JOURNAL OF SCIENTIFIC EXPLORATION

VOLUME 2, NUMBER 2, 1988

A Publication of the Society for Scientific Exploration

PERGAMON PRESS

New York ■ Oxford ■ Beijing ■ Frankfurt
São Paulo ■ Sydney ■ Tokyo ■ Toronto

JOURNAL OF SCIENTIFIC EXPLORATION

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Publishing, Subscription and Advertising Offices: Pergamon Press, Inc., Fairview Park, Elmsford, New York 10523, USA and Pergamon Press plc, Headington Hill Hall, Oxford OX3 0BW, England.

Published Two Per Annum. Annual institutional subscription rate (1989): US$75.00; *Two-year institutional subscription rate* (1989/90): US$142.50. *Professional subscription rate* (1989): US$40.00. Prices are subject to change without notice. Members of the Society of Scientific Exploration receive personal subscriptions as part of their dues; details of membership are available upon request. Notify 8 weeks in advance of address change with a copy of the subscription mailing label. *Microform subscriptions*: Back issues of all previously published volumes are available direct from Pergamon Press. Back issues of Pergamon journals in microform can be obtained from UMI, 300 North Zeeb Road, Ann Arbor, MI 48106, USA.

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The Item-Fee Code for this publication is: 0892-3310/88 $3.00 + .00

Printed in the U.S.A.

Journal of Scientific Exploration, Vol. 2, No. 2, pp. 91–92, 1988
Pergamon Press plc. Printed in the USA.

0892–3310/88 $3.00+.00
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Editorial

As the incoming editor of the *Journal of Scientific Exploration,* I owe a great deal to my predecessor, Prof. Ronald A. Howard. Without articles there is no journal, but without a journal there are generally no articles. It was that circular situation that confronted Ron Howard at the outset, and it was his energy, enthusiasm and plain hard work that got us off the ground. For that I, the other members of the Society of Scientific Exploration, and whoever comes to read this journal in search of knowledge, ideas, and perhaps most of all inspiration to explore the unknown, owe him our thanks and gratitude.

The world is full of things we do not understand, but more profound is the realization that the world is almost certainly full of yet more things that have not even risen to our collective level of awareness. As philosophers of science such as Thomas Kuhn, Paul Feyerabend, Kurt Hübner—just to name a few from the modern scene—have emphasized over and over, it has often happened in history, and therefore is surely happening now, that a phenomenon is not even noticed until the mindset is in place to grasp the recognition of something staring us in the face. Having countless times heard the derisive story of the eminent churchmen who refused to even look through Galileo's telescope to see the plain sight of Jupiter and its moons because such a thing could not be, it was a revelation to me to read an explanation, one day, of why their actions might have been perfectly reasonable after all: Not only was it a fact that the optics of the day yielded only a rather distorted view of magnified, distant things here on earth, but there was a solid theoretical foundation in Aristotelian physics for believing that terrestrial laws of optics were inapplicable in the celestial realm, and so there was no reason to believe that the "plain sight" of that planetary system would be anything but a distorted illusion. Is it farfetched to wonder what phenomena of nature there might be which our modern scientific worldview obstructs from our vision?

My point here is not really to chastise science, for after all most of the member of SSE, myself included, are practicing scientists. Ever since Galileo we have been on the path of scientific exploration and there is no undoing that and every reason not to seek that in any event. The name of our journal—*The Journal of Scientific Exploration*—makes clear both our objective—exploration—and our methodology—science. It may well be that there are other, even more fruitful, alternative ways to explore the unknown, and it may well be that there are phenomena that are real but inherently incomprehensible in the scientific sense, phenomena in which the essence is lost when reduced to scientific analysis. How could one hope to translate couplets of Shakespeare into coupled linear equations? But for better or

worse, we have set ourselves the task of using the known tools and techniques of science to explore phenomena at the very limits of, or hopefully not incomprehensibly far beyond, the scientific frontiers, and we refrain from the doubly risky venture of setting off into uncharted territory with uncalibrated instruments of navigation.

This approach makes the objectives of our journal clear, as I see them. First, JSE exists to provide a forum for communication of rational investigations into anomalous phenomena, and these constitute our research articles. Moreover as a scholarly, refereed journal, published by an established, respected academic press, and subscribed to by libraries, we carry out the research tradition and obligation to archive our investigations, and the knowledge that our journal may remain on the shelves for years to come is a good and proper motivation to keep our standards high. Secondly, it is both legitimate and useful to analyze and reflect upon scientific methods and scientific thought processes, including the social factors that influence them, and by and large such articles constitute our invited essays. These will of necessity be more speculative in nature, but in place of rigor and precision we substitute the complementary standards of depth and insight.

These two classes of articles are the primary elements of JSE fulfulling the objectives of the SSE. In addition, book reviews and letters commenting upon the articles will appear. As has been our policy, articles from non-SSE members are welcome provided they are sponsored in writing by a member of the society; no such restriction applies to letters.

I wholeheartedly agree with the final statement in Ron Howard's lead-off editorial in issue 1: "Exploration should be fun." I look forward to the fun of seeing new ideas landing on my desk. I also welcome comments, suggestions, and, yes, criticisms from, you, our readers.

Bernhard M. Haisch
Editor, JSE

Journal of Scientific Exploration, Vol. 2, No. 2, pp. 93–142, 1988
Pergamon Press plc. Printed in the USA.

0892–3310/88 $3.00+.00
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INVITED ESSAY

UFOs and NASA

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Abstract—In 1977 President Carter's Science Advisor recommended that a small panel of inquiry be formed by NASA to see if there had been any new significant findings on UFOs since the US Air Force-sponsored investigation of UFOs ("Condon Report") a decade earlier. Five months later, NASA responded to that recommendation by proposing "to take no steps to establish a research activity in this area or to convene a symposium on the subject." This article offers a partial inside look at how that decision was made at NASA.

Introduction

Forty years ago Kenneth Arnold's sighting of "flying saucers" inaugurated the modern era of observation of Unidentified Flying Objects, or UFOs. The possibility that some UFOs are actually spacecraft, bearing intelligent beings from another world, has focused intense public interest on the subject.

While only a very small number of reputable scientists has ever taken UFOs seriously, the related search for radio signals from other civilizations has slowly increased in "respectability" over the decades following the pioneering suggestion of such searches by Cocconi and Morrison (1959). A turning point occurred, however, when Hart (1975), and Tipler (1980), argued convincingly that an intelligent civilization in the galaxy would rapidly physically colonize the galaxy (see also Jones, 1981). Their suggested conclusion is that we are in fact the only civilization in our galaxy, if not the Universe.

An alternative conclusion is that one should perhaps take more seriously the possibility that some UFO reports do represent manifestations of galactic intelligence.

The canonical study of that possibility is "Scientific Study of Unidentified Flying Objects" (Condon & Gillmor, 1968), the so-called "Condon Report," which concluded, despite Condon's clearly negative feelings about the value of UFO study, that of 59 cases studied, two involved "probable UFOs" and two "possible UFOs" (Sturrock, 1987).

Over the second half of the year 1977, the National Aeronautics and Space Administration found itself, as a result of a letter from the White House, considering whether more should be done on the subject of UFOs, and in particular, whether NASA should do it.

NASA's final response, which came at the end of 1977, was worded cautiously, but in effect said "no" to the White House. The present paper bears on how NASA coped with the White House request.

Acceptance by the establishment of the notion that alien intelligences are active in the vicinity of the Earth, would involve a profound change in a fundamental paradigm that governs our activity as a society. (For example, President Reagan has remarked, to Mikhail Gorbachev, that American and Soviet societies would bury their differences if the world were threatened by an alien intelligence.)

Also, NASA has a large science constituency. According to a *Science Magazine* report (16 December, 1977, p. 1128) "NASA seems to fear that the reopening of the question of the genuineness of visitors from outer space will legitimize a subject most establishment scientists consider phony and a waste of time."

How exactly did NASA cope with this "hot potato," and why did NASA decline the White House request? In the next section I describe NASA's interaction with the White House, and in the following section I specify more completely the aim of the present paper. The remainder of the paper details aspects of NASA's activity in dealing with the White House request.

UFOs: NASA and the White House

On July 21, 1977, Dr. Frank Press, Science Advisor to President Jimmy Carter, sent a "Dear Bob" letter to Dr. Robert Frosch, Administrator of the National Aeronautics and Space Administration.

The letter opens by indicating that the White House "is becoming a focal point for an increasing number of inquiries concerning UFO's," and Press suggests that NASA should instead become the focal point for general correspondence, and that those inquiries which come to the White House henceforth be sent to the designated desk at NASA.

Press goes on, however, to say that "since it has been nearly a decade since the Condan (sic) report, I believe that a small panel of inquiry could be formed to see if there are any new significant findings" on the subject of UFOs. He suggested that the panel of inquiry "could be formed by NASA," and stated that "since this is a public relations problem as much as anything else, people who are known to be interested in the problem and also highly known, such as Carl Sagan, ought to be involved."

His letter is reproduced at the end of the paper as Appendix 1.

NASA, and the country, were aware that President Carter himself had once reported a UFO sighting. In an early draft of an Information Sheet (Number 78-1), prepared in early 1978 (that is, following NASA's responses to Press's letters), NASA described Carter's sighting as follows:

> *PRESIDENT CARTER's UFO SIGHTING*—While serving as Governor of Georgia, Mr. Carter reported to NICAP that he had seen a bright, moving object in the sky over Leary, GA in October 1969. He said the object was visible for 10 to 12 minutes

and, at one point, shone as brightly as the Moon. The regional NICAP representative investigated the sighting and reported there was no evidence to support anything beyond placing what Mr. Carter saw in its "unidentified" category.

Thus, it could not be completely clear to Dr. Frosch exactly what the significance of Press's letter was—simply an attempt to clear Press's desk of mail that he was not equipped to answer, or a White House expression of real interest in UFOs.

Frosch responded to Press on September 6, 1977 (Appendix 2), indicating that NASA was "inclined to agree with your recommendation," but indicating that "there are a number of questions that need to be resolved before any formal program is undertaken." In particular he noted that "a panel of inquiry such as you suggest . . . would require some additional resources [money] for the inquiry and for follow on activity . . . we should assure ourselves that an inquiry is justified. I believe we could do this by naming a NASA project officer to review reports of the last ten years and to provide a specific recommendation relative to any further inquiry by the end of this year. If you concur, I will initiate this action." He enclosed, for Press's information, a NASA Information Sheet (76-6) on "Unidentified Flying Objects" (Appendix 3). Press gave the requested concurrence on September 14, 1977 (Appendix 4).

Then, on December 21, 1977, Frosch, in a remarkable letter to Press, "proposed" that "NASA take no steps to establish a research activity in this area [UFO's] or to convene a symposium on the subject" (Appendix 5). There is no mention of a project officer, or of any review "of reports of the last ten years," but Frosch indicates that "we have given considerable thought to the question of what else the United States might and should do in the area of UFO research. There is an absence of tangible or physical evidence available for thorough laboratory analysis," and he indicates that "we stand ready to respond to any *bona fide* physical evidence . . ."

Purpose of This Paper

What happened within NASA, resulting in the two letters that Frosch wrote to Press? How does a government agency formulate a response to, in effect, the President of the United States, on a topic of the peculiar sensitivity, interest, and controversial nature, as UFOs? It is the purpose of the present paper not to actually answer that question, but to provide information bearing on that question. To actually answer the question, as we shall see, would require substantial additional information from many individuals. Thus, the present paper represents an "interim report" that might be followed in the future by a more global inquiry by others.

In order to understand why this paper is not more comprehensive, it is necessary to understand how NASA works, and its structure.

NASA

NASA is a large organization, with Centers in various parts of the country. In 1976 (but to a much lesser extent today) the Directors of the various Centers played a powerful role in NASA policy making. Apart from those specific individuals, however, NASA policy making was concentrated entirely in persons at NASA Headquarters in Washington, DC.

The structure of NASA Headquarters, as of January 1978, is shown in Figure 1. From October 1976, to August 1978, I was Deputy to Mr. Bland Norris, Director of the Astrophysics Division, which was part of the Office of Space Science; that Office was directed by the Associate Administrator for Space Science, Dr. Noel W. Hinners; his Deputy was Mr. Tony Calio. Hinners reported directly to the Administrator, as did Dr. John Naugle, Science Advisor. Policy making stopped with Hinners, all of us below being providers of information and advice, and executors of policy.

NASA Headquarters was an interesting and, to an academic person, strange place. It had a great degree of vertical structure. Hinners would occasionally, and in an almost embarrassed manner, stroll around the fifth floor from the "front office" he inhabited. The Administrator (on the seventh floor) was even more inhibited—an Administrator's ramble in the building was a formal, prepared, activity. Such things did not happen often. This was not a reflection of the personalities of the two men (in fact both are personally warm, intelligent, and charming); it was a product of the institutional structure of NASA itself.

Coherence of policy and activity was maintained in a clever, effective way, best illustrated by an example. Suppose that scientist John Doe writes an angry letter to someone at some level in NASA. The letter is immediately copied (often without the recipient even seeing it) and sent to all the lowest levels in Headquarters that the secretary deems relevant, considering the content of the letter (Naugle joked that he had once received a letter from an old girlfriend, "and it had gone *everywhere,* since the subjects were so *general*!"). A draft reply is generated by the lowest-level person on the distribution, and this draft works its way up through the entire organization for, finally, signature and mailing by the original recipient (who now reads the letter perhaps for the first time). At any point in the chain, the draft reply letter may be rewritten by the higher official. Each level must concur with the version sent higher, by initialing in the appropriate place.

This system works extremely well. Everyone in NASA Headquarters who needs to know about the letter knows it, and attempts from outside to set the system against itself are doomed. This same mechanism is often used in areas of policy, to obtain comment from many levels.

The result of this vertical structure, however, is that it often is not clear in the end just where the policy that is "signed off on" by the responsible senior official actually originated, or what precisely motivated specific items or changes in items. Each individual sees only what flows up or down through

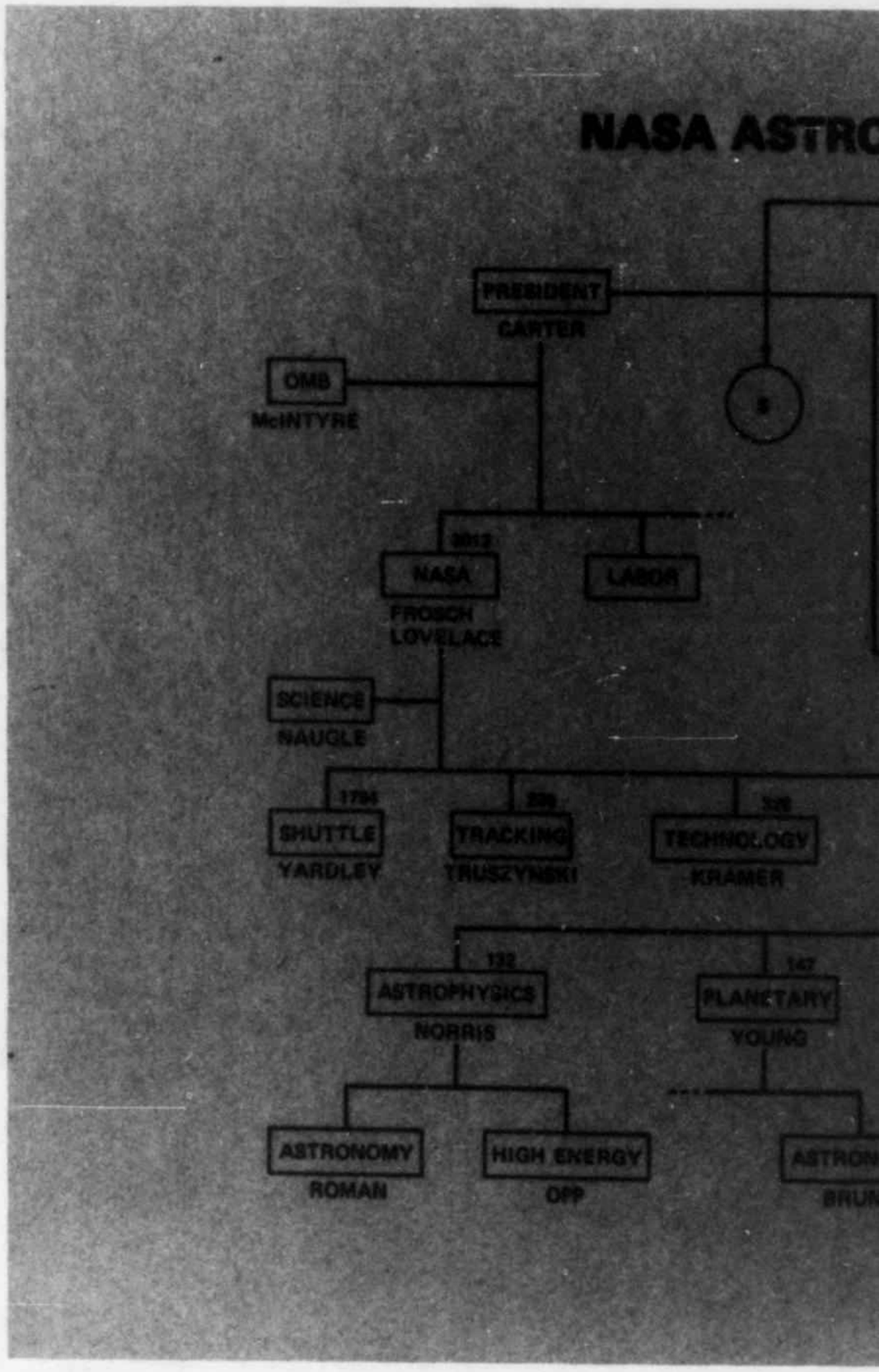
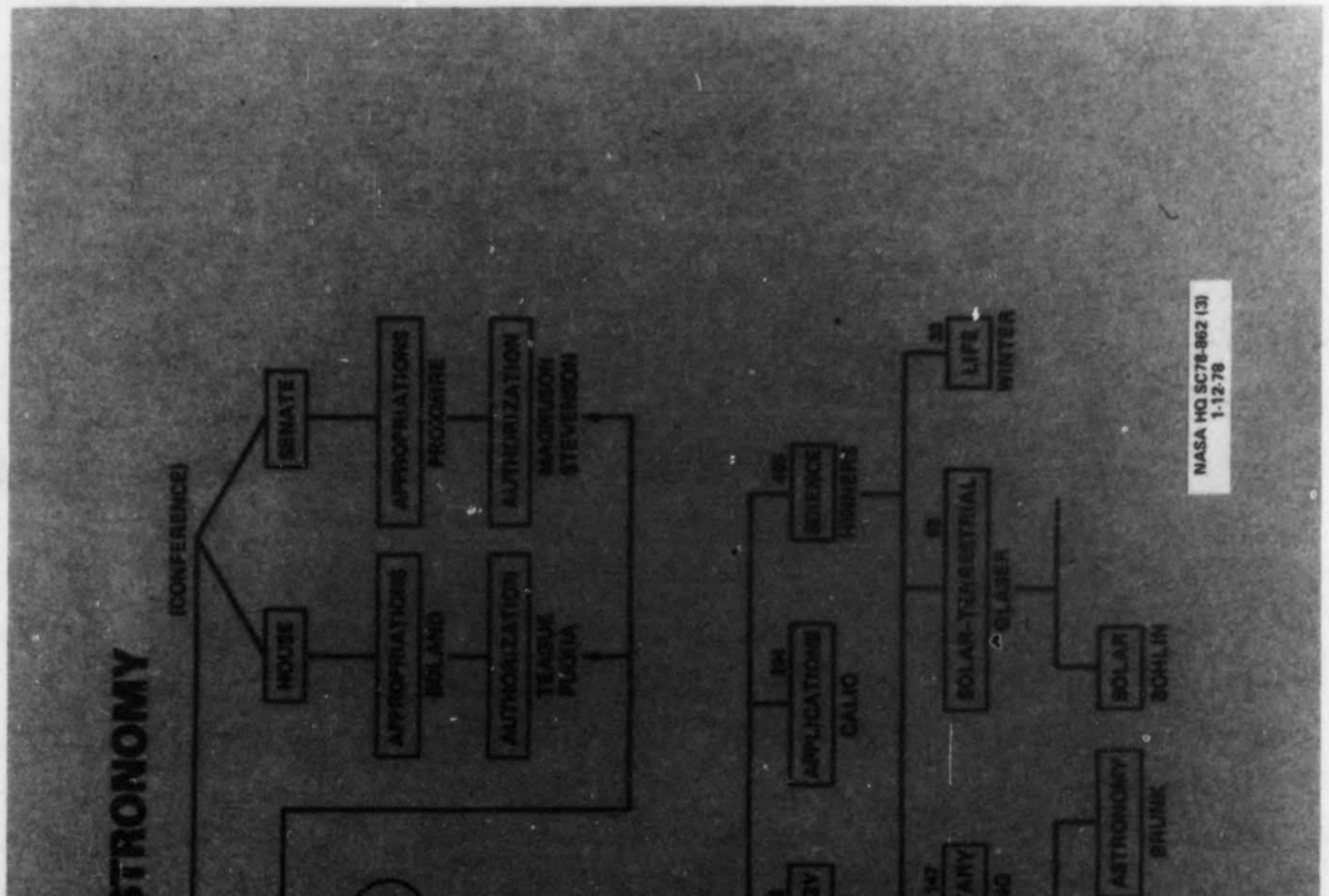


Fig. 1. The structure of policy making, policy a
NASA, in January 1978. Numbers indica
dollars.



olicy administration, and funding, with regard to indicate annual budget in hundreds of millions of

his or her level, not the total picture, which is seen by no one. This of course holds true in the case of my personal perspective on the NASA-White House UFO activity. Thus, in the present paper on NASA's activity regarding the UFO question, I can only present certain documents, and attempt a little detective work toward the question of "who shot John?", that is, who made the basic NASA decision concerning UFOs, a decision apparently still in force today. Let us begin.

The Author at NASA

Bland Norris telephoned me (I was an Associate Professor at The Johns Hopkins University) from Woods Hole during the summer of 1976, and in effect offered me the position of his Deputy. He was almost certainly influenced to do this by George Field, the eminent astronomer who was then head of the Physical Science Committee (PSC) (the internal NASA Committee advisory to Hinners). At Woods Hole a high-level group was studying the Hornig Committee report on the proposed management structure for the proposed Space Telescope, and some of the group apparently felt that having a scientist (such as myself) from the astrophysics community move to NASA Headquarters for a few years would help in "selling" the Space Telescope to the Office of Management and Budget, and Congress. Norris, an excellent engineer and administrator, had no knowledge of astrophysics (although he did take a Community College night course in astronomy during my period at NASA). On my arrival at Headquarters, I found that in fact Space Telescope was in excellent hands with Warren Keller and Nancy Roman. Indeed, a pleasant surprise was the uniformly excellent quality of people I found in the Office of Space Science.

At about the same time that I arrived, David Morrison, a well-known scientist from the University of Hawaii, came to occupy a roughly similar position in the Planetary Division. Morrison was to try to sell Galileo (a mission to the planet Jupiter), while I sold Space Telescope, and there was friendly rivalry between us. (Both Space Telescope and Galileo did succeed in becoming approved missions but—a decade later—neither has been launched.)

Shortly after my arrival at NASA, Hinners' Deputy, Tony Calio, strolled down the hall to my office with something on his mind. I did not know, yet, how unusual this was. He wanted someone to handle SETI, the "Search for Extraterrestrial Intelligence," and he had fixed on me. As this subject is clearly relevant to NASA's attitude toward UFOs, I will expand on this, my earlier (and later) involvement with SETI.

John Billingham, at NASA's Ames Research Center, was the person who was focusing an attempt to get NASA to fund, and indeed to carry out, a radio search for extraterrestrial intelligence. A detailed report, funded by NASA (Morrison, Billingham, & Wolfe, 1977) was in preparation. Calio asked me to look into the matter and recommend whether the Office of

Space Science should fund SETI, and at what level. An interesting complication was that NASA's Jet Propulsion Laboratory (JPL) had a different approach to SETI than had Ames, and wanted to carry out its program. In essence, Ames wanted to look with very great sensitivity at small numbers of nearby stars, at specific frequencies which Ames had somehow deduced would be used by other civilizations, while JPL wanted to survey the whole sky at a vast number of frequencies, paying the price, of course, of greatly reduced sensitivity. I quickly became convinced that the JPL approach was the right one, and that the Ames approach was a waste of money. It seemed to me that it was not right for the young, new, poor civilization (us) to have to spend taxpayers' money to get great receiving sensitivity; instead, the old, experienced, rich civilization (them) should spend the money to get great transmitting power. Also, I felt that if there were civilizations broadcasting from nearby stars, we would already know it; that in fact they would be aware of us and might even be here (UFOs?).

A good indication of my attitude on these matters is given by the letter I wrote (Appendix 6) to Major Ret. Colman S. Von Keviczky, in reply to his letter (Appendix 7) to Ichtiaque Rasool (who was Hinners' personal science advisor).

Billingham pressed me hard to come up with some immediate funding for his SETI activity and, quite remarkably, I was able to do so. Someone had told me that the front office had some few hundred thousand dollars available, as a result of some reprogramming. This was unusual; normally only the lowest level people at NASA Headquarters actually had money, and if you tried to take it from them, they made you very aware of how much damage you were doing. Ed Wash, Hinners' excellent financial man, told me in his usual worried way that he had wanted to reserve the reprogramming money for solar sailing (which was about to enter a "shoot-out" with the solar electric propulsion over which was to become the planetary program propulsion "new technology" of the 1980's—rather pathetic in retrospect!). However, he gave me half the money, which I gave to Ames.

In formulating our budget for the next fiscal year, Norris and I placed the JPL program adjacent to, but above, the Ames program, with both of them right at the very bottom of our Astrophysics Budget priorities, and then we sent the budget up to Hinners for possible re-prioritization and for merging of our budget with those of the Planetary, Solar-Terrestrial, and Life Sciences (see Figure 1) Divisions.

Tony Calio himself was quite enthusiastic on the subject of SETI. Hinners was considerably less enthusiastic; in fact Calio told me at one point "this is the only thing Noel and I have ever come apart on." Possibly Hinners did lack belief in the reasonableness of SETI, but I suspect that his greatest concern was for the stature of his science program and its prospects. At a public lecture at Princeton, Hinners asked the audience to "vote" as to whether SETI represented a proper use of public funds (they agreed it did).

It was my understanding, some weeks after budget submission, that I had

won a victory, albeit a Pyrrhic one: The NASA budget that emerged, and was sent to the President's Office of Management and Budget (OMB), contained only the JPL program, but unfortunately at too low a priority level to survive OMB. I was astonished that when the budget was returned from OMB, SETI was still included; that is, it had been moved to a sufficiently higher position in the Carter "zero-based budgeting" priority that it had survived OMB's financial knife.

This is a vivid example of how one can seldom be certain of "who shot John." For example, at whose level was the JPL program separated from the Ames program? Hinners? Frosch? And who at OMB did the reprioritizing, and why? On the latter question, I was able to obtain some information, much later. On May 17, 1978, Alan Lovelace, Frosch's Deputy, wrote to W. Bowman Cutter, Executive Associate Director for Budget, Office of Management and Budget, inviting him to come over to NASA Headquarters some evening for dinner, and an informal briefing on astronomy by me and David Morrison. Cutter had apparently expressed an interest in such a briefing some time in April. The dinner took place on July 13, 1978. Frosch was not able to be present, and neither was Hinners: The senior NASA person present was Adrienne Timothy, who had replaced Rasool, who had left NASA. The dinner and subsequent slide shows went very well; in particular, Morrison did his usual brilliant job expounding the spectacular NASA planetary images. At dinner things were sufficiently informal that I felt able to mildly enquire about how it had happened that SETI had survived OMB. Cutter replied "I took astronomy at Harvard from Carl Sagan, and I did it for Carl."

The subsequent history of SETI on Capitol Hill, was the award of a Golden Fleece by Senator Proxmire (February 16, 1978), and the rejection by Congress of initiation of a SETI program. I was the person who was invited to the Hill to expound the program to Proxmire's aide, in preparation for the Fleece—rather fun, but sad for SETI. (After I left NASA, SETI was given to Life Sciences, and Proxmire's aquiescence was somehow obtained. A SETI program—JPL *and* Ames—is moving forward today.)

Our presence at the Cutter dinner is an example of the special role that Morrison and I played while at NASA, as active scientists with much greater technical knowledge of our fields than almost anyone else at Headquarters (but, I must add, negligible administrative ability, at least in my case, compared with almost anyone else at Headquarters). We were called on when technical substance and scientific depth were needed. I will end this section with another example of this, which is of special interest because President Carter was directly involved.

On November 15, 1977, I found on my desk a note for Bland Norris from David Williamson, Jr. I later learned that Williamson was "Code AX," Special Projects (Hinners was Code S, Science, and I was Code SAD, Science Astrophysics Deputy; the reader can use these Codes to track "who saw what," in certain of the appendices). Williamson was located on the

seventh floor, with the Administrator (Code A). (Williamson will play a prominent role in the discussion, below, of the UFO situation.) The note said "Bland . . . Jeff is expected to be calling Dick starting November 16 in the morning . . . Jeff has a 3″ reflector . . . Frank Press hopes we can come up with a 7″ Questar electric . . . the 7th floor offers its appreciation to the 5th floor for such an effective and controlled reaction." Bland let me know that my guess was right: "Jeff" was Jeff Carter, son of President Carter.

At Bland's request, I telephoned Frank Press, who let me know that the President and/or his son (it was not clear which) wanted to borrow a small telescope to take to Camp David over Thanksgiving.

To NASA Headquarters, "telescope" is a budget item that the astronomers want too many of. What it is physically, and where one might be obtained, was unknown. I exaggerate, but certainly, Headquarters contained only paper; no telescopes. The request had been routed from Frosch (an oceanographer) to Hinners (a geologist) to Henry (an astronomer). Someone found out that Marshall Spaceflight Center, in Huntsville, Alabama, had a 7″ Questar telescope, and that furthermore, by great luck, a NASA plane was flying from Huntsville to Washington the next day (Press was emphatic that the President wanted no special flights or other waste of taxpayer dollars). I called Jeff, and later I had my wife, Dr. Rita Mahon, meet me at National Airport with my car. We loaded the large wooden crate in the trunk, and arrived at the White House about seven p.m. on Friday, November 18, 1977.

Rita and I spent about half an hour with President and Mrs. Carter, Amy Carter, and Jeff and Annette Carter, assembling the Questar and trying it out on the upper floor balcony of the south side of the White House. The night was mostly cloudy, but the moon was visible. President Carter kept the telescope for about a week, and then Bland Norris and I retrieved it from Jeff, who said that his father had made good use of it at Camp David.

UFOs

The reader now has some understanding of the environment at NASA Headquarters at the time that Frosch's letter of July 21, 1977 (Appendix 1), was received. Action (see Appendix 1) was assigned to Code F, but I don't recall a Code F, and my August 1978 Headquarters telephone book does not include any Code F. On July 29, Herbert J. Rowe, Associate Administrator for External Affairs (Code L), sent a note, confirming a meeting to be held August 3, 1977, 3:30–4:00 p.m., "to discuss the position NASA should take in regards to Dr. Press' recommendation," to the following persons: Gen. Crow, Dr. Hinners, Mr. Chapman, Mr. Newman, a person representing Joe Allen, and Dr. Henry. Dr. Joseph P. Allen, who was Director of the Office of Legislative Affairs (Code C), is the well-known astronaut ("we deliver"). Duward L. Crow was Assistant to the Deputy Administrator, Alan M.

Lovelace. Robert A. Newman was the Director of the Public Affairs Division, located in Code L. I am not certain what Chapman's position was; possibly he was Deputy to Rowe. My memory of the meeting is of desultory conversation, including General Crow saying in a bemused manner that his daughter believed implicitly that as a former Air Force General, he knew all about UFOs and was keeping it secret from the public. I believe that the only result of the meeting was that action (jargon for responsibility) was handed to Dr. Hinners, to formulate a recommendation to the Administrator.

After the meeting, I spoke in the hall with Hinners, letting him know that for many years I had been Astrophysics consultant to the Aerial Phenomena Research Organization (APRO), a private UFO research group that was located in Tucson, Arizona. My motivation was partly to let Hinners know that I had some specific relevant expertise on the subject at hand, but also to "warn" him that I was not a completely disinterested neutral party on the controversial question of UFOs.

From that point on, the only meetings on the subject that I recall attending were one or two very small (or even one-on-one) meetings in his office with David Williamson Jr. Williamson impressed me as being the most intelligent person in the building. He discoursed on UFOs to me at some length, and in an extremely knowledgeable way, and, as we shall see, he formulated for Hinners' signature the recommendation that finally went to Frosch.

In the meantime, it had leaked out to the world that Henry might be designated the NASA project officer for UFOs (in the end, no one was). For example, on September 22, 1977, Alan C. Holt, of VISIT, Inc., wrote to me "We understand that your assignment as Project Officer is forthcoming and that the 2–3 month study will begin approximately October 1." I sent the letter up to Hinners, with that sentence highlighted. Hinners replied "you sure do draw 'em in, SAD. I suspect they got word of this from out there somewhere." I received a November 7, 1977 newsletter from Stanton T. Friedman which contained the item "NASA will be taking a look at the UFO question in response to a lot of pressure on the White House which in turn pressured the Science Advisor which then pushed NASA. The scientist in charge of the inquiry is Dr. Richard Henry, Department of Astrophysics at NASA Headquarters." On November 1, 1977, someone in Senator Hatfield's office telephoned NASA, and Herbert Rowe (Code L) wrote, on November 3, to Senator Hatfield, in response ". . . NASA at this time is conducting a study of the literature for approximately the last ten years to determine whether it might be worthwhile to conduct any further investigation of UFO's at this time. A project officer has been assigned to the task of reviewing the UFO literature and he is presently organizing this task . . ." Of course, by this time Frosch and Press had had an exchange of letters (Appendices 2 and 4), and Rowe surely believed that a project officer must by now have been designated. Blind copies of Rowe's letter went to two Code L files (chronological and subject) . . . and to "SA/Henry." A type-

written note was attached: "Who is the project officer? He should be informed that a number of definitive overview documents have been (sic) by investigators at the request of the Committee on the subject of UFO's and these studies would be of help to him in compiling this information." A blue mark appeared at the disjoint point in the second sentence, and in blue the first sentence was crossed out and "Info for Dr. Henry fr Code C." inserted.

In addition to a certain number of letters from "pro"-UFO types, I had received two communications from "debunkers"—Phil Klass sent me (October 1, 1977) a copy of his book, *UFOs Explained* (Klass, 1976) marked "To Richard Henry with the hope this may shed useful light on an old controversy—And help you and NASA avoid the fate of "Tar Baby" and the late Dr. Ed Condon!" And Robert Scheaffer wrote to me on letterhead of the Committee for the Scientific Investigation of Claims of the Paranormal, ". . . be prepared to be deluged by mail from every kook and crackpot in the country, and even worse, be prepared for letters from the 'scientific' UFO investigators, who will appear reasonable and sane enough, yet are true believers in every sense of the word . . .". I replied (October 5, 1977) "I have not yet been assigned . . . you are very kind to warn me about other people who may write to me with views that differ from your own."

Of course, at this time I was attempting to think through what NASA's response to Press should be. However, Hinners had not asked me to do anything at all, much less prepare options or recommendations.

On October 20, 1977, I apparently saw for the first time Frosch's September 6 letter to Press (Appendix 2), and I immediately communicated my concern to Hinners (Appendix 8). My concern was that Frosch had promised more than he could deliver. I took the opportunity to recommend that "the NASA Project Officer chosen be given the highest U.S. security clearance, and also be provided with a letter from President Carter establishing his need to know regarding unidentified aerial phenomena." I went on to say that "If this procedure in not followed, there will be a hole as big as a barn door in any NASA "specific recommendation" that is *negative* on UFO's."

On October 21, 1977, I received a telephone call from Phil Klass, mildly enquiring whether I was indeed the project officer, and whether I had had any previous association with UFOs. I answered him frankly, and subsequently I decided to put down formally on paper for Hinners what I had previously explained to him verbally. My memo is reproduced as Appendix 9. The only part of the memo that needs clarification is item 3B; I did not literally mean "other dimensions"; this phrase is a result of having read John Keel's book, *Operation Trojan Horse* (Keel, 1970). The book impressed me as nonsense, but left me with an openness to the possibility that our present world-view is *fundamentally wrong;* it is this possibility that I intended to convey succinctly.

About this time, I must have learned of Press' concurrence on naming a project officer (Appendix 4) and I was surely expecting to either be named

project officer, or at least asked for advice as to who should be named. It appeared to me that Frosch was now *committed* to naming a project officer.

I expressed my thoughts as to what I would recommend be done, if I were named, in a draft memo for Hinners to send to Frosch, but it was never typed or submitted to Hinners for consideration, because I wasn't asked. According to the draft, Hinners would ask Henry to ask Dr. Stephen P. Maran (of NASA's Goddard Spaceflight Center, in Greenbelt, Maryland) to be the Project Officer. Maran would spend "two months full time" assembling information on "post-Condon" UFO reports, from APRO and other "pro"-UFO organizations, and obtain comment on these reports from Klass and Schaeffer. Maran would then draft a conclusion "as to whether or not further investigation of these incidents is warranted. He will not attempt to come to a specific conclusion on any one incident; that would be the goal of a full investigation. Rather, he will examine the whole pattern of incidents and ask, and suggest an answer to, the global question, is further work indicated. In the event that he feels that the answer is yes, he will sketch the nature of such an investigation, and indicate how it might come to some definite conclusion. His report will be reviewed by Dr. Henry, myself, and David Williamson, and presented to you on January 2, 1978."

At this point, I had not spoken with Maran, but the question was moot. The request from Hinners never came.

We now reach what, to my best information, is the critical point in NASA's efforts to deal with the UFO/White House situation. On October 31, 1977, Dave Williamson generated and distributed a draft memorandum, to be from Hinners to the Administrator. My copy arrived in an envelope marked "*EYES ONLY* SA/Dr. Henry." Despite the dramatics, the document, like all documents that I read at NASA, was not classified, even so much as "Confidential."

The draft memorandum is reproduced as Appendix 10, and as far as I recollect is identical to what I finally concurred in (verbally to Hinners) and that was sent by Hinners to Frosch. I will not summarize Appendix 10 here, as it needs to be read in its entirety at this point.

I thought the draft masterful. I also felt that while the draft recommended Option 2, anyone reading it would instantly grab for Option 1.

There was one thing that was wrong in the memo: the claim of lack of "tangible or physical evidence." There is in fact plenty of such evidence (for what it is worth). In the event, the Administrator's final decision, clearly based on this memo, dealt directly with that defect by stating to Press (Appendix 5) "we stand ready to respond to any bona fide physical evidence from credible sources." Frosch's letter to Press in fact combines parts of each of the two options, and was drafted by Williamson (see the last line of Appendix 5).

I had mixed feelings about the situation, before and after Frosch wrote his final letter to Press. A clear anomaly in the draft memo is the recommendation that the first phases of Option 2 be run out of Headquarters, and particularly at an extraordinarily high level (Hinners, Williamson, Chap-

man). NASA did *nothing* substantive at Headquarters itself. There is simply not sufficient manpower for Headquarters to carry out its administrative/budget/policy activity *and* projects as well (although of course Williamson's title was Assistant Administrator for "Special Projects").

This fact was rapidly brought into focus by a letter (Appendix 11) from Stanford University astrophysicist Peter A. Sturrock to Frosch, immediately following public release of Frosch's final letter to Press. Sturrock wanted to know, in effect, where to send the tangible evidence. The problem that this presented to Headquarters was nicely summarized in a memorandum by O. B. Lloyd, Jr. (Chief, Public Services Branch, Code L; Appendix 12). I received this memorandum with a copy of a "buck slip" from Bob Newman to Ken Chapman reading "Bill raises some good points here. Comments?" Chapman replied on the same form, "the original science problem was worked by Naugle/Hinners—I suggest we ask them for a position on handling any evidence. There are now two letters in suggesting or offering material evidence. Send a note to Naugle/Hinners asking how they plan to proceed." The slip is then marked "S-1 1. Hinners," and "P-1 2. Naugle," and finally scrawled on it is "Action to SC-Henry."

This finally gave me a chance to lay out my views to Hinners in some detail, and I did so in a memorandum on January 17, 1978 (Appendix 13). I thought that (a) NASA should be active, not passive, and (b) the substantive activity should take place at a NASA Center, as with any other NASA activity. I had by now spoken briefly, on one occasion, with my friend Steve Maran at Goddard, and he had not declined the role I envisaged for a Project Scientist. As my memo makes clear, I thought he would be ideal for the job.

And this is the end of the story. There was no response from Hinners to my memo. Sturrock, I understand, pursued an attempt to have NASA analyse a sample of material believed by some to be from a UFO. My file on UFOs, marked by me (for better or for worse), "The Secret NASA UFO file," contains a letter (Appendix 14) indicating that I did a little work supporting Hinners' and Williamson's handling of the follow-up, but I certainly did not do much. The final version of Information Sheet 78-1 (Appendix 15) represents to the world NASA's official position on UFOs. I had no hand in generating it. The draft of it that I have, indicates that the information on UFO groups was provided to Code L by Williamson. There is mention in 78-1 of Frosch's offer to respond to *bona fide* physical evidence, but no suggestion as to how to go about this.

I left NASA in the fall of 1978 to resume my academic position at The Johns Hopkins University.

Conclusion

Why did NASA turn down the President of the United States on UFOs? There is only fragmentary evidence, and so no definite conclusion is possible. We can, however, look at various possibilities.

a) Inhibition by Aliens

A reason that I have maintained an interest in UFOs since graduate school is that they are a perfectly possible "unscientific" element in the world. By "unscientific," I mean the following. Einstein's famous dictum, "Raffiniert ist der Herrgott, aber Boschaft ist Er Nicht,"* clearly does not apply to aliens, who might be very "Boschaft" ("malicious, spiteful, mischievous, malignant, wicked") indeed. With perhaps a billion years of biotechnology behind them, they could, if so inclined, insert agents at will into our society. You might not even be aware that you are an alien agent, if you are.

No conclusion on this possibility seems possible.

b) Inhibition by the US Government

Does the US government know all about UFOs and did a carefully placed person within NASA act to deflect/discourage any NASA investigation of UFOs?

There is some evidence, although it may well be fraudulent, of a US government "cover up" on UFOs (Moseley, 1987).

The person who "shot John" on UFOs, at NASA, surely was Dave Williamson (although he clearly had aid from others, including the author). His role in doing so was not at all hidden. On Saturday, November 26, 1977, while President Carter was using the Questar at Camp David, I was in a swimming pool in Florida. I happened to glance at a newspaper vending machine, and an eerie feeling of unreality swept over me. Staring at me was Dave Williamson, in a front-page photograph.

The notion that Williamson, or someone else at NASA, knowing that UFOs do include clear evidence for alien intelligence, deflected the inquiry to protect this government-held secret, can, I think, probably be rejected, simply because if it were true, why would the President or his science advisor have made the request in the first place? Of course, one could still imagine a "John Poindexter"-type isolation of the President, but this seems unlikely.

c) Belief by NASA That UFOs Are Nonsense

All I can say regarding this possibility is that I myself do not think that UFOs are nonsense, and no one at NASA Headquarters ever expressed such views in my presence. The general attitude seemed to me to be what I might call "repressed open mindedness."

d) Fear of Ridicule

I felt this myself, and expressed it to Hinners (Appendix 9).

NASA Headquarters scientists and administrators had no fear of the scientific community. As no man is a hero to his valet, so no Nobel Prize

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* "The Lord God is subtle, but He is not malicious."

winner is a hero to his grant administrator. But the negative reputation of UFO studies clearly had its effect on NASA.

e) More Important Things To Do

I have a note, dated November 29, 1977, from "Noel" [Hinners] to "Dave" [Williamson] reading "A sampler! It's obvious that going route of Option 1 will buy us neg. kudos but let's just be prepared. An Option 1 decision based on *no* look is fraught with the difficulties we've been discussing."

The Option 1 that is mentioned is presumably that in the Williamson draft memorandum (Appendix 10).

This suggests that Hinners favored Option 1. Just as in the case of Hinners' attitude on SETI, I speculate that his fundamental motivation was a desire not to cloud his extremely effective NASA space science program with activity that might detract in some way. I felt the same way. Perhaps in this affair, having a positive attitude to UFO study, I should have taken a more aggressive stance than I did, but I was worried about having to pay for it: If you are the one who wants it, generally you are the one who pays for it, and I considered it wrong to spend astrophysics funds on UFO study, despite feeling that UFO study was a worthy use of public funds. Also, I did not feel that a Goddard project was very likely to produce a more substantial result than did Condon's Colorado project. The only real defect in the Colorado project was in the Director's inaccurate summary, not the substance of the investigation itself. If the UFO phenomenon includes an element that is due to the presence of an alien intelligence in our vicinity, it was doubtful to me that that fact could ever be established by a "Blue Book" or "Colorado" or my proposed "Goddard" and its follow-up, that is, by incremental investigation and accumulation of cases of varying degrees of credibility. Carl Sagan has said that extraordinary claims require extraordinary proof, and he is right. Extraordinary proof of the "reality" of UFOs cannot easily come from us; it must surely come from the UFOs, if it will.

Postscript

The manuscript of this paper was sent to President Carter, Frank Press, David Williamson, and Noel Hinners for comment. Williamson, responding for himself and Hinners, made clear that the NASA program of analysis of hard evidence was considerably more extensive than I had realized: "We entertained a great number of inquiries and ran a number of analyses . . . we developed a simple procedure for anyone's getting a suspect sample to NASA (with a quitclaim so we could cut, drill holes, and so on) . . . I am glad we had the courage to do the right thing for the right reason." Press responded but had no comment to make. Carter returned my letter and marked it "I don't have any comment, except below"; and below, beside my sentence "The most important point that you could clarify, if you will, is

whether you yourself were behind Frank Press' letter of July 21, 1977, to NASA," is the word "no."

Author's Note. Photo reproduction (rather than typesetting) has been used for the Appendices, in order to leave clear and apparent all of the tracking notes and approvals that are on the original documents. Some price is paid, of course, in terms of legibility.

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Appendix 1

July 21, 1977, Letter From Dr. Frank Press to Dr. Robert Frosch

EXECUTIVE OFFICE OF THE PRESIDENT
OFFICE OF SCIENCE AND TECHNOLOGY POLICY
WASHINGTON, D.C. 20500

July 21, 1977

Dear Bob:

We have discovered that the White House is becoming the focal point for an increasing number of inquiries concerning UFO's. As you know, there appears to be a national revival of interest in the matter with a younger generation becoming involved. Those of us in the Executive Office are ill-equipped to handle these kinds of inquiries.

It seems to me that the focal point for the UFO question ought to be in NASA. I recommend two things: since it has been nearly a decade since the Condan report, I believe that a small panel of inquiry could be formed to see if there are any new significant findings. Since this is a public relations problem as much as anything else, people who are known to be interested in the problem and also highly known, such as Carl Sagan, ought to be involved. This is a panel of inquiry that could be formed by NASA.

The second thing I would like to suggest is that NASA become the focal point for general correspondence and that those inquiries which come to the White House be sent to the designated desk at NASA.

Yours sincerely,

Frank

Frank Press
Director

Robert Frosch
Administrator
National Aeronautics and
Space Administration
Washington, D.C. 20546

Action Copy to F
Info Copy to A, ADA, AA, AK, L --
A34611
Rec'd in NASA 7-25-77
Suspense Date 8-8-77
Prepare Reply for Signature of A

Appendix 2

September 6, 1977, Letter From Dr. Robert Frosch to Dr. Frank Press

Ly: SA/Dr. Henry

SEP 6 1977

Honorable Frank Press
Director
Office of Science and Technology Policy
Executive Office of the President
Washington, DC 20500

Dear Frank:

In your letter of July 21 you expressed the view that NASA should be the focal point for UFO matters, and specifically recommended that a panel of inquiry be formed by NASA to see if there are any significant new findings since the Condon report and that NASA become the focal point for general correspondence and inquiries.

While we are inclined to agree with your recommendation, there are a number of questions which need to be resolved before any formal program is undertaken. You may know that the Air Force served as the focal point for UFO matters during the 1960's and devoted considerable resources to the program. It, however, concluded, in the absence of significant findings, that the program warranted no more than routine form letter answers to inquiries and has been handling the program in that manner since about 1970. It now handles a small number of inquiries, perhaps 10 to 12 monthly. NASA, likewise, handles routine inquiries by form letter response, 10 to 12 formal inquiries and a somewhat larger number of public inquiries monthly. NASA uses the information sheet attached in its responses. The Air Force uses similar data.

From the point of view of the Administration as a whole, this is economical. However, it fails to provide a recognized focal point for technical appraisal of sightings and understandably results in some frustration to individuals making what they consider to be serious inquiries.

A panel of inquiry such as you suggest might possibly discover new significant findings. It would certainly generate current

interest and could lead to the designation of NASA as the focal point for UFO matters. It would require some additional resources for the inquiry and for follow-on activity. Before committing to this, I feel that we should assure ourselves that an inquiry is justified. I believe we could do this by naming a NASA project officer to review reports of the last ten years and to provide a specific recommendation relative to any further inquiry by the end of this year. If you concur, I will initiate this action.

Very truly yours,

Original Signed By
Robert A. Frosch

Robert A. Frosch
Administrator

Enclosure

cc: AA
AC
ADA
S
F
L
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C
AE

F/RA Newman:elt:8/18/77 A-34611
Rewritten:ADA/L:Rowe/Crow:8/25/77
Rewritten:ADA/Crow:aom:9/1/77

Appendix 3

NASA Information Sheet 76-6, "Unidentified Flying Objects"

National Aeronautics and Space Administration

INFORMATION SHEET
FGM 76-6

Prepared by

FGM/Office of Public Affairs
NASA Headquarters
Washington, D.C. 20546

UNIDENTIFIED FLYING OBJECTS

NASA is not involved in research concerning unidentified flying objects. Reports of unidentified objects entering U.S. air space are of interest to the U.S. military as a regular part of defense surveillance, but no government agency is conducting an ongoing investigation of UFOs at this time.

An extensive study known as Project Blue Book was undertaken in the 1960's by the U.S. Air Force through a contract with the University of Colorado. Based on the findings of this study as reviewed by the National Academy of Sciences, the Air Force terminated the project December 17, 1969.

The University of Colorado report, entitled Scientific Study of Unidentified Flying Objects, was published in paperback by Bantam Books. A three-volume photoduplication (AD 680:975-6-7) may be purchased for $18 from the National Technical Information Service, U.S. Department of Commerce, Springfield, Virginia 22151. The complete reports were transferred from Air Force storage in July 1976 to The National Archives, 8th Street and Pennsylvania Avenue, N.W., Washington, DC 20408. Those wishing to view the report must obtain a researcher's permit from the National Archives and Records Service.

The University of Colorado study reached the following conclusions: (1) there was no evidence that the subject of UFOs had been "shrouded in official security"; (2) UFOs did not constitute any hazard to national security; (3) two decades of UFO study had made no significant contribution to scientific knowledge; and (4) further extensive study of the general topic could not be expected to contribute meaningfully to the advancement of science.

The panel of the National Academy of Sciences agreed with these conclusions and further commented, "On the basis of present knowledge the least likely explanation of UFOs is the hypothesis of extraterrestrial visitations by intelligent beings."

Although the U.S. government no longer dedicates funds and personnel to the study of UFOs, investigations are continuing under private auspices. The Center for UFO Studies, P.O. Box 11, Northfield, Illinois 60093 (telephone 312/491-1870) is a source for publications and information on UFO phenomena. The National Investigating Committee on Aerial Phenomena, Suite 23, 3535 University Boulevard, Kensington, Maryland 20795 (telephone 301/949-1267) also replies to requests for general information. Both organizations investigate reported sightings of unidentified flying objects.

July 1976

Appendix 4

September 14, 1977, Letter From Dr. Frank Press to Dr. Robert Frosch

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EXECUTIVE OFFICE OF THE PRESIDENT
OFFICE OF SCIENCE AND TECHNOLOGY POLICY
WASHINGTON, D.C. 20500

September 14, 1977

Dear Bob:

I have your letter of September 6 responding to my letter of July 21 recommending that NASA become a focal point for Federal activity in UFO matters.

I am pleased that you agree that NASA can handle the public inquiries on UFOs. The fact that my Office and the White House can direct such inquiries to NASA will relieve my small staff of a responsibility we are not equipped to handle. Therefore, I have asked my assistant, Stan Schneider, who has discussed this matter with your Executive Officer, Ed Andrews, to forward all our UFO inquiries to NASA.

Regarding the recommendation for NASA to become a focal point for the scientific and technical appraisal of the UFO phenomenon, I can understand your reluctance to commit the agency to a formal program before evaluating the current status and recent history of UFO activity and determining what might be involved in conducting a serious study on this matter. Therefore, I concur with your idea of assigning a project officer at NASA to review the situation before deciding whether to undertake a more formal inquiry.

By copy of this letter, I am informing Jim Purks of the White House Media Liaison Office of our exchange of ideas on this subject so that they are in the communications loop on this situation. I will suggest that he forward all public inquiries on UFOs to the White House to NASA (Code 4) for response.

I would appreciate it if NASA could keep my office, through Stan Schneider, informed of any progress the agency makes toward a decision on a possible UFO study.

Yours sincerely,

Frank Press
Director

Honorable Robert A. Frosch
Administrator
National Aeronautics and
Space Administration
Washington, D.C. 20546

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A5461
Rec'd in NASA 9-19-77
Response Date NONE
Response Reply for —
Signature of
DAA 9-21-77

Appendix 5

December 21, 1977, Letter From Dr. Robert Frosch to Dr. Frank Press

December 21, 1977

Honorable Frank Press
Director
Office of Science and Technology
Policy
Executive Office of the President
Washington, DC 20500

Dear Frank:

In response to your letter of September 14, 1977, regarding NASA's possible role in UFO matters, we are fully prepared at this time to continue responding to public inquiries along the same lines as we have in the past. If some new element of hard evidence is brought to our attention, in the future, it would be entirely appropriate for a NASA laboratory to analyze and report upon an otherwise unexplained organic or inorganic sample; we stand ready to respond to any bona fide physical evidence from credible sources. We intend to leave the door clearly open for such a possibility.

We have given considerable thought to the question of what else the United States might and should do in the area of UFO research. There is an absence of tangible or physical evidence available for thorough laboratory analysis. And because of the absence of such evidence, we have not been able to devise a sound scientific procedure for investigating these phenomena. To proceed on a research task without a disciplinary framework and an exploratory technique in mind would be wasteful and probably unproductive. I do not feel that we could mount a research effort without a better starting point than we have been able to identify thus far. I would therefore propose that NASA take no steps to establish a research activity in this area or to convene a symposium on this subject.

I wish in no way to indicate that NASA has come to any conclusion about these phenomena as such; institutionally, we retain an open mind, a keen sense of scientific curiosity, and a willingness to analyze technical problems within our competence.

Very truly yours,

Robert A. Frosch
Administrator

bcc: A, AD, S-1, L-1, AX, NHS-23
LF/Waggoner, NHS/Lichty

AX-1/D.Williamson,Jr.:djs:12-20-77

Appendix 6

August 17, 1977, Letter From Dr. Richard Henry to Major Ret. Colman S. Von Keviczky

AUG 17 1977

SA(RCH:jb)

Major Ret. Colman S. VonKevicsky, MMSE
Director of ICUFON
35-40 75 Street, Suite 4G
Jackson Heights, NY 11372

Dear Major VonKeviczky:

Dr. Ichtiaque Rasool has asked me to reply to your letter of August 9, 1977.

NASA's Office of Space Science is indeed considering, at the present time, whether to go ahead with a radio search for intelligent extraterrestrial signals.

If there were clear evidence that extraterrestrials were presently in the vicinity of the earth, we would certainly cast our investigation in that direction instead. However, such a clear evidence does not exist. Instead, what exists is a baffling collection of intriguing anecdotal evidence for mysterious phenomena, usually referred to as "UFO's." I have personally followed the UFO phenomenon for many years, as Astrophysics consultant to a major UFO investigation group. I have been disappointed, as the years have gone by, that nothing substantial has emerged from the intensive research efforts of several very competent independent research group. This does not mean that the phenomenon is not real, but it does mean that extracting verifiable information from it is a formidable problem.

The Office of Space Science is charged with exploring the space environment of the earth, and studying the universe. We place first priority on straightforward scientific investigations of the cosmos. Even a radio search for intelligent signals is considered very speculative, and I am sure the we will have our work cut out for us in selling the concept.

I fully recognize that the <u>possibility</u> exists that we are taking the wrong approach. It is a matter of management judgement. I am personally convinced that the radio search is a very worthwhile undertaking.

Yours sincerely,

(original very faint; re-typed May 1988)

Richard C. Henry

Appendix 7

August 9, 1977, Letter From Major Ret. Colman S. Von Keviczky to Dr. Ichtiaque Rasool

• ICUFON •

INTERCONTINENTAL U.F.O. GALACTIC SPACECRAFT - RESEARCH AND ANALYTIC NETWORK®

DIR. OF PROJECT: COLMAN VONKEVICZKY, MMSE. MEMBER OF THE AMERICAN INSTITUTE OF AERONAUTICS AND ASTRONAUTICS (A.I.A.A.)

OFFICE OF THE
DIRECTOR OF PROJECT

HEADQUARTERS:
35-40 75TH STREET, SUITE: 4G
JACKSON HEIGHTS, N. Y. 11372
TEL: (212) 672-7948 U.S.A.

August 9, 1977

Dr. Ichtiaque Rasool, Chief Scientist
NASA Office of Space Science
Washington D.C., 20546

Dear Dr. Rasool:

It is my obligation to inform you about our memorandum addressed to the United Nations Secretary General and the member nations' Permanent Representatives regarding the taped messages which will be launched by the Voyager I and II sounds to contact possible extraterrestrial intelligence within the solar system and beyond.

I am sorry to express our firm belief resulted of our 25 years of military, scientific and technological research and their evidences, that the time urge to the NASA's scientific community to change their views upon the project SETI and seek rather communication with the exploring galactic forces and their operation authorities, than wasting time and $ billions to search ETI in the depth of the Universe.

Existence of Galactic Powers and their earthbound operation has been officially verified from the year of 1947 by the highest responsible authorities of the US national defense and security: as the Presidents and their Chiefs of Staff, Disposals for armed and retorting confrontation by the strategic defense global emergency are still in effect up to date, which should constitute also a logical explanation of the radio astronomy contact's fiascos,- why are we ignored by the ETI.

In deliberation, that the UFO problem is above all an international security problem, your orthodox scientists should pay serious attention that their wilfull negligence and further habitual polemy on the UFOs, in case of a fatal impact - which is a step from open hostilites - could easily lead not to a "Scientific Watergate" but to a "Nuremberg Trial". Namely the crime against the peace and humanity is qualified as a "supreme war crime" in the Charter of the United Nations, adopted by the General Assembly on February 14, 1946.

Your kind attention to the enclosures would be gratefully appreciated.

Yours respectfully,

Major Ret. Colman S. VonKeviczky, MMSE
Director of ICUFON

Enclosures.

EUROPEAN CONTINENTAL HQ.: DUIST.e.V., KARL L. VEIT Pres., 62 WIESBADEN-SCHIERSTEIN, POSTFACH: 17185, WEST GERMANY.

Appendix 8

October 20, 1977, Memorandum, Dr. Richard Henry to Dr. Noel Hinners

NASA

National Aeronautics and
Space Administration

Washington. D.C.
20546

Reply to Attn of SA (RCH:abw)

OCT 20 1977

MEMORANDUM

TO: S/Associate Administrator for Space Science

FROM: SAD/Deputy Director of Astrophysics Programs

SUBJECT: UFO's

I have now seen A's letter of September 6, 1977, to Frank Press, on the subject of UFO's, and I am a bit concerned on a few points. Frosch has agreed to ". . . name a NASA Project Officer to review reports of the last ten years and to provide a specific recommendation relative to any further inquiry by the end of this year." My concern is that the volume of reports for the last ten years is far beyond what even a moderately, well-staffed project at a NASA center could possibly reevaluate between now and the end of the year. For NASA to make a "specific recommendation" on the basis of what could actually be accomplished in that period of time would open NASA to a valid charge of either whitewash or idiocy (depending on which way the recommendation went).

I have a second concern. There is belief among some Americans that the government knows all about UFO's, but that it is all highly classified. I recommend that the NASA Project Officer chosen be given the highest U.S. security clearance, and also be provided with a letter from President Carter establishing his "need to know" regarding unidentified aerial phenomena. If this procedure is not followed, there will be a hole as big as a barn door in any NASA "specific recommendation" that is negative on UFO's.

R C Henry

Richard C. Henry

Appendix 9

October 21, 1977, Memorandum From Dr. Richard C. Henry to Dr. Noel Hinners

NASA

National Aeronautics and
Space Administration

Washington, D.C.
20546

October 21, 1977

Reply to Attn of SAD(RCH:ap)

MEMORANDUM

TO: S/Associate Administrator for Space Science

FROM: SAD/Deputy Director, Astrophysics Programs

SUBJECT: My Previous Experience in the Study of UFO's

Some weeks ago I was invited to attend, with you, a meeting in Herb Rowe's office to discuss a letter that Dr. Frosch had received from Dr. Frank Press on UFO's. I did not solicit that invitation. Immediately after the meeting, I informed you verbally that I had an interest of long standing in UFO's, and that I was consultant in astrophysics to a leading "amateur" UFO organization. I explained these facts also to my immediate supervisor, Bland Norris.

Yesterday I received a call from Phil Klass of Aviation Week. He asked if I were in charge of UFO's for NASA, and I said that I had not been selected for the task, but that I might well be. He asked if I had any previous association with UFO's, and I detailed it. Klass is the author of "UFO's Explained".

I would like to make explicitly clear to you what my involvement with UFO's has been, and what my views on the subject are:

1. I have been a member of the Aerial Phenomena Research Organization (APRO) for more than ten years, and their consultant on astrophysics for perhaps eight years. APRO is run by Coral and Jim Lorenzen, in Tucson, Arizona. Membership in APRO does not involve acceptance of any particular views on the nature of UFO's, but Coral and Jim most emphatically believe that visitors from other worlds are involved!

As astrophysics consultant, I have performed one task for APRO. I analyzed a supposed "star map" that had been received by radio in some mysterious way. I showed that the "map" was not a map of the region of sky claimed. This was published in the APRO bulletin.

On another occasion, I became suspicious of a sighting reported in the APRO bulletin and showed that the sighting was almost certainly Venus. I wrote to Coral and she published my finding.

2. For the past several years, I have been a member of Alan Hynek's "invisible college" - qualified scientists who feel that the UFO phenomenon deserves attention. Hynek himself is the former Air Force consultant on UFO's. He was a great scoffer, but in recent years he has come to take UFO's very seriously. I have performed no tasks for Alan.

3. My views on UFO's are:

   A. The UFO-report phenomenon exists, is widespread, and is of great interest to a large segment of the American people.

   B. I see no à priori reason why some of the UFO reports could not be due to sightings of visitors from other worlds or other dimensions.

   C. I see no overwhelming indication that any UFO report is due to "extraterrestrials".

   D. I confess to occasionally feeling, about UFO's, like the small boy who on Christmas morning found only a pile of horse manure under the tree. Undeterred, he cheerfully dug away, reasoning that there had to be a pony somewhere!

   E. I feel that the Condon investigation did not adequately deal with the UFO phenomenon, and that further government investigation is warranted.

4. In previous "impartial" investigations it has been deemed essential to have, as a leader, a person who has had no significant previous interest or experience in UFO's. The result, in my view, has been

very unsatisfactory: total immersion in UFO's rapidly produces in such people a polarization of opinion one way or the other that compromises the integrity of the investigation. Despite this, the alternative is perhaps even more unsatisfactory.

5. My view 3E above indicates that I already have an opinion on the particular subject that NASA has been asked to investigate.

6. UFO's are (as Phil Klass indicates, in a note to me in the copy of his book, which he kindly sent me) a "tar baby". A scientist who touches the tar baby once, as I have, runs the risk of getting deeper and deeper in goo. I don't have a strong stomach for it, and would prefer to avoid it. But, I also want to make sure that NASA itself does not get badly tarred.

R. C. Henry

Richard C. Henry

Appendix 10

October 31, 1977, Draft Memorandum (by D. Williamson) From Dr. Noel Hinners to Dr. Robert Frosch

D R A F T
X:DWilliamson,Jr.
10-31-77

MEMORANDUM

TO: A/Administrator

FROM: S/Associate Administrator for Space Science

SUBJECT: UFO Study Considerations

Following the recent exchange of correspondence with Frank Press, I have been giving the UFO matter some thought, especially the question of what NASA could reasonably do in both the short and long term.

The environment since the 1969 Condon report seems to have changed:

- o There is a widespread interest in UFO's (and in related paranormal phenomena) that cannot be dismissed lightly as involving only a fringe of the population; probably 50% of the United States believes that "something" in the way of persistent phenomena exists or has existed.

- o The UN is currently considering a resolution to establish a specialized agency for UFO matters.

- o In France, the CNES has been formally charged with setting up a UFO study activity under Claude Poher.

- o There are many apparently viable private organizations in the United States with responsible memberships that are following the UFO phenomena from several different view points.

- o There seems to have been an increase in reports of the "near encounter" type (Pascagoula, Miss., 1973; Prospect, Ky., 1977) over the last few years.

- o There is a general feeling among the UFO organizations at least that the United States Government "knows" far more than it has released, and may even have pieces of UFO hardware in hand.

Open-mindedness about UFO and paranormal phenomena seems to be becoming more "respectable" in the general public. Books and articles flourish. The IEEE is often a forum for matters that would have been rejected out of hand ten years ago. Classified and unclassified research supported by Federal agencies has brushed the UFO community (proponents of "remote viewing" often cross-couple with UFO proponents). At the same time, there are vocal "debunking" groups active on the other side of the issue; the matter seems rather polarized in modern society.

There are two major problems involved in considering any review of the UFO phenomenon by NASA: first, an apparent lack of any tangible or physical evidence available for laboratory analysis; second, the absence of any sound scientific protocol for investigating the phenomenon first-hand. There is a plethora of secondary source material -- human observation and reports thereon -- but hearsay is difficult to deal with scientifically. There are, of course, other problems as well: the probability of hoaxes, the tendency for any investigator to pre-judge, the delicate interface of the Government with any private individual reporting an incident, and the danger of projecting an inaccurate NASA or Administration image. All in all, undertaking a formal study at this time appears fraught with perils.

It appears that NASA has two immediate choices, each with its follow-on implications:

1. We could, on the basis of the situation outlined above and without taking further action, recommend to OSTP that we see no responsible way at this time for the Federal government, and especially NASA, to investigate the UFO phenomenon.

   a. This approach might encourage the vocal pro-UFO groups to continue their charges of cover-up and bureaucratic blindness.

   b. It might avoid fomenting controversy and division within the science community NASA deals with.

c. It would require no change in our current PIO responses to the public.

d. It would divert no resources from those higher priority applications, science, and technology activities which are our legislated charges.

e. It would also be begging the question.

2. We could make a formal request, from my office or Ken Chapman's, to the largest and best-known of the UFO organizations (APRO, NICAP, MUFON, CUFOS, etc.) requesting them to submit their "best" cases to aid us in determining the Government's possible role. We could then compile this material into a usable format, do some first-order checks (probably involving some interviews and data-gathering), and, before drawing our own conclusions, ask for a "peer review"--possibly by the Smithsonian Institution. NASA would then make its own assessment as to whether further research were warranted or not, and if so, in what direction it should proceed. As a minimum, having gone this far and this publicly, NASA should stand ready to investigate new hard evidence that might come in -- this could logically be an added assignment for ARC and MSFC, depending on the physical or biological character of the evidence.

a. This approach commits NASA and the Administration publicly to at least <u>some</u> review of the phenomenon; an eventual negative decision will not satisfy the enthusiasts and a positive one will enrage the non-enthusiasts.

b. It will encourage a great deal of correspondence on both sides of the question; it may lead to a rash of sightings, hoaxes, and/or public excitement.

c. It will place severe demands on the few NASA people involved in the first phase: there will be problems of workload, peer pressure, and pre-judgment.

d. If any follow-through becomes necessary, the resources needed could be quite large -- travel, tests, interviews, and reports.

I recommend that: we proceed with the first phases of Option 2, under a Headquarters team of myself, Chapman, and Williamson; that we take time to tap the private organizations properly and not establish an arbitrary deadline; and that we consider further actions early next year.

Noel W. Hinners

Appendix 11

December 30, 1977, Letter From Dr. Peter Sturrock to Dr. Noel Hinners

INSTITUTE FOR PLASMA RESEARCH
STANFORD UNIVERSITY
VIA CRESPI, STANFORD, CALIFORNIA 94305

December 30, 1977

Dr. Robert A. Frosch
Administrator
National Aeronautics and Space
Administration
Washington, D.C. 20546

Dear Dr. Frosch:

Thank you for your kind letter dated December 22. I have subsequently learned from news articles that you have decided that NASA should not undertake an investigation of the UFO problem. I understand from your letter tha a key reason for this decision is the difficulty of conducting a scientific investigation "where the criteria of reproducible or recurrent observations are not available."

The news reports have quoted you as stating that "if some new element of hard evidence is brought to our attention in the future, it would be entirely appropriate for a NASA laboratory to analyze and report upon an otherwise unexplained organic or inorganic sample." As I mentioned in my letter dated December 2, my colleagues and I in the Study Group on Anomalous Phenomena have obtained access to some physical evidence such as films, material samples, etc. The cooperation of NASA laboratories would be most helpful in obtaining meaningful assessments of these items of evidence.

For this reason, I would greatly appreciate your advising me whether, in line with your quoted statement, I may seek photographic, chemical and metallurgical analyses of such samples from NASA laboratories.

Sincerely yours,

P.A. Sturrock
Professor of Space Science
and Astrophysics

PAS/gc

[illegible] Copy to LF
[illegible] Copy to A, ADB, AY, L
A1460
[illegible] NASA 78-01-03
[illegible] Date 78-01-17
[illegible] for A

Appendix 12

January 6, 1978, Memorandum From O. B. Lloyd, Jr., to LF-6/Director of Public Affairs

NASA
National Aeronautics and
Space Administration

Washington, D.C.
20546

Ken Chapman -
Bill Taiz2 Bone
good points here.
Comments?
Bob Newman
1/9

Reply to Attn of: LFF-3 January 6, 1978

MEMORANDUM

TO: LF-6/Director of Public Affairs

FROM: LFF-3/Chief, Public Services Branch

SUBJECT: Procedure for Receiving Alleged UFO-related "Physical Evidence" for Analysis by NASA

The attached letter from Professor P. A. Sturrock seeking NASA analysis of certain physical evidence concerning anomalous phenomena is probably a prelude to similar communications. Should it be the only such letter, NASA still needs a procedure for receiving, documenting, processing and safegaurding the materials.

It would seem appropriate that before any such material would be received by NASA the sender be required to advise NASA of certain specifics, such as:

- the nature of the evidence
- is this all of the evidence or is this a portion of a larger amount known to exist?
- what is the size, weight of the materials?
- liability - will the government be expected to return the materials in the precise condition they are received?
- what about loss through testing, evaporation or other processes?
- if acccepted by the government, would NASA be expected to provide security (such as is now required for lunar samples)?

In the interest of security and documentation it would appear that one point should be designated to receive all evidence. Further, a person with technical expertise should be responsible for:

- o application of policies and procedures, as established by NASA headquarters, in processing evidence at NASA centers
- o selection of the appropriate testing facility or facilities
- o transportation of the evidence from the receiving point to the appropriate NASA facility
- o compiling and forwarding of findings resulting from the analysis
- o return to the sender, or such other disposition as may be determined, of the evidence.

Since the letter to Dr. Press from the Administrator invites submission of bona fide physical UFO evidence, NASA would cppear obligated to proceed toward ultimate acceptance of the materials offered by Dr. Sturrock. I would propose he be sent an interim letter outlining the preparatory actions noted above, assure him of the agency's interest in his offer and request such detailed information as noted above.

Meantime, I would suggest bringing together to agree on a procedure representation embracing all aspects of the activity, including scientific, legal, security and Public Affairs. In expectation that there may be submissions from foreign as well as domestic sources, representation should probably be included from International Affairs.

Finally, NASA liaison with other branches of the government should be kept apprised in event there is a development of importance.

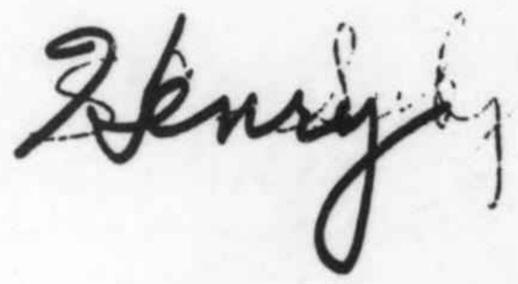
If you concur in this general approach, I will prepare an interim response to Professor Sturrock.

O. B. Lloyd, Jr.

Att.

Appendix 13

January 17, 1978, Memorandum From Dr. Richard Henry to Dr. Noel Hinners



JAN 17 1978

SC(RCH:jb)

MEMORANDUM

TO: S-1/Associate Administrator for Space Science

FROM: SC-7/Deputy Director of Astrophysics Division

SUBJECT: UFO Matters

I write this memo in response to your request, of 17 January 1978, that I provide you with a suggested response to Bob Newman's request for suggestions on how to deal with the issues raised by Peter Sturrock's letter on UFO "hard evidence."

Let me move back a few steps and review the whole NASA UFO situation.

Some time ago I gave you by telephone my concurrence on the draft memo that Dave Williamson provided you to use in advising the Administrator concerning a response to Frank Press' request. I have just re-read that draft, and I still think that it is a fine memo. There is only one point in it that I would now (too late!) question, and that is the statement that there is an "absence of any sound scientific protocol for investigating the (UFO) phenomenon first hand." The National Academy of Sciences endorsed the Condon study of UFO's, and specifically endorsed their procedures (protocol). It hardly does for us to say that no sound protocol is possible! I do agree with Dave that a protocol is extraordinarily difficult. The point is, that to be meaningful the protocol must cover the possibility that the UFO phenomenon is due in part to intelligences far beyond our own. I very much doubt that an intellectually inferior species can study an intellectually far superior species if the superior species chooses not to be studied. They could run rings around us!

Be that as it may, the memo offered to Dr. Frosch two suggested options: 1) Try to duck out of it completely, or 2) Do a study. Your recommendation to Dr. Frosch was to follow option two some ways, and review matters early in 1978.

2

Dr. Frosch's letter of 21 December 1977 to Frank Press indicates that he chose, in my judgement, the worst features of each of the two options. We turned-down Frank Press before the world; we dismissed UFO's without a study (feature "e" of Option 1); yet!...We started (it seems) a NASA UFO "Hard Evidence" Analysis Program ("UFOHEAP"). Furthermore, UFOHEAP is not a program intellectually directed and given coherence by NASA officials, scientists, and technologists, but rather is a "reaction" program, controlled in key respects by whoever in the world chooses to submit what they consider "hard evidence" to NASA.

What to do?

I suggest that there are three options that are sustainable:

Option 1. Consistent Follow-through. Bill Lloyd's 6 January 1978 memo to Bob Newman covers this option very well--including many things I would not have thought of. The activity should be run out of one Center--a focal point--although the actual analysis would be done at various centers, depending on the type of analysis needed. At the chosen Center there should be one key person in charge of the operation, and he/she must be scientifically/technically sound and politically astute.

In favor of this option:

Consistency

Against this option:

It places NASA in an intolerable position. We have no UFO program and no position on UFO's as such, yet we are the Pope of UFO Evidence. Ravening hordes of bunkers and debunkess will attack every NASA "pronouncement from the chair."

Option 2. Dodge. Interpret "hard evidence" so strictly that no activity results. This would return us, at some additional cost in credibility, to the joys of Dave's original Option 1.

In favor of this option:

All the virtues of the original Option 1.

Against this option:

In addition to the defects of the original Option 1, we violate the apparent spirit of Dr. Frosch's 21 December 1977 letter to Frank Press.

Option 3. Bull-by-the-Horns.

Pretend NASA is simply following through on the 21 December letter, but actually mount a modest active (rather than passive) activity. Have NASA run UFO's, not UFO's run NASA. This would be, in effect, deciding to follow the Option 2 that you originally offered to Bob.

In favor of this option:

It faces up to a real national concern, and furthermore it does so in a much more low-key way than if NASA had directly proceeded with the original Option 2.

Against this option:

All the defects of the original Option 2. Also, there is the danger of it appearing that NASA is conducting a "secret" UFO study.

Recommendation

I recommend Option 3. My feeling is that NASA is now stuck to the tar-baby, so lets deal with it properly.

If Option 3 is chosen, there are certain key decisions to be made. My recommendations on these follow. The activity should be run by the Office of Space Science. Additional resources should be provided to you to cover this activity (of course!). Management of the activity should be assigned to the Astrophysics Division, and a Program Scientist/Manager (Frank Martin) should be assigned. The activity should be based at a single Center (GSFC), although of course technical resources of many Centers would be used. A Project Scientist should be appointed. My strong recommendation is that this should be Dr. Stephen Maran of GSFC. He is a skeptic on UFO's; he is extremely sharp and energetic; and he is politically acute.

Dr. Maran should be instructed to take a low-key but positive approach to the UFO problem. He should approach the reputable independent UFO groups (APRO, CUFOS, NICAP, MUFON) and make NASA's technical expertise directly available to them. In addition to this, he should work toward the definition of a coherent larger-scale active UFO program that would deal with the continuing phenomenon in a coherent and intellectually sophisticated manner--this has never been done (to my knowledge!).

Changes would be necessary in the draft PIO UFO material that exists.

4

The Administrator may prefer to choose Option 2 (Dodge!). I wouldn't blame him for this, but if he does, he should do it solidly and consistently. We should not be mushy on UFO's.

Dr. Richard C. Henry

Richard C. Henry

SC/RCHenry:jb:53665:1/17/78

Appendix 14

January 31, 1978, Draft Letter (by Henry) From Dr. Noel Hinners to Dr. Harley Rutledge

Dr. Henry

D R A F T
RCH:jb
1/31/78

Dr. Harley D. Rutledge
Chairman, Physics Dept.
Southeast Missouri State University
Cape Girardeau, MO 63701

Dear Dr. Rutledge:

I am replying to your letter of January 5, 1978, concerning the possibility of NASA support for your work on UFO's.

You have indicated that you are aware of NASA's position on UFO's. In his letter of December 21, 1977, to the President's Science Advisor, Dr. Frosch stated that with regard to UFO's, NASA "retains an open mind, a keen sense of scientific curiosity, and a willingness to analyze technical problems within our competence." He also indicated that NASA does not feel that a research effort could be mounted without "a better starting point than we have been able to identify thus far." Because of this, NASA is taking no steps to establish a research activity in this area. We do "stand ready to respond to any bona fide physical evidence from credible sources."

I gather from the newspaper account which you enclose that you have not yet acquired examples of the type of

2

"physical evidence" which Dr. Frosch referenced. Thus, I am not in a position to encourage you to submit a proposal.

If you have substantial "non-physical" evidence on specific sightings or encounters, I do suggest that you provide details to Allen Hynek (P. O. Box 11, Northfield, Illinois 60093, Phone 312/491-1870). He has just started a compilation of case reports for the Government of France.

Sincerely,

David Williamson, Jr.
Assistant for Special Projects

SC Henry

SC Norris

SD Stofan

S Original Signed by Noel W. Hinners FEB ? 1978
Hinners

SCH/RCHenry:jb:53665:1/31/78

Appendix 15

NASA Information Sheet 78-1, "Unidentified Flying Objects"

National Aeronautics and Space Administration

INFORMATION SHEET

Number 78-1

Prepared by:

LFF-3/Public Services Branch
Office of External Relations
NASA Headquarters
Washington, DC 20546

UNIDENTIFIED FLYING OBJECTS

The information contained here has been compiled to respond to queries on Unidentified Flying Objects directed to the White House as well as NASA.

NASA is the focal point for answering public inquiries to the White House relating to UFOs. NASA is not engaged in a research program involving these phenomena, nor is any other government agency.

BACKGROUND

In July of 1977, Dr. Frank Press, Director of Science and Technology Policy, Executive Office of the President, wrote to Dr. Robert A. Frosch, the NASA Administrator, suggesting NASA should answer all UFO-related mail and also to consider whether NASA should conduct an active research program on UFOs. In a letter dated December 21, 1977, Dr. Frosch agreed that NASA will continue to respond to UFO-related mail as it has in the past and, if a new element of hard evidence that UFOs exist is brought to NASA's attention from a credible source, NASA will analyze the unexplained organic or inorganic sample and report its findings.

Quoting from Dr. Frosch's December 21 letter: "...If some new element of hard evidence is brought to our attention in the future, it would be entirely appropriate for a NASA laboratory to analyze and report upon an otherwise unexplained organic or inorganic sample; we stand ready to respond to any bona fide physical evidence from credible sources. We intend to leave the door clearly open for such a possibility.

"We have given considerable thought to the question of what else the United States might and should do in the area of UFO research. There is an absence of tangible or physical evidence available for thorough laboratory analysis. And, because of the absence of such evidence, we have not been able to devise a sound scientific procedure for investigating these phenomena. To proceed on a research task without a sound disciplinary framework and an exploratory technique in mind would be wasteful and probably unproductive.

"I do not feel that we could mount a research effort without a better starting point than we have been able to identify thus far. I would therefore propose that NASA take no steps to establish research in this area or to convene a symposium on this subject.

"I wish in no way to indicate that NASA has come to any conclusion about these phenomena as such; institutionally, we retain an open mind, a keen sense of scientific curiosity and a willingness to analyze technical problems within our competence."

Reports of unidentified objects entering United States air space are of interest to the military as a regular part of defense surveillance. Beyond that, the U.S. Air Force no longer investigates reports of UFO sightings.

This was not always the case. On December 17, 1969, the Secretary of the Air Force announced the termination of Project Blue Book, the Air Force program for UFO investigation started in 1947.

The decision to discontinue UFO investigations, the USAF said, was based on: (1) an evaluation of a report (often called the Condon Report) prepared by the University of Colorado and entitled "Scientific Study of Unidentified Flying Objects;" (2) a review of the University of Colorado report by the National Academy of Sciences; (3) past UFO studies; and (4) Air Force experience investigating UFO reports for two decades.

As a result of these investigations and studies, and experience gained from investigating UFO reports since 1948, the conclusions of the Air Force were: (1) no UFO reported, investigated, and evaluated by the Air Force has ever given any indication of threat to our national security; (2) there has been no evidence submitted to or discovered by the Air Force that sightings categorized as "unidentified" represent technological developments or principles beyond the range of present day scientific knowledge; and (3) there has been no evidence indicating that sightings categorized as "unidentified" are extraterrestrial vehicles.

With the termination of Project Blue Book, the Air Force regulation establishing and controlling the program for investigating and analyzing UFOs was rescinded. All documentation regarding the former Blue Book investigation has been permanently transferred to the Modern Military Branch, National Archives and Records Service, 8th Street and Pennsylvania Avenue, N.W., Washington, DC 20408, and is available for public review and analysis. Those wishing to review this material may obtain a researcher's permit from the National Archives and Record Service.

Also available:

Scientific Study of Unidentified Flying Objects. Condon Report study conducted by the University of Colorado under contract F44620-76-C-0035. Three volumes, 1,465p. 68 plates. Photoduplicated hard copies of the official report may be ordered for $6 per volume, $18 the set of three, as AD 680:975, AD 680:976, and AD 680:977, from the National Technical Information Service, U.S. Department of Commerce, Springfield, VA 22151.

Review of University of Colorado Report on Unidentified Flying Objects. Review of report by a panel of the National Academy of Sciences. National Academy of Sciences, 1969, 6p. Photoduplicated hard copies may be ordered for $3 as AD 688:541 from the National Technical Information Service, U.S. Department of Commerce, Springfield, VA 22151.

NASA is aware of the many UFO reports made in recent years. However, the majority of inquiries to NASA concerning UFO sightings address themselves to the reported sightings by astronauts during Earth orbital and lunar missions and the report by President Carter while serving as Governor of Georgia.

During several space missions NASA astronauts reported phenomena not immediately explainable. However, in every instance NASA satisfied itself that what had been observed was nothing which could be termed abnormal in the space environment. The air-to-ground tapes of all manned missions are available at the Johnson Space Center, Houston, for review by the serious researcher.

On October 12, 1973, while serving as Governor of Georgia, Mr. Carter responded to inquiries from the National Investigations Committee on Aerial Phenomena (NICAP) saying that he had seen a bright, moving object in the sky over Leary, Georgia, in October of 1969. He said the object was visible for 10 to 12 minutes and, at one point, shone as brightly as the Moon. The regional NICAP representative investigated the sighting and reported there was no evidence to support anything beyond placing what Mr. Carter saw in NICAP's "unidentified" category. However, it has been suggested by some students of aerial phenomena that Mr. Carter may have viewed the Planet Venus which, at certain times, may appear many times brighter than a star of the first-mangitude.

Since NASA is not engaged in day-to-day UFO research, it does not review UFO-related articles intended for publication, evaluate UFO-type spacecraft drawings or accept accounts of UFO sightings or applications for employment in the field of aerial phenomena investigation. All such material will be returned with NASA's thanks to the sender.

A number of universities and scientific organizations have considered UFO phenomena during periodic meetings and seminars. In addition, a number of private domestic and foreign groups continue to review UFO sighting reports actively. Some of these organizations are:

(1) National Investigations Committee on
Aerial Phenomena
John L. Acuff, Director
Suite 23
3535 University Boulevard, West
Kensington, MD 20795
(301) 949-1267

(2) The Committee for the Scientific Investigation
of Claims of the Paranormal
UFO Subcommittee
Robert Sheaffer, Chairman
9805 McMillan Avenue
Silver Spring, MD 20910
(301) 589-8371

(3) Aerial Phenomena Research Organization
James and Coral Lorenzen, Directors
3910 E. Kleindale Road
Tucson, AZ 85712
(602) 793-1825

(4) Mutual UFO Network
Walter H. Andrus, Jr., Director
103 Old Towne Road
Seguin, TX 78155
(512) 379-9216

(5) The Center for UFO Studies
Dr. J. Allen Hynek, Director
924 Chicago Avenue
Evanston, IL 60202
(312) ·491-1780

February 1, 1978

Journal of Scientific Exploration, Vol. 2, No. 2, pp. 143–154, 1988
Pergamon Press plc. Printed in the USA.

0892–3310/88 $3.00+.00
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INVITED ESSAY

The Nature of Time

YERVANT TERZIAN

Cornell University, Ithaca, NY 14853

Abstract—One of the most fundamental concepts in our experience of existence is the flow of time—continuously from the past to the future. Yet, the basic nature of time as part of the description of the universe is not understood at all. The conservation laws of physics seem to be time-symmetrical, every detailed action could occur in reverse, which argues that the concept of the passage of time is not needed in nature. Yet, that time flows in one direction remains part of our experience. Can time stop? Can we influence the future? Can we influence the past? The historical and thermodynamic arrows of time are discussed and several enigmas and contradictions about the nature of time are revealed. The concept of "entropy" and its relation to the universe as a whole is explored.

The fundamental changes of our notions of a uniformly flowing time, made by Albert Einstein in his Special and General Theories of Relativity are pointed out, and several "paradoxes" and "anomalous" examples are described.

The nature of time and its relation to the Big-Bang cosmology is discussed, and the question "What was before the Big-Bang" is addressed.

Introduction

How many times have you wished to do something over again? To go back in time and flow through the past once more, to relive a pleasant moment or to change a decision and avoid a calamity. No matter how strong our wishes are, our experience shows that we can never travel back into the past.

One of the most basic notions of human consciousness is the continuous flow of time from the past to the future. This perception defines our psychological arrow of time. The passage of time, however, is not simply an illusion, it is the way that the universe manifests itself for macroscopic phenomena.

Even though everything in our experience happens with the underlying flow of time, we still do not have a basic or complete understanding of the

Acknowledgements. The author wishes to thank Tommy Gold for stimulating discussions and Carl Sagan for his comments on the manuscript. The author is associated with the National Astronomy and Ionosphere Center which is operated by Cornell University under a management agreement with the National Science Foundation.

nature of time. It is possible that a complete understanding of the nature of time can only occur when we have a satisfactory model of cosmology, of the initial conditions of the evolving universe, and of total existence.

With the spiralling progress in science and technology during this century we have come to recognize that our home in the universe does not occupy or represent any special place or circumstance.

The historical picture relates that the earth was formed about five billion years ago as a by-product while the sun gravitationally contracted from a dense interstellar cloud. This happened in one corner of the Milky Way galaxy which by that epoch had already formed over 100 billion other similar stars like the sun. The physical and chemical conditions on the earth produced what we call a biological evolution during the last few billion years of which we happen to be one of the most complex and sophisticated products.

Our powerful telescopes have shown that the observable universe is filled with millions of other galaxies similar to our Milky Way, each one containing billions of stars more or less like the sun. In this immense universe our galaxy does not occupy a special place and, like a tree in a forest, it seems lost in the crowd of other galaxies. Figure 1 shows schematically our home in the universe.

In the observable universe we have come to understand that the multitude of differences we observe are due to evolutionary changes which behave according to the same natural laws of the cosmos. We have learned, for example, that stars get birth from the gravitational collapse of interstellar clouds, then they live a long life of billions of years primarily by converting hydrogen to helium and in the process releasing large amounts of nuclear energy which keep them hot and luminous. When small stars deplete their nuclear fuel they cool off and fade away, while large ones undergo some violent explosions during the last stages of their evolution. It appears that such evolutionary processes would still take place all over the universe, defining the direction of the arrow of time, even if we did not exist.

For the giant scientists like Galileo, Newton and Maxwell, who constructed the foundations of most of modern physics using the scientific method, all events in the universe were assumed to take place in a uniformly flowing time. However, at the beginning of this century the most famous scientist of recent times, Albert Einstein, presented new theories which were able to predict nature with a much higher accuracy than with previously known physical laws. In doing so Einstein's new theories altered our understanding of the nature of time, and it was no longer necessary to assume that time was a uniformly flowing property of the universe. In fact, Einstein was able to show that the passage of time can be faster or slower depending on the presence and strengths of gravitational fields which are produced by the various masses of stars and galaxies in the universe. That is to say, time flows at different rates depending where it is measured. Einstein's new work constitutes essentially the only major and profound revision of the nature of

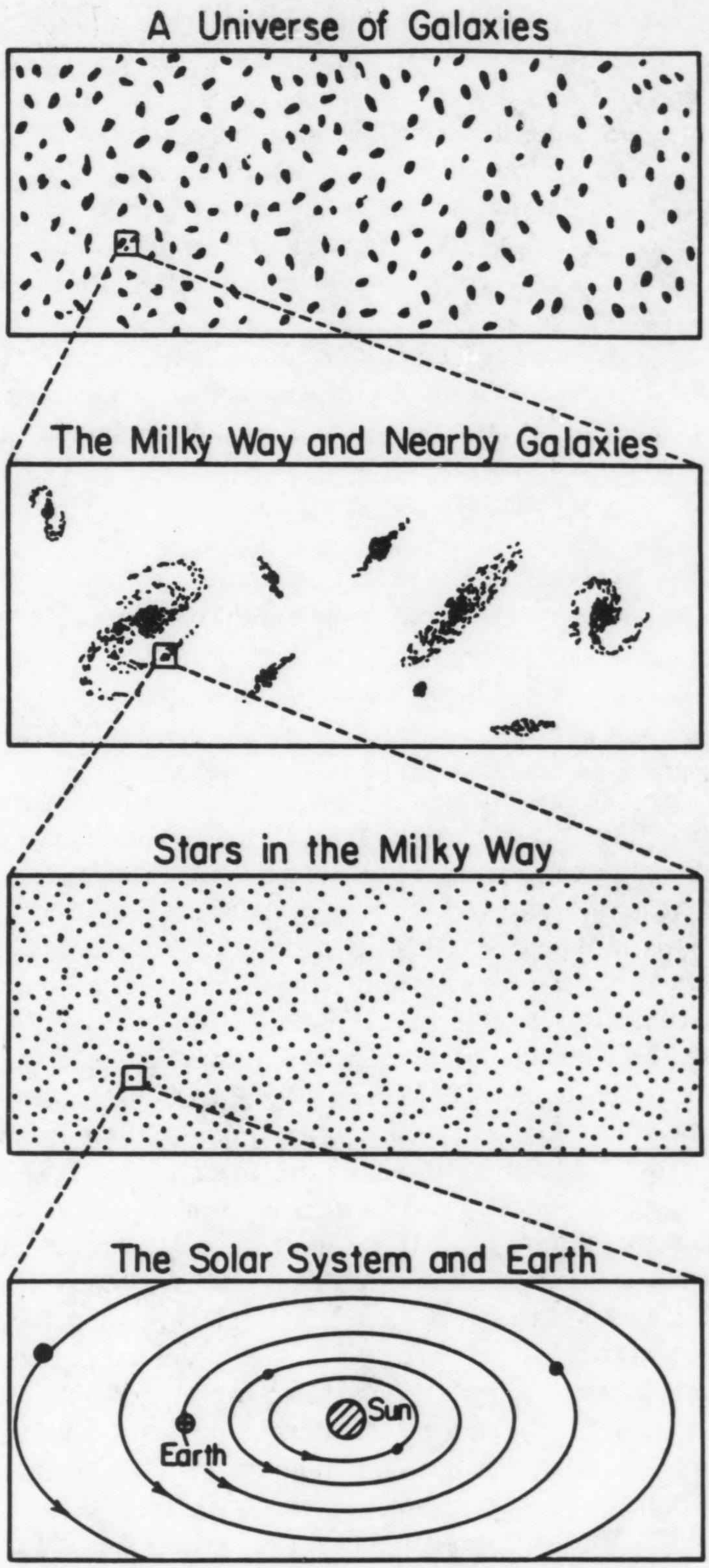

Fig. 1. A schematic representation of our home in the universe.

time in our attempts to understand the universe. Yet it is still not entirely clear what time really is and we do not know how to explain it completely. Time perhaps remains as the most mysterious concept we know and think we experience. On the other hand, we have become very sophisticated in measuring intervals of time with our exotic and accurate clocks.

Through the ages philosophers have certainly asked questions like "why does time never go backwards?" or "can time stop?". They quickly discarded such ideas since the behavior of events in our experience clearly did not permit such possibilities.

What is the difference between past and future? We think we can change and direct the future (or can we?). Can we change the past perhaps under some very special circumstances? We think we can remember the past, but not the future. However, with our scientific progress we can predict the future, and in many instances very accurately. Is this "remembering the future"?

Any possible explanation of the nature of time will most probably be interrelated with cosmology and the origin and evolution of the universe.

The Direction of Time

A. Cosmic Evolution

Modern theories of the universe suggest that the observational evidence is very strong that the universe began about 15 to 20 billion years ago from a primordial fireball explosion in a Big-Bang (Fowler, 1987). The expansion of the universe is still observed today as galaxies are seen to fly apart from each other with very high speeds. The primordial fireball was exceedingly hot, with temperatures of more than 10^{12} K. During the expansion the temperature of the universe decreased to the present very cold cosmic bath of only 3 K. This cosmic background radiation has been detected and accurately measured (Penzias & Wilson, 1965), and constitutes strong evidence in favor of the Big-Bang theory of the origin of the universe.

After the initial stages of the Big-Bang the expanding and cooling matter separated into giant clouds which formed the millions of galaxies we see (Figure 2). Galaxies in turn formed countless stars. Almost five billion years after the formation of the Milky Way galaxy, the sun was formed with its surrounding planets. On one of these minor planets, the earth, complex molecular systems were formed that led to the development of biology and life. All these processes indicate a progression from the simpler to the more complex, the generation of order or information from relative chaos or disorder. This evolutionary progression in one direction indicates one possible arrow of time. Such a time direction can be defined as the "Historical Arrow of Time," the major steps of which can be traced as follows:

(a) Evolution of Matter.
Big-Bang explosion, formation of galaxies and stars, formation of heavy elements, formation of planetary systems, and the evolution of life.

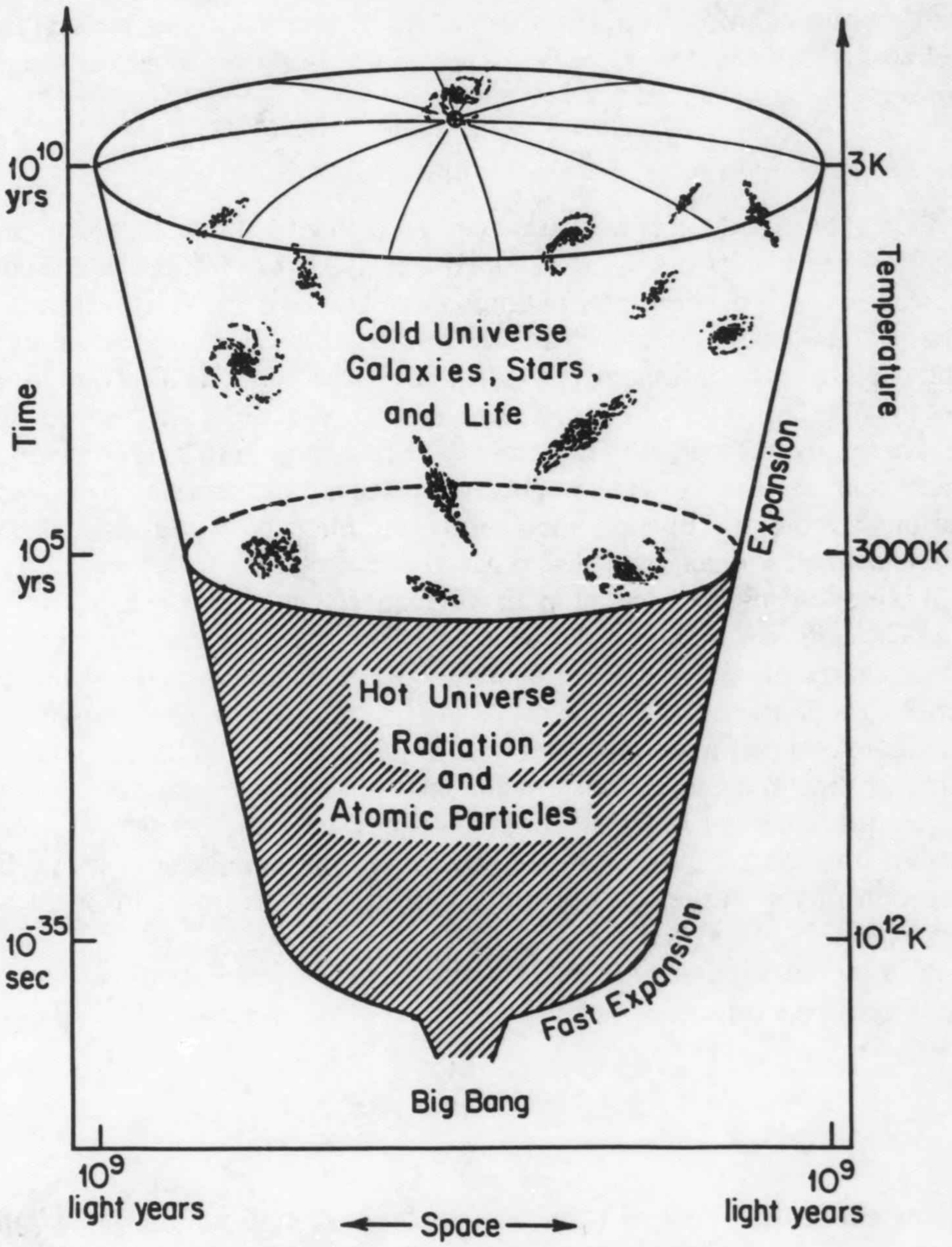

Fig. 2. The Big-Bang model of the evolving universe.

(b) Evolution of Life.
Complex organic molecules, bacteria and multicell organisms, development of fish and mammals, and humans.

(c) Evolution of Humans.
Cave humans, development of fire and language, settlements and agriculture, writing, science and technology, and the conquest of space.

In all the examples listed above, as the arrow of time is pointing to the future, simple things become more complex and matter seems to get more

and more organized, from disorder into a higher order. (As we shall see below this happens at the expense of increasing disorder in other parts of the universe.)

B. The Thermodynamic Arrow of Time

Paradoxically, the arrow of time can be defined with the opposite processes than the ones discussed in the Historical Arrow of Time, that is with processes which destroy information and create disorder. Such behavior is also well understood in nature and derives its attribute from the expansion and cooling of the entire universe. Heat flows always in one direction, from hot to cold. This is the celebrated physics principle called "The Second Law of Thermodynamics." Hot stars radiate their energy into cold interstellar space, and whereas stars are complex systems of atomic particles undergoing various atomic and nuclear interactions, the interstellar and intergalactic space is much simpler and is less organized.

It is easy to observe what happens when an ice cube is put in a cup of hot tea. The information and order which exists in the structure of the crystal which represents the ice cube is soon lost in the melting process, while the arrow of time points from the past to the future. We can also observe that the liquid of a perfume evaporates from an open bottle and the structure or order existing in the liquid gets lost and instead we have the random motion of gaseous molecules in the air. In both of these examples, order was transformed into relative disorder while the arrow of time was progressing in the direction of the future. Such examples have been described in detail by Layzer (1975).

Both the thermodynamic and historical arrows of time seem capable of defining time's direction—albeit with exactly the opposite physical processes.

Entropy

One convenient method of measuring order or information relative to disorder or chaos is the concept of entropy. Low entropy corresponds to relative order and high entropy to disorder. Within this framework, the arrow of time suggests that the historical universe progresses from high entropy to low entropy, while the thermodynamic arrow of time from low entropy to high entropy.

From the cosmological point of view, however, the expanding and cooling of the Big-Bang clearly indicate that entropy is constantly increasing, as is described by the Second Law of Thermodynamics. One is therefore forced to conclude that the Big-Bang began with low entropy or maximum order and information. As the universe expands the net order is continuously being converted into disorder or chaos—things get worse! It is important then to realize that the universe began from a smooth and orderly state. This maximum order is probably a characteristic of the required perfect sym-

metry at the moment of the Big-Bang, when all the forces of nature were combined into one.

Paradoxically soon after the Big-Bang, while the universe was still in a very hot state, matter was mostly in thermal equilibrium and atomic particles were moving chaotically in a great disorder—that is, the universe seemed to be in a high entropy state! The answer to this paradox probably lies in the unknown nature of gravitational entropy and the local space–time conditions prevailing at the time of the Big-Bang. These conditions, sometimes referred to as quantum gravity, remain very obscure and mysterious (Penrose, 1979).

It is also clear from observing the universe, and from our discussion of the historical arrow of time, that during the Big-Bang expansion, entropy fluctuations began to grow and some parts of the universe decreased in entropy and increased in order and information. The formation of galaxies, stars, planets and life are clear examples. However, it is important to realize that the net entropy of all these systems together with the rest of the universe also increased as the time arrow progressed into the future. It appears that for every ordering of things there is, on a larger scale, a greater increase of disorder. Hence, the net entropy of the universe always increases, and eventually all information will change into chaos for an ever expanding universe.

Time Asymmetry and Irreversibility

Both the historical and thermodynamic arrows of time define preferred directions in time, which seem irreversible. We have never been able to travel back into history, nor do we observe the molecules of the evaporated perfume all gather back into the perfume bottle to form a liquid, nor does the ice cube in the cup of hot tea get colder and the tea hotter by extracting energy from the ice cube. These processes seem irreversible in the observable universe.

However, an examination of the conservation laws of physics indicates that these processes are not forbidden. The equations of physics are time-symmetrical, they do not distinguish between past and future. The laws of physics indicate that every detailed action could occur in reverse. It seems that the concept of the passage of time is not essential. At the microscopic level of atoms, the world changes but does not necessarily evolve.

Yet in the macroscopic world there is no doubt at all that time flows only in one direction—and this needs an explanation. What is the reason for the irreversibility of time which we observe? If the origin of irreversibility is not in the mathematical presentation of physics it must then be related to the boundary conditions and the initial conditions prevailing during the Big-Bang explosion of the universe. What happened at the moment of the Big-Bang to give a direction to the arrow of time?

If we could isolate a perfectly insulated enclosure and make sure that it has the same temperature all over, then it would not be possible to define the passage of time within the enclosure. It would not be possible to determine

what is past and what is future within the enclosure, and time's arrow would have disappeared. In this hypothetical system we find a time-symmetrical static world, where time has no flow or passage or meaning. In the real world however, we can never isolate an "enclosure" or anything else completely from the rest of the universe. If systems are left alone, there is always a heat sink described by the cooling of the universe and the Second Law of Thermodynamics. Any system that has a possible heat sink, so that its temperature decreases, will always show the anisotropy of time.

In the macroscopic world effects occur after causes, entropy is increasing, and psychologically there is a sense of advancing time, which seems irreversible—even though the laws of physics do not prevent a time symmetry for microscopic processes, that is, a few perfume molecules may always find their way back into the perfume bottle.

Natural physical processes indicate that they operate in a basic symmetry. A fundamental theorem of physics requires that physical processes remain invariant with respect to the direction of the flow of time T, with respect to the electric charge of matter C, and with respect to the positions and motions of matter P (parity). This CPT theorem indicates that a universe with the reversed CPT properties will be indistinguishable from the one we started with. However, there is one known exception to CPT invariance, which may indicate a preferred direction of time. This is the decay of the unstable subatomic particles known as K-mesons. It appears that the natural decay rate for the K-mesons may be slightly different in a time-reversed world, as indicated experimentally by the observed CP symmetry violation (indicating a possible T-symmetry violation, assuming CPT invariance). This is the only known time asymmetric physical process, but it is deduced by indirect reasoning.

In the description of a flowing time we find interesting paradoxes which point out our limitations in understanding completely the nature of time.

The following are examples describing time asymmetry and irreversibility. Suppose we were given three colored snapshots of a hot, warm, and cold iron bar that were taken in an isolated enclosure and we were asked to arrange them in the time order that they were taken (Figure 3). One snapshot, labelled B, showed a blue iron bar. The second snapshot Y, showed a yellow iron bar, and the third snapshot R showed a black iron bar. It is easy to conclude that the time sequence of the snapshots were BYR, since the second law of thermodynamics clearly proceeds from hot to cool. Now assume that we were given three more snapshots of the iron bar all of which looked alike—that is, they were taken after the iron bar had cooled and was at the same temperature as its surroundings. In this case it would be impossible to deduce which snapshot was taken first, second, or third, and the direction of the arrow of time would be undetermined.

In another example consider that we make a movie of randomly moving molecules of a gas in an enclosure. We can then play the movie forwards and backwards and it would look pretty much the same, and we would not be able to tell which direction time was flowing when the movie was taken.

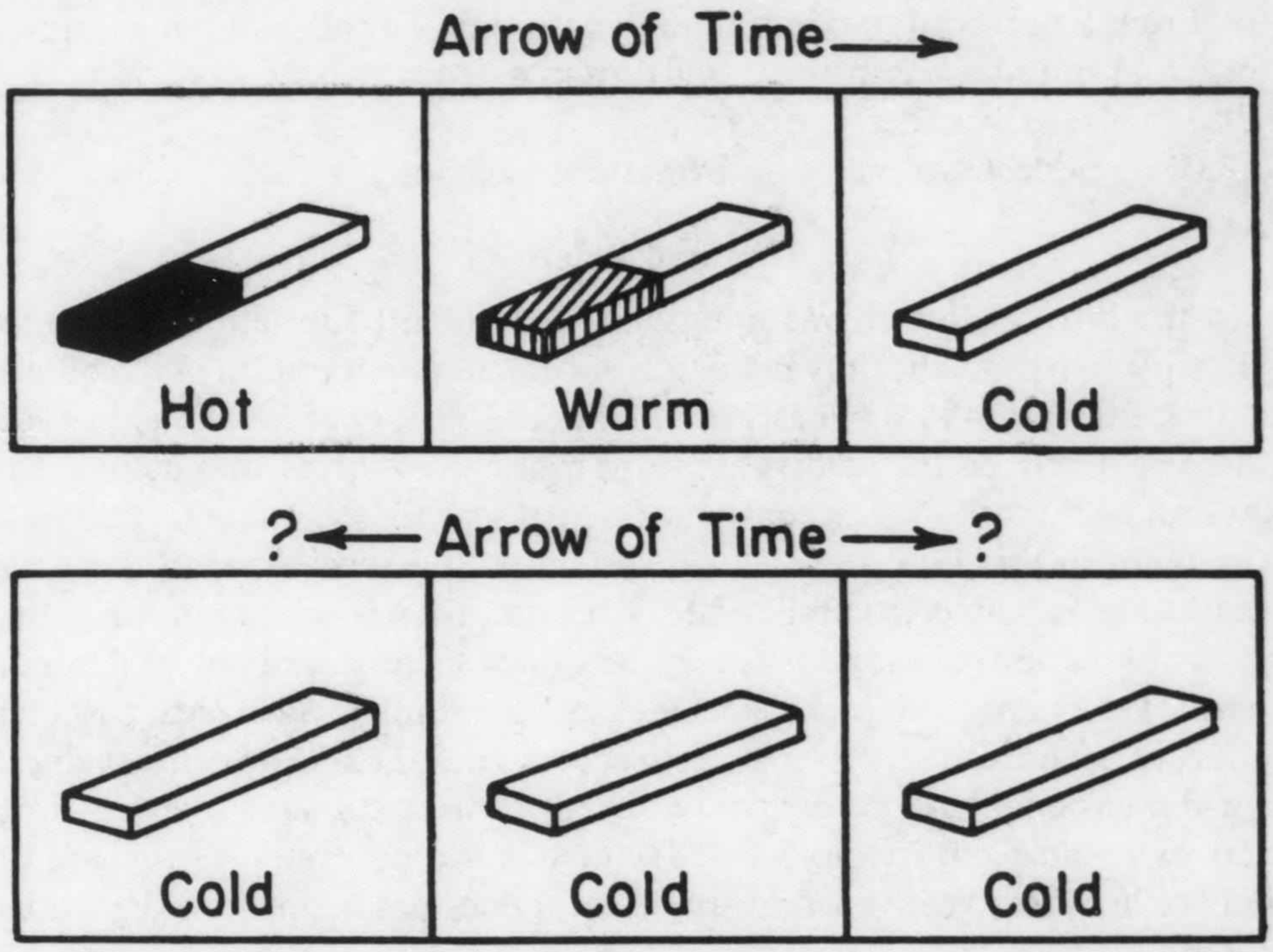

Fig. 3. The arrow of time. Snapshots of a cooling iron bar, and an iron bar at the same temperature in an isolated enclosure.

Another unusual experiment was suggested by Philip Morrison (1967). How can we make a film where a hen becomes smaller, becomes a chick, crawls into a shell and becomes an egg? Running the film backwards is cheating, of course! But, if we take an incubator with 10,000 eggs, and while our clock is running forwards, the first frame we take is that of an old hen, the second of another hen a little younger than the first, and so on to a newly hatched egg, then playing the movie forwards will produce the effect of seeing an old chicken become younger. Provided that we cannot distinguish one chicken from another, this would have the effect of statistically backward running time. Such a movie made with people probably does not work, for hens it might work if they all had the same color and looked alike, but such a film made for electrons will always work since there is no known way to distinguish one electron from the other.

Although it is evident that for macroscopic phenomena time runs only in one direction, into the future; it is also clear that the laws of nature are time-symmetrical and at the microscopic level of atomic particles the direction of time, or even its meaning and reality, remain ambiguous.

We can summarize the universal direction of time in the following somewhat related ways:

(a) From the Big-Bang to total expansion to infinity (or expansion to zero velocity followed by collapse back to the Big-Bang if the universe contains enough mass for gravity to stop the expansion).

(b) From a universal maximum temperature to a minimum temperature.
(c) From minimum entropy to maximum entropy.
(d) From relative order to disorder.
(e) From perfect symmetry (information) to chaos.

Time and Relativity

In the introduction it was mentioned that Albert Einstein's theories of relativity constitute the only major advancement in our notion of the nature of time. Einstein (1905) put forward his Special Theory of Relativity in 1905 where he described the relative uniform motion of objects, and defined the physical world with four dimensions, three of space *x, y, z,* and one of time *t*. Any point in this four-dimensional system with a specific set of *x, y, z, t* coordinates is defined as an "event." An event then is unique in the entire history of the universe, and it occurs only once since at best one of the four coordinates is assumed to change. One can then think of all events, past and future, in the universe by their *x, y, z, t* coordinates. That is we can imagine a four-dimensional map of everything in all of time. In such a space–time map, as pointed out by Gold (1974), there is no movement or passage of time, rather the objective world simply *is,* it does not happen. In the words of Penrose (1979, p. 592) the Special Theory of Relativity "leads to a picture in which potentialities becoming actualities is either highly subjective or meaningless."

This celebrated new physics resulted in the famous Twin Paradox, where one twin leaves the earth for a long interstellar voyage and when he returns he finds that his twin brother has aged much faster than he. This illustrates how motion makes the rate of the passage of time to be non-uniform.

The General Theory of Relativity published by Einstein in 1916, described a new theory of gravitation. Gravity in this theory is viewed not as a force between two masses, but as the curvature of space–time that these masses create around them. That is, objects fall towards each other not by a real gravitational attractive force, but by the nature of their masses producing a curved space around them where nearby objects roll towards them. This theory deals not only with uniform motion, but with accelerated motion as well. It predicts that the rate of time passage is not the same from place to place and that it depends on the strength of gravity felt at a certain location, more explicitly on the degree of the curvature of space–time at that place.

This theory has been used to describe the existence of the mysterious black holes, as mass entities where the curvature of space–time is complete. Such objects of extreme gravity are wrapped around space–time, and all space and time avenues with the rest of the universe are cut-off (Misner, Thorne & Wheeler, 1973). All communications with the inside of the black hole cease to be physically possible. To a distant observer a time clock near the black hole would appear to advance very slowly, and on the surface of the black hole time would appear to stop. This most fascinating result creates fundamental questions about the relationship of black holes with the evolving universe, which appears to have an arrow of time.

Clearly the gravity, or the space–time curvature, of a black hole is so extreme that not even a ray of light can escape from it. A distant observer then receives no light from a black hole and it appears black to him. The existence of the black hole however, can be felt by its attractive gravitational potential that it exerts on its surroundings. Any object close to a black hole will deduce its presence by the pull that it would feel on itself. We cannot see black holes, but we can be sure that they are there! It should also be mentioned that in 1974 Steven Hawking (1974) unexpectedly showed that black holes are not entirely black. Due to quantum mechanical effects black holes may evaporate, but for a black hole the mass of a typical star, this Hawking radiation is completely unimportant since the time of evaporation is computed to be much much longer than the age of the Big-Bang universe.

The General Theory of Relativity also presents paradoxes which appear to defy our psychological common sense. Thomas Gold (1975) has described the Mother and Baby Paradox where a loving mother does not wish her baby to grow old. Hypothetically, every night when the baby sleeps she surrounds the baby's crib with huge masses, creating a strong gravitational field—where time passes slowly. She then goes out in space and spends the nights and in the mornings she takes the masses away, and repeats the process every night thus achieving a cumulative slowing of the passage of time for the baby. However, I think that an even deeper paradox results if we exchange the baby with the mother, then the mother eventually becomes younger than her baby! Beware! This is not equivalent to travelling into the past. Nature seems to permit such behavior and the meaning of time becomes even more mysterious.

It is not unusual to think that there is a relation between the very early Big-Bang and a super black hole. How is time to be understood during the early instances of the formation of the universe?

Relativity puts an inseparable bond between the notion of time passage and the existence of matter and energy in the universe. Time seems to pass at different rates depending on what masses are available. Time passes slowly near masses, and in vast empty spaces time would go faster compared to ours. The whole concept of time passage seems to be entirely due to the fact that there are masses around in the universe.

Conclusions

For living beings, for stars and galaxies, and for the evolution of the universe there seems to be an arrow of time, yet in detail the concept of the nature of time remains uncertain. The best observation about time is presented by the Second Law of Thermodynamics, and the best description of time is given by the Theories of Relativity. A deeper understanding of the nature of time may come when we are able to explain the physics of the Big-Bang in much more detail.

One could ask, what was before the Big-Bang? Or did space and time, and energy and matter begin at the Big-Bang with zero space, zero time, and with no matter or energy? As Stephen Hawking (1988) has described, if time had no boundary than it ceases to be well defined at the Big-Bang. Just like the

direction of North is not well defined at the North pole. Asking what happened before the Big-Bang is like asking for a point say one kilometer North of the North pole! Since in relativity the time coordinate is equivalent to the other three space coordinates, then this analogy is a particularly vivid one.

A more complex model of the Big-Bang is the model of an oscillating universe, where the universe continuously expands and contracts in an infinity of time. In such a model it is not possible to define the direction of time. Perhaps time runs forwards during the expansion phases and backwards during the contraction phases—that is, cause and effect may be reversed during the contractions. Such models remain very hypothetical, and may not be realistic since it is possible that consecutive Big-Bangs may damp out the expansion in only a few cycles.

Our understanding of the physical world has been enormously enhanced with the spiraling progress of science and technology. The unity of knowledge has revealed a mysterious and highly sophisticated universe. While we still interpret all things in terms of a subjective flowing time, we do understand that the nature of time remains unknown. Any deeper understanding of the origin of the universe will have to include a coherent explanation of the nature of time.

The somewhat confusing state of our understanding of the nature of time was perhaps best put by T. S. Eliot (1971, p. 121) who once reflected as follows:

Or say, that the end precedes the beginning,
And the end and the beginning were always there,
Before the beginning and after the end.
And all is always now.

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Journal of Scientific Exploration, Vol. 2, No. 2, pp. 155–179, 1988
Pergamon Press plc. Printed in the USA.

0892–3310/88 $3.00+.00
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Operator-Related Anomalies in a Random Mechanical Cascade

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Abstract—Experiments with a "Random Mechanical Cascade" (RMC) apparatus have yielded anomalous results correlated with pre-stated intentions of human operators. Based upon a common statistical demonstration device, this machine allows 9000 polystyrene balls to drop through a matrix of 330 pegs, scattering them into 19 collecting bins with a population distribution that is approximately Gaussian. As the balls enter the bins, exact counts are accumulated photoelectrically, displayed as feedback for the operator, and recorded on-line. Operators attempt to shift the mean of the developing distributions to the right or left, relative to a concurrently generated baseline distribution. Of the 25 operators who have completed one or more experimental series with this device, four have achieved anomalous separations of their right and left efforts, and two others have displayed significant separations of either their right or left efforts from their baselines. The overall mean difference of right versus left efforts concatenated across the total data base of 87 series (3393 runs), has a probability against chance of $<10^{-4}$, with 15% of the individual series significant at $p < .05$, and 63% conforming to the intended directions.

The concatenated results display a stark and curious asymmetry, in that virtually all of the right vs. left separation is provided by the left vs. baseline separation. This pattern also appears in the data of several individual operators, and is not attributable to any known physical asymmetry in the experimental system. In addition to the systematic asymmetric deviation of the distribution means, cumulative excesses in the variances of the left and right distributions relative to baseline are also observed, progressing to statistical probabilities of .003 in the left efforts, but only .2 in the right. More detailed study of the individual bin population patterns reveals that while most of the bins contribute to the overall mean shifts and variance changes, those on the outer portions are more influential than those near the center.

Operator achievements tend to compound marginally but systematically in cumulative deviation patterns characteristic of the particular individuals and, in several cases, similar to those produced by the same operators in microelectronic Random Event Generator (REG) experiments. Within these characteristic patterns of achievement, some operators also show sensitivities to secondary experimental parameters, such as instructed vs. volitional establishment of the intended directions, or the presence or absence of feedback displays. Other successful operators seem insensitive to such options.

Acknowledgement. The Princeton Engineering Anomalies Research program is supported by grants from The McDonnell Foundation, The John E. Fetzer Foundation, Inc., Helix Investments, Ltd., and The Ohrstrom Foundation.

Two major protocol variations have been explored, one employing remote operators, the other, multiple operators. In the former, operators with well-established performance in local experiments attempt to influence the bin distributions from remote locations up to several thousand miles from the laboratory. Significant results are again obtained that are quite similar to those of the local experiments, with the exception that the overall right and left distribution variances are smaller than those of the baseline. In the multiple operator experiments, early results show little resemblance to those achieved by the participating individuals alone.

Introduction

Through an extensive program of experiments previously reported in this Journal and elsewhere (Jahn & Dunne, 1986; Jahn, Dunne, & Nelson, 1987; Nelson, Dunne, & Jahn, 1984), it has been established that human operators can systematically influence the output of various microelectronic random event generators (REGs) to a marginal, but statistically significant extent, in accordance with pre-stated intentions. Specifically, the results have been found to correlate strongly with the individual operators and their intended directions of effort, and in some cases with certain secondary parameters such as whether the effort is volitionally chosen or randomly assigned, or whether the sequencing of trials is manual or automatic. Curiously, however, the performance seems less dependent on the details of the microelectronic noise source and processing circuitry, including various hard-wired or programmed pseudo-random systems.

The demonstrated insensitivity of these microscopic experiments to their interior details suggests broader exploration of the response of other classes of random physical processes to operator intention. In particular, it seems important to ascertain whether similar anomalies can be demonstrated on macroscopic systems employing other than electronic interactions. For this purpose, we have developed and applied a device termed a "Random Mechanical Cascade," which is a large-scale variant on the prototypical "Galtons Desk."

In 1894, the British eugenicist Francis Galton described a mechanical apparatus to illustrate certain statistical aspects of natural evolution. His description and interpretation are sufficiently relevant and ingenuous to bear repetition:

> It is a frame glazed in front, leaving a depth of about a quarter of an inch behind the glass. Strips are placed in the upper part to act as a funnel. Below the outlet of the funnel stand a succession of rows of pins stuck squarely into the backboard, and below these again are a series of vertical compartments. A charge of small shot is inclosed. When the frame is held topsy-turvy, all the shot runs to the upper end; then, when it is turned back into its working position, the desired action commences. . . . The shot passes through the funnel and issuing from its narrow end, scampers deviously down through the pins in a curious and interesting way; each of them darting a step to the right or left, as the case may be, every time it strikes a pin. The

pins are disposed in a quincunx fashion, so that every descending shot strikes against a pin in each successive row. The cascade issuing from the funnel broadens as it descends, and, at length, every shot finds itself caught in a compartment immediately after freeing itself from the last row of pins. The outline of the columns of shot that accumulate in the successive compartments approximates to the Curve of Frequency, and is closely of the same shape however often the experiment is repeated.

The principle on which the action of the apparatus depends is, that a number of small and independent accidents befall each shot in its career. In rare cases, a long run of luck continues to favour the course of a particular shot towards either outside place, but in the large majority of instances the number of accidents that cause Deviation to the right, balance in a greater or less degree those that cause Deviation to the left. Therefore most of the shot finds its way into the compartments that are situated near to a perpendicular line drawn from the outlet of the funnel, and the Frequency with which shots stray to different distances to the right or left of that line diminishes in a much faster ratio than those distances increase. This illustrates and explains the reason why mediocrity is so common. (Galton, 1894, pp. 63–65)

Galton's concept of demonstrating the development of Gaussian distributions by the compounding of a multitude of random binary events has since been extended into numerous other sectors, and various versions of his machine may be seen in science museums and instructional laboratories throughout the world. Our particular interest here is as a target for anomalous man/machine interactions.

Equipment and Protocol

Our RMC machine is roughly $10' \times 6'$ in size, and employs 9000 polystyrene spheres $\frac{3}{4}''$ in diameter, cascading through a quincunx array of 330 $\frac{3}{4}''$ nylon pins on $3\frac{1}{4}''$ centers, in much the same fashion as Galton described (Figure 1). A belt and bucket conveyor transports the balls from a plenum at the bottom to a funnel at the top, from which they bounce down through the matrix of pins in complex stochastic paths, accumulating finally in 19 parallel vertical collecting bins arranged across the bottom. The front of the pin chamber and collecting bins is clear acrylic, so that both the active cascade of the balls and their disposition into the developing distribution of bin populations are visible as feedback to the operator. By appropriate combinations of peg spacings, ball inlet configuration, and bali and peg material properties, the resulting distribution of ball populations in the collecting bins can be brought to a good approximation of a normal Gaussian distribution.

The entrance to each collecting bin is equipped with an optoelectronic counter. All 19 counters are scanned on-line by a microprocessor which transmits in real time the ordered accumulation of counts in each bin to LED displays at the bottom, and to a microcomputer where the complete sequence is registered on disk as a file of 9000 characters. Each file is indexed by file number, direction of effort, operator, date, time, humidity and temperature within the pin cavity, and various other experimental parameters.

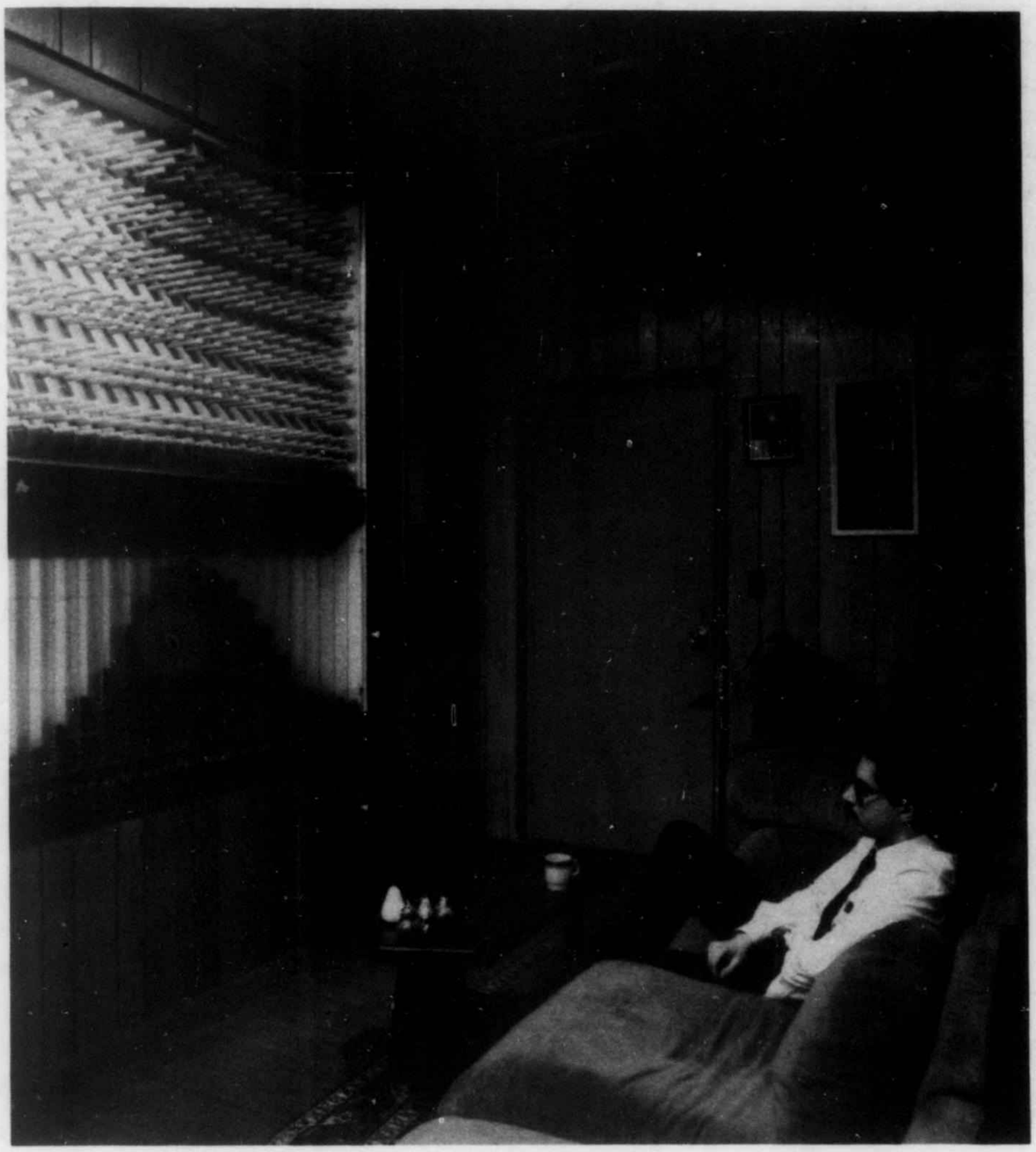

Fig. 1. Random Mechanical Cascade apparatus (RMC).

A logbook is also maintained with the same indexing information, a photograph of each completed run distribution showing the bin totals displayed on the LED's, the bin totals registered by the computer, other summary information including total populations right and left of the center bin, a right-left ratio, the distribution mean and standard deviation, and any appropriate comments.

The standard experimental protocol calls for the operator, seated on a couch about eight feet from the machine, to attempt to distort the distribution of balls to the right or higher numbered bins (RT), or to the left or lower numbered bins (LT), or to generate a baseline (BL). These intentions are interspersed in concomitant sets of three runs, each of which lasts about 12 minutes. The sequence of each tripolar set of runs may be chosen by the operator (volitional assignment), or may be defined by some pre-established recipe (instructed assignment). Each set must be completed in one session,

lasting about one hour. The operator has the choice of lighted or blank LED feedback displays, and employs his own subjective strategies. Operators schedule their own sessions, and are encouraged to generate large data bases, usually divided into independent experimental series of 10 tripolar sets of 3 runs.* Secondary parameters are fixed within a given series, but the operator's subjective strategy is not controlled. All operators are uncompensated anonymous volunteers who are willing to provide the requisite large data bases. None claims special abilities, and no screening or training of operators is attempted. (Although generic pronouns are used throughout this text, roughly equal numbers of male and female operators have contributed to the data base.)

The primary purpose of the tripolar experimental format is to mitigate any possible biasing effects of physical or environmental changes in the operation of the machine, such as long-term drift resulting from pin or ball wear, or the shorter-term influences of temperature or humidity. Variation of humidity, for example, has been found to correlate with small changes in the distribution variance, and very slightly with changes in the mean. (A detailed assessment of long-term drift of the distribution mean, including contributions from wear and mechanical factors, vibration, humidity, temperature, gravitational and tidal effects, and operator position is available in Nelson, Dunne, & Jahn, 1988a, which also includes a description of qualification and calibration procedures and the treatment of any technical problems that may arise in a given run.)

The machine, its counting and data recording systems, and the experimental protocol just described have evolved over the course of the program (Nelson, Dunne, & Jahn, 1983). In the earliest exploratory experiments, data were manually recorded as mechanically measured bin fill heights, but it soon became clear that these were compromised by differential stacking patterns of the balls, so that the much more precise photoelectronic counting system was devised. Similarly, early attempts to establish a universal baseline distribution by copius calibration data had to be abandoned once the effects of temporally varying physical conditions, such as temperature, humidity, pin and ball wear became apparent. At that point, it was decided to include only differential data generated in matched tripolar runs in the formal data base. All preceding runs, and any subsequent runs deviating from that protocol, are now regarded as exploratory, and indexed as such in a separate data file. Also included in the exploratory data are several series in which operators attempted to influence the accumulations in particular individual bins. Although interesting results were obtained in these experi-

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* Prior to October, 1983, 20 sets constituted a series. In a few cases, operators have completed slightly less or more than the prescribed number of sets per series due to technical malfunction, record-keeping errors, or unavoidable departure from the laboratory. Rather than exclude such data, they have been included as odd-size series. If less than 7 sets have been generated, however, the series has been voided and the data retained in a separate file.

ments, this paper is confined to tripolar protocols addressing the shift of the distribution mean.

Statistical Treatment

Given the three-dimensional complexity of the individual ball collisions with the pegs, the complicating effects of ball-ball collisions, and the irregularities of the inlet flow, any deterministic dynamical modeling of the flux of balls through the pin matrix is unattainable. In this respect, the RMC experiment differs substantially from the strictly binary REGs, where precise theoretical expectations are derivable. However, a rudimentary quasi-binary model of the RMC statistical process, analogous to that more rigorously employed for the REG experiments, can be heuristically based on the virtually Gaussian character of the data distributions. As shown in Figure 2, a histogram of the 19 bin populations for the 1131 baseline runs of the formal data base is nicely fit by a Gaussian curve normalized to the distribution mean and standard deviation. To be sure, there is a slight excess population of the end bins by balls reflected from the side walls of the machine, and of the center bins by balls falling directly through the finite pin matrix, but these do not compromise the gross utility of the Gaussian model.

Such a quasi-binary approach can illustrate the immense statistical leverage of this kind of experiment. For example, using bin number as the statistical variable, the mean of this quasi-Gaussian distribution, μ, is found to be approximately 10.023 and the standard deviation, σ, about 3.27 bins. Although this distribution derives from a very complex ensemble of collisional events, it can be modeled, for statistical purposes, as if it were the result of N simple binary events for each ball, in each of which it is deflected

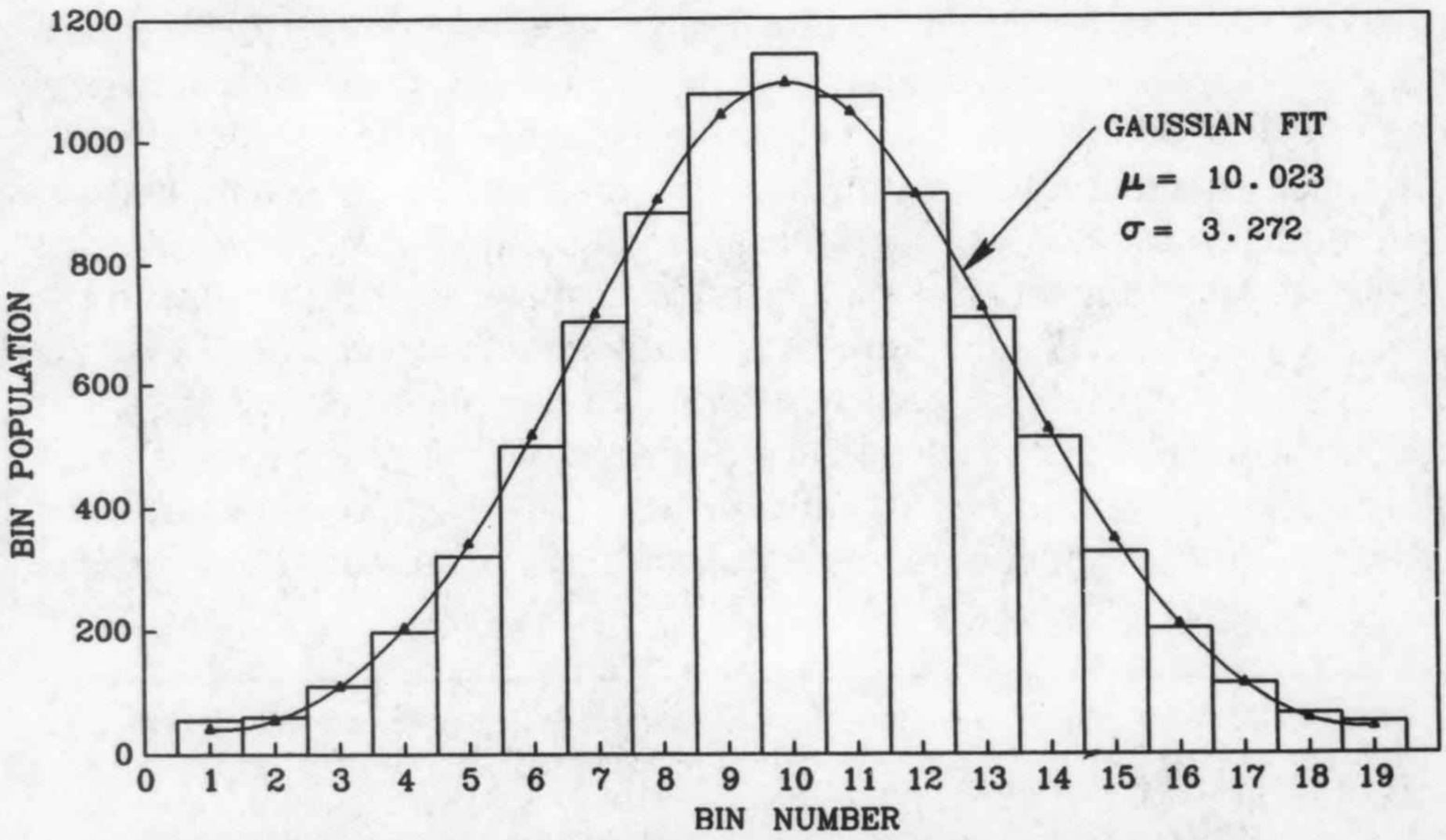

Fig. 2. RMC: Baseline mean bin populations on theoretical Gaussian (1131 runs).

either to the right or to the left by some uniform distance, $D = 0.5$ bin. If the probabilities for deflection in either direction are equal, binomial statistics requires a distribution variance of $\sigma^2 = D^2N$. Hence, the complex physical process actually experienced by a single ball is equivalent to $N \approx 43$ elementary binary choices, which may be regarded as a minimum estimate of the information content of a single ball's trajectory. Since there are 9000 such "ball-trials" in a single run, the statistical power of each run consists of approximately 387,000 binary equivalent bits. (By comparison, a typical REG run of 50 trials yields 10,000 bits.)

The stochastic dynamical behavior of this quasi-binary system is found empirically to converge to a stable statistical ensemble with a well-bounded mean after roughly one-third of the 9000 balls have been processed. In similar fashion, the optimum number of runs per experimental series, which must be a trade-off between operator comfort and statistical confidence, can be assessed in terms of the standard error of the estimates of the series mean. For example, the earliest series lengths were arbitrarily set at 20 runs per intention, for which the standard error corresponding to a 95% confidence interval around the grand baseline mean was found to be .0078 bins. But for the first 10 runs of these series, the standard error was only .0111 bins, still adequately small to allow any systematic anomalies to cumulate rapidly to significant deviations from expected values. Thus, we were able to respond to operator complaints that the 20-run series were too long by reducing the protocol to 10 runs per intention, and the series data accumulation rate increased accordingly.

The absence of any precise theoretical expectations, combined with the long-term variations in the calibration data mentioned earlier, force any search for distribution anomalies correlated with operator intention to proceed on a local differential basis. That is, only the cumulative systematic differences among the right, left, and baseline efforts within a given tripolar set can profitably be assessed. The most appropriate statistical tool for this purpose is Student's *t*-test for paired observations, supplemented by a conventional one-way analysis of variance (ANOVA). More specifically, the primary assessment of the effect of operator intention on the distributions employs correlated *t*-tests to compare differences within the tripolar data sets for each series, or for larger concatenations, taking into account the small, but occasionally significant correlations among the three strings of data due to common influences of machine wear, humidity, and other possible but unidentified effects.

The difference between the right and left efforts (RT–LT) is regarded as the primary indicator of an effect of operator intention. It is then possible to perform only one other fully orthogonal comparison, namely that between the baseline and the algebraic average of the right and left efforts. However, since interpretation of this latter quantity is somewhat complex, we have chosen to present the conceptually simpler paired *t*-tests for both RT–BL and LT–BL differences, supplemented by various graphic displays. While

these tests are not fully independent of the primary RT–LT comparison, they are instructive in locating the three intentions relative to each other, and, of course, any one of the three tests constitutes a coherent, standardized measure across series.

Even in this local differential treatment, the raw data show correlations among the three data strings, perhaps attributable to shorter-term vagaries of machine operation. These are assessed by Pearson product-moment coefficients, which are helpful in explaining the considerable range of the standard deviation of the differences, and thus the variations in the size of the mean shifts that may be regarded as significant. For example, as the correlation between RT and LT data increases, the standard error of the differences decreases, yielding a larger *t*-score for a given mean difference.

For a supplementary perspective, the three conditions, RT, LT, BL, can be considered as three treatments in a conventional one-way analysis of variance. This approach can establish an overall estimate of unexplained, or error, variance appropriate for the comparisons, and it can explore covariates such as humidity and temperature, but it cannot incorporate any of the secondary experimental parameters, such as feedback mode or instructional/volitional options, since only a few operators have explored these systematically.

Consonant with the earlier REG experiments, it proves instructive to display the primary indicator of the effect of operator intention on the RMC, that is, the RT-LT difference, as a graph of cumulative deviation from the expected difference, given no effect, of zero. Likewise, the separate behaviors of RT and LT may be graphed as cumulative differences from the local BL values. To establish the scale of the cumulative deviations, envelopes of the *t*-scores corresponding to given probabilities against chance may be superimposed on these graphs.

Overall Results

Mean Shifts Correlated With Intention

The primary RMC data base consists of 87 series (3393 runs) generated by 25 individual operators. The overall results are shown in Figure 3, and in Table 1.* For the grand concatenation of these data, the paired *t*-test for RT–LT yields $t = 3.89$, $p < 10^{-4}$, one-tailed in direction of intention, with 15% of the series and 24% of the individual operator data bases beyond the 5% chance probability level and 63% splitting RT–LT in the intended direction. The one-way ANOVA yields a comparably significant departure from chance expectation ($F = 8.13$, with 2 and 3390 *df*, $p = 3 \times 10^{-4}$). The

* All tables are in the Appendix. Much more detailed results of all RMC experiments performed to date are presented in two PEAR Technical Reports (Nelson, Dunne, & Jahn, 1988a and 1988b). Many of the summary conclusions of this paper are supported by data displayed there, but not included here.

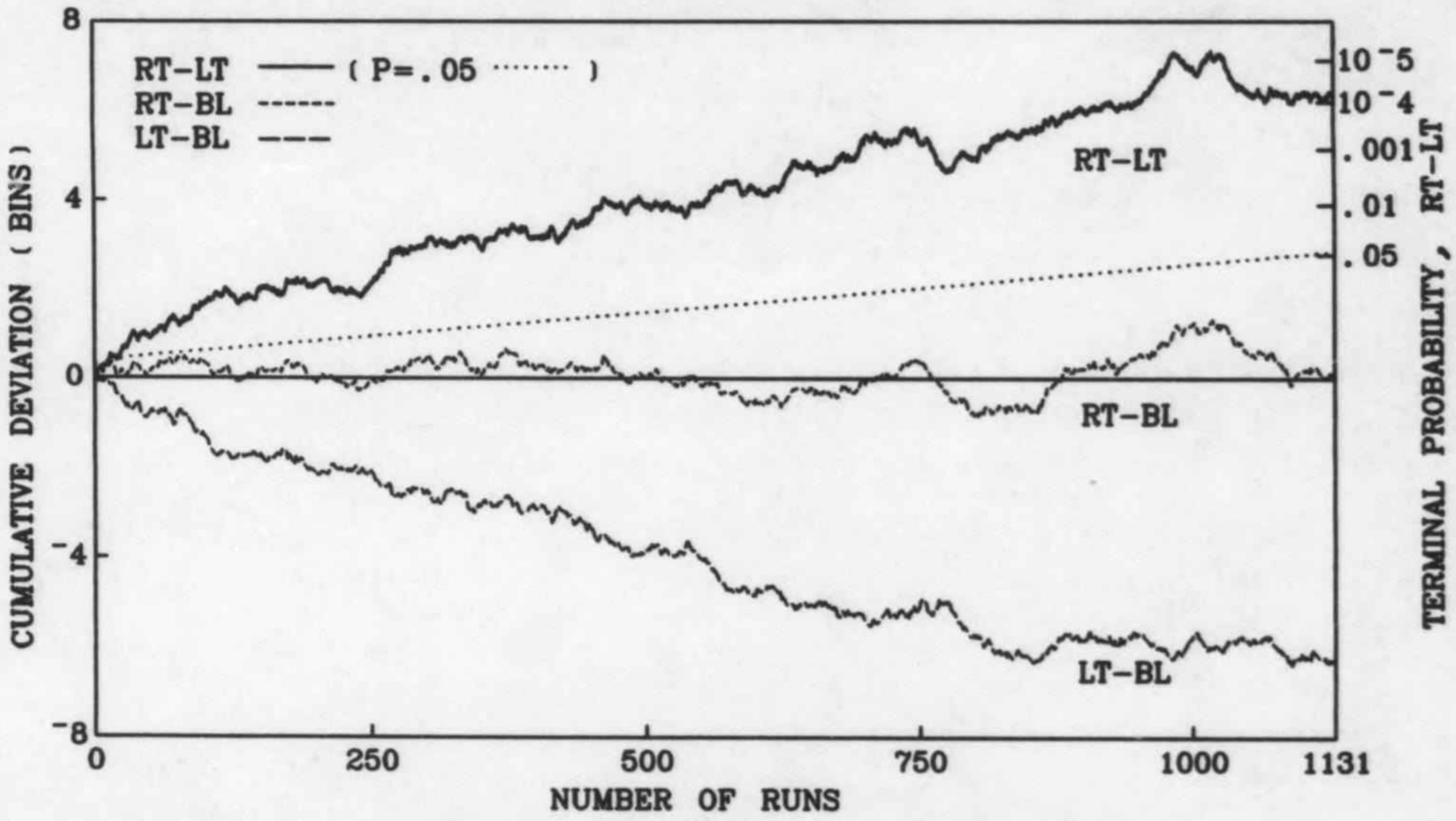

Fig. 3. RMC cumulative deviations: all data.

cumulative deviation graphs that trace the progress of these mean shifts through the various operators and series display notable consistencies of linear trends superimposed on backgrounds of stochastic variations. Thus, by any of the statistical criteria employed, there is clear evidence of a significant anomaly compounding from the gradual accumulation of small but consistent shifts in the distribution means, in a fashion quite similar to that found in the microelectronic REG and pseudo-REG experiments.

Asymmetry Effect

Beyond its existence and magnitude, the overall RMC anomaly entails an additional curiosity not found in the REG experiments. Namely, the concatenated results shown in Figure 3 display a stark left-going asymmetry. In fact, the right and baseline efforts are statistically indistinguishable over the full course of these experiments, so that the entire RT–LT effect is contained in the systematic separation of the left efforts from both of the others. This asymmetry is not explainable in terms of any known physical bias in the experimental system, especially given the tripolar protocol and differential data reduction process, and hence can be correlated only with operator intention, suggesting that it may have some psychological or neurological implication. Examination of the individual operator data bases might tempt the assignment of much of this effect to one or two of the most prolific operators, who individually show strong left-going asymmetries. However, removal of their contributions fails to symmetrize the remaining data. Alternately, the influences of every operator on the total data base may be balanced by concatenating equal subsets of the individual data, for example the first 10 runs only of each of the 25 operators (Figure 4). Again the traces proceed steadily toward significant terminal values for RT–LT and LT–BL

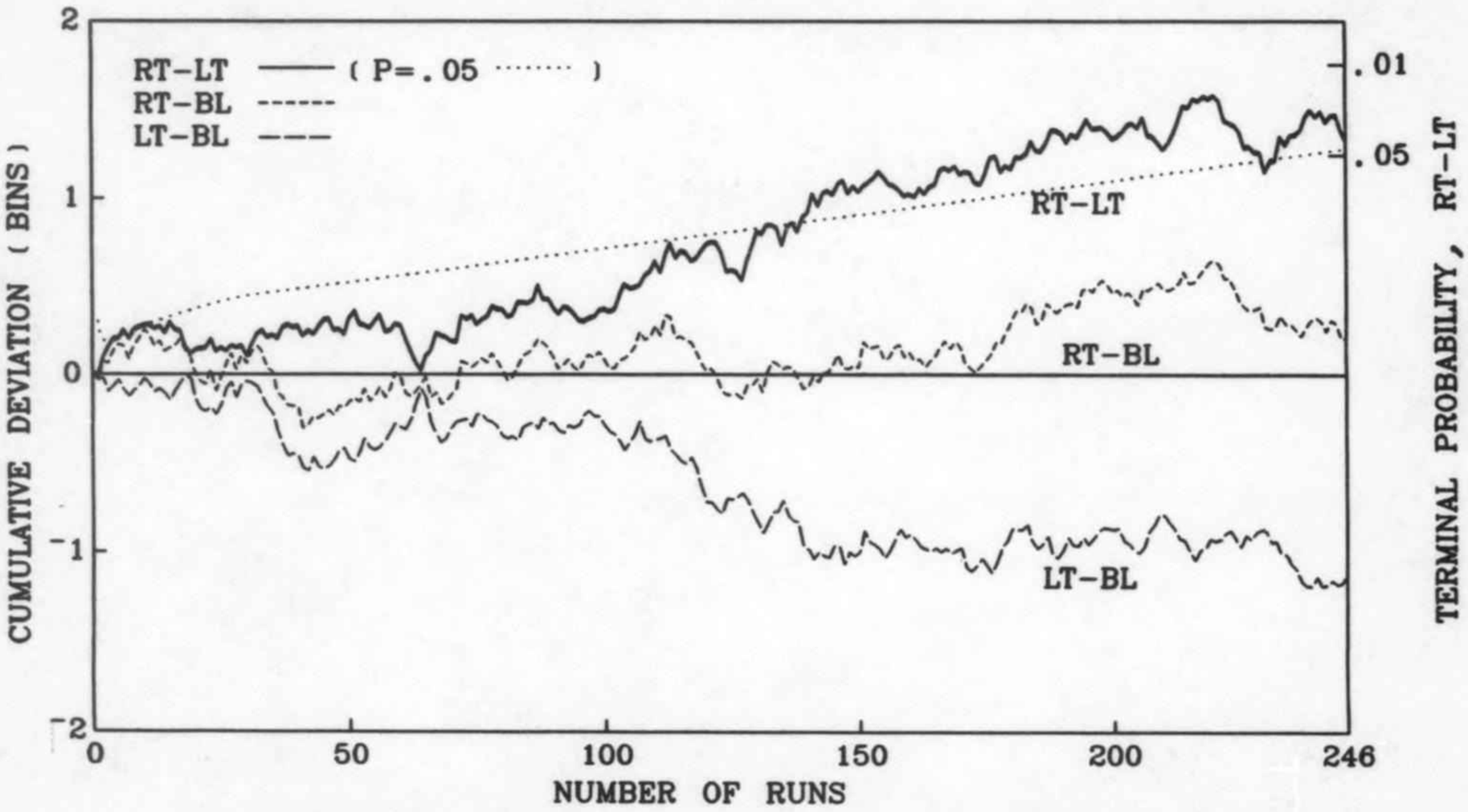

Fig. 4. RMC cumulative deviations: all operators, first 10 runs.

and show the same asymmetry, indicating that the overall effect reflects contributions from all operators in the group.

Secondary Parameters

Analysis of the composite data base for correlations with secondary parameters is necessarily limited since all operators have not contributed equally to the various categories. The mode of instruction, that is, whether the order of intentions in each tripolar set is chosen by the operator at the time of the session (volitional), or is prespecified for the entire series (instructed) has been varied in six operator data bases, and across these, the qualitative results seem to indicate a preference for the volitional mode; although the difference is not statistically significant ($t = 0.895$, $p = .371$, 2-tailed, $df = 779$). Similarly, the feedback has been varied for three operators by having the LED counter displays on or off, and here even less difference is found, although the results of each mode remain individually significant (LED on: $t = 1.829$, $p = .034$, 1-tailed, $df = 359$; LED off: $t = 2.845$, $p = .002$, 1-tailed, $df = 249$; Δt on/off = 0.878, $p = .379$, 2-tailed, $df = 608$). Less well defined parameters, such as date or time of day, have also been examined and show no clear influences across the total data. However, when these various parameters are analyzed on an individual operator basis, some correlations can be quite strong, indicating important operator-specific sensitivities. For example, with the LED display off, one operator (55) achieves a RT–LT t-score of 1.915 ($p = .029$), but with the display lit, performs at chance ($t = 0.294$, $p = .385$). Similarly, in the Volitional mode, another operator (70) obtains a RT–LT t-score of 2.143 ($p = .019$), but in the Instructed mode produces results slightly opposite to intention (t

= −0.693, p = .253). Several other examples are displayed in Nelson, Dunne, and Jahn, 1988b.

Variance and Goodness-of-Fit Effects

In the REG experiments, it was found instructive to array all terminal *series* scores as frequency distribution histograms (Jahn, Dunne, & Nelson, 1987; Jahn, Nelson, & Dunne, 1985). To pursue similar displays of the RMC results, we must again utilize the differential treatments, RT–LT, RT–BL, and LT–BL, comparing the distribution of series t-scores against a theoretical t-distribution for the nine degrees of freedom appropriate to a ten-run series. The results in Figures 5a, b, c confirm the shift of the distribution in the direction of intention for RT–LT and LT–BL, and in addition show an increase in the distribution variance in all three cases.

Similar frequency histograms for the *run* means, show little effect on the variance of the distributions, but the BL data are found to fit the expected Gaussian better than expected by chance (χ^2_{BL} = 7.75, 17 *df*, p = .97) while both the RT and LT distributions are relatively rough, (χ^2_{RT} = 21.63, p = .21 and χ^2_{LT} = 35.05, p = .0065).

It next seems reasonable to inquire whether the individual run bin population distributions might themselves display variance changes as ancillary effects of operator intention. In the REG studies, no such effects were observed at the trial or run levels, but the situation appears to be quite different for the RMC. Figure 6 displays the cumulative differences in the standard deviations of the RT and LT runs compared to the BL run of the same set. The generally positive trends culminate as a highly significant increase in the LT run variances relative to the BL, and a non-significant increase in the RT run variances. This effect is more pronounced for some individual operators than others, but the concatenated data suggest that, on balance, both LT and RT efforts to shift the mean also entail a broadening of the bin population distributions in some fashion.

Individual Bin Populations

To pursue such goodness-of-fit issues in yet more detail, the response of all 19 individual bin populations to operator intention may be extracted from the data base management system. Over a sufficiently large number of runs, each of these bin populations should, by chance, distribute normally about some mean, with some standard deviation, in terms of which parameters any given population anomaly may be statistically quantified. Immediately instructive is a graphic display of the differences in individual bin populations between the RT, LT, and BL run sets over the entire data base, that highlights the major contributions to the overall distribution mean shifts (Figures 7a,b,c). On these histograms the significances of each of these differences is indicated by a .05 probability envelope, computed on the basis of the individual bin population standard deviations. Consistent with the

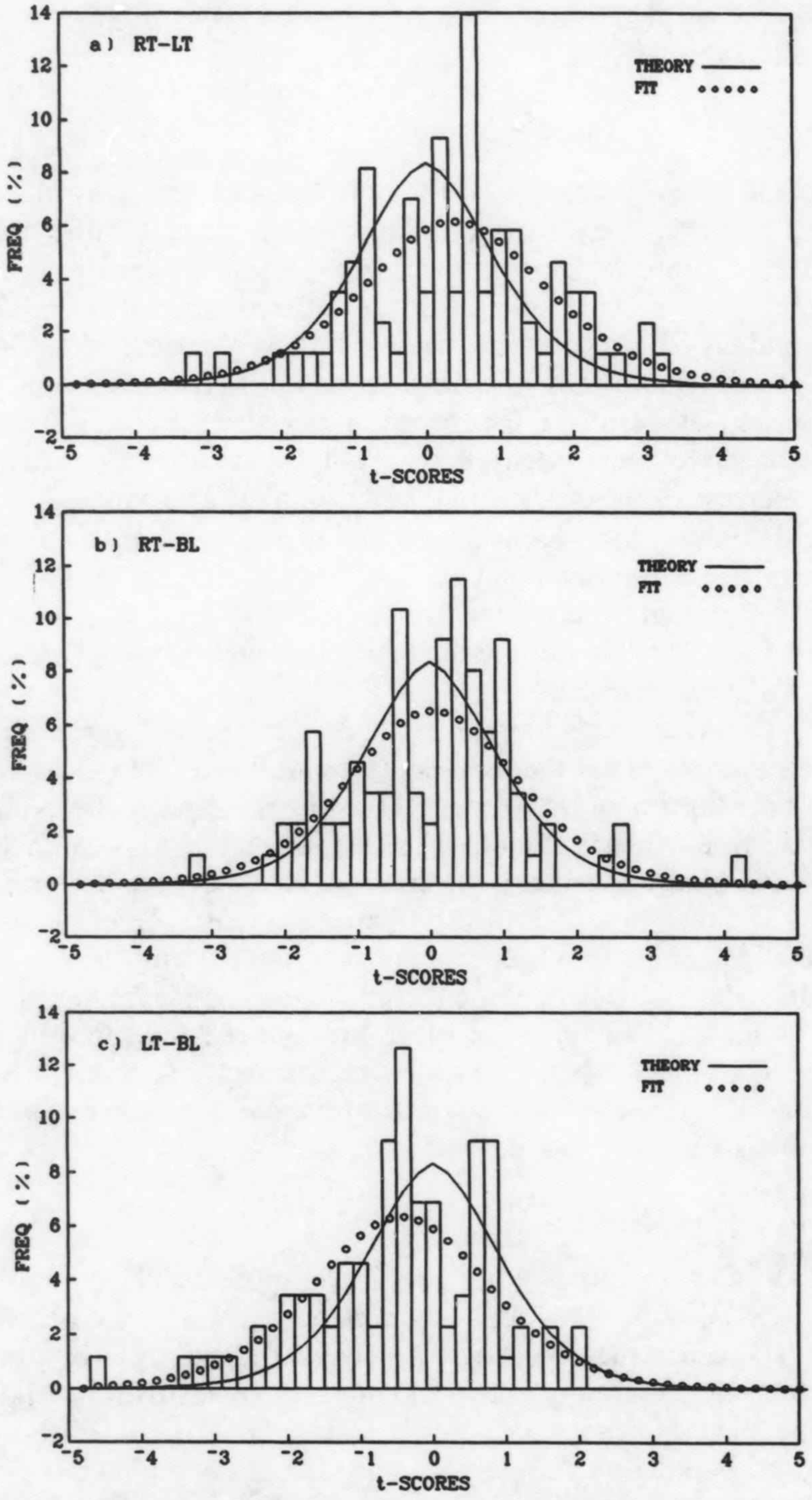

Fig. 5. RMC series *t*-score histograms: a) RT–LT; b) RT–BL; c) LT–BL.

overall behavior of the run means, the RT-BL bin population differences appear quite randomly distributed, whereas a majority of the LT–BL and RT–LT differences are supportive of the mean shifts. Because of their larger

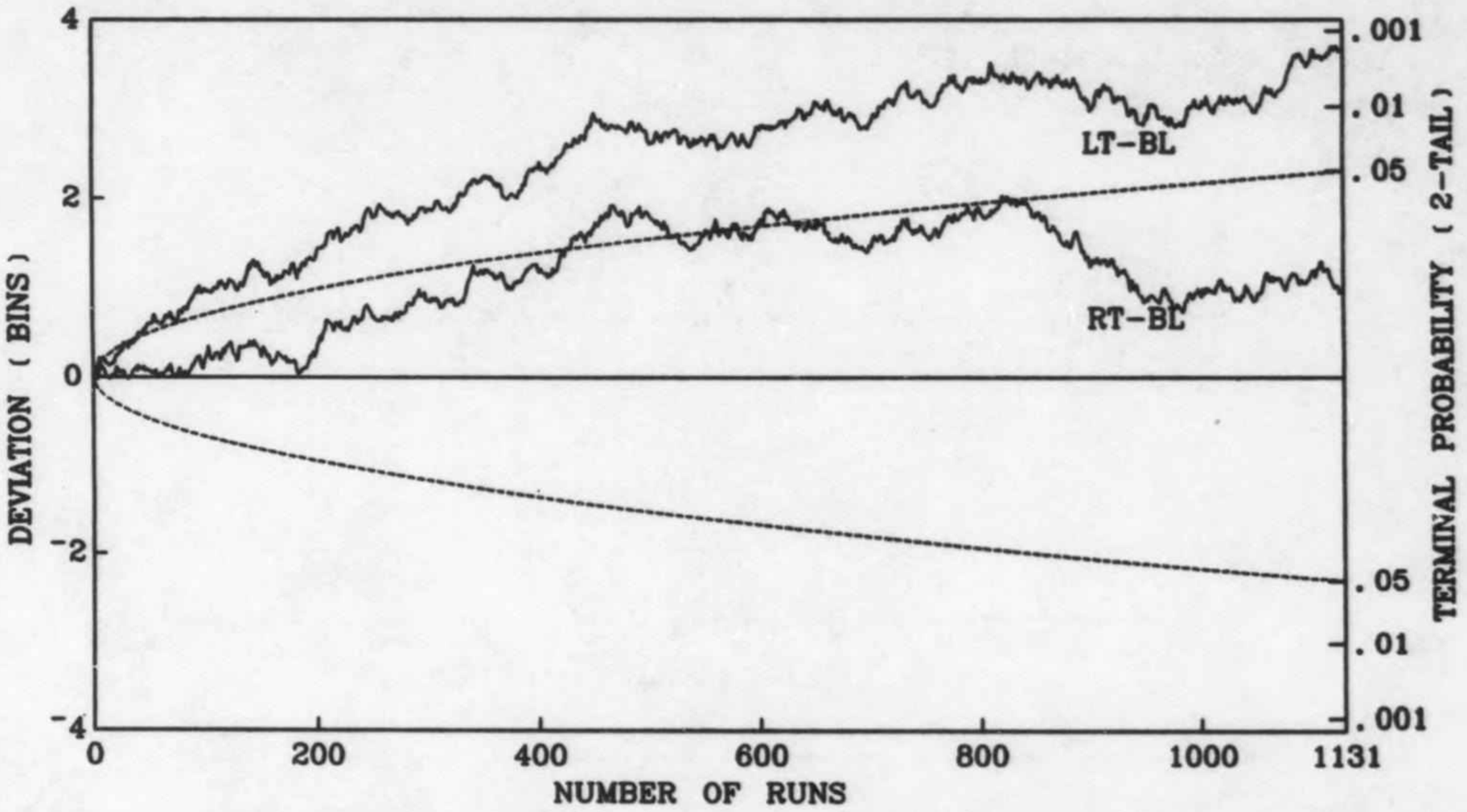

Fig. 6. RMC cumulative deviations of standard deviations: all data.

"leverage," outer bins contribute more heavily to the shifts than inner bins.

The data base management system also allows assessment of the temporal evolution of the bin-filling patterns. Although this has not yet been pursued systematically, cursory examination of these and of the progress of the overall mean of the developing distributions has not indicated any strong pattern of localization of the anomalous effects in any given portion of the 12-minute experimental period. Rather, like the REG effects, these seem statistically well distributed throughout the entire run.

Individual Operator Results

As found in our REG experiments, many of the RMC cumulative deviation graphs for individual operators are sufficiently replicable and internally consistent to be regarded as characteristic "signatures" of achievement by those particular operators. These signatures vary considerably from one operator to another and, in some cases, are found to be quite sensitive to the secondary experimental parameters. The grand concatenations of results across operators presented above, although reinforcing the credibility of the phenomenon, tend to obscure these important, potentially instructive, individual differences. Hence, the balance of this article will address individual operator performance.

Correlations With Intention

Four of the 25 operators have achieved statistically significant overall RT–LT separations positively correlated with their directional intentions, based on *t*-tests for paired data. Two show significant overall results in

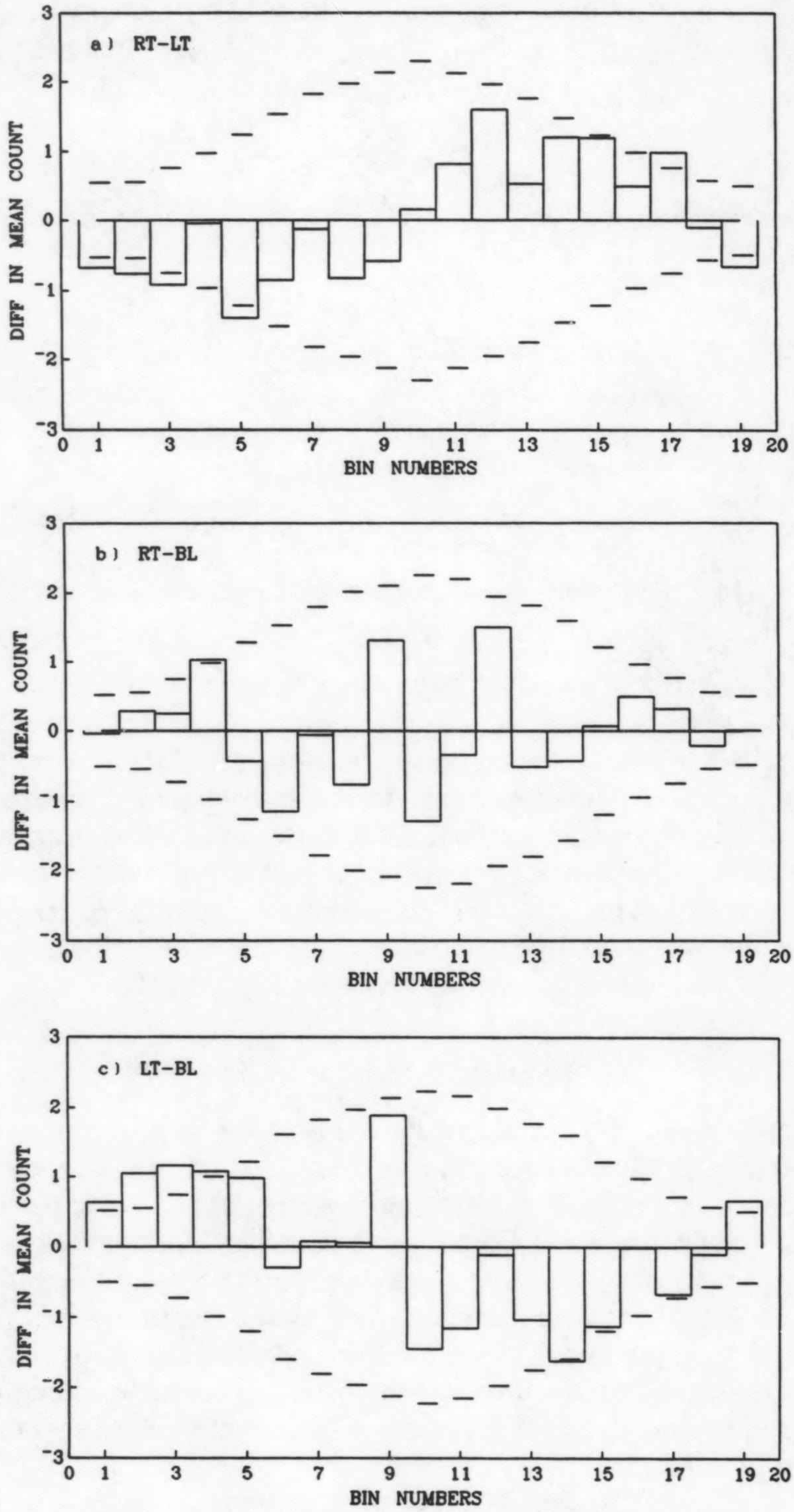

Fig. 7. RMC bin population differences: a) RT–LT; b) RT–BL; c) LT–BL.

RT–BL and two others have significant LT–BL achievements. Three operators' results are opposite to their intentions to a degree unlikely by chance, one each in RT–BL, LT–BL, and RT–LT (Table 1). Detailed statistical analyses for all the individual and compounded series are available (Nelson, Dunne, & Jahn, 1988b).

More informative than these summary results, however, is the internal consistency of many of the individual cumulative deviation graphs, wherein the characteristic signatures of performance become apparent. Several of these cases display virtually linear slopes of effect superimposed on the background of statistical fluctuations, reflecting individual preferences for a particular direction of intention, and establishing individual scales of achievement. In a few cases, the RMC signatures are qualitatively similar to those achieved by the same operators on the REG experiment, suggesting that the overall phenomenon may be less device-specific than operator-specific.

Secondary Parameters

Individual operator sensitivities to secondary parameters, such as the mode of instruction, or the feedback display being on or off, are also characteristically varied. In some cases, an operator's performance may be categorically different in the volitional protocol than in the instructed, or when live feedback is provided or denied. Yet other operators seem insensitive to these options. As mentioned, the tendency toward larger variances in the RT and LT data is not uniformly shared by all operators, and the individual bin population patterns vary considerably from one operator to another (Nelson, Dunne, & Jahn, 1988b). All of this is consistent with the individual operator variations found in the REG studies, and suggests that performance is in some way related to characteristics of personal consciousness, as well as to physical aspects of the devices and processes with which they are interacting.

One other finding of the REG studies that seems to carry over to the RMC results is the departure of some baseline data from fully chance behavior (Jahn, Dunne, & Nelson, 1987; Jahn, Nelson, & Dunne, 1985). Although, for all the reasons mentioned earlier, RMC data processing is restricted to differential criteria, examination of the individual operator raw data nonetheless suggests that in several cases the RT–BL or LT–BL differences may be attributable to unusually high or low baseline trends. As one example, the right-going efforts of operator 42 have a grand mean of 10.017, which is actually *below* the grand baseline mean for all operators of 10.023, yet is still highly significant relative to the baseline mean of 9.997. Other cases of this sort are included in Table 1.

Remote Protocol

All of the data reported and discussed above have been obtained with the operators seated on a couch a few feet before the machine, with its operation

clearly visible to them. Again in parallel to our REG research, a complementary program of experiments has been undertaken wherein operators who have already generated data in this "local" protocol, attempt the same influence on the RMC performance from remote locations. In this remote protocol, the operator arranges in advance a specific time during which a set of three runs will be initiated by members of the laboratory staff, who remain blind to the sequence of intentions. The order is chosen by the operator, who communicates this information by telephone or letter after the results are recorded, but before any feedback is given. Ten operators have so far completed 26 such series, again of 10 tripolar sets each, over distances ranging up to several thousand miles.

Of these completed series, four have shown cumulative deviations beyond the .05 chance expectation for the RT–LT difference. The concatenated results, presented in Figure 8, show remarkably similar characteristics to the local data, including the strong left-going asymmetry, and the average magnitude of the RT–LT split (.0064 bins, compared to .0057 local). The only overall distinction in the remote data is in the standard deviations of the runs, which are consistently, though not significantly *smaller* in the right and left conditions than in the baselines, compared to the reverse for the local runs. The individual bin populations are consistent with this feature, in the sense that a greater portion of the mean shift burden seems to be borne by the inner bins. These data are summarized in Table 2 and presented in detail in Nelson, Dunne and Jahn, 1988a, b.

Multiple Operator Protocol

A second major protocol variation has been undertaken to explore the effect of more than one operator simultaneously attempting to influence the

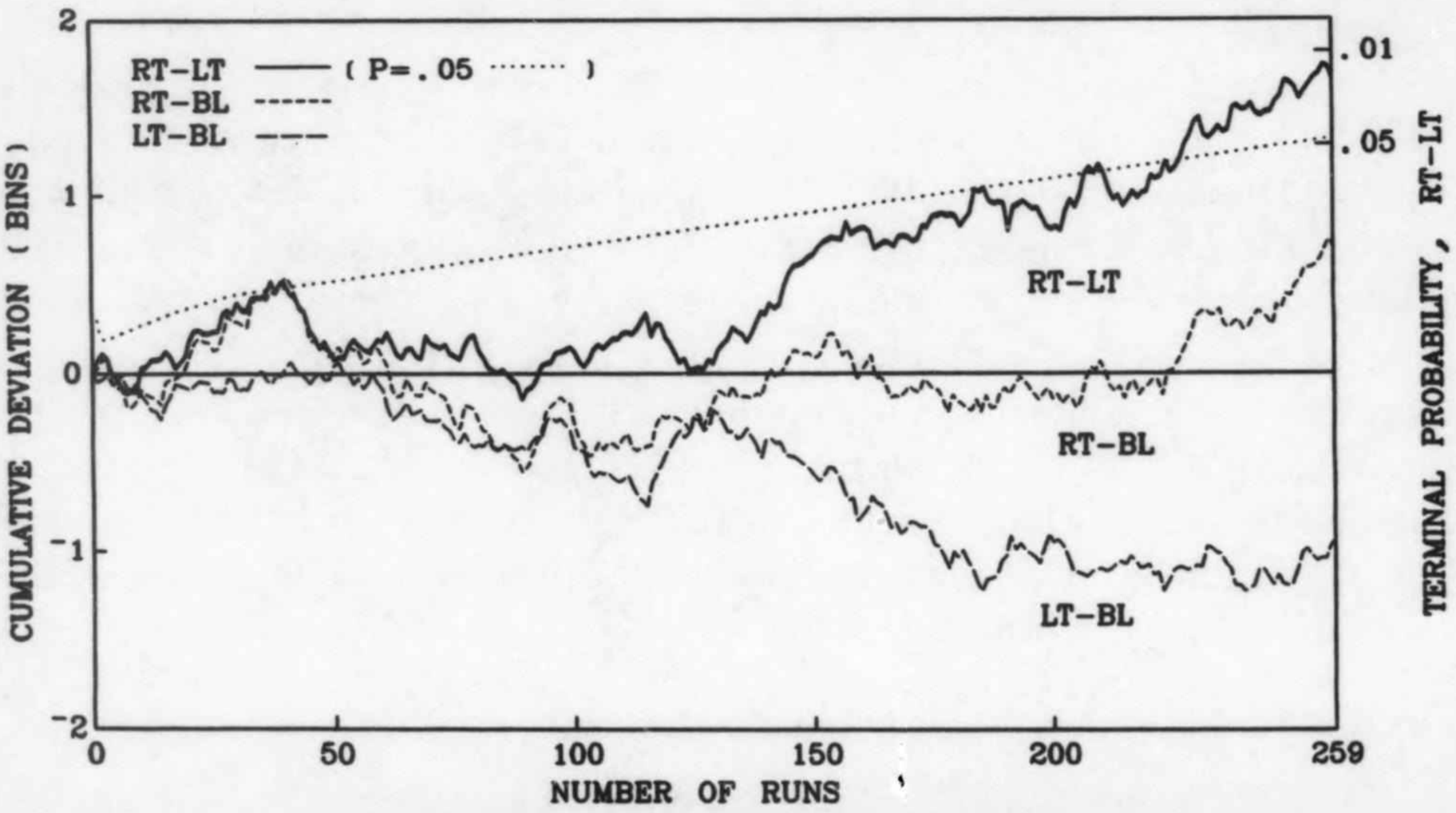

Fig. 8. Remote RMC cumulative deviations: all data.

distribution. Nine local series by seven co-operator pairs, along with two remote series by two, and one remote series by seven cooperating individuals have been performed, following the same technical procedures as for single operators. So far, these results show no overall RT–LT separation, although in contrast to the single operator local and remote patterns, both the LT- and RT-going efforts display strong *right*-going trends relative to BL, with the RT–BL values significant at the $p = .044$ level and the LT–BL opposite to intention at $p = .055$ (Figure 9). These results are summarized in Table 3 and detailed in Nelson, Dunne and Jahn, 1988a, b. Although this data base is far from sufficient to distinguish replicable patterns for given operator pairs to compare with the individual results, it already appears that no simple linear combination of the operators' influences obtains. Rather, consistent with the predictions of our theoretical model, a more complex superposition seems to be involved (Jahn & Dunne, 1986; Jahn & Dunne, 1987).

Summary

The RMC has proven to be an effective and efficient experiment for exploring the interaction of human operators with random physical systems, but in several respects the results are quite complex and will require much more study to comprehend fully. At this juncture, the findings may be summarized in the following categories:

1. There is clear evidence of a significant anomalous separation of the means of the overall right- and left-intended experimental distributions, correlated with the pre-stated goals of the human operators.
2. Compared to the concurrently generated baseline data, there is a stark and unexplained asymmetry in the directional results: Namely, the

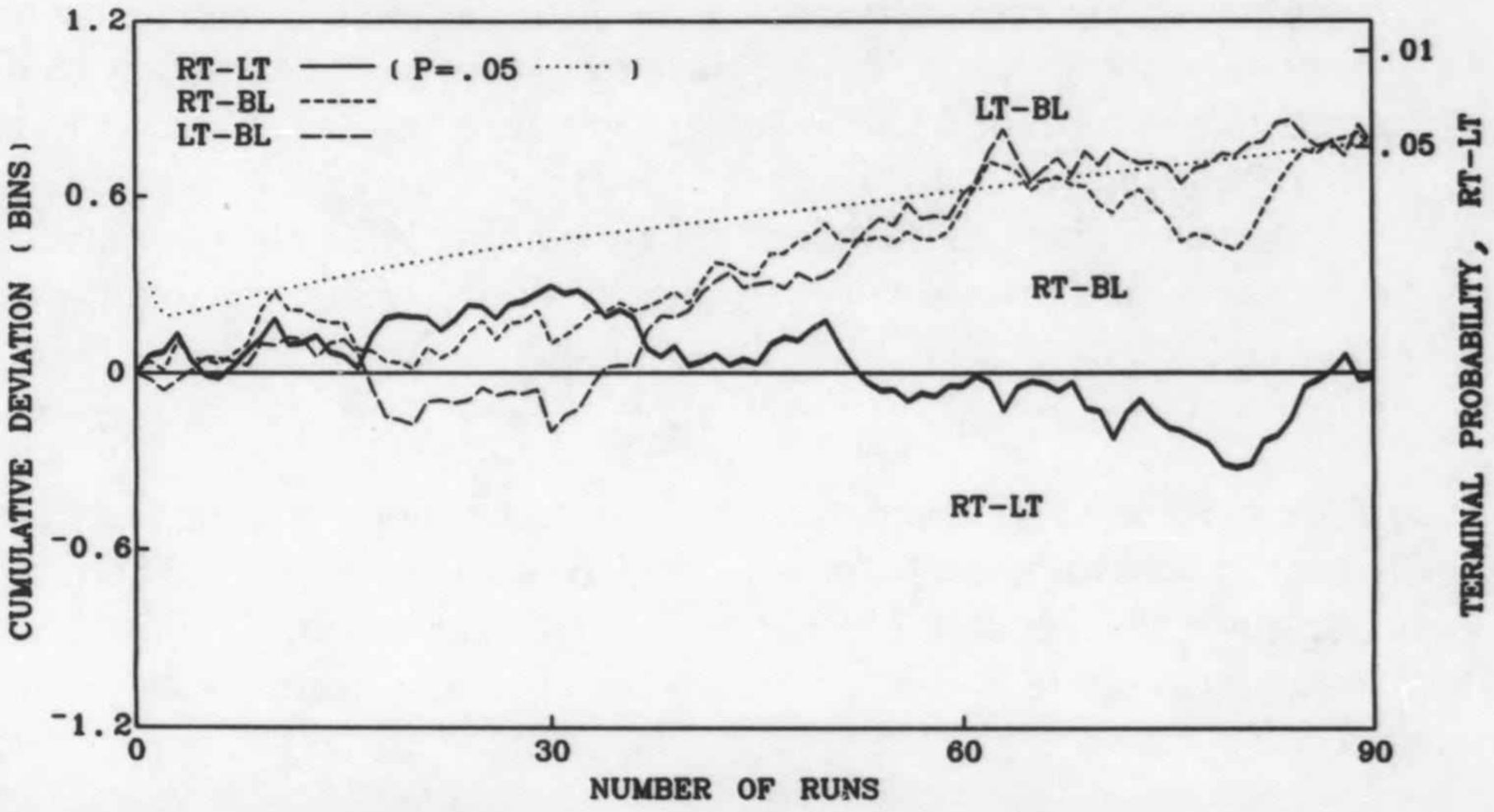

Fig. 9. Multiple operator RMC cumulative deviations: all data.

RT–LT separation resides almost totally in the LT–BL disparity; the RT and BL data are statistically indistinguishable.

3. The overall effects reflect the integration of very small shifts that compound with some regularity as the data base increases, and cannot be attributed to large contributions from any one operator.
4. Concatenation of individual bin population data indicates that the majority of bins contribute to the overall mean shift trends, but that the outer bins are more effective than those near the center.
5. Preliminary examination of the evolution of the distribution means over the course of individual runs indicates no clear pattern of concentration of effects in any particular portion of the 12-minute experimental periods.
6. The standard deviations of the experimental distributions tend to be larger for the right and left-intentioned runs than for the baselines. The LT–BL excess compounds to a highly significant value over the full data concatenation.
7. Results are clearly operator-specific, often displaying statistically repeatable characteristic trends that differ from one operator to another.
8. In several cases the individual operator performances are sensitive to secondary parameters of the experimental protocol; in other cases, they are not.
9. Operator signatures on RMC often show similarities to effects obtained by the same individuals on the micro-electronic REG and pseudo-REG experiments, implying that while the results are operator-specific, and in some cases condition-specific, they may not be so device-specific.
10. Although RMC data must be processed on a differential basis, there is good indication that for some operators the differential effects are driven as much by anomalies in the baseline as by those of the right and left efforts.
11. Separation of operator and machine by distances up to several thousand miles does not appear to inhibit the effect, or to alter its primary characteristics, except for a narrowing of the right and left run distribution variances, relative to baseline.
12. On the basis of very limited data, the cooperative efforts of two or more operators simultaneously interacting with the device appear to produce radically different results from the individual influences.

Again, more detailed substantiation of these conclusions may be found in the two Technical Reports (Nelson, Dunne, & Jahn, 1988a and 1988b).

In summary, the Random Mechanical Cascade has become an important tool in the ongoing study of the interactions of human consciousness with random physical systems. Taken in conjunction with the findings of our other experiments, the RMC results have confirmed the active role of human intention in the establishment of physical reality, and have provided

additional insights to guide the evolution of a more incisive theoretical model of the underlying processes.

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Appendix

TABLE 1A
Local, RT–LT

Opr.	# Series	# Pairs	RT Mean	LT Mean	RT–LT S.D. Diff.	RT–LT *t*-score	RT–LT Prob.	# Series $p < .05$	# Series $p < .5$	*t*-score Corr.
10	17	270	10.0297	10.0195	.0521	3.233	7×10^{-4}	7 (1)	11	1.659
14	1	19	10.0273	10.0278	.0417	−0.053	(.479)	—	—	1.059
16	4	37	10.0208	10.0035	.0509	2.067	.023	1	3	0.966
17	1	10	9.9968	9.9994	.0534	−0.157	(.440)	—	—	−1.895
20	2	20	10.0222	10.0228	.0339	−0.076	(.470)	—	1	2.553
21	1	10	9.9781	10.0124	.0388	−2.799	(.010)	— (1)	—	2.327
41	7	91	10.0242	10.0243	.0527	−0.013	(.495)	— (1)	3	0.605
42	3	30	10.0165	9.9990	.0426	2.250	.016	1	3	−0.317
44	1	20	10.0362	10.0343	.0531	0.162	.436	—	1	0.437
49	1	10	10.0291	10.0212	.0375	0.659	.263	—	1	0.091
51	1	10	10.0066	10.0002	.0307	0.655	.264	—	1	2.260
53	2	20	10.0064	10.0134	.0400	−0.782	(.222)	—	1	0.519
55	20	300	10.0283	10.0244	.0490	1.373	.085	2	13	2.994
63	1	7	10.0342	10.0126	.0358	1.600	.080	—	1	1.750
64	1	10	10.0219	10.0241	.0523	−0.135	(.448)	—	—	−0.817
66	1	10	9.9907	10.0017	.0478	−0.728	(.243)	—	—	0.034
68	2	40	10.0180	10.0221	.0508	−0.511	(.306)	—	1	0.233
69	1	11	10.0253	10.0121	.0359	1.219	.126	—	1	1.019
70	5	50	10.0206	10.0107	.0499	1.412	.082	2	3	−0.039
78	4	40	9.9919	9.9910	.0529	0.111	.456	—	2	0.550
79	1	9	10.0448	10.0329	.0666	0.535	.304	—	1	−0.072
84	3	30	10.0138	10.0058	.0549	0.799	.215	—	2	−0.316
91	1	16	10.0325	10.0395	.0383	−0.729	(.239)	—	—	1.394
93	3	31	10.0219	10.0045	.0466	2.075	.023	—	3	1.078
94	3	30	9.9977	9.9841	.0462	1.605	.060	—	3	0.550
All	87	1131	10.0229	10.0172	.0493	3.891	5×10^{-5}	13 (3)	55	6.301

TABLE 1B
Local, RT–BL

Opr.	# Series	# Pairs	BL Mean	RT Mean	RT–BL S.D. Diff.	RT–BL *t*-score	RT–BL Prob.	# Series $p < .05$	# Series $p < .5$
10	17	270	10.0328	10.0297	.0508	−0.990	(.162)	— (2)	6
14	1	19	10.0322	10.0273	.0483	−0.440	(.333)	—	—
16	4	37	10.0145	10.0208	.0496	0.778	.221	—	3
17	1	10	10.0022	9.9968	.0471	−0.361	(.363)	—	—
20	2	20	10.0014	10.0222	.0471	1.977	.031	1	2
21	1	10	10.0000	9.9781	.0423	−1.644	(.067)	—	—
41	7	91	10.0211	10.0242	.0531	0.559	.289	—	4
42	3	30	9.9968	10.0165	.0449	2.410	.011	1	3
44	1	20	10.0420	10.0362	.0457	−0.566	(.289)	—	—
49	1	10	10.0219	10.0291	.0462	0.488	.319	—	1
51	1	10	10.0047	10.0066	.0456	0.133	.448	—	1
53	2	20	10.0173	10.0064	.0589	−0.830	(.209)	—	—
55	20	300	10.0272	10.0283	.0497	0.375	.354	2 (1)	11
63	1	7	10.0207	10.0342	.0458	0.782	.232	—	1
64	1	10	10.0164	10.0219	.0476	0.364	.362	—	1
66	1	10	10.0210	9.9907	.0306	−3.142	(.006)	— (1)	—
68	2	40	10.0224	10.0180	.0453	−0.604	(.275)	—	1
69	1	11	10.0463	10.0253	.0679	−1.023	(.165)	—	—
70	5	50	10.0185	10.0206	.0572	0.269	.394	— (1)	4
78	4	40	9.9979	9.9919	.0559	−0.683	(.249)	—	2
79	1	9	10.0214	10.0448	.0649	1.079	.156	—	1
84	3	30	10.0123	10.0138	.0425	0.189	.426	—	2
91	1	16	10.0290	10.0325	.0462	0.306	.382	—	1
93	3	31	10.0142	10.0219	.0492	0.875	.194	—	2
94	3	30	10.0041	9.9977	.0557	−0.636	(.265)	— (1)	2
All	87	1131	10.0229	10.0229	.0501	0.047	.481	4 (6)	48

TABLE 1C
Local, LT–BL

Opr.	# Series	# Pairs	BL Mean	LT Mean	LT–BL S.D. Diff.	LT–BL *t*-score	LT–BL Prob.	# Series $p < .05$	# Series $p < .5$
10	17	270	10.0328	10.0195	.0536	−4.077	2×10^{-5}	6	12
14	1	19	10.0322	10.0278	.0402	−0.474	.321	—	1
16	4	37	10.0145	10.0035	.0480	−1.389	.087	1	3
17	1	10	10.0022	9.9994	.0404	−0.213	.418	—	1
20	2	20	10.0014	10.0228	.0435	2.201	(.020)	— (1)	—
21	1	10	10.0000	10.0124	.0388	1.010	(.169)	—	—
41	7	91	10.0211	10.0243	.0487	0.624	(.267)	—	3
42	3	30	9.9968	9.9990	.0529	0.232	(.409)	—	1
44	1	20	10.0420	10.0343	.0483	−0.715	.242	—	1
49	1	10	10.0219	10.0212	.0369	−0.058	.478	—	1
51	1	10	10.0047	10.0002	.0322	−0.436	.337	—	1
53	2	20	10.0173	10.0134	.0528	−0.332	.372	—	1
55	20	300	10.0272	10.0244	.0494	−0.985	.163	1	13
63	1	7	10.0207	10.0126	.0391	−0.550	.301	—	1
64	1	10	10.0164	10.0241	.0506	0.482	(.321)	—	—
66	1	10	10.0210	10.0017	.0590	−1.038	.163	—	1
68	2	40	10.0224	10.0221	.0469	−0.031	.488	1 (1)	1
69	1	11	10.0463	10.0121	.0680	−1.664	.064	—	1
70	5	50	10.0185	10.0107	.0484	−1.136	.131	—	3
78	4	40	9.9979	9.9910	.0515	−0.856	.199	—	3
79	1	9	10.0214	10.0329	.0394	0.872	(.204)	—	—
84	3	30	10.0123	10.0058	.0435	−0.823	.209	1	2
91	1	16	10.0290	10.0395	.0566	0.743	(.235)	—	—
93	3	31	10.0142	10.0045	.0440	−1.221	.116	—	2
94	3	30	10.0041	9.9841	.0466	−2.349	.013	1	3
All	87	1131	10.0229	10.0172	.0500	−3.787	8×10^{-5}	11 (2)	55

TABLE 2A
Remote, RT–LT

Opr.	# Series	# Pairs	RT Mean	LT Mean	RT–LT S.D. Diff.	RT–LT *t*-score	RT–LT Prob.	# Series $p < .05$	# Series $p < .5$	*t*-score Corr.
10	6	61	10.0171	10.0126	.0538	0.651	.259	—	4	−0.713
12	1	9	10.0020	9.9808	.0580	1.100	.152	—	1	−0.876
16	7	70	10.0047	9.9981	.0520	1.070	.144	2	4	0.260
41	1	10	10.0150	10.0126	.0262	0.288	.390	—	1	2.208
49	3	30	10.0076	9.9934	.0392	1.991	.028	1	3	1.958
68	2	20	9.9994	10.0108	.0386	−1.326	(.100)	— (1)	—	0.874
69	1	9	10.0318	10.0429	.0480	−0.691	(.255)	—	—	−2.092
78	1	10	10.0163	9.9944	.0449	1.538	.079	—	1	0.185
93	2	20	10.0258	10.0139	.0533	1.000	.165	—	2	−0.917
94	2	20	10.0075	10.0000	.0376	0.890	.192	1	1	0.368
All	26	259	10.0111	10.0047	.0482	2.139	.017	4 (1)	17	1.197

TABLE 2B
Remote, RT–BL

Opr.	# Series	# Pairs	BL Mean	RT Mean	RT–BL S.D. Diff.	RT–BL *t*-score	RT–BL Prob.	# Series $p < .05$	# Series $p < .5$
10	6	61	10.0159	10.0171	.0546	0.180	.429	—	3
12	1	9	9.9956	10.0020	.0439	0.443	.335	—	1
16	7	70	10.0013	10.0047	.0526	0.539	.296	1	3
41	1	10	10.0290	10.0150	.0491	−0.905	(.195)	—	—
49	3	30	10.0078	10.0076	.0564	−0.021	(.492)	1	1
68	2	20	10.0100	9.9994	.0545	−0.867	(.199)	— (1)	1
69	1	9	10.0488	10.0318	.0608	−0.834	(.214)	—	—
78	1	10	9.9797	10.0163	.0432	2.680	.013	1	1
93	2	20	10.0109	10.0258	.0488	1.367	.094	—	2
94	2	20	9.9984	10.0075	.0556	0.731	.237	—	1
All	26	259	10.0084	10.0111	.0533	0.826	.205	3 (1)	13

TABLE 2C
Remote, LT–BL

Opr.	# Series	# Pairs	BT Mean	LT Mean	S.D. Diff. LT–BL	LT–BL *t*-score	LT–BL Prob.	# Series $p < .05$	# Series $p < .5$
10	6	61	10.0159	10.0126	.0501	−0.503	.309	—	3
12	1	9	9.9956	9.9808	.0420	−1.057	.161	—	1
16	7	70	10.0013	9.9981	.0457	−0.597	.276	1	4
41	1	10	10.0290	10.0126	.0515	−1.011	.169	—	1
49	3	30	10.0078	9.9934	.0491	−1.610	.059	—	3
68	2	20	10.0100	10.0108	.0468	0.084	(.467)	—	1
69	1	9	10.0488	10.0429	.0395	−0.446	.334	—	1
78	1	10	9.9797	9.9944	.0516	0.904	(.195)	—	—
93	2	20	10.0109	10.0139	.0385	0.351	(.365)	—	1
94	2	20	9.9984	10.0000	.0405	0.177	(.431)	—	1
All	26	259	10.0084	10.0047	.0462	−1.278	.101	1	16

TABLE 3A
Multiple operators, RT–LT

Opr.	# Series	# Pairs	RT Mean	LT Mean	RT–LT S.D. Diff.	RT–LT *t*-score	RT–LT Prob.	# Series $p < .05$	# Series $p < .5$	*t*-score Corr.
220	2	20	10.0133	10.0190	.0446	−0.573	(.287)	—	1	1.115
244	1	10	10.0248	10.0170	.0483	0.511	.311	—	1	−1.442
251	1	10	10.0187	10.0079	.0343	0.994	.173	—	1	1.247
252	1	10	10.0168	10.0159	.0455	0.066	.474	—	1	−0.456
277	1	10	9.9923	10.0195	.0430	−2.001	(.038)	— (1)	—	0.091
284	2	20	10.0136	10.0041	.0544	0.775	.224	—	2	−0.401
299	1	10	10.0093	10.0106	.0647	−0.064	(.475)	—	—	−1.241
All	9	90	10.0128	10.0130	.0486	−0.034	(.487)	— (1)	6	−0.251
2 Operators, remote, RT–LT										
294	2	20	10.0006	10.0017	.0470	−0.098	(.462)	—	1	−0.881
7 Operators, remote, RT–LT										
750	1	10	10.0016	10.0001	.0479	0.101	.461	—	1	−0.361

TABLE 3B
Multiple operators, RT–BL

Opr.	# Series	# Pairs	BL Mean	RT Mean	RT–BL S.D. Diff.	RT–BL *t*-score	RT–BL Prob.	# Series $p < .05$	# Series $p < .5$
220	2	20	10.0011	10.0133	.0366	1.483	.077	1	1
244	1	10	10.0114	10.0248	.0499	0.849	.209	—	1
251	1	10	10.0102	10.0187	.0619	0.434	.337	—	1
252	1	10	10.0018	10.0168	.0501	0.947	.184	—	1
277	1	10	9.9785	9.9923	.0288	1.518	.082	—	1
284	2	20	10.0129	10.0136	.0515	0.057	.478	— (1)	1
299	1	10	10.0099	10.0093	.0525	−0.036	(.486)	—	—
All	9	90	10.0044	10.0128	.0463	1.723	.044	1 (1)	6
2 Operators, remote, RT–BL									
294	2	20	9.9958	10.0006	.0460	0.469	.322	—	2
7 Operators, remote, RT–BL									
750	1	10	10.0246	10.0016	.0272	−2.677	(.013)	— (1)	—

TABLE 3C
Multiple operators, RT–BL

Opr.	# Series	# Pairs	BL Mean	LT Mean	LT–BL S.D. Diff.	LT–BL *t*-score	LT–BL Prob.	# Series $p < .05$	# Series $p < .5$
220	2	20	10.0011	10.0190	.0421	1.896	(.037)	—	—
244	1	10	10.0114	10.0170	.0450	0.392	(.352)	—	—
251	1	10	10.0102	10.0079	.0562	−0.129	.450	—	1
252	1	10	10.0018	10.0159	.0678	0.656	(.264)	—	—
277	1	10	9.9785	10.0195	.0394	3.294	(.005)	— (1)	—
284	2	20	10.0129	10.0041	.0468	−0.838	.206	—	1
299	1	10	10.0099	10.0106	.0569	0.040	(.485)	—	—
All	9	90	10.0044	10.0130	.0505	1.613	(.055)	— (1)	2
2 Operators, remote, LT–BL									
294	2	20	9.9958	10.0017	.0504	0.520	(.305)	—	—
7 Operators, remote, LT–BL									
750	1	10	10.0246	10.0001	.0524	−1.480	.087	—	1

Journal of Scientific Exploration, Vol. 2, No. 2, pp. 181–201, 1988
Pergamon Press plc. Printed in the USA.

0892–3310/88 $3.00+.00
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Archaeological Anomalies in the Bahamas

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Abstract—Controversial claims have been made for the presence of anomalous underwater archaeological sites in the Bahamas by a number of investigators. The proponents emphasize extraordinary explanations for the anomalies and tend to bypass the scientific journals in favor of popular presentations with little scientific rigor. The skeptics debunk selected claims for some of the sites, do not adequately address the prominent anomalous aspects, and attempt to fit explanations with which they disagree into a general category of cult archaeology. This paper reviews the work of the proponents and skeptics, discusses some of the reasons why they are unable to reach agreement, and addresses the relevance of the controversy to the response of the archaeological community to extraordinary claims.

Introduction

Since the 1960's numerous claims have been made for the presence of underwater archaeological sites in the Bahamas which contain the remains of cultures capable of constructing large stone walls and buildings. Generally, mainstream archaeologists have not taken these claims seriously for two reasons. First, the sites themselves are anomalous because most of the area of the Bahama banks has been underwater since at least 8000 B.C., long before the appearance of the Mayas or any other high civilization in the Americas. Discovery of submerged cities would force a major reconstruction of American prehistory. Second, many of the proponents of these sites have conducted their research in an unorthodox manner, and have published in popular books and magazines rather than in scientific journals. In some cases they have linked themselves with psychics, UFOs and stories of the Bermuda Triangle, casting further doubt on their credibility from the point of view of the archaeological establishment.

As in other areas of anomalies research, proponents and skeptics have often drawn opposite conclusions from the same evidence. The evidence for

I thank Robert Jeffries, James Windsor, John Gifford, and three anonymous reviewers for their helpful comments on earlier drafts of this manuscript. An earlier version of this paper was presented at the Third Annual Meeting of The Society for Scientific Exploration, Princeton, NJ, October, 1984. Requests for reprints should be sent to Douglas G. Richards, Atlantic University, P.O. Box 595, Virginia Beach, VA 23451.

and against the sites has yet to be presented in a balanced way, and the treatments of the subject to date have generated more heat than light. The wildest claims of the proponents have inspired attacks and charges of cultism by the skeptics, that contribute little to resolving the essential issue of whether anomalous findings do, in fact, exist.

Archaeology differs from many scientific fields in that amateurs have played, and continue to play, a major role, and are taken seriously by academic archaeologists. In contrast to many other fields, misguided amateurs can do irreparable damage such as destructive treasure hunting. Thus there is good reason not to dismiss amateurs as "pseudoscientists," even though they may not entirely follow rigorous academic standards, but rather to educate them. Despite the diatribes by some archaeologists (e.g., McKusick, 1982, 1984) against "psychic" archaeology and cultism, public interest in the subject has remained high, and some archaeologists see this as an opportunity for education of amateurs in archaeological methods, rather than as a cult to be debunked.

Unlike some popular anomalies in other fields, the Bahamas sites do not exhibit the "shyness" property, and are available for anyone to investigate. In this paper I will discuss the anomalous sites, the existing evidence for and against their man-made origin, and the ancillary conflicts which have diverted attention from the scientific issues. In the conclusion I will consider the response of the archaeological community to extraordinary claims, and the appropriateness of the label "cult archaeology."

The Anomalous Bahamas Sites

What are the sites being considered in this paper? They are purported to be the remains of high cultures, on the level of the Mayas, located underwater on the Bahama banks. These sites were presumably built when the banks were above water at a time of lower sea level, perhaps 10,000 years ago (c.f., Milliman & Emery, 1968). Much of the controversy has focused on a single site, known as the "road" or "wall" site near the island of Bimini, 45 miles east of Miami, Florida. A small amount of work has been done on a few other sites, including a "temple" near Andros island, and "columns" near Bimini. Numerous other roads, walls, geometrical patterns and pyramids have been described, most from aerial sightings, but have in general not been confirmed by independent sightings, nor is it possible to determine the exact locations from the published reports (Berlitz, 1984; Valentine, 1969, 1973, 1976).

In contrast, the sites of ancient human occupation discovered so far by mainstream archaeologists are those of Lucayan Indians, the inhabitants of the Bahamas before the arrival of Columbus. These sites are only a few hundred years old (Sears & Sullivan, 1978). There is no evidence at all of any higher culture. However, these archaeologists have considered only sites on land, most likely because such sites are more easily accessible than

submerged sites, and because the submerged portion of the Bahama Bank could not have been occupied in the last few hundred years.

The presence of early man on the Bahama Bank has become increasingly likely in the light of recent evidence of ancient occupation of Florida and the Caribbean islands. Evidence of human occupation dating to at least 12,000 years ago has been found submerged in sinkholes in Florida (Clausen et al., 1979). Shell middens (piles of accumulated waste) and other remains have been found dating back several thousand years in Cuba and Hispaniola, the two closest islands to the Bahamas (Cruxent & Rouse, 1969). Mammoth teeth have been found in ancient coastal areas submerged by rising water from the melting of the glaciers (Whitmore, Emery, Cooke, & Swift, 1967), and archaeologists have searched for artifacts of man on the U.S. continental shelf (Edwards & Emery, 1977). It is not unreasonable to speculate that the Bahama Bank was occupied when it was above water, although no evidence of such occupation has yet been accepted by mainstream archaeologists.

The problem is not so much with the concept of ancient man in the Bahamas as it is with the nature of the probable cultural level. Many ancient man sites elsewhere in the Caribbean consist of shell middens which would be difficult to find under water. The people looking for remains of higher cultures are looking for buildings and roads, evidence much easier to find under water if it exists. However, most mainstream archaeologists would not even consider such a search because they do not believe such a civilization ever existed. Thus it is not surprising that mainstream archaeologists have not made any of the purported discoveries.

Historically, underwater exploration in search of ancient man in these areas has been stimulated by two sources outside mainstream archaeology: psychics and transatlantic diffusionists. The most visible psychic source was the work of Edgar Cayce (1877–1945), an American psychic who spoke of the Bahamas as the last portion of the ancient civilization of Atlantis left above the waves. According to Cayce, Atlantis, which had been inundated in approximately 10,000 BC, would begin to rise again in '68 or '69 (E. E. Cayce, 1968). Although most archaeologists have had little regard for this "psychic archaeology," a geologist in 1958 did a substantial amount of research suggesting that at least some of Cayce's predictions were surprisingly consistent with recent geological discoveries, and that Bimini was not an altogether unreasonable location in which to look for underwater ruins. The geologist, whose work is presented in H. L. Cayce (1980), wished to remain anonymous, fearing damage to his career if his work in this controversial area were to become known.

The second source of interest in the Bahamas is harder to define, but appears to be independent of the Cayce material and more related to the hypothesis of ancient transatlantic cultural diffusion. The diffusionist point of view is much broader in scope than the strict Atlantis hypothesis. Sources for American civilization could include virtually any Old World culture,

from any period in history. Marx (1971), for example, brings up the possibility of Phoenicians. The most visible of the Bahamas explorers in this category were J. Manson Valentine, a zoologist and amateur archaeologist, and Dmitri Rebikoff, an underwater explorer and photographer.

My discussion of the history of this controversy will focus primarily on the "road" site near Bimini, which has been the most extensively studied site, and will briefly touch on the "temple" and on the problem of finding and investigating anomalous features.

The Bimini Road Site

The road site was discovered in 1968 by J. Manson Valentine, and has been described in articles by Lindstrom (1980, 1982), Marx (1971), Rebikoff (1972, 1979), and Valentine (1969, 1973, 1976), and books by Ferro and Grumley (1970) and Zink (1978). Skeptical articles describing the site include those by Gifford (1973b), Gifford and Ball (1980), Harrison (1971), McKusick and Shinn (1980), and Shinn (1978). The site consists of rows of stone blocks about one-half mile north of Paradise Point, North Bimini. The longest row of blocks is about 1600 feet long, with a J-shaped bend at one end. Although from the air the formation looks relatively uniform, the blocks themselves vary considerably. In some short sections, the blocks are generally square or rectangular, and give the very strong impression of a human-constructed wall. The Rebikoff articles contain an underwater photomosaic of the best example of the wall-like formation. In other areas of the site, however, the block-like structure grades into apparently random fracturing of the stones resembling natural beachrock deposits. The site was initially thought to be a buried wall, but it was soon established that it was composed of only a single layer of blocks, and it became known as the "road." Explorers on both sides of the controversy agree that it was unlikely to have been an actual road, but beyond that, the interpretations vary widely.

The key point in contention is whether the stones are beachrock, fractured naturally in place, or blocks carved and positioned by human agency. Beachrock is a common formation in the Bimini area, and consists of slabs of cemented sand which typically form along the shore (Scoffin, 1970). Beachrock naturally fractures as the sand underneath shifts, and can occasionally form rectangular blocks. Since beachrock forms along the beach, a submerged line of eroded blocks oriented parallel to the current beach would be highly suggestive of a beachrock deposit formed at a time of lower sea level. The skeptics feel that the road site is an example of this type of formation. The proponents of the man-made interpretation point to several anomalies which they feel argue against the natural beachrock explanation. These include an orientation not parallel to the beach and unusually geometrical construction.

From almost the day it was discovered, the site has generated controversy. According to Marx (1971), rather than getting caught up in the Atlantis controversy, Valentine and Rebikoff had hoped to keep the discovery of the road a secret. However, lacking capital for more intensive explorations, they joined forces with aircraft pilots Robert Brush and Trigg Adams, explorers who had been inspired by Cayce. Together they founded the Marine Archaeology Research Society (MARS). MARS expeditions in 1969 discovered several more sites of interest near the road, including clusters of cement and marble columns in several locations.

By 1970, the owners of the land on shore nearest the road site had obtained an exclusive excavation permit from the Bahamas government. They proceeded to exclude both the MARS group and Valentine and Rebikoff, who by this time had broken away from MARS and joined forces with Marx, an underwater explorer and archaeology editor for Argosy magazine. The Valentine group was not allowed to excavate the site, although they were allowed to dive on it (Marx, 1971). This "claim jumping" by the landowners on shore angered the original MARS explorers, who were unable to obtain permits for further work and confirm their interpretation of the site (Adams, 1971).

With the permit holder's permission, two groups closer to the mainstream were permitted to study the site in detail. They were led by Wyman Harrison, a geologist from Virginia Beach, and John Gifford, a graduate student in marine geology at the University of Miami.

Harrison (1971), published a short paper in the prestigious British journal *Nature,* in which he concluded that the stones were natural beachrock, and that the columns had come from a ship. Harrison's paper was the first response from the scientific community to the publicity about Atlantis. It begins by referring to the popular articles published by Valentine, Marx and Rebikoff, and the book by Ferro and Grumley. The Harrison paper, however, consists primarily of skeptical speculation about the possible natural origins of the road site, and presents no real data. Harrison also discusses the columns, and makes a somewhat better case that they are cargo left from a shipwreck. He does not address reports of columns arranged in a circle at a distance from shore (c.f., Zink, 1978), though, and only describes the jumble of columns near the Bimini entrance point. Harrison was not an archaeologist, and made no attempt at a comprehensive archaeological study of the purported anomalies. It seems likely that the paper was published solely to debunk the accounts in the popular press.

The study by Gifford, on the other hand, was far more thorough. Gifford, in work for his Master's thesis in marine geology, did extensive investigation of a small section of the road site, supported in part by the National Geographic Society. Unlike Harrison, Gifford measured and mapped the blocks, and performed a detailed analysis of the composition of the blocks. Gifford became convinced that the blocks had been formed naturally, and advanced this point of view in a lengthy paper (Gifford & Ball, 1980), and

in a short journal note in response to the Rebikoff (1972) article (Gifford, 1973b).

Despite his skepticism about the road site, Gifford joined forces with two amateur archaeologists, Talbot Lindstrom and Steven Proctor, the founders of the Scientific Exploration and Archaeological Society (SEAS). SEAS espoused the transatlantic diffusionist viewpoint, and was founded with the intention of gathering high-quality evidence of ancient transatlantic contact. The SEAS group returned in 1972 and 1979 and began exploration of the other promising sites in the area. They excavated some of the columns, and found them over a far wider area than reported by Harrison. From aerial surveys they discovered a linear feature off North Bimini, that, in contrast to the road site, made an oblique angle with the beach and cut across the submerged beach lines. Underwater, the feature consisted of regularly spaced piles of stones, extending for over a mile and a half across the sea bottom. They dubbed the feature "Proctor's Road."

The SEAS findings, more anomalous than those reported in the mainstream literature by Harrison and Gifford, were published only in *The Epigraphic Society Occasional Publications* (Lindstrom, 1980), a journal generally presenting the extreme transatlantic diffusionist point of view, and not taken seriously by the mainstream archaeological community. SEAS conducted several expeditions to Bimini as well as to sites in Central America. Once again, the work was not reported in the scientific literature, but rather in the *Explorers Journal* (Lindstrom, 1982). Still, this presentation was far more professional than the popular articles by others, but has not been referred to in the literature on either side of the controversy.

The next major player on the scene was David Zink, an English professor from Lamar University in Texas. Zink, who intensively studied the road site from 1974 to 1979 in a series of expeditions, was also an amateur, self-taught in archaeology. He enlisted the help of several geologists and archaeologists, however, and his work had the potential of being the definitive study of the road site. His supporters included the President of the Bahamian Senate, who established The Bahamas Antiquities Institute with Zink as Director of Research. He won the Explorer of the Year award from the International Explorers Society in 1976, and made numerous television appearances. In the end, however, his work had the effect of further polarizing the scientific community, and led in part to the McKusick (1982, 1984) attacks on psychic archaeology. His 1978 book, *The Stones of Atlantis,* not only concluded that the road was the ruins of an Atlantean structure, but that extraterrestrials from the Pleiades had been involved in its construction. Placing himself in the camp of the UFO and Bermuda Triangle advocates, he alienated himself even from the supporters of the Edgar Cayce Atlantis theory, and, by 1980, was unable to obtain sufficient funding and ceased his research. In contrast to the articles by his detractors, none of his work was ever published in the professional journals, and the detailed survey that he

did accomplish is not clearly described in his book for comparison with the debunkers' accounts. Zink did, however, present some of his work to the scientific community at the annual meeting of the Society for Historical Archaeology and Conference on Underwater Archaeology (Mahlman & Zink, 1982).

Zink's major contribution was a detailed mapping of the entire site, far more than the few blocks studied by Gifford. His data confirm Gifford's conclusions: There is only one course of blocks, arrayed in rows approximately parallel to shore. He did, however, turn up several additional anomalies which have not been addressed by his critics. The longer lead of the road takes a 90° turn perpendicular to shore, an unusual formation for beachrock. There is a fracture in the seabed running under the blocks at a different angle from the joints in the blocks, an anomaly for the hypothesis of natural fracturing. In several cases, large blocks are resting on smaller blocks beneath, although this does not appear to be a true second course of blocks. Zink was also the only investigator to address another important archaeological issue: the presence or absence of artifacts in addition to the blocks themselves. Although he recovered a stone building block and a possible marble sculpture, the context was not preserved and the artifacts themselves were undatable. There is no reason to assume that they were associated with the blocks. Unfortunately, Zink's popular presentation made it unlikely that any mainstream archaeologist would take his work seriously.

The remaining study of interest was by Eugene Shinn, an orthodox geologist (McKusick & Shinn, 1980; Shinn, 1978). One of the articles reporting his work (McKusick & Shinn, 1980) notes that he is a member of the U.S. Geological Survey (USGS), but that the study was at his own expense and not part of a USGS-sponsored project. That even a skeptical article has a disclaimer of this sort is indicative of the controversial nature of the subject. Shinn addressed the important question of whether or not the blocks were arranged by man. He reasoned that blocks which had formed in place and fractured would have identical sediment bedding in adjacent blocks. Blocks which had been moved by human agency would be likely to show different bedding patterns. He drilled cores into adjacent blocks, a procedure attempted with little success by Zink as well. X-ray photographs of some of Shinn's cores showed that the blocks had similar bedding patterns, and the bedding planes and dip direction convinced him that they had been formed as beachrock on a sloping beach and fractured in place. In other cores, taken from the north part of the site, large pebbles in the rock prevented formation of laminations, but Shinn concluded that the similarity from block to block was further evidence that they had fractured in place (Shinn, 1978).

In another part of Shinn's project, Carbon-14 dates were taken on the material in several cores. Although the spread was quite wide, the dates ranged around 3000 years before the present. This recent date was inter-

preted to show that the blocks could not have been formed in the time frame required by the Atlantis hypothesis. It is worth noting, however, that the dates *are* consistent with the transatlantic diffusion by Phoenicians hypothesis, a point of view not discussed by McKusick and Shinn. Since sea level curves (e.g., Milliman & Emery, 1968) indicate that the site was long under water by this date, the Carbon-14 finding is also anomalous. McKusick and Shinn explain it away by saying that a substantial amount of sand (seven to nine feet) eroded from underneath the blocks, submerging them to their present depth. Considering the precise linear arrangement and relative lack of disturbance of the site (c.f. pictures by Rebikoff, 1972 and Zink, 1978), this explanation also does not seem parsimonious. Given the wide range of the Carbon-14 dates, perhaps the most likely explanation is that the samples were contaminated by relatively recent biological intrusion. Zink (1978), in an appendix to his book, provides a balanced and even somewhat skeptical discussion of the wide spread of dates from Gifford's work and the sea level curves of others. In their *Nature* publication, McKusick and Shinn (1980) were so intent on debunking that they failed to address anomalies generated by their own work, and devoted much of the article to an attack on transatlantic diffusionists and the Cayce "religious cult." They interpret the Bimini controversy as a "clash between scientific interpretation and religious dogma." As I have shown, the proponents of these sites as significant anomalies are a more heterogeneous group than McKusick and Shinn would imply. Far from being a coherent dogmatic position, the hypotheses of the proponents range from Atlantis to Phoenicians to extraterrestrials. In the midst of the competing claims and hypotheses, however, the question of the nature of the anomalies has still to be addressed objectively.

The contradictory descriptions of the site by those investigators who believe it to be man-made and the skeptics who believe it to be a natural deposit illustrate the ways in which the same evidence can be arranged to fit preconceived hypotheses. The investigators of the site who believe it to be man-made emphasize the regular block-like structure, whereas the skeptics emphasize the random character of the rest of the site. Similarly, the skeptics describe the site as being parallel to the beach line, and therefore likely to be a submerged beachrock deposit, whereas the believers note that it is not quite parallel to the present beach line, and therefore likely to be a man-made formation. Other characteristics which might discriminate between natural and man-made features also receive different interpretations. Cores through the blocks reveal similar, but not identical layers. For the skeptics this is further evidence that the blocks were all originally part of a single deposit which fractured in place. For the believers the slight differences between the blocks are evidence that they were assembled from different sources. There is not even general agreement on the composition of the blocks. The skeptics (e.g., McKusick & Shinn, 1980), as well as Zink (1978), say that they are composed of beachrock, whereas Rebikoff (1979) describes micrite, a different type of rock. Some of the more puzzling evidence, such

as the fact that some of the large blocks are resting on smaller stones (Rebikoff, 1979; Zink, 1978), has received no comment from the skeptics.

Figures 1a and 1b illustrate the complexity of the road site area. There are numerous submerged beach lines and other features criss-crossing the area. Note the 90° curve in the road, and "Proctor's Road" cutting diagonally across the submerged beach lines.

The Andros "Temple" Site

The temple site was discovered from the air in 1968 near the island of Andros by pilots Robert Brush and Trigg Adams. It is a stone, foundation-like structure approximately 60 × 100 ft. Later in 1968, J. Manson Valentine and Dmitri Rebikoff investigated the site, and described 3-foot thick, skillfully worked limestone walls rising 2 feet above the ocean bed, and resembling the Mayan "Temple of the Turtles" in floor plan (Zink, 1978). Marx (1971) has described several more sites and concluded that they were man-made. Marx noted that even before Valentine and Rebikoff had explored their site, the press was already reporting that an Atlantean temple had been found. Due to the relative inaccessibility of these sites, and the uncertainty concerning their locations, no one else has made a careful study of them. The Atlantean temple story circulated for nearly 10 years largely unchallenged. In 1976, David Zink, travelling with the Cousteau group during the filming of "Calypso's Search for Atlantis," examined the Valentine-Rebikoff site, and found it to be composed of much smaller stones, with no evidence of additional stones below the sea bottom. That, and the claim by a man from Andros that he helped build it as a sponge pen in the 1930's, convinced Zink that it was unlikely to be of ancient origin (Zink, 1978). McKusick (1982) gives Zink credit for some good judgement in this case. Nevertheless, the site, and others like it, has still not been studied by a professional archaeologist, and Zink's observations differ from those of Marx as well as Valentine.

Searching for Anomalies in the Bahamas

One of the central problems with investigation of underwater anomalies is the difficulty of relocating sites. The road site is within a half mile of shore and easily accessible, but the others have not been available for study. In 1983, in an effort to make more reliable maps of other bottom features on the Bahama bank, I obtained photographs from Landsats 1 and 4, two satellites which provide photographic coverage of the Bahamas from an altitude of about 400 miles. The size of the smallest picture element in the Landsat 4 photograph was about 30 meters, and in the Landsat 1 photograph about 100 meters. This resolution was not good enough to identify buildings, either modern ones on land or ancient ones under water. Despite the low resolution, however, several anomalous large-scale geometric pat-

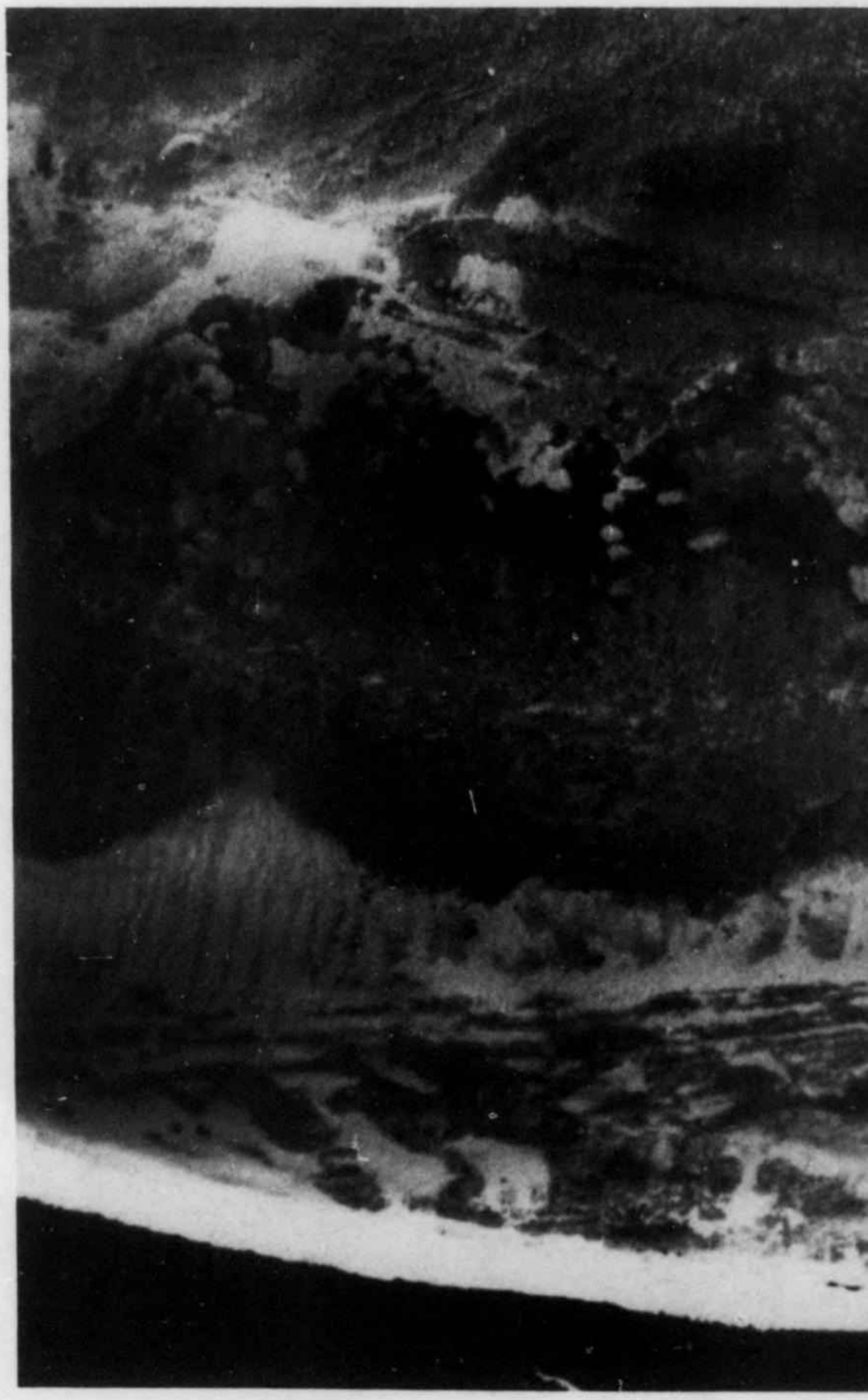
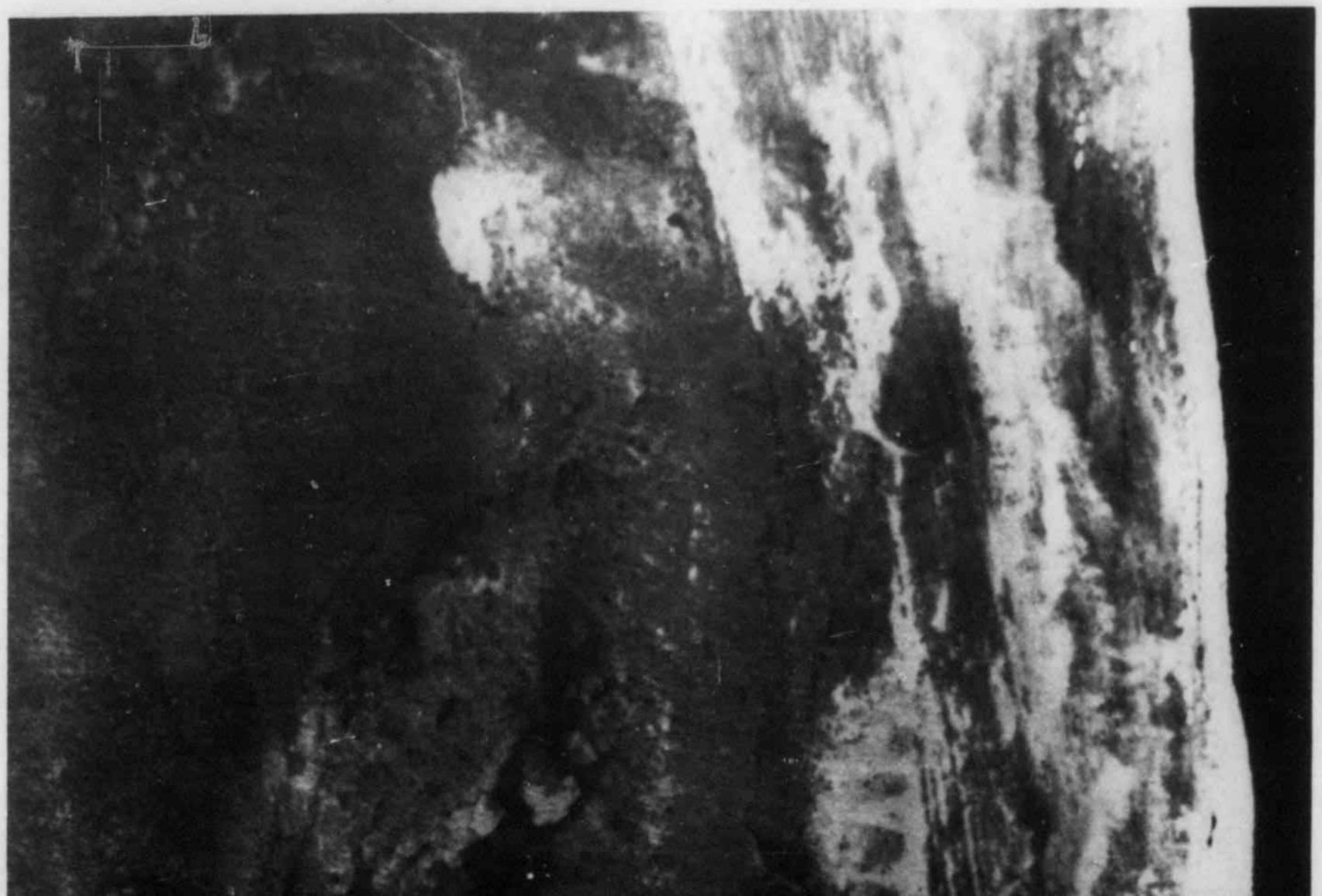
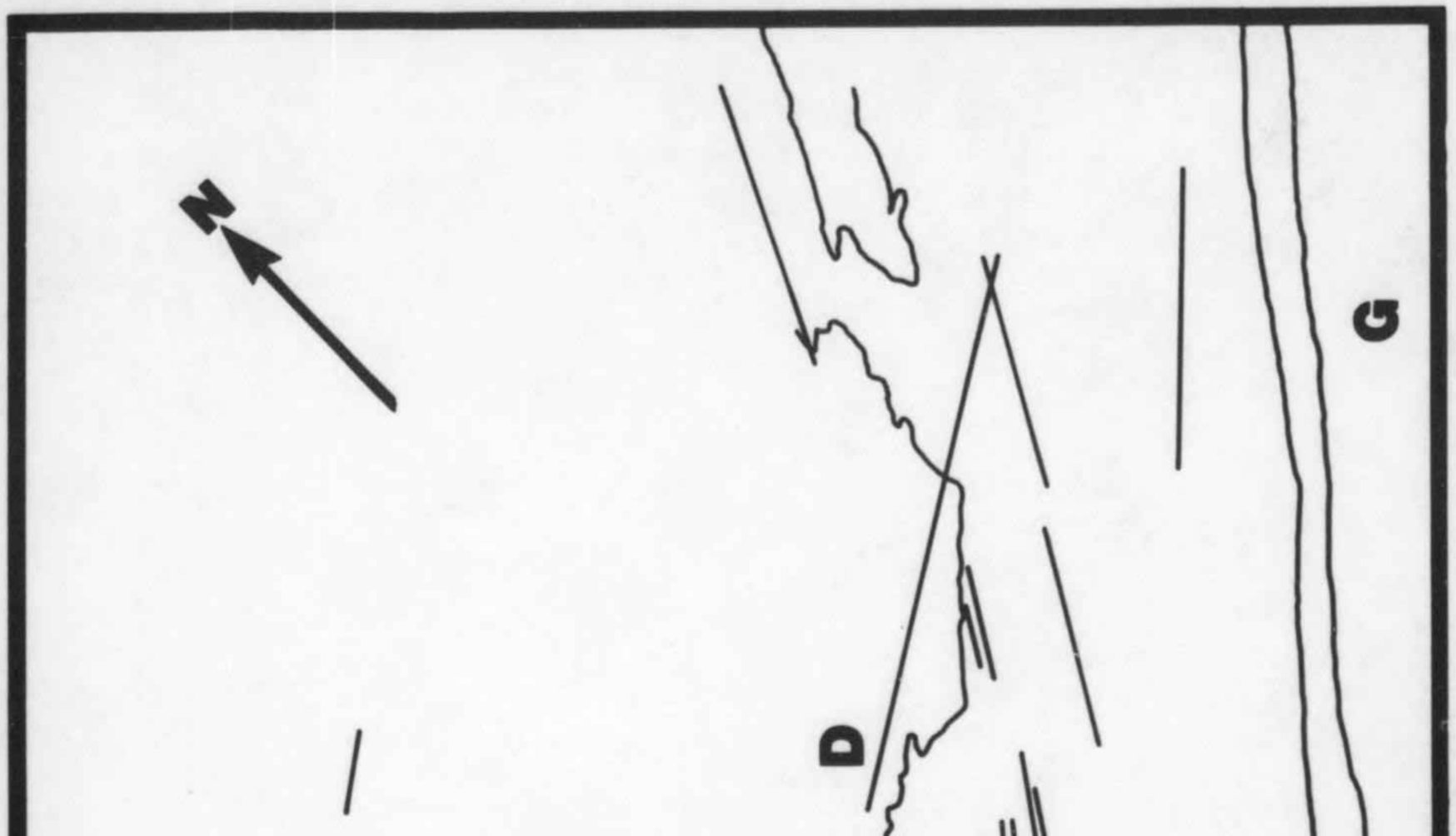


Fig. 1a. Vertical aerial photograph from about
road site; photograph provided by the



about 8000 feet altitude showing the area around the
y the Edgar Cayce Foundation.

Fig. 1b. Map drawn from the photograph, highlighting fea
road site; B, the parallel rows of stones; C, the sho
blocks; D, "Proctor's road" (note how it cuts diag
E, ancient beach lines; F, current beach; G, fores



ing features including: A, the 90° bend in the
he short section containing the most regular
ts diagonally across the ancient beach lines);
, forested shore. Scale mark is ¼ mile.

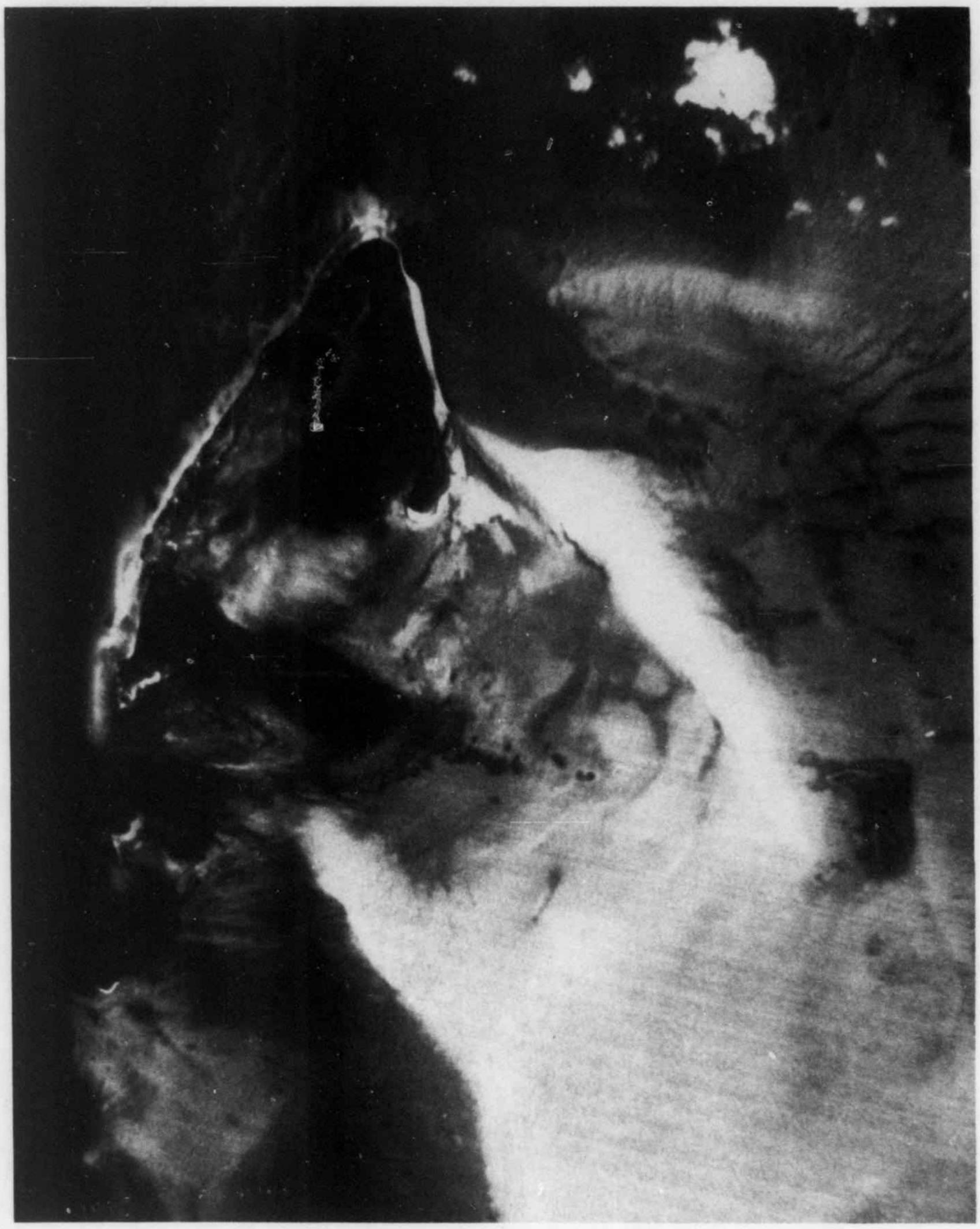


Fig. 2a. Thematic mapper satellite photograph of Bimini area from Landsat 4, blue-green band, 14 January 1983; photograph provided by the U.S. Geological Survey.

terns were clearly visible. These included some roughly pentagon-shaped figures, rectangles, and a figure with a right-angle corner and mile-long sides, oriented very close to true north–south, east–west. Figure 2a is a Landsat 4 photograph showing some of these anomalous features near Bimini (see also Figure 2b, a map drawn from the photograph).

Comparison of the photographs with Valentine's work revealed that at least one of the anomalous features was not a new discovery, and that satellite mapping offers a major improvement in the ability to accurately relocate sites. Valentine (1976) reported and attempted to map one of the same rectangles from a low-altitude aircraft. His location was incorrect by

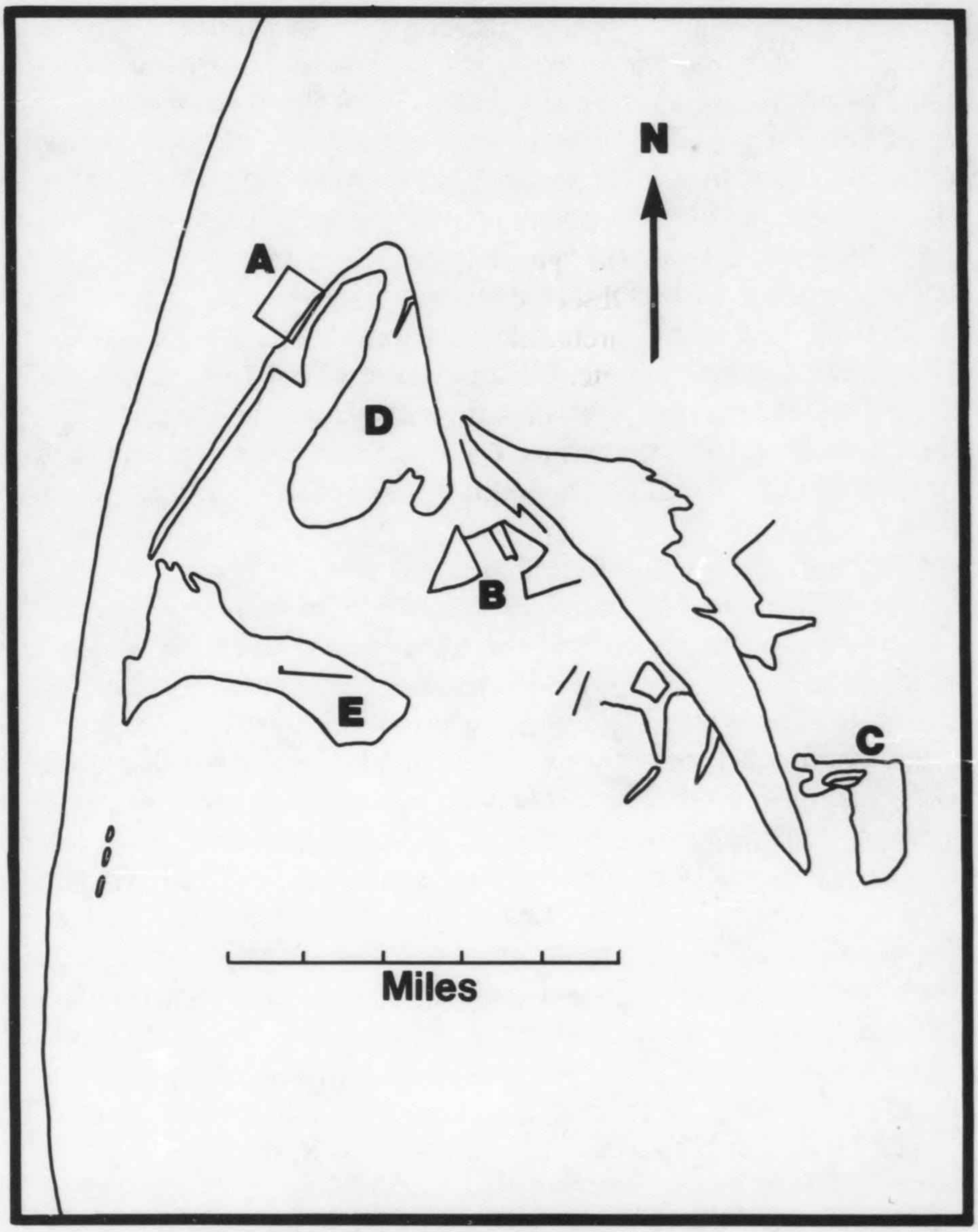


Fig. 2b. Map drawn from the photograph, highlighting features including: A, the area covered in Figure 1; B, a "triangle," "rectangle," and "pentagon"; C, a rectangular corner; D, the island of North Bimini; E, the island of South Bimini, with mile-long aircraft runway. All features other than the islands are submerged in 3 to 20 feet of water.

about one-half mile, and the size he gave was off by nearly a factor of 10 from the satellite photograph.

The satellite photographs provided no information regarding the depths of the patterns or their causes. To further study the anomalies I conducted a short expedition to survey the areas from low-flying aircraft and from the surface. Patterns identified from the satellite photographs were clearly recognizable from an altitude of 1000 to 6000 feet, and photographed using water-penetration techniques I had developed previously (Richards, 1980).

The patterns were still clearly unusual, but there was no further detail suggestive of buildings or other human construction. In a small boat I travelled to some of the patterns for on-site study. From the surface, there was no sense of an anomaly. The patterns were caused by differential growth of seagrass and abundance of white sand. No unusual structures were visible.

This work illustrates the problem of studying these anomalies. Valentine has called some of his patterns "ghost patterns," visible from the air but not distinctive at the surface. Observed from a high altitude, the patterns are quite unusual, and a land archaeologist would take similar evidence as a good indicator of a possible site. Whatever causes these patterns, however, is likely to lie under several feet of sediment. Thus, for the moment, the patterns remain simply anomalous. Calling them "cities" or "buildings" is not justified, but they should not simply be dismissed without further investigation.

The problem of discriminating man-made ruins from natural geological formations is by no means unique to these sites. For example, Jones (1983), has described the ancient walls in the Menai straits of North Wales. Even more than the Bahamas sites, the Welsh sites have persistently been linked with Atlantis. The Welsh walls, which were submerged as the sea level rose, were apparently ancient fish traps. Their builders made use of natural bedrock formations, blending man-made walls in as necessary. Jones notes the difficulty of separating true discoveries of submerged walls from natural forms said in legends to be sunken Atlantean cities. Jones' careful study is the type of work sadly lacking in the Bahamas. What is different about these sites in the Bahamas is that the strong views on both sides, combined with the inherent unlikelihood of any ruins at all, have made it difficult to obtain objective descriptions or evaluations of the sites. The reader attempting to assess the evidence usually does not have the opportunity for on-site inspection, and must rely on his perceptions of the reasonableness of the evidence and arguments from both sides. As I will show next, the ways in which the proponents and skeptics have pursued these claims has emphasized the extraordinary nature of the explanations, while downplaying the actual evidence and discouraging the resolution of the anomalies.

The Pursuit of Unconventional Archaeology

The anomalies discussed here would seem to be relatively easy to resolve, either as unusual geological features or as archaeological ruins. Why, then, has there been a sustained controversy? Debunkers have simplified it into a struggle between cult archaeology and science, yet the actual situation is more complex. There are issues concerning the methods used for studying the anomalies, and issues concerning the role of the popular press and the extraordinary explanations that have been advanced by some of the proponents.

The search for the anomalies themselves, and attempts to verify their man-made origin, is what Truzzi (1977) has called a cryptoscience. The claims are of an extraordinary anomaly—ruins—within the purview of the existing science of archaeology. In theory, achieving scientific acceptance would simply require a validated find of man-made ruins. Failure of a skeptic to find a site, however, does not disprove claims of their existence. Thus the existence of a multiplicity of possible sites, most of which have never been investigated, sets the stage for the long-term persistence of these claims. Ironically, it has not only been the uncritical nature of the popular accounts, but also the polemical tone of the response by the skeptics, which has perpetuated the controversy.

Methodological Issues

The methodology of the investigators is central to the resolution of an anomaly. To some degree, the differences in approach between archaeologists and geologists have contributed to the problem of resolving these anomalies. They are anomalies only to archaeology. That is, current reconstructions of prehistory do not allow for ruins in this area. Archaeology looks for patterns in context. Geology provides tools which can help resolve the natural or man-made origin of those patterns, but geological methods only have meaning in the context of an archaeological investigation. Thus the proponents (e.g., Zink, 1978) can agree that the road site blocks are indeed composed of beachrock, while at the same time arguing for the hand of man in their arrangement. Geological arguments such as those of Gifford and Ball (1980) and Shinn (1978) largely miss the point of view of the proponents. No one has critiqued arguments based on large-scale patterns, such as Zink's (1978) contention that the road site is megalithic, or Rebikoff's (1979) contention that the road site is an ancient harborwork, presumably because of the reasoning that beachrock formed *in situ* precludes any modification of the site on a large scale. The work of Jones (1983) in Wales demonstrates that this assumption is not necessarily correct because a site may have both natural and arranged components.

The actual fieldwork of the proponents (in contrast to their extraordinary explanations) has been appropriate to the early exploratory phases of amateur archaeological work: locating and mapping finds so that specialists can determine their origin. In some cases, proponents collaborated with relatively mainstream researchers (e.g., Lindstrom and Gifford, or Zink and the Cousteau Society), and differ primarily in their interpretation of the results. In this phase, the proponents have done as well or better than the skeptics. For example, Rebikoff (1972, 1979) has produced an excellent photomosaic, and Zink (1978) has produced an overall plan and detailed drawings of selected areas of the road site. The Gifford and Ball (1980) article has some maps of the area, but does not include the details of the entire road site. In contrast, the Harrison (1971) article contains a somewhat inaccurate dia-

gram of a small portion of the road site, and Shinn (1978) included only a diagram of the blocks that he cored, without noting their relationship to the rest of the site. McKusick and Shinn (1980) provide no diagram at all that might allow identification of the source of their samples.

On the other hand, the proponents have not done well in the next phase of the investigation: the specialized work which could determine the origin of the sites. Zink (1978) removed potential artifacts without regard to context, making it impossible to determine whether they were in any way associated with the site. This is an important problem, since Shinn (1978) has pointed out that the absence of confirmed artifacts considerably weakens the case for a man-made interpretation of the site. Gifford and Ball (1980) and Shinn (1978), with their greater access to professional resources, were able to have tests performed on geological samples. However, Zink (1978) indicates that he, too, understood the importance of such tests as cores through adjacent blocks and Carbon-14 dating, and collected samples for these tests, although he reported no results.

The methodological problems of proponents such as Zink are those that would be expected in any largely amateur undertaking, and do not in themselves distinguish this work from other amateur archaeology. In most sciences, all phases are almost exclusively conducted by professionals. In archaeology, on the other hand, much of the work is performed by amateurs, some of whom have status equal to professionals and present papers and make original contributions to the science. Both amateurs and professionals acknowledge the valuable function amateurs serve by discovering sites the professionals would otherwise miss, because the demands on the latter preclude this sort of low-yield exploration (Stebbins, 1980). In the case of the Bahamas sites, however, the professionals have taken primarily a debunking stance, rather than using these popular anomalies as an opportunity to educate amateurs in more sophisticated archaeological methods.

One significant methodological issue has tended to overshadow the normal aspects of these investigations—the use of "psychics." Since one source of interest in these sites was the work of Edgar Cayce, psychics have been a part of the process from the beginning, and their use has tended to polarize opinion.

Psychics have been used in two ways in archaeology, neither of which typically receives support from mainstream archaeologists. First, in the exploration phase, they have been used to locate potential sites. Second, following site discovery, they have been used to provide details about the sites or artifacts that cannot be obtained using the standard archaeological methods (Goodman, 1977). This is a significant distinction. A site prediction is verifiable by the actual locating of a site, whereas other information may include extraordinary explanations which are difficult or impossible to verify.

Use of psychics and dowsers to locate sites has become surprisingly common among archaeologists, though controversial. For example, Schwartz

and DeMattei (1986) collaborated with The Institute of Nautical Archaeology, very much a mainstream organization, to test the usefulness of psychics in locating shipwrecks. In principle, use of the intuition of a person labeled "psychic" is no different than using an archaeologist's "intuition" to locate a site. Several mainstream archaeologists have been willing to consider the use of psychics or dowsers in this manner (Martin, in the preface to Goodman, 1977; Hume, 1974; Thomas, 1979).

The use of psychics to provide details about artifacts or sites that cannot be obtained using archaeological methods is considerably more problematic, and is unlikely to ever be considered a valid source of evidence by the archaeological community. It goes against the entire tradition of archaeological reasoning based on the physical evidence, and is a major source of the extraordinary explanations which fuel the controversy. Critics (e.g., McKusick, 1982, 1984 and Feder, 1980) have confounded these two concepts, lumping them both in the category of "cult archaeology."

Extraordinary Explanations and Popular Support

The original discovery of the sites was accompanied by some extraordinary claims and attracted substantial media attention, leading to a reflexive negative response by the scientific community. As McClenon (1984, p. 72) has noted, "continual interaction with the media is repugnant to established scientists and further supports their rejection of the deviant researcher." After only a cursory study of the road site, it was rejected as an archaeological anomaly in the Harrison (1971) article in *Nature*. The article implied that the other anomalous sites were also unworthy of further investigation.

Increasing adherence to scientific methodology by the investigators is one way for the study of a rejected anomaly to regain some legitimacy in the eyes of the scientific community. Despite the debunking article by Harrison, the subject was considered appropriate for a Master's thesis (i.e., Gifford, 1973a). This strictly geological study of the road site (Gifford & Ball, 1980) was funded and published by the National Geographic Society. Since it resolved the road site anomaly in favor of a natural explanation, and contained no extraordinary claims, further funding became unlikely.

For those who felt that there were still claims worthy of investigation, the alternative was to generate support from the general public. The popular media are needed as an aid to recruitment and funding. Consequently, the anomaly must have an unusual quality to generate media interest. The double-bind is that this quality, and the subsequent media attention, "spoils the act" for the deviant researcher in his or her attempt to gain legitimacy within science (McClenon, 1984). The situation is especially touchy in archaeology, since more than most disciplines this field has long depended on media publicity and public funding.

Despite the attraction of media attention, there were substantial differences among the approaches of the various proponents. Valentine, the dis-

coverer of the road site, published in the house publication of the Miami Museum of Science, and in the *Explorers Journal,* both of which are outlets for both professional and amateur scientists, and are more closely related to scientific journals than to popular mass media. Similarly, Lindstrom's work was published in the *Explorers Journal* and *The Epigraphic Society Occasional Publications;* again, not professional journals, but perhaps the only outlets available to those conducting amateur archaeology on anomalous sites. Rebikoff published in the *Explorers Journal* and the *International Journal of Nautical Archaeology,* the latter being a major mainstream journal in the field. Even Zink presented some of his work at a mainstream scientific meeting (Mahlman & Zink, 1982).

Thus, in many cases, the investigators, whatever extraordinary explanations they may have preferred, were serious amateurs following the scientific method to the best of their ability and attempted to publish in the nearest thing to scientific media (e.g., the *Explorers Journal*) that would accept their work. There is little in their writings that could be deemed antiscientific.

The popular books, on the other hand, were typically not written by the principals in the work. The first book, that by Ferro and Grumley (1970), was more a travelogue and journal of the personalities involved than a report of an expedition. The Berlitz (1969, 1984) books are the major source popularizing Valentine's work, yet Berlitz himself did not conduct a study of the sites. Most of the other popular books are entirely secondary sources, repeating Valentine and Berlitz, and often adding other speculations without attribution.

Zink's (1978) book must be considered separately. It was written in a sensational style in an effort to obtain support from the general public, yet contained descriptions of several years of in-depth work. Zink's explanations, including extraterrestrials as well as Atlantis, are the most extraordinary ones proposed, yet paradoxically he also conducted the most extensive field work, and was fairly successful in eliciting participation in his expeditions from academically oriented people, including geologists and archaeologists. Were it not for the choice to seek popular support, the field work itself could certainly have been presented in a more conservative format. Unfortunately, the book provided ample ammunition for the critics of cult archaeology.

The continued media attention provoked an even stronger response from the critics. Attention turned from the details of the sites to attacks on the investigators and their motivations. By the 1980's, the entire subject had been labeled a "hoax" (Shinn, 1978), and proponents, regardless of their approach, labeled, "religious cultists" (McKusick, 1984; McKusick & Shinn, 1980). As outlined in McClenon's (1984) model of deviant science, any investigator who became interested in the rejected anomaly was now labeled as deviant, reducing chances for publication, promotion, and tenure, and thus virtually guaranteeing that any further research had to be pursued outside the mainstream scientific channels.

Conclusion

The Bahamas controversy can be looked at in the larger perspective of the response of the archaeological community to extraordinary claims. Stiebing (1987) addresses the phenomenon of cult archaeology or pseudoarchaeology. He notes that many divergent viewpoints are at odds with mainstream archaeology, but feels that three common features allow them to be treated as a single phenomenon: (1) the unscientific nature of the evidence and methodology, (2) the tendency to provide simple, compact answers to complex, difficult issues, and (3) the presence of a persecution complex and ambivalent attitude toward the scientific establishment. The Bahamas controversy can be examined with regard to these criteria.

First, the Bahamas investigations were generally conducted in a manner appropriate to the early stages of amateur exploration of potential sites, with some of the proponents performing more careful survey work than any of the skeptics. Some proponents were also aware of scientific methods for more in-depth study and verification, although they did not successfully carry out such study. In general, it is the explanations, not the methods, which are extraordinary.

Second, the explanations of the proponents are far from simple and compact, with several competing factions espousing Atlanteans, Phoenicians, and extraterrestrials. An argument could be made that these are separate cults, since the Atlantean and Phoenician interpretations are mutually exclusive. However, the proponents with differing views often joined together (e.g., in the MARS organization), and seemed to have a high tolerance for a diversity of explanations. Stiebing's feature seems more descriptive of the debunkers, who appear to feel that our current knowledge of prehistory rules out the presence of any underwater sites in the Bahamas, regardless of the explanation advanced. Although all the explanations currently offered by the proponents may be wrong, any confirmed site is likely to require *some* extraordinary explanation.

Finally, a persecution complex would not be surprising because the proponents are, in fact, persecuted. They have not, however, answered attacks such as those of McKusick with attacks of their own. Their attitude toward the scientific establishment is driven by the need, on the one hand, for popular support in order to survive, and on the other hand, their recognition that scientific methods will be required to validate the anomalies and carry on advanced work. Nevertheless, it is certainly true that they tend to stand by their extraordinary explanations, and have not made a sustained effort to engage in a scientific dialog. Nor, of course, have their opponents.

Am I saying that cults related to archaeology, as addressed by McKusick and Stiebing, do not exist? No, the *popular response* to the Bahamas claims, as represented by the numerous articles in the media, is often uncritical and the nature of the material may encourage cultism. Neither do I feel that any of the claims of the proponents are established. The weight of the evidence

on the road site, primarily the study of Gifford and Ball (1980) rather than the polemics of the debunkers, favors a natural origin.

But the investigators of the sites themselves are a heterogeneous group with an interest in extraordinary anomalies. Regardless of their preconceptions and media hype, they may have discovered genuine sites worthy of further investigation. Unfortunately, the labeling of the entire area of inquiry as religious cultism has caused the rejection of other anomalies which have simply not been investigated, and has helped fuel an antiscientific bias in the general public who perceive an elite unwilling to examine the evidence. The debunking stance has actually been counterproductive, helping to perpetuate the mythology of the road site, rather than encouraging careful study of all the anomalies.

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Journal of Scientific Exploration, Vol. 2, No. 2, pp. 203–216, 1988
Pergamon Press plc. Printed in the USA.

0892–3310/88 $3.00+.00
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Evidence for a Short-Period Internal Clock in Humans

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Abstract—The concept of an internal clock in humans and animals has had many supporters and detractors over the years. In this article, we demonstrate the apparent existence of an extremely precise time sense in humans, but the process is not related to conscious estimates of the passage of time. Instead, the experiments indicate that there is a mechanism, operating below the level of consciousness, that, with occasional feedback, can keep track of clock time. The precision of the system is quite extraordinary; the observations are consistent with synchronization between the internal timekeeper and clock time to within an averaged value of one part in 10^4.

Introduction

Time estimation experiments are generally classifiable into two groups—those in which various aspects of the circadian rhythm are studied, and those in which abilities to estimate shorter (ultradian) time periods under various circumstances are investigated. Circadian rhythm studies tend to be experiments conducted without feedback, since it is the free-running frequencies that are sought. Ultradian studies may include feedback, to enable subjects to improve their performance.

One of the puzzling observations about circadian rhythms is the discrepancy between the natural 24-hour cycle and the period that develops when environmental clues are removed (Aschoff, 1981; Moore-Ede, Sulzmann, & Fuller, 1982). Experiments with humans and animals show that very precise free-running periods are established, although generally distinctly different from the 24-hour period, often by several hours. Of what benefit is the capability to measure 27-hour periods with a precision of a few minutes, as has been observed in laboratory studies? If one can succeed in demonstrating the usefulness to an organism of having a free-running period longer than the natural one, would this explanation also hold for the cases where the period is much shorter than 24 hours?

Acknowledgements. I would like to acknowledge the encouragement that I received from C. S. Pittendrigh, J. F. Kihlstrom, and the late G. Rattray-Taylor and A. Koestler, as well as the constructive criticism from the referees. Thanks are due to D. L. Huestis, M. J. Coggiola, K. T. Gillen, and C. Spindt of SRI for technical assistance, and to G. Burch of Statek Corp. for his aid in providing components for the off-frequency watch. Finally, the cooperation of DRC and PLS was greatly appreciated.

The present study is one for which there is little precedent. Time estimation is carried out unconsciously, with the task, set by the "mind" itself, being to stay synchronized to an external clock. How this is done, and perhaps more importantly, why it is done, is far from clear. Conceptually, it may be akin to the circadian rhythm studies, but occupies a much higher frequency domain.

The genesis of these experiments was an observation by the author, using a digital watch, that there appeared to be a strong tendency to look at the time just at the change of minutes. That is to say, the typical "seconds" reading would be 55-04. An *a priori* prediction for the results of such an experiment is surely that the seconds reading should be randomly distributed from 00 to 59. Thus, the observations suggest that there is a bias in the system. Extensive studies were subsequently carried out to locate this apparent bias, and that is the topic of this article. The principal subject is the author, but midway through the experiments two other subjects were accidentally located, who were making the same observations independently.

Results

The basic experiment is extremely simple. The subject merely writes down the time that he observes when he consults his watch throughout the day, with emphasis on the "seconds" reading. Each reading must be recorded, and as the subject becomes used to writing down the data it becomes a routine. Ideally, he does not find himself looking at his watch more than usual.

A variety of experiments were carried out, to attempt to understand the nature of the phenomenon, and to determine whether it was merely an observational artifact. To establish typical results, the most basic protocol was to write down watch readings, day after day, for several months. Data were not taken every day, but once started in the morning, all data were recorded the rest of the day.

In Figure 1 are presented the results of 5 months of data acquisition, totalling 1489 watch readings. Both actual points and 3-point averaged smoothing of the histogram are shown. The peaking around 00 is very striking, as is the symmetry of the distribution. A χ^2 test, assuming an expected random distribution, indicates that the probability that this is the case is 2×10^{-15}. This number is sufficiently small that we need not dwell on the possibility that the effect is due to chance—it is either a real phenomenon, or there is a bias in the way the data are taken.

In Figure 2 the data for individual days are displayed, showing the distribution of days having particular fractions of readings in the 55-04 second range. For the 39 days on which readings were taken, there is only one for which the fraction is lower than the chance value, 0.167, and the maximum value is 0.45.

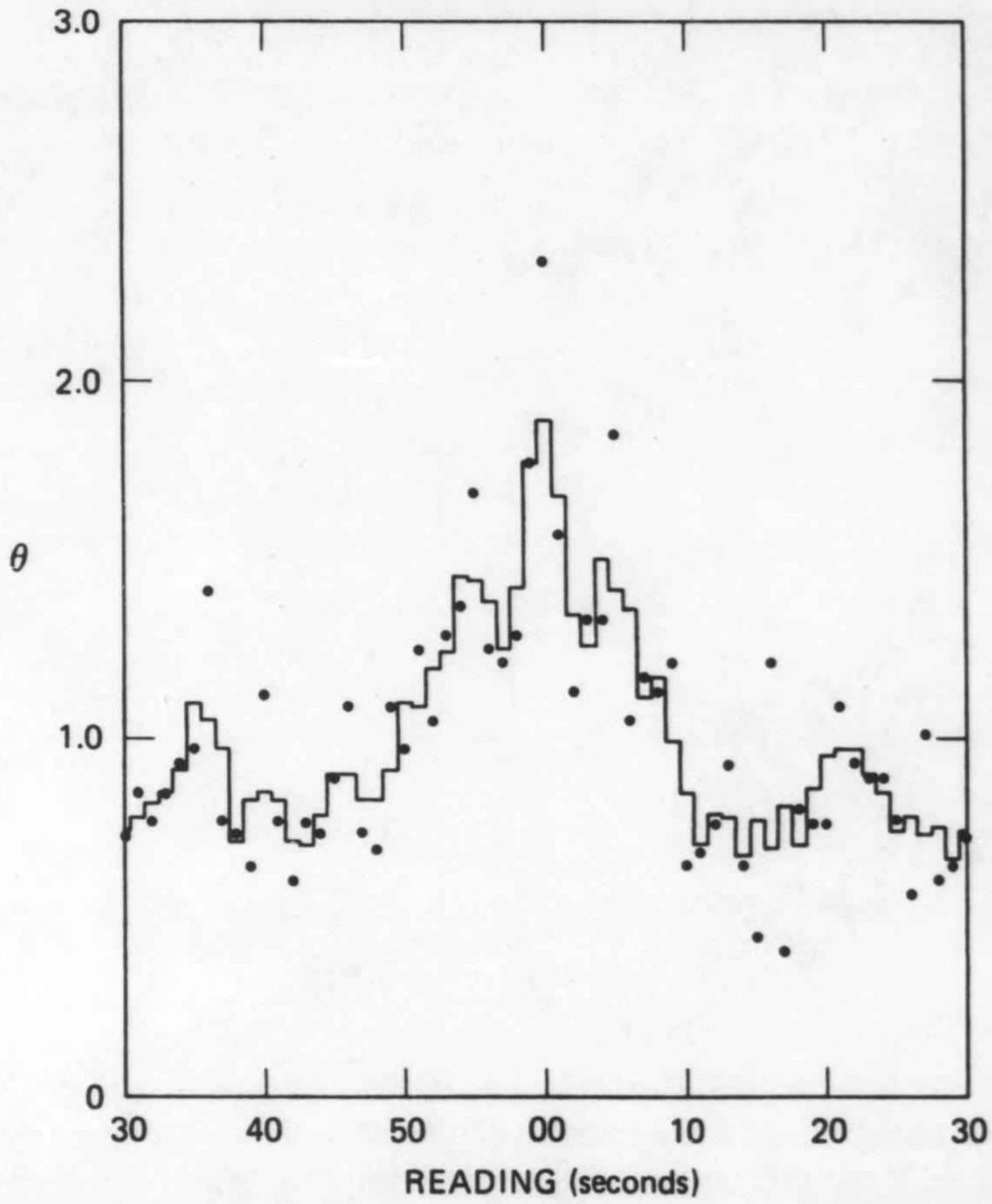

Fig. 1. Frequency distribution (TGS), 1489 points. Ordinate is normalized frequency, $\theta = (60/1489)r$, where r is total number of readings (dots) or readings smoothed by $\sum (r \pm 1)/3$ (line).

There are basically two potential data collection problems. One possibility is that if the subject has a preconceived idea of the desired experimental outcome, then he may consciously or subconsciously choose to ignore a sufficient number of "unsatisfactory" readings so that the data show the desired result. It seems more reasonable to assume that "unsatisfactory" readings might be ignored than that "satisfactory" readings would be invented. It is presumed that any data manipulation is unconscious.

A second issue is that the data may be valid, but not related to time estimation. For example, if digital watches give out a subliminal EM or audio signal at the change of minutes, then conceivably the subject might respond by looking at his watch. In this case, the data would exhibit a threshold at 00.

An essential part of any scientific study is to determine what parameters are related, that is, how can the observed outcome be changed. The results of Figure 1, taken at face value, suggest that, in anthropomorphic terms, there is an internal timekeeper that occasionally requests a time check. It has

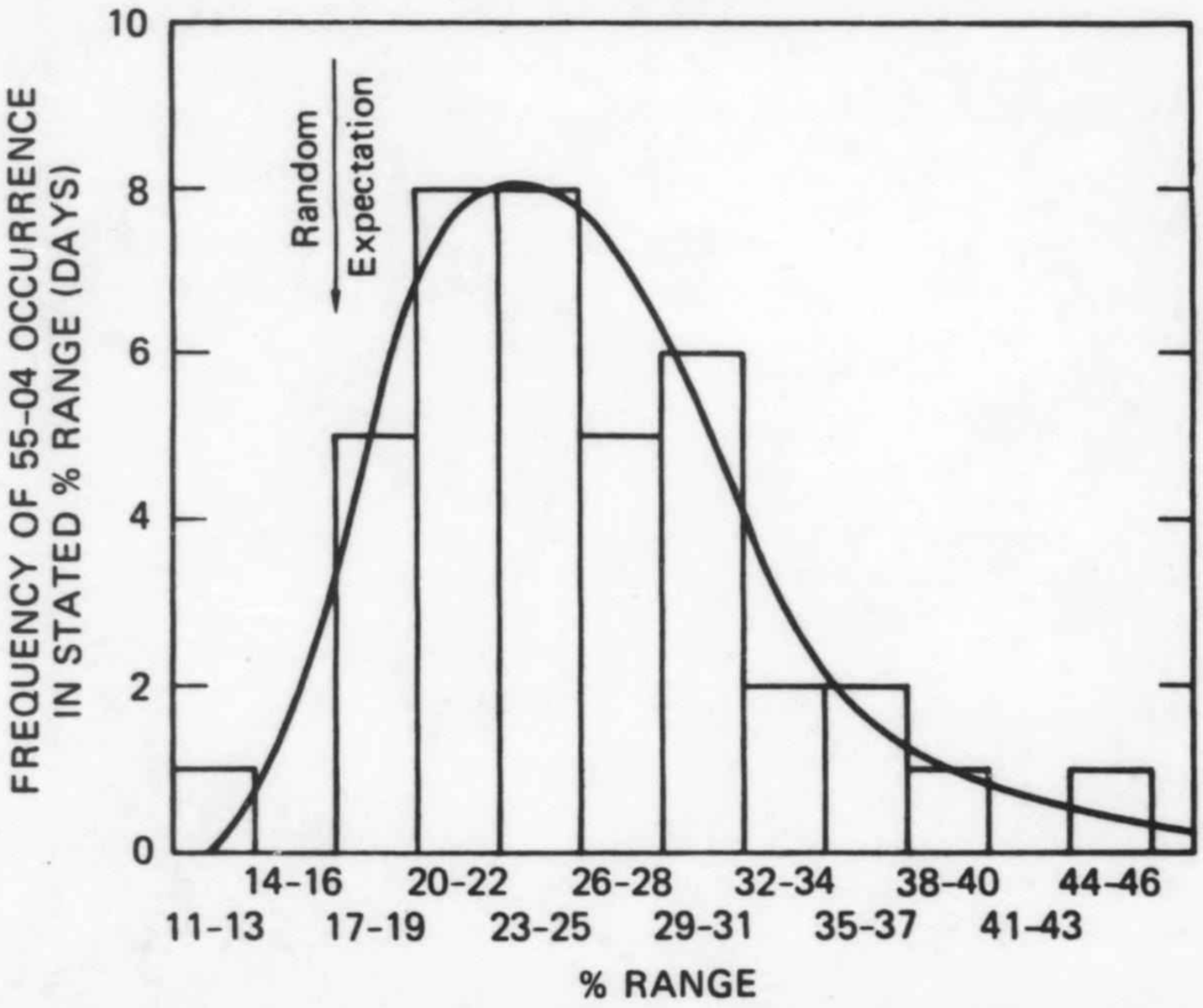

Fig. 2. Observed daily percentage occurrence of seconds within the range 55-04, 39 days (TGS).

chosen to synchronize itself to the watch, and finds that for the purpose of feedback, a 00 reading is more acceptable than an arbitrary number.

Since the 60-second period is a human construct, and a relatively recent one, it is not reasonable to suppose that it is a period natural to humans, as is 24 hours (in spite of the fact that the free-running period is more typically 25 hours). Therefore, it must be presumed that the timekeeper has adjusted its period to that of the watch. If that is so, there is an obvious question that arises. What is the effect of changing the period and phase of the watch? If there is no effect, it would be consistent with the idea that the watch sends a signal to the wearer at the change of minutes, and that there is no internal timekeeper. If there is an effect which disrupts the phenomenon, it will provide information on the resynchronization time, and will weigh against the idea that the subject is unconsciously manipulating the data.

In order to change the watch period in a sensible manner, an extremely small adjustment is needed. One second per minute, for example, is far too large, since in the average time between readings, 30–40 minutes, the effective period change would be as large as the maximum phase change of 30 seconds. A further reason why such a large change is unsatisfactory is because a watch that loses or gains a half hour a day is useless for telling time, and it is important in these experiments that the natural impulse to consult one's watch be perturbed as little as possible.

A much smaller change is required, and a quartz crystal reject was obtained whose period differed from absolute time by only 1 part in 10^3, gaining 100 seconds per day. In Table 1, data are presented that show the

TABLE 1
Frequency distribution in 6-second intervals (TGS)

Intervals	A	B	C	D	E	F
56–01	234	53	47	41	68	375
02–07	195	39	24	28	63	286
08–13	133	21	30	14	52	198
14–19	109	18	21	25	65	173
20–25	135	30	27	18	71	210
26–31	109	20	20	15	63	164
32–37	141	27	15	28	56	211
38–43	115	29	23	17	73	184
44–49	129	20	19	14	65	182
50–55	189	29	27	29	57	274
Average	149	29	25	23	63	226
N	1489	286	253	229	633	2257
χ^2	108.7	35.9	27.6	30.8	6.4	175.7
p	10^{-19}	3×10^{-5}	8×10^{-4}	2×10^{-4}	0.7	$\ll 10^{-25}$

A. Initial run, 5 months, Figures 1 and 2, standard watch.
B. Run immediately preceding frequency alternations, fast watch.
C. Frequency alternation, fast watch.
D. Frequency alternations, standard watch.
E. Phase shift run, 30-second displacement every 24 hours, standard watch.
F. Sum of runs A–D.

results of these frequency changes. Because of the limited number of readings, the data are collected in 6-second bins. Column A shows the results of the initial run, from Figure 1. These data were taken 15–20 months prior to those in Columns B–D. Column B consists of data taken over a period of one month with the fast watch, demonstrating that a slightly different frequency results in no change in performance. The data in Columns C and D were taken with the fast watch and the standard watch, respectively. The watches were switched approximately every day, but at irregular times, and at each switch the new watch was synchronized to the old, so as not to change phase. There is little difference between the results in the three columns; each distribution is seen to differ greatly from chance expectations, and moreover, each peaks near 00. The fraction in each bin for a random distribution should be 0.10. What is observed in the 56–01 second bin for columns A–D is, respectively, 0.157, 0.185, 0.186, and 0.179. It therefore appears that the internal timekeeper is not tied to an absolute period, but can adjust easily to a slightly different period. Included in Table 1 are values of χ^2, based on the expectation of a random distribution, and the probability of obtaining such a result by chance.

If a gradual change can be accomodated, is this also true for an abrupt phase change? Column E in Table 1 shows the effect of changing the watch phase by 30 seconds every day, for a period of several weeks. It is evident that the ability to favor readings near 00 has vanished, and that the distribu-

tion is now random; the fraction in the 56–01 second bin is 0.107. Whereas the frequency change experiment did not exclude the possibility that the phenomenon was due to a subliminal watch signal, this phase change experiment shows this not to be the case, for detection of such a signal would be independent of phase or period.

The phase shift experiment is quite critical, since it shows that the positive results can be turned on and off. The obvious follow-up experiment is the determination of how long it takes for resynchronization to be achieved. Such experiments are shown in Figure 3, where data were taken before (a)

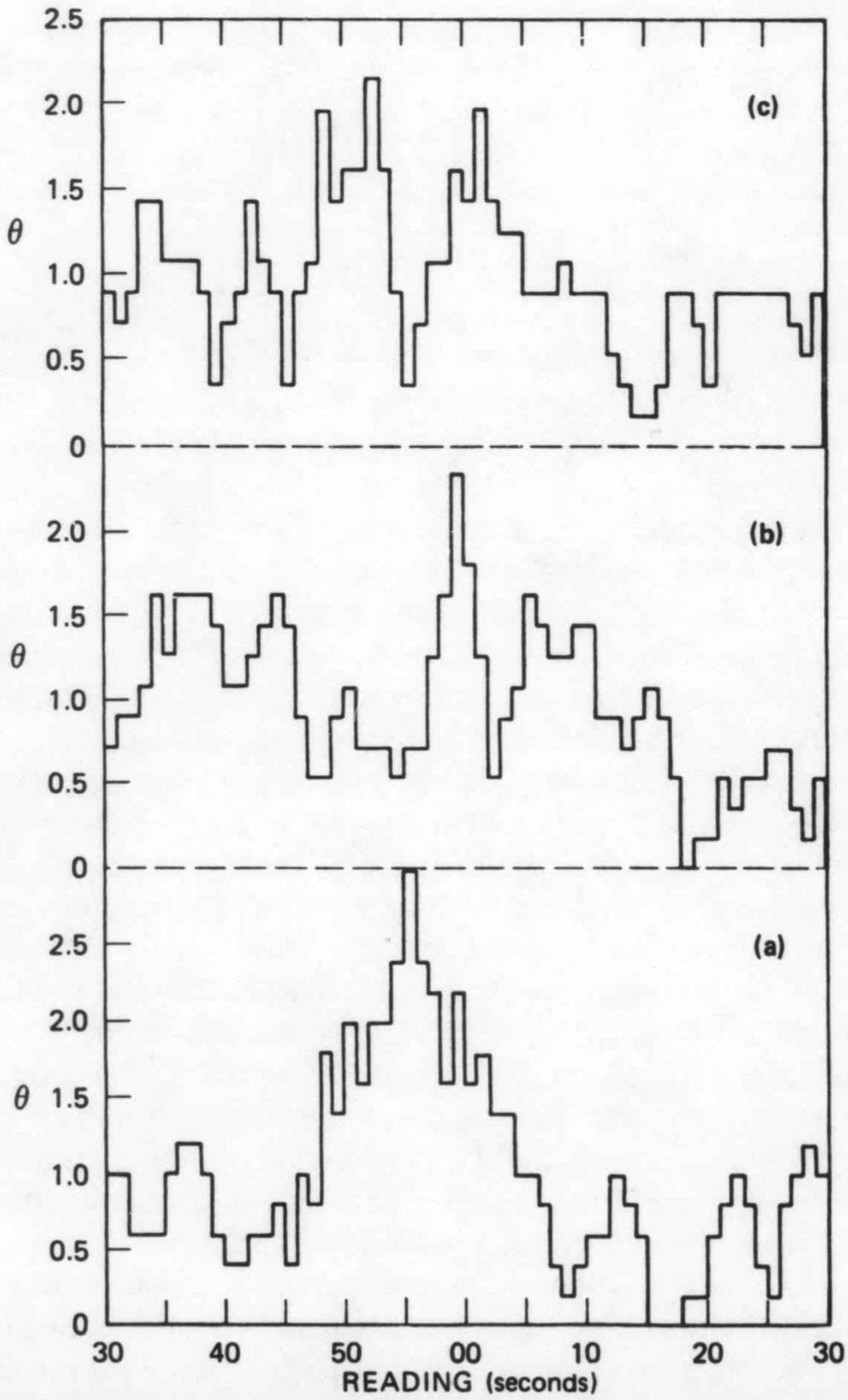

Fig. 3. Frequency distribution (TGS) for 30-second phase shifts. a) 5-day control period, b) 4-day period after shift, c) 4-day period after second 30-second shift.

and after (b) a 30-second phase change. The 5-day control period gave the usual 00 peaking, and after the phase change the data became random. At the end of the four days, 00 peaking started to reappear, which was then suppressed by a second 30-second shift (c). Although not apparent in the figures, immediately after the phase shifts there were indications of peaking around 30. It therefore appears that an abrupt 30-second change destroys the coupling between the watch and the timekeeper, whereas when the same phase change is introduced slowly, over the 8 hours that it took the fast watch to drift 30-seconds, there was no difficulty in following it. The observation that several days are required for resynchronization is consistent with the fact that daily 30-second changes result in chance distributions. There are many mechanical analogs to such a situation, the issue being the tightness in the feedback coupling; the more loosely two system components are coupled, the more difficult it is for the driven component to follow an abrupt change in the driver mode.

When many of these results had already been obtained, and a manuscript circulated, I discovered that one of my colleagues (DRC) had been carrying out the identical experiment, over a period of several months. The reasons were the same—he had seen, on first obtaining a digital watch, that the seconds readings around 00 appeared surprisingly often. Although his observations were not as extensive as my own, the effect was even more pronounced. He had adopted the same protocol, with a conscious bias in that if the first reading of the day was close to 00, he would continue to take readings throughout the day. However, the effect is so strong that the first reading can be ignored with little change in the results.

Of particular interest is the fact that DRC also did a resynchronization experiment. After initially establishing the timekeeping effect with a digital watch, he used an analogue watch for several weeks, then returned to the digital watch and started recording the data. He observed that for a period of 10 days the readings were random, but on the 11th day 00 peaking appeared, and continued for at least 6 months. The synchronized data are plotted in Figure 4, with and without the first reading of the day. In either case, the distribution is more sharply peaked than that of Figure 1. The data are tabulated in Table 2. Column A shows all the synchronized data, while in Column B, the first reading of the day has been excluded. In Column C are the data from the 10-day unsynchronized period, clearly showing a random distribution, and for comparison, Column D shows data from a random number table. The fraction of points in the 56–01 second bin for the four columns is 0.240, 0.208, 0.072, and 0.120.

Particularly noteworthy in the experiments of DRC is the fact that he is in the habit of wearing long-sleeved shirts. This assures that it is very unlikely for him to look at the watch in an inadvertent manner—two deliberate actions must be taken to make a reading, pushing up his sleeve and focusing his eyes.

Yet a third subject (PLS) was found, through a mutual acquaintance, who had been carrying out the same measurements for the same reasons. In his

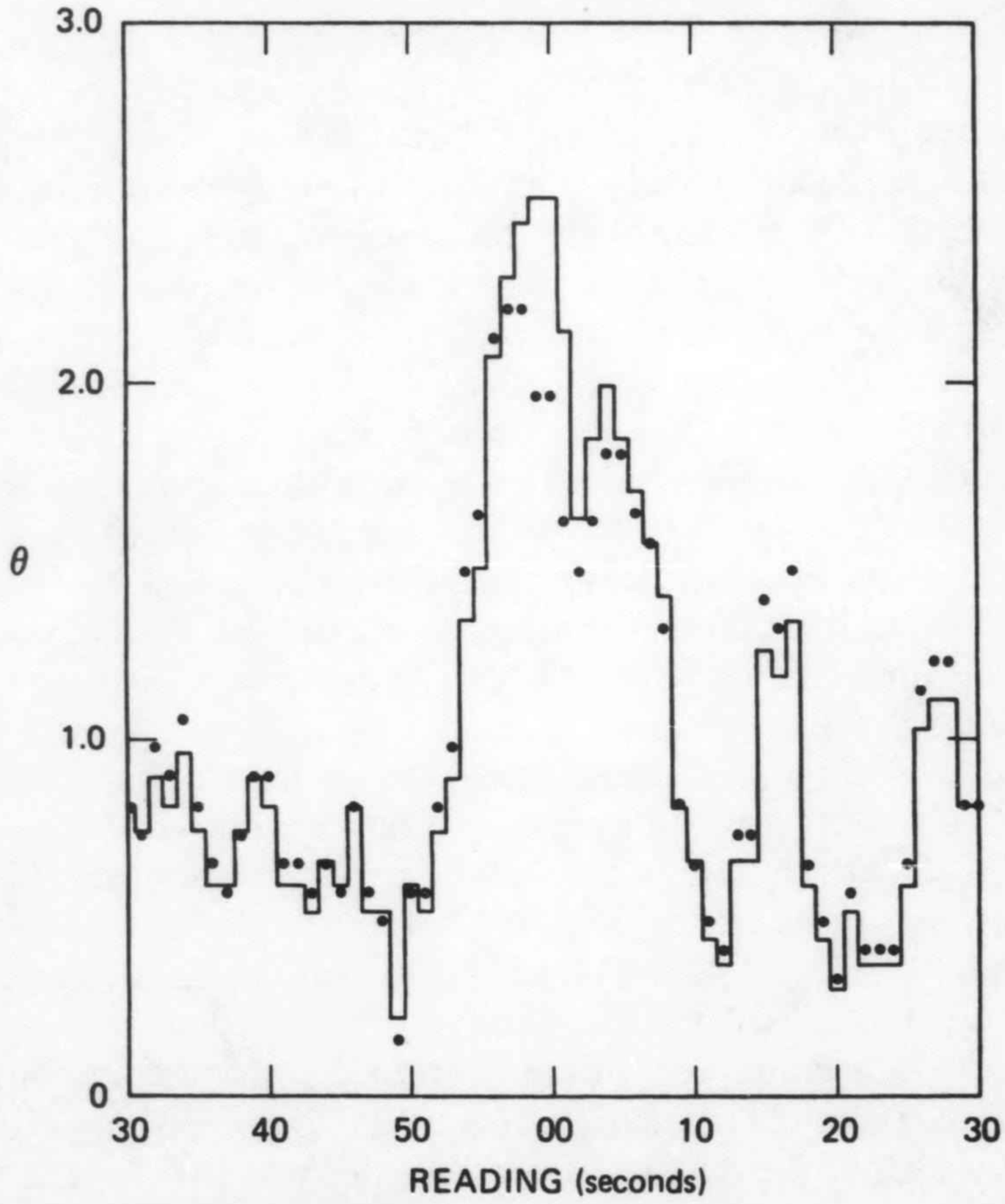

Fig. 4. Smoothed and normalized frequency distribution (DRC), 271 points. Line represents total data, dots represent data excluding first reading of day.

own analysis of his results he suggests that the explanation lies with a watch signal, because the peak appears asymmetric, suddenly rising at 00. This is of course different from the results of the author and DRC, where the peak is symmetric and commences well before 00. PLS also noticed that there would be periods, typically one month, where the effect was strong, and periods where it would vanish. Subsequently, both DRC and the author subjectively confirmed that there are periods of high and low "success" rates. However, the data presented in Figure 1 and Table 1 were taken before this apparent fact was known, and thus there was no bias against "unsatisfactory" months.

It should be noted that these experiments are unique in that *any* slippage in frequency between the timekeeper and clock time will produce a null result in the experiment. A period of 59.9 seconds accumulates to six seconds per hour, a discrepancy sufficient to accomplish this. It is known that circadian and ultradian rhythms are affected by drugs, diet, and stress (Church, 1984), and thus it is credible to find periods when the effect is indiscernible.

TABLE 2
Frequency distribution in 6-second intervals (DRC)

Intervals	A	B	C	D
56–01	65	51	14	23
02–07	48	41	28	20
08–13	16	15	22	19
14–19	25	25	12	12
20–25	12	12	25	15
26–31	26	25	20	22
32–37	19	19	27	28
38–43	20	19	17	18
44–49	15	13	13	15
50–55	25	25	16	22
Average	27.1	24.5	19.4	19.4
N	271	245	194	194
χ^2	92.1	57.7	16.1	11.7
p	5×10^{-16}	1×10^{-8}	0.07	0.25

A. After synchronization achieved, all data.
B. First reading excluded.
C. Before synchronization, 10 days.
D. Data from random number table.

The issue of the feedback requirement is of course of great interest. How often is the information required? The answer would appear to be, not very often. This conclusion is drawn from the fact that there is no feedback at night, yet if one goes to bed synchronized, one wakes up synchronized. Just as important, if one goes to bed unsynchronized, one is not synchronized the next day. Therefore, synchronization persists throughout the night, 3×10^4 seconds, and cannot be induced by the first reading of the morning (the latter point is obvious, since resynchronization takes several days). One may thereby conclude that the system has an averaged precision on the order of one part in 10^4. This figure is consistent with the data of DRC, which were obtained with an average of only 12 readings per day, just half of my own average.

The writer is unaware of any living system that maintains a frequency to that precision, or needs to. Enright (1979) has remarked that observations of free-running circadian periods in house finches and flying squirrels exhibit a precision of one part in 500 (equivalent to a 59.9 second minute), yet the present observations improve on this figure by more than an order of magnitude. Assuming that a chemical system is the basis for the operation of the timekeeper, it must therefore operate in a manner that is rather independent of the environment, and in particular it must be strongly temperature-independent.

A part of this study was carried out during a period when the circadian rhythm of the author was disrupted by ±8 hours, with no obvious effect on the synchronization. Beljan et al. (1972) have shown that the length of time

needed to resynchronize the psychomotor performance rhythm after travel across six time zones is several days (cited in Moore-Ede et al., 1982, p. 323). It thus appears that these two phenomena, which differ in frequency by more than three orders of magnitude, are not closely coupled in one sense, yet exhibit very similar resynchronization times when phase shifts comparable to their periods are introduced.

As regards temperature, humans exhibit a circadian rhythm with an amplitude of ±1°C (Sweeney & Hastings, 1960). Activation energies of many biological chemical reactions are such that the corresponding reaction rate change is ±10%. Thus, strong temperature compensation must be required for the reactions that determine the operation of the timekeeper.

Discussion

There are three major questions that need to be addressed. Is the interpretation of the data as an indication of an internal timekeeper valid? If so, how does such a process operate? And most important, why should such an ability exist?

In terms of the usual protocol for psychological experiments, the first question can only be answered affirmatively if adequate controls are provided. The nature of the experiment makes this difficult, as the principal subject is the experimenter. Although the discovery of an independent observer making the same observations and reaching the same conclusions is extremely encouraging, it has not yet been possible to carry out a fully controlled experiment.

The reasons for this are complex. This is not a laboratory experiment, and data are only forthcoming at a subject's own pace. A study takes months, and it is necessary for a subject to conscientiously take data, and yet be sufficiently removed from the experiment so that he looks at his watch only to check the time, not to get a data point. The more complicated the protocol, the more aware a subject is of the whole process. The expected need for feedback conflicts with possible control experiments, in which the seconds readings are automatically recorded but not perceived by the subject.

It is for these reasons that the experimenter-as-subject approach has been used. Since neither DRC nor myself had any preconceptions as to results, it is difficult to claim that we have subconsciously fabricated them. It is true enough that 00 is a very symmetric number, and that one might be particularly attuned to its observation. However, to make the data of Figure 1 appear random requires that the 58 00's that were in fact observed (compared to the chance expectation of 25) must be accompanied by 59 × 58 non-00's. In other words, all 00's would have been recorded, but 60% of all readings were unconsciously discarded. I estimate that 5–10% of readings were not recorded, having been made in adverse circumstances (moving vehicles, etc.), but the idea that more than half of them were not recorded is not realistic. Furthermore, such an explanation can hardly rationalize the

shapes of the distributions in Figures 1 and 4. It must be noted that one cannot glance at a digital watch as one can an analogue watch; the eyes must focus to register the digits. Furthermore, I had no preconceptions concerning the phase and period shift experiments, yet they gave diametrically opposite results. In spite of the lack of the usual types of controls, I consider the data robust enough to be grounds for further study.

The second question, concerning how such a process might occur, is beyond the expertise of the writer. There is an extensive literature on circadian rhythms, and it has been shown that the suprachiasmatic nuclei in the hypothalamus are an important site of the timekeeping ability in mammals (Groos & Daan, 1985; Moore-Ede et al., 1982). However, there is no basis to claim that the same organ would be involved at the much higher frequencies under consideration here. In fact, there is considerable evidence that even for circadian periods, organisms contain multiple oscillatory systems (Takahashi & Zatz, 1982). Current attempts to explain the mechanism of timing tend to use information-processing models (Gibbon, Church, & Meck, 1984) and a cognitive approach, as opposed to the older internal clock concept.

The how and the why are inextricably linked, for there seems to be no obvious reason why such a remarkable ability should exist, or what would be the evolutionary pressures that would cause it to develop. The ability to measure short intervals of time repetitively and with great precision has no obvious purpose, yet the experiments indicate that it is not only an ability, but that the organism actually *requires* the temporal information. Were this not so, there would be no peaking in Figures 1 and 4, since if the ability existed without the need for synchronization, then the phenomenon would not manifest itself in this way, and would remain unrecognized. The difference between these studies and conscious time estimation experiments should be emphasized. Performance in the latter, for 6-second and 10-second intervals, has been shown to depend on the circadian cycle phase, and is typically of ±10% precision (Poppel & Giedke, 1970).

The principal conclusion of the present experiments is that looking at one's watch may take place at non-random times. The thought of checking the time does not necessarily arise spontaneously, or because one has a schedule to meet. It can arise to satisfy the requirements of the internal timekeeper. In fact, the effect can be quantified, by comparing the random expectations with the non-random observations in Figures 1 and 4. The 00 peaks have a width, measured from the base line, of 20 seconds in Figure 1 and 16 seconds in Figure 4. Since the data and their interpretation lead to the conclusion that the timekeeper itself is very accurate, this width is indicative of a response lag between the command, originating with the timekeeper, and the conscious mind; the data in the 50 → 00 interval would seem to correspond to an anticipatory response. Ignoring the other minor peaks that appear in Figures 1 and 4 (although they may be significant), one interpretation of the data is that during the 10 → 50 second interval the

decision to look at one's watch is "consciously" controlled, whereas in the 50 → 10 second interval it is partially controlled by the timekeeper. Treating the data in Figure 1 in this manner, one finds that 40% of the points in the 50 → 10 second range lie above the base line and are therefore due to the timekeeper, and these points represent 19% of the total number. From Figure 4, 65% of the points in the 53 → 09 second range lie above the base line, representing 32% of the total number. Therefore, my own data indicate that four times out of five, the decision to look at my watch is *not* controlled by the timekeeper. The data of DRC indicate that this is true for him two times out of three.

One may well ask how this urge to know the precise time is satisfied in people who don't wear watches; obviously there is at present no answer. If people have "natural" periods on the order of 60 seconds, their utility is as mysterious as the natural non-24-hour circadian periods mentioned earlier. Only by entraining to a watch are these rhythms observed. One might argue that it is well known that the brain tends to process and store a variety of forms of information made available to it, for reasons that we do not understand. A piece of music not heard for 40 years may suddenly be brought back to conscious memory, which means that the temporal structure has remained intact. There may be some relationship between such information processing and the unexpected temporal behavior described here.

It is difficult to place these observations within the framework of current studies of time perception, because they are not task oriented. They represent a sidewards glimpse of a natural process, in that the data are obtained in as passive a manner as possible. It is presumed, although not proven here, that a deliberate attempt to guess when the seconds are close to 00 will not be successful. There are many descriptions of ultradian experiments, involving periods of time of a few seconds. For example, Treisman (1984) has attempted to relate the ability to estimate four-second intervals to α-rhythm frequencies, finding that, over a period of time, the time estimation ability is much more variable than changes in the α-rhythm frequency. It is my belief that the non-conscious aspect of the present experiments may be most reasonably compared with hypnotic and sleep studies. As pointed out by Block (1979), estimation of time duration during sleep involves an unknown subconscious process.

Regarding hypnotic investigations, there is an extensive literature on time perception experiments carried out in the period 1890–1925. There is a tendency for modern researchers to ignore studies of this vintage, but this work provides the closest parallel to the present observations. The experiments, pioneered by Bramwell (1900, 1930) are based on the supposition that there is a class of subjects (somnambules) who can be given suggestions under hypnosis of which they have no waking memory. Bramwell studied several individuals over a number of years, using extensive variations in testing their performance on a single task—the estimation of a period of time told to them while under hypnosis. For example, the subject would be

told to make a mark on a piece of paper in 9278 minutes, and would be observed to do so at the appropriate time with surprising accuracy, and with no comprehension of why she was doing it.

These experiments were taken up in later years by Mitchell (1908) and by Hooper (1923), with similar results, and no subsequent studies impugned the validity of the observations or the method, although there is evidence that these early studies were misunderstood (Wolberg, 1948; Le Cron & Bordeaux, 1949). The connection to the present experiments is evident—keeping track of minutes without the aid of a clock is equivalent to synchronizing on 00. The difference is that the hypnotic task is more difficult, since the target is a specific minute. It should be noted that Bramwell's experiments were not designed to study time estimation while under hypnosis, which has been shown elsewhere to be no more accurate than in the waking state (Stalnaker & Richardson, 1930), but instead were directed towards an investigation of unconscious timekeeping. It is certainly true that experimental protocols in 90-year-old studies should be questioned, but it is still important to recognize that the present experiments are not without precedent. The range of unconscious cognitive processes, including hypnotic effects, has recently been discussed by Kihlstrom (1987).

In a recent book by Richard Feynman (1988), the author describes how he and John Tukey, as graduate students, discovered that the process of counting can be extremely precise (pp. 53–59). Feynman found that counting silently to 60 always took him 48 seconds, and that in attempting to distract himself from counting by performing other tasks simultaneously, he observed that under almost all circumstances that he could devise, including reading, the period remained invariant. Paying less and less attention to the counting did not degrade the ability to keep time.

This relates to the current studies in an obvious manner. An extension of the ability described by Feynman would be the ability to count without being aware of it, which is exactly what is required for the present experiments to be successful. The subjects of Bramwell (1900), when asked to explain under hypnosis how they were able to keep track of the time with such accuracy, said that they counted seconds continuously. There is also at least one case of an autistic child with a phenomenal timekeeping sense whose attention was always directed towards counting (Rothenberg, 1977, p. 225).

Conclusions

The principal conclusion of this study is that there is a capacity to recognize location in the time domain with great precision. These passive time estimation studies indicate that the internal timekeeper will adjust its frequency to whatever fixed-frequency source is available. Once the timekeeper has been entrained to a source, a phase change will break the entrainment, and several days are needed to reestablish synchronization.

There may or may not be limits to the periods that can be tracked; the present investigation has shown that two periods around 60 seconds can be followed with an averaged precision on the order of one part in 10^4. The manifestation of the effect is particularly remarkable in that it suggests that the timekeeper, operating at an unconscious level, periodically stimulates the conscious mind to provide calibration information, and that, for the two subjects investigated, 20–30% of watch-consulting decisions are timekeeper-triggered.

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Journal of Scientific Exploration, Vol. 2, No. 2, pp. 217–238, 1988
Pergamon Press plc. Printed in the USA.

0892–3310/88 $3.00+.00
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Three New Cases of the Reincarnation Type in Sri Lanka With Written Records Made Before Verifications

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Abstract—Three new cases in Sri Lanka of children who claim to remember previous lives were identified before the statements made by the children subjects of the cases had been verified. The authors made a written record of what the child said and then located a family corresponding to the child's statements. Although none of the children stated the name of the deceased person whose life the child seemed to remember, they all furnished details that, taken together, were sufficiently specific to identify one particular person as the only person corresponding to the child's statements. Careful inquiries about the possibilities for the normal communication of information from one family to the other before the case developed provide no evidence of such communication and make it seem almost impossible that it could have occurred. The written records of exactly what the child said about the previous life make it possible to exclude distortion of memories of the child's statements on the part of informants after the two families concerned have met. The children seem to have shown paranormal knowledge about deceased persons who were previously completely unknown to their families.

Introduction

Children who claim to remember previous lives can be found with little difficulty in South Asia, parts of western Asia, West Africa, and in some other parts of the world. A survey of a randomly sampled population in northern India showed an incidence of one such case in 500 persons (Barker & Pasricha, 1979). Previous articles and books have reported 62 cases of this

We wish to thank Tissa Jayawardane for his skill in learning about these cases and making preliminary investigations of them. H.S.S. Nissanka furnished helpful information about landslides in the highland area south of Kandy, Sri Lanka. Susan Adams gave helpful editorial assistance.

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type in detail (Stevenson, 1974, 1975, 1977, 1980, 1983). In addition, the features of the cases have been analyzed in cross-cultural comparisons (Cook, Pasricha, Samararatne, U Win Maung, & Stevenson, 1983) and in comparisons of cases occurring two generations apart (Pasricha & Stevenson. 1987).

The subjects of these cases often show behavior, such as phobias, philias, and play, that is unusual in their family but that accords with behavior that the person whose life the child recalls was known to have shown or that could be plausibly attributed to him. However, such behavior could derive from the child's belief that he had been a particular person. For example, a phobia of knives would be appropriate for a person who believed that he had been stabbed to death in a previous life; the belief by itself is not evidence that the subject had a previous life that ended in stabbing. More valuable evidence can, however, derive from the child's statements about the previous life. Yet not all of these statements can qualify as satisfactory. For this to happen we must know not only that the statements are correct for events in the life of a particular person; we must also know that the child could not have obtained the information in his statements by normal means of communication. These are difficult criteria to satisfy, for reasons that we shall next explain.

First, in a large number of cases the child does not make statements that are sufficiently specific to permit tracing a deceased person corresponding to them. (We call such a person the "previous personality" of the case.) These unverified cases (which we call "unsolved cases") may include memories of real previous lives; but we cannot know this, and they may be only fantasies. The incidence of unsolved cases varies from one country to another; they are particularly common in Sri Lanka and among non-tribal cases of the United States. In a second large group of cases the subject's family and that of the previous personality were acquainted before the case developed, and information about the previous personality might have reached the subject normally. There remains a third large group of cases in which the two families were unrelated and unacquainted before the case developed. Moreover, they often live so far apart—perhaps 50, 100, or more kilometers from each other—that (given the difficulties of communication in Asia) it is extremely unlikely that the subject's family could have learned anything normally about the previous personality's family before the case developed.

Unfortunately, investigators of cases rarely learn about them before the two families concerned have met. If the subject of a case furnishes information about the previous personality that seems to his parents sufficiently specific (by including proper names of places and persons) and if the distance involved is not too great, the subject's family will usually try to trace the family of which he is talking. They may be impelled to do this by their own curiosity, by the child's strongly expressed wish to go to the other family, or for both reasons. When the families meet, they naturally exchange information about what the subject said concerning the previous life and the

extent to which what he said corresponds to facts in the life of a deceased member of the other family. In this exchange members of one or both families may credit the child with having more accurate knowledge about the previous personality than he did in fact have before the families met. This improvement of the case may occur unconsciously and without any intention to deceive. These circumstances make particularly important the rare cases in which someone made a written record of exactly what the subject said *before* the two families met. Despite our long-standing awareness of the importance of such cases they still number only about one percent of the cases admitted to the series documented at the University of Virginia. More exactly, among approximately 2,500 cases someone made a written record of the subject's statements before they were verified in only 24 cases. Reports of three cases of this group in India (Stevenson, 1966/1974, 1975) and of two in Sri Lanka (Stevenson, 1966/1974, 1977) have been published.

A means of increasing the number of such cases has been obvious for many years, but has proven difficult to implement. It is to have a person able to identify the cases living in an area where they occur; and that person must quickly reach any case of which he learns and record the subject's statements about the previous life before the subject's parents (or someone else) take the subject to the previous family.

Sri Lanka appears to be a country well suited for such an effort. We have learned of cases there in which the two families had not met that came first to the attention of newspaper reporters. The reporters identified a family corresponding to the subject's statements and often took the subject to that family. They then published a report of the case in a newspaper (which often provided our first information about the case). However, the reporters (with a single exception known to us) were only interested in the immediate news value of the case, and they made no written list of the child's statements before taking him to the other family. Such cases, therefore, could not be included in the small series of these special cases (with written records before verification) that we are trying to increase.

Circumstances finally gave our team a slight advantage over the newspaper reporters in the race to learn first about new cases. Mr. Tissa Jayawardane (T.J.) has been assisting in our research in Sri Lanka for many years. He notified us of new cases that he learned about, and he often accompanied one or both of us on tours to investigate cases. However, his activity on behalf of the research was sporadic and largely confined to the periods when one of us was actually investigating a case. Then in 1985 he unexpectedly became able to devote his entire time to the research. He quickly widened the network of his informants for cases and soon began to learn of many new ones. Some of these were unsolved and probably are insoluble; in others, the two families had already met even before T.J. reached them. Nevertheless, in several instances he reached the scene of the case before the families had met, made a written record of the subject's statements, and

then went on to identify a family corresponding to the statements (which family the subject's family had not met or heard about). Our team has now studied four of these cases in Sri Lanka and the present paper reports three of these. (For reasons of space we omit the fourth case here in order to provide sufficient details about the other three.)

These four cases are less than 10% of all the Sri Lanka cases that we have learned about over the period of studying them. At least 50 other Sri Lanka cases have come to our attention during the approximately three years since we learned of the first of these four cases. In all the other cases either the two families had already met by the time we reached the site of the case or the subject had given information that was insufficiently specific to permit tracing a family corresponding to his statements (unsolved cases). It was fortunate for our investigations that in two of the three cases here reported, the families concerned were widely separated geographically; and in the third case, although the families lived much closer to each other, the child's parents were indifferent about the verification of her statements. No report of these cases has been published in a newspaper or magazine. T.J. learned of them through private sources.

Methods of Investigation

Interviews with firsthand witnesses for relevant information are the principal instruments of investigation. On the subject's side of a case the important informants are the subject's parents, but older siblings, grandparents, and other relatives may provide supplemental information. We always try to interview the subject, but young children vary greatly in their willingness to talk with us. On the side of the previous personality, that person's parents, siblings, and spouse (if the person was married) are the important informants.

We also study any pertinent written documents that are available, but these are rare in Sri Lanka, except for birth and death certificates. In one of the cases that we report here newspaper accounts of the accident in which a previous personality had died provided some confirmatory information. For the same case we examined the report of an inquest.

The investigation of the cases proceeded in the following general manner. When T.J. learned of a new case he went to the family concerned as soon as possible. He obtained an exact address and recorded the main demographic information about the subject. He drew up a list of the subject's statements about the previous life, noting the names of the informants for these. He would advise the subject's family not to try to find the previous personality's family before we had done so. He then notified us about the case. At the same time, if the case seemed solvable, he would go to the place mentioned by the subject and try to find a family corresponding to the statements. If successful in this he would notify us.

As soon as possible thereafter, G.S. would go to the subject's family for more detailed interviews. At this time he would often note statements that

the informants had not mentioned earlier to T.J. If he interviewed them before the two families had met, we counted these additional statements in the list of those recorded before verification, even though T.J. had already verified some of the statements that he had recorded earlier. G.S. sometimes also went to the previous personality's family and confirmed the verifications of the subject's statements with them.

In the final stage of the investigation, I.S. (accompanied by G.S. and T.J.) interviewed (sometimes twice) members of both families concerned in the case. Although the informants sometimes mentioned a few additional statements in these later interviews, I.S. concentrated his attention mainly on two aspects of the case: the verification of the subject's statements with the previous personality's family, and the possibilities for some normal communication of information from that family to the subject's.

In making our independent verifications of the subject's statements we always obtained information from two, and sometimes from several, informants for the identified previous family. When visiting them we also examined for ourselves roads, houses, shops, and other details of surroundings that had figured in the subject's statements. These direct observations freed us from dependence on the informants for the verification of these details, although we naturally had to rely on their memories concerning changes in buildings that had occurred after the previous personality's death as well as for information about events in the family life that had figured in the subject's statements.

G.S. acted as an interpreter for I.S., who speaks no Sinhalese, although he can sometimes understand elements of the exchanges between the speaker and the interpreter. Some informants could speak English. All were Sinhalese Buddhists. We rarely use tape recorders, preferring instead to make handwritten notes which record questions asked and answers given by each informant. Details of interviewing techniques have been described elsewhere (Stevenson, 1966/1974, 1975).

In the interest of brevity we omit from the case reports that follow many of the details that a complete report of each case would include. For example, we shall only mention the names of informants where doing so would help readers to identify persons referred to more than once. We also omit some details of the subjects' behaviors that related to their statements. Instead, we shall focus attention on the following two features of each case: the key statements made by the subject that were verified as corresponding to events in the life and death of a particular deceased person and the possibilities for the normal communication of information about the previous personality to the subject or the subject's family.

Case Reports

The Case of Thusitha Silva

Thusitha Silva was born near Payagala, Sri Lanka, on July 29, 1981. Her parents were Gunadasa Silva and his wife, Gunaseeli. Gunadasa Silva was a tailor. Thusitha was the sixth of the family's seven children.

When Thusitha was about three years old she heard someone mention Kataragama, and she began to say that she was from there. She said that she lived near the river there and that a dumb boy had pushed her into the river. She implied, without clearly stating, that she had then drowned. (Thusitha had a marked phobia of water.) She said her father was a farmer and also had a boutique for selling flowers which was near the Kiri Vehera (Buddhist stupa). She said that her house was near the main Hindu Temple (Devale) at Kataragama. She gave her father's name as Rathu Herath and said that he was bald and wore a sarong. (Thusitha's father wore trousers.) Thusitha did not give a name for herself in the previous life and indeed gave no proper names apart from "Kataragama" and "Rathu Herath." She never explicitly said that she had been a girl in the previous life, but she mentioned frocks and also objected to having her hair cut; so her parents inferred that she was talking about the life of a girl.

Tissa Jayawardane learned of this case in the autumn of 1985 and visited Thusitha and her family for the first time on November 15, 1985. Having recorded the above statements and some others he went to Kataragama. Here we should explain that Payagala is a small town (population in 1981: 6,000) on the western coast of Sri Lanka south of Colombo, and Kataragama, a well-known place of pilgrimage, is in the southeastern area of the island, in the interior (Obeyesekere, 1981; Wirz, 1966). Kataragama is approximately 220 km by road from Payagala. It is also a small town (population in 1987: approximately 17,500) and consists almost entirely of temples and supporting buildings together with residences for the persons who maintain the temples and supply the needs of the pilgrims. A moderately large river, the Manik Ganga, runs through the town.

T.J. went first to the police station in Kataragama, where he inquired for a family having a son who was dumb. He was directed to a double row of flower stalls along the pavement of the main road to the Buddhist stupa, known as the Kiri Vehera. (The vendors at these stalls sell flowers to pilgrims for their use in worship.) Upon inquiring again among the flower vendors he was told to go to a particular flower stall, and at that one he asked whether a young girl of the family had drowned. He was told that a young daughter of the family had drowned in the river some years earlier, and one of her brothers was dumb. According to T.J.'s notes, Thusitha had made 13 verifiable statements and all but three of these were correct for the family with the dumb child who had lost a girl from drowning.

In the second phase of the investigation (in December 1985), G.S. learned about 17 additional statements that Thusitha had made, and he recorded these. The two families still had not met (and, so far as we know, this is still true), so that, as mentioned earlier, we consider our record of these statements uncontaminated by any contact between the two families. Two of these 17 additional statements were unverifiable, but the other 15 were correct for the family of the drowned girl. A few of these statements, such as that one of the houses where the family had lived had had a thatched roof,

were of wide applicability. A few others, such as that there were crocodiles in the river, could be regarded as part of information generally known about Kataragama. However, several of the additional statements that G.S. recorded were about unusual or specific details, and we will mention these. Thusitha said that her father, in addition to being a farmer and selling flowers, was also a priest at the temple. She mentioned that the family had had two homes and that one of them had glass in the roof. She referred to the water in the river being low. She spoke of dogs that were tied up and fed meat. She said her previous family had a utensil for sifting rice that was better than the one her family had. She described, with imitative actions, how the pilgrims smash coconuts on the ground at the temple in Kataragama.

Western readers unfamiliar with Sri Lanka may not immediately appreciate the unusualness of the details in several of these statements. For example, there are plenty of dogs in Sri Lanka, but most of them are stray mongrels who live as scavengers; few are kept as pets. Also, most Sinhalese who are Buddhists would abhor hunting, although Christian Sinhalese might not. It happened that the family of the drowned girl had neighbors who hunted, and they fed meat from the animals they killed to a dog chained in their compound. This would be an unusual situation in Sri Lanka. Another unusual detail was that of a glass (skylight) in the roof of the house. Devotees at Hindu temples other than the one at Kataragama may smash coconuts as part of their worship; however, Thusitha had never had occasion to see this ritual.

In the third phase of the investigation, I.S. (accompanied by G.S. and T.J.) went to Thusitha's family and then to Kataragama. Each family was visited twice in this phase, once in November 1986 and again in October 1987. We learned that the girl who had drowned, who was called Nimalkanthi, had been not quite two years old when she died, in about June 1974. Nimalkanthi had gone to the river with her mother, who was washing clothes there. She was playing near her mother with two of her brothers one of whom was the dumb one. Her mother apparently became absorbed in her washing, and then suddenly noticed that Nimalkanthi was missing. The brother who could speak could not say where she had gone. Nimalkanthi's mother raised an alarm, a search was made, and Nimalkanthi's dead body was recovered from the river. It is unlikely that the dumb brother had pushed Nimalkanthi into the water, but all three children had been playing around just before she disappeared. It seems probable that she lost her footing and slid or fell into the water; she could not swim. Thusitha's statement that the dumb brother had pushed her into the river thus remains unverified, and it is probably incorrect. However, the brother may have pushed her playfully just before she drowned accidentally.

Two of Thusitha's verifiable statements were definitely incorrect. She said that her father of the previous life was bald, but Nimalkanthi's father (whom we interviewed) had a good head of hair. She said his name was Rathu

Herath, but it was Dharmadasa. There were, however, two bald men in the family—Nimalkanthi's maternal grandfather and a maternal uncle—and Nimalkanthi would have seen them often. And a cousin by marriage, whom Nimalkanthi saw from time to time, was called Herath (not Rathu Herath). Thus one could argue that Thusitha's memories included some confusions of the adult men in her family, but we do not wish to emphasize this explanation.

Another of Thusitha's statements was incorrect for Nimalkanthi's lifetime, but not for the period after her death. She said that she had sisters (but did not say how many). Nimalkanthi had one sister, and about 18 months after her death, her mother gave birth to another daughter.

Concerning the possibilities for previous acquaintance between the families concerned, we are confident that they had none. Nimalkanthi's family had never even heard of Thusitha when we first met them. Nimalkanthi's father had never been to Payagala; he had passed through it only on his way to a larger town called Kalutara, also on the west coast of Sri Lanka. Thusitha's family had never gone to Kataragama in an effort to verify her statements. Gunadasa Silva said he had hoped to do this, but for various reasons—largely the needs of his tailoring business—he had never got around to this.

In the years 1980–81 Gunadasa Silva had gone "very often" to Kataragama. On one visit only, when she was two months pregnant with Thusitha, his wife, Gunaseeli, had gone with him. Gunadasa had bathed in the river in the usual way of pilgrims and had purchased flowers from the flower stalls near the Kiri Vehera. He could not remember the name—if he ever knew it—of the flower vendor from whom he purchased most of the flowers he bought. Thus he had gone to Kataragama after Nimalkanthi's death, but had stopped going there before Thusitha's birth. Thusitha, incidentally, said that she had seen her father at Kataragama, a reference on her part to a presumed discarnate existence between the death of Nimalkanthi and her own birth.[1]

We made inquiries in Kataragama about the frequency of drownings in the river. The police station had records available only for the three years of 1985–87. There had been one drowning in 1985, none in 1986, and one (up to October) in 1987. The coroner of Kataragama had died in 1986 and his records were not available. The coroner of the neighboring town of Tissamaharama, who had been acting coroner at Kataragama for almost a year (since the death of its regular coroner), had no detailed figures of drownings in the river at Kataragama; however, he estimated that one occurred about

[1] Most children who claim to remember previous lives say nothing about events after death in the previous life and before their birth. Memories of a discarnate existence are particularly rare in Sri Lanka cases. The case of Disna Samarasinghe (Stevenson, 1977) is exceptional. When the children do make comments about such "intermediate" experiences, they frequently include the child's explanation of how it came to be in its family, instead of in some other family.

every two years, mostly among pilgrims. The registrar of births and deaths at Kataragama did not keep records beyond each year, at the end of which the records were sent to the government office (kachcheri) of the next largest administrative area. The records were not classified according to causes of death. The registrar said that there had been no drownings so far in 1987 (contrary to the police records). She estimated that two children drowned in the river each year, a much higher estimate than other sources suggested.

There were 20 stalls of flower vendors on either side of the broad avenue that leads to the Buddhist stupa (Kiri Vehera) in Kataragama. On the day of our inquiries one stall was unattended, but we asked the vendors at all the others whether any member of their family was dumb and whether any member had drowned. One vendor's family had a cousin who was dumb; no other family (apart from Nimalkanthi's) had any dumb member. No family except Nimalkanthi's had lost a member through drowning.

Comment. Despite Thusitha's failure to state correctly any proper names other than that of Kataragama, we have no doubt that we have identified the only family to which her statements could refer. The single detail that her (previous) father had a flower stall near the Kiri Vehera in Kataragama immediately restricted the possibilities to about 20 families. Of these, only one had both a son who was dumb and a daughter who drowned in the river. The various other details Thusitha mentioned are hardly necessary for increasing the correctness of the identification of the family to which Thusitha's statements correspond, although they do provide additional confirmation.

The Case of Iranga Jayakody

Iranga Jayakody was born in Uragasmanhandiya, Sri Lanka, on June 29, 1981. Her parents were M.H.P. Jayakody and his wife, Nimali. Iranga's father was a schoolteacher and an astrologer. She was the seventh and youngest child, and also the only daughter, in the family. Uragasmanhandiya is a small village (population estimated in 1987: 3,100).

When Iranga was between three and four years old she began to talk about a previous life that she said she had lived in Elpitiya, a small town (population estimated in 1987: 6,200) located about 15 km from Uragasmanhandiya. Her family had neighbors one of whom was from a place called Matugama, and Iranga seems to have been first stimulated to talk about the previous life when she heard the neighbor referring to Matugama. She then said that she had had (meaning in a previous life) a mother who came from Matugama. After this, she gradually made a large number of statements concerning the life she claimed to remember. These statements included details of events in the family life, descriptions of the family's house and its surroundings, and the description of a shop where bananas were sold that the previous personality's father had owned. She said that she had three sisters, one of whom was married. She said that she had been attending a

school that was much larger than her present school. At the school she wore a white uniform, but changed into other clothes when she came home from school and studied. She mentioned only one personal name (which remains unverified) and only one place name additional to Elpitiya. This was Matugama, the town from which her (previous) mother came. She did not mention how she had died in the previous life. Iranga also showed several traits of behavior that were unusual in her family and that were subsequently found to correspond with behavior that the subsequently identified previous personality was known to have shown or that would have been appropriate for her. The most remarkable of these behaviors was an extreme modesty about any exposure of her body, especially her breasts, which she first showed when only three years old.

In December 1985, T.J. learned about the case. In the same month he visited Iranga and her parents and recorded a list of 18 statements that her parents remembered Iranga had made about the previous life. In February 1986, Iranga went to Elpitiya with her family to attend a wedding, and while there she pointed toward a road and said it was the way to her previous house. However, her parents had no time then and no interest in pursuing the matter, so they brought Iranga home, somewhat disappointed.

In July 1986, T.J. went to Elpitiya and from his list of Iranga's statements he provisionally identified a family corresponding to them. He did this by inquiring among the vendors of bananas whether any had lost a daughter of school age. T.J. interviewed four of the members of the family and verified all but two of the statements he had recorded from Iranga's parents. The parents of the family had died, and his informants were brothers and sisters of the candidate previous personality.

On August 11, 1986 G.S. and T.J. interviewed Iranga's parents again and recorded another 25 statements not previously recorded by T.J. (and probably not earlier mentioned to him). They then went to Elpitiya and interviewed a member (Podi Haminie) of the family T.J. had earlier identified as the one correctly corresponding to Iranga's statements.

This family had lost a daughter, Punchihamie, who had died on May 5, 1950, at the age of 13. Punchihamie had been ill for a year or more before her death and had been paralyzed on the left side of her body. Doctors in Colombo had diagnosed a brain tumor and proposed an operation, but she was taken home and died there. (We remain uncertain about whether the family had refused an operation or whether the doctors considered the tumor inoperable when they first diagnosed it.) In a further interview with Punchihamie's younger sister, Podi Haminie, G.S. verified nearly all of Iranga's statements.

The following day (August 12, 1986) G.S. and T.J. took Iranga to Elpitiya (with her parents) with a view to seeing whether she could recognize people and places there. Iranga seemed to recognize the old road or path from the highway to Punchihamie's house (not then much used, because a new, wider road provided easier access to the house). However, at the house she did not

clearly (or even vaguely) recognize anyone or any object with which Punchihamie had been familiar. She seemed comfortable in the (for her) strange situation, but not familiar with it in a specific way.

In the third phase of the investigation, I.S. (accompanied by G.S. and T.J.) visited both the families on November 3–4, 1986. We then gave particular attention to the possibilities for normal contact between the families concerned and to the verifications of Iranga's statements. For the verifications we interviewed again two of Punchihamie's sisters, Podi Haminie and Emalinnona. In October 1987, we had another interview with Iranga's mother; and we also visited Elpitiya again, mainly to determine the number and location of the banana vendors.

T.J. and G.S. had recorded (before the two families met) 43 statements Iranga's parents said she had made about the previous life. Of these two were incorrect and three unverifiable or doubtful. One other statement was not literally correct, but could be considered correct from the perspective of a Sri Lanka child. Iranga had said that her younger sister had a bicycle. This was not true of Punchihamie's real younger sister, Podi Haminie. However, the daughter of a neighbor had a bicycle and Podi Haminie played with it. Also, the neighbor's daughter, whose bicycle was played with, was known to Punchihamie's family (in the manner of Asians) as "younger sister." The elimination of these six statements that were wrong, unverifiable, or doubtful left 37 statements all of which were correct for Punchihamie. Some of these might have applied to many village homes in Sri Lanka. This would be true for example, of Iranga's references to a Jasmine creeper and Jak trees at the house. However, many other statements had a much more restricted applicability, and although no single one of them was decisive, taken together they convinced us that Iranga was talking about Punchihamie's life and no one else's.

We will now describe the more important of Iranga's statements that, in our view, specified the family and the person of whom she was speaking. We begin with the fact that Elpitiya is a small town with only two main streets, which are both continuations of highways through the town. We found six boutiques (as small shops are known in Sri Lanka) that sold bananas and learned of three more that had formerly sold bananas, but no longer did so. These were among about 100 boutiques extending along the main roads. The choice among the owners of these few boutiques where bananas were sold became further narrowed by the requirement that the owner have married a woman from Matugama and have had four daughters of whom one had married. Further, Iranga said that the family lived in a house approached along a road through a jungle with rubber and cinnamon trees, and it was both near the boutique and near a temple; the house had red walls and a kitchen with a thatched roof; and a well of the family had been destroyed by rain, but the family still had two other wells, one for washing and drinking and one for bathing. Iranga, in addition to stating, as mentioned, that she had attended a large school to which she went wearing a

white uniform from which she changed on returning home, also said that she had attended a Buddhist Sunday School. She had gold earrings given to her by her father and wore her hair in two plats. She was a middle sister and had a younger sister. All these details were correct for Punchihamie and her family.

Iranga referred correctly to several features of the boutique and house that had been present during Punchihamie's life, but were subsequently changed. For example, the boutique where bananas were sold had had a roof of coconut leaves, but later the roof was changed to one of tile. The walls of the house had been red, but were subsequently painted white. The kitchen had had a thatched roof, but its roof was later tiled also.

We shall next mention and briefly discuss three of Iranga's statements that are unverified or doubtful. She referred to someone called Wijepala. No one in Punchihamie's family could place with certainty a person of that name, although Podi Haminie thought Wijepala might have been the name of an employee. Iranga also referred to her older sister and her mother going to the hospital and returning with a "younger sister." It happened that during Punchihamie's life both her mother and her older sister had given birth to daughters. Both of these baby girls would have been regarded by Punchihamie as "younger sisters." It is possible that Iranga had fused memories of these two births. Iranga said that she had gone to village fairs with her mother. This was correct, but she also said that (on one occasion) she could not find her mother at the fair and then found herself in her (present) family. When we asked Podi Haminie whether Punchihamie had ever been lost at a fair, she could not remember such an episode. She then thought she remembered (without certainty) that Punchihamie had gone to a fair by herself and had there become ill. This was the onset of the illness from which she subsequently died. However, Punchihamie's older sister, Emalinnona, remembered that Punchihamie had first become ill when at school, where she had fainted or collapsed.

Members of the two families concerned in the case had not known each other before the case developed. Iranga's mother said that their family had no connections with Elpitiya; they did their shopping in Uragasmanhandiya. However, Iranga's father had visited patients in the hospital at Elpitiya, and he had sometimes stopped briefly in Elpitiya on his way to other places to which he would travel by bus. Also, Iranga's family had attended a wedding in Elpitiya, so they evidently had some acquaintances there. This does not mean that they knew or knew about Punchihamie's family, and it seems extremely unlikely that they did. That they were unacquainted with Punchihamie's family seems further shown by their indifference to Iranga's effort, when they were in Elpitiya for the wedding, to show them the way to the house where she said she had lived in the previous life. If they had recognized the road as one on which someone they knew lived, they would have remembered this later.

Punchihamie's father had had a relative in Uragasmanhandiya, and he went there sometimes to visit the relative. Also, there had been a well-known monk at Uragasmanhandiya who reputedly had healing powers, and sick persons were sometimes taken to him for healing. Punchihamie's family had taken her to this monk in Uragasmanhandiya only a few weeks before she died. (At that time Iranga's family were still living in Ampurai, far away in the east of Sri Lanka.) Punchihamie's younger sister, when she learned about Iranga, became eager to meet her, and if she had known about Iranga and her family before we brought the two families together, she would certainly have gone to Uragasmandhandiya and met them. We asked Iranga's father whether, when the two families had met, they discovered that they had had mutual friends or other connections, and he said that they had not. There might have been occasions when they happened to be at the same place at the same time in Elpitiya, such as at the bus stand or at the hospital, but this does not mean that they knew each other or had ever formally met. To summarize, our inquiries showed that each family had some acquaintances or relatives in the community of the other family and each had visited the other community; but we are satisfied that they had not known each other before the case developed.

Comment. Many of Iranga's statements taken one by one could apply to a number of families in Elpitiya. Among the sellers of bananas originally questioned by T.J., only two had lost daughters. However, one of these had lost two daughters who were under school age and Iranga had spoken about attending school, as Punchihamie, the daughter of the other banana seller, had done. The identification is additionally further specified by many of Iranga's other statements. When we add detail after detail the collective applicability of all her statements to other persons becomes steadily reduced until it becomes clear that Iranga was talking about the life of Punchihamie and no one else.

The Case of Subashini Gunasekera

Subashini Gunasekera was born in the hospital at Madampe, Sri Lanka, on January 13, 1980. Her parents were M.G.M. Gunasekera and his wife, Podi Menike. They were both schoolteachers. Subashini was their second daughter and fourth (and youngest) child. From before the time of Subashini's birth the family lived in Kuliyapitiya, which is a small town (population in 1987: approximately 5,000) in the western central area of Sri Lanka about 35 km from the west coast. By road it is about 75 km west and slightly north of Kandy.

When Subashini was about 3 years old, she began to speak about a previous life. She said that she had been "trapped" when a hill fell on her house and that this had happened at Sinhapitiya, Gampola. She gave some details of the family she was remembering, including that she had an older brother,

an older sister, a younger brother, and a younger sister. She referred to someone called Vasini who was where she lived, but she did not state who Vasini was; and she did not give a name for herself in the previous life. Gradually she mentioned other details, such as that her family worked on a tea plantation, where her mother and brother plucked tea, and where they had a water tap that could not be fully closed off. She said that when the hill began falling it made a sound like "Gudu, Gudu." Her mother, she said, called her and asked her to take a torch (flashlight) and go out to see whether the hill was coming down on the house. She said that then she was "trapped" and came to her (present) family with the torch.

Gampola is in the highlands of Sri Lanka about 20 km south and slightly west of Kandy and therefore about 95 km by road from Kuliyapitiya. Sinhapitiya is also a small town (population in 1987: approximately 5,000) about 1 km south of Gampola. Subashini's mother had close relatives in villages in the area of Gampola. An older sister lived 10 km from Gampola and an older brother about 15 km from it. She and her husband visited these relatives at least once a year. Podi Menike heard about a landslide at Sinhapitiya in 1977, soon after it happened, but she learned no details about it and read no newspaper report of it. She must not have mentioned the matter to her husband, because M.G.M. Gunasekera said that he had known nothing about a landslide at Sinhapitiya until Subashini began talking about one.

When Subashini was about three years old, her parents attended a wedding in the region of Gampola and Subashini accompanied them. Subashini's father told his in-laws about her statements referring to a previous life. Podi Menike's brother-in-law remembered that some years earlier there had been a landslide at Sinhapitiya with some deaths. Thinking to learn more about the accuracy of Subashini's statements, her father took her along a road on the tea estate where, he had been told, the landslide had occurred. However, Subashini became frightened, screamed, and refused to go on, saying that she was afraid of being "trapped." M.G.M. Gunasekera therefore turned back and did not meet any of the families who had lost members in the landslide. Subsequently, he wrote to his wife's older brother and asked him to make further inquiries. His brother-in-law verified that there had been deaths of workers in the landslide, that the deaths had included members of a Sinhalese family who had been living in "lines" (as described by Subashini), and that a son of the family had been working in a shop in Gampola. That was the sum of all that M.G.M. Gunasekera had verified before we reached the scene of the case. He appeared to have lost interest in it, because he had discontinued his inquiries.

T.J. learned about the case in late 1983, and he first visited Subashini and her family on November 24, 1983. At that time Subashini was not quite four years old, and as she was still speaking about the previous life, he recorded 10 statements directly from her. (Other members of her family subsequently corroborated that she had been stating these details earlier.)

T.J. sent the list of Subashini's statements to us. He also sent a photocopy

of a newspaper report of a landslide at Sinhapitiya that was published (three days after the landslide) on October 25, 1977 in the *Ceylon Daily Mirror.* (This included a photograph of caskets containing the bodies of some of the victims buried in the landslide.) Here we shall digress to describe the landslide briefly. Our information about it came mainly from surviving members of one of the families whose houses were destroyed, from one of their neighbors, and from the owner/manager (I.B. Herath) of the tea estate on which the landslide occurred. The newspaper report mentioned above (and another that we obtained subsequently) also provided information as did a copy of the inquest that we examined. The landslide occurred on a high hill near the upper limits of a large tea estate. Heavy rains had been falling and in the early evening—estimates of the time varied between 7:30 p.m. and 8:30 p.m.—the earth with heavy rocks above a line of workers' houses began falling and quickly completely covered the houses and their occupants. It took some days to recover all the bodies. One informant said that 17 persons had died, but the estate's owner/manager said 28 persons had died. They had all been living in a line of small houses (called "lines") where the workers on the plantations lived. As it was evening, many of the residents were in the houses when the landslide occurred.

To return to our investigation of the case, during 1984–85 we did little fieldwork in Sri Lanka, and it was not until May 1986 that we resumed work on this case. In that month G.S. went twice to Sinhapitiya. He first met I.B. Herath, the owner/manager of the tea estate on which the landslide had occurred, who then arranged for G.S. to meet a surviving male member of one of the families whose houses had been covered by the landslide. This man was H.G. Piyasena, and he verified the accuracy of most of Subashini's statements for the life of his younger sister, Devi Mallika, who, with four other members of their family, including both their parents, had died in the landslide on October 22, 1977. H.G. Piyasena had himself been away from the house at the time, and so he had escaped; accordingly, he could not verify Subashini's statement that when the landslide began, her mother asked her to take a torch and see whether the hill was coming down.

In the next phase of our investigation (August–September 1986), G.S. interviewed both of Subashini's parents and recorded an additional 22 items that Subashini had stated about the previous life. He then arranged for Subashini and her parents to go with him and T.J. to Sinhapitiya, where they were to meet members of Devi Mallika's family at the home of the tea estate's owner/manager. There Subashini recognized H.G. Piyasena by calling him "older brother," but she failed to recognize Mallika, Devi Mallika's older sister, and a neighbor of the family, R.W.K. Banda, who had known Devi Mallika well. (Subashini's recognition of H.G. Piyasena was marred, because he pushed himself forward from a group and stood in front of Subashini; G.S. then asked her "Who is he?" Thus, although she had no verbal clue to his identity, she might have inferred that he was an older brother of the family.)

At the time of this meeting, G.S. went over the complete list of Subashini's recorded statements, which now contained 32 items. He found that all but seven of these were correct for the life of Devi Mallika. Devi Mallika's older brother and sister provided most of the verifications, but R.W.K. Banda also contributed some information.

The party consisting of Subashini's family, G.S., and T.J. took the car that had brought them to Sinhapitiya along the road leading toward the upper levels of the tea estate. They reached the place where Subashini had earlier reacted with fear so that her father had had to bring her away. On this second occasion—three and a half years later—she showed no fear; she also did not seem to recognize any place along the way. The car could not go to the site of the landslide and the party turned back.

In November 1986, I.S. (accompanied by G.S. and T.J.) met Subashini and her parents at Kuliyapitiya. We went over some of the main features of the case again and learned more about Podi Menike's relatives who lived in villages near Sinhapitiya. We then went to Sinhapitiya (near Gampola) and continued the investigation there. It seemed important for us to examine the site of the landslide for ourselves. This required walking uphill for about 4 km from where the estate's jeep could take us no farther. At the site of the landslide and on a neighboring hill we met again Devi Mallika's older brother, H.G. Piyasena, and her older sister, Mallika. H.G. Piyasena took us to the site of the landslide. Abundant vegetation had completely covered the area and no trace of the destroyed line of houses remained. However, H.G. Piyasena showed the sites of some details that Subashini had mentioned. From examining the steep terrain, we could easily imagine how the landslide had occurred. We also saw some of the typical residential lines where laborers on the estate lived and received a vivid impression of the extreme poverty of the families living in these tiny, squalid houses. In this case, far more than in most Sri Lanka cases, the two families were separated widely in their socioeconomic statuses.

In Gampola we examined and copied part of the inquest into the deaths of persons who had lost their lives in the landslide.

In October 1987 we had another interview with Subashini's parents and we went again to the area of Gampola. On this occasion we met and interviewed Podi Menike's sister, brother-in-law, and brother. We also obtained additional information about the occurrence of landslides with deaths in the area of Sinhapitiya.

We mentioned above that all but seven of Subashini's statements were correct for the life of Devi Mallika. The seven exceptional statements were unverifiable or wrong; we think five of them deserve brief mention and discussion. Two of them were the statements mentioned earlier referring to the previous mother having asked her to take a torch and see whether the hill was coming down on the house. From lack of eyewitnesses these remain unverified but are plausible. The house had no electricity and the family used torches at night; also, Devi Mallika was the oldest of three children in

the house at the time and so the one likely to have been asked by her mother to see what was happening. We were also unable to verify a reference that Subashini made to an older brother having come home shortly before the landslide and then gone out again to have his supper elsewhere. One of Devi Mallika's older brothers, Chandrasena, had come home at that time. He then left the house after his father asked him to request another older brother to come to see him; thus Chandrasena escaped being killed in the accident. He had not left the house, so far as we could learn, because his supper was not ready; but it was possible that Subashini had a somewhat muddled memory of this older brother. (We have not yet been able to meet him.) Subashini also referred to an "uncle" who was strict, and he could not be identified. It is just possible that Subashini was referring here to R.W.K. Banda, the neighbor we mentioned earlier. Devi Mallika might have regarded him, in the Sri Lanka manner, as an "uncle." He was in the police force, and Devi Mallika might have associated his occupation with strictness and therefore thought of him as strict. Also, although he was friendly and even affectionate with her, he would sometimes tease her by pretending to be strict. The fifth statement of this group may perhaps be explained as an example of confusion between two closely similar Sinhalese words. Subashini had said—or had been thought by her older brother to have said—that the previous house was near a waterfall. This was not true of Devi Mallika's house, but there was a stream nearby. The Sinhalese word for stream is *ala* and that for a waterfall is *dialla,* hence the possibility of a confusion. (We discuss below an eighth item, a name Subashini mentioned, which is inexact, although we have counted it as correct.)

Subashini used some words and phrases that were not current in her family, but appropriate for the life that she seemed to be remembering. For example, she referred to the previous father by the low country word *Thatha,* whereas she addressed her father as *Apachie,* using the word customary with the Kandyan (up country) Sri Lankans. Devi Mallika had addressed her father as Thatha. In referring to the row of houses called lines, in which laborers on tea estates are housed she referred to *line kamera* and *lime.* Both these terms are used by the residents of the tea estates to refer to these lines of houses. (The word *lime* [in this context] may be a sort of collapsed fusion of *line kamera* or it may derive from the Tamil word *layam,* which means a horse stable.)

We will next describe the reasoning we followed in deciding that Subashini was talking about the life of Devi Mallika and not that of some other person. Subashini had mentioned "the hill coming down" (an obvious reference to a landslide), and she said that she was from Sinhapitiya, Gampola. From I.B. Herath, who had lived in Sinhapitiya all his life (being then 36 years old), from newspaper correspondents of the area south of Kandy, and from two "old-timer" townsfolk whom we interviewed, we ascertained that for the previous 25 years and probably for much longer, there had been only one major landslide with fatalities at Sinhapitiya, that of October 22, 1977.

In that accident, however, Devi Mallika was one of the perhaps 28 persons who were killed, and we need to show how we could decide that Subashini was talking about her life and not that of another person killed in this accident. It happens that although there were about eight houses in the lines destroyed in the landslide, all but one of these were occupied by Tamils.[2] Subashini had made it clear that her family were Sinhalese. She had mentioned Tamils living in the lines, and then some of her siblings had teased her about being a Tamil; this had made her angry, as it would not have done if she had been remembering the life of a Tamil. A further clue for this detail came from Subashini's statement that there had been a Buddhist Temple in the area where she had lived. Sinhalese people are mostly Buddhists, although some are Christians; Tamils are nearly always Hindus. Subashini was therefore referring to the single Sinhalese family living in the residential lines covered by the landslide. In that family, there were 11 children, although they were not all living at home at the time of the accident. In fact, only the three youngest children—two girls and a boy—were in the house with their parents when the accident occurred. These three children and their parents were all killed. Subashini mentioned the father and mother of the previous life and made (correct) descriptive remarks about them, such as that the previous father had a big belly and that the previous mother was larger than her mother. Some of her other remarks, such as references to a blue frock and to a kite (both of which Devi Mallika had), also clearly pointed to the life of a female child of the family, not that of one of the adults. Devi Mallika was the oldest of the three children killed and the only one of them who could say, as Subashini did, that she had a younger brother and a younger sister. She had also given the name of Vasini, not as that of herself in the previous life, but as that of a girl who was perhaps a member of the family. We think the name "Vasini" was Subashini's modified recollection of the pet name of Devi Mallika's younger sister, the baby of the family, who was one and a half years old at the time of the landslide. This child's given name was Chandrakanthie, but her pet name was Vasanthie. This name is closely similar to Vasini. We have no doubt, therefore, that Subashini was speaking about the life of Devi Mallika and no one else. Devi Mallika was about seven years old when she died.[3]

In addition to her statements about the previous life that provided clues to

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[2] The Sinhalese for centuries were independent cultivators and they did not like to become employees of other people. For this reason the British tea planters of the 19th century brought Tamils from India to work on the highland tea estates. Even at the present time Tamils are the principal laborers on the tea estates, and it is somewhat unusual to find Sinhalese among them.

[3] We obtained estimates of Devi Mallika's age at her death that varied widely between a low of three and a half years and a high of seven years. (One of the coroner's records gave her age as four years, but the information for this may have derived from an uninformed neighbor of the family; another of his records gave her age as seven and noted that this information came from her older sister.) We have adopted the age of seven on which Devi Mallika's older sister, Mallika, and the family's neighbor, R.W.K. Banda, agreed.

the identification of the previous personality, Subashini made other remarks and showed behavior harmonious with the life of a poor family living on a tea estate. She was able to describe tea bushes, which she could never have seen in the area where her family lived; it has a distinctly different vegetation from that of the region around Gampola. She commented that her younger brother was given more milk than she, which indicated a life in poverty as did her habit of taking with her tea only a small amount of sugar on the palm of her hand from which she licked it up. (Devi Mallika's older sister, Mallika, said that this was the practice in their family because they could afford so little sugar.) Devi Mallika was particularly fond of her father and slept with him more often than with her mother. Subashini similarly preferred to sleep with her father. Subashini also had a marked phobia of thunder and lightning; the other children of the family had no such phobia.

To conclude the report of Subashini's case we shall mention again the relatives of Subashini's mother, Podi Menike, who lived in villages in the general area of Gampola. Devi Mallika's family had relatives in two villages of this area, and she had been taken there. It is possible that after her death some of her family were in this area at times when Subashini's parents were also there. One may suppose that Subashini's parents or Subashini herself overheard Devi Mallika's relatives talking about the landslide of 1977. If this happened, the occasion would not have been a social one because of the wide disparity in social status between the families. Moreover, we do not believe that Subashini or her parents could have assimilated 25 correct details about a strange family without her parents later remembering at least some of these.

Devi Mallika's family had no connections with Kuliyapitiya, and we can confidently exclude the possibility that Subashini and her family would have learned about Devi Mallika in the area where they lived.

Comment. This case requires less comment than the preceding two. The subject said she remembered a landslide that was unique in a place that she named. She gave details about a family and a daughter of that family that could apply to only one person, a girl who had perished in the landslide.

Summary of Statements by the Three Subjects

We have summarized in Table 1 the statements each subject made, and we have stated the percentage of verifiable statements that were correct. Although we have not conducted a systematic examination of the accuracy of the children's statements in cases of this type, we believe that other subjects whose cases we have investigated showed similar levels of accuracy. We should emphasize, however, that the identification of a deceased person corresponding to a child's statements depends on the specificity of the statements more than on their number. A few specific statements having restricted applicability, such as proper names, may suffice for correct identification when many statements of wide applicability would not.

TABLE 1
Percentage of correct statements made by subjects

	Thusitha	Iranga	Subashini
Total number of statements recorded	30	43	32
Unverifiable statements	2	3	3
Verifiable statements	28	43	29
Correct statements	23	37	25
Incorrect statements	5	3	4
Percentage of verifiable statements that were correct	82%	92%	86%

Discussion

Before discussing the particular strength of the three cases here reported, we wish to place them in the larger context of investigations of cases of this type. Among the approximately 180 cases that we have investigated in Sri Lanka these three cases are among the strongest in the evidence they provide of some paranormal process. However, some other cases are as strong as these three, or even stronger. We think readers can best appraise the strengths of these cases by studying reports of a number of them together—certainly more than three only—and we hope the present paper will stimulate readers unfamiliar with these cases to examine some of our other reports of them and a general survey of this research that I.S. has published (Stevenson, 1987).

None of the three subjects of these cases stated the name of the person whose life they seemed to recall. Indeed, as is almost the rule among Sinhalese subjects, they mentioned few personal names of any kind.[4] However, they all mentioned the names of the places where the previous life had occurred, and they all gave sufficiently specific additional details so that it was possible to identify a deceased person—in each case another child—whose life and death corresponded to the subject's statements.

An important point is whether—given the children's failure to mention personal names—their statements might have applied equally well to other deceased children. We think they do not, but we have tried to furnish enough detail so that other readers may form their own opinion on the matter. A second and equally important point is whether the subjects might somehow have obtained the correct information they showed by normal

[4] In their daily intercourse with each other—even within families—Sinhalese people do not much use personal names in speaking with each other. One of us has offered elsewhere further observations on this habit, which is almost a phobia of using personal names (Stevenson, 1977). Whatever its origin, the reluctance to use personal names probably has a bearing on the infrequency with which the subjects of cases in Sri Lanka include such names among their statements about previous lives they seem to be remembering.

means, and we think that we have shown that in these cases this is extremely unlikely if not impossible. We feel warranted therefore in concluding that the subjects of these three cases had all obtained detailed knowledge about a particular deceased person by some paranormal process.

During the nearly 30 years that have passed since the systematic investigation of these cases began, a variety of interpretations for them have been put forward, both by us and by other persons who have read our reports. The leading interpretations are: fraud, cryptomnesia (source amnesia), unintentional distortion of memories on the part of the informants (paramnesia), extrasensory perception on the part of the subject, possession, and reincarnation. We shall not review the arguments for and against each of these interpretations. Interested readers may study full discussions of them elsewhere (Stevenson, 1966/1974, 1975, 1987). Suffice it to say here that although each of the interpretations that are alternative to reincarnation may be correct for a few cases, all but one break down when applied to most of the cases. The exceptional interpretation, however, is extremely difficult to exclude. We refer to that of paramnesia, which means that, without being aware that they have done so, the informants for the families concerned in a case have so muddled their memories of what the subject said and of what was true about the identified deceased person as to vitiate the case. This possibility has become further elaborated into what we may call the sociopsychological interpretation of the cases. According to it, in a culture having a belief in reincarnation a child who seems to speak about a previous life will be encouraged to say more. What he says then leads his parents somehow to find another family whose members come to believe that the child has been speaking about a deceased member of their family. The two families exchange information about details, and they end by crediting the subject with having had much more knowledge about the identified deceased person than he really had had. Chari (1962, 1987) has been a particularly articulate and long-standing exponent of this interpretation. Brody (1979) gave a succinct as well as a fair exposition of it.

Because we recognize the plausibility of the sociopsychological interpretation, at least for some cases, we attach great importance to the cases of the present group: ones in which someone (ourselves preferably) makes a written record of the subject's statements before they are verified. As mentioned in our Introduction, the present three cases belong to a still small group of 24 cases. However, our recent success in finding the present cases encourages us to think that we can find other cases of the type. Their investigation should assist considerably in reducing the number of possible interpretations of cases suggestive of reincarnation.

Conclusions

In three cases of children (in Sri Lanka) claiming to remember previous lives, written records of the child's statements were made before they were

verified. It was possible in each case to find a family that had lost a member whose life corresponded to the subject's statements. The statements of the subject, taken as a group, were sufficiently specific so that they could not have corresponded to the life of any other person. We believe we have excluded normal transmission of the correct information to the subjects and that they obtained the correct information they showed about the concerned deceased person by some paranormal process.

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Journal of Scientific Exploration, Vol. 2, No. 2, pp. 239–240, 1988
Pergamon Press plc. Printed in the USA.

0892–3310/88 $3.00+.00
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LETTERS TO THE EDITOR

Comment on Beloff's "Parapsychology: The Continuing Impasse"

Professor Beloff (1987) surely echoes the feelings of many besides myself when he characterizes the state of parapsychology as a continuing impasse. In response to his implied challenge, I wish to draw attention to a specific line of research that may offer a way around the obstacles to progress that he describes.

In recent years, much attention has been directed to so-called near-death experiences (NDEs). Persons in life-threatening situations have NDEs in from 35% to 40% of cases (see Ring, 1980; Gallup, 1982; Sabom, 1982); they can also occur in other circumstances. NDEs take a variety of forms, and often include out-of-body sensations, in which the subject has the impression of looking down on his or her body from above and observing the course of events. There are many anecdotal reports of close correspondence between a subject's account and what actually happened, despite the fact that the subject was unconscious, and indeed appeared dead, at the time. Sabom, a cardiologist, interviewed patients who had had such experiences while undergoing cardiac resuscitation and compared their accounts with the written reports of the attending physicians. He claimed that many of the accounts matched closely, and his published examples bear this out (Sabom, 1982). Such correspondences are, of course, evidence for psi if the patients had no alternative way of obtaining the information.

Although questions have been raised about the adequacy of Sabom's controls in eliminating the latter source of uncertainty (Blackmore, 1984, 1985), his findings remain persuasive enough to merit attempts at confirmation (Blackmore agrees), particularly in conjunction with the other reports mentioned above. I suggest, accordingly, that further research of this sort, that is, a series of studies similar to Sabom's (with improved controls), offers a reasonable chance of yielding generally acceptable evidence for psi.

To justify this degree of optimism, let us note how this approach could avoid some of the difficulties discussed by Beloff. NDEs have the virtues of Beloff's first, "spontaneous" category of evidence, but linking the patients' accounts with the medical reports can generate evidence of his third, scientifically acceptable type. Moreoever, the resuscitation-room setting should remove the need for considering the issue of deliberate deception, which as Beloff indicates can be almost impossible to resolve to the satisfaction of critics. This means that the problem of making sure that the patients are not getting all their information from ordinary sensory input, while challenging, should be manageable (possibly by inserting suitably placed visual stimuli in

the resuscitation room). Experience with NDEs suggests that abundant data could readily be gathered. Finally, since research support is hard to come by, it is worth noting that the investigators' own time would be almost the only expense involved.

Actually, in view of the foregoing remarks and the continuing general interest in NDEs, it is noteworthy that Sabom's work has not been followed up. Perhaps Sabom's own experience (I am told that his NDE research hurt his career) discouraged others from following in his footsteps. At any rate, I hope this note will stir up some discussion, and ultimately action, along these lines.

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John Beloff's Reply

The attraction of NDEs from the parapsychological point of view is that they are relatively common. Their drawback is that, as with all spontaneous experiences, verification is a problem. Indeed, it would almost require that at least one member of the resuscitation team should be a trained parapsychologist who would make sure that all the necessary on-the-spot checks were carried out of the patient's account on regaining consciousness and who would even devise suitable tests for ESP beforehand. Consequently, I am not as optimistic as Wilfred Kincaid that NDEs are likely to break this impasse of more than a hundred years' standing which is not to say that I do not welcome further NDE research as a worthwhile adjunct.

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Journal of Scientific Exploration, Vol. 2, No. 2, pp. 241–244, 1988
Pergamon Press plc. Printed in the USA.

0892–3310/88 $3.00+.00
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BOOK REVIEW

Children Who Remember Previous Lives: A Question of Reincarnation, by Ian Stevenson. Charlottesville, VA: University of Virginia Press, 1987, 354 pp. Cloth: $35.00, Paper: $14.95.

This is an impressive book for several reasons: It is admirably written in a style that is both scholarly and readable; the work it presents has been thoroughly analyzed; the experience of the author in the field is considerable; and virtually every question, criticism or suggestion that popped into my mind as I read the accounts or pondered the implications for myself appeared by and by in the text. Moreover this book is only one in a long series of books and articles on the subject of reincarnation that Dr. Stevenson has written in his more than 25 years of study. This gave me the reassuring feeling that there was much to back up the material presented. I never had the feeling that data were being overinterpreted or that conclusions were being grasped for.

Stevenson says he is not out to convince the reader of anything, at least in this one book. He states: "I should regard disapprovingly anyone who, solely from reading this book, moved from skepticism—or ignorance—concerning reincarnation to a settled conviction that it occurs." It is this approach that sets the work of Stevenson apart; after 25 years of accumulating the most comprehensive and unique data base on reincarnation, he still views it and presents it in the Baconian tradition: Let the evidence speak for itself. The University of Virginia files now contain over 2000 investigated cases; 65 have been published as detailed case reports.

Concerning motivation for studying such an inherently uncontrollable (in a laboratory sense) phenomenon, I agree with Stevenson that, "It is better to learn what is probable about important matters than to be certain about trivial ones." Life after death is undeniably an important phenomenon! Apparently 67% of the American population at large believes in an afterlife; apparently only 16% of scientists do.

Following the introduction there is a brief chapter giving an overview of the belief in reincarnation. Voltaire wrote, "It is not more surprising to be born twice than once." This is not a majority opinion in western culture, but our general disbelief or ignorance in reincarnation is probably a minority opinion. Evidence indicates that believers in reincarnation possibly outnumber those who do not on a planetary scale. One can approximately make the generalization that most Christians, Jews, Moslems (excluding Shiites), Atheists and Scientists discount reincarnation, whereas most everyone else has some belief in it.

Stevenson states: "When Westerners who have mistakenly thought that only the people of Southeast Asia believe in reincarnation learn for the first time about the belief among other peoples, they are inclined to imagine that these other people must have concepts about reincarnation similar to those of the Hindus and Buddhists. This is wrong." There are in fact many variations on the who, how, when and why of reincarnation, such as the choosing of one's future family or the working of "karma"—not, by the way, a concept universally associated with the idea of reincarnation. Stevenson himself essentially discounts—thankfully—the idea that one might return as an animal, but some of the other possible elements and aspects of reincarnation can actually be addressed, albeit tentatively, in the assembled data base.

Chapter three discusses the various types of evidence for reincarnation: past life readings by "sensitives" of which practically nothing positive can be said; hypnotic regression, which has more pitfalls and is in the end probably no more effective than conscious recall but of which Stevenson states, "Although I am skeptical . . . I do not reject all of them as worthless"; and various other deliberate or accidental revelations stemming from dreams and nightmares, déja vu experiences, illnesses and drugs, mediation, etc., all of which involve adults. However, it is the spontaneous experiences of children which, Stevenson believes, probably do not arise from a lifelong and uninventoriable clutter of obscure facts and fantasies and so have a particularly pristine character. Moreover the overlaying of present life experiences has not yet crowded out all the data from a putative past life. Those are the motivations for his approach.

The next chapter is a summary of 12 cases, none of which are discussed in exhaustive depth as are the published reports and the books of case histories. Kant said, in referring to the cumulative probability of "ghost stories": ". . . although I doubt each one taken by itself, when they are considered as a group I have some belief in them." The same goes for these accounts. Any self-respecting scientist or lawyer could pick any one apart individually, but collectively they present a respectable case. It is also reassuring that Stevenson himself is ever aware of the deficiencies in the data, the alternate explanations, the methodology problems and the like, and makes no bones about saying so time and again.

The following two chapters, five and six, deal with typical case characteristics and methods of analysis. The most fully developed case would have five characteristics: a pre-mortem prediction of rebirth; an "announcing dream" often to an expectant mother that this individual is going to be reborn; a birthmark somehow characteristic of the former personality; statements of fact by the child usually between the ages of, say, 2 to 6 concerning the circumstances of the previous life; and, lastly, behaviour patterns of some sort that match the previous personality. Few cases, of course, show all of these. The principal memories cluster around events near the end of the previous life and usually include death memories, especially in the case of

violent death, which, by the way, appears to be a selection factor favoring conscious recall in that an exceptional percentage of Stevenson's cases involve a past life ending violently.

In chapter seven the author considers various alternative explanations to reincarnation which might serve to explain his collected data. Yes, he has been hoaxed in a few cases, but the time involved, motivation for profit of some kind thereby, degree of conspiracy necessary among large numbers of individuals and the like argue against this explanation in most cases. Could it be that children have picked up information from their parents or other sources without even being aware of it? Yes, it could be, in some instances, but there are many others where no reasonable, normal channels of communication could have conveyed the reported and subsequently verified information. What about extrasensory perception? Two factors argue against this: First, the children studied usually do not show any other evidence of psychic proclivity, and secondly, it is not just a matter of conveyed information about another personality, but of manifestations of behavioural attributes reminiscent of that person.

A number of cultures include belief in reincarnation, but there is by no means uniformity of opinion concerning the details. The Druses of Lebanon do not believe that a soul can exist apart from a body for even an instant, and so reincarnation for them must take place instantaneously after death, which of course fixes a rigid interval between death and rebirth. They also do not believe that sex can change between lifetimes. Most other "reincarnational cultures" do not believe in any fixed interval, do believe in a discarnate realm, and do allow for a succession of lives as both male and female. Stevenson's cases, in fact, show interesting trends in the area of sex and sex-change. First of all, out of a series of 1095 cases, 62% of the subjects were male. Secondly, girls who remember previous lives as males outnumber the reverse situation by three to one. It is not at all clear what this means, but the author presents a good discussion of how cultural beliefs are likely to provide strong selection effects in reported cases, to the extent of suppressing entirely whatever the culture is not prepared to believe.

The explanatory value of reincarnation in the arena of human behaviour is obvious: Phobias, unusual aptitudes, obsessions, sexual attitudes are all examples of characteristics perhaps better understood in terms of the concrete circumstances of a previous life than by the sometimes convoluted logic of psychoanalysis. It is a fair proposal to make that in addition to genetics and environment, the third factor of past life experiences shapes the human personality.

The author himself admonishes that "it would be rash to generalize from so few cases," but this is in fact what the final two chapters do, and Stevenson does this with all the appropriate caveats constantly stated. To be truthful most readers would be dissappointed if the author did not try to draw inferences; who would not be curious about what to expect when we die? As I stated at the outset, it is the fact that these tentative conclusions and

generalizations are based on much more than just the 12 cases presented in this book that makes me give serious consideration to the outlined possibilities. A number of questions and speculations are thoughtfully presented to bring the book to a conclusion; some examples follow.

There is little evidence to suggest rebirth as other than a human, although just from numerical arguments it is plausible that over the course of evolution consciousnesses could evolve from the animal realm into the human. More than one life is sometimes remembered, and so it makes sense to think of a succession of lives as the process. There is no evidence that suicide leads to a judgmental punishment; it merely carries the irresolution over into another life. The process of obtaining a new life is anything but random, and so it should not be surprising to find oneself involved with some of the same personalities, as from a previous life, although in altered relationships. The idea of reward or punishment would appear to be more a matter of carrying over attributes and attitudes that will help or hinder in a subsequent life. These and other possibilities are discussed in a sensible and always carefully qualified way.

Most scientists deny the possibility that the question of survival after death can be addressed scientifically, and yet studies such as this, along with investigations of near-death experiences, are showing that there are ways to do just that. Dr. Stevenson's work since the early 1960's has succeeded in transforming the concept of reincarnation from a religious notion into a field of scientific inquiry. Stevenson states over and over that the evidence he is gathering must speak for itself. "Reincarnation is the last interpretation we should accept for the cases, and we should adopt it only after we have eliminated all the alternative explanations"; "I do not think any single (case) offers compelling evidence of reincarnation"; "I am not claiming that it is the only possible interpretation for these cases, just that it seems the best one among all those that I have mentioned."

Our own wishes are perhaps the greatest pitfall to research of this sort, for who does not wish to believe that this one life is not the totality of what we may have, but lest we discount everything on account of our wishes, it is appropriate to remind ourselves that it is equally wrong to assume that what we desire must, since we desire it, therefore be false. Stevenson poses the question to the reader as well as it can be put: "After you have read my detailed case reports, I do not think you will say there is *no* evidence for reincarnation, although you may certainly find it unconvincing. If you reach that point, I think it is fair to ask you: 'What evidence, if you had it, *would* convince you of reincarnation?' "

I highly recommend this book.

Bernhard M. Haisch
Editor, JSE

JOURNAL OF SCIENTIFIC EXPLORATION

A Publication of the Society for Scientific Exploration

List of Contents
and
Author Index
Volume 1, 1987
Volume 2, 1988

PERGAMON PRESS
New York ■ Oxford ■ Beijing ■ Frankfurt
São Paulo ■ Sydney ■ Tokyo ■ Toronto

JOURNAL OF SCIENTIFIC EXPLORATION

A Publication of the Society for Scientific Exploration

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Publishing, Subscription and Advertising Offices: Pergamon Press, Inc., Fairview Park, Elmsford, New York 10523, USA and Pergamon Press plc, Headington Hill Hall, Oxford OX3 0BW, England.

Published Two Per Annum. Annual institutional subscription rate (1989): US$75.00; *Two-year institutional subscription rate* (1989/90): US$142.50. *Professional subscription rate* (1989): US$40.00. Prices are subject to change without notice. Members of the Society of Scientific Exploration receive personal subscriptions as part of their dues; details of membership are available upon request. Notify 8 weeks in advance of address change with a copy of the subscription mailing label. *Microform subscriptions*: Back issues of all previously published volumes are available direct from Pergamon Press. Back issues of Pergamon journals in microform can be obtained from UMI, 300 North Zeeb Road, Ann Arbor, MI 48106, USA.

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The Item-Fee Code for this publication is: 0892-3310/88 $3.00 + .00

Printed in the U.S.A.

LIST OF CONTENTS

Volume 1, Number 1 **1987**

CONTENTS

Page		
i	Editorial	*Ronald A. Howard*
1	A Brief History of the Society for Scientific Exploration	*Peter A. Sturrock*
3	Alterations in Recollection of Unusual and Unexpected Events	*David F. Hall* *Susan J. McFeaters* *Elizabeth F. Loftus*
11	Toward a Quantitative Theory of Intellectual Discovery (Especially in Physics)	*Richard G. Fowler*
21	Engineering Anomalies Research	*R. G. Jahn* *B. J. Dunne* *R. D. Nelson*
51	Society and Scientific Anomalies: Common Knowledge About the Loch Ness Monster	*Henry H. Bauer*
75	An Analysis of the Condon Report on the Colorado UFO Project	*P. A. Sturrock*

Volume 1, Number 2 **1987**

CONTENTS

Page		
	Letter to the Editor	
101	Comments on Bauer's "Loch Ness Monster," Volume 1, Number 1, 1987	*James Westman*
103	The Strange Properties of Psychokinesis	*Helmut Schmidt*

119	What Do We Mean by "Scientific?"	*Henry H. Bauer*
129	Analysis of a UFO Photograph	*Richard F. Haines*
149	Analysis and Discussion of the Images of a Cluster of Periodically Flashing Lights Filmed Off the Coast of New Zealand	*Bruce Maccabee*
	Correspondence	
191	Parapsychology: The Continuing Impasse	*John Beloff*

Volume 2, Number 1 **1988**

CONTENTS

Page		
1	Commonalities in Arguments Over Anomalies	*Henry H. Bauer*
13	Remote Viewing and Computer Communications—An Experiment	*Jacques Vallee*
29	Is There a Mars Effect?	*Michel Gauquelin*
53	Raising the Hurdle for the Athletes' Mars Effect: Asscciation Co-Varies With Eminence	*Suitbert Ertel*
	Correspondence	
83	Expanding Ball of Light (EBL) Phenomenon	*Richard F. Haines*
	Book Review	
87	*Origins—A Skeptic's Guide to the Creation of Life on Earth,* by Robert Shapiro	*Henry H. Bauer*

Volume 2, Number 2 **1988**

CONTENTS

Page		
91	Editorial	*Bernhard M. Haisch*

Invited Essays

93 UFOs and NASA — *Richard C. Henry*

143 The Nature of Time — *Yervant Terzian*

Research Articles

155 Operator-Related Anomalies in a Random Mechanical Cascade — *Brenda J. Dunne, Roger D. Nelson, Robert G. Jahn*

181 Archaeological Anomalies in the Bahamas — *Douglas G. Richards*

203 Evidence for a Short-Period Internal Clock in Humans — *Tom G. Slanger*

217 Three New Cases of the Reincarnation Type in Sri Lanka With Written Records Made Before Verifications — *Ian Stevenson, Godwin Samararatne*

Letters to the Editor

239 Comment on Beloff's "Parapsychology: The Continuing Impasse" — *Wilfred M. Kincaid*

240 John Beloff's Reply — *John Beloff*

Book Review

241 *Children Who Remember Previous Lives: A Question of Reincarnation,* by Ian Stevenson — *Bernhard M. Haisch*

I List of Contents and Author Index Volume 1, 1987; Volume 2, 1988

Author Index
Volume 1, 1987

Bauer, H. H., 51, 119
Beloff, J., 191

Dunne, B. J., 21

Fowler, R. G., 11

Haines, R. F., 129
Hall, D. F., 3
Howard, R. A., i

Jahn, R. G., 21

Loftus, E. F., 3

Maccabee, B., 149
McFeaters, S. J., 3

Nelson, R. D., 21

Schmidt, H., 103
Sturrock, P. A., 1, 75

Westman, J., 101

Author Index
Volume 2, 1988

Bauer, H. H., 1, 87
Beloff, J., 240

Dunne, B. J., 155

Ertel, S., 53

Gauquelin, M., 29

Haines, R. F., 83
Haisch, B. M., 91, 241
Henry, R. C., 93

Jahn, R. G., 155

Kincaid, W. M., 239

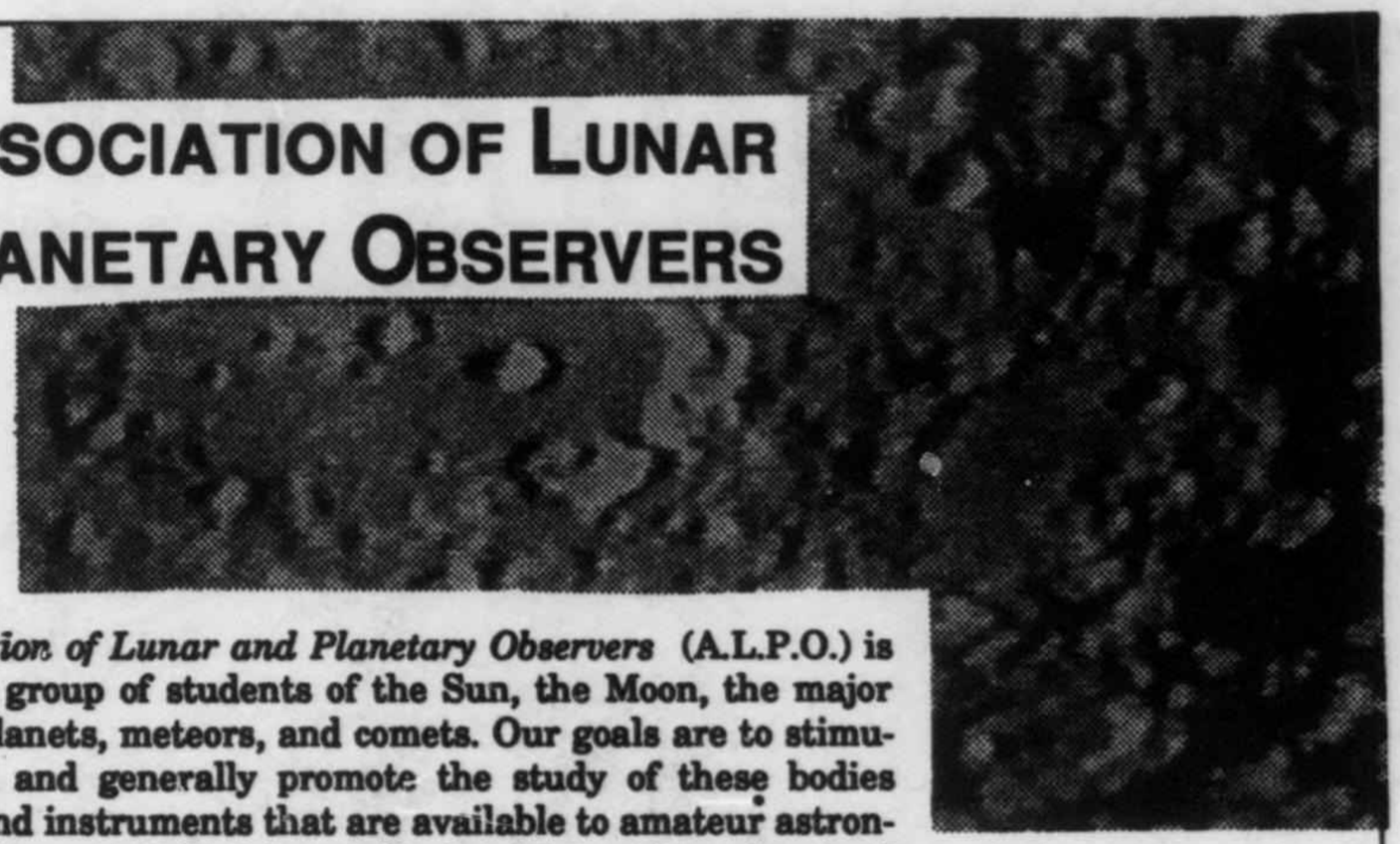
Nelson, R. D., 155

Richards, D. G., 181

Samararatne, G., 217
Slanger, T. G., 203
Stevenson, I., 217

Terzian, Y., 143

Vallee, J., 13



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Subscription Information

Published 4 issues per annum	Volume 22, 1989
Individual Subscription Rate (1989)	US$ 40.00
Annual Library Subscription Rate (1989)	US$ 190.00
Two-year Library Subscription Rate (1989/90)	US$ 361.00

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D R BATES (UK), Recombination in the normal *E* and *F* layers of the ionosphere.
R J MOFFETT (UK), Rates of electron cooling in the upper atmosphere.
M B McELROY, R J SALAWITCH & S C WOFSY (USA), Chemistry of the Antarctic stratosphere.
S M SILVERMAN (USA), Yule analysis of sunspots and auroral phenomena.
O A TROSHICHEV, M G GUSEV, S V NICKOLASHKIN & V P SAMSONOV (USSR), Features of the polar cap aurorae in the southern polar region.
J J LOPEZ-MORENO, R RODRIGO, F MORENO, M LOPEZ-PUERTAS & A MOLINA (Spain), Rocket measurements of O_2 infrared atmospheric system in the nightglow.
T NEUBERT & K J HARKER (USA), Magnetic fields in the vicinity of pulsed electron beams in space.
L STENFLO (Sweden), **M Y YU & P K SHUKLA** (FRG), Scattering of ion-cyclotron Alfvén waves by drift waves in a collisional plasma.
M P FREEMAN & D J SOUTHWOOD (UK), The effect of magnetospheric erosion on mid- and high-latitude ionospheric flows.

Indexed/Abstracted in: *Current Contents, INSPEC, PASCAL-CNRS Database, World Aviation Directory*

Subscription Information

1989: Volume 37 (12 issues)
Annual subscription (1989) **DM 1315.00**
Two-year rate (1989/90) **DM 2498.50**
ISSN: 0032-0633

Advertising rate card available on request. Back issues and current subscriptions are also available in microform.
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Volume 2, Number 2 **1988**

CONTENTS

Page		
91	Editorial	*Bernhard M. Haisch*
	Invited Essays	
93	UFOs and NASA	*Richard C. Henry*
143	The Nature of Time	*Yervant Terzian*
	Research Articles	
155	Operator-Related Anomalies in a Random Mechanical Cascade	*Brenda J. Dunne* *Roger D. Nelson* *Robert G. Jahn*
181	Archaeological Anomalies in the Bahamas	*Douglas G. Richards*
203	Evidence for a Short-Period Internal Clock in Humans	*Tom G. Slanger*
217	Three New Cases of the Reincarnation Type in Sri Lanka With Written Records Made Before Verifications	*Ian Stevenson* *Godwin Samararatne*
	Letters to the Editor	
239	Comment on Beloff's "Parapsychology: The Continuing Impasse"	*Wilfred M. Kincaid*
240	John Beloff's Reply	*John Beloff*
	Book Review	
241	*Children Who Remember Previous Lives: A Question of Reincarnation,* by Ian Stevenson	*Bernhard M. Haisch*
I	List of Contents and Author Index Volume 1, 1987; Volume 2, 1988	

Indexed in CABS **ISSN 0892–3310**
(836)