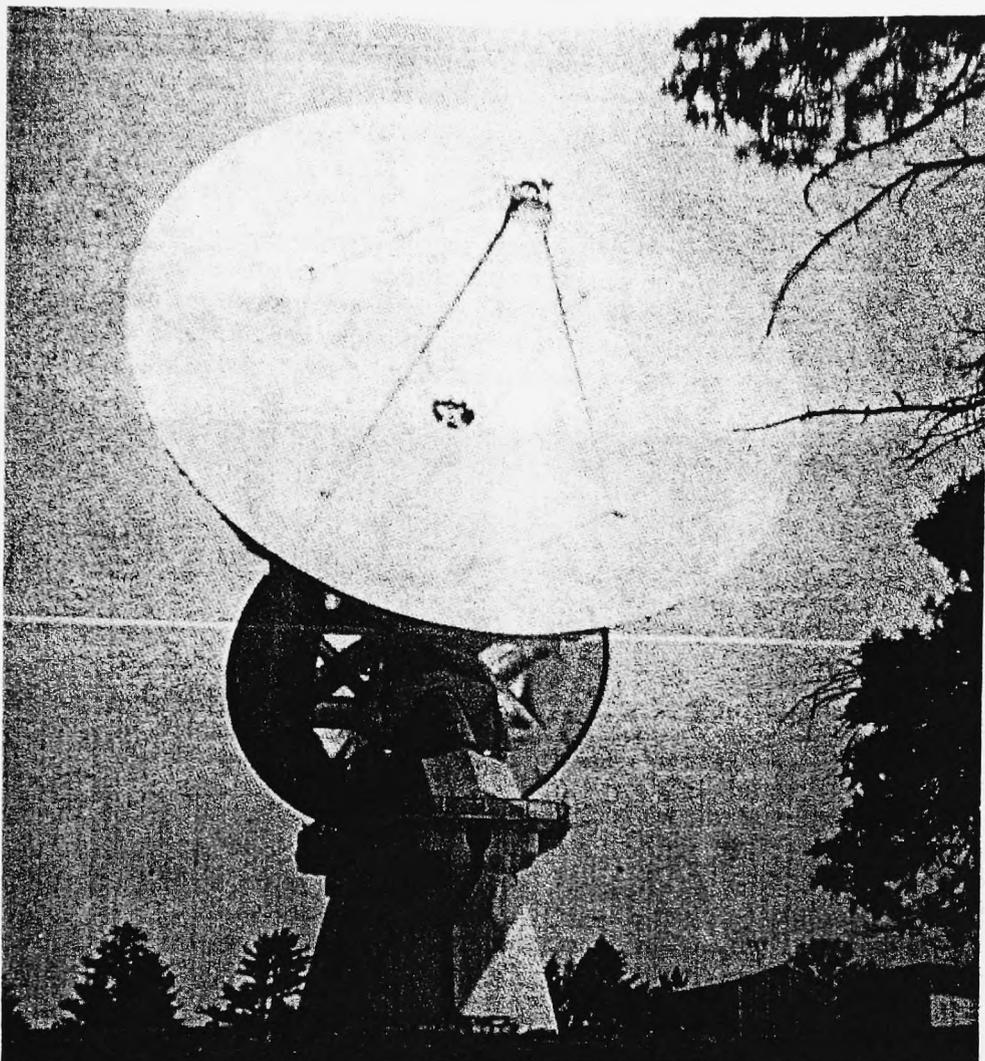


FIGURE 17.—140-foot radio telescope at Green Bank, W. Va.

Coincidentally, we on Earth inadvertently have been sending out such signals for the past few decades, through our use of radio and television and of military and scientific radar. At present, mankind's major interstellar emissary is commercial radio and TV.

But to communicate this way, we should follow a play on Biblical dictum: In the case of SETI, it is better to receive than to send. We fail to know not only exactly where and how to send but also when. Even if we knew that a certain star had a habitable planet and that radio was the proper medium, our message might not arrive there at the proper epoch: It could come before the inhabitants had evolved to the level of radio technology or after they had passed that era or even after they had annihilated themselves. If we were to receive a signal, such problems would vanish. On the other hand, if each civilization were to follow such logic, none would transmit, at least until it could do so without being exorbitantly consumptive. Because our radio ability is in its infancy, we are as technologically primitive as a civilization can be and yet achieve



interstellar communication. Thus, if we make contact, the other beings must be as scientifically advanced as we, and possible eons ahead.

On 8 April 1960, the first serious attempt by man was made to intercept signals from other beings, when a radio telescope was tuned to 21 cm. (1420 MHz) and pointed at a Sun-like star about 10.5 light-years away. Since then other searches have been made in the United States and abroad, looking toward a handful of nearby candidate stars, usually at 21 cm., because interstellar hydrogen emits at that wavelength, producing a prominent natural marker presumably known to all advanced beings [fig. 18].

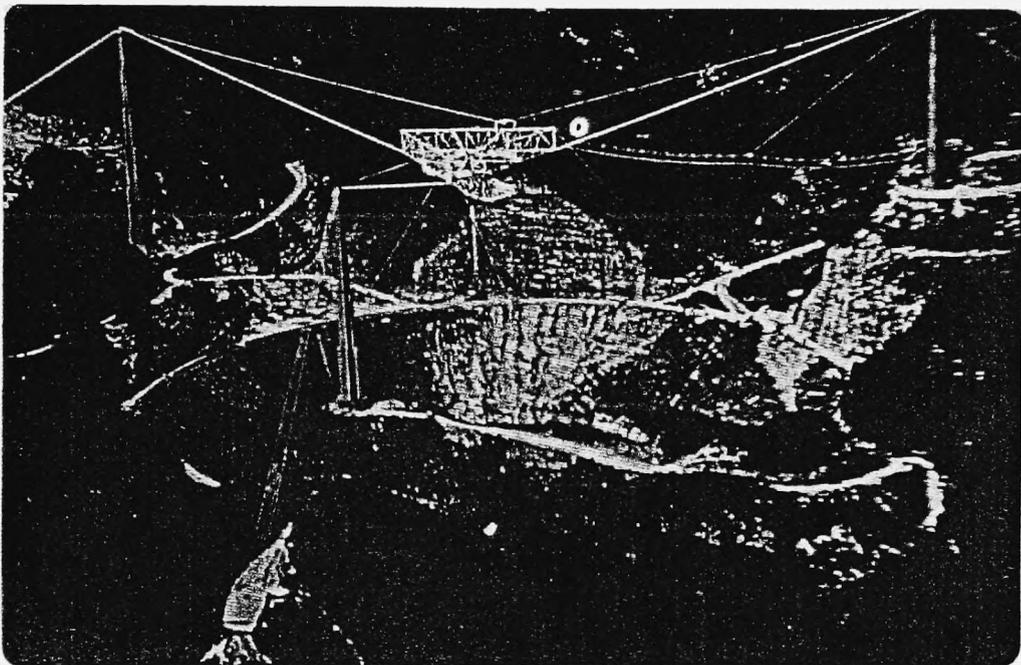


FIG. 18.—1,000-foot radio dish at Arecibo, P.R.

The attempts to date have been negative, as one would expect considering the range of unknowns involved. Nevertheless, it is sobering to realize that today the world's largest radio telescope—the 300 meter diameter surface at Arecibo, Puerto Rico—would be capable of communicating with its twin, if one existed, even in a distant part of the galaxy.

Contemporary science is already able to identify candidate stars and likely wavelengths, and contemporary equipment could detect the signals if they were transmitted with devices no more powerful than our own. These are but a few of the reasons why many serious scientists treat the search with new respect: Science fiction is rapidly becoming science fact.

Presumably, the initial discovery of life elsewhere will be made one-way and at-a-distance; i.e., we will infer the existence of life on another planet by detecting non-natural, quasi-coded radio emissions from it. Such signals could be intentional messages or inadvertent radio noise, revealing the other planet's advanced technology. Of course, other scenarios probably will occur, especially



among advanced beings, but irrespective of the details, inhabitants here seemingly will have at least these options: to avoid response; to return a radio signal that can be readily detected and decoded; or, possibly, to launch spacecraft toward the other planet. Clearly, if Earth ever discovers such a radio emission, mankind will face monumental decisions.

Regardless of the wisdom of making initial contact, it could occur serendipitously: At our modern level of technology, we might accidentally discover other civilizations or vice versa. Although we can only poorly affect the ability of others to detect us, we can strongly influence our capability to find them, by making concerted searches. But should they be made? [Fig. 19.]



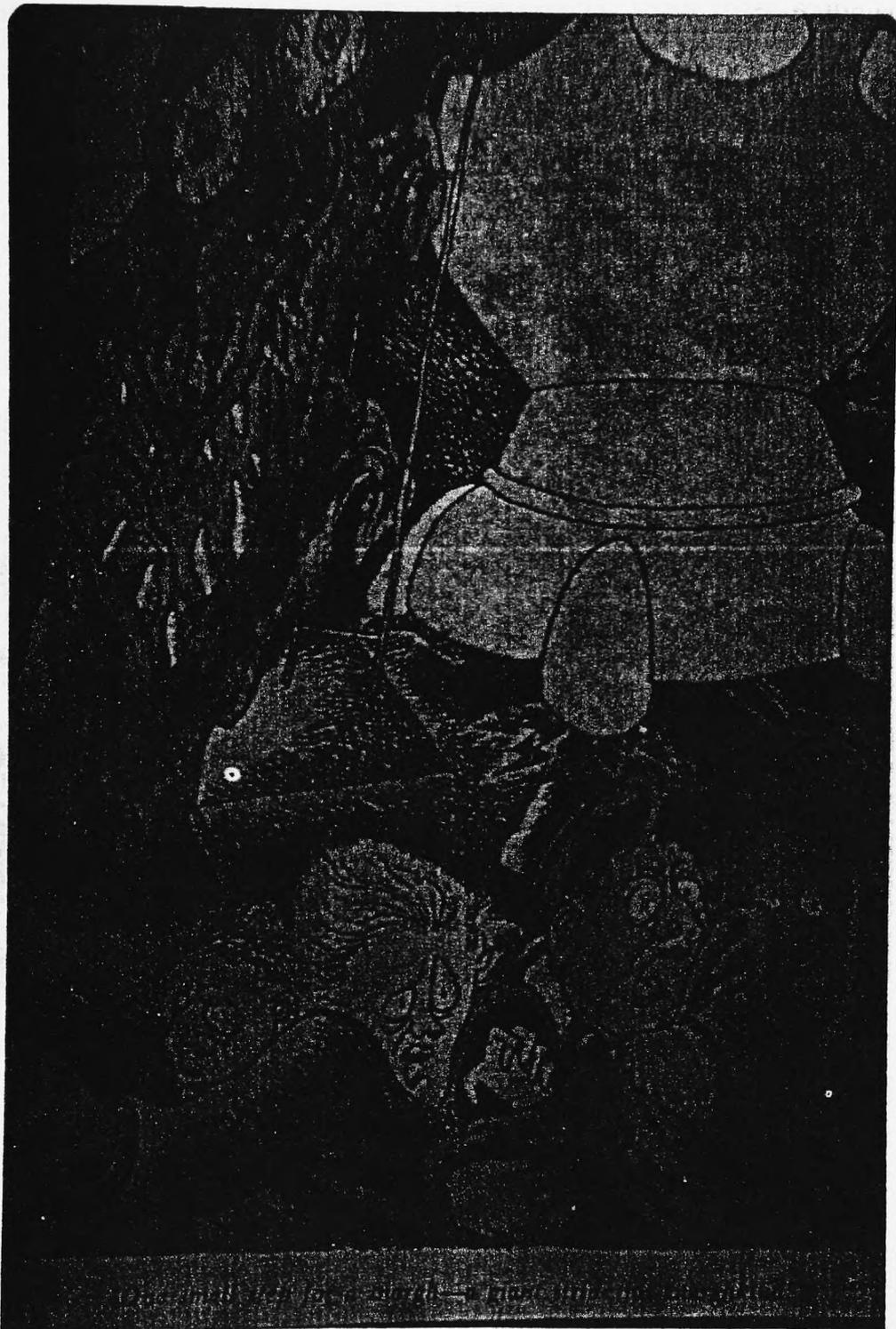


FIG. 19.—Cartoon with caption: "One small step for a znargh—a giant stride for znarghkind!"

Science fiction is replete with tales of potential hazards, a common one being the invasion of Earth. But such holocausts would require the other beings to be aware of us, as well as we of



them; also, it assumes the feasibility of interstellar space flight. As another danger, the beings elsewhere could subvert us intentionally with their seemingly beneficent broadcasts, playing on our ignorance. Obviously mankind would have to proceed carefully, guarding against impetuosity or gullibility. And there is the serious possibility of culture shock. Just the realization that we truly are not alone might be traumatic, but would it threaten our egos or shatter our institutions? Even the converse could happen: At last the commonality of all Earthlings might become more apparent. But suppose the messages we received were both benign and voluminous, enabling us to leap centuries in knowledge. Would that celestial umbilical cord rob us of our own ingenuity, or would it inspire us to new heights? [Fig. 20.]

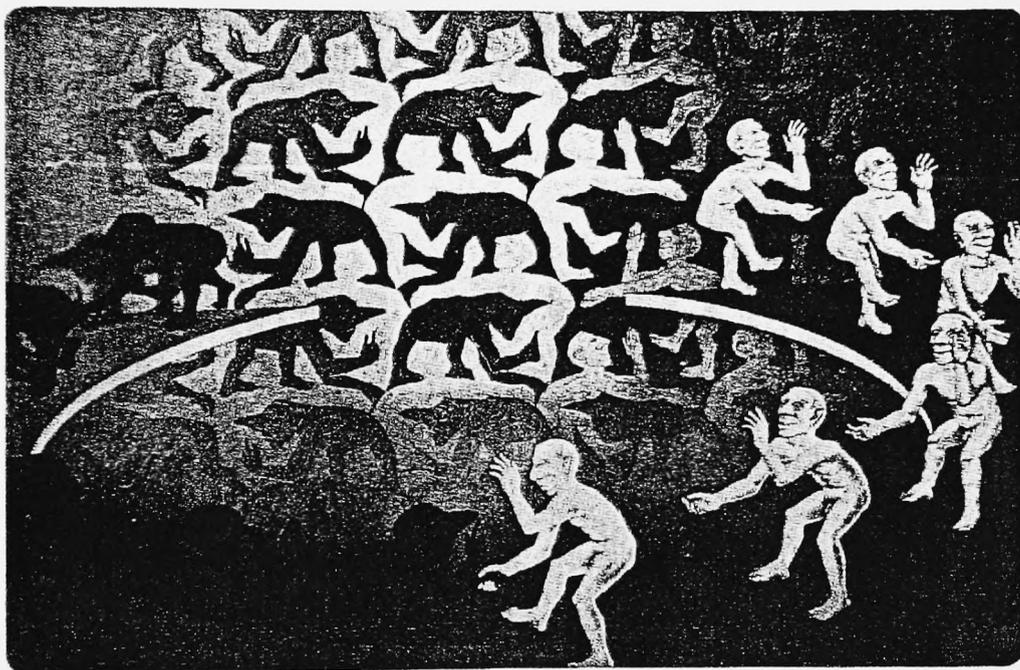


FIG. 20.—“Encounter” by M. C. Escher: Would friendship and a galactic community occur?

The most frequently cited benefit to come from contact would be instant technological gain. Even though this might occur, the immense distances militate against lively repartee. A two-way conversation would require tens or possibly hundreds of years, even using radio and anti-cryptography (that is, readily readable messages based on parameters in nature). But if information could be transmitted and deciphered—from an advanced, friendly civilization to us—it might enhance our understanding not only of science and technology but also of arts and humanities. Perhaps it would provide us with new aesthetic forms, raising our consciousness and making life more rewarding. And the very existence of such long-lived civilizations would prove that the rigors of survival are not inevitably debilitating. In short, we possibly could join what has been called the “galactic heritage”.



To view this matter properly and possibly even to perceive ourselves in rightful context, we should consider briefly science's greatest story—that of cosmic evolution. Literally, it is the saga of all that was, is, or will ever be. And it rests upon that most profound concept: time.

We kill time, spend time, lose time, make time, beat time, take time, waste time; but we virtually never consider time, much less understand it. Now, let us go back in time, even to its beginning.

For countless ages, there truly was darkness on the face of the deep. Then, some 15 billion years ago, a cataclysmic explosion brought forth the cosmos itself. And so the universe was born [fig. 21].

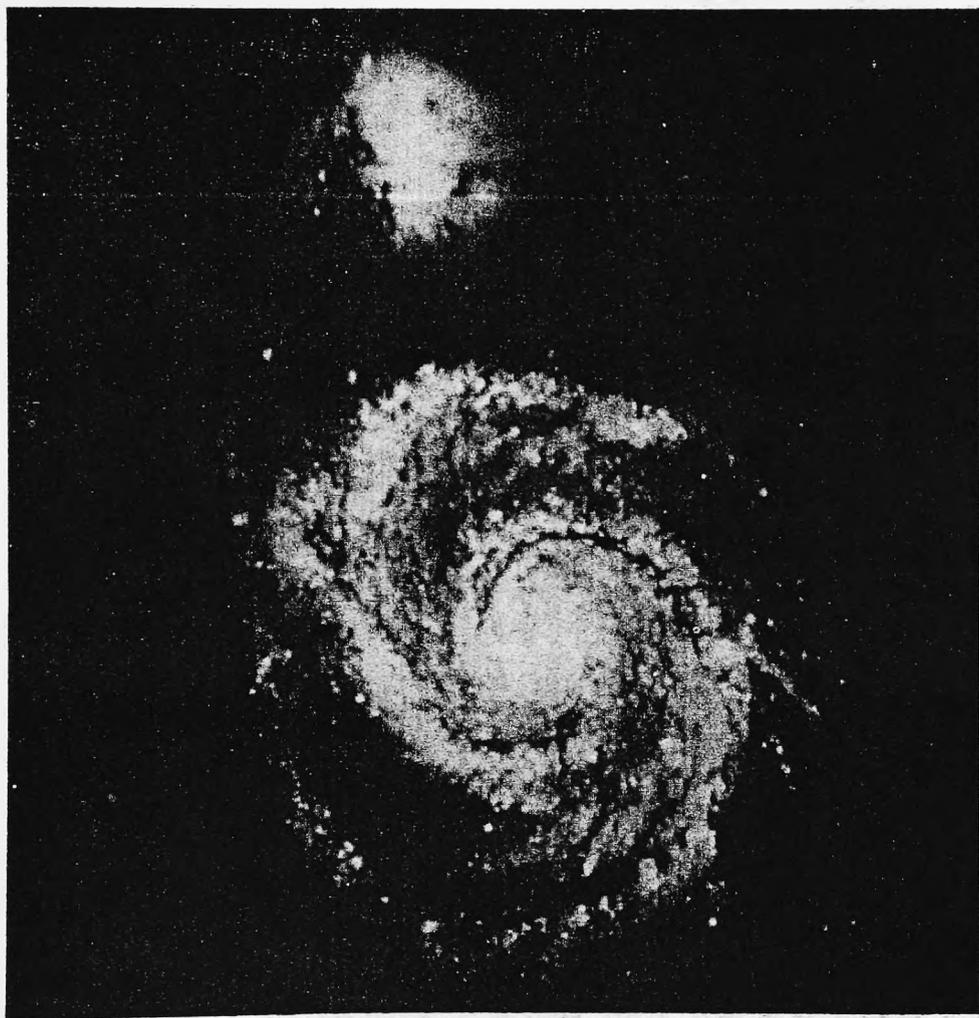


FIGURE 21.—Spiral galaxy.

Even today we live in the lingering remnants of the radiation created by that awesome fireball. As the initial blast expanded and cooled, thermonuclear reactions produced elements from the primeval soup of fundamental particles. Then, billions of years after the explosion, large ensembles of material clumped because of their mutual attraction. And so the protogalaxies were born [fig. 22].





FIGURE 22.—Interstellar gas and dust.



In those early galaxies, some of the gas and dust congealed by its own gravity to form large, amorphous, spherical balls, which slowly shrank and ultimately flickered into light. And so the stars were born.

In the hearts of those stars, nuclear furnaces raged. In a form of stellar alchemy, hundreds of millions of tons of hydrogen were converted into helium every second. They have burned thus for all these billions of years. As the stars formed, debris was left behind, some of which locked gravitationally to the stars. And so the planets were born.

Surrounding many of those planets must have been early atmospheres, perhaps similar to the Earth's in its primordial days. Some of those atmospheres must have contained the very chemicals that later brought forth life on Earth. And the same processes that were to operate here might even then have operated elsewhere, possibly thousands or millions or even hundreds of millions of times. Ubiquitous life may have flickered into existence and started onto the laborious path of evolution. And if so, intelligent, potentially communicative life was born [fig. 23].



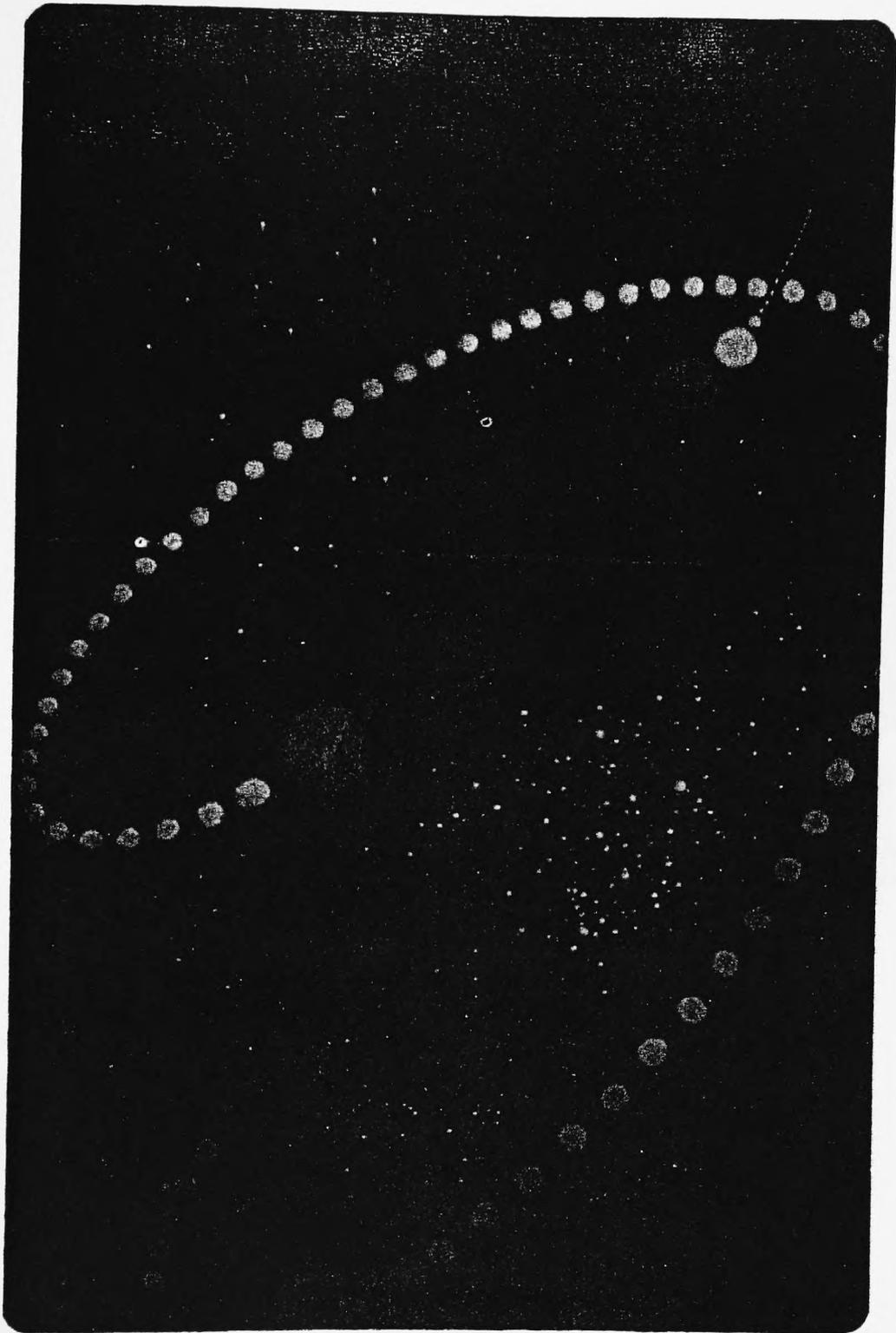


FIGURE 23.—Evolution of Sun-like stars, from interstellar matter, through a long period of steady luminosity, to expansion, contraction, and death



Billions of years went by, with galaxies forming and stars burning, expanding, exploding, shrinking, collapsing, dying. Then, some five billion years ago, on the remote periphery of an average galaxy in an unexceptional part of the universe, a particular star was born [fig. 24].



FIGURE 24.—Mosaic painting of the entire sky, showing the Milky Way, composed of gas, dust, and tens of billions of stars, including our Sun.

On a piece of debris No. 3, special events were to take place, for several billion years later the chemical and biological processes [fig. 25] of that young planet's atmosphere and oceans yielded a strange new apparition—Man [fig. 26].



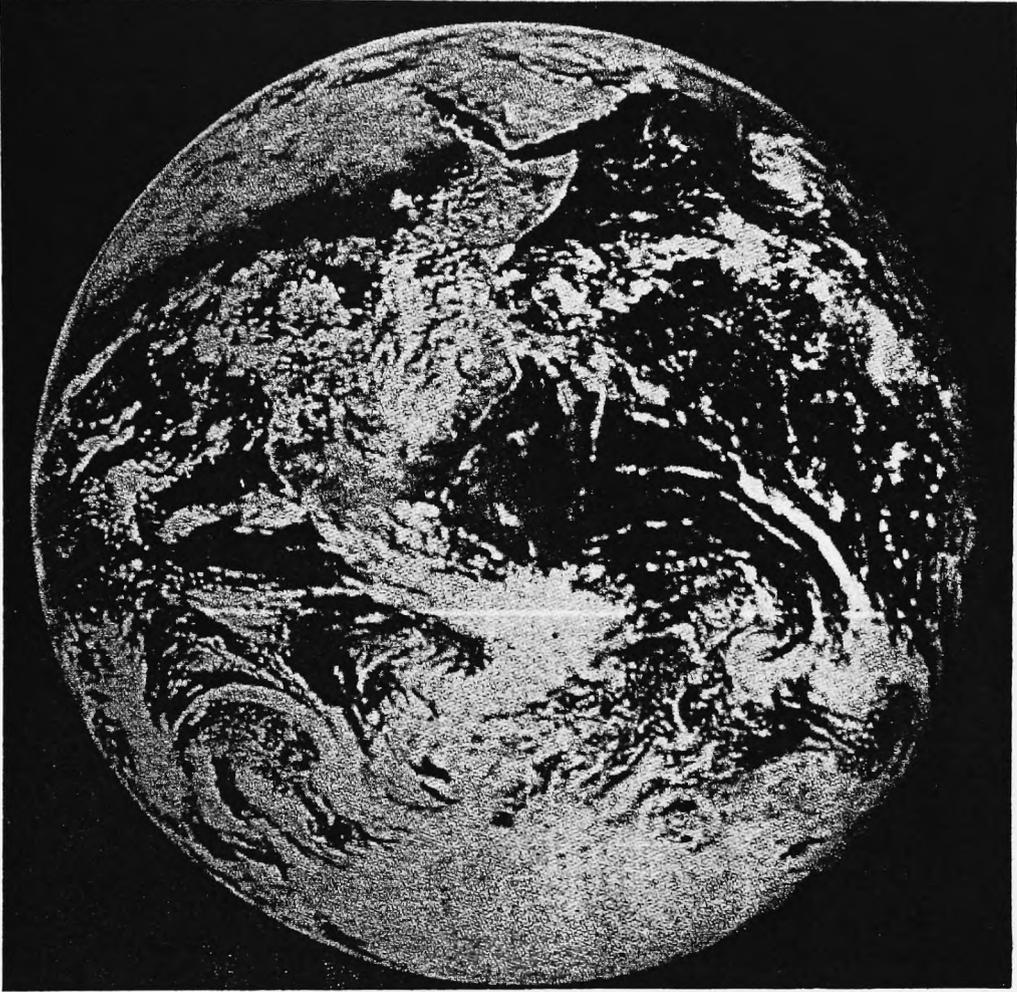


FIGURE 25.—Earth, as seen by Apollo 17.



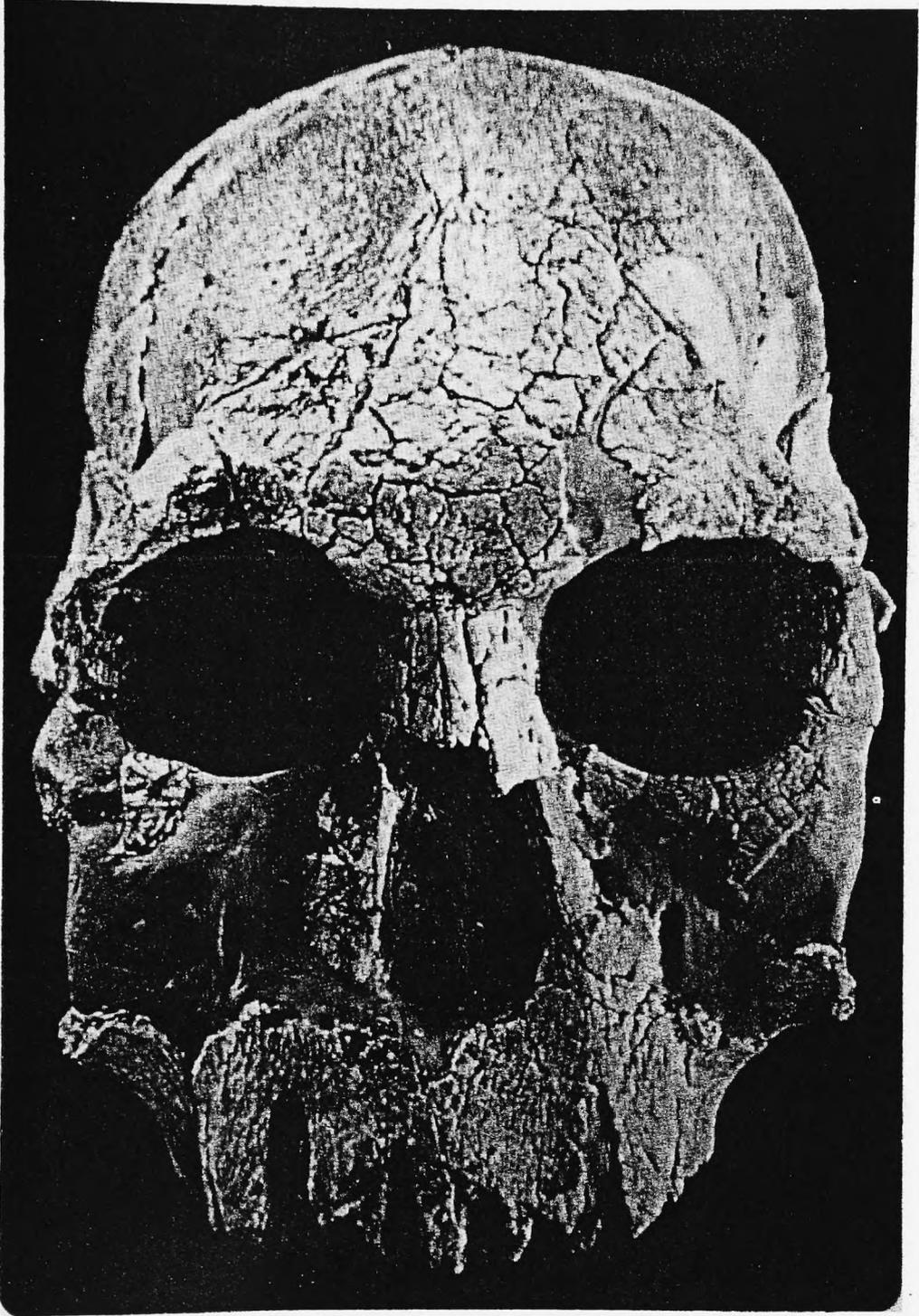


FIGURE 26.—Australopithecus Robustus, predecessor of Man.

After more eons elapsed, Man evolved, creating societies and civilizations and education and nations and war and peace and law and [fig. 27] science and technology and our modern world. And here we are in the year marked in time as 1978 [fig. 28].



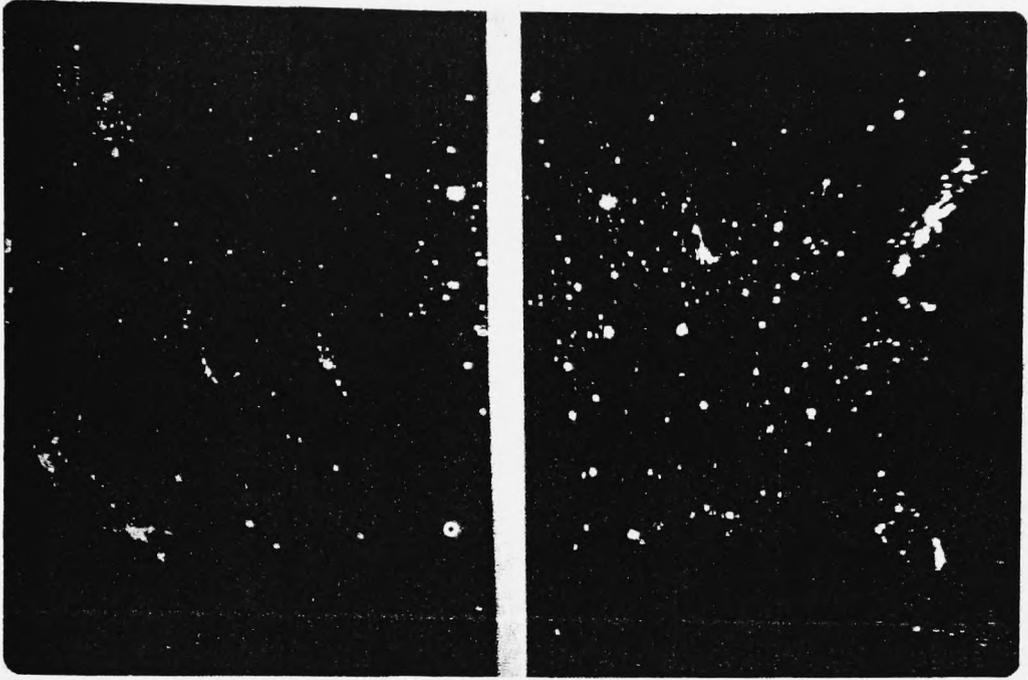


FIGURE 27.—The United States as photographed by Earth satellite, showing centers of heat and light: a Rorschach test, continent-wide.



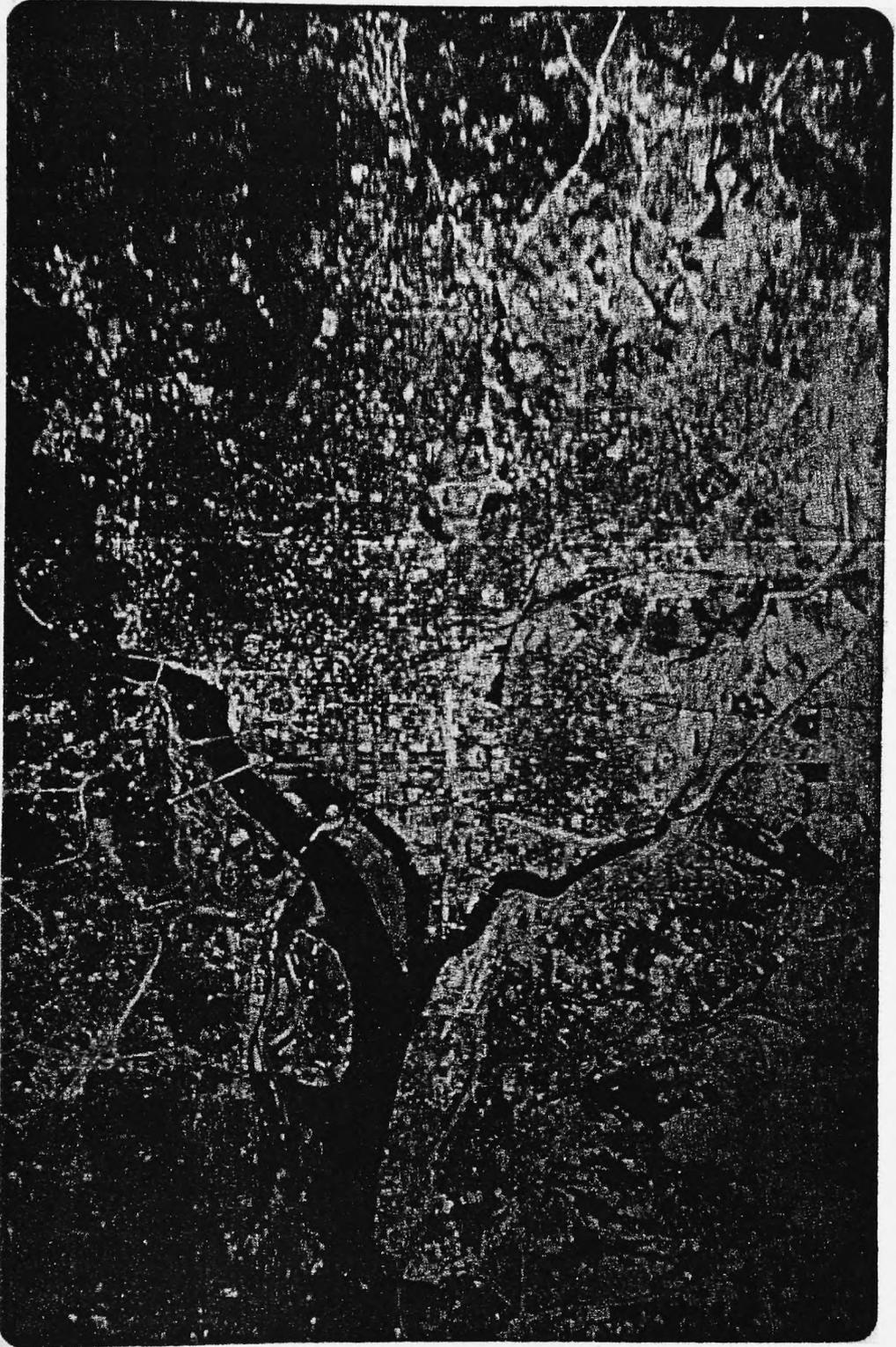


FIGURE 28.—Washington, D.C., in infrared light, as photographed from an aircraft.



In such a cosmic sweep through space and time any rational person must feel an almost overpowering smallness. Our day-to-day concerns and trite prejudices shrink into microscopic, infinitesimal nothingness.

Yet, there is another side, and it gives dignity and worth to life itself. For even though our physiques are but specks in space and our lifespans are but ticks in time, we nonetheless can comprehend a system far vaster than ourselves. Our ethical and moral concerns, together with this ability, constitute some of the most ennobling characteristics of humankind [fig. 29].

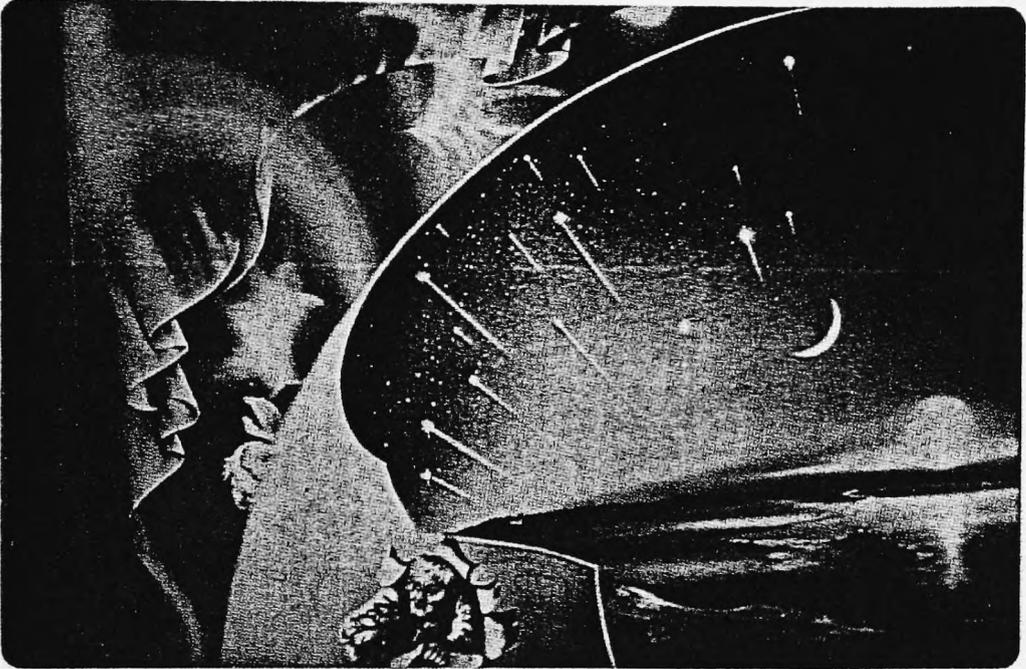


FIGURE 29.—“Man Looking Into Outer Space,” early 16th century woodcut.

And with our ability to comprehend comes a yearning to explore, to move out of Cradle Earth, even to know our kin beyond. If the estimates of scores of modern scientists are correct, at this instant there may be thousands if not millions of advanced civilizations, whose radio transmissions are passing not merely through space but even through our very bodies. If we only knew exactly where and how to look and had the will to do so, life on this planet might at last reach childhood's end.

Rarely, however, do major triumphs come easily. Requiring monumental ingenuity and resolve, this search would be like virtually no other effort known before. What we contemplate here would challenge our imagination and wisdom with unprecedented effort in diverse disciplines; moreover, it would test our fortitude, possibly spanning not only terms of Presidents but also lifetimes of scientists and even of nations.

But irrespective of the pros and cons of searching, would the inquisitive human mind be willing to forgo the quest? Within the next few decades we will have explored most of the “ghetto Earth”,  
 \_\_\_\_\_be viewed in its present state of cosmic quar-



antine. Will humankind limit its vision over astronomical time periods to this minute locale, especially with the ability to do otherwise at hand?

The sternest arbiter of all is not the President, the Congress, or even the People; it is Time. Before that unforgiving master, how will our judgments stand? As we sit in the Court of Ferdinand and Isabella, will we give Columbus his ships? Are we willing to look through Galileo's telescope? Dare we join Darwin on the Beagle? How long will we tarry at Newton's seashore, while the ocean of truth lies undiscovered?

Only in exceptional epochs do we face such decisions. Now is one of those times. Even though the issues here perhaps seem vague, implausible, and futuristic, their ultimate significance possibly could dwarf the Apollo Project, if not the entire space program.

But about this search we are fearful—of appearing naive, of venturing imprudently, of proceeding hastily, of squandering resources, of confusing priorities, even of achieving success. Yet, as Bronowski has noted generally: "We are all afraid—for our confidence, for the future, for the world. Yet every man, every civilization, has gone forward because of its engagement with what it has set itself to do."

Our root questions here come down to these:

1. Are our reasoned suppositions and circumstantial evidence misleading or wrong? Because we can know the answer to this uncertainty at best, are we yet confident enough to proceed? If not, when will we be—at what stage of discovery, with what degree of certitude?

2. How much are we willing to pay to achieve contact—in terms of money, time, and commitment—especially if for decades the search were unsuccessful?

3. And even if the search is practical now, do we actually want to know if there is life beyond Earth? Do we want to make contact?

Each person must answer individually.

1. For myself, I am impressed by the current evidence, even if not entirely convinced. That the modern findings are inconclusive and incomplete is unarguable, but so too is the unmistakable fact that they cannot be ignored. Science never rests. Contrary to popular belief, science is not an immutable body of absolutes—codified, comprehensive, and final; rather, it itself is a growing, evolving process. And that generic truth holds for the case here. Instead of only lamenting about our ignorance, let us also rejoice at our knowledge; while steadfastly recognizing our limitations, let us likewise acknowledge our achievements. And of late, they have included the piecing together of a wondrous mosaic—spanning eons of time and leaping megaparsecs in distance, ranging from stellar core to atomic nucleus, from primordial egg to human gene. The data are not all in, nor will they ever be. But from what we do know, a reasonable person could reasonably conclude that we likely are not alone.

2. Any responsible search should include, at core, cutting-edge science and technology; accrued benefits should be both potential and immediate. Like in any pursuit, the suitable level-of-effort in this one should be simultaneously frugal and adequate, commensurate with the task. But would even most modest funding for such a



search be extravagant or wasteful? In short, would the search be worth the cost, however low? If contact eventually were achieved, the \$24 purchase of Manhattan Island would pale in comparison; and even if the search were not successful, the ancillary scientific and technological benefits alone would justify it.

3. I believe mankind's desire to know if we are alone and to make contact with life elsewhere is instinctive, innate, indelible. If not now, then, soon, a methodical search will begin. If not by the United States, then by other nations, it will be made. And I believe here and now to be the proper place and time. To me, the answer is clear and irrefutable: Yes, let us proceed.

Mr. FUQUA. Thank you very much, Dr. Berendzen. If your schedule permits, we will proceed with the other witnesses and then have the questions and answers.

The next witness will be Dr. Philip Morrison, Institute Professor and Professor of Physics at the Massachusetts Institute of Technology.

We want to welcome you here and we thank you for taking your time to join us.

#### STATEMENT OF DR. PHILIP MORRISON

Dr. MORRISON. Thank you very much, Mr. Chairman, and members of the committee and ladies and gentlemen.

We've just entered the 20th year since public attention was drawn in the scientific community to the fact that we had in our hands, even at that time and much more clearly now, what is more or less a unique possibility of signaling across the tremendous distances of interstellar space.

No light, no sound, or signal of any sort that we know except particular parts of the spectrum, such as the microwave region and possibly some newly developed lasers are possible means to make our presence known across the tremendous gap of interstellar space.

Of all these means, it became clear about 20 years ago that the microwave dishes of the radio astronomers were so curiously powerful, compared to the natural means of noisemaking with which the universe is normally fitted, that they present a unique opportunity for new signaling, if intelligent beings exist so that there could be two ends of the communication.

Of the millions of species, no organism on this Earth uses radio save ourselves. Most of them do use light. Many use infrared and other forms of radiation, but the Sun is a poor emitter of radio radiation. Most other stars are also.

The only way in which radio is made in our galaxy extensively is in the large, very extensive clouds of gas through space, on a much larger scale than any star or planet or—in fantastic amounts. But it is diffuse in direction and waveband.

There, radio emission is found. It is that which the radio astronomers like to study. But this uniqueness of radio, this artificiality of our coherent radio, has ultimately no natural competition. It is the fundamental physical reason why the microwave spectrum has been found to be the most advantageous.



I would like to make a few remarks looking back over the 19 or 20 years to see what has happen since the early suggestions were made.

I believe we can look back on the 20 years with some satisfaction. It has been a long time, a good third of my lifetime. I think it could be regarded and will be regarded as the pioneer stage of this investigation.

Take Newton's discovery of gravitational forces and the theory of the motion of the planets. This dominated science for 150 years. But you can look back and show that the generation before him were pioneers, they were groping near the prize here and there. They were on the verge, but didn't quite get there. They did not—but they did open the way.

In the same way the pioneer search begun by Frank Drake in 1960 and carried on since by half a dozen teams of radio astronomers in the United States, Canada, and the U.S.S.R., are landmarks in the search, as it will eventually be chronicled.

It's fair to say for all of them that they were at the most preliminary, hopeful investigations. None were prepared to continue with improvements, expansions, systematic searching step by step for all possible targets, with capabilities commensurate with less than the most optimistic estimates.

The pioneers show the way. We owe a debt to the pioneer investigators. But that they had not achieved positive results was only to be expected.

It hardly could have been otherwise. I used to think when I heard of Drake's first experiment that what we have established was that not every star was broadcasting at us a strong 1,420 megacycle signal. Until Drake that could have been the case, he could have opened up a universe crammed with communications.

We know that is not the case. We know life is not everywhere. But yet it may be in very many places. We have looked in the haystack very little. The pioneer workers, a few radio astronomers, were financing themselves under the table, using spare time and existing equipment, working on weekends, and so on.

They have been people that walk by the great haystack and pick up a handful of straw. They looked for a needle but they didn't find one. This is not exactly a fair and full search.

I think we are still in the pioneer phase. For me, the end is quite clear. The end of this phase will be marked by some sort of an effort very much like the one proposed by NASA or the administration.

It would be a scheme that did involve some degree of specially constructed instrumentation, designed and devoted to the purpose, and the progressive use of it for some time by a number of people, systematically dividing the search up and trying to do the best job they could.

They would share the task. The scale of this effort is a scale of one important radio project, not even of the most important—it seems to me this would not represent the mounting of the final search commensurate with ultimate success, the kind of search I think eventually human beings will carry out.

Rather it is the minimum search that you can say is the beginning of a genuine effort to find out whether or not we are alone. I



am confident it will start in 1 year or later, this year or some other year.

It seems to be that its logic is compelling. The sentence we wrote 20 years ago still goes: That the probability of success is unknown but the probability of success—well, it is nothing if you don't search.

What are the changes in the 20 years? I see two. One positive and one negative, both worth discussing.

The first change is the extraordinary rise of power of the digital computer which is symbolized to everyone by the little pocket calculator carried by every student and that is found on all the counters of the discount houses.

It more and more astounds us with low price and tremendous capability and reliability. It is literally the case that in your pocket for \$20 you can carry a computing power that would have been regarded as a first-class triumph a generation ago.

Any university would have been proud to have it. I used some of the old ones. I can say this firmly. Now people can use this not only for the calculations that we see everywhere, but also, for very much throughout the entire domain of technology. In radio astronomy what it means is quite clear.

It means we can easily have dedicated computer systems capable of searching the radio channels, 1 million at a time, instead of one at a time, or, as the best technology installed and used day to day would now be, about 1,000 channels at a time.

You search, but there are a million marks on your dial. This makes the search powerful. It enables us to think of searching in a systematic way without guessing too much as to the answer, as the earlier pioneers tried to do.

A first step of this kind has been taken here and there. One experiment was done this past spring by a young Harvard physics professor. He was able to use a general-purpose computer in a specialized way to simulate a narrow band, 40,000-channel receiver.

When we have a few of those that can be used all the time over 1 million channels, there's no question that will place the search on a fully systematic and professional basis. But until we do that we still remain in a valuable and interesting pioneer phase.

The most negative feature in the past 20 years, has been the slowness of the maturity of our answer to the auxiliary questions of the search, questions that don't direct themselves to the search but rather to our estimates of the probability of its success.

Are there distant planets? Are those planets earthlike? Can they support life? These questions I have hoped we would be closer to answering than we are today. We have not found anywhere in our solar system a clear sign of the presence of life, even the first evolutionary stages of life.

We have not even found for sure those companions to stars that we call planets. The earlier searches have been carried on for decades by brilliantly applied older means, means I can characterize as the means of the old astronomers, and photographic plates carefully measured and carefully stored for a long period of time, and rather elaborate calculations.



Those techniques are not the best that we have. In the last couple of years, we see the beginning of bringing that domain into the modern world. The data handling is now much faster.

We don't have to be quite as extraordinarily patient to get results. I think in the next 5 or 10 years new systems and modern astronomical equipment will enable us to answer this.

This will bring candidates where we have some reason to believe there might be life. There are several new methods and many have been suggested.

I anticipate a powerful growth in the next decade. If we are wise in using our resources we will have a successful search for planets.

This has nothing to do with the final question. It is only a precondition, but it is valuable. It is interesting. The point never was that we should be able to estimate what the chances are before you search. The point is that only the search itself, the search alone, can make empirical as to what is a very old speculation. With marvelous images on the television screen and film I have a feeling that many people lose the sharper contrast between simulation and speculation and reality, between fact and what could be.

I myself think that distinction is very sharp indeed. I don't think we can claim at all that theory and analogy, even given a very large increase in reliable theories of formation of planets or origin of life or evolution, could add to the actual search anything more than interesting and useful background. In short, you must try.

There are three positions which I find in the present world expressed freely by writers and the scientific community that I would like to mention.

I could name each one after a scholar. The most conservative view is the one I would refer to Aristotle, and he was no stick-in-the-mud. This was 2,300 years ago. It was this: One planet is the little blue footstool in the universe, and no other place. In those hundred thousand million stars in our galaxy, thousands of millions of galaxies, no other place harbors radio telescopes, even though we have seen the suns in all those places.

The other story that's coming up makes the assumption that the counterparts of our society sustain indefinite growth in numbers. Call it Malthusian.

We would move among the stars to fill up a great expanding sphere until two spheres collide. It is exactly like the large-scale films you see on television every day in which, in spite of the technology, the human motivation and social structures look like daring pirate stories.

I don't think it is a plausible picture, because I think we are finite beings with finite capabilities. They are surely growing, but they are bound to be limited. They are not certain to become universal.

Nor are we certain to remain alone. Rather we are somewhere in between. The Copernican assumption of mediocrity seems so characteristic of the entire structure of the world that we see that it is extraordinary to hold any other view.

One thing is sure. We are not going to contradict any of these philosophers except by trying to find out in fact. That is what we have to do. It requires patience. It requires a mixed strategy.



You don't put every egg in one basket. We have to recognize we face ignorance. We must do many things. We modify our plans to search a variety of frequencies, directions, and times. This can only be done in the presence of a thriving, systematic effort.

It is not a big part of science, but it is an important part. It promises perhaps the most important change in human thought for a very long time.

An urgent problem is the growing interference from satellite signals. Some of this arises from somebody carrying on the business of communication or of radar reconnaissance. That may interfere with our search. But much is merely sloppy, avoidable noise, something built and orbited with no great attention to the state of the art, for not many people can enforce laws on the satellites. In either case the interference is grave.

In the next year an international conference in Geneva will fix the use of the radio waves. That is the World Administrative Radio Conference. I hope that some protection, of course short of an absolute monopoly for our search, will come.

An important fact there will be the demonstrated seriousness of the effort toward systematic search. The international technical organization that operates the radio conference has put the matter on the agenda.

It remains for the nations in their sovereignty to decide—on the basis of one country, one vote—what to do about the problem.

We may well see coming out of this conference a genuine recognition of its importance.

In the spring of 1959, a Cambridge biochemist turned historian published volume 3 of his great work "Science and Civilization in China." His name is Joseph Needham.

He cites from the 13th century Chinese philosopher Teng Mu who wrote, in one of the quaint essays which were the best thought of the time: How unreasonable it would be to suppose that besides the heavens and Earth which we can see, that there are no other heavens and no other earths? We in our time have still to follow this long-marked path.

Thank you.

Mr. FUQUA. Your complete statement will be part of the record.

[The biographical sketch and prepared statement of Dr. Morrison follow:]

#### DR. PHILIP MORRISON

Born: New Jersey, 1915; Ph. D. University of California, Berkeley, 1940. Theoretical physics and astrophysics; physicist on Manhattan Project. 1942-46: a Group Leader Trinity test of first atomic bomb, and Tinian airbase for combat missions. Faculty, Cornell and now MIT (1963- ), with long-standing interest in science education at primary and secondary levels as well. Regular book reviewer for *Scientific American* ((1964-present). Founding member and past chairman, Federation of American Scientists. Chairman NASA Scientific Working Group on SETI, 1975-77.

#### TWENTY YEARS AFTER

(By P. Morrison)

A brief personal statement on the search for extraterrestrial communications as it looks now, by the co-author of the first scientific paper on the search effort, published in 1959.



Twenty years may be long enough to justify a few personal reminiscences. They begin during a chamber music performance in the Cornell student center when I first came to think about the promise of gamma-ray astronomy. The idea seemed good, and I am afraid I paid less than due attention to the quartet. By the end of 1958, I had published the first summary of what one might learn from gamma-ray astronomy. (Like most theorists, I badly under-estimated experimental difficulties, and it was to take almost fifteen years before ingenious experiments could get much real result in this field.) But the paper was interesting, and made the challenge an inviting one. A few months after its publication, one spring day in 1959 my ingenious friend Giuseppe Cocconi came into my office; which then looked northward out over a small lake to the green hills of Ithaca. Giuseppe has an unlikely question. Would not gamma rays, he asked, be the very medium of choice for communication between the stars? They would work, that was plain, and my answer was enthusiastic yet cautious. Shouldn't we look at all the spectrum for its possibilities, and genuinely seek out the best means for such a link? In those days Jodrell Bank was in the news for its big dish, and the fiasco of Sugar Grove was in the planning stage; I do not recall whether we knew even the mere rumor of the big Arecibo dish, then perhaps a little more than a gleam in the eye of the Cornell radio physicists. Certainly neither of us knew the rudiments of radio astronomy. It took a few weeks of reading and discussion to come to understand that: "The wide radio band from, say, 1Mc to 10<sup>4</sup>Mc, remains as the rational choice." We wrote and rewrote together the five-page letter *Searching for Interstellar Communications* which from the first we had hoped to publish in the rather speculative pages of *Nature*. Giuseppe wrote to Sir Bernard Lovell directly, urging him to devote some time for this task at Jodrell Bank, but that radio astronomer was intensely skeptical (Sir Bernard later wrote that he regretted his indifference to our proposal.) We knew of course that our proposal was both unorthodox and improbable, but we held, and we still hold, that its argument compels serious attention. "The probability of success is difficult to estimate; but if we never search, the chance of success is zero." I sent the letter to London via Professor Patrick Blackett, the influential and imaginative physicist then in Imperial College, whom I knew personally, to seek his good offices in having it accepted by *Nature*. He acted promptly and successfully, and the note soon appeared. By the time of its publication, I had left the United States for a year's stay abroad, around-the-world sabbatic, and I recall the far-away hints of public interest, as various journalists tried to reach me in Europe or Asia during this or that brief stay after the article had come out in late September.

How does the question look after these twenty years. First of all, we have passed through what I would call the pioneer stage. Since *Ozma* at Green Bank, Frank Drake's path-breaking 1960 beginning of the search, a half-a-dozen teams of radio astronomers in the USA, in Canada, and in the USSR have devoted serious effort to the search. A few of these have even used specially-built equipment for some time, but on the whole one could characterize their searches as preliminary. None of them prepared to continue, with improvements and expansion of effort, for a protracted search, gaining capabilities commensurate with less optimistic estimates of the task. These pioneers have shown the way, and our debt to their ideas and devotion is great, but the absence of positive results so far is not at all unexpected. These are people who have walked past the haystack, picked up a few handfuls of straw, and cleverly looked for a needle. That pioneer phase is not quite over; for me, its end would be marked by an effort rather like that proposed by NASA in the spring of 1978 to Congress for funding: a scheme aiming at specially constructed equipment, and its progressive use in a systematic way for some years, on the scale of one important radio astronomy project. The task would be shared among several big dishes, using each one only for a portion of the time, rather than devoting one dish to the work exclusively. It is humanly unattractive to demand that individual people spend decades in a search which offers no intermediate success, even though the eventual outcome is of great social importance. Much better, it seems, to divide the task, so that no one finds a whole career spent in an important but so far fruitless search.

The ubiquitous rise of computing power has been the most positive change in twenty years. People speak easily now of megachannel spectral analyzers and more; it becomes clear that we can carry out a search even if all the guessed strategies about special channels or directions or times prove wrong. What seems needed is to pass from the pioneer phase to a systematic stage, and to learn how and what to do by serious trials. The most negative change in twenty years has been the slowness of the scientific maturity of the auxiliary questions. We have as yet no clear result and have found no life anywhere in the solar system pointing to the existence of ~~any other planetary~~ systems. The early claims of dark planetary companions to



some stars now seem less than certain, and they were in any case not close analogies to our system, since the stars we have measured astrometrically so far are nearby red dwarfs, and the companions we are sensitive to rather massive. Here is where I hope for the earliest real success: several new methods enable a much wider search for "solar systems" within the next decade.

For me, of course, the point has never been that we need to know much more about the chances before we search. The point is that search alone can make empirical what is after all a very old speculation. Whether we are alone or not needs to rest on experimental search, not on a string of evolutionary inferences. For I do not believe that our science is yet close to reliable theories of planet formation, of the origin of life, or especially the evolution and duration of communicative societies.

A curious view has grown up, which I may call Malthusian. It makes something like the assumption of Malthus, that human society or its counterpart will sustain indefinite growth in numbers and capability, so that we will be pressed to dwell and travel among the stars, filling the Galaxy in a geological epoch or less. Then a simple look at the Galaxy, which shows no signs of intense colonization, strongly suggests that we here must be the first. "Where is everybody?" is a question already more than thirty years old. Even this argument is more plausible than the opposite one, which I might father upon Aristotle, that our planet is the single little blue footstool in all the universe and no other place can harbor mind—and radio telescopes. But it seems to me neither position is as persuasive as the Copernican view taken in our 1959 letter, which suggests that we are finite beings, with finite capabilities, growing but limited, who are by no means sure either to become universal or to remain alone. Perhaps we can find out, and that is what we ought to try to do.

Finally, the task requires patience and a mixed strategy, that is, a plan which does not place every egg in one basket. We face ignorance, and we seek to lessen it. That requires a modest stance, willing to try in many directions, and to learn by doing. Even the microwave choice is not gainsaid, though it seems still to me much the most likely. Indeed, I find myself still of the belief that the 21-cm line remains the best guide, of course, not right on top of it, but in its neighborhood, implying a small band search tempered by other knowledge. For example, if ever there are to be found the heroic extragalactic signals—hard to accept because of their demands on power and patience—they are most likely emitted at the 21 cm line *as viewed in the cosmic rest frame*, the frame defined by the microwave thermal background. That is universally known to every observer, and even now defined to a part in a thousand. (Of course one should look at shifting frequencies as different directions are scanned, for the sun is moving with respect to that great stationary frame.) But the frequency choice is almost without ambiguity. The many rf lines which have been found since the 21-centimeter discovery seem by their very number to lack equal promise, though they ought not be disregarded. In the same way, narrow-band acquisition signals still seem the best choice, but other forms of modulation, far less familiar than steady cw carriers or even simple fast pulses, deserve consideration. All of this will follow as the search effort becomes stronger and more systematic.

The problem of interference from satellite signals, whether deliberate signals or merely sloppy noise, is grave; one very much hopes for recognition of the SETI task and for some measures of frequency protection to come from the World Administrative Radio Conference this coming fall in Geneva. Every interested person can play some part by urging through his or her government, protection for the rest of the century, the time span to be addressed at Geneva.

Just today as I wrote this piece, I realized that new 1959 astronomy volume of Joseph Needham's magnificent work, *Science and Civilization in China* had caught my imagination during the same months that we were working out the *Nature* letter. I do not at all recall noticing it, but it would be agreeable to think that I had read his translation from old Teng Mu (on pg. 221) in volume III of Needham), ending with the now familiar words:

"How unreasonable it would be to suppose that, besides the heavens and earth which we can see, there are no other heavens and no other earths?" (*From the Lute of Po Ya*, 13th century AD.)

Mr. FUQUA. Our next witness is Dr. Noel Hinners, Associate Administrator for Space Sciences, National Aeronautics and Space Administration. We are happy to have you. You may proceed with ~~your statement.~~



## STATEMENT OF DR. NOEL HINNERS

Dr. HINNERS. Thank you, Mr. Chairman.

It is with some chagrin that I follow the two previous witnesses who have been so eloquent in telling why one might be interested in SETI.

In considering the NASA portion of the President's fiscal year 1979 budget, this subcommittee approved the request for initiation of a modest radio search for extraterrestrial radio signals. I was encouraged by that action, because I regard the proposed "Search for Extraterrestrial Intelligence" (SETI) first as an imaginative, relatively low-cost gamble that could provide immense rewards in the event of a success, and second, as an effort which would provide an excellent return of science data and technology development, regardless of the SETI aspect.

Mr. Chairman, for these hearings, you have assembled some of the world's foremost experts to testify on the scientific, philosophical, and technical aspects of SETI. I cannot, and have no intention of trying to compete with them on their own grounds. What I will do, instead, is to paint for you the picture that I have drawn from their various arguments and tell you why I believe that it was wise and timely for NASA to request funding in a competitive environment, for a modest SETI effort.

The history of civilization is one of expanding horizons. At the same time that European man explored the other continents, and found other civilizations, he also, by means of the telescope, discovered other worlds, and asked whether those other worlds were peopled. We still do not know the answer to this question.

What we do know, through our program of planetary exploration, is that the nearby planets do not contain civilizations, and in the case of Mars, most likely not even life. But what of the distant worlds? What of the hundred billion stars of our own galaxy? Do they have planets? Life? Civilizations? Technology? And what of the billions of other galaxies in our universe? Among these billions piled upon billions, are we alone? Are we "it"?

These billions upon billions of other worlds that we believe may exist are so vastly more distant than are the planets of our own Sun, that even the most basic information about them is extraordinarily difficult to obtain.

For example, although many astrophysicists believe that planets are likely to be extremely plentiful around other stars, we do not even know for sure if even one other star is attended by planets!

In fact, even the very powerful space telescope will have a chance of detecting the presence of Jupiter-size planets only for the nearest 40 to 50 stars. But the occurrence of planets outside our solar system is only the first unknown. If there are planets, do they harbor life? We don't know. And how do you know that life, if present, develops intelligence? You don't. Does intelligence necessarily lead to technology, and to radio broadcasts? We don't know! How, then, can we judge whether or not a SETI search will be successful? The straight forward answer is that we cannot.

I believe that this is a very important conclusion, and is one that is often misrepresented. Even enthusiasts for the SETI program will say to me something like this: "The chance of success is very small, but the rewards of success would be very great." It seems



clear to me, however, that the chance of success is not small, it is unknown.

It cannot be calculated in any really meaningful way. It is perfectly possible that the SETI program that we have proposed or some future program will find large numbers of broadcasting civilizations. We have yet to look.

While there have been a few pioneering "stab-in-the-dark" type investigations, a comprehensive wide-frequency systematic search of the kind you have authorized has never been made.

You might say that we have "dipped our hand in the ocean," and not caught a fish; and now we have suggested that instead we tow a small net behind our little boat for a while.

We don't know if this ocean is sterile or is teeming; we haven't given it a meaningful try!

You can see that the "negative" arguments don't hold much weight with me. Just as I don't believe anyone who says that he can prove that civilizations are highly likely out there, I don't believe those who say that there is practically no chance that there are civilizations out there.

I am impressed by the fact that we exist—nature can do it. Four and a half billion years of evolution can lead to civilizations. The chain is immensely complex. It is possible to argue that the chain is so complex that it has happened only once in our universe; but the argument cannot be conclusive just because we have only one star (our own Sun) where we have experimentally tested the argument.

If we accept that a modest activity in this exploration of the totally unknown is justified, and I am convinced that it is, then we must ask that activity, and what level of activity, makes sense and can be afforded.

In trying to decide our search strategy, we are forced to try to guess the strategy (or equally important, the lack of strategy) of civilizations out there are broadcasting. The mere fact that they are assumed to be broadcasting means that they are "like us" in many respects.

You may ask how likely that is, but that would return us to the original argument, and we have agreed, I hope, that we do not have any meaningful ability to calculate probabilities of this kind. All we can do is ask the question, "What will our civilization be like in 100 years, in 500 years, in 5,000 or 5 million years?", and try to search for such civilization out there.

How would civilization communicate if they were so inclined? Very advanced civilizations might well have means that we know absolutely nothing about; for example, by neutrinos, so speculation of this type serves no immediate purpose. However, ordinary radio/television is so technically simple and so efficient that it is hard to imagine any way to beat it. The transmitters necessary to be detected require little or no advance from our relatively young technology. A major space solar power station, pointed out to the universe instead of directing its power to the mother planet, would be a reasonable SETI transmitter. I will not speculate on whether we will build such power stations soon—that is a topic much more controversial than extraterrestrial intelligence.



I am not now proposing, and I don't think anyone else is proposing, that we do any major, deliberate beamed broadcasting ourselves (I am for the while ignoring the "leakage" broadcast radiation arising from the abundant radio and TV transmission.) We are much too young and new a civilization to be able to afford the enormous cost of the necessary transmitters and energy. I do want to note, however, that the United States has already sent coded signals into "space" for Arecibo and that both Pioneer and Voyager spacecraft contain messages for "whomever" might come upon them after their escape from the solar system.

The fact that we are not conducting major space broadcasts does not, of course, prove that civilizations do not deliberately broadcast. The point is, that we are a terribly young civilization, and the universe is terribly old. We have had a village-building civilization on this planet for only a few thousand years, and significant technology for only 100 years or so. The Earth and Sun are 4.6 billion years old. This means that there may be billions of Earth-like planets in our galaxy with civilizations that reached our state of development billions of years ago. What will Earth be like in a billion years? What will be our GNP? What will it cost such a civilization to build the enormous transmitters needed?

Speculation of this kind can become a bit ludicrous, but one great attraction of SETI is that it allows us to probe such intriguing questions in a meaningful way. By looking for other civilizations, we are in effect, trying to look into our future.

If we accept that radio and television broadcasts are what we should be looking for, we still have to decide on a detailed strategy. NASA has looked carefully at the options, and we have come up with a two-tiered strategy that offers a lot of advantages.

Our first priority has been an all-sky broad-frequency survey, which we proposed to carry out at the Jet Propulsion Laboratory. As you know, JPL operates the NASA Deep Space Network (DSN), which is the facility that is, in fact, currently being used by us to actually detect extraterrestrial signals—those from our own spacecraft far out in the reaches of interplanetary space. It is no coincidence, therefore, that we propose to conduct a SETI activity at JPL.

The strategy involved in this particular SETI effort, which is the one that you have authorized, can be described as "cast a wide net,"—cover as much of the sky as possible, and cover as much as possible of the radio frequency spectrum. The price that is paid for this breadth is sensitivity. The broadcasting civilization would have to have truly enormous transmitters. But such transmitters are physically entirely possible.

A second tier has been proposed by scientists at the NASA Ames Research Center. This second part has not yet appeared in a Presidential budget, and of course, you will understand that I cannot discuss the administration's intentions for fiscal year 1980. The Ames proposal involves a so-called targeted search, in which much higher sensitivity is achieved at the cost of drastically narrowing the area of sky, and the number of frequencies searched. This in effect means that we must guess where they are, and guess what frequency they'll broadcast at but this does mean that they don't have to put so much resources into a transmitter. In practice, there



is a feeling that civilizations might be most common at stars that are very similar to the Sun, so Ames proposes to concentrate on observing such stars, in a particular frequency region, called "the waterhole," which some scientists argue is most likely to be the frequency band of broadcast. Putting these two assumptions together means that you concentrate your efforts and thereby achieve much higher sensitivity.

In putting together a program for the administration to consider, and subsequently for the Congress to consider, I must of course, be extremely mindful of cost, and not just near-term cost, but also runout cost. The JPL effort has a beginning and an end, whereby we will have detected civilization or we will not. If we have not, we will be able to say that extremely developed advanced civilizations are now absent from our part of the galaxy. Of course, it would be possible subsequently to mount a repeat all sky broad-frequency survey at higher sensitivity, which would, if negative, tell us that even less-developed civilizations are absent. The Ames proposal is a somewhat different story, simply because the philosophy is one that tries to maximize sensitivity. Similar to JPL, Ames has a well defined short-term proposal for technology development and a targeted search using existing large antennas.

We considered it better, in terms of a fiscal year 1979 initiation, to make a broad radio survey first, even given the much lower sensitivity than the targeted search, on account of increasing pollution of the airwaves. In addition, NASA needs to know the interference environment for planning communications with our spacecraft. This area of concern was recently heightened by our potential difficulties with Earth-based-radar interference with TDRSS. The JPL survey will be of great value in this regard in that it will sample the sky radio environment over most foreseeably usable communication frequencies. This is not to say that a similar argument might not be germane to the Ames project. In fact, the communications pollution is increasing perhaps most rapidly in the region of the waterhole itself, although recently developed techniques indicate that we have a way around that problem.

The Ames and JPL programs are similar in that they both are dependent on the remarkable advances that are taking place in electronics—the microprocessor revolution. This makes possible, at reasonable expense, the million-channel analyzers that are at the heart of both programs. A prototype 64,000-channel analyzer is being built currently as part of JPL's ongoing activity in investigation of the electromagnetic interference environment of the Earth.

Construction of analyzers of this new type is bringing a technological revolution to radio science.

Finally, let me make a general remark or two on the broader question of understanding the origin of life in the universe, and NASA's role in exploring for it.

The radio SETI search is only one approach, albeit the most dramatic in its implications. Many approved NASA science programs bear on this most fundamental question.

For example, the Viking landers provided basic data bearing on planetary physical and chemical conditions which must be reconciled with the origin of life on Earth. Our sounding rocket pro-



gram, and the Copernicus satellite, discovered interstellar molecular hydrogen, a basic building block of life.

Radio astronomy has revealed many complex interstellar organic, that is, carbon-based, molecules, including molecules that are expected to combine to form amino acids. Natural, not-produced-by-life, amino acids have been found in meteorites and laboratory research has shown how clay minerals may play a role in concentrating critical amino acids to the point where they may play a vital role in prebiotic chemical evolution.

As I mentioned before, space telescope may produce evidence for the existence of planets around nearby stars. Future projects that might include a more sophisticated ground-based search for extrasolar planets, using advanced versions of conventional astrometric and spectroscopic techniques.

And our infrared astronomy program is providing us with increasing detail on the process of star formation. It is in the regions of star formation that the molecules are most concentrated. If planets form cold, those interstellar molecules may actually contribute to the origin of planetary life.

Some of our other astronomy satellites will provide data on interstellar silicate grains and elucidate their role in catalyzing organic molecule formation.

The radio search for extraterrestrial intelligence is a bold attempt to leapfrog directly to an answer to one of the most important questions confronting us—are we all alone in this incomprehensibly vast universe?

In view of the fact that Congress chose not to appropriate funds in fiscal year 1979 to start SETI, this may be a time to think more carefully about the role of SETI as but one aspect of the larger question of the origin of life.

In so doing, I believe that we must now work to describe better our integral program of fundamental science that is providing us with a greater understanding of the origin of life and the potential of the rest of the universe for bearing life. Within that context, the radio SETI search can be seen in perspective and as one relatively low cost, albeit very important, element.

Thank you for your attention. I would be pleased to respond to questions.

Mr. FUQUA. Thank you very much. If the other two witnesses could rejoin the table we will try to proceed with the questions.

Now, we will start the discussion.

Well, this program was offered a famous award that is given away frequently by a member of the other body and he indicated that it was a luxury that the country could ill afford.

There was not one scintilla of evidence that life existed and that there were more higher priorities available.

Now, Dr. Berendzen, could you comment on that or how do you answer a question like that?

Dr. BERENDZEN. It's a fair question. It should be asked. Basically, it's a question of the appropriate level-of-effort of this project, particularly contrasted with our gross national product and with the national budget. And it also reasonably raises the question of our current level of scientific knowledge.



At the outset, it must be stressed that any responsible SETI program should have, at core, cutting-edge science and technology, which presumably would lead to beneficial findings and results, irrespective of whether ultimate contact were made or not. Also because such a search would be fundamentally multidisciplinary and international, it might help beneficially to join fields and nations, at least among the scientists.

And SETI would press all scientific fields to the limit of what is known and even beyond. When you consider the expenditure being requested for this SETI program in contrast with many other projects and in context of its potential yield, I think it is worth it.

Mr. FUQUA. I will let Dr. Hinners plead executive privilege if he wants to.

Dr. MORRISON. I support what Dr. Berendzen said. I don't think it is indicated that all of those statements need be taken quite so seriously. I think they were meant a little bit very cleverly—as a kind of dry humor.

I might answer the same way. I do recall a cartoon that impressed me that showed a few people sitting around and a distinguished figure walked by. One said, "Yes, Prometheus. I recognize he discovered fire, but what has he done for us lately?"

It is not my intention to try to convince him or anyone else that this is a worthwhile effort per se.

For 20 years we have discussed what is involved, and what is implied. If it doesn't appeal to anyone, then there is every reason not to do this. But the objective nature of the arguments show it is supremely worthwhile. I think if you asked 100 persons in the country, at random 85 will think it's already going on and is the purpose of much astronomical work. Of course that view is quite wrong.

It is strange that it would be regarded as not worthwhile.

Mr. FUQUA. Thank you.

To what extent are some of the other nations of the world supporting extraterrestrial intelligence research?

Dr. MORRISON. I think we are probably first or second in this. The Soviets have a number of very interested scientists in positions of responsibility.

They implement some of their ideas. They have done somewhat more in the past than we have. At one point several years ago, they established a few inexpensive receiving stations across the breadth of the country in order to carry out the idea of search.

I don't personally think it was a good idea, but it was initiated by an able, young astronomer. Certainly the plans expressed there (in the U.S.S.R.) are as big, or somewhat bigger, than the plans expressed in this country.

The implementation of the plans is larger than our present level. The Canadian astronomers have made a nice contribution using one of the two big Canadian radio installations. They continue to work at a modest level.

As I said nobody anywhere has got beyond what I would call the pioneer phase in this investigation.

Mr. FUQUA. Thank you. Now, Dr. Hinners, the radio frequency spectrum is getting rather crowded and the level of signals from space are expected to be extremely low. Do you see any way of



conducting a radio search from spacecraft, as you mentioned, such as the Voyager or the Pioneer, or incorporating a search capability from spacecraft in conjunction with some of the future space science missions?

Dr. HINNERS. Eventually, yes. We are focusing our effort now trying to assure that the waterhole is protected. At various time we have considered things like the use of the far side of the Moon as a radio observatory site.

Also, we have looked at techniques for shielding the satellites from the terrestrial radio environment.

Mr. FUQUA. Thank you. Dr. Berendzen, you touched on this in the answer to my first question, and you referred in your paper to the potential significance of this research.

How important do you think this research is to the Nation?

Dr. BERENDZEN. I think the importance would come in at least three different forms.

First, if there ultimately were contact, it is impossible now to guess what the results would be. But surely at first there would be a flurry of media activity, probably unprecedented in world history. Whether or not this would continue would depend upon our ability to understand and to learn from the messages we received. Obviously, though, just the detection of such a signal would itself be profoundly important. Beyond that, however, the importance of the contact would depend in large part on whether we could decipher and understand the message. Many of us now believe, perhaps optimistically, that in due course we could understand it. Note that with SETI, communication could not be based upon a common cultural heritage as is the usual case in terrestrial communication. But even with extraterrestrial beings we would have in common at least one thing—the universe itself, the common place in which we all reside. Thus, if extraterrestrials were to speak in scientific terms and we were to interpret their message as such, we might be able to understand the transmission. In that case, the impact of contact could be colossal.

Second, however, until contact were made or if it were never achieved, the search itself could have multitudinous benefits in science and technology. For instance, the search for habitable planets outside our solar system is itself a significant issue in modern astronomy, as are the related matters of thoroughly understanding stellar evolution, planetary evolution, atmospheric evolution, and even the Earth's own chemical and biological evolution some 4.7 billion years ago. These are key scientific research problems, aside from whether contact were ever made or even if the search per se were never undertaken.

Third, irrespective of contact and irrespective of the scientific aspect of the search, SETI could have a profoundly great value in stimulating general public education. And this would be in responsible science, not in the pseudo-science in which our Nation now often drowns. If done properly, SETI should focus on some of the truly most engaging aspects of modern sciences. this could be of great benefit.

Despite the searches conducted to date, as yet we have used only a handful of frequencies to study less than one-millionth of 1 percent of the stars in our galaxy, to say nothing of the hundreds



of billions of other galaxies. Perhaps the most beneficial lesson we would learn from SETI is how much we have yet to learn. Yet, as Einstein put it, the ultimate incomprehensibility of the world is its comprehensibility.

Mr. FUQUA. Thank you.

Now, a question that I'm frequently asked as a member of the Government and some by the scientific community—that I believe invariably comes up in the scientific community is—in the event that a signal is detected, who will release the information to the general public? Will it be the President or NASA or the National Science Foundation or will it be kept in close surveillance by those in the scientific community until they have been able to digest the information or to understand it in order to make it public?

Just who will decide whether or not to send another response and what type of response?

Dr. MORRISON. Let me start at the end. I think we should realize this problem of making a response is not a matter of just coming to the telegraph key and pressing a button.

It is extremely probable, should we come to the happy day that we have a signal that we believe is real, that the instrumentation for making any response will tax the maximum capability of our science and technology, to build some big dish and to make a powerful transmitter.

There would be a long program of design, not to say of funding. It could then begin. There would not be any instantaneous answer.

In my opinion, I think most people would concur, it is extremely unlikely the messages would be on the time-scale of a newspaper. It is more like reading in a library and less like reading the daily papers, even once there is a message.

It would be elaborate, complex collection of signals slowly would be deciphered and painstakingly interpreted with conflicting meanings, over a very long period of years. It is like university study or scholarship. Then some reporters might quit filing.

The very acquisition, the first time it is found, once it is genuine, once we have the evidence it is real, that is tremendous news. There is no question that such news would be eagerly sought, and widely published.

I am not giving the design of any organization for doing it. We are not at the stage of finding the signal.

One of the reasons we should begin a systematic search is to face this problem squarely. The main thing I want to say at the moment is that what we will first see most likely will be a false alarm.

We have had many false alarms already. I gave a talk on this not long ago to international journalists, young men and women that come to Harvard University each year representing 30 to 50 newspapers around the world.

One very intelligent man, a political specialist from a Western European country, said he'd read all about it. That contact had in fact, been made; he was surprised that I did not know about it.

It was hard for me to displace his view of reality. I pointed out there is a kind of shaky margin between the real and the false. That is true in spite of our best efforts when computers and signal



processing is new. If they tell you it's a real signal only a few times, I think you might still be wrong.

The way to be sure will be to conduct the whole thing in the ordinary manner of scientific exploration: relatively publicly, free of access to any competent, well-behaved observer who wants to work with the group.

This should be done in the familiar scientific fashion. I don't think it should be regarded as a secret or a matter of state. It should be regarded as a scientific exploration.

I don't think that means you can allow arbitrary people to come and grab the data, take it home, make their own things out of it. It should be done in an orderly, systematic way, with great hospitality, with an effort to show to the press, especially to those able to judge the validity of the signal whether it is a real signal.

If we had such a candidate signal the first thing would be to summon an international committee to study the data. Are we being fooled by something we haven't understood? It's quite likely we will in fact be fooled a number of times before we understand that the signal is unmistakably coming from beings alien and yet like ourselves.

Mr. FUQUA. I concur wholeheartedly in your answer.

My response has always been similar to the lines that you have expressed. I don't think there is any way that the Government should or could keep anything of this type under wrap. I see no reason that it should.

Mr. Lloyd, have you questions?

Mr. LLOYD. Thank you very much, Mr. Chairman.

I would like to say that I am indeed awed by the presence of our three eminent witnesses today. I would like to thank you very much for joining us.

I have listened to your testimony, really, in an almost spellbound manner. I am one of those people that has participated over the years—having read scientific fiction and so on and so forth.

Even though I am unqualified to assimilate what you have said and to interpret these concepts, I am nevertheless stimulated by your discussions, and I am delighted.

As our chairman has already indicated, I think there has to be a great emphasis—and I will get to questions in a moment so bear with me—there has to be great emphasis in this area of exploration.

I am always appalled and I guess angered is a good word when I see people at any governmental level that try to refute or try to roll back or demand evidence—concrete evidence, whatever that requires—evidence of the existence of other life.

In essence, they are saying it does not exist. It is always easy, as our colleague has indicated, for those of us in governmental positions—particularly legislative positions, to demagog the issue, because it is so easy to scoff at.

We are really unable to justify the sums of money that we put into these things to encourage people like yourself. It comes down, as you indicated earlier, to a time function and when does time run out?

Time never runs out. It is dynamic. It is ongoing. We can project not only through this galaxy but perhaps galaxies in the future.



Who knows what the actual measurement of time is? Are we a function of the measurement of time and therefore are we an hour or a second or infinitely more? The answer is—I don't know.

We speculate. We just know of our own existence. We have a function, however crude it may be, as to a measurement of time. In that timespan, we sincerely hope that in the random thrashings about that we have, that maybe we will indeed make contact with somebody. Better yet, maybe they will make contact with us, whoever, they may be.

I'm just trying to express my personal delight that people of your capability are, indeed, out there looking with some hopefulness that we could amass data that could indicate that this is the right way to go, so that the search itself, and not necessarily even the acquisition of a target but that the search itself is no longer random but it begins to have a direction and a purpose.

I would say to you I'm delighted to have you here today. With that I will get down to asking each of you the same question because you can all respond as you so desire.

What would you say, in light of your own experience, would be a reasonable amount of financial effort on the part of this committee to go forward in our involvements in this specific area?

We can start right to left or left to right in answering.

Dr. BERENDZEN. As a private citizen, I find it difficult to make responsible recommendations in detail about the Federal budget, because of the enormous number and array of competing priorities and needs of our Nation. But specifically with respect to SETI, it is important to realize that single-year funding would be inadequate. What would be needed would be resolve and commitment, possibly spanning many years. And a number of other things ideally would be helpful, too—for example, possibly a dedicated radio and research facility to be allocated specifically for this study. Also, there should be adequate funding to support fundamental research on the origin of life on Earth, which, as I said earlier, would have significant scientific importance besides SETI, including in biochemistry and geophysics. Likewise, there should be adequate research funds for ongoing astronomy studies, to find for certain the extra-solar system planets we confidently believe exist. And radio technology itself would have to be improved.

Now, what would all this come to in actual dollars? The amounts for NASA's proposed SETI program would come out to about 1 cent per U.S. citizen per year.

What would the American public find to be a reasonable amount to spend on such an endeavor? If a Gallup poll were taken, undoubtedly funding as great as that for the Apollo program would be considered wildly exorbitant, especially given the Nation's social programs. But suppose the proposed level of funding were what NASA in fact is contemplating—namely, about a nickel per citizen for the entire 7-year program. In that case, I suspect the public response would be almost unanimously favorable, especially if beneficial science and technology would be ancillary byproducts.

Actually, what could occur here is not excessive funding but rather inadequate funding. What could happen is that eventually the funding would be given but that it would not be commensurate



with a serious level-of-effort, in which case the search would begin but with inadequate support.

A few million dollars per year for a period of about 5 to 10 years would be the scale of the commitment we should now consider, which would come out to be roughly a total per capita project cost of 5 cents.

Mr. LLOYD. Thank you.

Dr. MORRISON. I think I would agree with the general assessment. I would put it perhaps somewhat differently. I think we need to establish a professional community of people, with different means and different skills, working at the problem in some fairly serious way. At the moment, there is no such thing. There is the occasional person who does a few things.

A few devoted people have written about it here and there; in some radio observatory a small team of people work together. That is clearly all that is happening.

Now it is time to pass beyond that stage. We should engage a few tens of persons. (There are several hundred persons working directly in all of radio astronomy in the United States.)

It seems something on the order of a few percent of that effort is appropriate to the situation. That would mean some capital expenditures, dozens of persons of different skills communicating with each other, and a few small teams in different centers, doing what they should do.

Emphatically, there should be some feeling that this was a persistent commitment. People and places could flow in and out of the job. The job could be transferred from one place to another. I'm not very enthusiastic to take one place and make it become the overall focus of all searches.

I think that would be difficult. It is difficult to give persons the sole task of carrying out speculative exploration, no matter how important the goal.

One must be enormously dedicated to give a whole lifetime to a search in the hope that it might be successful. It is better to have people in a number of centers each giving some time to an overall effort, but nobody devoting an entire life career to it.

Each could say I spent some years in this search, but in addition I did many other things that gave immediate satisfaction.

Otherwise, you are asking for lonely, dedicated persons who spend decades to get no single result, except that one chancy great result that engages the whole world. It is very difficult to do that.

It is better to spread the task out among many, with all of them devoting some part of their time and facilities to research of a more conventional, less exploratory type.

Dr. HINNERS. I think what the two other witnesses have said is consistent with our thinking. I was influenced somewhat by people thinking like them. We talked about a 7-year program in our proposal. We recognize it needs the continuity.

Now, the fiscal year 1979 proposal would average about \$2 million per year. We have found in our research programs, that a critical mass of scientists is required. People that are interested in the problem. That would be 5 to 10 individual investigators working independently on the program.



Now, the funding for a community like that would be about \$500,000 or \$1 million per year, depending on the equipment and how vigorous the research is with that equipment.

Funding for the equipment part is of similar magnitude. You come up with what I think is a minimum, viable program of many years duration, averaging a couple of million dollars per year.

I frankly would not feel uncomfortable in going up to \$4 to \$5 million a year. That is comparable to many other scientific endeavors we undertake on a continuing, dedicated, long-range basis.

We did prioritize this past year. That was one of the reasons the SETI program was in the budget. We said, yes, this is something that NASA should be in as part of its program of exploration.

Mr. LLOYD. Thank you. We look at a very specific problem. You have alluded to it, each of you and specifically Dr. Morrison did.

That is the public attitude.

Dr. MORRISON. Yes.

Mr. LLOYD. As we all know, we watch TV shows, or we see "Star Wars." Instantaneously, we, the general public are delighted, we are enthused.

On the other hand, as has been pointed out here, it is extremely easy when we get a proposition 13, as we have in my home State of California, to say the Government has to be cut.

The easiest way to cut Government is to not start a program. That saves money because we never have to go into it. If you don't go into it, you are never challenged to put money or additional effort into a situation.

That is really where we are with a program of this type which requires hard scientific vision, which is at the very least, almost discernible by people such as myself.

Then it is injected into the general public. I'm not any different than they are. I am a product of what they are. At least I've had some additional exposure which they may not have had.

Therefore, I guess the question is, to each of you.

Indeed, if we start such a program, how can we sell this to the American public so that we won't get a cheap shot on the backside, which is a function of demagoguery or that we don't get the impatience that we have spent  $x$  number of dollars and nothing has occurred?

Therefore, plainly we have wasted the money.

How do we, as legislators, address this so that we as legislators—such as the chairman and I and the members of the committee can go back and say go forward and let's get started on this because it is terribly important that man pursue this kind of goal?

Dr. BERENDZEN. Two decades ago, C. P. Snow asserted that what is spoken at MIT is understood by the general public about as well as if the scientists were speaking nothing but Tibetan. Regrettably, the problem today is probably even worse! Scientific knowledge in the last two decades has increased exponentially whereas public understanding of science has grown linearly, at best.

Although many culprits could be identified for causing this situation, the answer to your question and the root of this general lack of scientific literacy comes down to one fundamental, profound concept—education. With better science education, the American public and through it the American Congress could better under-



stand why many scientists and several scientific organizations find SETI to be an enthralling and responsible concept, one worthy of financial support even in light of out other scientific needs and even in an era of proposition 13.

Now, how to achieve such an educational Nirvana? One of the first steps would be to overcome the media's fascination with the glamorous and tawdry and to stress instead what is potentially even more engaging: modern science fact. As Carl Segan has noted, not only can science fact be far more interesting than science fiction but also it has the additional value of being true, or at least true to the limits of our current understanding.

I believe educators and scientists have responsibilities to make better known to the public what they are finding and why they believe these endeavors are credible and important. The media, the Congress, and the U.S. Government in general should aid in that endeavor.

(As an obvious example, SETI is often confused with other alleged ways to attempt contact with extraterrestrial intelligence that many of us believe do not possess merit yet receive wide press attention.)

Dr. Morrison alluded earlier to the possibility of false starts. Indeed, there already have been some, perhaps one of the most notable occurring over a decade ago with the discovery of the first pulsars, when periodic radio blips were found. A first explanation tentatively offered in jest was the "LGM theory"—the "Little Green Men theory." But rather than rush off imprudently to inundate the public with false claims, the scientific community proceeded responsibly. And pulsars soon were found to be explicable as natural phenomena, not as extraterrestrial intelligence. Unfortunately, such sober approaches have not always been taken.

The root issue here is the same fundamental one President Johnson 15 years ago said ultimately would be the solution to all of our problems—education. If that isn't the cardinal issue here, I don't know what is.

Mr. LLOYD. Thank you.

Dr. MORRISON. I am perhaps a little more optimistic. Who could disagree with the wisdom of what Dr. Berendzen said? But I find—and I go to other campuses than MIT by and large, the matter is a generational matter.

I am not any younger than the members of the committee. I can say with some honesty that a few people are still as odd as I am and spend all their time doing strange things which don't butter any parsnips, don't make any money, don't defend us against our enemies overseas.

But I think we may have modified the way all of us think and live. A lot of young people believe this. I don't know whether it is true. But are people exclusively concerned with this year's budget and its economic problems?

I recognize there are many things that occupy legislatures. I hope we will be wise enough to start this going now. I think it will be well received. I don't think that there is indifference or antagonism out in the community.

I find the younger people in the world believe this is an absolute necessity, that sooner or later must be done. They are rather



skeptical that any government, say ours, with its checks and reexaminations could undertake a long-range program that does not promise immediate economic or military return.

That skepticism is, I think justified by events. When the halls are occupied by the generations of persons who are now just students, I think there will be a shift in attitude.

The question is: can we have the attitudinal shift in a few cases where it is reasonable, even in advance of considerable changes in the world?

I think it's sure to come one way or the other.

Dr. HINNERS. I agree that education is at the base of our challenge. I have been trying to understand who I have to educate. I think that I know who a couple of them are.

Mr. LLOYD. Don't leave us out. This is your golden opportunity.

Dr. HINNERS. I think that the chairman and the committee have tried to become educated. I think that is a wonderful thing. I wish we could get the same kind of exposure on the appropriations side.

Seriously, one has to reflect on experience. Now, the public that we talked to is enthusiastic. The subject often comes up at meetings that I've been to and I ask the public groups, should we invest a couple of million dollars a year in SETI? I take votes at the end of the meeting. The response is almost always positive and unanimous. There may be one or two dissenters however. I don't think the problem is with the public. I, of course, don't know how to reach the whole 200 million.

Mr. LLOYD. Don't you think there's a possibility you could be preaching to a choir here?

Dr. HINNERS. The audiences I talk to, and they are nonscience audiences—

Mr. LLOYD. But you're talking to me and I want you to know I'm not an antagonistic audience. I may not know what you're talking about but I'm very positive in my personal reaction about what all of you have to say.

You can take a survey with me or my colleagues on the subcommittee, I'm sure you would have no problems. But, you have solved only a fraction of your problems with this body alone.

I have never won an election on those kinds of odds. What I'm saying to you is that I think we have a long way to go. I really do want you to address, as you go along, how do you approach that, because precisely that is what is going through my mind.

Proposition 13, frankly, is not all bad. We are wasteful in government. You know that.

The point I make is that we have to translate your activities into the public domain and the public arena.

For every person that is positive, I will lay you odds I can show you 10 that are not.

Dr. MORRISON. It would be interesting to have a poll.

Mr. LLOYD. It might be worthwhile. The three of you gentlemen do not come in contact with one another normally. Well, you do within your own discipline because you're keenly aware of who you are and where you are.

However, this is probably as intimate a contact as you have had. The three of you together are infinitely stronger in projecting the one-mindedness, to the general public, than you are individually.



Perhaps 1,000 people could be utilized and they may not necessarily be collected into one area. In this case you just can't pursue the scientific approach, but perhaps we could use them for a single presentation. Also, whenever you go into a community, wherever it may be, you should utter the words you have done here today.

Now, the press in the community will be more interested. Since you have uttered the same words here, they will then discover you.

One of the requirements in our society is that the press must discover you. It is really true. It has nothing to do with being good or bad. I'm always amazed. I have been flying airplanes 37 years. I am not without my own capability in this, but all of a sudden since I became a Congressman and I'm here—and I've flown the B-1—people have discovered me.

I want you to know I'm not one iota better as an aviator and probably worse today than I was 20 years ago. But I am discovered, I am an expert. I am something that is a real—well, I get a kick out of it and I'm sure you've had the same experience.

People talk about how they flew with me when—I don't remember all of those instances. Maybe I don't want to. It has been reported I once looped the Golden Gate Bridge. If I did that, I don't care to have it remembered. It is possible, even with the aircraft we had at that time to do it.

I make that point since that, too, is why you must get into the general arena—to collectively do this, so you can project, so that my good chairman and I and the members of this committee can be far more supportive.

I guess that's what I'm really asking for. I'm asking you to address this among yourselves. To this we will help you.

I am of no value in your scientific endeavor. If you told me to put together circuitry, I would be incapable. I understand a few things but I can't design circuitry. I don't know where a capacitor and a resistor would go.

I don't know what frequencies would be best. I don't know the methodology of radio transmission or if it is the best way to go. I am back there with the guy that will light the kerosene, in the canal on the Sahara Desert. That is where I am. Or take a whole series of mirrors and collect the sun's energy and shoot that out into space. It is in the same category as the guy with kerosene. As a result I can't help you, but I can help you in the other area. That is the only offer I have.

Thank you very much, Mr. Chairman. I've enjoyed this.

Mr. FUQUA. Thank you, Mr. Lloyd.

Now, thank you very much for being here. I have one closing question. Now, Dr. Berendzen, let me ask you this. I think my colleague had touched on this, but what would be the impact of this program if it were started next year or 10 years; how would that change the program?

Dr. BERENDZEN. Whether there is a funded program or not, there undoubtedly will continue to be sporadic searches across the Nation and in a few other countries. The Soviet Union is embarking upon a preliminary study, for example. To date, a handful of radio astronomers have devoted some of their own time, usually between regular scientific research projects. It is possible—unlikely perhaps, but possible—that these uncoordinated efforts will prove



successful. On the other hand, the chances of success of a concentrated, methodical search would be substantially greater.

If the media and the educators and scientists genuinely attempted to educate the public—not to bend people's views for funding—but truly to open to the public the beauty we are finding, then I think the constituencies to whom you are responsible would indeed be more supportive of science in general and of SETI in particular. And this would be particularly true of SETI if there were solid ancillary benefits in science and technology that would be of general utility to the Nation and to other areas of modern scientific research.

Mr. FUQUA. Thank you.

Dr. BERENDZEN. Thank you.

Mr. FUQUA. Thank you, gentlemen. I thank all three of you for being with us this morning and sharing your thoughts on the subject matter.

The subcommittee will stand in recess until 2 p.m. this afternoon in this room.

#### AFTERNOON SESSION

Mr. FUQUA. The subcommittee will be in order.

This afternoon we continue the hearings on extraterrestrial intelligence research.

The first witness this afternoon will be Dr. George C. Pimentel, Deputy Director, National Science Foundation. The next witness will be Dr. A. G. W. Cameron, Chairman, Space Science Board of the National Academy of Sciences.

We are pleased to have you here this afternoon, Dr. Pimentel. You may proceed.

#### STATEMENT OF DR. GEORGE C. PIMENTEL

Dr. PIMENTEL. Thank you, Mr. Chairman.

I am pleased to take part in these hearings on the search for extraterrestrial intelligence. I should say that I do not speak today on behalf of the National Science Foundation but rather as an individual scientist who has had a sustained interest in the subject matter of these hearings.

The possibility of communicating with extraterrestrial intelligence is exciting not only to scientists but to the general public. In my view, the relatively small-scale experiments being conducted today are proper efforts. They consume only a minute portion of the time and resources available for scientific research. They have a rightful place in the process of scientific inquiry. It is through such investigations that important and unexpected scientific knowledge is sometimes obtained.

In the search for extraterrestrial intelligence, scientists ask two prime questions. What is the probability that other intelligent beings inhabit our galaxy and, if they do, how should we go about the job of trying to detect their presence? Generally, scientists propose to look for beings who might have evolved much as we have—on the surface of a hospitable planet located close to a star, like our sun, that would provide the heat and light necessary for the development and function of intelligent life.



During the past 20 years, a number of publications and discussions have considered such a search. There have been several conferences and international discussions, and an initial search, Project OZMA, has been followed by several other experiments. All of these searches to date are considered to have been unsuccessful. For the sake of your discussion today, I would like to assume the possibility of extraterrestrial intelligence and proceed directly to the second prime question: What is involved in the job of trying to detect the presence of other life in the universe?

Consider, first, the possibility that some other civilization in space might be detecting a portion of the radio transmissions from Earth. As you know, radio waves do escape from Earth's atmosphere. However, at most, these signals have traveled no more than 50 light years from this planet. We have been transmitting radio waves only about 50 years. Indeed, only in the past 30 years have we transmitted significantly at short wavelengths and with sufficient power likely to be detectable by eavesdroppers on planets surrounding nearby stars.

Let me suppose that an eavesdropper has detected a transmission from Earth, has replied, and the reply is arriving now at the Earth. Given the roundtrip travel time for radio waves the locus of the eavesdropper could be no more than 15 light years away from us. As it turns out, within a distance of 15 light years of the sun, there are about 50 stars. Most of them are known to be of types that would not provide planets with hospitable environments.

Indeed, there is good evidence that probably only two of these stars might provide the right conditions for the evolution of intelligent life. Both of these stars have been studied in Project OZMA and then in subsequent searches. So far, there has been no evidence of signals that might have originated from an intelligent source.

Where do we go from here? The next step might be the consideration of other stars farther away, but of the type that are appropriate to support the development of life on their own planets. Such stars tend to be 100 or 200 light years away in our own portion of the galaxy.

These distances cause a critical problem. If inhabitants of planets near any of these stars want to use radio waves to get in touch with other intelligent life in the galaxy, they have two choices. They may beam the message directly toward the intended receivers or, if they don't know the location of such receivers, they may broadcast in all directions. The latter mode of trying to attract attention, which might be referred to as an omnidirectional beacon, requires an extremely high power level to create a signal strong enough to be detected over distances as large as several hundred light years. The odds are that such an omnidirectional beacon would require a prohibitively large amount of energy.

The inhabitants of such planets have an alternative, of course. They could try to pinpoint their targets by focusing the beam and aiming it at the most likely regions. In this way, they could save energy but would still encounter another problem. We at the receiving end would have to be looking in the right direction, at the right frequency, and at the right time to be able to get the mes-



sage. Each of these factors conspires to decrease the probability of contact.

Let me return for a moment to the mechanics of beaming such a signal. If such a signal is operated within a very narrow band, it saves energy and makes it more obvious to a listener that the signal is artificial, not natural, in origin. Unfortunately, such a signal puts an additional burden upon us as intended receivers. We must have equipment capable of detecting the narrow band signal. For such studies, new equipment would have to be developed.

These considerations lead me to conclude that the likelihood of success in a large scale SETI project is quite small. The sender won't be able to devote much attention to our solar system, as one of a multitude of likely places.

As I have noted, in order to detect his occasional message sent our way, we have to be tuned in on his program in just the right direction and at just the right time.

Nevertheless, I am not led to the conclusion that we should forget about trying to detect extraterrestrial life. My expectation is that if such life is indeed detected during my lifetime, it will occur serendipitously in the course of other activities of our best astronomers trying to understand better the structure of the universe. They will be looking at nearby stars with our largest telescopes, trying to learn how stellar systems evolve, when multiple-star systems form, when single stars with planetary systems form. Astronomers will be working to develop better receivers for new spectral ranges. If there were some extraterrestrial life out there sending us messages 100 or 200 years ago, our chances of hearing them tomorrow or next year are probably best advanced by devoting as much of our scientific resources as we can afford to the best astronomy of the day, and giving serendipity its chance to help us out.

This concludes my formal statement, Mr. Chairman. I will be pleased to answer any questions that you or other members of the subcommittee may have.

Mr. FUQUA. Thank you, Dr. Pimentel.

As we have done previously, we will now hear from the other witness since there may be some interplay with some of the answers.

Dr. PIMENTEL. May I remain at the table?

Mr. FUQUA. Yes.

Our next witness will be Dr. A. G. W. Cameron, Chairman, Space Science Board, National Academy of Sciences. Dr. Cameron was recently a witness before the Subcommittee on International Space Activities. We are happy to have you, Dr. Cameron.

Dr. CAMERON. Thank you, Mr. Chairman.

Mr. FUQUA. You may proceed and read your statement for the record.

#### STATEMENT OF DR. A. G. W. CAMERON

Dr. CAMERON. Mr. Chairman, I am pleased to appear here today to discuss SETI. The views that I will present are my own and do not represent any position taken by the Space Science Board.

SETI is one important component of a larger and extremely fundamental question: What is the role of biology in the universe?



If a search for extraterrestrial intelligence yields positive results, then we would conclude that intelligent species in a communicative mode are fairly widespread throughout our own galaxy.

On the other hand, if we fail to find such systems, the lesson is less clear-cut. Regardless of whether we have lots of neighbors or we are alone in space, knowing which alternative is correct will be very important to the human race and its ultimate development. Should we feel elated if we find lots of neighbors and gloomy if we find that we are alone? The ultimate destiny of the human race will be quite different in these two cases, but it would be premature to judge that the one destiny would be better than another. The role of science is always to find the facts of nature, and humanity builds upon these facts for its own purposes.

I propose to address the problem of SETI in terms of this larger problem of which it is a part. What is the role of biology in the universe? NASA programs are involved in a majority of the areas of research on this question. Two very fundamental questions that NASA has undertaken to address are the origin and evolution of the solar system and the origin and the development of life in the solar system. These questions lie at the heart of any attempt to determine the actual prevalence of biology elsewhere.

NASA has had a very successful and vigorous planetary exploration program. The fundamental question which this program seeks to answer is how the origin and development of the solar system took place. In the inner solar system, our attention is currently focused on the triad of major terrestrial planets: Venus, Earth, and Mars.

Through comparative planetology we seek to understand how the Earth became very hospitable for life and its sister planets did not.

We seek to provide a large range of fact which will heavily constrain theories about the origin and development of the solar system, so that we can ultimately acquire reasonable theoretical tools which have some degree of predictive power with regard to the prevalence of earthlike planets elsewhere.

But no general theory of planetary formation can command widespread acceptance without some degree of observational verification.

Considerations of the problem of SETI have focused attention on the possibility of detecting giant planets circling nearby stars. One possible approach is the classical astrometric one, in which the positions of the stars on the sky are very accurately determined so that wiggles in their motions due to the presence of giant planets can be detected.

It has been estimated that the use of modern technology to improve astrometric telescopes will allow improvements in the positional determinations of stars on the sky of between one and two orders of magnitude in precision.

By going into space it may be possible to add yet another order of magnitude in positional accuracy. This explains why NASA has a natural interest in fostering a planet-detection program which not only grows out of the intellectually vigorous program of solar system studies, but may also be a predecessor to the development of space astrometry.



In recent years, NASA has carried out biological investigations associated with the Viking project. The discovery that the surface of Mars is a violently oxidizing environment and hence, severely hostile to known forms of life emphasizes the biological uniqueness of our own planet within the solar system.

However, the fact that we have a negative answer concerning the existence of life on Mars now does not detract from interest in Mars as a possible environment in which substantial prebiological chemical evolution may have occurred.

The attention of biologists interested in questions of the origin of life has now shifted back to the more fundamental questions. Radio astronomy studies have shown that the molecular clouds of the interstellar medium have a great variety of complex organic molecules, many of them key molecules for the early development of life on Earth.

Biochemists are clearly very interested in how such biologically important molecules can have been produced under such seemingly inhospitable conditions.

When such clouds collapse to form stars and accompanying planetary systems, much of this organic material is likely to be stored inside comets.

This possible connection of comets with the organic chemistry of the molecular interstellar clouds explains why a number of life scientists are beginning to become interested in comet rendezvous missions in which such questions can be investigated at close range. It is possible that the Earth acquired the bulk of its volatiles, including its organic compounds, through collisions with comets early in its history.

If the comets did not do the job, then carbonaceous meteorites almost certainly did, and these also represent interesting environments in which there has been a great deal of synthesis of organic compounds which are very prevalent in living materials.

It is a very big jump from the first living things upon the surface on the Earth to the complex interrelated living species which we have today.

There is a considerable range of biological thought which holds that living creatures were formed as a result of increasing organic molecular complexity in favorable environments upon the Earth.

Such living creatures depend upon suitable nutrient materials being present in their environments. However, when such nutrients ran low, the living creatures could survive only by evolution and adaptation, and it is clear that the ultimate direction of such evolution and adaptation is toward an enormously complex interdependency of any species upon all the other species in its environment.

The study of such interrelated dependencies is called ecology.

If mankind aspires to live in space in significant numbers with a minimum supply problem, then it is necessary to create something approaching a closed ecology. The trouble is, the only closed ecology that we know is that of the Earth as a whole, which hardly provides a good model for a space station or human colony in space.

Even so, we really do not understand the closed ecology of the Earth. There are global biological cycles, such as the production



and absorption of carbon dioxide, which must truly be studied on a global basis.

NASA has many of the necessary tools in the form of Earth observation satellites which can be utilized to learn much more about the global ecology and possibly about regional ecologies. Only by learning much more about biological interdependencies can we gain some insight into how biological evolution may have progressed upon strange planets elsewhere in the galaxy.

This year the administration proposed to Congress a new initiative of modest cost which carried the SETI label. This has not fared very well in the appropriations process. I think that the SETI label was prematurely applied to such a modest program as this one and that it may have led to some misunderstanding of the new technological advances that were intended.

The basic idea of SETI is to search the skies with high sensitivity for signals in very narrow bands of wavelength which may be of artificial origin. Such a program would make use of the techniques of radio astronomy, but would provide radio astronomy with a new tool, a spectrometer which would detect all narrow-banded signals within a wide wave-length region.

The funds requested by NASA would have allowed the building of this new tool and its use by the Jet Propulsion Laboratory in an all-sky survey to look for narrow-banded signals.

Such activities are clearly very important precursors to a serious SETI program, but the probability that this modest initial attempt would succeed in discovering extraterrestrial intelligence appears to be small.

Its real importance is that it would utilize an important new scientific tool (of ultimate use for SETI) to determine whether unexpected phenomena were present in nature.

Such a tool would clearly be important in radio astronomy studies. Radio emission lines of interstellar molecules are usually discovered only after specific searches have been made at predetermined wavelengths.

With a true radio astronomy spectrometer it will be possible to observe all of the lines present which have an appreciable intensity. Such results are likely to be of considerable interest to parts of the biological community as well as to radio astronomers.

The ultimate use of radio astronomy techniques for SETI will probably depend mainly on targeted searches. Much of the strategy that has been proposed for a SETI program has centered around the hypothesis that there may be beacons.

These are omnidirectional broadcast devices designed to establish communication with emerging civilizations such as ours. If you are looking for beacons, then there is a substantial chain of logic which suggests that they should broadcast in the range of frequencies known as the "waterhole" between the interstellar lines associated with the hydrogen atom and the hydroxyl ion.

Much of the suggested SETI strategy is centered around extensive listening to selected targets in the sky in this range of frequencies. I find the beacon theory interesting but far from scientifically persuasive.



If beacons do not exist, then all is not necessarily lost. Certain types of signals emitted by advanced civilizations may be as strong as or even much stronger than beacons.

Interplanetary radars are a case in point. The trouble is that such emissions are in narrow beams and therefore a long period of listening to a particular target may be required before such signals could be confirmed.

This is known as eavesdropping. One among many problems connected with an eavesdropping program is that there is no special reason why the emitted signals should be confined to the waterhole.

Therefore, the problem of the search is greatly complicated by the need to cover a much wider wavelength range in which artificial signals of terrestrial origin can provide a very bothersome background.

Because the radiospectrum is becoming more heavily utilized all the time, the ultimate solution for SETI investigations such as eavesdropping may require the establishment of sensitive antennas on the back side of the Moon, where they will be shielded from terrestrial interference. This may also become necessary for the most sensitive work in radio astronomy generally.

Mr. Chairman, I have taken the liberty of discussing a rather broader subject than that which is specifically the focus of these hearings, but the concept of SETI is intrinsically a part of this larger subject.

There is already a wide range of scientific activity going on, both in the physical and the biological sciences, directly relevant to the role of biology in the universe.

NASA is or will be intrinsically involved across this broad spectrum of fundamental studies.

Some of the new funding which NASA is likely to request during the next few years will find a very good use in what I call SERI precursor studies with applications across a wide scientific spectrum.

SETI is one component, albeit a potentially very important one, of investigations of the general biological prevalence questions. It is logical that an appropriate portion of the funding available for these diverse projects should go into attempts to listen for artificial signals from other stars.

But it is important that our knowledge relevant to the prevalence of biology in the universe should advance simultaneously across a wide spectrum of disciplines, and much of the scientific rationale for carrying out a SETI program lies in its being an intrinsic part of the endeavor.

Major portions of this spectrum of activities are intimately associated with NASA goals and missions, and it is logical that SETI activities should have a place among NASA scientific programs.

That concludes my statement.

Mr. FUQUA. Thank you very much, Dr. Cameron for your testimony. We now have some questions for both of you. If you feel so inclined to give comments that somebody has asked about earlier, feel free to do so.



Dr. Pimentel do you believe that the SETI related finding by NSF is at the right level or do you expect any increase in this effort, particularly if NASA does not pick up the ball?

Dr. PIMENTEL. The present level of participation in SETI activities at NSF is connected almost wholly, I believe, with providing access to the telescopes and observation facilities operated by NSF.

The Foundation's contribution can be translated into a dollar amount through the cost of operating the observatories. It is quite modest, less than \$100,000 per year. I believe that this is a reasonable investment and a reasonable use of a modest part of the observing time of the observatories.

Now, if NASA were not to continue funding such activities, I would expect that some of our best astronomers would do it on a basis that could be appropriately called, in this case, moonlighting. My feeling is that the activities should be sponsored at the current modest scale.

Mr. FUQUA. Dr. Cameron, the space telescope is going to be available in a very few years. What type of role do you think it can play in detecting other planetary systems and analyzing their biological characteristics?

Also, you mentioned in your testimony about the astrometric telescope. Could the space telescope be classified as in astrometric telescope?

Dr. CAMERON. There has been a suggestion made that the space telescope could be adapted for attempts to discover giant planets associated with other stars by direct observations.

Now, Dr. Oliver, who I believe will testify tomorrow, has taken the lead in that particular analysis. You might wish to ask him about it. But in principle that is a way in which the space telescope could be used.

It requires a special adaptation that might not be most generally desirable for other purposes unless it was done during a period in which the shuttle was rendezvousing with the space telescope and a very special filter was placed across the aperture and taken away afterward.

As far as the astrometric telescopes are concerned, the character of my testimony was that, using ground-based techniques we can do much better than we have done in the past which generally involved the use of telescopes a half a century or more old.

This is simply by bringing modern technology to bear upon the problem and constructing special purpose instruments for astrometry.

There have been some studies under NASA sponsorship dealing specifically with how this might be done. There have been some interesting suggestions along the lines that this is possible.

It would appear that a modest program of ground-based activities should precede any special purpose telescope put into space for that purpose.

When you go into space, the general technique involved would probably be swinging the telescope through angles and making use of the opportunity to do very accurate timing in order to translate time to angular separation.

Mr. FUQUA. Then it could be used?



Dr. CAMERON. I was talking about two things, the space telescope could be used as a special purpose experiment for direct detection. In going to space to do astrometry, we could not use it per se. It would have to be an especially designed instrument.

Mr. FUQUA. The telescope, as designed, could it be adapted without a great deal of modification?

Dr. CAMERON. That has not been studied. I suspect it would not be an ideal design.

Mr. FUQUA. What effort, internationally, is there going on in SETI type activities, particularly—we discussed this morning about the U.S.S.R. program.

Dr. PIMENTEL. I know of the interest in the U.S.S.R., but I cannot answer in a quantitative way. Perhaps Dr. Cameron can.

Dr. CAMERON. There has been a long-standing interest in the Soviet Union as to the possibility of doing SETI studies.

There have, in fact, been several activities carried out in which Soviet astronomers have devoted some time, actually to listening to select targets.

I think there were three or four programs. I cannot recall specific details. I know they have a continuing interest in maintaining activities of that type.

Dr. PIMENTEL. Possibly one of the most competent observational instruments available would be the radio telescope in Germany. It has a 100 meter steerable antenna. I wonder if it has been devoted to the SETI studies?

Dr. CAMERON. I am not aware of it.

Mr. FUQUA. What rationale is there for funding the SETI type programs through NASA rather than NSF?

Dr. PIMENTEL. A possible answer is that NASA, in the course of its planetary program, has contributed significantly to our attempts to find life outside the terrestrial atmosphere. In the course of the Viking program, NASA has significantly advanced the question of whether there might be life on another planet within our solar system. Since this community of interest has been developed within NASA, it is logical for NASA to proceed to ask if one might find life outside our planetary system.

The continued encouragement of this community of interest would be one rationale of why SETI might be more logically placed in NASA.

Mr. FUQUA. Now, Dr. Cameron, what would be the impact of a negative finding of life in our own solar system on the search for life or intelligent life in some other solar system?

Would there be a correlation?

Dr. CAMERON. I think the realization that we are biologically alone in our own solar system has had already an intense intellectual impact on the thinking and feeling here on Earth.

The pictures published of the Earth taken from spacecraft coming back from the Moon have surely, played an enormous role in the ecology movement.

The ecology movement has influenced a great deal of legislation coming out of this Congress. It has impacted on the thinking of many people. So the question as to what the impact of actually finding life elsewhere would be, is a little hard to answer.



Certainly it would be tremendous but it is likely to be so tremendous that it would reshape almost everything we think about in ways that I cannot foretell. Whether that would be good or bad is not my role to judge.

It would however, have an enormous impact and it would influence what the human race does downstream in a very profound way. As I stated in my testimony, the contrary finding might take longer to be reached.

This would be the conclusion that we are also alone in at least the nearby portion of the galaxy. That also may have an impact similar to seeing pictures of the Earth alone in space.

Probably it would have an even deeper or longer lasting impact on the future of the human race in a different way. Again, it is not my role to judge this.

Mr. FUQUA. You mentioned in your testimony that a better understanding about the Earth's ecology is needed so that we might look at how to analyze radiation from space. Has any progress been made in this and does the NASA SETI program incorporate any of these concepts?

Dr. CAMERON. I think NASA has potentially useful tools for studying ecology in a global sense—they have SEASAT and other observation programs. They have LANDSAT. Now, the trouble is I believe that the funding for research as to how to utilize the data that comes back from the spacecraft for biological and ecological purposes has been greatly missing in the past.

The attempt has been to have a quick product in the form of a picture that can be distributed for people to work with at the State and local level.

That part of it, I think, has been an astounding success in many respects. But the mechanism to get from those pictures data that will be of ecological and biological interest has not yet been found.

I think that it is very important to learn how to extract data of general global ecological interest but it will take a lot of development money in order to find out what is possible to be done.

I see administrative and institutional barriers that would be in the way of doing this. These include the fact that Earth observations are part of the applications program.

There has been a great lack of funds in the past to use the output of the applications programs associated with Earth observations which include biological science.

I believe that we don't really know in detail what is possible. I think it is very important that we should find out.

Furthermore, we should clear away some of the other administrative barriers to being able to look at the Earth globally and scientifically such as whether observations belong to this or that agency of the Government.

I think there is also great difficulty in taking data from one set of observations that resides at the Department of Interior and correlating this with others that reside at the Commerce Department of other agencies.

This is what one needs to do. There are substantial barriers of nonscientific types. These must be overcome if we are to develop a global understanding of planetary biology and ecology.



Mr. FUQUA. Now, Dr. Pimentel, what programs have the United States had in trying to find intelligent transmission from space? As to this, have all been funded by NSF?

Has any other agency funded it?

Dr. PIMENTEL. NASA has helped to fund a SETI project exclusively through the use of installations that are NSF-operated. These are the Green Bank telescope and Arecibo which has a larger listening area. NSF has contributed by making facilities available; the individual scientists have been funded through NASA.

Mr. FUQUA. What has been the level of funding? Do you recall?

Dr. PIMENTEL. The estimated cost of NSF installations providing services—

Mr. FUQUA. I mean NSF funding for these programs.

Dr. PIMENTEL. It amounts to the cost of operating facilities for the time devoted to the NASA observational program. The figure is in the neighborhood of \$50,000 to \$100,000.

Mr. FUQUA. That's all that's been spent?

Dr. PIMENTEL. That is an annual investment over the past few years. I understand that the NASA funding might be in the order of three times that.

Mr. FUQUA. What percentage or fraction does that represent of the NSF radio astronomy funding?

Dr. PIMENTEL. The total astronomy budget from NSF is in the neighborhood of \$60 million; the cost of SETI to NSF would be a small fraction of 1 percent.

Radio astronomy itself gets a fraction of that total; perhaps a third or so. The final figure would be in the neighborhood of a few tenths of a percent of the amount spent on radio astronomy.

Mr. FUQUA. Mr. Winn.

Mr. WINN. Thank you, Mr. Chairman.

Dr. Cameron, why hasn't the Space Science Board taken a position on SETI?

Dr. CAMERON. The reasons are a bit complex. They have, I think, much to do with the fact (a) NASA hasn't specifically asked us and (b) we are not really properly structured to do so.

We could restructure it to do so if it became a goal.

I think that deserves an explanation. The things that one can do in developing radio astronomy techniques which are of primary importance to do the SETI studies are of great interest. But they are not of the highest priority to radio astronomy.

Similarly, doing astrometric studies for planet detection tends not to be the highest priority goal for people that want to study astrophysics.

What it amounts to is that one could ask how could one bring together a wide variety of tools and many disciplines to bear on SETI and related studies; what would be the strategy for doing this?

We could structure an interdisciplinary committee which would look precisely at how such a strategy could be developed for that purpose. As we are presently structured with disciplinary committees, if one asked each of the disciplinary committees how they would view doing something that is SETI-related in relation to the



rest of their discipline, the chances are that it would be given a high priority.

Certainly, however, it would not be at the top of the list in that discipline. I think the message is, that doing SETI has different goals than doing the individual disciplines within the sciences that tend to contribute to SETI.

Mr. WINN. I don't really visualize SETI—how deep in space will the program communicate for instance? Would it be 4 or 100 light years?

Dr. CAMERON. I think probably one way of describing this is that if one were to use the telescope at Arecibo to send signals to another civilization, then those signals could be received by another civilization clear across on the other side of the galaxy with a telescope similar to Arecibo, if pointed at the right direction at the right moment for the signal to be received.

So, with existing technology there really isn't any problem about reaching very far out into space. The problem is more one of time in the sense that for those signal to be received by any likely receiving civilization and for them to answer requires probably at the minimum some decades and more likely some centuries. That amount of time has not yet passed.

Also, we need to know that we are going to listen to them in the right direction at the right time. I call it the needle in the haystack problem.

As far as modern technology is concerned signal strength at large distances is not the large problem. It is the needle in the haystack or where to point the telescope and when and where to expect an incoming signal. That is the whole reason for the theory that a more advanced civilization would establish beacons in space that would broadcast in all directions at the same time.

It is a way for newly arriving civilizations such as ours to discover where there is intelligent life and look for associated signals that will carry information with them.

The trouble with the beacon theory is that I am not sure that we, as a race, when we are older, would expend the effort to establish a beacon of our own. That is why I find the idea that there may be beacons interesting but I'm not persuaded that they have to be there. It requires a degree of altruism that I'm not sure we have.

Dr. PIMENTEL. Could I add a word? Dr. Cameron has addressed the beacon-to-beacon type of transmission. I asked the same question of an NSF astronomer about the other type of communication: There might be life outside our solar system trying to detect random noises from this planet.

The NSF astronomer estimated that a megawatt ultra high frequency TV station on Earth, scattering noise in all directions, might be detected by a facility like Arecibo at about 2 light years.

On the other hand, our much more powerful radar, such as that used in interplanetary studies might be detected by a listening device of that quality at some 20 light years.

Mr. WINN. Thank you. Thank you very much for that interpretation and clarification. Now that both of you have gotten into the act, let me talk about the span of the program.

It's my understanding that it is based on a 7-year period. Based on the immensity of the task, is it a realistic timeframe or can the



committee expect this to be a continuing program. I would like to have both views.

Dr. CAMERON. I think, sir, you are referring to the NASA funding request of the past year regarding the 7-year period.

Mr. WINN. Yes.

Dr. CAMERON. That was for a specific experiment to be carried out at the Jet Propulsion Laboratory to do an all-sky survey to look for a narrow-banded signal.

I really don't consider that as having a very significant likelihood of detecting extraterrestrial signals except possibly if there were very bright beacons to be detected in the appropriate wavelength range.

In an all-sky survey it is more important to make sure that there are not unexpected signals which exist in nature which would be picked up by this technique and which a more sensitive targeted search used by SETI might find confusing, if they were discovered.

I prefer to call what NASA called SETI in the last budget hearings—to call it instead a kind of SETI precursor—it would develop some of the technology that would be useful in a SETI program later.

I really don't consider it was a SETI program in its own right. Now, if one wants to undertake a SETI-type program seriously, then one must contemplate larger amounts of funding and longer periods of time because with such large distances between the stars, the communication time is very long.

There are so many targets and so much time needed for each target that the total endeavor to have really listened in a serious way would be very time consuming.

Mr. WINN. That was my point even if we go to the all-sky survey, do you think that can be accomplished in 7 years?

Dr. CAMERON. For what it is intended for, I think the 7-year period is reasonable for that specific program. I would like to emphasize I think that is more of a SETI-type precursor.

It would be to find out if there are some natural phenomena and also to look to see if there are SETI-type signals so strong and powerful that they would knock us over when we actually listen in on the right wavelength.

Mr. WINN. If no contacts have been identified, what is the next logical step? Do we stop or go in a different direction? Do we use a different type of a survey. I'm trying to look further down the road past 7 years. I won't comment at this time on the money part.

Dr. CAMERON. I think that if one is serious about doing SETI one would start targeted searches much sooner than 7 years downstream. When you enter into any kind of a new field of astronomy, and in a certain sense, SETI is such—one of the first things you would do is an all-sky survey to find if there is anything unexpected.

The 7-year survey is that kind of survey. It is not done with the greatest sensitivity that one would like. The programs that have been proposed to do the targeted searches or to look very specifically at small parts of the sky only where there are nearby stars would have very much greater sensitivity than the all-sky survey to detect something from targets.



They would require somewhat different techniques to be set up. They could well take a longer period of time to carry out. It would be a serious budgetary as well as scientific question to ask how much time, effort and money one would like to spend in increasing progressively the sensitivity of the targeted searches that one could carry out, with what we have available in modern technology.

Mr. WINN. Would you care to comment on that?

Dr. PIMENTEL. I could speak perhaps on the question of whether the 7-year period could be given any credence. Let me suggest a possible way of doing so. One can ask what we might expect of the available observing time on the relatively precious large telescopes. On a given observing night, perhaps a searcher could wrest, on the average, 10 minutes to an hour from the astronomers wanting to investigate the universe in a systematic way. Perhaps times of that order, 10 minutes to 1 hour, on a given observing night might be sufficient to investigate one likely stellar target.

With that as a premise, and assuming 200 nights a year of useful observing because of the weather, such a program would allow the investigation of perhaps 200 targets a year. Seven times 200 would be about 1,400 stellar systems, a reasonable number of likely stellar systems within 200 or 300 light years.

Consequently, it can be said that this kind of a program in a 7-year period does make some sense. I don't have the slightest idea whether this type of analysis had anything to do with the way that NASA arrived at its program.

Mr. WINN. You're talking about a time that is set aside?

Dr. PIMENTEL. Yes. That's the way it has been conducted.

Mr. WINN. Thank you both very much.

Mr. FUQUA. Thank you.

Now, Dr. Pimentel, NASA has proposed a narrow-band signal system for their SETI-type program. Does that fit in with radio astronomy?

Dr. PIMENTEL. Yes, sir. I'm sure that any improvement in narrow-band detection systems will be considered a boon in the advance of radio astronomy. I think that the witness tomorrow, Dr. Heeschen, would be interested in answering that question more authoritatively.

Mr. FUQUA. Does NSF have any intention of funding a program for such a device for radio astronomy?

Dr. PIMENTEL. We regularly put certain funds from the astronomy budget into the advancement of detection techniques. Whether the specific kind of detection technique developed for a SETI program would be exactly what astronomers think best for advancing radio astronomy, I could not answer authoritatively.

Mr. FUQUA. I want to thank you both for being here this afternoon. I apologize for the delay but due to the session we couldn't predict what would happen. I do want to thank you for being here and providing us with your testimony. The subcommittee will stand adjourned until tomorrow morning at 10 in this room.

[Whereupon, at 2:30 p.m., the subcommittee adjourned, to reconvene at 10 a.m., September 20, 1978.]



# EXTRATERRESTRIAL INTELLIGENCE RESEARCH

WEDNESDAY, SEPTEMBER 20, 1978

HOUSE OF REPRESENTATIVES,  
COMMITTEE ON SCIENCE AND TECHNOLOGY,  
SUBCOMMITTEE ON SPACE SCIENCE  
AND APPLICATIONS,  
*Washington, D.C.*

The subcommittee met, pursuant to recess, at 10 a.m., in room 2318, Rayburn House Office Building, Hon. Don Fuqua (chairman of the subcommittee) presiding.

Mr. FUQUA. The subcommittee will be in order.

The subcommittee will conclude the hearings on extraterrestrial intelligence research this morning.

Our first witness is Dr. David Heeschen, Director of the National Radio and Astronomy Observatory.

Dr. Heeschen, we are happy to welcome you this morning.

He will be followed by Dr. Bernard M. Oliver, vice president of Research and Development for Hewlett-Packard Corp.

We will be happy to hear from you at this time, Dr. Heeschen. Thank you very much for being here this morning.

## STATEMENT OF DAVID S. HEESCHEN, Ph. D., DIRECTOR, NATIONAL RADIO AND ASTRONOMY OBSERVATORY

Dr. HEESCHEN. Mr. Chairman and members of the subcommittee, the concept that intelligent life may exist beyond Earth, and that we might possibly detect its existence, is tremendously exciting. The development of these ideas, catalyzed in part by the search for radio signals from nearby extraterrestrial civilizations over the last 20 years or so, must be one of the major intellectual and philosophical events of this century. The growing acceptance of the possibility that we are not alone in the universe and that we may eventually actually communicate with and learn from an extraterrestrial civilization is in itself important. When and if we do, in fact, make contact, the consequences could be very great. Further development of these ideas seems desirable.

The first serious attempt to detect signals from extraterrestrial intelligence was the famous Project Ozma, done by Frank Drake at the National Radio Astronomy Observatory—NRAO—in 1960. Since then, a number of other searches have been conducted at NRAO, Arecibo and elsewhere. I believe these searches rank among the most important developments of recent times, and certainly of my lifetime, and I am proud that the NRAO has been involved in them.



All of these pioneering searches have had certain features in common. All were undertaken by extremely able, imaginative and innovative individuals; all used existing equipment, sometimes with ingenious modifications; all occupied rather small amounts of time of the searchers and of the telescopes.

At NRAO, such searches have never taken more than 1 percent of the total observing time in a given year, and since 1960 the time spent averages less than 0.1 percent per year. Finally, all searches to date have produced only negative results.

That the results of these searches were all negative is, of course, not unexpected; neither is it very important. The principal value of the observations, I think, comes simply from doing them. Doing them helps call attention to the general questions and stimulates further thinking and discussion on the subject. I think that may be the principal value of all observational searches for extraterrestrial intelligence, past and future, for a long time, namely, the encouragement of the intellectual search.

As already mentioned, the development of the concept of extraterrestrial civilizations and concomitant speculations about their nature, lifetimes, attitudes toward us, et cetera, seems extremely exciting and important to me, and must inevitably have great impact on our society. What further impact the actual detection of extraterrestrial intelligence might have is not at all clear.

I am impressed by the huge uncertainty and diversity of opinion that exists regarding just about all aspects of the search for extraterrestrial intelligence. Nobody really knows very much about even the most basic scientific questions—where and how many are the planets that might support life, how does life originate, how does intelligence develop, et cetera.

The more esoteric problems—how do civilizations evolve and how long will they survive, how far will technology advance beyond what we know now and in what directions, how might civilizations best communicate over interstellar distances, and will they even be interested in doing so, et cetera—are not even amenable to scientific investigation. In the face of this massive ignorance, all estimates of the probability of existence of extraterrestrial civilizations, their number, best strategies for detection, et cetera, are pure speculation—fascinating, exciting, important speculation, but still speculation.

The scientific questions relating to the existence of extraterrestrial intelligence, however, are amenable to investigation. We can learn more about planets and about the origin of life and intelligence. The most positive and fruitful steps that can be taken toward the search for extraterrestrial intelligence are to reduce the uncertainties in the scientific aspects of the problem; therefore, I suggest that the first and most important element of our U.S. search strategy should be a sound and healthy scientific program in each of the areas of interest.

In contrast, I don't think that direct searches for other civilizations, by looking for signals, should be given high scientific priority; their scientific content is just too low. However, such searches might have high priority for other than scientific reasons, as exploration or adventure, like the lunar landings, or for their intellectual stimulation and catalyzing effects.



I am convinced that it is both appropriate and desirable to devote some reasonable effort and money to such direct searches, partly to stimulate the intellectual search and partly because it is a wonderful adventure for mankind, with potential, far-reaching consequences. But I don't know how much money would be reasonable and I don't know of any rationale to help determine what is reasonable. It is completely impossible to estimate what it would cost to detect an extraterrestrial civilization or even to guess what might be accomplished toward that goal with any given investment in direct searches.

The specific programs proposed by NASA are not familiar to me in any detail, so I will make only a few general comments about them.

The idea of an all-sky search at all frequencies strikes me as being rather heavy-handed and unimaginative. It certainly highlights our lack of understanding of how to go about the search. But I don't see any harm in it either, as long as expenditures are modest and can be afforded.

A targeted search program of specific objects in a specific frequency range chosen on the basis of some logical argument is much more appealing. It at least is based on positive ideas, even if there is no assurance that the ideas are correct.

In any case, I hope these proposals can be judged on the basis of their merit as part of the intellectual adventure of the search for extraterrestrial intelligence and not on the basis of any anticipated scientific results or technological spinoff.

The potential scientific by-products of the proposed searches are not particularly impressive, although there is always the possibility of something totally new and unexpected turning up.

Similarly, the contributions to instrumentation development do not appear sufficiently interesting to warrant much consideration. Some of the equipment to be built would, of course, be useful for other purposes. For example, lower-noise radiometers at various frequencies are always needed for radio astronomy, but that which is really needed most badly will be acquired anyway.

Many astronomers I have talked with are concerned—and I share their concern, at least to some extent—that the cost of carrying out these searches may be charged to science, and that this, in turn, might still further reduce the money made available for non-SETI science. I think this would be most unfortunate, should it occur.

SETI is a great adventure which might have tremendous ultimate consequences but presently it is and should be principally an intellectual endeavor. It would be dangerous to conduct the actual searches at the expense of science.

In summary, I find the intellectual search tremendously fascinating and stimulating and I think its value can hardly be exaggerated. But extensive, direct searching seems to me to be much less necessary or desirable at this stage, largely because we know so little that I think the chance of success is almost vanishingly small.

Incidentally, it was mentioned yesterday that the probability of determining success is impossible to determine, and I agree with that, and this statement is a highly subjective statement on my part, but it is what I believe.



We need to encourage and maintain healthy science in general, for it is through this avenue that I think the greatest contribution to the search for extraterrestrial civilizations will come.

We should also foster and stimulate the intellectual investigations, in part by supporting some minimum level of direct searches. Beyond that, it is a wonderful adventure for mankind, certainly harmless and maybe eventually supremely useful. We should support as much as we can afford and as the most able people are willing to undertake.

Thank you.

Mr. FUQUA. Thank you very much, Dr. Heeschen.

We are going to have to take a short recess, as I anticipated. We have a quorum call and if your schedule permits, could you wait and we would like to ask some questions of both you and Dr. Oliver together when we conclude his statement?

Dr. HEESCHEN. Yes.

Mr. FUQUA. Thank you very much.

We will be in recess for a few minutes.

[Brief recess.]

Mr. FUQUA. The subcommittee will be in order.

Our next witness will be Dr. Bernard Oliver.

Dr. Oliver, we are happy to welcome you this morning and will be happy to hear your statement.

**STATEMENT OF BERNARD M. OLIVER, PH. D., VICE PRESIDENT,  
RESEARCH AND DEVELOPMENT, HEWLETT-PACKARD CORP.**

Dr. OLIVER. Thank you, Mr. Chairman.

I want to express my gratitude to you and to the committee in general for the opportunity to appear here. I hope you will convey that message to the members who are not present.

We believe today that the universe is about 15 billion years old. We believe that at birth it was an enormous fireball of elementary particles and hard radiation that either flashed into existence for unknown—and perhaps unknowable—reasons, or else it represents the crushed remains of an earlier universe recycling itself. Either of these concepts is a staggering thought, but that is what science tells us today. Our philosophy will be profoundly affected when we finally decide which of these alternatives is true.

This fireball rapidly expanded as a blaze of light and cooled into hydrogen and a little helium, the only two elements that were left over after the fireball.

At 10 million years of age, these gases had begun to clump into vast clouds that were ultimately to become galaxies. At this point they were cool enough so there was no more light, and as the Good Book says, "Darkness was everywhere on the face of the deep."

Then the first massive stars began to form, and again there was light. Not a uniform blaze anymore, but quadrillions of pinpoints of life, the early stars, arranged in the gigantic pinwheels that we call galaxies. The universe, in other words, was superficially much the same as the universe we contemplate today, but in actual fact it was very different because there were no heavy elements out of which any rocky planets could form. There was only hydrogen and helium, so the universe contained no planets with living things.



The next step in cosmic evolution was for heavy elements to be cooked in the cores of early stars. These stars died explosive deaths, and these explosions hurled the heavy elements back into space to enrich the hydrogen and helium that was already there. Thus later stars could form with planetary systems like ours.

The calcium in our bones, the carbon in our tissues, the iron in our blood and the oxygen we breathe were all made by stars that blazed unseen in the early universe. We are indeed little bits of stardust. So is the Earth itself. So indeed are countless quadrillions of rocky planets made of the dust from the earliest exploding stars.

Earthlike planets are now thought to accrete out of stardust alone and not out of the hydrogen and helium, because it is now believed that the young stars blow these gases away from them in regions near the star. The planets accrete out of nickel-iron meteorites, stony meteorites and carbonaceous chondrites—tarry clumps of sand that may account for petroleum. These meteorites are still falling on Earth; they are assumed to be representative of the debris out of which the planets themselves formed.

The hydrogen and helium, so common on the outer giant planets, was swept away near the young Sun, leaving only small rocky planets, devoid of atmospheres. The atmospheres of Venus, Earth, and Mars are the result of gases poured out of volcanoes—steam, carbon dioxide, methane, ammonia, and many others. A significant step in the explanation of life occurred when Harold Urey and Stanley Miller took mixtures of these gases, and exposed them to ultraviolet light. They found that amino acids, polypeptides, sugars—the chemicals necessary to form DNA—were formed in great quantities. We think that on Earth these compounds rained down into the early oceans to produce a “chicken soup” a nutrient broth containing the chemical compounds out of which life start.

Life appears to have begun in the sea and to have begun rather quickly, because there is an almost negligible gap between the time the Earth became fertile and the time when life first appeared. On a cosmic time scale, that interval was very short; it may have been only a few hundred years.

One of the earliest forms of life to develop was blue-green algae. Over the early aeons these algae kept extracting the carbon-dioxide from the atmosphere as fast as the volcanoes poured it forth. They converted it to organic matter and released oxygen into the atmosphere. When enough oxygen had accumulated, animal life appeared.

On Venus a different story occurred. There the planet was too hot for the water vapor to condense into oceans, so the life-engendering process involving these biochemical precursors did not take place. No life started there and the carbon-dioxide accumulated into a crushing atmosphere that now suffocates the planet. The hundred-fold heavier atmosphere of Venus is a blanket that lets sunlight in but won't let the planet's heat out. As a result, Venus is hot enough to melt lead. Had it not been for life—for all these blue-green algae—Earth would be like Venus. So treat them with respect. We owe our blue skies and green hills to a living fossil that is still here today and stubbornly refuses to be killed in our swimming pools.



I think we should pause at this time and ask ourselves: "How can we accept this story so calmly? Isn't science describing a miracle to us?" If you had witnessed the fireball of creation, would you ever have believed that in only 15 billion short years, through the wonderful laws of physics and chemistry, some of the matter of that fiery chaos would have evolved itself into apple blossoms? Into birds? Eyes? And even into minds? Would you believe that that complexity could come out of so chaotic a thing as the fireball?

In 15 billion years, a small part of the universe now contemplates the whole of it. Our minds regard the universe which originated us, which begat us. What greater miracle could there be than the one spread before us?

But there are more aspects to this miracle. We know of nothing unique about Earth that might have restricted life to this planet. Indeed, we believe that there may be as many as 10 billion "good" Earths circling "good" Suns in our galaxy.

There are billions of galaxies, and so it seems that intelligent life is a common phenomenon in the universe. Let us ponder what that means. The fireball has evolved into life, and probably not only life here but life scattered throughout the universe. Advanced cultures besprinkle our galaxy today, but even if there are millions of them, they will be so far from one another on the average that, with rare exceptions, none can afford to cross the gulfs of space to visit their nearest neighbors, even if they knew where those neighbors were.

Decades of science fiction have beguiled the popular mind into accepting interstellar travel as a future reality for us and a present reality for other races. Yet the hard facts are that even with no technological limitations, interstellar flight is so costly in time or energy, or both, as to be economically impossible, except *in extremis*. If a race knew its Sun was going to blow up into a red giant and incinerate its world, it might make an effort to save itself. But except for such rare events, I cannot really accept interstellar travel as a reality.

At chemical velocities, the velocities of present rockets, it would take tens of thousands of years to reach the nearest star and return. If we shorten the time to the lifespan of a man, then the energy expenditure is enormous.



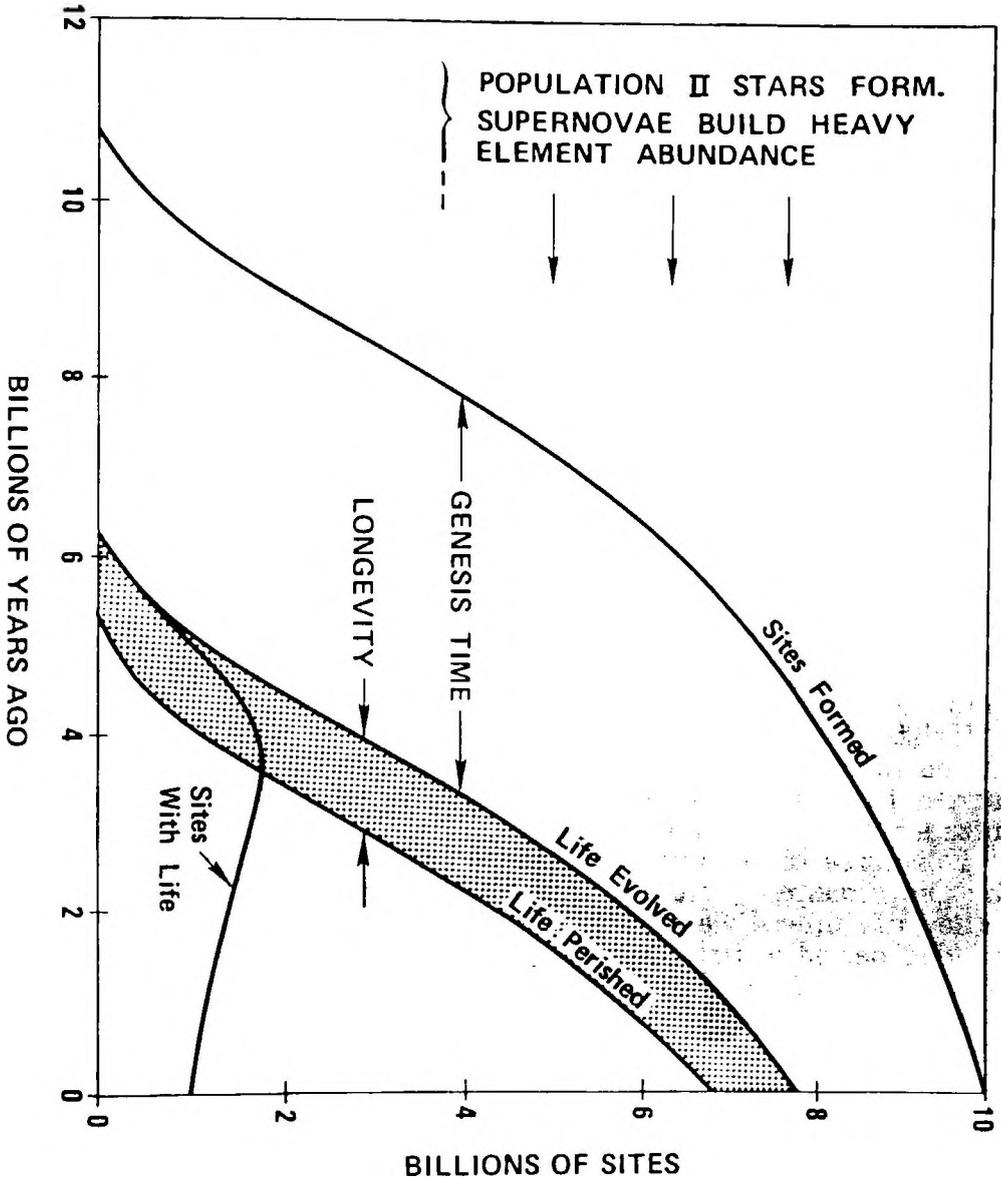


FIGURE 1

Figure 1 attempts to show the buildup of lifesites in the galaxy as we presently conceive it. The line to the left, labeled "Sites Formed," shows how the Population I stars with planetary systems around them might have accumulated billions of years ago in the galaxy.

About  $4\frac{1}{2}$  billion years later, if our evolutionary scale is typical, these lifesites, these planets, would have life evolved upon them, life certainly not identical but similar in complexity to ours. Sometime later those civilizations will have gone through their histories and will have perished. We don't know what that longevity is, but on this graph the line labeled "Life Perished" is drawn 1 billion years after the line labeled "Life Evolved". The height of the shaded band gives the number of civilizations that coexist at any one time. That number has been drawn as the curve at the bottom labeled "Sites with Life."



We see, if the shape of these curves is correct and if the assumed longevity is about right, that there were intelligent civilizations in this galaxy as long as 5 billion or 6 billion years ago. In fact, there may have been more of them 4 billion years ago now, because the rate of star formation was higher 8 billion years ago than it was at the time our Sun was formed.

Mr. FUQUA. What level of intelligence are you talking about?

Dr. OLIVER. A level that permits these civilizations to communicate with each other. They have advanced to a technological state where they have radiotelescopes.

The other interesting thing is that the number that exist now is approximately equal to the average longevity in years. If their average longevity is 1 billion years, then there would be 1 billion of them in our galaxy at the present time.

The question, you see, is whether all of these cultures are going to exist forever in isolation. It is very hard to imagine that that will happen, that islands of life will have sprung up throughout the universe and will never come into contact. The question is only when and how.

We already have the technology to make this contact, we have the technology with which to detect them and ultimately to communicate with them.

Existing radio telescopes, if pointed in the right direction and tuned to the right frequency, could detect their counterparts clear across the galaxy—could, but I assert, never will, unless we organize a rational search strategy.

I disagree completely with the serendipity philosophy expressed by Dr. Pimentel yesterday. I don't think that we will ever detect other life unless we make a serious search and do so for an extended period of time. Such a program is what we are discussing. This is "SETI", the Search for Extraterrestrial Intelligence.

But now I would like to emphasize—and I am in agreement with several of the other speakers on this—that SETI is only one aspect of a broader program to which I have given the name "Biocosmology." This encompasses all research aimed at explaining the origin of life, of predicting the frequency and location of highly evolved life, and of searching for that life by the most promising method or methods.

Biocosmology could include something as apparently unrelated as searching for petroleum or methane in deep preorganic formations, for, if Thomas Gold is right and the Earth is full of primordial hydrocarbons, it would mean that petroleum is not the result of some special geologic era on Earth and that other habitable planets would have oil for their beginning technologies. It would, incidentally, mean that we have much more than what was previously thought.

From time to time, as new discoveries are made, new topics may appear as desirable additions to a biocosmology program while others may drop out. For the present I would recommend that the program comprise at least the following subjects:

Extrasolar Planetary Detection. This has already been mentioned, but let me say a few more words about it. Theory today suggests that planetary systems should accompany most stars. We think they do, but we do not know. If we could determine the



frequency of planetary systems and, in particular, of Earth-like planets, we would be more confident of our estimates of the number of advanced cultures that now exist. If we discovered that only certain classes of stars had suitable planetary systems it would reduce the number of target stars to be searched.

Attempts have been made over many decades to infer the existence of planets around nearby stars from the apparent wobble in their position, as the planet swings its heavier partner around their common center of gravity. It now appears that these observations are too full of instrumental errors to be significant.

Ames Research Center has conducted two workshops and one summer study on planetary detection and has concluded that more powerful approaches can be brought to bear. Two in particular require special instruments in space and would be excellent Shuttle missions.

One scheme proposes to spin an infrared interferometer around an axis which is the line of sight to a star, causing any planet, in effect, to appear to orbit at high speed. As the planet passes in and out of the fringes of the interferometer, a modulation in the output signal will occur. Because the brightness of the planet compared with the star is much higher in the infrared, the modulation should be detectable.

The other scheme involves a telescope so constructed that the light of the star image does not diffract and scatter into a disk large enough to drown the planet.

The reason we can't see planets around other stars is that the star-light swamps out the planet. If you were 30 light years from our solar system and looked back at our Sun and at Jupiter, Jupiter would be 250 million times dimmer than the Sun and only a half second of arc away. No present telescope could separate those two objects. But it now appears that a special kind of telescope would be able to.

If the light transmission of the telescope objective is tapered smoothly to zero at the rim—rather than remaining clear to the rim and suddenly dropping to zero there—the diffracted light is greatly reduced. In principle this would allow Jupiter to be seen, in spite of the glare of the Sun, by an observer 30 light years away, using only a two meter telescope.

I think this procedure should be investigated as an adjunct to the proposed space telescope. The planet would be visible, of course, as a very faint object, and it would be necessary to take three exposures to determine that it was a planet and not a background star. These exposures separated by about one-fourth of the planet's period would show that the faint object was moving on a curve. This test would therefore require something like 2 years; whereas, using classical astrometry, we would have to wait for 2 or 3 complete orbits, as a time on the order of 30 or 40 years.

Another advantage of direct imaging is that one might just possibly be able to do colorimetry on the planet and star. Only planets with oceans would be expected to be bluer than their star, only planets with chlorophyll or its equivalent would be expected to be greener. Thus it could happen that the first evidence of extraterrestrial life would be detected in an extrasolar planetary search program. Not intelligent life, perhaps, but self-replicating organ-



isms abundant enough to paint the planet, as blue-green algae once did here, as grass and trees do now.

I recommend strong support of this program for extra solar planetary detection.

We probably will never be able to observe planets around any but the nearest 100 or 200 stars. We may be able to detect only giant planets. Good statistics on the frequency of "good" earths may have to come from theory rather than observation. So I also think theoretical studies, such as those that Cameron and others are doing, deserve continuing support.

A second aspect of biocosmology would be to continue to study the origin of life. The chemical precursors of life—the amino acids, polypeptides, et cetera—can be formed in a variety of ways. Some of them are even detected in interstellar space. But the route or routes by which these precursors became DNA, and by which DNA developed into the living cell, have not been discovered. We think the process occurred very rapidly on Earth, perhaps in no more than a century, but our beliefs would be more convincing if we could demonstrate a likely route or even let life form again by the same route. Then we could say with confidence, "See, it will happen other places."

So the transition from chemistry to biochemistry, which we might call "proto-biochemistry" should be given further support.

A third aspect of biocosmology should be studies of the evolution of intelligence. We think intelligence has survival value and would be favored in natural selection. The sentient organism withdraws from danger; the insensate organism does not; the intelligent animal uses better strategies to seek food, avoid danger and protect its young than the stupid animal. But are those factors sufficient to produce with high probability social beings or are we rare exceptions? If they had not been mysteriously extinguished, would the dinosaurs have evolved into intelligent social species, or do only mammals do this?

A number of unanswered questions make our estimates of the frequency of intelligent life uncertain. If sensible programs attacking those questions can be defined, I think they deserve support.

Last, we have SETI itself. It might be argued that SETI should be delayed until we find answers to all of the uncertainties in our estimates of the amount of extraterrestrial intelligent life. Why proceed until we are sure? Won't our technology continue to improve and make the job easier? Won't some totally new form of communication be discovered that would obsolete present ideas? The answer is no. I think we should begin now, for several reasons:

No matter how much study we give the problem, certain questions will remain and the search will always be a gamble. We will probably never know the average longevity of advanced civilizations and the number of civilizations around us depends directly on this factor. We can say that those who do search and are successful probably increase their longevity enormously because they find themselves in contact with other successful civilizations, ones that have solved their planetary management problems.

Our technology will improve but not significantly. Our receivers are almost as sensitive as theory allows. Our data processing technology will improve a great deal but it is already adequate and



only a small part of the cost of a large system. But, most important, any improved technology will be useless if we delay too long. The microwave spectrum, where we must search, is filling up with interference. If we do not get busy and stake out certain appropriate bands for SETI, and search these and others while we can, we may forever close our only window on other life, dooming the human race to galactic isolation.

It is very unlikely that any new and superior form of communication will be discovered, but one cannot guarantee this; however, it is irrational not to use existing means on the grounds that some day superior means may be found. This way one can forever rationalize doing nothing. Columbus did not wait for jet aircraft to be developed.

Since the central theme of this meeting is SETI in particular and not so much the other aspects of biocosmology, let me describe at this point what I believe to be a rational SETI program: It is the culmination of two decades of thought on the subject by interested scientists and engineers.

First of all, to signal across the gulfs of space, some physical particle or form of energy must be sent and received. Of all the various candidates for this task, the photon is the winner, the photon or electromagnetic waves.

Photons have enormously less energy than any other particle travelling at nearly the speed of light except neutrinos. But neutrinos are expensive to generate and it takes a light year thickness of lead to capture them.

Photons are easy to generate and easy to capture. They are the least energetic and therefore the cheapest bullets we can use. Out of all the wide spectrum from X-rays down to radio waves, the best portion is the microwave region.

This is because there is the less interfering noise in this region. At lower frequencies the galaxy gets very noisy and more power is needed to override this noise. At high frequencies the effective noise increases because the signal itself gets "lumpy." Each photon has more energy, so more power is required to receive a given number of photons per second.

In the microwave region, both of these sources of noise become less than the cosmic radiation of the "big bang" and that sets the floor on our ability to communicate.

Now the more the spectrum, and the greater the number of directions we have to search in a given time, the less time we can spend on a given frequency channel and given direction. The signal must therefore have more power to deliver enough energy in this shorter time, which says our receiving sensitivity is less. The conventional wisdom of SETI is therefore to find ways of reducing the amount of spectrum to be searched and the number of directions in which we must look.

In the microwave region, certain cardinal frequencies have been identified. Cocconi and Morrison in 1959 pointed out that the hydrogen line at 1420 MHz was a naturally marked frequency in the best part of the spectrum. The Cyclops team in 1971 extended this concept by observing that the hydrogen and hydroxyl lines define, between them, a relatively narrow frequency band in the best part of the spectrum.



Because H and OH together form water and because water is a symbol of life, the team dubbed this band the "waterhole" and suggested that this might be where different Galactic intelligent species meet. A rational SETI program would search this band thoroughly but not exclusively. This is another reason for wanting to move rapidly, because we want to search other bands than this one. We don't want to put all our eggs in that one basket, even with a targeted search.

Speaking of targets, it is hard to conceive of intelligent life evolving and existing anywhere except in the vicinity of certain middle-class stars—isolated main sequence, F, G and K stars, not too different from our Sun.

A rational SETI program would concentrate its search in the direction of such stars, beginning with the closest and proceeding to greater and greater distances from the Sun as the receiving sensitivity was increased.

The Cyclops study—1971—showed that it was technologically feasible, if it became necessary, to construct phased antenna arrays having collecting areas equivalent to a single antenna miles in diameter.

Because we made artist's renderings of such an array and showed these pictures widely, this huge array costing tens of billions of dollars has become synonymous with SETI in people's minds. They think an antenna array of this size is needed to ensure success. This may be true, but it is also possible that SETI could succeed with only one antenna, a much less formidable installation.

If we must depend upon overhearing leakage signals—eavesdropping on signals not intended for us—and must do so out to 100 light years, we would need the full array. But there are other possibilities. Suppose that within that range of 100 light years another civilization has already, for its SETI program, built a huge array of, say, 2,000 antennas, and, after a long search, during which they detected no signals, they now use the array half the time to beam signals at the 2,000 "good" suns, within 100 light years of them. Each target sun would then have a strong signal beamed at it half the time. If our sun were one of the targets we could detect that signal with only one antenna on our part.

If we were to install a dedicated facility and begin to expand it, it would start with the single antenna you see and would grow as we added further antennas.

As soon as the SETI array exceeded four 100-meter antennas, it would be the most powerful radiotelescope on Earth. A rational SETI program would share this facility with radioastronomy some appropriate fraction of the time, so that positive results could keep coming out of the facility.

Quite probably the array would repay its debt to society on the basis of its radio- and radar astronomy discoveries alone.

The SETI aspects of the program would be automated except for maintenance. The negative results, which would be sure to occur for a while, would therefore not discourage the computer as they would human minds.

The searches that would be conducted this way and the targeted search proposed using existing antennas would be 10 to 100 times



more sensitive than any that have covered an appreciable frequency band to date, because we would have the right data processing equipment in our system. That has not been the case up to the present time.

At the outset, with only a single antenna, the sensitivity is low, even for a targeted search; nevertheless, certain kinds of beamed signals would be detectable, as we said. For an all-sky survey covering about one-eighth of the microwave spectrum in the same time, using one antenna, the sensitivity would be about one-ten thousandth as great.

The sky survey is intended to receive signals from beyond 100 light years, since anything closer would be picked up in the initial targeted search; but to be picked up on the all-sky survey, a transmitter at 100 light years would have to beam 100 million watts at us with a 100-meter antenna.

If the other civilization were to beam this 100 million watts at us, and at all the "good" suns within 100 light years of them, this would require 200 billion watts, or roughly the electrical power consumption of the United States.

Beyond 300 light years, there would be so many stars to beam signals at that the sending civilization would give up beaming and would radiate in all directions. The prepared sky survey would then find them only if they radiated into space as microwaves the power of a million nuclear powerplants. I find this rather hard to believe, even for an advanced civilization.

Thus it is difficult to imagine sources powerful enough to be discovered with a small-scale sky survey that are not also close enough to be discovered with a small targeted search. Only when and if a very large facility has been constructed and targeted searches of long duration out to great distances are being conducted would a sky survey using the same antennas at the same time be cost effective. A rational SETI program would concentrate on a targeted search, piggybacking a sky survey using the same system.

I recommend that the above programs in biocosmology be undertaken as a continuing mission for NASA. I say continuing because one cannot say in 5 years we will find other intelligent life, or in 3 years we will discover how life began. The program is a mixture of research and exploration. If we knew what we will find, we would not have to embark.

The discovery of other life is not only a legitimate mission for NASA but it is also an essential one without which popular interest and support will fade. It is probably fair to say that NASA exists because it was felt that a space program might discover other life in the solar system. We now know that won't happen; we must look deeper into space.

In my opinion, biocosmology would enjoy great popular support. Even unscientific motion pictures and fraudulent books regarding extraterrestrial life enjoy unprecedented popularity. Surely a soundly based scientific program would be accepted. In fact, most laymen assume that a SETI program is already going on, and I feel it is high time that NASA lived up to this popular belief.

Even if this committee and your Senate counterpart endorse biocosmology and SETI, a significant program in this area will never become a reality unless your colleagues in the appropriations



committees are convinced of its worth. It is a futile exercise to plan for years, to win your endorsement, to win the support of the President's Science Adviser and of the OMB, only to have the House and the Senate Appropriations Committees strike the program from the budget.

I realize that the budget, must be controlled, but I recommend that we do this by eliminating large programs that are failures or have outlived their usefulness. If our penury prevents us from initiating new programs, we are indeed lost. You prune a tree by sawing off old, dead branches and balancing its shape, not by nipping off new buds. That is the way to kill a tree.

You must help us clear these political roadblocks or NASA will never be able to embark on this exciting electronic voyage of discovery. You must help us convince our Ferdinands and Isabellas, and we may then show you completely new worlds, perhaps even a federation of new worlds comprising the mainstream of life in the galaxy.

Thank you, Mr. Chairman.

Mr. FUQUA. Thank you, Dr. Oliver.

I have some questions for both witnesses, but we do have a rollcall vote on at this time, so we will have to take another short break, and I will be right back.

Dr. OLIVER. Thank you.

[Brief recess.]

Mr. FUQUA. The subcommittee will resume.

Dr. Heesch, you mentioned that scientific basis should be the prerequisite for a search for extraterrestrial intelligence. How would you describe the nature of this scientific basis? What should we be looking for, what are the thrusts—

Dr. HEESCHEN. Of the scientific elements of the search?

Mr. FUQUA. Yes.

Dr. HEESCHEN. I think Dr. Oliver described them very well, as did Cameron to some extent yesterday. These biological questions, the planetary questions, have all been, I think, set out pretty well by Oliver and Cameron.

Mr. FUQUA. What do you think that we should know before we get into this?

Dr. HEESCHEN. I don't think we should wait to do a search until we know any particular thing. I just feel the greatest progress will come through the general course of scientific investigations. That is certainly where the greatest progress has come so far, and I don't know of any particular discovery or thing that we might learn that would say now is the time to go ahead or this assures that there is intelligence or isn't.

Mr. FUQUA. You don't think then that there are any milestones that we should achieve prior to making the search?

Dr. HEESCHEN. Not prior to making a search, no.

Mr. FUQUA. This is a basic research program?

Dr. HEESCHEN. Yes. I think that a certain amount of searching is a necessary and desirable thing now, independently of the general scientific work.

I also believe that the scientific work itself is even more important than the direct searching.



Mr. FUQUA. Do you think that the radiofrequency spectrum is the most beneficial regime for detecting evidence?

Dr. HEESCHEN. I personally think it is, yes.

Mr. FUQUA. At the National Radio and Astronomy Observatory, which you head, do you anticipate any future SETI studies there?

Dr. HEESCHEN. Yes. The amount of work that has been done on SETI at the NRAO—and I believe at other observatories—has been determined entirely by the amount that competent people have been willing to undertake up to now and not by any policy decisions on the part of the institutions.

It would be quite feasible and easy for NRAO to undertake a lot more SETI-type searches than have been done up to now if the people to do it would come along. The NRAO does not in itself have institutional programs. We are a national center that is meant to serve the scientific community as a whole and the vast majority of the work done at NRAO is done by investigators from the various universities and other institutions.

Mr. FUQUA. Dr. Oliver, you mentioned in your statement that you felt that SETI involved analysis of a broad spectrum and not necessarily well-defined. I am not trying to put words in your mouth or change the context of your statement—but for SETI, analysis of a broad spectrum is probably not based on as scientific a foundation as some of the arguments that you advanced, in one of the slides you were showing, about the H and the OH lines. You also mentioned that you thought that SETI should be initiated at this time.

Could you expand on that?

Dr. OLIVER. I think the word “spectrum” is ambiguous. Are you saying that SETI should cover a broad portion of the radiospectrum?

Mr. FUQUA. Yes.

Dr. OLIVER. I think that while we have the radiospectrum open to us, it would be very unwise not to examine other frequencies than these naturally defined ones that we have talked about—the “waterhole, hydrogen line and so on.” We should with all haste examine these bands because in a few years they will be filling up; this is one of the things that gives some urgency to the program.

I might say in furtherance of the first question you asked Dr. Heeschén, that the Ames targeted search proposal is to develop data processing equipment and low noise front-ends for receivers that could be applied to facilities such as the NRAO to conduct these searches at a higher sensitivity level than has ever been done before.

We are not just repeating earlier negative research; we are going to be a hundred times more sensitive with a minimum expenditure, and I think that David welcomes such equipment.

Dr. HEESCHEN. Surely.

Mr. Chairman, may I add a comment to Mr. Oliver’s remarks about filling up of the frequency spectrum?

Mr. FUQUA. Certainly.

Mr. HEESCHEN. Because this is, I think, an extremely serious thing, not only for SETI but also for all of radioastronomy and radioscience, that we are squandering away the frequency spec-



trum. Furthermore, NASA is its own worst enemy in this regard because it is the one that is doing it.

Mr. FUQUA. Dr. Oliver, you indicated—this is further elaboration of the first question I asked—that you preferred the targeted research with specific stars as opposed to NASA's survey of the whole sky. How would this affect the size of the funding requirements for such a program?

Dr. OLIVER. The targeted search was about one-third of the budget that was approved last year, whereas the all-sky survey accounts for about two-thirds, if my memory serves me correctly. In other words, the targeted search is not only, in my mind, the more effective way to go, but also it is the less expensive way to go; in a time of severe funding limitation it would seem additionally rational to concentrate on it.

Mr. FUQUA. I would like to ask both of you to comment on this: Do you think the scientific community is in a position now to choose the targets for such a search?

Dr. OLIVER. If you accept the belief that civilizations in general evolve as we did, around a middle-class star—and it is hard to see how civilizations could develop or exist for long times out in the blackness of space without any source of light and heat—then the answer to your question is yes, because we do know the main sequence, F, G and K stars, out to about 100 light years. In other words, the targets are already present and represented in our star catalogs. If we try to go beyond that distance, we run into difficulty. We would have to make thorough stellar census in the optical region to find the right kinds of suns.

Mr. FUQUA. Do you care to comment, Dr. Heeschén?

Dr. HEESCHEN. I think I agree with that. We certainly know enough to select a reasonable group of targets, as Dr. Oliver said. There is a question about whether it is nearby, whether the civilizations will be relatively nearby or very distant, and if those people who opt for the second option, that in general the civilizations will be few and far between and therefore on the average very distant, then what Dr. Oliver says about requiring an optical search to find the proper stars would be correct, but it is logical to start with the nearby objects and those we do know about.

Mr. FUQUA. At the NRAO, are your facilities capable of detecting the level of signals that NASA is proposing in their SETI program?

Dr. HEESCHEN. I am not sure of the answer to that. I think that we are not capable of detecting the levels that are being proposed in the targeted search program of NASA. I think that we are capable, although here I would have to go back and make a calculation—and maybe Dr. Oliver knows the answer—I think we probably are capable of equaling the sensitivity of the so-called full-sky survey.

Dr. OLIVER. We were just doing the calculation for the VLA. At the present operating level it has a sensitivity on the order of 10 to the minus 21st— $10^{-21}$ —watts per square meter. The targeted search, because of its narrow frequency resolution and low noise front-end, is around 10 to the minus 27th— $10^{-27}$ —watts per square meter or about a million times more sensitive. That is because of the data processing and the special equipment that we are proposing.



Dr. HEESCHEN. And not because of some fundamental difference in the basic antennas.

Dr. OLIVER. I would like to emphasize that, in my opinion, sensitivity is the name of the game. If you imagine you already have a certain receiving capability and you ask yourself: "which would pay off most, to search more frequencies or to search more directions, or to have higher sensitivity, I think you will conclude the last. The argument goes this way:

If you are already listening in the quietest part of the spectrum, then if you widen your search band, you will increase your probability of detection but not in direct proportion to bandwidth since the added bands are not as good.

If you search in more directions and you already are looking into the most probable directions, you will increase your probability of success but not in proportion to the number of directions, since the added directions are not as good. But if you increase the sensitivity, the probability of success goes up as a three halves power of sensitivity. Four times the sensitivity will put eight times the number of targets within your reach.

So, if you are going to do any one thing, you should certainly go for more sensitivity first.

Mr. FUQUA. All the witnesses, I think, have emphasized the importance of the search for extraterrestrial—intelligence.

How much should that research influence the goals and objectives of the NASA space science program? Maybe both of you would like to comment on that.

Dr. OLIVER. Perhaps I could lead off by saying that I think it is an inherent part of the NASA charter. If you read the Space Act, you will find that the search for life was included in it. Since that search has given only negative results in the solar system, I would favor extending the search beyond our star to other stars.

Dr. HEESCHEN. I am not able to give as reasonable an answer one way or the other. I am not familiar with NASA's charter or what specifically they should or should not do in the space program.

At the moment, the logic that I see for NASA doing it is that they have proposed it and nobody else has. I don't see any other, but maybe it is there.

Mr. FUQUA. With the bells ringing again, I guess this is as good a place as any to thank you very much for being here this morning and sharing with us your thoughts about a very intriguing and intellectually stimulating discussion about extraterrestrial intelligence.

Thank you so much.

Dr. OLIVER. Thank you, Mr. Chairman.

Mr. FUQUA. The subcommittee will adjourn until Monday, September 25, at 9:30 a.m., in this room, to review NASA programs. [Whereupon, at 11:30 a.m., the hearing was adjourned.]





