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Editorial

Nothing seems to be at the same time more refreshing to the spirit or more threatening to it than an idea or experience that does not fit within our personal conceptions of man and the cosmos that surrounds him. It is both fashionable and useful to speak of such anomalies as challenges to the current paradigms of certain fields of human knowledge. Some of these challenges are soon seen to be without substance, often after they have achieved considerable popularity. Others rest at the fringe of acceptance as the result of extensive, but inconclusive evidence. The remainder in the course of time change the paradigms and become commonplace.

The principles by which a new idea, usually suggested by an anomalous phenomenon, is explored and ultimately accorded one of these fates, is itself a worthy subject of study. Those who have become associated with a challenge have been threatened with consequences that range from death, through ridicule, to the benevolent sympathy of friends who wonder why an otherwise sensible person has developed such an interest. The scientists who consider themselves among the most serious students of new phenomena are subject to the same treatment. It is therefore not surprising that the very people who are best-placed to explore anomalous phenomena are those who may perceive the greatest risks in doing so.

The Society for Scientific Exploration is dedicated to using the highest standards of scientific inquiry to study the ideas and concepts that challenge our current paradigms. A major contribution of the Society is its publication of this journal, in which original scientific papers, thoroughly peer-reviewed, will address scientific questions that do not fit well within the current avenues of scientific publication.

Having emphasized the serious purpose of this publication, let me quickly add that serious does not mean dry or arcane. The ideas we shall be exploring are refreshing and provocative — challenges to the imagination as well as to the intellect. To explore means "to seek for or after; strive to attain by search; make a first or preliminary study of; investigate, examine." Exploration should be fun.

Ronald A. Howard

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A Brief History of the Society for Scientific Exploration

In 1981, a number of scientists and other scholars in the United States came to the conclusion that there was a need for a new initiative in scientific research. They could identify a number of phenomena or apparent phenomena that, for various reasons, were not being studied by the scientific community, but were potentially interesting and even important. Indeed, it was clear that some phenomena, if they are indeed real, will challenge some of our established scientific beliefs.

We could appreciate the difficulties of accommodating such studies in the present-day highly compartmentalized structure of science. Universities are, by their very name, dedicated to a comprehensive world view of knowledge. Yet, in fact, universities are like corporations in having divisions (called Schools), and further divisions (called Departments) within Schools. Departmental structure closely reflects the partition of learning among scholarly societies, most journals serving only one branch of learning. Add to these considerations the fact that funding opportunities come mainly from Federal agencies that are similarly structured, and we can see the difficulty of carrying out research that does not fall neatly into presently recognized compartments.

However, the fact that knowledge is presently structured in a certain way, and the fact that certain topics may seem curious or even bizarre, does not mean that these topics should be ignored. It is of course possible for any scientist or scholar to spend some small part of his or her time in whatever kind of research he or she may choose. However, the scientific process does not comprise the uncorrelated work of a few individuals or even many individuals. It has been argued persuasively that the process of science is the process of building a rational consensus based on objective data and analysis. Scholarly publications are essential for the process of building such a consensus. For these reasons, it was clear to the Committee that real progress in the study of anomalies requires the publication of a new journal designed specifically for that purpose.

A journal, however, needs a philosophical and a logistical base. On occasion, a scholar may single-handedly create a new journal by assuming the role and responsibilities of editor. Sometimes a committee will be established for that purpose, but committees tend to grow by adding like-minded new members. To be effective, a journal should be open to all points of view on a given subject, and those responsible for the journal should make every effort to encourage all viewpoints.

For the above reasons, it was clear that the first requirement was to form a new society, and it would then be the responsibility of the society to make plans for the publication of a journal. For this reason, the following scholars

established themselves as the Founding Committee of a new society—the *Society for Scientific Exploration, formed for the Study of Anomalous Phenomena:* Professor George O. Abell, Department of Astronomy, University of California, Los Angeles; Professor Bart J. Bok, Steward Observatory, University of Arizona; Professor Robert F. Creegan, Department of Philosophy, State University of New York, Albany; Professor Persi Diaconis, Department of Statistics, Stanford University; Professor Thomas Gold, Center for Radiophysics & Space Research, Cornell University; Professor Robert Jahn, School of Engineering & Applied Science, Princeton University; Professor Roger N. Shepard, Department of Psychology, Stanford University; Professor George L. Siscoe, Department of Atmospheric Sciences, University of California, Los Angeles; Professor Ian Stevenson, Department of Psychiatry, University of Virginia Medical Center; Professor Peter A. Sturrock, Institute for Plasma Research, Stanford University; Professor William B. Thompson, Department of Physics, University of California, San Diego; Professor James Trefil, Department of Physics, University of Virginia; and Professor Marcello Truzzi, Department of Sociology, Eastern Michigan University.

The Founding Committee was too small to constitute an adequate base for a new society. For this reason, the committee invited many of their colleagues to join in their enterprise, and eventually drew up a list of one hundred Founding Members. These Founding Members were distinguished scholars, all of whom had faculty appointments in the United States or Canada.

The Founding Committee nominated the first set of officers of the new Society. Thereafter officers have been elected by the full members of the Society. The first meeting of Council was held at the Joseph Henry Building of the National Academy of Sciences in Washington on January 5, 1982, and the first annual meeting was held at the University of Maryland on June 3 to 5, 1982. Subsequent meetings have been held at the University of Virginia, at Princeton University, again at the University of Virginia, in San Francisco, and most recently in Austin. The Seventh Annual Meeting is scheduled for June 2 to 4, 1988, at Cornell University.

The Society has published and continues to publish a semi-annual newsletter, the *Explorer.* However, one of the prime objectives of SSE has been the publication of this journal, the *Journal of Scientific Exploration.*

We hope that you, dear reader, will enjoy and be stimulated by the contents of this journal, and we invite you to join with us in this exciting venture of intellectual exploration.

For further information about SSE, please contact the Secretary, Professor Laurence W. Fredrick, Department of Astronomy, P. O. Box 3818, University of Virginia, Charlottesville, VA 22903.

Peter A. Sturrock
President of SSE

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Alterations in Recollection of Unusual and Unexpected Events

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Introduction

One of the authors, Elizabeth F. Loftus, has for some years been investigating the circumstances under which information received subsequent to a complex natural event, such as an accident or a crime, causes systematic and predictable changes in witnesses' recollections of the event (see Loftus, 1979, for a review of this research). Sightings of unidentified flying objects (UFOs) and of unknown or rare animal species (the topic of cryptozoology) are in some ways very similar to the witnessing of a crime or of an accident. In particular, crimes, accidents, UFOs, and unidentifiable animals are all phenomena that present considerable challenges to accurate perception, interpretation, and recall.

In the present paper, we first very briefly review some recent findings on the alteration of human recollection. This review is necessarily less than comprehensive, but we hope that it gives at least the flavor of recent research. Next, we suggest some applications of recent recollection-change research to the evaluation of reports from persons who claim to have witnessed anomalous events, such as the appearance of UFOs.

The Alteration of Eyewitness Recollections

Numerous experiments have been conducted showing that memory for details of a complex event, such as a crime or an accident, can under some circumstances be affected by both pre- and post-event experiences. The experimental paradigm used in recollection-change studies can be described in terms of two essential stages. (1) Acquisition: A subject views for the first time a slide sequence or a film depicting an event, such as a simulated automobile accident or a crime. (2) Retention and change: A subject encounters and incorporates additional information subsequent to the original event. New information might be embedded in a misleading message or in a biasing question, or even in a sketch or a photograph. Private rehearsal of the event, or discussion with friends or family, or even questioning by a careless investigator, can be a source of misleading opinions and information. In other

cases, misleading information is derived not from current sources, but from older memories that are revived in the course of reminiscence. Whatever the source, additional information is acquired and is often readily integrated with original memory for the event. Thus, both pre- or post-event information has in fact altered the content of what is recalled or recognized. Once created, the new memory can be as real and as vivid to the person as a memory acquired as the result of "genuine" perception.

Loftus, Miller, and Burns' (1978) experiment can be regarded as a prototype of the recollection-change procedure. In that experiment, subjects viewed a slide sequence that depicted an automobile accident. In the middle of the sequence one group of subjects viewed a red Datsun stopped at a yield sign, while a second group of subjects viewed the same red Datsun stopped at a stop sign. Either immediately after the slide presentation, or following some longer interval of delay, one of two sets of questions was distributed to each subject. Some subjects were given questions that included a potentially misleading item of information, whereas other subjects received questions that included no misinformation. For example, the item "Did another car pass the red Datsun while it was stopped at the stop sign?" contained misinformation when it was asked of subjects who had actually viewed a yield sign. Finally, subjects were tested for recollection of details of the original incident, including recollection of the sign. Depending somewhat on the intervals between viewing the accident and receiving subsequent questions, and between the accident and the final test of recollection, as many as 80% of the subjects indicated that the content of what they remembered had been altered by the post-event misinformation. Specifically, they recalled a stop sign when a yield sign had actually been seen, or a yield sign when a stop sign had been seen. Thus, memory for a complex natural event appears to be vulnerable to the influence of information received subsequent to the event itself. What follows is a summary of some of the major experimental variables that have been found to moderate the alteration of memory by post-event information.

Critical Factors in Recollection Change

It is not possible in this paper to offer a comprehensive review of the growing body of recollection change literature. Short of being comprehensive, it is possible to briefly describe several variables that are already fairly well understood, and to summarize some of the experiments that served initially to demonstrate the significance of these variables. Such relatively well understood variables include (1) the intervals between an event, a subsequent misleading message, and a final test of recollection; (2) the presence of warnings; (3) the syntactic form of questions and messages; and (4) attitude change.

Intervals of Delay

The intervals of delay between viewing an initially unexpected event, encountering a subsequent potentially misleading message, and engaging in a

final test of recollection have all been found to be major determinants of change in memory for an event (Loftus et al., 1978). In this research, the number of subjects for whom a change in recollection occurred increased with longer as opposed to shorter intervals before encountering the post-event misinformation. Additionally, the number of subjects for whom a change in recollection occurred was greatest when the final test of recollection occurred immediately after reading the post-event misinformation rather than after some delay. Thus, recollection change appears to be enhanced by the fading of original memory with the passage of time, and the change is most readily secured if tested while the post-event misinformation is still relatively current.

Warnings

A series of experiments reported by Greene, Flynn, and Loftus (1982) indicates that the alteration of recollection can often be minimized if subjects are warned that a post-event message that they are about to receive might contain misinformation. However, the immediacy of the warning appears to be critical. That is, to be effective, the warning must be given immediately before presentation of the otherwise misleading message.

Subjects began by viewing slides that depicted a wallet snatching. Five minutes after viewing, subjects were exposed to a version of the event ostensibly written by another witness. For some subjects the version included some items of misinformation. Some of the misinformed subjects were warned that the message might contain inaccuracies. For different groups of subjects, the warning was given either immediately before the slides were presented, immediately after the slide presentation, immediately before the misleading message was presented, or immediately before the final test of recollection. Finally, a test of recollection was given and subjects were urged to respond on the basis of their personal memories.

The results indicated that warning given immediately prior to the presentation of misinformation increased subjects resistance to the misinformation and minimized the changes in recollection. However, warnings given at other times, earlier or later, were of little or no value to subjects. Another variable in the study, the amount of time taken by subjects to read the message, provided a clue to the interpretation of these results. In brief, subjects who received warnings immediately prior to reading the misinformation read more slowly, and no doubt more carefully, than did subjects who received warnings at other points in time.

In sum, there appears to be one optimal moment when warnings are effective in reducing subjects' usual gullibility and lack of caution. That critical moment comes just as subjects are about to be exposed to post-event misinformation.

The Syntactic Form of Misleading Questions

We have seen that properly timed warnings make it possible for subjects to detect discrepancies in a post-event message. In a rather similar manner,

the syntax of a potentially misleading message can also serve to conceal, or to reveal, the misinformation.

In one experiment (Loftus, 1981; Loftus & Greene, 1980), students attending a university lecture were surprised by an intruder who suddenly entered the lecture hall, loudly insulted the professor, and then just as suddenly departed. Immediately following the unexpected event, students answered a 15 item questionnaire calling for recollection of details of the incident.

For some students, the questionnaire included one item that referred misleadingly to a nonexistent moustache. In fact, two versions of the misleading question were presented to different groups of subjects. In one version, the moustache was the subject of a simple interrogative sentence: Was the moustache worn by the tall intruder light or dark brown? In the second version, the moustache was the object of an auxiliary clause: Did the intruder who was tall and had a moustache say anything to the professor? A third group was not asked anything about the moustache at all.

Two days later, subjects were once more tested for recollection of details of the incident. In this final test, subjects were asked to indicate whether they had actually seen a moustache on the intruder. Many of the subjects who had been misleadingly questioned about the moustache reported that the intruder had indeed been wearing a moustache. Few of the subjects who had not encountered a misleading question believed that there had been a moustache. Equally significant, subjects were more likely to falsely recall the nonexistent facial hair if it had been suggested in an auxiliary clause than if it had occurred as the subject of a question.

A clue to explaining the greater effectiveness of the auxiliary clause lies in the complementary finding that most subjects, upon further interrogation, were typically unable after two days to recall having read a reference to a moustache when the moustache had been mentioned in an auxiliary clause. However, subjects were more likely to recall having read about a moustache when it appeared as the focus of an intervening question. It seems plausible that more attention would be given to the subject of a simple sentence than to the object of a minor clause in a complex sentence. We conclude that subjects' recollections are more readily altered by misinformation that has been casually or unintentionally assimilated than by information that has been given direct and critical attention. We also conclude that misinformation that has been slowly scrutinized is likely to be rejected, whereas misinformation that is quickly assimilated with minimal attention can be added indiscriminately to the existing store of information about an event.

Attitude Change

Apparently, a change in attitude can under some circumstances induce a corresponding change in recollection. Recent studies (Ross, McFarland, & Fletcher, 1981; Ross, McFarland, Conway, & Zanna, 1983) have shown that people are typically revisionist historians with respect to their autobiographies. In a typical experiment, subjects received a persuasive message intended to

alter their attitudes towards the benefits of vigorous physical exercise. The message was attributed either to a highly credible source, a leading researcher in the area of exercise, or alternatively, to a less credible source, a spokesperson for the Fat is Beautiful Organization. Not surprisingly, attitude change was greatest in subjects for whom the persuasive message had been attributed to a credible source. Finally, subjects were asked to recall their own recent exercise programs. Interestingly, subjects exposed to the credible communicator recalled their exercise routines as having been less vigorous than did subjects exposed to the less persuasive communicator. In short, people sometimes alter recollection of their own recent behavior to conform to newly acquired attitudes.

In summary, we have reviewed a number of studies indicating the capacity of several variables to affect the alteration of recollection. In several of these studies, the subjects' detection of discrepancies in a post-event message, or failure to detect discrepancies appears to have been a crucial factor. Indeed, retention intervals, warnings, and syntax are all variables that appear to affect subjects' ability to detect discrepancies. Thus, the detection of discrepancies appears to be an important mediating variable, that is, a common underlying factor in the operation of a number of circumstantial variables. In a similar vein, if people assume a correspondence between their present attitudes and their past behavior, then they are likely to recall selectively supporting evidence and to overlook, or to fail to recall, that which is less supportive. Thus, attitude-consistent information is strengthened in the process of recollection, and attitude-inconsistent information fades or is replaced. In any case, the basic fact that newly acquired information, as well as recently changed attitudes, can alter recollection has been well established. We will turn now to the specific problem of assessing the likelihood of recollection alteration in eyewitness accounts of UFOs.

Post-event Information and the UFO Witness: Betty Hill

Perhaps the most famous UFO witness is Betty Hill (Schwarz, 1977a, 1977b; Story, 1980). In any case, enough has been reported about the Hill case to allow consideration of several factors that could have contributed to the alteration of Betty Hill's recollection of an unusual event.

Betty Hill believes that she and her husband, Barney, were abducted by humanoids on the night of September 19, 1961, taken aboard an extraterrestrial vehicle, examined, and then apparently hypnotized by the humanoids before finally being released. According to Betty Hill's account, the humanoids had hypnotized the captives to prevent them from being able to recall the encounter. Nevertheless, Betty had disturbing dreams that suggested the experiences of a humanoid abduction. Eventually, in 1964, Betty Hill was able under hypnosis to recount a rather peculiar experience to psychiatrist Benjamin Simon.

It matters not at all, for purposes of the present analysis, whether the Hills were really abducted by humanoids. Obviously, we have no basis for answering

such a question. What we can accomplish is simply to identify some salient factors that are likely to have affected recollection in such a case.

First, it is significant that Betty Hill first recalled the event three years after it is purported to have occurred. As we have noted, the passage of time between an event and eventual recollection increases the likelihood of recollection change.

Second, Betty Hill had dreams that included images of humanoids and of spheroidal craft. Such dreams are apparently common, even among people who have never been abducted by extraterrestrials (Jung, 1959). Betty discussed these dream images with her husband and with her psychiatrist. It is conceivable, from the perspective of the present analysis, that dream images could have been a source of misleading and erroneous information. That is, Betty Hill could have incorporated the memory of a dream with the memory of a waking experience.

Third, Betty Hill's psychiatrist, Benjamin Simon, evidently supported Betty's interpretation of her memories, including the interpretation of her dreams and of hypnotically induced experiences, as being recollections of a genuine encounter with extraterrestrials. Recent research indicates that hypnosis often increases a witness's susceptibility to the memory-altering effects of misleading messages (Putnam, 1979; Sanders & Simmons, 1983). Thus, by placing his client in a state of hypnosis, and by allowing her to discuss dream images while in that hypnotic state, psychiatrist Simon may have inadvertently facilitated the alteration of recollection.

Fourth, Betty Hill claims to have had many other inexplicable experiences, both before and after the abduction. Betty Hill evidently interprets many events as being paranormal and is usually able to recall details that support such paranormal interpretations. It appears quite plausible that Betty Hill's recollections, regardless of their validity, have been affected by her favorable attitude towards paranormality as an explanatory concept.

In brief, dreams, hypnotic imaginings, and discussions with her spouse and with her psychiatrist are likely to have provided an adequate amount of misleading information to result in at least some degree of change in memory for Betty Hill. The length of time that elapsed between the original incident and eventual recollection and the witness's favorable attitudes towards paranormality may have contributed to memory alteration, too. The Hill case is one of the most widely credited UFO sightings. We have shown that the possibility of recollection change must be considered in an analysis of any such eyewitness account of a complex and obscure event.

Conclusions—How to Assess the Likelihood of Recollection Change in Eyewitness Accounts of Unusual and Unexpected Events

An eyewitness's recollection of an unusual and unexpected event, such as the appearance of a UFO or of an unidentifiable animal, is likely to undergo alteration, and thus to be relatively less reliable, if:

1. A considerable period of time has elapsed since the original event. "A considerable period of time" is, of course, a relative concept. There are no absolute temporal bounds for reliable recollection. However, laboratory studies have shown that intervals of 30 minutes or less can be critical in memory for the kind of complex events that we have been considering.

2. The witness has been recently exposed to potentially misleading post-event information. Such information might be acquired through messages, interviewers' biasing questions, the witness's own reminiscence, discussion with friends or family, dreams, reading material, or any of a number of other sources. Often, subtle, easily overlooked sources of misinformation have been shown to have the most devastating effects on the reliability of eyewitness reports.

3. The witness holds attitudes, or has recently undergone a change of attitude, that would induce a particular bias in the recollection of details of an event.

Some exposure to potentially misleading information and some consequent risk of adulteration of eyewitness reports is, perhaps, unavoidable. The impact in any given case of such factors as the witness's own reminiscences or of conversations with friends can only be roughly assessed. Other sources of potentially misleading information can, however, be controlled. Biases of the principle investigator or of the field interviewer, for example, are sources of potentially misleading information, but researcher- and interviewer-bias effects can be curtailed with procedural and methodological controls (Rosenthal, 1966; Barber, 1976). Such controls would include the following: (1) standardization of interview protocol; (2) careful wording of questions to avoid inadvertent clues or biases; (3) extensive training and rehearsal of interviewers; (4) employing interviewers who have not been informed of research hypotheses; (5) "blind" control, that is, leaving the interviewer uninformed about the interviewee's identity, or status, within the design of the investigation; and, finally, (6) creation of a "quasi-experimental" research design (Campbell & Stanley, 1966), that is, employment of appropriate control groups, for example, a group of bogus witnesses who would enact the role of "actual" UFO witnesses. Controlling the effects of researcher-and-interviewer bias is especially important when the research depends on the validity of an eyewitness's report of an otherwise unverifiable and nonreplicable event.

In short, biased questioning by a careless investigator or recent discussion with family or friends can be a source of misleading opinions and information. Added information is acquired and is often readily combined with original memory for the event. When created, the new memory can be as real to the person as a memory acquired as the result of true perception.

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Toward a Quantitative Theory of Intellectual Discovery (Especially in Physics)

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Abstract—By the study of time intervals in a subjective yet consistently chosen temporally ordered list of the critical ideas which comprise Physics, a quantitative theory of the growth of these ideas is inferred which takes the entirely plausible form that the rate of growth of ideas is proportional to the totality of known ideas multiplied by the totality of people in the world. There is some slight titillating indication in the data that the rate of fundamental discovery in Physics has been decreasing abnormally over the past 50 years.

Introduction

Humankind have never ceased to marvel at the flow of their ideas, and to wonder about the process. Of late the focus has been on the seemingly accelerated pace of acquisition of knowledge, and many reasons have been advanced, ranging from the very perceptive observation of Henry Adams (1918) "ideas beget ideas" to the fatuous suggestion that people are getting smarter. Rescher (1978) has analyzed the existing body of thought about this question in a recent book. In a series of papers, the author, (Fowler, 1982, 1983) has shown that the proper variable in terms of which human questions should be discussed is not calendar time, which is an arbitrary and hence largely meaningless idea, but rather population numbers. In this light it will be shown that a quantitative formulation of the problem of idea growth may be possible. The development of this concept has depended upon data taken from the special body of knowledge known as Physics, and its applicability may be limited to that field. Any further generalization to other or larger spheres of ideas will be left to others.

Idea Magnitudes

Knowledge consists of an enormous number of discrete and overlapping ideas. To form a complete list even in a well defined field such as Physics defies the effort of one person, and would amount to compiling the indexes of perhaps a hundred thousand books. We require such a list of ideas and the point in time at which they occurred if we are to make any progress with this problem. Ideas, like stars, are not all of equal brilliance. To deny this is to argue against the validity of Nobel Prizes. We will use this generally accepted fact to render our problem tractable. First magnitude ideas, like stars, occur

in clusters of lesser ideas which, unlike stars, usually have an intimate relation to the fruition of the "great" idea. In making the list given in the Appendix, the author drew on the experience of the many writers over the past century who have selected topics for their general physics texts, as well as upon the insights in topic selection of the numerous historians of the field. A few subjective rules were employed in making the list:

1. A major idea is one without which no further progress in all or one of the major subdivisions of the understanding of Nature could be made.
2. The list should be sufficient as a basis for a complete beginning course or an encyclopedia article.
3. In dealing with a cluster of discoveries like the mesons, the first discovery/idea was included because it was first. The second was also included because it showed multiplicity. But no more ideas from that family were counted until a novel or unifying concept was involved.
4. Each principle included was chained backward as well as forward in order to answer the question of what crucial concepts must have been known before it could be perceived.

A similar list of major ideas that appeared over a shorter time span was prepared by Auerbach (1910). Auerbach's list, continuing the stellar analogy, might be called a list to the second magnitude (500 items in 150 years). After augmenting Auerbach's detailed list, Rainoff (1929) proposed that creativity had been subject to wavelike fluctuations over this period. The perspective of this present article is to consider a wider sweep of history. While it is not essential that the list should contain all of the topics requisite to its full elucidation, it is important that they be of equal magnitude. It should be remarked that Rainoff's hypothesis is not borne out by the analysis presented here.

In assessing the merit of the conclusions reached in this paper it is important for the reader to know the origin of the idea list on which they are based. Had it been derived purely for the purpose of this analysis itself, it would be easy to discard the result on grounds of special pleading in selection of the items. Such however was not the case. The initial list, comprising 90% of the final items, was prepared by the author in 1966 in response to a request from the editors of World Book Encyclopedia to assist them in analyzing the completeness of their coverage of the discipline of Physics. Only some time afterwards did the question proposed in the introduction occur to the author. Subsequently, certain additions to the original list were made for two reasons. First, when the problem was attacked seriously in 1979, a few essential ancient ideas which are not specifically "physical" in nature and a very few overlooked modern ideas were added (marked with a *). Second, in 1982 a conscious attempt (marked with a #) was made with the cautiously solicited advice of others to overcome the apparent deficiency in the numbers of ideas since

1920. Neither set of modifications alters substantially the basic thrust of this paper, nor was the contemporary deficiency removed.

Relationship of Ideas to Population

Historians supply us with dates for the various discoveries, but all that is revealed by using time as an independent variable is the well known observation that ideas are coming faster and faster to mankind. It is only when we compare our list to the populations prevailing at the time of the discovery that real understanding emerges. To obtain population numbers, we turn to the many demographers who have made studies, estimates, and good guesses. I have analyzed these in the previously cited article (Fowler, 1983) where I showed that the simple empirical relation (N = global population)

$$(1) \qquad dN/dt = \alpha N^2$$

has in recent times, and perhaps even for a million years, described human world population growth far better than the conventionally invoked exponential law. In all that time, it appears, the constant α has had only two values, 5.08×10^{-12} after the Renaissance, and 2.1×10^{-12} before. Using this second value for smoothing and extrapolation to ancient times, populations prevailing at any discovery time have been calculated and are given in the table in the Appendix.

I was first led toward the thought that there might be something interesting to be said about idea growth by having made a crude plot of the mean of the time intervals to the discovery next before and next after a given discovery against the population prevailing. A few samples of points generated by this rough method are given as hollow circles in Fig. 1 to show that even it foreshadows the final result. Curvefitting the complete set of this crude data on the period between ideas yields the power law $T = 2.5 \times 10^{18}/N^2$ which does not fit badly over three orders of magnitude in N. T is directly related to the reciprocal rate of growth of I, the idea total; that is to say, to dI/dt, but only loosely because of the inordinately large steps used in the early years, and so the precise relation above does not stand up in the final analysis. To obtain a better result we need to apply methods of finite differences.

The raggedness of the previous plot came because it is obtained by differentiation of raw data. A better method is to smooth the data first by forming the sum of the number of ideas prior to any given population level as shown in Fig. 2. This curve too is a power law $I = kN^a$, but is much more clearly defined, with two regions, in which k and a take the value pairs (.0156, .335) and (1.675×10^{-5}, .714) respectively. Since we know the temporal dependence of population, we can use the integrated curve first to determine the expected behavior of the dI/dt curve, and later to assist us in the calculation of more accurate data points for that curve. Thus

$$(2) \qquad dI/dt = kaN^{(a-1)}(dN/dt) = ka\alpha N^{(a+1)}.$$

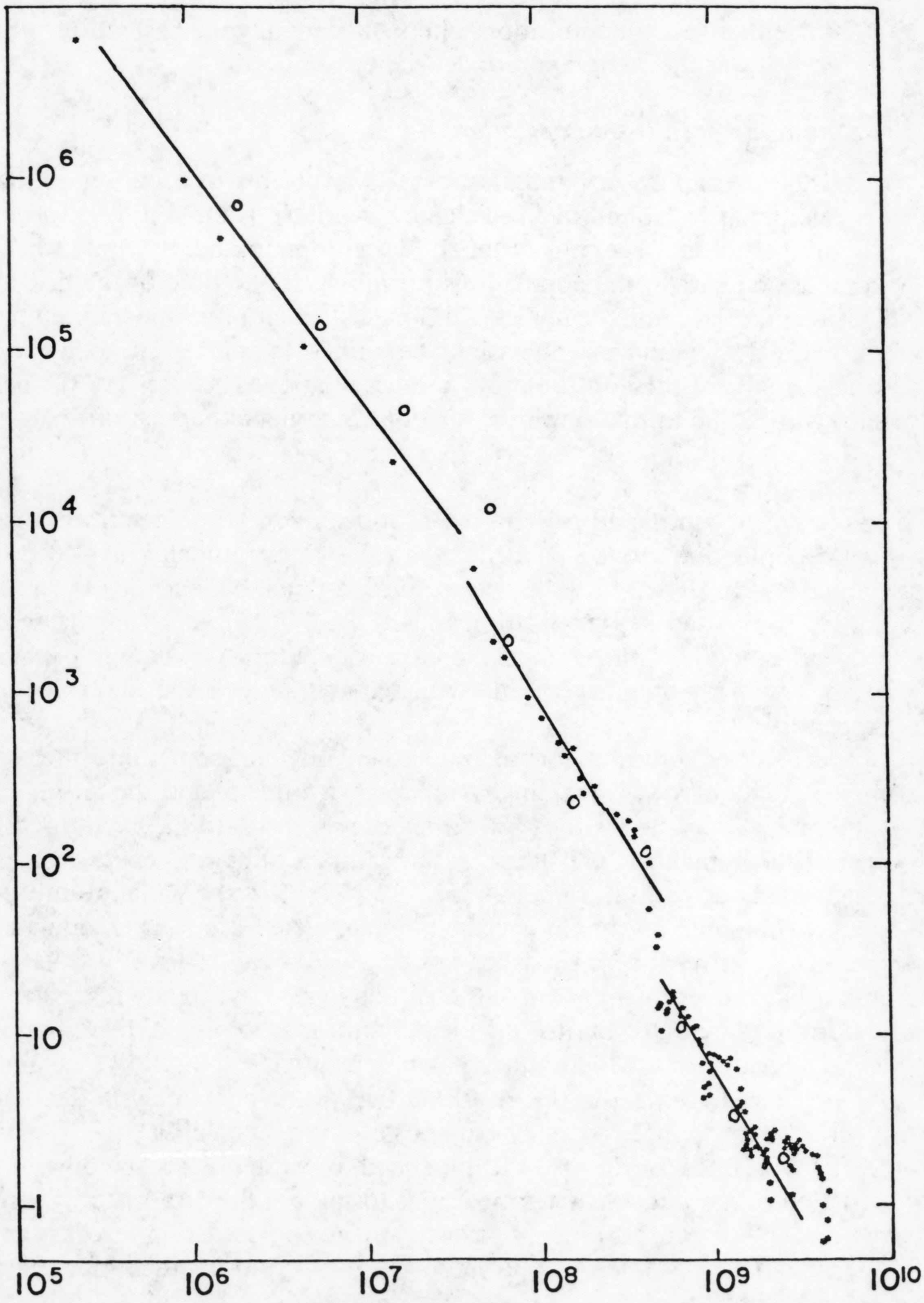


Fig. 1. Time intervals between ideas plotted against population. Open circles are examples of simple time differences between adjacent ideas. Solid dots are values of dt/dI obtained by finite difference methods, smoothed over five successive ideas. Curve segments are dt/dI from the theory.

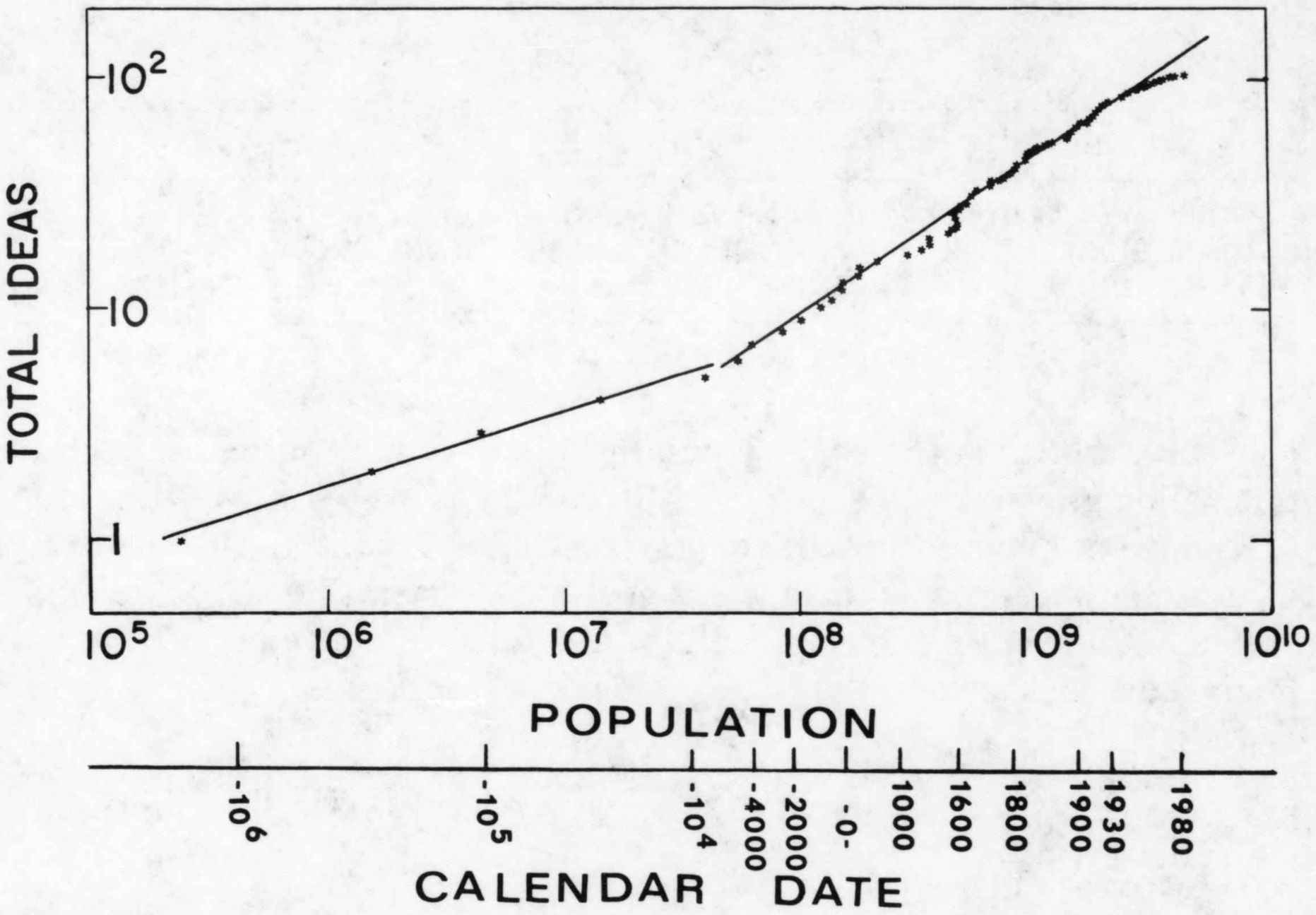


Fig. 2. Accumulated (integrated) number of ideas plotted against population. Note slope change at the fifth idea.

This leads directly to a quantitative reformulation of Henry Adams' hypothesis (cited in the introduction) which must be intuitively obvious to everyone as soon as it is stated in words. The equation is

$$\mathrm{d}I/\mathrm{d}t = a\alpha IN. \tag{3}$$

Translated, this equation becomes the theorem: "the rate of generation of ideas is proportional jointly to the number of minds to which they can occur, and to the number of ideas already available for those minds to use."

This result can be written in another form:

$$I = \exp\left(a\alpha \int N\,\mathrm{d}t\right). \tag{4}$$

This equation makes the slightly different statement that the accumulation of ideas (knowledge) grows exponentially with the number of man years lived.

The differential equation, Eq. (3), for idea growth is the classical model of any random creative collision process such as electrons colliding with atoms to produce new electrons. Analogously, therefore, we might think of idea creation as the result of a "collision" between minds and ideas. The coefficient α is then the probability of the process occurring per person per unit time. The data in Table 1 show that there have been three major changes in this rate throughout history. These data are computed from the values of the

TABLE 1
Population constants

Interval	$1/a\alpha$	$a\alpha$
$-1 \times 10E6$ to -7000	$1.44 \times 10E12$ man-years	$6.93 \times 10E-13$/man-year
-7000 to $+1650$	$6.67 \times 10E11$ man-years	$1.50 \times 10E-12$ man-year
$+1650$ to $+1930$	$2.76 \times 10E11$ man-years	$3.63 \times 10E-12$ man-year

constant a obtained from Fig. 2, and the values of the constant α from the population growth law.

Knowledge of the dependence of I on t enables us to relate the true derivative dt/dI to the experimental data which goes by unit steps. If $\langle a(I)\rangle$ and $\langle k(I)\rangle$ are the local average values of a and k at I fitted over five points, the time interval per idea at I is

$$dt/dI = \frac{\langle a(I)\rangle}{\alpha\langle k(I)\rangle I^s} \tag{5}$$

where $s = 1 + \langle a(I)\rangle$.

If the span of the calculation is over adjacent ideas the irregularity of their spacing makes a very erratic, although not unusable, curve. After trying several values of the number of points in a group, n = 5 was selected. We can now see very definitely that there is a break in the time interval curve at the same point as in the accumulated idea curve. After considering and rejecting the possibility that overlooked ideas or incorrect estimates of population could account for the break, the nature of the discovery at the break came to my attention. It was the invention of writing. The felicitousness of this fact seems hardly accidental. Writing definitely increased the probability that a wider range of minds would interact with the existing ideas. The data indicate that the interaction increased by a factor of two. A priori one might expect that a similar acceleration would have occurred at the invention of printing, but such was not the case, from which we might conclude that dissemination of ideas over space was already largely adequate, and not as important as propagation through time. Although there was no break in the slope of the accumulated idea curve around the 15th century, as plotted against population, there would have been a very sharp break if it had been plotted against time. This shows itself as a second break in the idea rate curve and was caused by the acceleration in population growth at this time rather than by a change in the idea process itself. It is tempting to suggest that the development of printing was a technological advance, and as such had its influence upon technology rather than science directly, and hence in some complex manner led to the mediaeval change in population growth rate which in turn altered the idea growth rate. Ideas per year increased in 1500, but ideas per person did not.

We may now complete the study of the data by plotting the analytical

expression for dt/dI upon the experimental points, as shown by the solid lines in Fig. 1. This clearly reveals the segmented character of the data, which we can now see was indicated by the original roughly calculated points also.

Conclusion

What might be the basis for the inference that idea growth in Physics has depended on the totality of minds everywhere, when we all know that most of the recent advances have taken place in the Occident among a few highly educated people? In the matter of universality, it is easy to show that communication has been much more rapid than is often supposed over the whole globe for perhaps a million years. And before one restricts his attention to the unknown numbers of the highly educated he should consider that their numbers are probably proportional to the total, and that the unseen labors of men in the most remote reaches of the globe have provided stimulus, means, and incentive to many if not most of the discoveries made in the main spheres of activity. Finally, the list contains only the first magnitude ideas, but innumerable lesser ones which may have had their origin all around the world went into their conception, and even many of the major ideas themselves have a non-Occidental origin. A more controversial observation from the accumulated idea curve (Fig. 2) is that since 1920 there has been a marked and increasing deficiency of ideas. As of 1980 a full third as many ideas are lacking as are in the entire list! There have often been suggestions that our model of Nature was approaching completeness. But retrospective analysis of the last wave of this feeling, post-Maxwell, shows that at that time a deficiency of 10% at most was in evidence. Earlier, post Aristotle, there was a period when the deficiency reached 20%. The data here seem to show that saturation might indeed now be at hand. And if the limits on particle physics (i.e., on the number of possible neutrinos) which recent analysis (Turner & Schramm, 1979) suggests are validated, the toes of the skeleton of Physics may at last be in sight. On the other hand it is entirely possible that the list is defective in the recent epoch owing to inadequate perspective, although it would seem difficult to overlook about 35 ideas of the stature of the discovery of the neutron, let us say. But with that possibility in mind another revision was undertaken to include all elements of the nuclear and particle problems that seemed to parallel the growth of electromagnetic theory (marked # in the Appendix). Still it was not possible to exorcise the saturation demon. Perhaps whole constellations of solid state and low temperature discoveries of great moment have been omitted, but many other scientists seem to share in the author's belief that these are interesting and extremely useful but not salient matters of Physics. Perhaps on the other hand the word fundamental has at last achieved a full meaning with the emergence of quarks and leptons. Nevertheless, it would be wrong to lay any great stress upon this observation,

nor can it be further tested without danger of special pleading. The modicum of objectivity which went into the development of the original list given as Table 1 is now dispersed, and we must await a fresh attack on the problem.

Summary

In summary, the central body of the idea list, running from −5000 to around 1920 deals with a period in which it might be argued that we have a fairly clear idea of which ideas were significant. In this range we find good support for the theorem proposed: that the rate of growth in the number of ideas is proportional jointly to the number of ideas in hand and the number of minds to consider them. Although fluctuations in the growth rate have occurred from time to time, there is no evidence for the theory of Rainoff that it varies periodically.

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APPENDIX A
Roster of ideas, concepts, and discoveries

Event	Date	Population	d*t*/d*I*
1 Tools	$-1.5\ 10^{+06}$	$3.2\ 10^{+05}$	$5.6\ 10^{+06}$
2 Abstract language	−300000	$1.6\ 10^{+06}$	$4.7\ 10^{+05}$
3 Counting*	−100000	$4.6\ 10^{+06}$	84000
4 Control of fire	−30000	$1.5\ 10^{+07}$	25000
5 Measurement of space	−9000	$4.1\ 10^{+07}$	9800
6 Writing*	−6000	$5.5\ 10^{+07}$	4500
7 Measurement of time	−5000	$6.3\ 10^{+07}$	1900
8 Metallurgy	−3000	$8.5\ 10^{+07}$	1200
9 Wheel	−2000	$1.0\ 10^{+08}$	750
10 Electricity	−1100	$1.3\ 10^{+08}$	500
11 Magnetism	−800	$1.4\ 10^{+08}$	370
12 Zero	−500	$1.5\ 10^{+08}$	270
13 Concept of "nature"	−400	$1.6\ 10^{+08}$	280
14 Mechanics	−200	$1.7\ 10^{+08}$	320
15 Sound as waves	−100	$1.8\ 10^{+08}$	350
16 Chemical change	400	$2.2\ 10^{+08}$	280
17 Refraction of light	1000	$3.0\ 10^{+08}$	170
18 Observability of nature	1200	$3.4\ 10^{+08}$	79

APPENDIX A (*continued*)

Event	Date	Population	d*t*/d*I*
19 Continuity of space-time	1300	$3.7\ 10^{+08}$	58
20 Validity test for theory	1300	$3.7\ 10^{+08}$	41
21 Heliocentrism	1543	$4.5\ 10^{+08}$	28
22 Dichotomy of charge	1550	$4.6\ 10^{+08}$	18
23 Principle of equivalence*	1589	$4.8\ 10^{+08}$	11
24 Dipolarity of magnetism	1600	$4.8\ 10^{+08}$	6.5
25 Optical instruments	1608	$4.7\ 10^{+08}$	5.5
26 Dispersion	1611	$4.8\ 10^{+08}$	8.0
27 Planetary laws	1618	$4.8\ 10^{+08}$	11
28 Acceleration	1638	$5.1\ 10^{+08}$	12
29 Gravitation	1666	$5.5\ 10^{+08}$	12
30 The calculus	1667	$5.5\ 10^{+08}$	12
31 Finite light speed*	1676	$5.6\ 10^{+08}$	12
32 Concept of mass	1687	$5.8\ 10^{+08}$	13
33 Action–reaction	1687	$5.8\ 10^{+08}$	16
34 Insulators	1730	$6.7\ 10^{+08}$	17
35 Duality of electric charge	1733	$6.8\ 10^{+08}$	17
36 Fluid mechanics	1755	$7.3\ 10^{+08}$	15
37 Electrochemistry	1776	$7.9\ 10^{+08}$	13
38 Inverse square laws	1785	$8.2\ 10^{+08}$	9.5
39 Generalized coordinates	1788	$8.3\ 10^{+08}$	7.1
40 Current electricity	1800	$8.8\ 10^{+08}$	5.5
41 Photochemistry	1801	$8.8\ 10^{+08}$	4.8
42 Diffraction	1803	$8.9\ 10^{+08}$	4.7
43 Atomic chemistry*	1803	$8.9\ 10^{+08}$	4.5
44 Spectroscopy	1820	$9.6\ 10^{+08}$	4.0
45 Electromagnetism	1820	$9.6\ 10^{+08}$	3.6
46 Fourier series*	1822	$9.7\ 10^{+08}$	3.8
47 Electrodynamics	1823	$9.8\ 10^{+08}$	5.1
48 Ideal engine	1824	$9.8\ 10^{+08}$	6.6
49 Electromagnetic induction	1831	$1.0\ 10^{+09}$	8.5
50 Energy conservation	1848	$1.1\ 10^{+09}$	9.4
51 Kinetic theory*	1857	$1.2\ 10^{+09}$	9.8
52 Displacement currents	1864	$1.2\ 10^{+09}$	9.0
53 Statistical mechanics	1871	$1.3\ 10^{+09}$	7.5
54 Balmer's series	1885	$1.4\ 10^{+09}$	5.6
55 Null ether drift	1887	$1.4\ 10^{+09}$	4.6
56 Radio waves	1887	$1.4\ 10^{+09}$	3.5
57 Photoelectricity	1888	$1.4\ 10^{+09}$	2.6
58 X-rays	1894	$1.5\ 10^{+09}$	2.2
59 Zeeman effect	1896	$1.5\ 10^{+09}$	2.3
60 Radioactivity	1896	$1.5\ 10^{+09}$	2.2
61 Electron	1897	$1.5\ 10^{+09}$	2.3
62 Photon	1901	$1.6\ 10^{+09}$	2.4
63 Mass-energy equivalence	1905	$1.6\ 10^{+09}$	2.5
64 Stimulated emission*	1905	$1.6\ 10^{+09}$	2.4
65 Geiger counter#	1908	$1.7\ 10^{+09}$	2.2
66 Nuclear atom	1911	$1.7\ 10^{+09}$	2.0
67 Superconductivity*	1911	$1.7\ 10^{+09}$	2.4
68 Cloud chamber#	1912	$1.7\ 10^{+09}$	2.8
69 General relativity#	1915	$1.8\ 10^{+09}$	2.8
70 Parity#	1922	$1.9\ 10^{+09}$	2.5
71 DeBroglie waves	1924	$2.0\ 10^{+09}$	2.2
72 Electron spin	1925	$2.0\ 10^{+09}$	1.5

APPENDIX A (*continued*)

Event	Date	Population	d*t*/d*I*
73 Exclusion principle	1925	$2.0\ 10^{+09}$	1.1
74 Artificial radioactivity	1925	$2.0\ 10^{+09}$	1.0
75 Beta-ray dilemma	1927	$2.0\ 10^{+09}$	1.1
76 Electron diffraction#	1927	$2.0\ 10^{+09}$	1.3
77 Cyclotron#	1930	$2.1\ 10^{+09}$	1.5
78 Superfluidity*	1932	$2.1\ 10^{+09}$	1.8
79 Neutron	1932	$2.1\ 10^{+09}$	2.2
80 Positron	1932	$2.1\ 10^{+09}$	2.7
81 Mu meson	1936	$2.2\ 10^{+09}$	3.1
82 Pi meson	1947	$2.5\ 10^{+09}$	2.8
83 Controlled semiconduction*	1948	$2.6\ 10^{+09}$	2.3
84 Quantum electrodynamics*	1949	$2.6\ 10^{+09}$	1.9
85 Computers*	1949	$2.6\ 10^{+09}$	1.7
86 Strong focussing#	1952	$2.7\ 10^{+09}$	1.2
87 Neutrino detection*	1953	$2.7\ 10^{+09}$	1.3
88 Bubble chamber#	1953	$2.7\ 10^{+09}$	1.5
89 Antiproton	1955	$2.8\ 10^{+09}$	1.8
90 Parity violation	1956	$2.9\ 10^{+09}$	1.7
91 BCS theory	1957	$2.9\ 10^{+09}$	1.9
92 Strangeness	1958	$3.0\ 10^{+09}$	2.7
93 Big bang#	1963	$3.2\ 10^{+09}$	2.8
94 Symmetry theory	1964	$3.2\ 10^{+09}$	2.6
95 Laser*	1968	$3.5\ 10^{+09}$	2.4
96 Josephson effect*	1971	$3.7\ 10^{+09}$	2.1
97 J/psi particle	1974	$3.9\ 10^{+09}$	1.3
98 Quark structure of nucleons*	1975	$4.0\ 10^{+09}$	.93
99 Color force#	1976	$4.1\ 10^{+09}$	.65
100 Color multiplicity#	1976	$4.1\ 10^{+09}$	.76
101 Quantum chromodynamics*	1976	$4.1\ 10^{+09}$	.91
102 Flavor#	1977	$4.1\ 10^{+09}$	1.0
103 Grand unified theory*	1979	$4.3\ 10^{+09}$	1.2
104 Gluon*	1979	$4.3\ 10^{+09}$	1.6
105 W particles#	1982	$4.6\ 10^{+09}$	1.7

Note. The symbol * denotes a concept that was added on a second searching scrutiny to achieve greater completeness. The symbol # prior to about 1963 refers to an idea added in a conscious effort to overcome the apparent deficiency in the number of ideas since 1920. After 1963, it refers to an attempt to add ideas on nuclear and particle physics which parallel the growth of electromagnetic theory.

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Engineering Anomalies Research

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Abstract—Anomalous consciousness-related phenomena of possible relevance to basic physical science and modern engineering practice are addressed experimentally and theoretically in an effort to identify those devices, systems, and processes most likely to display operator-related anomalies in their performance, and to illuminate the characteristics of such aberrations. Three interrelated sectors of effort are pursued: the design, implementation, operation, and interpretation of experiments in low-level psychokinesis; the development of analytical methodologies for quantitative assessment of precognitive remote perception data; and the development of theoretical models useful for correlation of the experimental data, design of better experiments, and explication of the phenomena on fundamental grounds.

The primary effect observed in the psychokinesis experiments is a marginal but replicable shift of the mean of output count distributions with respect to empirical baselines or theoretical expectations, with no discernible alterations in any higher moments. Over large data bases, these mean shifts can compound with considerable statistical regularity to high levels of significance, depending on the particular operator, the direction of effort, and other prevailing experimental conditions. In many cases, individual operator "signatures" of achievement are found to transfer across various experimental devices, including some driven by deterministic pseudo-random sources.

Quantitative analysis of a large data base of remote perception experiments reveals similar departures from chance expectation of the degree of target information acquired by anomalous means. Digital scoring techniques based on a spectrum of 30 binary descriptors, applied to all targets and perceptions in the experimental pool, consistently indicate acquisition of substantial topical and impressionistic information about remote geographical locations inaccessible by known sensory channels. The degree of such anomalous information acquisition appears independent of the spatial separation of the percipient from the target, up to global distances, and also independent of the temporal separation of the perception effort from the time of target specification by the agent, up to periods of precognition or retrocognition of several days.

In an attempt to illuminate these empirical results, a theoretical model has been proposed that invokes quantum mechanical metaphors to describe the interaction of consciousness with its environment. By representing consciousness by quantum mechanical wave functions and its physical environment by appropriate potential energy profiles, Schrödinger wave mechanics

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may be used to define eigenfunctions and eigenvalues indicative of psychological and physical experience, both normal and anomalous, in a form applicable to the experimental designs.

The experimental results in hand, along with the generic predictions of the theoretical model, suggest numerous short and longer term practical applications of the phenomena, and raise basic issues about the role of consciousness in the establishment of reality.

Introduction

Scholarly research into a broad range of anomalous consciousness-related phenomena over the past century has produced an array of provocative results, but none that can be regarded as fully convincing in the traditional scientific sense. Nor has this research yielded sufficient empirical correlations to support any existing category of theoretical model for description and comprehension of such effects, let alone to refine such models to functional utility. Nevertheless, the potential implications for many fields of human endeavor are sufficiently profound and pervasive that efforts for demonstration and resolution continue in several disciplines. Among these, the various fields of engineering science are neither immune from the implications nor impotent to contribute to the search. Throughout its three major domains of modern activity—the processing of energy, of materials, and of information—engineering engages a multitude of physical devices, systems, and situations that may be potentially vulnerable to such anomalous interactions. In particular, those involving sensitive man/machine interfaces, low-level signal processing units of the microprocessor genre, elaborate data-storage systems, devices utilizing random or pseudo-random noise sources, and very large-scale integrated circuits would seem to merit attention.

The research reported herein consists of three components, conceptually distinct, but in practice interrelated. The first is an ensemble of experiments in low-level psychokinesis—the interaction of human consciousness with some physical device, system, or process resulting in statistical behavior different from that expected on the basis of known science. The second addresses the process of precognitive remote perception—the acquisition of information about geographical targets remote in distance and time and inaccessible by any known sensory means. The primary interest here is the development of analytical methodologies for quantitative determination of the degree of information obtained by such processes. The third segment is an effort to develop a theoretical model to support the experimental program and provide some insight into the basic nature of the phenomena.

Particular experiments have been selected for their immediate and longer term relevance to the practice of modern engineering science, and for their amenability to controlled and systematic laboratory study. Physical and technical parameters are the primary concern throughout; systematic investigation of psychological or physiological correlates is secondary to the accumulation

of very large data bases by a relatively small number of participants. All operators are anonymous volunteers, none of whom claims extraordinary abilities, and no screening, training, or induction techniques are employed.

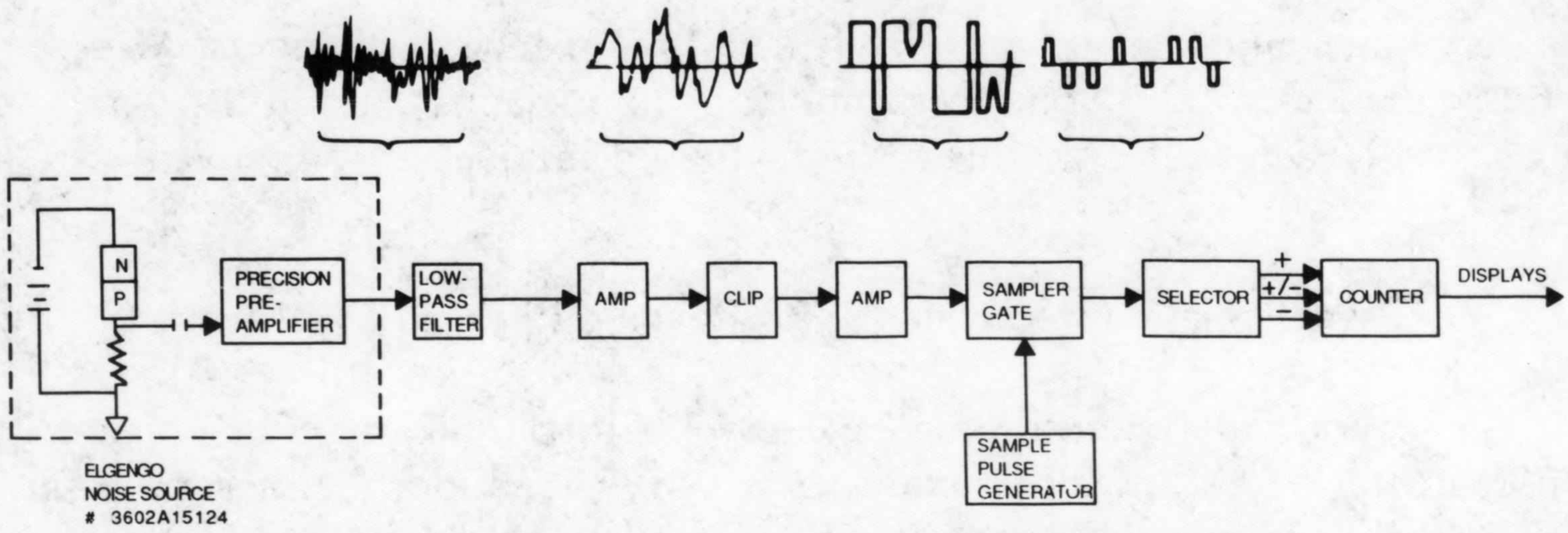
Psychokinetic Interactions With Random Physical Systems

The several experiments in low-level psychokinesis (PK), although diverse in character and scale, all embody some type of random physical process whose distribution is established empirically and, when possible, theoretically. Various human operators then attempt to distort those distributions in prestated directions. To guide them in that task, each experiment provides some form of feedback, usually a visual display, that tracks the degree of shift from the baseline distribution.

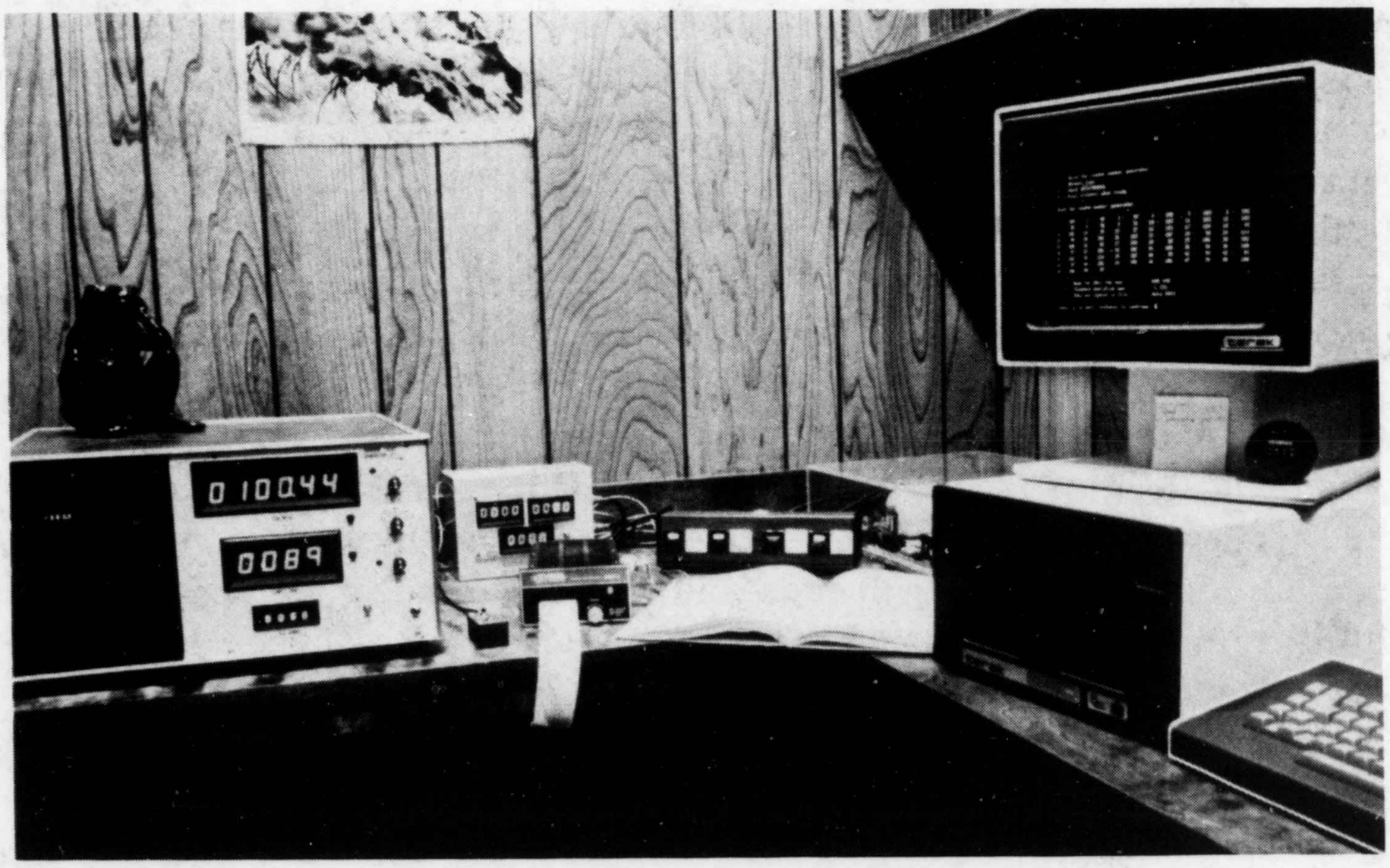
One such experiment belongs to a genre of random event generators (REG) widely used in contemporary studies of this class of phenomena (Krippner, 1977; Radin, May, & Thomson, 1985; Schmidt, 1970; Stanford, 1977). The particular device employed in this program is based upon a commercial microelectronic noise source whose output is transcribed by appropriate circuitry into a random train of positive and negative pulses, suitable for sampling and counting [Fig. 1(a)]. For most formal experiments, the device is set to generate "trials" of 200 pulses each at a rate of 1000 per second, and to count and display the number of those pulses which conform to the regular alternation: +, −, +, −, +, −, etc. Various display and recording units show the operator the results of the counting and insert them on-line into a digital database and computational system.

Figure 1(b) shows the experimental arrangement as seen by the operator, who sits a few feet from the device and its supporting equipment, for example, a computer terminal, a strip printer, and various fail-safe counters that guarantee the integrity of the data. The operator attempts to influence the process to produce a higher number of counts (PK^+) or a lower number of counts (PK^-), or to generate a baseline (BL), in accordance with pre-recorded intentions. In the protocol followed for the largest subset of our data base, data are generated in "runs" of 50 trials, accumulated in "sessions" comprising a minimum of five runs. While session lengths are left to the preference of the operator, a complete experimental "series" requires a full 7500 trials, or 50 runs in each of the three directions of intention. (A few early series consisted of 5000 trials, or 100 runs per intention.) To preclude any artifactual bias, the protocol requires the operator to intersperse sequences of each of the three intentions, PK^+, PK^-, and BL, with all other experimental conditions held constant.

An example of the type of data obtained in this experiment is shown in Fig. 2(a) as a distribution of scores for some 5000 baseline trials (i.e., one million pulses, or bits) taken by one operator, superimposed on the theoretical Gaussian approximation to the appropriate binomial statistics. With reference



(a) REG FUNCTIONAL BLOCK DIAGRAM



(b) REG EXPERIMENTAL ARRANGEMENT

Fig. 1. Random Event Generator.

to the same theoretical distribution, Fig. 2(b) displays the results of the same operator's efforts to shift this distribution toward higher or lower numbers of counts over the same number of PK^+ and PK^- trials, and Fig. 2(c) shows the best Gaussian fits to these data. The effects found in this experiment are usually confined to such marginal shifts of the mean of the distribution, with no perceptible changes in the standard deviation, higher moments, or other characteristics of the distribution.

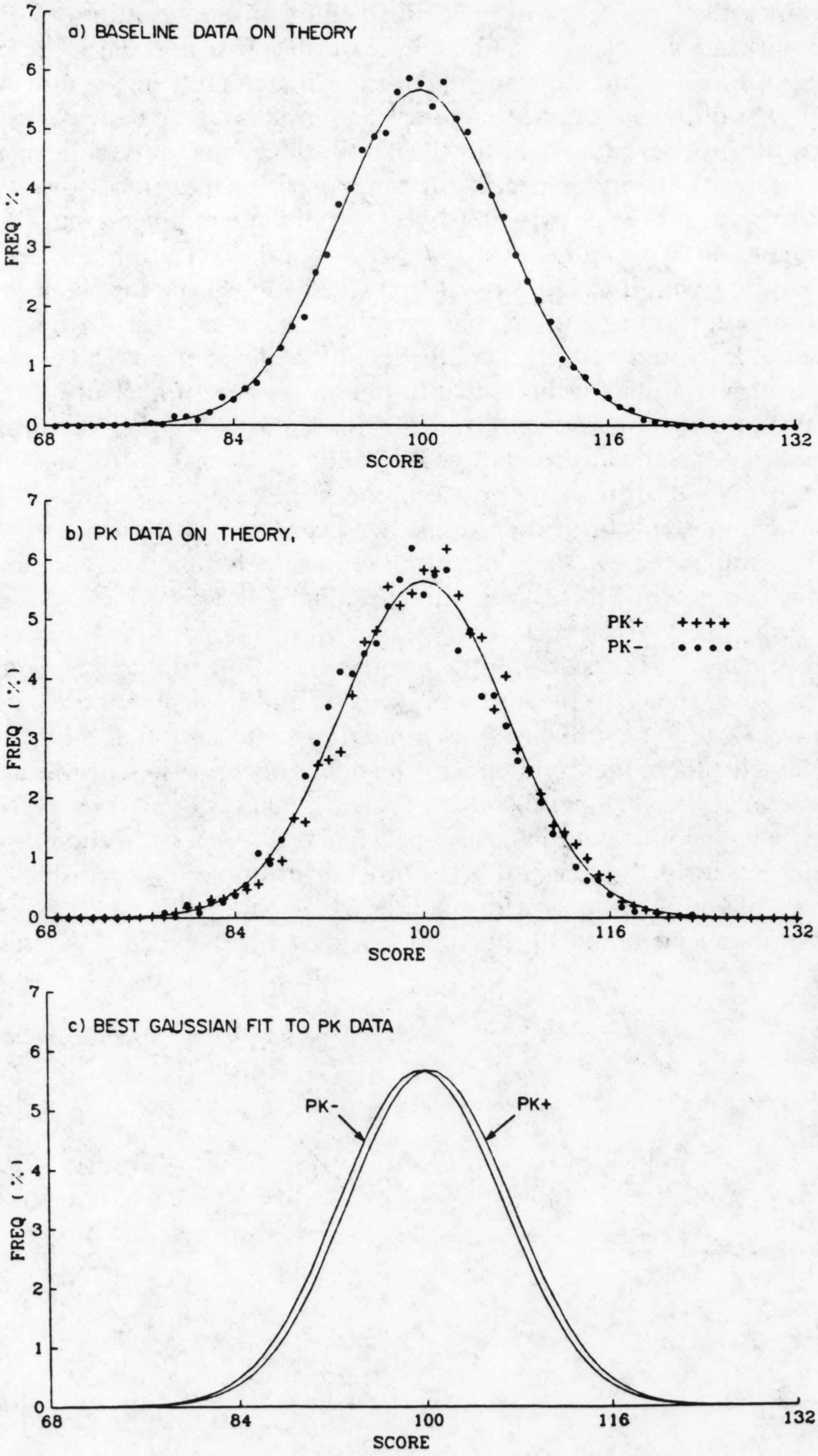


Fig. 2. REG frequency-of-count distributions.

To track the consistency of these small shifts of the mean and to display their statistical significance as a function of the data base size, the accumulated deviation of the counts from the chance mean are graphed as a function of the number of trials processed. Figure 3 employs the same data as Fig. 2, in such cumulative deviation plots for each of the three intentions of the operator relative to the theoretical mean. All three experimental traces display the stochastic variations to be expected in this sort of random process, but whereas the baseline curve meanders close to the theoretical expectation, the PK^+ and PK^- traces display almost linear systematic deviations from the chance mean that compound to progressively larger values as the number of trials accumulates. The dashed parabolas are the loci of the five percent chance expectation of reaching that accumulated deviation at that number of trials, and the scale at the right indicates the range of terminal chance probabilities. The terminal values of the means of these PK^+ and PK^- data, 100.264 and 99.509 respectively, differ from chance expectation by several standard error units, with the composite achievement unlikely by chance to the order of 10^{-6}.

Such cumulative deviation graphs are found to be quite operator specific and hence are referred to as "signatures." Figure 4 shows such signatures for a few of the many other operators working on this same experiment. Some operators achieve PK results in only one direction, some in neither, some in both, and some show inverted results. The PK^+ and PK^- achievement patterns for a given operator are typically asymmetrical, and are often found to be dependent on the conditions under which the operator is performing the experiment, such as the pulse counting rate, whether each trial in the run is initiated manually or automatically, or whether the operator chooses or is randomly assigned the direction of effort. For example, Fig. 5 displays the sensitivity of one operator's performance to the "volitional" and "instructed" modes of data generation. In the volitional mode, the operator chooses the

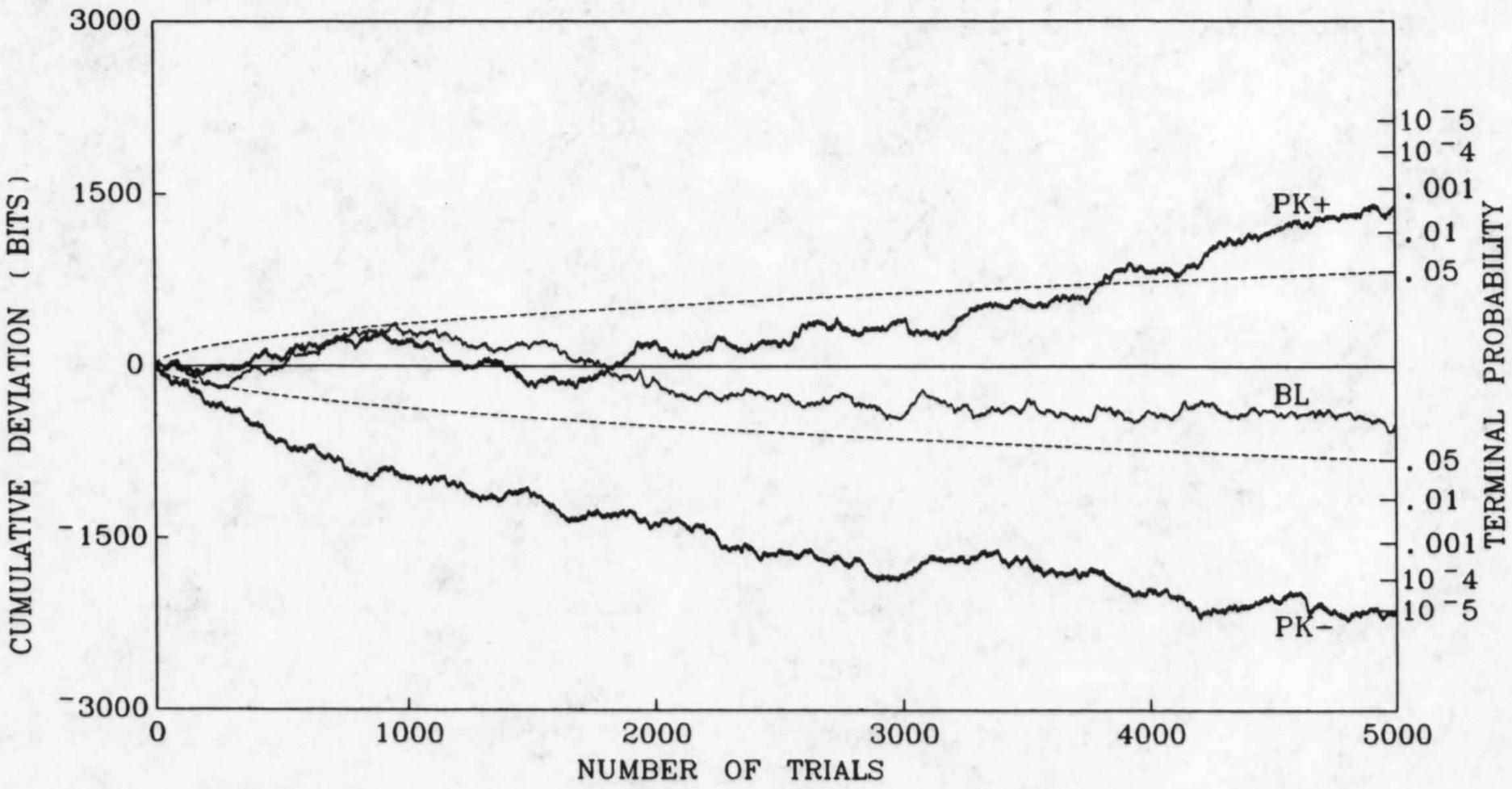


Fig. 3. REG cumulative deviations from theoretical mean.

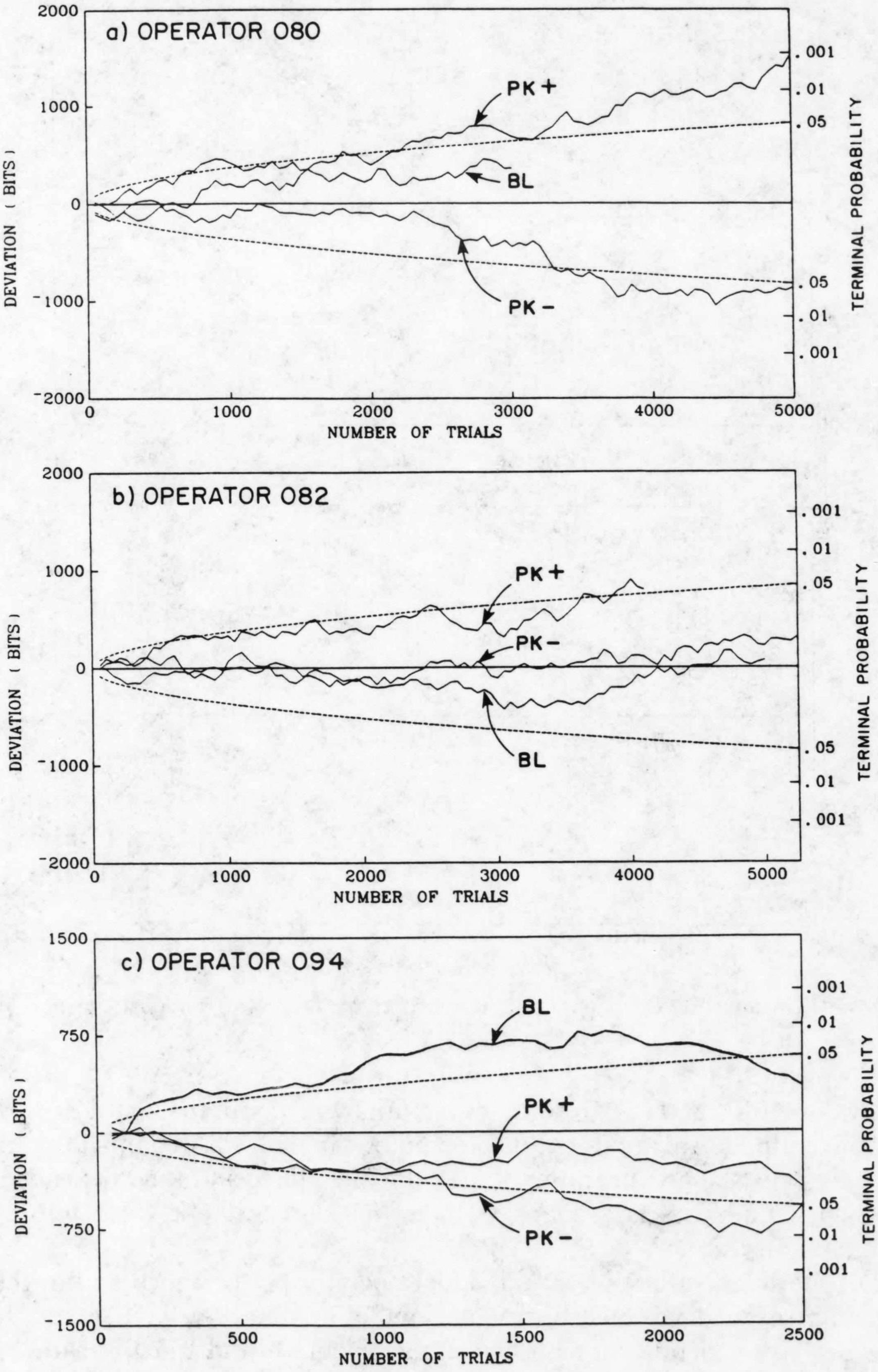


Fig. 4. REG cumulative deviations from theoretical mean: Various operators.

direction of effort and completes five runs (250 trials), or some multiple thereof, before changing the intention. In the instructed mode, a random number generated before each run assigns the direction of effort. Note that in the case

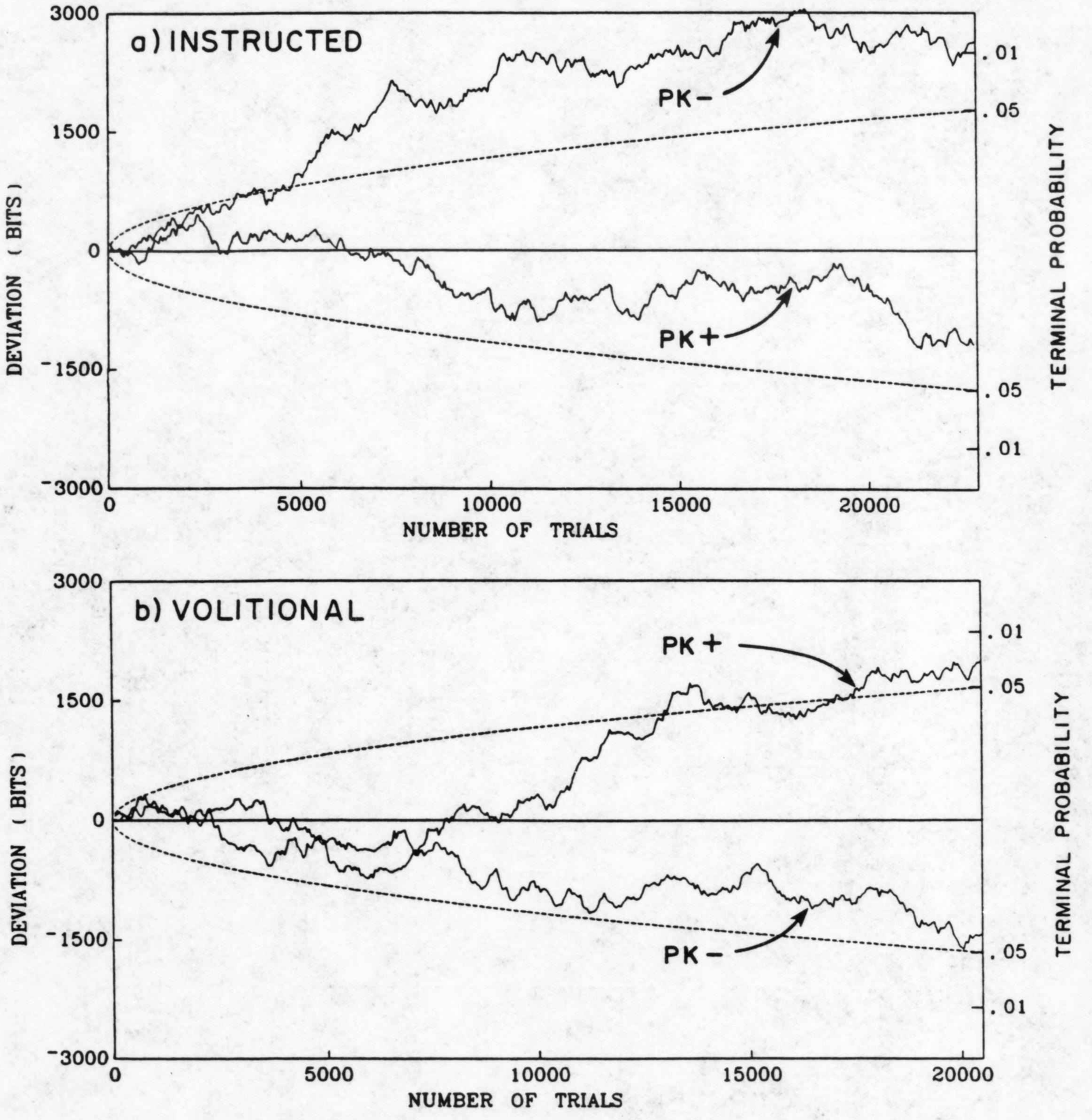


Fig. 5. REG cumulative deviations from theoretical mean: Instructed/volitional (operator 55).

shown, the PK^+ and PK^- results are essentially reversed, that is, those in the instructed mode are opposite to the operator's intention. A complete graphical and statistical compendium of operator signatures and their dependence on such parameters is available in a technical report (Nelson, Dunne & Jahn, 1984).

Despite these variations in individual operator performance and in their secondary dependence on experimental conditions, the overall REG data base also displays a significant statistical trend. Figure 6 and Table 1 show the combined results of the entire formal data base, comprising 87 completed series, and totalling over 250,000 trials per intention (>150 million bits). These data were generated by 33 different operators on two different machines over a period of approximately seven years. Again, the grand baseline mean remains close to the theoretical value, and the PK^+ and PK^- data, despite

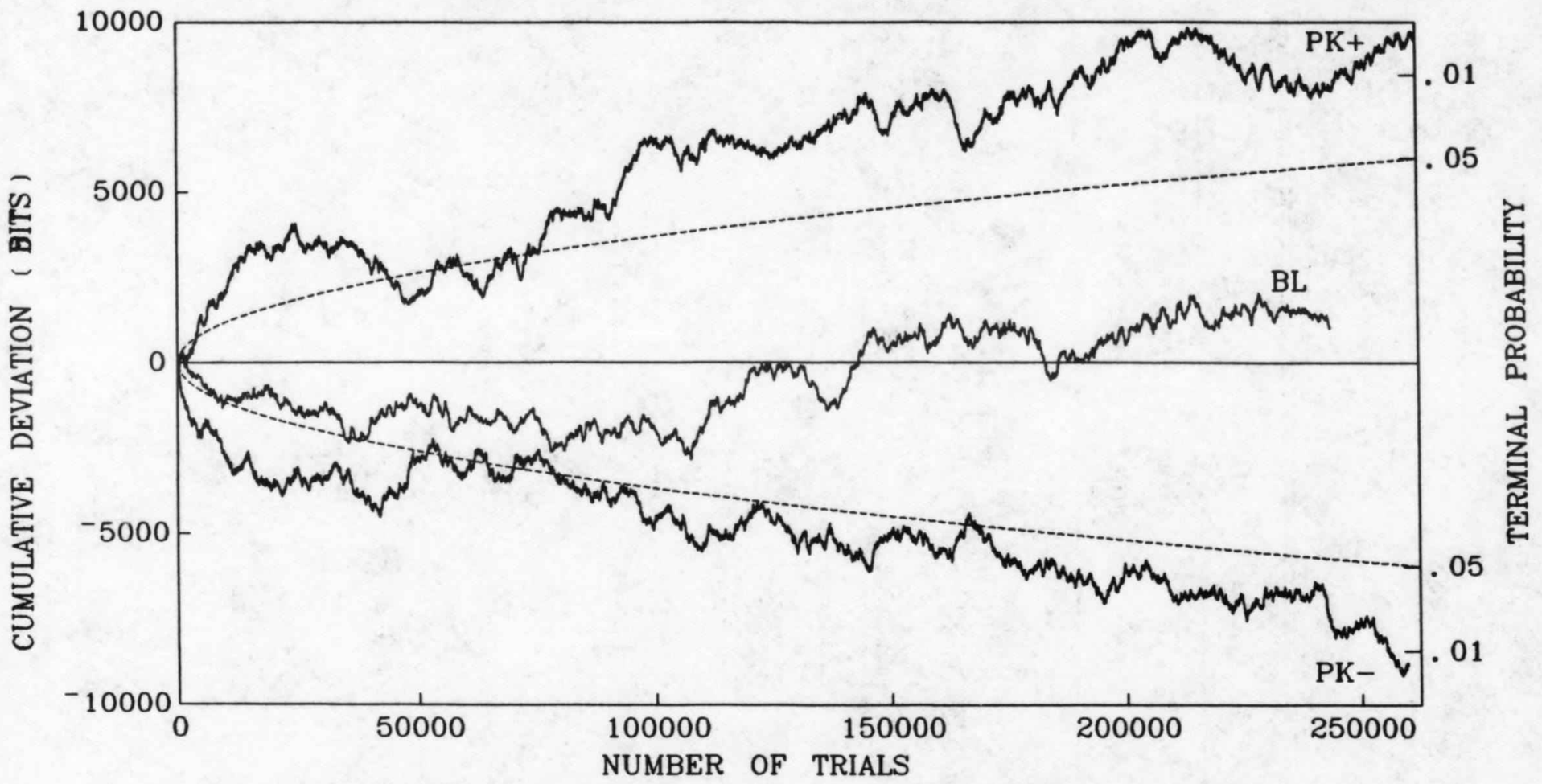


Fig. 6. REG cumulative deviations from theoretical mean: All data, 33 operators.

occasional reversals associated with particular operators and conditions, trend toward increasingly significant deviations in their stated directions. The probability of the indicated overall result occurring by chance is less than 2×10^{-4}.

Other instructive features of the overall REG data base appear in the distribution of terminal scores of the complete series. For example, Fig. 7 shows histograms and analytical fits of all 87 series *z*-scores for PK^+, PK^-, and baseline efforts. While the mean values of these three distributions are consistent with the terminal values of the cumulative deviation traces of Fig. 6, it is notable that the distributions of the PK^+ and PK^- series scores both have larger than expected variances, at significance levels of 0.06 (PK^+) and 0.01 (PK^-). Conversely, the distribution of baseline series scores is substantially compacted around the theoretical mean and totally devoid of any scores outside of the one-tailed significance criterion, $z > \pm 1.645$. The corresponding reduction in the variance of the baseline score distribution is significant at $p = 0.01$. Recalling that baseline data are generated under conditions identical to the PK series, save for the absence of a stated directional intention on the part of the operator, one is led to hypothesize that a conscious or unconscious motivation to achieve a "good" baseline may actually produce a third PK condition that entails an anomalous constriction of the distribution of scores. This issue is discussed in greater detail in a technical report (Jahn, Nelson, & Dunne, 1985).

Experiments such as these inevitably raise the question of the focus of the interaction between the consciousness of the operator and the machine. In particular, it is reasonable to ask whether the physical behavior of the noise source itself is affected during the PK efforts, and if so, in what way. One obvious strategy for addressing this question is to replace the source unit by other elements and compare results. Several similar microelectronic noise

TABLE 1
REG data summary by operator

	PK+							PK−					
Opr.	# Series	# Trials	Mean	z-Score	Prob.*	# Series $p < .05$*	# Series $p < .5$	# Trials	Mean	z-Score	Prob.*	# Series $p < .05$*	# Series $p < .5$
10	15	55,100	100.082	2.729	.003	3	12	55,050	99.896	−3.459	3×10^{-4}	4 (1)	13
14	3	8,000	100.070	0.885	.188	1	2	7,800	99.872	−1.603	.054	1	3
16	3	7,500	100.070	0.856	.196	—	2	7,500	99.763	−2.903	.002	1	3
19	1	2,950	100.030	0.232	.408	—	1	2,800	100.042	0.313	(.377)	—	—
20	3	7,550	100.087	1.064	.144	—	2	7,450	99.979	−0.262	.397	—	2
21	1	2,700	100.044	0.321	.374	—	1	2,300	100.156	1.056	(.146)	—	—
29	1	2,500	99.912	−0.625	(.266)	—	—	2,500	100.046	0.322	(.374)	—	—
30	2	5,000	100.026	0.262	.397	—	1	5,000	99.939	−0.606	.272	—	2
33	1	4,000	99.868	−1.178	(.119)	—	—	2,500	99.928	−0.512	.304	—	1
36	2	5,000	99.978	−0.218	(.414)	—	1	5,250	100.068	0.695	(.244)	—	1
41	5	13,450	100.023	0.373	.355	—	3	15,050	99.984	−0.273	.392	— (1)	4
42	1	2,700	100.094	0.691	.245	—	1	2,300	100.031	0.212	(.416)	—	—
44	2	5,300	99.781	−2.255	(.012)	— (1)	—	6,200	99.918	−0.914	.180	—	2
49	2	4,950	99.871	−1.284	(.099)	—	—	5,050	100.072	0.722	(.235)	—	1
53	3	7,550	99.937	−0.778	(.218)	— (1)	1	7,450	99.981	−0.236	.407	—	2
55	13	43,300	100.018	0.544	.293	2 (1)	7	43,400	100.028	0.818	(.207)	— (1)	5
59	2	5,100	100.046	0.461	.322	—	1	3,900	99.923	−0.684	.247	—	2
64	2	5,500	99.940	−0.625	(.266)	—	1	4,500	100.042	0.403	(.344)	—	1
65	1	2,600	100.207	1.489	.068	—	1	2,400	99.956	−0.303	.381	—	1
66	2	7,950	100.003	0.041	.484	—	1	7,050	99.930	−0.830	.203	—	1
68	1	4,650	99.955	−0.429	(.334)	—	—	5,350	100.005	0.048	(.481)	—	—
70	3	7,700	99.963	−0.459	(.323)	—	1	7,300	99.922	−0.940	.174	—	2

Table 1 (*continued*)

	PK+							PK−					
Opr.	# Series	# Trials	Mean	*z*-Score	Prob.*	# Series $p < .05$*	# Series $p < .5$	# Trials	Mean	*z*-Score	Prob.*	# Series $p < .05$*	# Series $p < .5$
80	2	7,500	100.185	2.272	.012	1	1	7,500	99.871	−1.581	.057	1	2
82	2	4,100	100.193	1.745	.041	1	2	5,250	100.060	0.615	(.269)	—	—
84	1	2,500	100.102	0.724	.235	—	1	2,500	99.765	−1.663	.048	1	1
85	1	5,100	100.088	0.891	.186	—	1	5,300	100.180	1.851	(.032)	— (1)	1
88	1	2,750	100.169	1.254	.105	—	1	3,000	100.084	0.648	(.258)	—	—
90	1	2,500	100.088	0.619	.268	—	1	2,500	99.968	−0.229	.409	—	1
92	2	7,400	99.941	−0.722	(.235)	1 (1)	1	5,400	100.087	0.903	(.183)	— (1)	1
93	2	5,000	100.144	1.440	.075	—	2	5,000	100.019	0.194	(.423)	—	1
94	4	10,000	100.056	0.796	.213	1	2	10,000	99.939	−0.863	.194	—	3
96	1	2,250	100.073	0.492	.311	—	1	2,750	100.085	0.634	(.263)	—	—
97	1	2,500	100.110	0.781	.218	—	1	2,500	99.935	−0.461	.322	—	1
All	87	262,650	100.037	2.666	.004	10 (4)	53	259,800	99.966	−2.444	.007	8 (5)	56

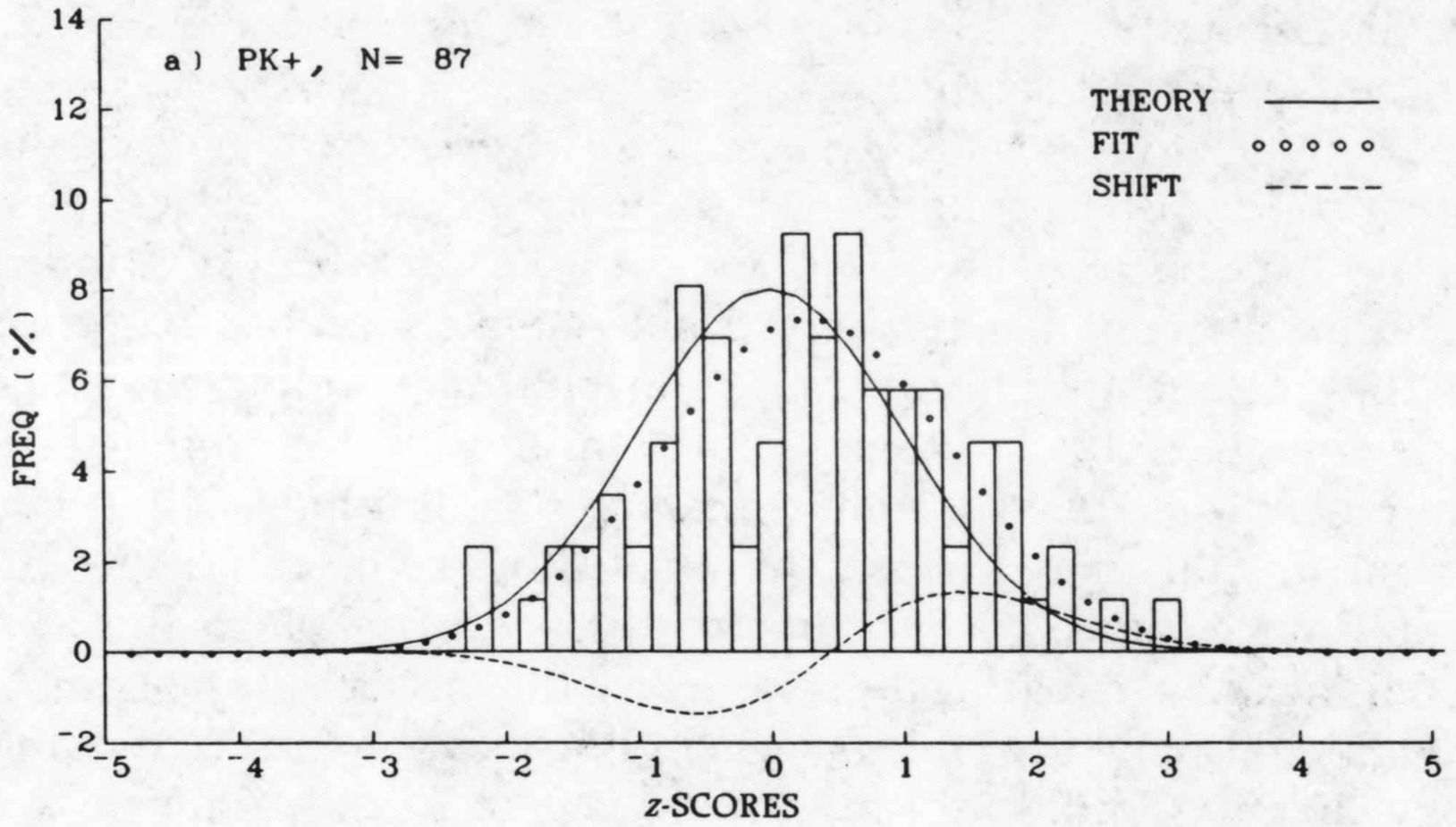
	Baseline							ΔPK				
Opr.	# Series**	# Trials	Mean	*z*-Score	Prob.	# Series $p < .05$	# Series Mean > 100	# Trials	*z*-Score	Prob.*	# Series $p < .05$*	# Series $p < .5$
10	13	59,100	100.011	0.389	.349	—	8	110,150	4.375	6×10^{-6}	4	14
14	3	7,250	99.936	−0.774	.219	—	1	15,800	1.756	.040	—	3
16	3	7,500	100.024	0.292	.385	—	1	15,000	2.658	.004	1	3
19	1	2,500	100.044	0.311	.378	—	1	5,750	−0.052	(.479)	—	—
20	3	7,500	99.956	−0.537	.296	—	0	15,000	0.940	.174	—	3
21	1	2,500	100.032	0.229	.409	—	1	5,000	−0.480	(.316)	—	—
29	1	2,500	100.076	0.540	.295	—	1	5,000	−0.670	(.251)	—	—
30	1	2,650	99.978	−0.162	.436	—	0	10,000	0.614	.270	—	1
33	1	2,500	100.053	0.376	.353	—	1	6,500	−0.607	(.272)	—	—
36	2	4,500	99.928	−0.679	.249	—	1	10,250	−0.650	(.258)	—	—

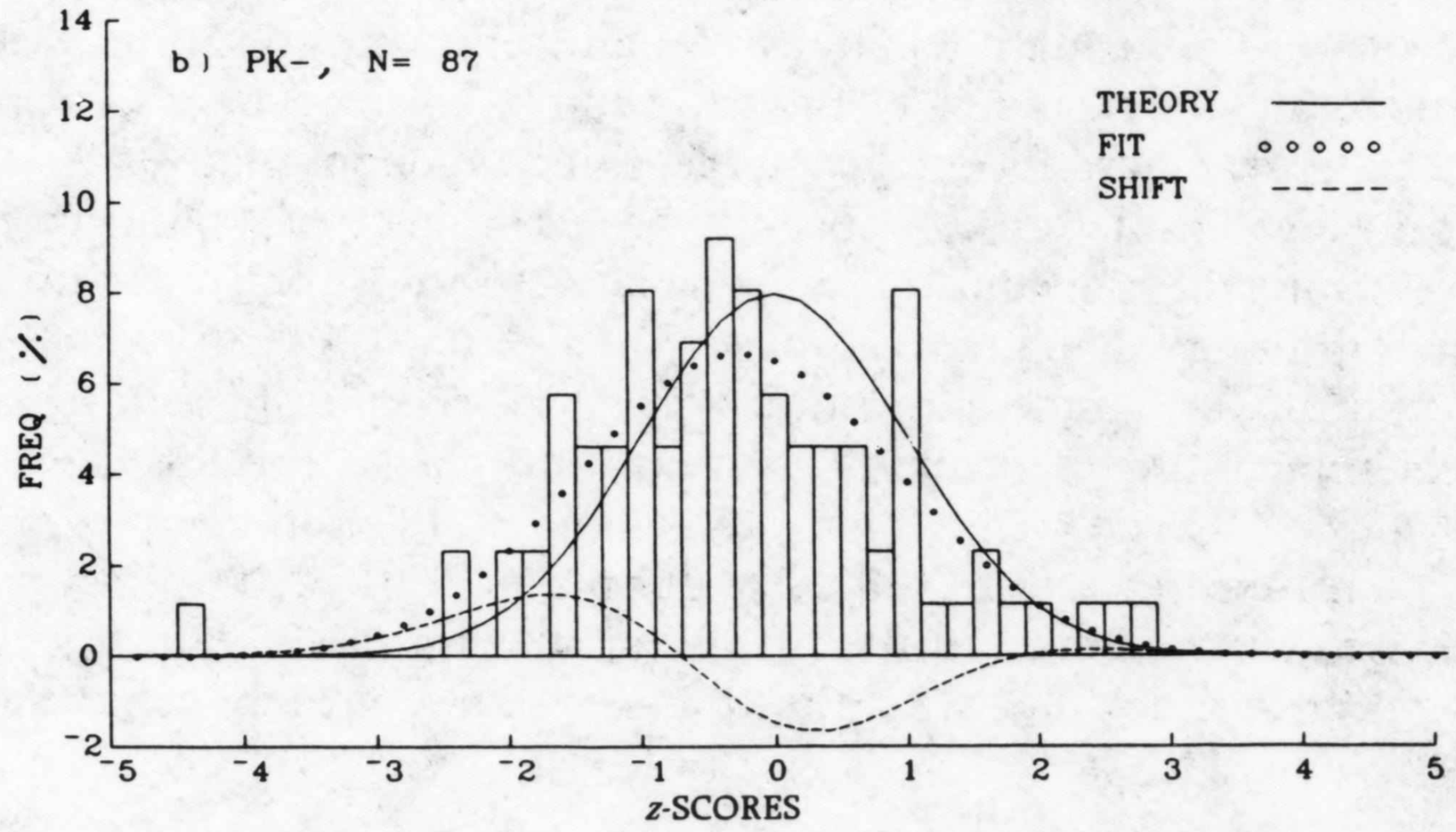
Table 1 (*continued*)

	Baseline							ΔPK				
Opr.	# Series**	# Trials	Mean	*z*-Score	Prob.	# Series $p < .05$	# Series Mean > 100	# Trials	*z*-Score	Prob.*	# Series $p < .05$*	# Series $p < .5$
41	3	9,800	100.012	0.166	.434	—	1	28,500	0.455	.325	—	4
42	1	2,500	99.964	−0.255	.400	—	0	5,000	0.364	.358	—	1
44	2	3,000	99.982	−0.137	.446	—	1	11,500	−0.860	(.195)	—	—
49	2	5,000	100.078	0.784	.217	—	2	10,000	−1.417	(.078)	— (1)	—
53	3	7,500	99.961	−0.477	.317	—	2	15,000	−0.386	(.350)	— (1)	2
55	10	40,000	100.004	0.113	.455	—	6	86,700	−0.194	(.423)	1 (1)	6
59	1	1,100	100.119	0.559	.288	—	1	9,000	0.798	.213	—	2
64	1	5,500	99.908	−0.965	.167	—	0	10,000	−0.734	(.232)	—	—
65	1	2,500	100.172	1.213	.112	—	1	5,000	1.284	.100	—	1
66	2	7,500	99.964	−0.444	.328	—	1	15,000	0.599	.275	—	1
68	1	4,950	100.158	1.572	.058	—	1	10,000	−0.328	(.371)	—	—
70	3	7,500	100.081	0.998	.159	—	2	15,000	0.327	.372	—	2
80	2	5,500	100.160	1.676	.047	—	2	15,000	2.724	.003	1	1
82	1	5,000	100.014	0.144	.443	—	1	9,350	0.695	.244	—	2
84	1	2,500	100.092	0.653	.257	—	1	5,000	1.688	.046	1	1
85	1	5,550	100.107	1.129	.129	—	1	10,400	−0.698	(.243)	—	—
88	1	2,900	99.915	−0.649	.258	—	0	5,750	0.399	.345	—	1
90	1	2,500	99.820	−1.270	.102	—	0	5,000	0.600	.274	—	1
92	2	4,950	99.900	−0.997	.159	—	0	12,800	−1.135	(.128)	1 (1)	1
93	2	5,000	99.827	−1.730	.042	—	0	10,000	0.881	.189	—	2
94	4	9,500	99.961	−0.543	.294	—	2	20,000	1.173	.120	1	3
96	1	2,500	99.914	−0.611	.271	—	0	5,000	−0.140	(.444)	—	—
97	1	2,500	100.104	0.735	.231	—	1	5,000	0.878	.190	—	1
All	76	243,750	100.004	0.282	.389	—	41	522,450	3.614	2×10^{-4}	10 (4)	56

* Numbers in parentheses indicate results opposite to intention.

** In some early series baselines were generated by experimenters, and these are not included in the table.





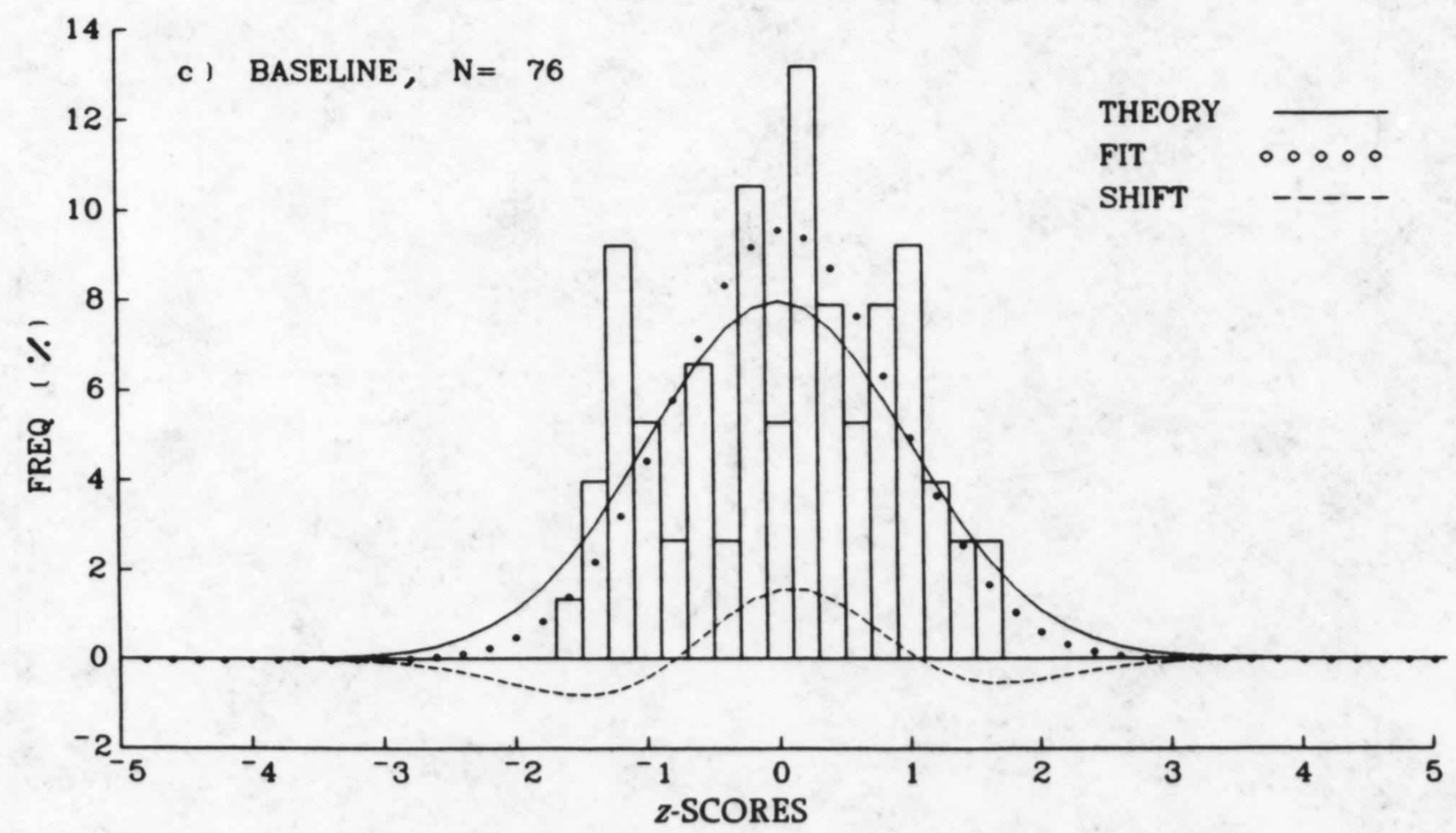


Fig. 7. REG frequency of series *z*-scores: All data, 33 operators.

units have indeed been so employed, with no discernible consequences on the overall pattern of data.

In an attempt to explore this issue more aggressively, a categorically different random source has been developed that may properly be termed "pseudo-random" in character. This device employs a feedback array of 31 microelectronic shift registers that produces a determinate repeating sequence of 2×10^9 bits at a set clock frequency. In the mode most commonly employed, this determinate sequence cycles continuously with a repetition period of about 60 hours, so that the only remaining non-deterministic aspect of the experiment is the time of incursion initiated by the operator. This pseudo-random source can be switched into the standard REG apparatus at an appropriate location, replacing the commercial microelectronic noise diode and its conditioning circuitry but leaving all subsequent sampling, counting, and display circuitry, feedback, and software identical to the standard version. From the perspective of the operator, this system is virtually indistinguishable from that of the standard REG, save for an identifying code printed on the strip tape, and the experimental protocols employed are identical. The results of 29 experimental series employing this pseudo-random source are also statistically significant with a probability against chance of .003, (Fig. 8 and Table 2), and the individual operator signatures show strong qualitative similarities to those achieved on the standard REG.

To pursue further the question of how device-specific are such signatures of achievement, a substantially different experimental device called a "Random Mechanical Cascade" (RMC) has been employed (Nelson, Dunne, & Jahn, 1987). This apparatus, some $6' \times 10'$ in dimension, allows 9,000 $\frac{3}{4}''$ polystyrene spheres to trickle downward through a quincunx array of 330 $\frac{3}{4}''$ diameter nylon pegs, whereby they are scattered into 19 collecting bins across the bot-

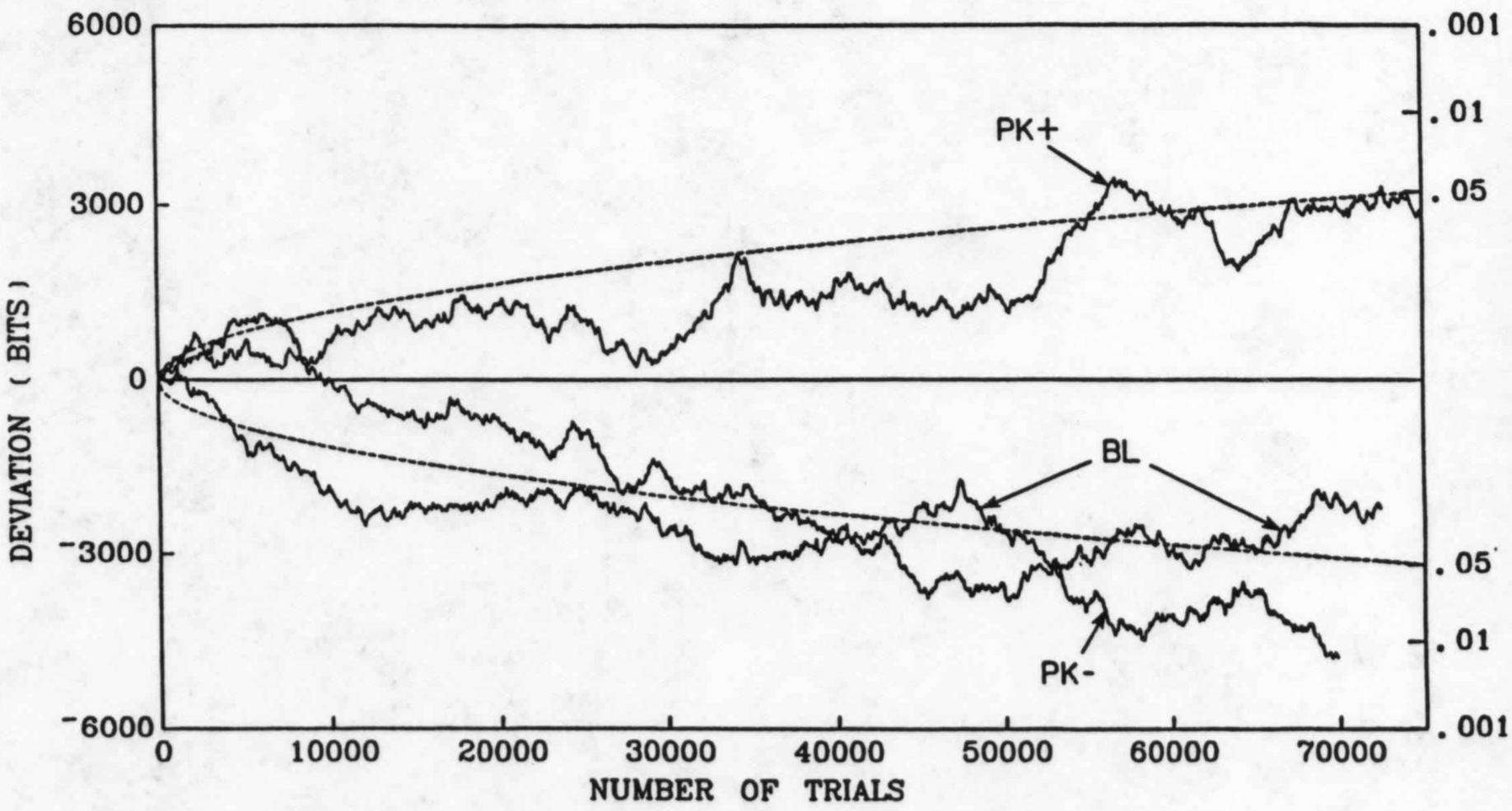


Fig. 8. Pseudo-REG cummulative deviations from theoretical mean: All data, 10 operators.

TABLE 2
Pseudo-REG data summary by operator

	PK+							PK−					
Opr.	# Series	# Trials	Mean	*z*-Score	Prob.	# Series $p < .05$	# Series $p < .5$	# Trials	Mean	*z*-Score	Prob.	# Series $p < .05$	# Series $p < .5$
10	9	23,000	100.059	1.269	.102	2	4	22,000	99.845	−3.249	.001	2	8
14	1	2,300	100.339	2.300	.011	1	1	2,700	100.197	1.448	(.074)	—	—
15	1	2,500	99.788	−1.499	(.067)	—	—	2,500	99.802	−1.397	.081	—	1
16	1	2,500	100.385	2.724	.003	1	1	2,500	100.122	0.860	(.195)	—	—
41	2	5,600	99.928	−0.758	(.224)	—	—	4,400	99.934	−0.616	.269	—	1
55	9	23,400	99.988	−0.251	(.401)	—	3	21,600	99.986	−0.293	.385	1	4
70	2	5,450	99.933	−0.695	(.243)	— (1)	1	4,550	99.921	−0.751	.226	1	1
80	1	2,500	100.082	0.583	.280	—	1	2,500	99.984	−0.116	.454	—	1
88	1	2,500	99.953	−0.334	(.369)	—	—	2,500	99.748	−1.782	.037	1	1
94	2	5,400	100.208	2.161	.015	1	2	4,600	99.978	−0.215	.415	—	1
All	29	75,150	100.037	1.418	.078	5 (1)	13	69,850	99.931	−2.564	.005	5	18

	Baseline							ΔPK				
Opr.	# Series	# Trials	Mean	*z*-Score	Prob.	Series $p < .05$	# Series Mean > 100	# Trials	*z*-Score	Prob.	# Series $p < .05$	# Series $p < .5$
10	9	22,500	99.957	−0.904	.183	—	3	45,000	3.179	.001	3	7
14	1	2,500	100.019	0.133	.447	—	1	5,000	0.496	.310	—	1
15	1	2,500	100.183	1.293	.098	—	1	5,000	−0.072	(.471)	—	—
16	1	2,500	99.875	−0.882	.189	—	0	5,000	1.318	.094	—	1
41	2	5,000	100.138	1.384	.083	—	2	10,000	−0.158	(.437)	—	1
55	9	22,500	99.939	−1.291	.098	—	2	45,000	0.022	.491	1 (1)	5
70	2	5,000	100.139	1.392	.082	—	2	10,000	−0.007	(.497)	1 (1)	1
80	1	2,500	100.089	0.631	.264	—	1	5,000	0.494	.311	—	1
88	1	2,500	99.886	−0.809	.209	—	0	5,000	1.024	.153	—	1
94	2	5,000	99.717	−2.834	.002	1	0	10,000	1.734	.041	1	2
All	29	72,500	99.969	−1.170	.121	1	12	145,000	2.801	.003	6 (2)	20

tom, filling them in close approximation to a Gaussian distribution (Fig. 9). The growing population of each of the bins is tracked photoelectrically and displayed via LED counters at the bottom of that bin, and recorded on-line in an appropriately coded computer file. The experimental protocol calls for the operator, seated on a couch approximately eight feet from the machine, to attempt on volition or instruction to distort the distribution to the right

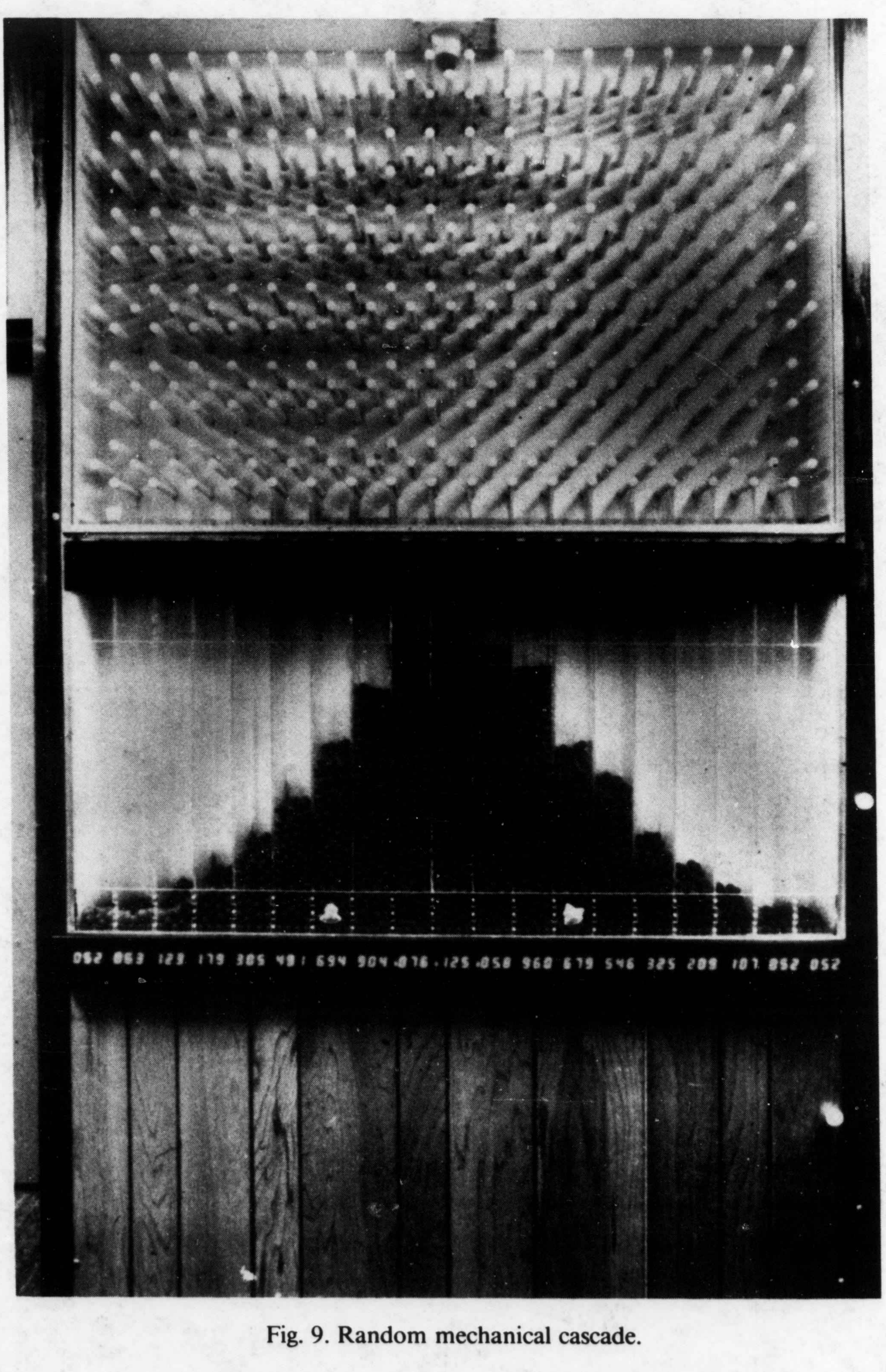


Fig. 9. Random mechanical cascade.

(PK^+) or to the left (PK^-), or to generate baselines. All data are acquired in concomitant sets of three runs, one under each of these intentions, to control against unforeseen artifactual influences. The temperature and humidity within the RMC apparatus are routinely recorded to assess any possible correlations with the experimental data.

Figure 10(a) displays as cumulative deviations the data of all 3072 runs by the 22 operators who have completed at least one formal series of 10 or 20 runs per intention. Once again, the total aberration is statistically significant, to the order of 3×10^{-6}, but in this case only the left-going efforts are independently significant. As can be seen in Table 3, most of this asymmetry is due to the characteristic contributions of two operators who happen to have exceptionally large individual data bases. With these two omitted, the com-

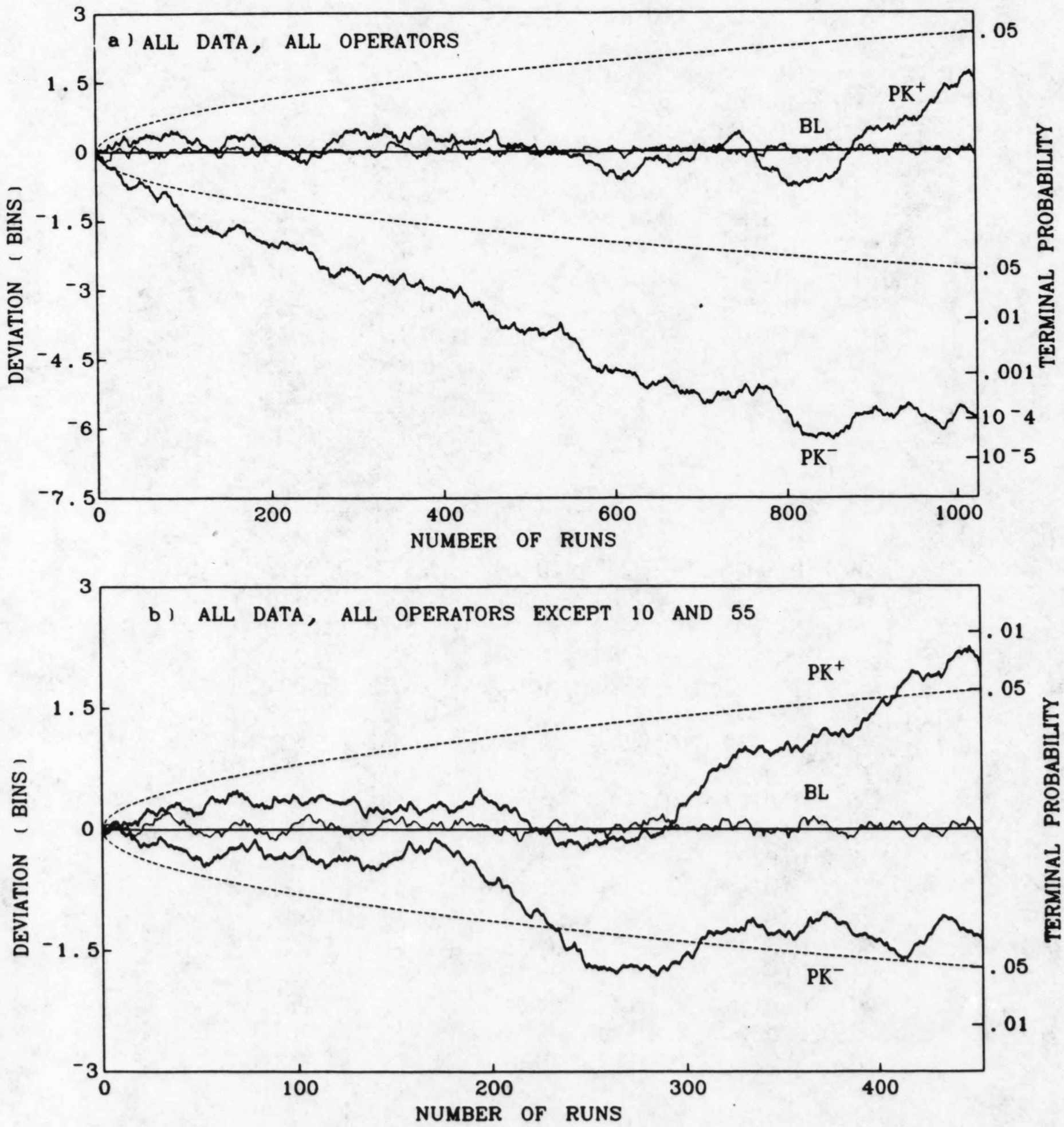


Fig. 10. RMC cumulative deviations from fitted baseline mean: (a) All data, 22 operators, (b) All data, 20 operators.

TABLE 3
RMC data summary by operator

Opr.	# Series	# Runs	BL mean	Mean	*SD**	*t*-Score*	Prob.	# Series $p < .05$	# Series $p < .5$
				PK$^+$ (right)					
10	17	270	10.0328	10.0297	.0508	−0.987	(.162)	— (2)	6
14	1	20	10.0336	10.0284	.0497	−.0472	(.321)	—	—
16	3	30	10.0111	10.0246	.0496	1.497	.073	—	3
20	2	20	10.0002	10.0226	.0505	1.975	.031	1	2
41	7	90	10.0205	10.0240	.0533	0.620	.268	—	4
42	3	30	9.9913	10.0181	.0452	3.248	.001	1	3
44	1	20	10.0419	10.0361	.0457	−0.571	(.287)	—	—
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	1	10	10.0283	10.0214	.0682	−0.321	(.378)	—	—
55	20	300	10.0272	10.0283	.0497	0.374	.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	−0.608	(.273)	—	1
69	1	11	10.0463	10.0253	.0679	−1.023	(.165)	—	—
70	4	40	10.0166	10.0278	.0541	1.310	.099	—	4
79	1	9	10.0214	10.0448	.0649	1.079	.156	—	1
84	1	10	10.0237	10.0348	.0376	0.937	.187	—	1
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	76	1024	10.0245	10.0260	.0506	0.978	.164	4 (5)	45
				PK$^-$ (left)					
10	17	270	10.0328	10.0195	.0536	−4.076	3×10^{-5}	6	14
14	1	20	10.0336	10.0259	.0418	−0.822	.211	—	1
16	3	30	10.0111	9.9994	.0507	−1.256	.110	1	2
20	2	20	10.0002	10.0259	.0448	2.566	(.009)	— (1)	—
41	7	90	10.0205	10.0241	.0488	0.697	(.244)	—	3
42	3	30	9.9913	9.9990	.0502	0.838	(.204)	—	—
44	1	20	10.0419	10.0342	.0484	−0.713	.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	1	10	10.0283	10.0195	.0632	−0.444	.334	—	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	4	40	10.0166	10.0122	.0454	−0.627	.267	—	2
79	1	9	10.0214	10.0329	.0394	0.872	(.204)	—	—
84	1	10	10.0237	10.0175	.0516	−0.375	.358	—	1
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	.0467	−2.350	.013	1	3
All	76	1024	10.0245	10.0190	.0503	−3.473	3×10^{-4}	10 (2)	49

Table 3 (*continued*)

Opr.	# Series	# Runs	BL mean	# Pairs of runs	*SD**	*t*-Score*	Prob.	# Series $p < .05$	# Series $p < .5$
				ΔPK (right–left)**					
10	17	270	10.0328	270	.0521	3.235	7×10^{-4}	7 (1)	11
14	1	20	10.0336	20	.0402	0.270	.395	—	1
16	3	30	10.0111	30	.0515	2.683	.006	1	3
20	2	20	10.0002	20	.0328	−0.461	(.325)	— (1)	1
41	7	90	10.0205	90	.0530	−0.018	(.493)	— (1)	3
42	3	30	9.9913	30	.0432	2.427	.011	1	3
44	1	20	10.0419	20	.0531	0.158	.438	—	1
49	1	10	10.0219	10	.0375	0.659	.263	—	1
51	1	10	10.0047	10	.0307	0.655	.264	—	1
53	1	10	10.0283	10	.0288	0.214	.418	—	1
55	20	300	10.0272	300	.0490	1.373	.085	2	13
63	1	7	10.0207	7	.0358	1.600	.080	—	1
64	1	10	10.0164	10	.0523	−0.134	(.448)	—	—
66	1	10	10.0210	10	.0478	−0.728	(.243)	—	—
68	2	40	10.0224	40	.0511	−0.510	(.306)	—	1
69	1	11	10.0463	11	.0359	1.218	.126	—	1
70	4	40	10.0166	40	.0463	2.143	.019	2	3
79	1	9	10.0214	9	.0666	0.535	.303	—	1
84	1	10	10.0237	10	.0566	0.965	.180	—	1
91	1	16	10.0290	16	.0383	−0.729	(.239)	—	—
93	3	31	10.0142	31	.0466	2.075	.023	—	3
94	3	30	10.0041	30	.0462	1.606	.060	—	3
All	76	1024	10.0245	1024	.0489	4.581	3×10^{-6}	13 (3)	53

* The *t*-score calculation for paired data replaces the usual *z*-score in these experiments, since an empirical baseline distribution is used as a reference in lieu of a theoretical distribution. For this calculation an empirical standard deviation (*SD*) of the differences of the means is also required.

** The ΔPK *t*-score is calculated for the difference between the PK^+ and PK^- data, unlike the REG data where ΔPK reflects the combined effect in direction of intention.

bined results of the remaining 20 operators are more symmetrical and still significant to the order of 7×10^{-4} [Fig. 10(b)].

Perhaps more importantly, the RMC results indicate operator-specific patterns of achievement similar to those found in the REG and pseudo-REG experiments, despite the widely different physical processes these three devices embody, a feature that may bear substantial implications for basic comprehension of the phenomena involved. For example, Fig. 11 compares the PK^+ and PK^- signatures of the same operator on all three PK experiments: the microelectonic REG, the deterministic pseudo-REG, and the macroscopic mechanical RMC. Each of these graphs represents a concatenation of a substantial number of experimental series conducted over long periods of time. Similar correspondences have been observed for a number of other operators, despite their characteristically different individual signatures. Thus it appears that although the observed effects are clearly operator-specific, and in many

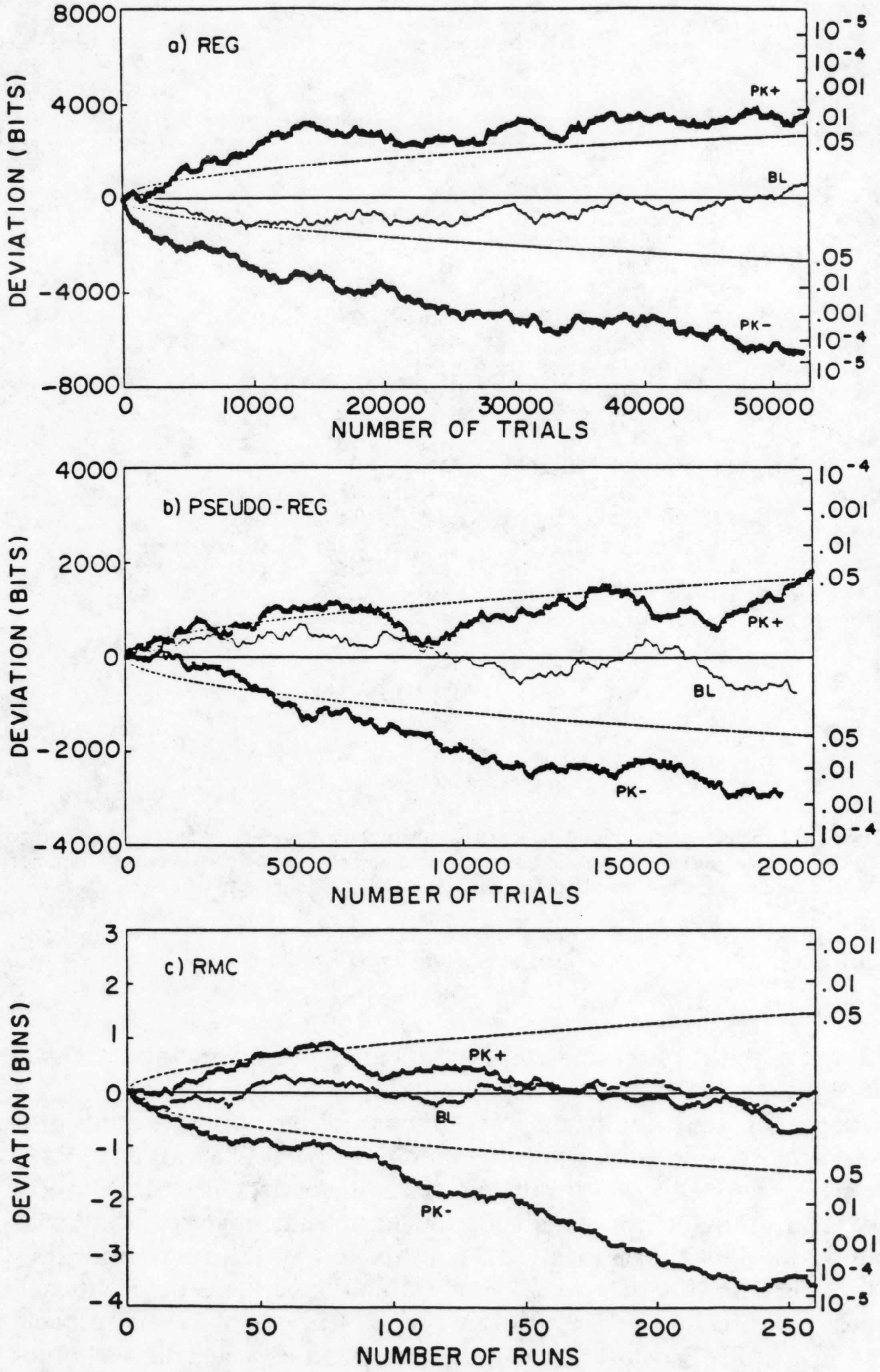


Fig. 11. Cumulative deviations: 3 devices, (operator 10).

cases condition-specific, they seem not to be nearly so device-specific. Such empirical evidence weakens phenomenological interpretations involving consciousness interacting directly with the random physical process itself, for example, with the flux of thermal electrons in the REG, or of the polystyrene balls in the RMC, and favors models that deal with aspects generic to all of these systems, for example, the information implicit in their output distributions.

Precognitive Remote Perception

The second major class of experimentation concerns the anomalous acquisition of information about remote geographical targets, inaccessible by any known sensory channel. The particular protocol followed is a variation on numerous similar studies elsewhere (Dunne & Bisaha, 1979; Hansen, Schlitz, & Tart, 1983; Puthoff & Targ, 1976; Schlitz & Gruber, 1980; Tart, Puthoff, & Targ, 1979), and is termed precognitive remote perception (PRP). Essentially, one participant, called the "percipient," is asked to generate a description of an unknown location where a second participant, called the "agent" is, was, or will be situated at a prescribed time. Initially the percipient records his impressions about the target in a free-response, stream-of-consciousness style, and then encodes them in some structured form amenable to analytical processing.

Most of the experiments reported here were conducted in a precognitive mode, wherein the percipient's impressions are recorded before the agent visits the target and, in many cases, before the target is even selected. Two modes of target selection have been employed, with no discernible effect on the experimental results. In the "instructed" mode, the target for each experiment is randomly selected from a large pool of potential targets previously prepared by a third person not otherwise involved in the experiment or its evaluation, and maintained so that no percipient or agent has access to it. In the "volitional" mode, the target is arbitrarily selected by the agent at the time specified for its visitation.

Figures 12–14 show a few examples of typical targets with portions of the corresponding free-response descriptions; more extensive data are presented elsewhere (Dunne, Jahn, & Nelson, 1983; Dunne, Jahn, & Nelson, 1985; Jahn, 1982; Jahn & Dunne, 1986, 1987; Nelson, Jahn, & Dunne, 1986). At present, the data in hand consist of 334 perceptions of this sort that range from virtually photographic accuracy, through varying degrees of correspondence to the details and overall ambience of the scene, to total irrelevance. In some cases, details that are central to the agent's view of the scene are ignored by the percipient, while minor aspects are escalated in importance. In other cases, there are spatial inversions or other geometrical distortions. Frequently, the more impressionistic aspects seem to be perceived more accurately than the analytical details.

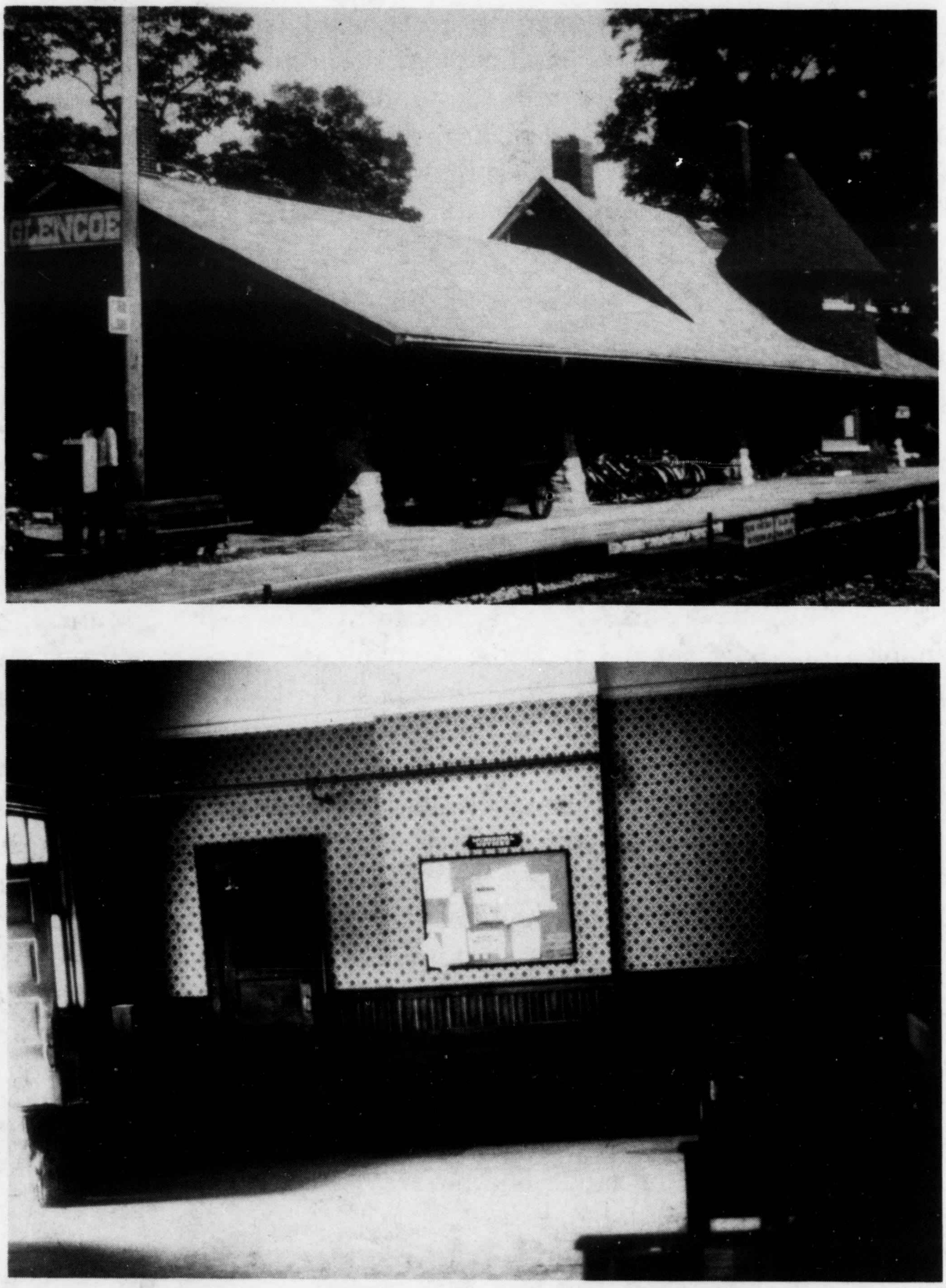


Fig. 12. PRP Target: NWRR station, Glencoe, Illinois. The percipient was some five or six miles away. The perception, generated 35 minutes precognitively, reads, in part:

". . . I see a train station, one of the commuter train stations that's on the expressway . . . I see a train coming . . . See just the front end of the train station. See a little bit within it. I think there are wooden planks on the floor. I hear like the clicking . . . of feet or shoes on the wooden floor . . . There are posters or something up, some kind of advertisements or posters on the wall in the train station. I see the benches. Getting the image of a sign, but I think it's probably the sign of what station it is. It's about 8 or 10 letters in the word. Maybe something like Clydeburn or Clayburn. Have the impression of this wooden floor being somewhat littered, just sort of dirty. I see the tracks. No train on the tracks right now. Empty tracks. . . ."



Fig. 13. PRP Target: Ruins of Urquardt Castle, Loch Ness, Scotland. The percipient was in New York City, some 3500 miles away. The perception, generated 14 hours retrocognitively, reads:

"Rocks with uneven holes. Also smoothness. Height. Ocean. Dark. Dark blue. White caps. Waves booming against rocks? On mountain or high rocks overlooking water. Dark green in distance. Gulls flying? Pelican on a post. Sand. A lighthouse? Tall structure. Round with a conical roof. High windows or window space with a path leading up to it. Or a larger structure or a castle." (Here there is a sketch of a castle abutment on the transcript.) "Old. Unused. Fallen apart. Feeling musty, or dark. Moss or grass growing in walls. Wood draw bridge? A black dog? Snow. Ice capping a mountain. High large cavernous hall. Castle."

The principal effort in this study has been to devise analytical methods to extract from such subjective evidence some quantitative measure of the degree of anomalous information acquisition. For this purpose, a code, or alphabet, of simple descriptive queries is employed, which can be addressed to all targets and all perceptions. These descriptors, 30 in number, range over a spectrum from quite factual discriminations, for example, whether the scene is indoors or outdoors, whether trees are present, or whether there are automobiles, to much more subjective aspects, such as whether the ambience is noisy or quiet, confined or expansive, hectic, or tranquil. Encoding of the target is normally performed by the agent at the time of visitation, and of the perception by the percipient after the free-response impression of the target has been recorded.

With the target and perception thus encoded, a variety of analytical scoring methods are invoked, described in detail elsewhere (Dunne et al., 1983; Jahn, Dunne, & Jahn, 1980; Jahn et al., 1982), that yield numerical scores indicative of the information content of each perception relative to its corresponding target. Most of these methods acknowledge the a priori probabilities of the various descriptors, that is, that more scenes tend to be outdoors than indoors, that more tend to have people in them than not, etc.; therefore, a perception



Fig. 14. PRP Target: Tretiakovskaia Gallereia, Moscow, U.S.S.R. The percipient was in Wisconsin, some 4500 miles away. The perception, generated 24 hours precognitively, reads, in part:

"Have the sensation of being in a very quiet, sombre, subdued sort of atmosphere. . . . Any color impressions I get are the same—greys, browns, dark subdued colors. I feel an oldness. . . . I'm thinking of a large church or something, or a castle. Some kind of building. It seems to be quite large. Sensation of sounds echoing, subdued colors. . . . I see several, maybe two to four round balls that seem to be on top of something. Maybe it's some kind of decoration. Like on top of something that's of a generally square shape. Almost like a square column with a ball on top. I have a very clear picture suddenly of an old building. It's quite large. There are windows with, like, arches. They may not be exactly arched; the arches come to a point on top, almost. Very impressive. It's a light grey color, very ornate. It comes to a point of some sort, but it's not a regular point. Like where it should be round on top it comes to a point. I'm not sure if it's windows or the shape of the building itself. . . . Great big double doors. . . . Just saw those square pillars with the balls on top again. They seem to be almost like an entranceway, one on either side. . . ."

that is accurate about less likely aspects achieves a higher score than one that correctly predicts more likely features. The scores are all normalized in some fashion, for example, by various chance expectations or by perfect scores. In some recipes ternary or quaternary descriptor responses are also employed, whereby the agent and the percipient can effectively reject or equivocate on a question, or express gradations of its importance.

The most powerful aspect of this coding approach is that unlike traditional impressionistic ranking procedures, digital scoring algorithms can be applied to compare any perception with a very large number of alternative targets—not just the 5 or 10 that could be compared by a human judge. The distribution of the mismatch scores, that is, the off-diagonal matrix elements of the perception/target array, has sufficiently Gaussian characteristics to serve as an empirical "chance" reference for statistical quantification of the correctly

matched scores. The process is illustrated in Fig. 15, where the larger dashed curve is the empirical chance distribution thus constructed by one particular binary scoring method from some 42,000 mismatch scores. In comparison, the solid line, denoting the distribution of proper target scores for 334 formal trials is seen to be distorted to the high score side. If we remove from that proper target score distribution the largest component that is a subset of the empirical chance distribution, (dotted curve), the residue (dot-dashed curve) should be an indication of the information acquired beyond the chance expectation for guessing. In this case, about 15% of the trial scores are involved in that extra-chance, positive information residue. More detailed numerical calculation yields a probability for this degree of information acquisition by chance of about 10^{-11} for the data and method illustrated. The other scoring methods show comparable results (Dunne, Jahn, & Nelson, 1983).

A primary interest is the dependence of this extra-chance information component on the physical parameters of the experiment, most notably on the distance between the percipient and the agent. As illustrated in Fig. 16, within the accuracy of the data and the statistical treatment just described, no significant dependence is found; up to intercontinental distances of several thousand miles, there appears to be no discernible advantage for closer targets. Certainly, there is no $1/r^2$ dependence that might be expected for various wave-propagation mechanisms that have been proposed for such phenomena (Kogan, 1968; Chari, 1977; Persinger, 1979).

Perhaps even more striking is the absence of any discernible dependence of perception accuracy on the time interval between perception effort and target visitation by the agent. Figure 17 shows the 334 formal trial scores

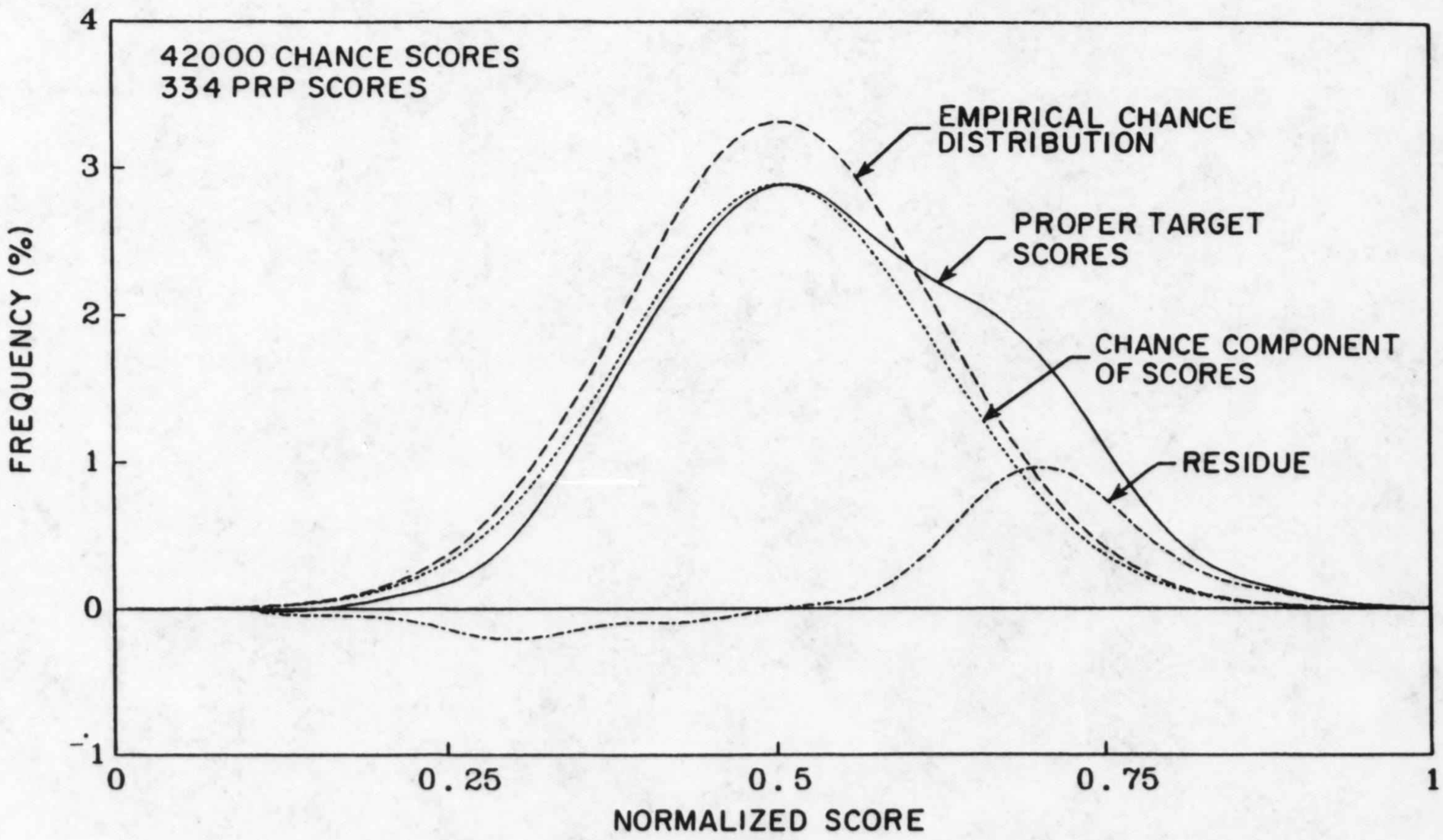


Fig. 15. PRP score distribution.

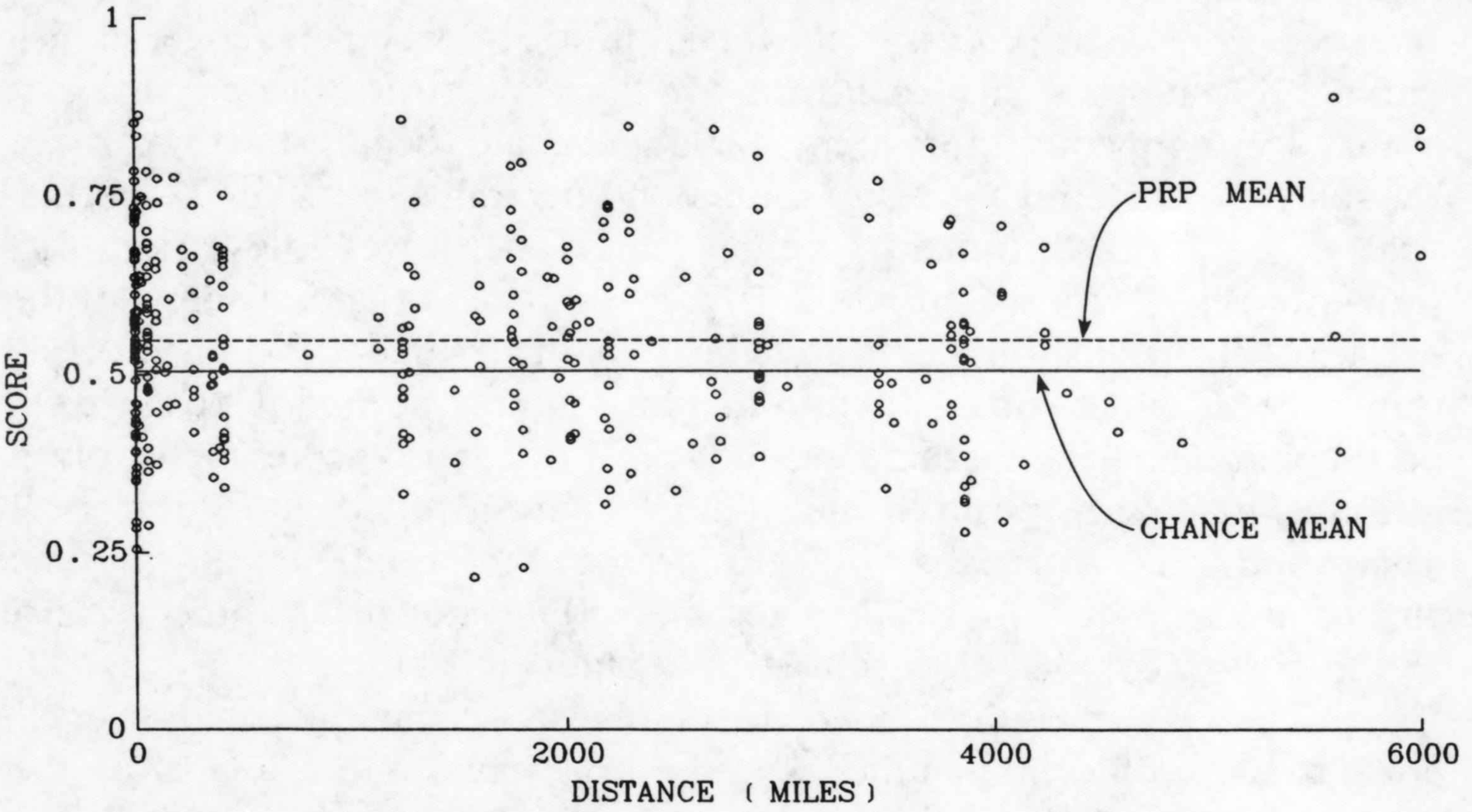


Fig. 16. PRP scores versus spatial separation, $N = 334$.

arrayed as a function of this time displacement. Zero abscissa denotes that the perception was dictated at the time of target visitation. Times to the right of this correspond to perceptions that were attempted precognitively by the indicated number of hours. Times to the left comprise a smaller body of data taken retrocognitively, wherein the percipient dictated the perception after the target had been visited, but before any information had been transferred by ordinary means. Again, over the range covered by these experiments, there

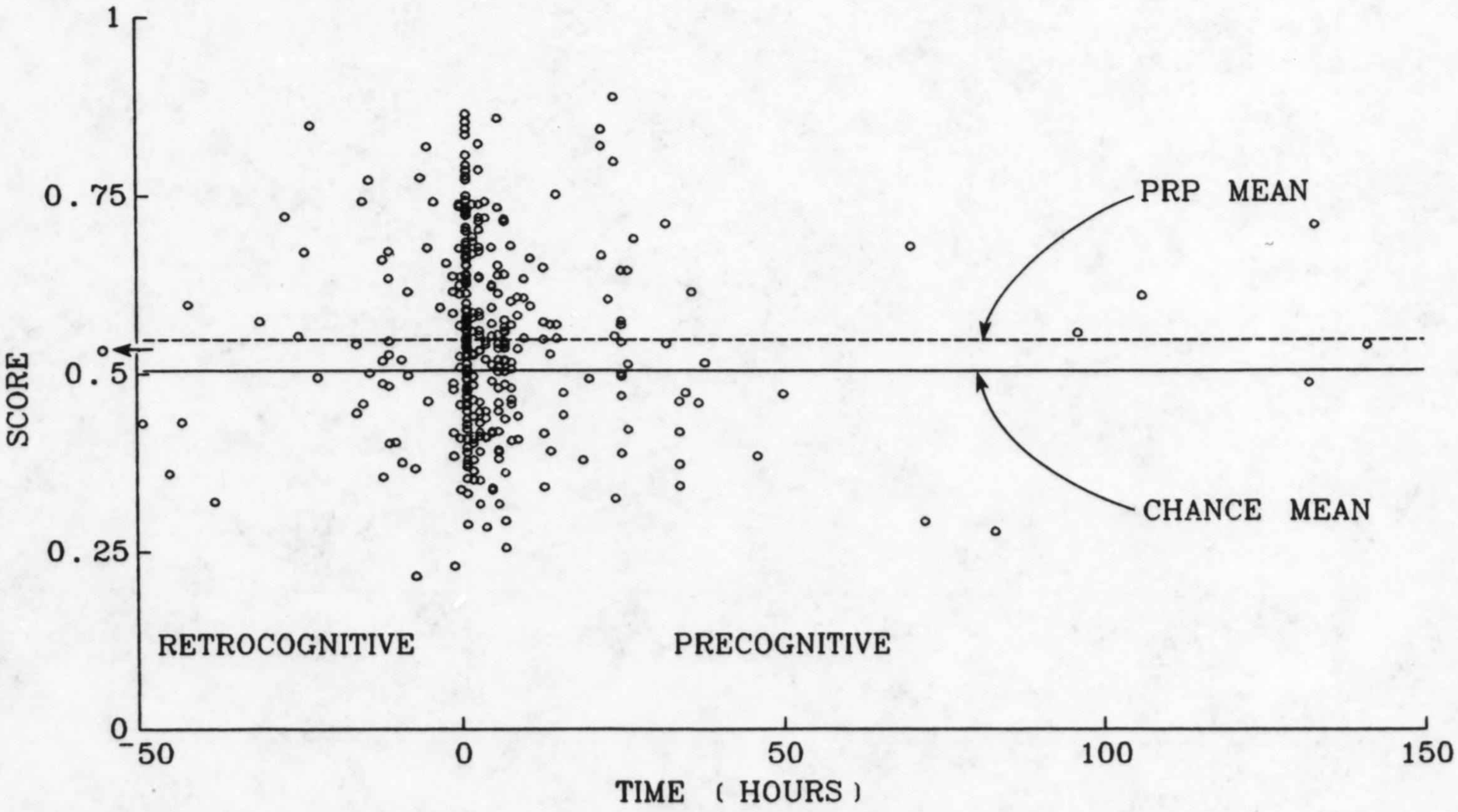


Fig. 17. PRP scores versus temporal separation, $N = 334$.

is no statistically significant dependence on this temporal parameter. Identification of the specific process whereby the consciousness of a percipient is apparently able to access points remote in both space and time from its current physical location is well beyond our present understanding, but data such as displayed in Figs. 16 and 17, if sustained in further experimentation, will severely delimit the physical mechanisms that could legitimately be invoked.

Clearly there is also interest in the dependence of the yield of such experiments on the psychological characteristics and strategies of the percipients and agents, both separately and as paired participants, but as noted earlier, this category of correlation has not been extensively pursued, in part because the data base is still far too small for such analyses to be effective. Nonetheless, the compounding data are routinely examined to gather impressions about the efficacy of the descriptor questions, the variability in individual interpretation of and response to the descriptors, and the physical and psychological correlates of the most successful trials.

Theoretical Considerations

Experiments like those outlined above beg for some form of theoretical model to help correlate data, design more incisive experiments, and interpret the observed effects in more fundamental terms. The literature of psychic research abounds with attempts to transpose various physical formalisms to this purpose: electromagnetic models, thermodynamic models, mechanical models, statistical mechanical models, hyperspace models, quantum mechanical models, and others (Bohm, 1971; Chari, 1977; Costa de Beauregard, 1979; Feinberg, 1975; Kogan, 1968; Persinger, 1979; Rauscher, 1979; von Lucadou & Kornwachs, 1979). Although these comprise an interesting body of effort, none of them seems fully competent to accommodate experimental data like those described above. Indeed, it appears that no simple application of existing physical theory is likely to prevail. In order to encompass the observed effects, a substantially more fundamental level of theoretical model will need be deployed, one which more explicitly acknowledges the role of consciousness in the definition of physical reality.

The model that has so far proven most serviceable for our purposes takes the position that reality, or experience, is constituted only in the interaction of consciousness with its environment, and thus that any physical theory, or any other scheme of conceptual organization, can only properly address the interaction, not the environment or the consciousness, per se. Similarly, it regards the common concepts and formalisms of physical theories as no more than useful organizing strategies adopted by the consciousness to order and process the information it acquires from the environment. Therefore, these should be as much reflective of the characteristics of the consciousness as of those of the environment or, more precisely, they should reflect the characteristics of the interaction of the two.

In this spirit, the model attempts to apply, via metaphor, the concepts and formalisms of elementary quantum mechanics to a representation of the interaction of consciousness with physical systems and processes, in a form that can accommodate both "normal" and "anomalous" behavior. Central to this metaphor is the assignment to consciousness of a partially wave-like character which can manifest itself in various interactions, much like the wave/particle duality of atomic scale phenomena. More specifically, by representing consciousness by quantum mechanical wave functions, and its physical environment by appropriate potential energy profiles, Schrödinger wave mechanics may be used to derive eigenfunctions and eigenvalues that can be associated with both the psychological and physical experiences of the consciousness/environment interaction. To bring this metaphor to pragmatic utility, it is necessary to relate certain mathematical aspects of the formalism, such as the coordinate system, the quantum numbers, and even the metric itself, with various impressionistic descriptors of consciousness, such as its intensity, perspective, approach/avoidance attitude, balance between cognitive and emotional activity, and receptive/active disposition. With these in hand, the generic principles of quantum mechanics—uncertainty, indistinguishability, correspondence, exclusion, etc.—as well as a number of specific computational applications, such as the central force field and atomic structure, covalent molecular bonds, barrier penetration, and quantum statistical collective behavior, become useful analogies for representation and correlation of a variety of consciousness-related physical effects, both normal and anomalous, and for the design of experiments to study these more systematically.

The full text and mechanics of this model are developed elsewhere (Jahn & Dunne, 1986, 1987), along with its application to particular experimental situations. Even in its comprehensive form, since the various associations it invokes are largely intuitive and empirical, and since few quantitative scales of consciousness properties yet exist, no more than semi-quantitative correlations can so far be made. Notwithstanding, comparison of our laboratory data and the informal testimony of our operators with appropriate aspects of the model have substantiated our confidence in the potential utility of the quantum mechanical metaphor and confirmed its value in providing a viable perspective and a facile language for the design of better experiments and the informal representation of the operators' cognitive and emotional attitudes and strategies. Beyond this, the model suggests a number of testable hypotheses, some of which are currently under study. For example, the postulated wave-mechanical nature of consciousness/environment interactions implies that the combined efforts of two or more consciousnesses attending to the same task may display constructive or destructive interference patterns, rather than simple linear superpositions. To test this, experiments utilizing the REG and RMC devices are underway to explore the effects of multiple operators addressing the same task simultaneously, compared to their individual signatures of achievement, and preliminary results appear to support the model. Yet other experiments are investigating the effects of spatial separation between

operator and device, to be compared with the same operator's performance under proximate conditions, in an effort to comprehend the consciousness distance parameter.

Applications and Implications

As mentioned in the introduction, this program of research is intended to address aspects of anomalous consciousness-related phenomena that could bear on engineering practice, in both the short and longer terms. Based on the results obtained to date, a few domains of immediate relevance indeed appear to merit more specific and detailed study, and others may present themselves in the longer view. In particular, the results with the random event generators raise the generic possibility of anomalous effects arising from conscious or unconscious interactions between human operators and any sensitive microelectronic information processing devices or systems, most especially those involving random or pseudo-random noise elements for signal initiation or reference profiles. In the remote perception category, numerous current applications of such techniques in national security and law enforcement, archaeological searches, natural resource prospecting and medical diagnosis, employing various aspects of the experimental and analytical techniques outlined above, could be cited. As in the basic research, the emphasis in many of these applications has already shifted from credibility demonstration to refinement of protocols, participant strategies, and data analysis techniques to enhance their individual and collective efficacy.

It is premature to speculate on the broader and more fundamental implications of the empirical results and supporting model to the basic scientific paradigm, other than possibly to suggest that some generalization of the prevailing "particulate" view of consciousness, with its linear, causal, objective premises, to a more "wave-mechanical," participatory, holistic conceptualization may allow the observed "anomalous" phenomena to be accommodated as normal consequences of bonded-system behavior.

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Society and Scientific Anomalies: Common Knowledge About the Loch Ness Monster

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Abstract—The public image of the Loch Ness monster is that of a mythical creature appropriately talked of in a joking manner. The sporadic and often jocular coverage of the subject in the press is consistent with that image. But magazine articles since the 1950s have tended to project a belief in the reality of the animals; the books on the subject even more so. Thus the public view and that purveyed by the mass media are at variance with the views of those who have studied the matter closely and recently. The public image still reflects the consensus of 50 years ago, when the subject first made news: editorial policies governing press coverage of the Loch Ness controversy are an instructive exemplar of how and what society learns about scientific anomalies.

In science, observational and theoretical claims are made and judged under well-established conventions. Apparatus, methods, modes of analyzing data are clearly specified by the claimants, and such specification is made easy by the fact that practitioners are trained within a shared tradition. Further, problem-solving proceeds on the basis of a pre-existing consensus as to what the problem is and, to a considerable extent, what the relevant data are.

That well-ordered state of affairs does not obtain where anomalies are concerned: there, it is not clear whether the problem is the actual existence of an anomaly (something that does not fit accepted theory) or whether the anomaly is an artefact of sampling, an experimental or observational error, or the like. Anomalies that surface within established specialities are, over the long haul, resolved by the same well-established traditions, among which orderly, refereed publication under tight rules of documentation and citation is of significant importance. But, at any given time, some anomalies lie outside any existing scientific speciality (or, it is not clear in which speciality they belong): for example, acupuncture (until quite recently) or UFOs. In such instances, even the documentation, verification, classification of the most rudimentary data are problematic. In many cases, for example, one wonders whether folklore, legend, and myth provide relevant data; and if so, in what manner? On the

I am grateful to those who made comments on earlier drafts of this article—most especially Ellsworth (Skip) Fuhrman, J. Richard Greenwell, Bernard Heuvelmans, and Ron Westrum. Susan Miller helped with bibliographic work and in scaling some of the data. Carla Knapp traced and obtained many items that were not locally or readily available.

question of sea-serpents, for instance—are tales of dragons relevant or not? And if so, relevant in the sense that they indicate that our forebears were inspired to such tales through sighting sea-serpents, or in the sense that our exposure to tales of dragons predisposes us to think mistakenly that we see sea-serpents under appropriate conditions?

Attempts to answer such questions lead to consideration of what has been written? by whom? when? how published? how verifiable? And these questions—interesting in themselves in many ways—need to be answered before the "objective" problem and data can be identified and solved. In short, where subjects are concerned that are anomalous, that could perhaps become matters of science but are not yet classed as such, analysis of the modes of transmission of the possibly relevant knowledge is a necessary endeavor. Westrum (1977, 1978, 1979) has discussed the social processes by which knowledge about (claims of) scientific anomalies has been transmitted and preserved in relation to UFOs, meteorites, and sea-serpents. The case of the Loch Ness monster also lends support to some of the generalizations adduced by Westrum: (a) fear of ridicule inhibits the reporting of anomalous events; (b) the rates of reporting and of publishing of reports are determined by the social demand for such reports and not by the rate of occurrence of events; (c) skeptics and debunkers frequently assume that a lack of reports reflects a lack of events, but this inference is invalid; (d) it is often, again invalidly, argued that if the phenomenon were real then Science would already know of it. However, the primary concern here will be to characterize the manner in which the Loch Ness controversy has been portrayed in magazines and newspapers, and the relationship between that and the popular view of the Loch Ness monster.

Method of Investigation

Collecting the Literature

A crucial difference between the literature on anomalies and that of science is the lack of organization of the former. On a scientific topic, one can reasonably expect to find almost all the relevant literature through the use of indexes, collections of abstracts, specialized journals, and contacts within the "invisible college." By contrast, one can be rather certain that one's coverage cannot be complete of items relating to such a subject as the Loch Ness monster. So it is necessary to begin here with a discussion of how the relevant literature was sought, and to consider how the analysis might be biased by the failure to locate some unknown number of items.

This study aims to characterize that knowledge about Loch Ness that is available to, and thus influences, the society as a whole. So the literature that is most difficult to trace and obtain is not of major concern here—the ephemeral bulletins, newsletters, reports, etc., which are issued by short-lived organizations and which circulate among that very small group of people that has a special interest in the subject. (The author has, however, had access to

a considerable portion of this material about Loch Ness for nearly two decades and this also served as one way of finding books that have been published and articles in established periodicals.)

"Books in Print" served to locate some works. Various indexes of periodicals were searched: Reader's Guide; International Index (later, Social Sciences and Humanities Index, later separated); Cumulated Magazine Subject Index 1907–1949; Popular Periodical Index; British Humanities Index; Monthly Periodical Index. Citations in the books and articles so found naturally led to other items. A preliminary bibliography was circulated to others who have been interested in the subject, and more items were collected through the help of those individuals, most notably Jean Berton, Tim Dinsdale, Bernard Heuvelmans, Roy Mackal, Marcello Truzzi, Ron Westrum, Constance Whyte. Ultimately, more than 30 books and nearly 350 items in regularly published periodicals were obtained (in addition to much miscellaneous material that is not of concern here). (This article deals only with items published in English, though some pieces in other languages were found and read.)

The coverage of newpapers was limited to ones that are indexed, and two to which the author subscribed for a dozen years (*Louisville Courier-Journal* and *Lexington* (Kentucky) *Herald-Leader*). In view of their influential nature, the *London Times* and the *New York Times* were important, and indexes are fortunately available. On the subject of Loch Ness, it seemed essential that at least one regional (Scottish) newspaper be included. The first choices might have been the *Inverness Courier* and the *Scotsman,* which reported regularly about the monster, but indexes were not available; as a reasonable alternative, the *Glasgow Herald* was used, which made indexes available until 1969. Nearly 200 items were obtained from the *Glasgow Herald* (1933 to 1969), more than 150 from the *London Times* (1933 to 1983), and 115 from the *New York Times* (also 1933 to 1983). Choice of 1933 as the starting point was dictated by the fact that the Loch Ness monster first attained national and international prominence in that year.

Also of interest are the actual numbers of sightings and reports of sightings. Here there enter some additional uncertainties—most obviously, that some of these are hoaxes or obviously mistaken identifications. For want of a better alternative, the assessments of otherwise reliable authors were accepted in counting reliable sightings of what were not obviously tree-trunks, swimming stags, cattle, and so forth; particular reliance was placed on the works of Gould (1934), Whyte (1957), and Mackal (1976), leading to some 305 reports of sightings from 1933 to 1969. (Since this analysis was carried out, a comprehensive list of sighting reports has been made available by Ulrich Magin (see Bauer, 1986), but Magin seeks to list *all* reported sightings including mistaken identifications.)

Coding the Data

Answers were wanted to a number of questions: to what degree has the Loch Ness monster been treated seriously as opposed to jocularly? Has there

been a change in attitude since 1933? Are there differences in attitude in different portions of the literature (for example, between scientific journals and general-interest periodicals)? How factually reliable are the various types of literature?

Thus, coding of certain features was called for. Newspaper reports were categorized as news items, letters, editorial comment, or features with by-line; and each item was scaled for attitude (+2 for belief through 0 for neutral to −2 for disbelief) and for jocularity (from 0 for not jocular to +2 for entirely jocular). Magazine articles were similarly scaled for attitude and jocularity; and also for reliability (from 0 for no factual errors to −2 for totally erroneous); for length (up to 1 paragraph; 1 paragraph to 2 pages; more than 2 pages); and for type of magazine (for an audience of scientists; popular science for a general audience; general-interest or mass-circulation; special-interest or small circulation; and specializing in anomalies). Chapters or sections of books were treated in the same way as magazine articles. (Not included were book reviews, fiction, and such miscellani as guide-books and pamphlets.)

Books dealing exclusively or mainly with the Loch Ness monster have already been characterized in an annotated bibliography (Bauer, 1980, 1982): all the books but one are written by more-or-less convinced believers in the reality of large animals in Loch Ness and the books vary from completely trustworthy to totally unreliable; those features are often met within the literature about anomalies—"fan" and "pro" books usually far outnumber the "debunking" works, and such scholarly virtues as reliable citing of references are frequently absent. (After the present analysis had been completed, two more books by disbelievers were published (Binns, 1983; Campbell, 1986) as well as the author's (Bauer, 1986) analysis of the controversy.)

The coding was done by the author. To obtain an estimate of reliability of the coding, every tenth article (chosen chronologically) was independently scaled for "attitude" by Susan Miller, who had initially no familiarity with the subject; her scaling agreed precisely with that assigned by the author for 71% of the items, and differed by only an average of 0.4 on the five-point scale for the whole sample. Scaling of the other items ought to be less sensitive to subjective bias—with the possible exception of "reliability." Perforce, however, the identification of errors of fact requires one who is thoroughly familiar with the body of literature but who is thereby also likely to have definite views. The author happens to believe that the Loch Ness monsters are real, yet-to-be-classified animals; and he might thus be prone to scale as low in reliability articles expressing disbelief, solely for that reason. There seems to be no solution to this dilemma, which involves the data in Table 11 and conclusions drawn therefrom; but as a rudimentary safeguard, I give here examples of what I regarded as clear errors of fact: Eyewitness descriptions evidence little agreement among one another (Anonymous, 1934a, 1934b; Burton, 1960a; Stewart-Gordon, 1977; Sweeney, 1977); on the other hand, they agree on a kraken-like (Mercatante, 1974) description, or one entailing a whiskery head and eight humps (Anonymous, 1937); and sightings have

never been made by more than one little knot of people at a given time (Gordon, 1978). Only one photograph has been obtained in 50 years (Sturdivant, 1976); presumably, the Surgeon's photograph, which has (again, mistakenly) been said to show the back (Stewart-Gordon, 1977) as well as the neck and head, to have been taken near Urquhart (Sweeney, 1977) in 1933 (Anonymous, 1976b; Walsh, 1976)—or, by the LNI (Loch Ness Investigation) at a range of four miles (Swan, 1976; Sweeney, 1976). Sightings and photographs are usually achieved under poor, misty conditions (Cohen, 1965; Sweeney, 1977; Walsh, 1976). Dinsdale filmed a motor-boat (Burton, 1960a, 1960b, 1962, 1969); the Royal Air Force unit which analyzed his film was (Sweeney, 1976) the official coordinating center for the search; the report on the film gave a possible length of up to 92 feet for the creature (Cohen, 1965, 1970; Holiday, 1966; Keel, 1970). The British government had earlier mounted an official investigation (Sweeney, 1977). Nowadays, Frank Searle is the most active monster-hunter (Gordon, 1978). The sonar unit used in 1968 by the team from Birmingham University was defective (Anonymous, 1968). From the focal length of a camera and the size of the negative, one can calculate the distance and size of a photographed object (Burton, 1962). Loch Ness has always been freshwater (Anonymous, 1968).

Notions of what is jocular are likely to be fairly uniform. Items were coded as "entirely jocular" when written tongue-in-cheek *throughout* with apparently no other purpose (Anonymous, 1934a, 1934b, 1937; Coren, 1976; Ford, 1934; Holden, 1974). "Definitely jocular" items contain very clearly expressed statements of a joking sort, but intermingled with serious or purportedly factual commentary (Anonymous, 1935, 1957, 1970; Gordon, 1978; Greenburg, 1974; Gregory, 1934; Klass, 1977; Mercatante, 1974; Swan, 1976). "Somewhat jocular" refers to items that evidence humor sufficiently gently that it does not necessarily imply disbelief in the reality of the monsters (Anonymous, 1975, 1976a, 1977; Bartusiak, 1979; Graves & Hodge, 1950; Scheider & Wallis, 1973; Sheldon & Kerr, 1972). Though these may seem to be straightforwardly distinct categories, residual uncertainties are inevitable. For example, how to class the article by Carl Sagan (1976) that purports to estimate the number of monsters? If intended as humor, it is expressed so straightfacedly that many will miss the point; yet one hopes that no one would *seriously* consider applying to this question the assumptions and mode of calculation used in that piece.

There are some further points of possible ambiguity which, however, are hardly likely to bias the conclusions drawn in this study. For instance, it is not always obvious whether an item ought to be included at all: in 1933 and 1934, newspapers would have as a headline "Another Sea Monster Seen"—"another" clearly referring to the furor about Loch Ness, but without any explicit mention of it; should such items be included? (They were.) Then again, how to class the book by Campbell (1972) about Morag, the sister of Nessie in Loch Morar? As a book about the Loch Ness animals, or as one which contains a section only devoted to Loch Ness? And it is also not always obvious, how to classify a periodical, as between scientific, popular science,

and general interest (here, the likely readership was used as a criterion, so that *Nature* and *Science* were classed as "scientific," and *Wildlife* and *National Geographic* as "popular science," for example).

Discussion

Type of Literature, Chronologically

Table 1 lists the numbers of items of different types that have appeared during five-year periods (with a separate period for the initial flurry in 1933 and 1934).

Several things are immediately apparent:

1. After the 1930s, there is a general *lack* of correlation between numbers of (eventually reported) sightings and numbers of items in newspapers; with less discrepancy in the *Glasgow Herald* than in the *London Times* and *N.Y. Times* (see also Table 2).
2. In the late 1950s, there was a marked increase in the rates of publication of articles and books.

Evidently, different things determined the coverage given by newspapers and by magazines. I shall examine in more detail the behavior of the news-

TABLE 1
Frequencies of sightings and writings, 1933–1983
(numbers of items in each category)

	Reported Sightings[a]	In *Glasgow Herald*	In *London Times*	In *New York Times*	Articles in Periodicals	Chapters or Sections of Books	Books Solely or Chiefly about Loch Ness
1933–34	106	91	65	46	47 (80%)[f]	1	1
1935–39	27	35	21[b]	3	5 (80%)	1	0
1940–44	17	1	1	0	2 (50%)	2	0
1945–49	12	8	0	2	5 (80%)	0	0
1950–54	22	13	3	2	7 (60%)	0	0
1955–59	16	10	1	1	16 (30%)[f]	4	1
1960–64	43	16	11	4	38 (70%)	2	2
1965–69	62	24	11	4	40 (25%)	3	4
1970–74	na	na	12	10	48 (35%)	7	8
1975–79	na	na	30[c]	43[d]	103 (35%)	16	14
1980–83[e]	na	na	5	0	53 (40%)	5	2
Totals	305	198	160	115	346	41	31

[a] See text for provenance.
[b] 19 of these in 1938; see text.
[c] 18 of these in 1975; see text.
[d] 37 of these in 1976; see text.
[e] Through mid-1983; the numbers were increased by 50% to make this period of $3\frac{1}{2}$ years roughly comparable to the other, 5-year periods.
[f] These percentages of the articles appeared in British (as opposed to American, Canadian, or Australian) magazines.
na—Not available to author.

TABLE 2
Numbers of newspaper items per reported sighting

	In *Glasgow Herald*	In *London Times*	In *New York Times*
1933–34	0.85	0.60	0.45
1935–39	1.30	0.80	0.10
1940–44	0.05	0.05	0.00
1945–49	0.65	0.00	0.15
1950–54	0.60	0.15	0.10
1955–59	0.65	0.05	0.05
1960–64	0.35	0.25	0.10
1965–69	0.40	0.20	0.05

Note. From data in Table 1.

papers, and then look further at the other literature. But first I shall seek to define the public attitude toward the monster.

The Public Image of the Loch Ness Monster

The Loch Ness monster rarely needs an introduction—few have not heard of it (or her, under the title of Nessie). The roller-coaster at Busch Gardens in Williamsburg, Virginia, is named in its honor. It is featured in television commercials—for example, a leathery back lifts out of the water a boat-load of kilted and tartan-capped men, in an advertisement for Kellogg's "Raisins, Rice and Rye," shown on U.S. television in 1981. Any serpentine shape coupled with a suggestion of tartan or whisky can, as every cartoonist knows, be used without more explicit identification. And Nessie has been featured in novels and short stories, for instance Charteris (1959), Hoyle and Hoyle (1971), Hughes (1964), Kellogg (1977), MacKenzie (1952).

Soon after Nessie became international news at the end of 1933, she became closely identified with sea serpents (Gould, 1933; Editorial, *Times,* December 9, 1933); and has thereafter shared the same public image—of a legendary, mythical, folklorish creature that some gullible people suppose to actually exist. Those who claim sightings are typically ridiculed: Burton (1961, p. 11), Dinsdale (1972, p. 58), Gould (1930, pp. 188, 189, 193), Heuvelmans (1968, p. 461), Whyte (1957, pp. 3, 42, 68, 75, 119, 157), Witchell (1974, pp. 128, 136). Pundits speak of Nessie in the same breath as they do of UFOs, yetis, P. T. Barnum, Atlantis, and so forth: see, for example, Cazeau and Scott (1979), Cohen (1965), Corliss (1976), *Daily Mail* (London; October 3, 1970), Frazier (1978), Goudsmit (1976), Nichols (1950), Sladek (1973), *Time* (12 December 1977, p. 100).

Nessie has, in fact, become the prototype and stereotype of the aquatic monster. Often, when such a creature is reported, it is described as "a South Pacific Nessie," for example, rather than as a South Pacific monster or sea-serpent (*Newsweek,* 1 August 1977, 77); or "Cherokees have own Loch Ness

monster" (Asheville (North Carolina) *Citizen-Times,* January 10, 1971, cited in Mackal (1980, p. 212)); and Nessie's existence serves to validate the search for a similar creature in Lake Champlain—for instance Vachon (1977) and Zarzynski (1984). A naturalist's sketched impression of a typical sea-serpent is also suggestive: his earlier sketch had resembled the shark-like mosasaurs, but subsequently he pictured the sea-serpent as a Nessie-like plesiosaur (Thone, 1937, 1949).

Belief that the Loch Ness monsters are real animals appears not to be widespread. According to a Gallup poll of 1978, only 13% of Americans believed in Nessie, comparable to the rate of belief in Bigfoot (13%) and ghosts (11%) and quite significantly less than that in clairvoyance (24%), astrology (29%), precognition (37%), demons or devils (39%), telepathy (51%), angels (54%), UFOs (57%) (Greenwell, 1980; Greenwell & King, 1980).

Nessie in the Newspapers

The English and the American national dailies have devoted approximately the same amounts of space (or lack thereof) to reporting events from Loch Ness (Tables 1 and 2). The similarity becomes even more striking when three distinct events are identified: in 1938, a proposed expedition excited a flurry of interest in Britain but not in the U.S.—19 of the 21 items in the *London Times* during 1935–1939 appeared on that occasion; in 1975, an announced and then cancelled symposium in Edinburgh, and a presentation at the Houses of Parliament, was responsible for two thirds of the 30 items in the *London Times* in the years 1975–1979; whereas the *New York Times* had, during 1976, more than three fourths of all the items that it published in the five years 1975–1979 and the tone of the coverge was much more positive than in other years (Table 4)—because it sponsored an expedition during that year (Pollak, 1976; Swan, 1976). Thus the Loch Ness story was judged equally newsworthy by national dailies in both countries, briefly, in 1933 and 1934, and equally not worth covering thereafter (except for the special occasions alluded to above).

But there is a clear difference between the *Glasgow Herald* and the national dailies. The frequency with which Nessies are mentioned in the *Herald* parallels quite closely the frequency of ultimately reported sightings (Tables 1 and 2)—about one published item for every two sightings rather regularly since the mid-1930s. By contrast, the items in the national dailies are drastically fewer (except at the "flap" times of 1933–34, 1975, and 1976) and show no correlation with the number of sightings. That difference is even more striking when the nature of the coverage is examined. Almost all the items in the *Glasgow Herald* (well over 90%), are purportedly factual accounts, without jocularity or editorial comment; but in the *London Times,* about half the items are letters to the editor, and another 1 out of every 12 is a feature story or editorial.

The *Glasgow Herald* appears not to have changed its approach from the very beginning: the possible reality of large animals in the loch was and is

TABLE 3
Frequencies of sightings and newspaper items, 1933–1934

	Reported Sightings[a]	Items in *London Times*	Items in *Glasgow Herald*
1933			
May	3	—	—
June	4	—	—
July	2	—	—
August	15	—	—
September	7	—	—
October	8	—	1
November	7	—	2
December	11	29	16
1934			
January	7	17	15
February	4	0	3
March	0	1	4
April	3	1	6
May	4	1	9
June	6	1	4
July	17	1	11
August	7	6	7
September	1	3	5
October	0	1	5
November	0	2	1
December	0	2	2
Totals	106	65	91

[a] See text for provenance.

judged to be of interest, relevant items were and are printed, and so coverage has roughly tended to increase and decrease with the rate of reported sightings over the years (though at least half of the eventually reported sightings were not published contemporaneously).

The *London Times* apparently could not tolerate such a sitting-on-the-fence posture over several decades. Further detail is given in Table 3, of sightings and of items in the *Times* and in the *Herald,* by the month, during 1933 and 1934. The *Times* first mentioned on December 8, 1933, the excitement aroused over Loch Ness, and on the following day carried an editorial and a lengthy feature by Rupert Gould, who had visited the loch, interviewed eyewitnesses, and concluded that a sea-serpent had become trapped in Loch Ness. The editorial, though somewhat tongue-in-cheek, left open the possibility that Gould might be right. During the next month or so, the *Times* published three dozen letters and a dozen-and-a-half news items. But on January 18, 1934, an editorial virtually called a halt: "The creature . . . is clearly of a retiring disposition, with an abnormal, a truly monstrous, dislike of publicity . . . 'tis gone—gone rather stale by now, perhaps, and ere very long to be gone west." So the *Times* had decided that Nessie was a myth, and for the subsequent 50 years translated that conclusion into a decision not to report

events at or about the loch (with the already noted exceptions in 1938 and in 1975).

Monster buffs could thus be tempted to contrast the open-minded *Herald* with the dogmatic *Times.* But it might be more accurate simply to note that each paper determined quite early what its attitude ought to be, and then stuck with that decision over five decades—during which time a great deal has happened at the loch: enough to produce a striking increase in the rate of publication of books and magazine articles (Table 1) and a marked change in the attitude expressed in those articles (Table 5 and related text, below)—not enough, however, to alter the stance taken by the daily newpapers. I have been unable to locate general studies of daily newspapers to determine whether such rigidity is characteristic of their treatment of a wide range of topics or whether it occurs only in some areas. But at least one amusing instance of that rigidity on the present subject has been pointed to previously: in 1970, the *Oban Times* (west coast of Scotland) stated that it had refused to print anything about aquatic monsters since the hoax perpetrated in the west of Scotland in 1877! (Campbell, 1972, p. 102)

One should recognize that there was, in 1934, substantive reason for the *Times*—or for anybody—to dismiss the Loch Ness affair as spurious. Late in December 1933, an expedition supported by the *Daily Mail* found a spoor at the water's edge—which spoor the British Museum identified as originating from a hippopotamus' foot (an umbrella-stand souvenir had been used). And, just a few days later, another hoax involved alleged footprints and the remains of a monstrous meal (Whyte, 1957, pp. 103–107). So hoaxes were an undeniable part of the Loch Ness story from the very beginning of the furor of the 1930s.

At any rate, since January 18, 1934, the *London Times* has projected a disbelieving and joking attitude in its infrequent editorial comments (May 27, 1938, December 22, 1966, December 3, 1975, August 11, 1978, February 16, 1980). But it has not closed its pages entirely to the opposite view, carrying neutrally worded pieces in 1960 (by Peter Scott, August 14) and 1975 (December 6; interview with Tim Dinsdale) about important photographic results at Loch Ness, and an article by a believer in 1968 (October 2, by Elizabeth Montgomery); on the other hand, a very negative article was printed in 1975 (December 27, by Adrian Desmond). On balance, and unchanging over the years, the *Times* has printed letters without favoring any given viewpoint; though typically the tone of these letters has had some touch of dry humor—at the least, the characteristically English tone of not taking too seriously even something one happens to believe quite firmly: see, for example, *London Times,* December 20, 1933, from Edward Cadogan; December 22, 1933, from J. C. Squire; August 11, 1934, from J. Carter Rendell. The tone is also illustrated by the first editorial on the subject, "On the Track of a Monster," December 9, 1933. For examples on a wide variety of subjects, see Gregory (1976).

Although the newspapers displayed long memories for their decisions on how to approach the subject, they evidenced not much memory for specific

details. Now well known among monster buffs and mentioned in all the books beginning with Gould (1934) is the story of St. Columba encountering an aquatic beast at the river Ness about 565 A.D. The *London Times* first drew its readers' attention to that through a letter to the editor printed on December 13, 1933; but published another on May 2, 1934, making the same point as though for the first time; and yet another on August 11, 1934; again on July 7, 1960 (three years after Whyte had written at some length about it in her well-received and reviewed book); on May 12, 1972, an editorial comment said the same thing; and yet again a letter on December 12, 1975. Perhaps this merely illustrates a rather obvious feature of daily newspapers: they do not provide much background on any subject, and reporters rather often fail to consult even obvious works of reference before writing typically short pieces. Thus Berton (1977, p. 29) noted that a French journalist who visited the loch in 1973 to write about the monster remained unaware that any serious books had been published on the subject! And in 1967, the English national dailies had been unaware that the Loch Ness Investigation had existed for five years (Campbell, 1972, p. 74). As commonly said, one ought not to believe everything one reads in the papers; but perhaps especially not about anomalous topics.

For journalists as for others, it is not necessarily easy to locate reliably detailed works on such matters as Loch Ness. On many subjects, quite a good way of getting a rapid and reasonably authoritative synopsis and clues to further reading is to consult an encyclopedia. At least into the mid-1970s, that would not have helped much in the case of the Loch Ness monster. *Collier's Encyclopedia* does not mention it at all. In a number of other encyclopedias, one finds only between a sentence and a paragraph, usually referring only to the "flap" of 1933—in *Britannica* (1970–1973 editions checked), *Chamber's* (1967), *Columbia-Viking* (1968), *International* (1964–1978), *New Caxton* (1977), and *World Book* (1973–1977): rather surprising, perhaps, that the last had no more, since it sponsored work at Loch Ness in the late 1960s. Only the *American* (1973–1980) has a relatively accurate and informative entry (of only four paragraphs, however, with another mention under "Sea-Serpents").

That editorial policy and not events or reported events has been all-important in determining the coverage given to Loch Ness is further illustrated by comparing items in the *New York Times,* the regional *Louisville Courier-Journal,* and the local *Lexington Herald-Leader.* Between 1967 and 1975, each of these papers had between half-a-dozen and two dozen pieces that mentioned Loch Ness—but on only one occasion (1975–76) was the same story about Loch Ness featured in all three papers, concerning the aborted symposium in Edinburgh (*Times* (New York): May 14, 1967; September 24, 1970; April 25, 1971; January 16, April 2, November 3, 1972; August 18, 1973; February 3, 17, 24, and June 2, 1974; November 23, December 2, 5, 11, 14, 19, 1975; *Courier-Journal* (Louisville): March 12, May 7, 1967; February 4, 1968; October 5, 1969; May 7, 1972; September 16, December 5, 1973; November 10, 23, 24, 27, December 7, 1975; *Herald-Leader* (Lexington,

Kentucky): November 24, 1970; November 3, December 3, 1972; November 23, December 10, 1975; January 1, 1976).

Stories about Loch Ness have been notably inaccurate: Gould (1934, pp. 14, 15, 38, 39, 64, 65, 74, 83, 84) and Whyte (1957, pp. 2, 12, 73, 79) gave details of instances in which the newspaper accounts differed sharply from those provided to the newspapers by the eyewitnesses. At least one photograph was published by a newspaper (*Sunday Express,* September 7, 1969) as possibly showing the monster even though the paper had been advised by the Loch Ness Investigation (Annual Report, 1969, p. 4n) that the photograph was clearly of the wake from a boat. During 1975 and 1976 in particular, quite inaccurate impressions were conveyed about the credentials of the team from the Academy of Applied Science (Meredith, 1976, 1977).

The lack of consistency, coherence, and depth of background that is characteristic of newspaper coverage means that misleading impressions are conveyed even if only inadvertently through sins of omission rather than of commission. Many newspapers (for example, Louisville (KY) *Courier-Journal,* July 22, 1977) carried the story of the carcass dredged up in the South Pacific by a Japanese trawler, a carcass that looked most plesiosaur-like; but readers were then left hanging, never learning the results of the tests of tissue samples later carried out. Again, a local paper (Lexington (KY) *Herald-Leader,* January 29, 1976) carried the news that an improved sonar apparatus had obtained a "sonar photograph" of Nessie; and a couple of days later (February 2) an editorial commented on the chance that Nessie might, after all, be real. Actually that "sonar photo" was a hoax (*New Scientist,* February 12, 1976, p. 346), but readers of that particular newspaper were never so informed.

The very sparse coverage (Tables 1 and 2) given to Loch Ness by the national newspapers after the furor of 1933–34 no doubt conveyed the impression that sightings had virtually ceased. Westrum (1977, 1979) has pointed out this characteristic feature of anomalies: it is fallacious to conclude that a lack of reports signifies a lack of events, since the making—let alone the publishing—of reports is a function of the social demand for reports of that nature (mediated or interpreted, in the case of newspapers, by editorial policy, availability of other news items, fortuitous simultaneity of reports, for example). In the present instance, we have relevant information from Rupert Gould (1937) that the London press had stopped taking note of Loch Ness despite more than 20 sightings for each of the four years from 1933 to 1936 inclusive.

To summarize the coverage in newspapers before turning to the other literature:

1. The rate of coverage is clearly determined by editorial decision rather than by actual developments; though some events (as in 1938, 1975, 1976, see above) could seem so newsworthy as to overcome the general policy of ignoring Loch Ness.
2. The Scottish regional paper took a stance of reporting rather than judging. The *London Times* gave more space to letters and comment than to

neutral reporting, and displayed a penchant for dry humor and points made tongue-in-cheek. Both *London* and *New York Times* project disbelief (Table 4, below).

3. No change in attitude over 50 years is evidenced: the *London* and *New York Times* became inclined to jocular disbelief in January 1934, and they continue so; the *Glasgow Herald* continues to report rather than to comment (except that in 1976, when the *New York Times* sponsored an expedition, its coverage was great and uncharacteristically projected belief in the reality of Nessies, see Pollak (1976), Swan (1976), and Table 4).
4. Comparison of the stories covered by three different papers reveals an almost complete lack of agreement as to what is newsworthy: over almost a decade, only one story from Loch Ness was featured by all three.
5. In addition to conveying false impressions as a result of the capricious and sporadic nature of the coverage, newspapers have been guilty of considerable inaccuracy, in reporting what eyewitnesses have said, for example. It follows that readers of newspapers cannot gain an accurate and coherent impression of the events at Loch Ness.

Polarization of Belief

In Table 4 are shown the degrees to which attitudes of belief or disbelief are projected in the different types of writings on the subject. The last column of the table ("average score") indicates that the books, taken as a whole, project quite strong belief; the national newspapers, a tendency to disbelief. Articles in periodicals tend *on the average* to an expression of belief. Perhaps more significant than these averages, however, are the distributions. An average score of neutral (numerically zero) could result from 100% of the sample being neutral, or from 50% strong belief and 50% strong disbelief, or from an infinite number of distributions between those two extremes. The first extreme would correspond to a consensus that the question is open, the other extreme to a completely polarized controversy in which proponents and opponents

TABLE 4
Expressed attitude by type of literature[a]

	Strong Belief (+2)	Tending to Belief (+1)	Neutral	Tending to Disbelief (−1)	Strong Disbelief (−2)	Average Score
Books	60%	30%	5%	0%	5%	+1.5
Chapters in Books	45%	10%	20%	15%	15%	+0.6
Articles in Periodicals	35%	10%	25%	10%	20%	+0.3
London Times	0%	30%	25%	25%	20%	−0.3
New York Times, except 1976	0%	30%	20%	30%	15%	−0.3
New York Times, 1976	0%	70%	10%	20%	0%	+0.5

[a] Percentages were individually rounded to the nearest 5% to avoid implying too high a degree of validity; sums of the rows may therefore be 95, 100, or 105%.

are equally dogmatic and equally numerous. In the present instance, it appears that there is more polarization of opinion in the periodical literature than in books (which are skewed to belief) or in national newspapers (skewed toward disbelief). A closer examination of the periodical literature reveals other points of interest.

The Loch Ness Monster in Magazines

Thirty-five percent of all articles in magazines have purveyed a strong or utterly decided belief that the Loch Ness monsters are real animals, and another 10% have been inclined to that belief; 20% strongly or utterly of the opinion that it is a matter of myth, hoaxes, mistaken identifications, mirages, and so forth, and another 10% inclined to that view; only 25% of the articles have taken a neutral stance (Table 4). Such polarization of opinion is, of course, only to be expected on a subject that is, by common consent, controversial—no doubt the literature on other anomalies would be similar in that respect. More likely to be idiosyncratic is the actual distribution of belief, and even more so any changes over time in that distribution. (Elsewhere I have suggested (Bauer, 1979, 1987) that the polarization of belief among different groups would be of interest in categorizing controversies. In such socially and politically important technical controversies as those about nuclear power, missile defences, or the environment, I would expect distributions of opinion to be comparable among scientists, the media, and the population as a whole. On the other hand, regarding claimed anomalies of popular interest (Loch Ness, Velikovsky, faith healing, etc.), the distributions would differ drastically between technical experts and the population as a whole.)

In Table 5, attitudes expressed in magazines are shown for six eras, which were chosen after examining projected beliefs year by year and aggregating years over which no significant change was apparent. In the first period, 1933–

TABLE 5
Changes in attitude over time in magazines

	Number of Articles per Year	Percentages[a] of Articles Expressing: Strong Belief (+2)	Tending to Belief (+1)	Neutral (0)	Tending to Disbelief (−1)	Strong Disbelief (−2)	Average Score
1933–34	24	25%	10%	20%	10%	35%	−0.2
1935–54	1	15%	5%	45%	5%	30%	−0.3
1955–64	5	40%	10%	20%	10%	20%	+0.4
1965–69	8	30%	30%	30%	0%	10%	+0.6
1970–78	15	45%	10%	20%	20%	10%	+0.6
1979–83	11[b]	15%	10%	40%	10%	25%	−0.2

[a] Individual percentages rounded to nearest 5%; totals may be 95, 100, or 105.
[b] 1979–83 included only the first half of 1983; the whole period was taken as $4\frac{1}{2}$ years in calculating this rate.

34, there was much publicity; opinion was quite polarized—and, on the average, inclined to disbelief. During the next 20 years, little was written about Loch Ness; consistent with that, expressed views were more likely to be neutral—the subject was less a matter of controversy and opinion was less polarized. From the mid-1950s to date, the volume of writing has increased. The decade beginning in 1955 saw the weight of opinion shift from slightly disbelieving to slightly believing, but the polarization was as great as in the initial controversy of 1933–34. From 1965 into the late 1970s, strong disbelief was expressed more rarely than strong belief—in 1965–69, there was almost a consensual attitude of belief; in 1970–78, opinion was less consensual but still one of belief and not markedly polarized. Since 1979, the attitude has swung back to skeptical, with an appreciable polarization of strong believers and strong disbelievers.

One can find plausible externalist and internalist rationalizations for these changes. First I shall take an internalist perspective, which is at the same time the perspective of one who believes the Loch Ness phenomenon to be that of the existence of unidentified animals. Comments from an externalist perspective, which is at the same time that of a skeptic, follow in a separate section of this paper.

For about 20 years after the flap of 1933–34, there was no organized search for Nessie and little was written about the matter: naturally, then, opinion became less polarized but with no appreciable change in the average weight of opinion. But events were setting the stage for a surge of belief: sightings continued (Table 1) and slowly the accumulating evidence convinced more of the local population; for example, that prominent citizens admitted at a meeting of the Inverness County Council in 1947 that they had seen the monster will have carried some considerable conviction (Herries, 1947). Moreover, documentation of the evidence became satisfactory for the first time in the 1950s (apart from Gould's pioneering effort in 1933–34) as a result of Constance Whyte's work and writing: in 1951, Maurice Burton brought to public attention a pamphlet by Whyte published in Inverness, and in 1957 Whyte's full book appeared. That book provided historical perspective, many eyewitness reports, important photography, and a most judicious discussion of the improbabilities of the whole matter. It sparked new interest: the Loch Ness Phenomena Investigation Bureau was established in the early 1960s and it organized searches at the loch during the next 10 years; beginning in 1970, annual expeditions were mounted by the Academy of Applied Science (based initially in Massachusetts, latterly in New Hampshire). New evidence for the reality of Nessies came from sonar experiments; from Dinsdale's movie obtained in 1960 and the Royal Air Force examination of that film in 1965; from underwater photographs made by the Academy in 1972 and 1975 (for details of all those matters, see Mackal (1976) and Witchell (1974)). So the attitude of belief that emerged in the 1950s and grew stronger through the next two decades reflected accumulating evidence from publicly visible and successful research at Loch Ness. But no new results of importance have come

TABLE 6
Changes in attitude in British and in American magazines

	Number of Articles per Year	Percentages of Articles Expressing: Strong Belief (+2)	Tending to Belief (+1)	Neutral (0)	Tending to Disbelief (−1)	Strong Disbelief (−2)	Average Score
British							
1933–34	19	30%	10%	20%	10%	30%	< −0.1
1935–54	0.7	25%	10%	45%	10%	15%	+0.2
1955–64	3	40%	15%	10%	10%	30%	+0.3
1965–69	2	30%	10%	10%	0%	50%	−0.3
1970–78	5	45%	10%	20%	20%	10%	+0.7
1979–83	4	15%	5%	40%	0%	35%	−0.4
American							
1933–34	5	10%	0%	20%	20%	50%	−1.0
1935–54	0.4	0%	0%	40%	0%	60%	−1.1
1955–64	2	45%	0%	40%	5%	15%	+0.5
1965–69	6	30%	35%	35%	5%	0%	+0.9
1970–78	9	45%	10%	20%	15%	10%	+0.6
1979–83	7	15%	10%	40%	15%	20%	−0.1

in since 1975; hence the weakening of belief over the last few years, with a distribution of attitudes quite similar to that of the earlier "quiet" years, 1935–54.

This analysis can be sharpened by looking separately at magazines published in Britain and in the U.S.A. (Table 6): the overall change from somewhat disbelieving initially, to somewhat believing from the mid-1950s to the late 1970s, and back to disbelief in recent years remains valid, but can be explained in more detail.

Note first that the literature was predominantly British in the first two periods of time but has been predominantly American since the 1960s; as already indicated in Table 1, 80% of articles about Loch Ness were published in British magazines in the 1930s and 1940s, but from the mid-1960s the proportion has been only between 25 and 40%. Second, expressed attitudes have been stronger in American than in British publications. The American articles were strongly negative up to the mid-1950s, whereas the British ones were mildly positive on the average; the American range of opinion was skewed to disbelief, whereas the British distribution was very polarized between proponents and opponents and neutral observers. That seems natural for a phenomenon geographically located in Britain, generating more controversy and writing there than across the Atlantic, where it could readily be dismissed as just another sea-serpent flap.

During 1955 to 1964, the American and British literatures were much more similar, but there was a big difference in the late 1960s. In Britain, the Loch Ness Investigation was at work, but was not given much publicity

(Campbell, 1972, p. 74); at the same time in America, Roy Mackal was enlisting moral and financial support from the Adventurers Club of Chicago, the Explorers Club, and notably from Field Enterprises of Chicago, proprietors of newspapers and of World Book Encylopedia—and Field Enterprises used its public-relations apparatus to generate much favorable publicity. So British periodicals expressed a predominantly skeptical attitude, sharply polarized between proponents and opponents, whereas American publications were virtually consensual, strongly skewed toward belief.

Since 1970, the literatures have again been quite similar. The important work has been done by the Academy of Applied Science, covered heavily in the U.S.A. because it is based there. So more is published in American than in British periodicals, but what is written shows no significant differences.

The literature can also be subdivided other than geographically: for example, by the nature of the magazine—scientific or general-interest; and for each such division, one might look for changes in attitude over time. A rigorously multivariate analysis does not seem appropriate, however, for a sample of the present size, the number of variables of potential interest, and the very simple method of coding used. Consequently some other aspects of the literature are examined in the following only for major possible correlations, with the sample subdivided in each case only by the variables among which correlations were sought. Consequently also, interpretations will focus only on obviously significant differences.

The periodicals were classified by categories between which significant differences would be expected: scientific (e.g., *Nature, Science, Limnology and Oceanography*); popular science (e.g., *Wildlife, National Geographic, Popular Mechanics, New Scientist*); general interest (e.g., *Time, Harper's, Field, Maclean's*); specialist (e.g., *Skin Diver, Elks Magazine, University of Chicago Magazine*); and specializing in anomalies (e.g., *Fate, Flying Saucer Review, Fortean Times*). The composition of the literature in these terms is shown in Table 7. Most worth noting, perhaps, is that only one quarter of the literature represents periodicals specializing in anomalies: the literature is predominantly in very respectable publications. Moreover, that was particularly the case dur-

TABLE 7
Proportion of articles in periodicals of different sorts
(percentages, rounded to nearest five percent)

	General Interest	Popular Science	Scientific	Specialist	Specializing in Anomalies	(Total Number of Articles)
1933–34	75	5	10	10	0	(47)
1935–54	55	5	0	5	35	(20)
1955–64	45	30	0	5	20	(54)
1965–69	15	20	10	15	45	(40)
1970–78	25	15	15	20	25	(134)
1979–83	15	50	5	15	20	(51)
Overall	35	20	10	15	25	(346)

TABLE 8
Attitudes projected in different sorts of periodical

	(Total Numbers of Articles)	Percentages of Articles Expressing: Strong Belief (+2)	Tending to Belief (+1)	Neutral (0)	Tending to Disbelief (−1)	Strong Disbelief (−2)	Average Score
Specializing in anomalies	(79)	40%	10%	35%	15%	5%	+0.7
Specialist	(48)	40%	10%	25%	5%	20%	+0.4
General interest	(115)	35%	15%	25%	10%	20%	+0.3
Popular science	(73)	30%	5%	20%	10%	30%	−0.1
Science	(30)	15%	20%	30%	10%	25%	0.0

ing the initial controversy in 1933–34. In the "quiet" years 1935–54, a larger proportion of the writing was in anomalistic journals, but still it was largely general-interest magazines that kept the matter in public view. Since the mid-1960s, coverage in journals of science and of popular science has equalled or exceeded that in periodicals of anomalies. This is more what one would expect of a topic of substantive scientific or technological interest than of such a typical pseudo-science as astrology, for example. Unfortunately, similar analyses of other subjects are not available, however.

Examination of the expressed attitude by type of periodical shows what one might expect (Table 8): the strongest degree of belief in periodicals concentrating on anomalous claims and phenomena, weakest belief in journals of science and popular science. The degree of polarization of expressed opinion seems not to differ very much between these categories, however; and indeed the variation of average expressed belief is less than one would find with many anomalous or occult subjects, where magazines specializing in anomalies would score at least as high in belief, but scientific journals—if they mentioned the subjects at all—would be strongly negative.

The analysis revealed no great differences among these types of periodicals in the degree of jocularity, as opposed to seriousness, with which the subject has been treated: about one article in five indulged in humor to some degree. Nor was there any marked difference in reliability: one article in four contains errors varying from minor to gross. There is a difference, however, in the proportion of lengthy articles in the various classes of journals: articles of more than two pages represented about two thirds of all the writing in the specialist periodicals, only one third to one quarter of those in general-interest and popular-science magazines, and only about one sixth of those in scientific journals. Those differences are hardly an occasion for surprise, however. A possible interpretation is that the author and type of article is more significant than the publication in which it appears. One who takes such a subject as Nessie seriously and treats it objectively may not be able to get published in a journal of science and may eventually turn to the periodicals specializing

TABLE 9
Disbelief and jocularity

	Completely Serious (0)	Slightly Jocular (0.5)	Jocular (1)	Utterly Jocular (2)	(Numbers of Articles)	Average Score on Jocularity
Strong belief	96%	2%	1%	1%	(116)	0.03
Leaning to belief	91%	6%	3%	0%	(36)	0.06
Neutral	81%	12%	6%	1%	(90)	0.14
Leaning to disbelief	48%	48%	4%	0%	(40)	0.29
Strong disbelief	63%	8%	19%	10%	(63)	0.42

in anomalies. The latter magazines, whose general level of reliability is not high, nevertheless publish a proportion of quite reliable, even well-documented pieces.

The tendency to jocularity was most marked in the most disbelieving writings; and positive belief was expressed more frequently in the longer articles than in the shorter ones (Tables 9, 10). (Though the "average" degree of jocularity increased as belief changed to disbelief, more of the strongly negative pieces were completely serious than were the tentatively negative ones. Perhaps this reflects the typical humorlessness of the strident disbeliever.)

Perhaps less to be expected was the greater frequency of clear errors of fact in the articles that expressed the most negative view of the Loch Ness phenomenon (Table 11). Writings by the believers are remarkably free of *gross* errors, though 20% have minor mistakes about such things as dates, type of photographic apparatus, and so forth. Neutrally written articles are about equally reliable; but about one third of the "debunking" articles contain quite serious errors on points of some importance.

Two possible explanations come readily to mind. From the viewpoint of a believer, the obvious explanation is that the debunkers are not familiar with the evidence—if they were, they would not remain disbelievers! That argument is consistent with the few data available about the distribution of belief among

TABLE 10
Length of article and attitude

	Strong Belief (+2)	Tend to Belief (+1)	Neutral (0)	Tend to Disbelief (−1)	Strong Disbelief (−2)	(Number of Articles)	Average Score
One paragraph or less	10%	5%	55%	15%	15%	(33)	−0.3
Between one paragraph and two pages	25%	15%	25%	15%	25%	(220)	0.0
More than two pages	65%	10%	15%	5%	10%	(92)	1.1

TABLE 11
Correlation between expressed disbelief and errors of fact

Expressed Attitude	Percentages of Articles With: No Significant Errors	Minor Errors	Definite Errors	Totally Erroneous Presentation	(Numbers of Articles)
Strong belief	77%	20%	2%	1%	(116)
Inclined to belief	78%	14%	—	8%	(36)
Neutral	88%	2%	6%	4%	(90)
Inclined to disbelief	69%	15%	13%	3%	(40)
Strong or utter disbelief	55%	16%	19%	10%	(63)

various groups. The population as a whole has learned about Loch Ness primarily from the newspapers, which have never presented a coherent or fully documented account, and only some 13% of Americans have come to believe in the reality of Nessies; on the other hand, nearly 40% of marine biologists give credence to the evidence that Nessies are real, presumably through reading some of the available books and articles in reputable periodicals (Greenwell, 1980; Greenwell & King, 1980). A second possible explanation, not mutually exclusive with the first, is that the debunkers are simply careless with the facts—because they regard their case as so self-evidently overwhelming, for example; there is clear evidence in the literature about anomalies that some debunkers do adopt that stance. For example, in the Velikovsky affair (Bauer, 1984), the astronomer McLaughlin admitted that he had not read Velikovsky's book (Juergens, 1966); other astronomers were very careless in argumentation (Knight, 1970, p. 136); some critics said that it was not worth the time and effort to make a fully reasoned response (Gardner, 1950; Lafleur, 1951; Payne-Gaposchkin, 1952). Another example concerns arguments over statistical data relating to planetary positions: members of the Committee for the Scientific Investigation of Claims of the Paranormal behaved most unethically as well as carelessly—falsifying results and attempting to cover up errors (Rawlins, 1981; *Skeptical Inquirer,* Winter 1981–82, vol. VI, no. 2, pp. 58–67).

Externalist Considerations

The change from projected disbelief that began in the 1950s is consistent with Whyte's publishing in 1957 of an excellent book written from a believer's perspective, and the subsequent events as outlined above. But it may also be that this change in belief resulted at least in part from some general openness on the part of society to recondite or unorthodox possibilities. Martin Gardner's collection of what he termed modern pseudo-science was published in 1952; and several of those subjects were particularly public matters during the 1950s—UFOs, dianetics, Bridey Murphy, Velikovsky, Lysenko. And it

was in 1951 that Shipton photographed the spoor of a supposed yeti, and three years later that the *Daily Mail* organized an expedition to search the Himalayas for these creatures (Sanderson, 1977, pp. 54–55). The Loch Ness monster was by no means unique among anomalies in attaining a measure of respectability in the 1950s (Bauer, 1986–87; Dutch, 1986).

Conclusions: The Literature and the Public Image

Books on the subject overwhelmingly treat Nessies as real. Magazine articles have been preponderantly positive for much of the last 25 years. But for the newspapers, Nessie remains a compound of myth, hoax, gullibility—and that has not changed over five decades. The public image corresponds to the last view. Somewhat similar conclusions emerged from a study of the literature dealing with UFOs (Strentz, 1970): newspapers were a more common source of information than were magazines, by a factor of 2.5; coverage was of "breaking" stories, and lacked depth and background; local papers gave relatively continuous coverage whereas national papers were sporadic in their attention.

It is understandable, of course, that newspapers are more influential persuaders of the public than are magazines or books. The papers are published daily, and reach a very large proportion of the population; the sum of the circulations of all daily papers exceeds by an order of magnitude even those of such magazines as *Reader's Digest* and *Time,* which have circulations in the millions (but appear only once a week or once a month). The numbers of books sold is miniscule in comparison. I asked the publishers for all books (Bauer, 1980, 1982) specifically dealing with Nessie for information about sales: for those two dozen volumes, the *total* of sales over the years has been between a quarter and half a million copies. Of course, each copy of a book has more than one reader; but it is not unreasonable to guess that the number of people who have read even one book about Nessie is comparable to the number of people who read a *single issue* of *Reader's Digest.* It is not irrelevant here to recall that three out of every five American adults have never read completely a book other than a textbook or the Bible (Gallup poll no. 47, May 1969).

Perhaps a little more surprising is the finding that the newspapers have hardly altered their editorial stances over 50 years, during which time there has been a marked shift of opinion expressed in serious writings on the subject in respectable journals. Yet that too is understandable. Newspapers are primarily concerned with current events; reporters have little time for research on the background of a story; and since there are no established experts or authorities on such matters as Nessie, the attempt by a reporter to set the matter in context could easily come to nought (see above concerning encyclopedias; and Berton (1977, p. 297) and Campbell (1972, p. 74)). By contrast, in recognized scientific specialities, reporters have access to authoritative information by making contact with publicly established experts whose knowledge remains up-to-date.

The nature of the literature about the Loch Ness monster is at variance with the public image which associates it with pseudo-science. Almost all the books on the subject come from reputable publishers, in contrast to the frequent appearance of obvious pseudo-science as self-publication or from houses established solely to publish about that particular claim or phenomenon (thus Kronos Press for Velikovskians; Pacific Meridian Publishing Company for Pattenist catastrophists, for instance). Articles about Nessie have appeared chiefly in the well-established general periodicals as well as in journals of science and of popular science; less than one quarter of the articles is in *Fortean Times, Fate, INFO, Pursuit,* and the like. In contrast, pseudo-science appears more in the latter and in journals established by followers of the particular cult, less frequently in the reputable general-interest periodicals, and hardly at all in journals of science. (But note that, as earlier mentioned, some reliable articles appear in some of the fringe periodicals; and they are also an irreplaceable source of information about anomalies.)

Not only is the nature of the literature about Loch Ness thus at variance with the public image, but its content is also: in that errors are much more common in the writings that deny than in those that affirm the evidence; and in that the evidence is given considerable credence by 40% of marine biologists, experts whose field is directly relevant (Greenwell & King, 1980). The present status of the matter makes it not only an interesting example of anomalies, but also one that can illuminate the problem of identifying demarcation criteria for what may be classed as science and what as pseudo-science.

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An Analysis of the Condon Report on the Colorado UFO Project

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Abstract—The "Condon Report," presenting the findings of the Colorado Project on a scientific study of unidentified flying objects, has been and remains the most influential public document concerning the scientific status of this problem. Hence, all current scientific work on the UFO problem must make reference to the Condon Report. For this reason, it remains important to understand the contents of this report, the work on which the report is based, and the relationship of the "Summary of the Study" and "Conclusions and Recommendations" to the body of the report. The present analysis of this report contains an overview, an analysis of evidence by categories, and a discussion of scientific methodology. The overview shows that most case studies were conducted by junior staff; the senior staff took little part, and the director took no part, in these investigations. The analysis of evidence by categories shows that there are substantial and significant differences between the findings of the project staff and those that the director attributes to the project. Although both the director and the staff are cautious in stating conclusions, the staff tend to emphasize challenging cases and unanswered questions, whereas the director emphasizes the difficulty of further study and the probability that there is no scientific knowledge to be gained.

Concerning methodology, it appears that the project was unable to identify current challenging cases that warranted truly exhaustive investigation. Nor did the project develop a uniform and systematic procedure for cataloging the large number of older cases with which they were provided. In drawing conclusions from the study of such a problem, the nature and scope of which are fraught with so much uncertainty, it would have been prudent to avoid theory-dependent arguments.

Introduction

The "UFO phenomenon," which is here taken to comprise those events that lead to reports of "unidentified flying objects," is of widespread public interest but elicits comparatively little interest from most scientists, who—to

An early version of this article was read by a number of people, some of whom were kind enough to send me their comments on that version. I acknowledge with gratitude comments received from T. Bloecher, S. J. Colby, H. R. Crane, D. M. Dennison, E. R. Hilgard, R. J. Low, H. M. Johnson, H. Mark, D. R. Saunders, F. E. Roach and O. G. Villard. A draft of the final version was read most carefully by Associate Editor David Jacobs and by an anonymous referee, both of whom made valuable suggestions that resulted in notable improvements in the article. However, there may well remain errors of fact or of perception, and I hope that readers will draw my attention to such further errors as they may detect.

judge from their public statements—perceive the study of this phenomenon not only as unproductive but also as not respectable. An editorial in *Science* (Abelson, 1974) refers to unidentified flying objects in a discussion of "pseudoscience," which is termed an "intellectual poison." On the other hand, a survey of members of the American Astronomical Society, which permitted members to express opinions under the cloak of anonymity, indicates that scientists are probably more interested in and more open-minded towards this subject than one would judge from their public statements (Sturrock, 1977).

The history of the UFO phenomenon in the United States is long and complex. Jacobs (1975) has given a comprehensive account of this history up to 1973 in his book *UFO Controversy in America.* This book presents a detailed account of the origin of the Colorado UFO Project, of which the following is a very brief encapsulation.

The United States Air Force carried out three consecutive studies of the UFO phenomenon over a 22-year period: Project Sign, from 1947 to 1948; Project Grudge from 1948 to 1952; and Project Blue Book from 1952 to 1969. Although these studies and their reports were initially classified, it appears that all reports (except Blue Book Special Report No. 13, if it ever existed) have now been declassified and are publicly available. The Air Force organized an "Ad Hoc Committee to Review Project Blue Book," and this committee met in February 1966. Its members were Brian O'Brien (chairman), Launor Carter, Jesse Orlansky, Richard Porter, Carl Sagan, and Willis A. Ware. This committee recommended that the Air Force negotiate contracts "with a few selected universities to provide selected teams to investigate promptly and in depth certain selected sightings of UFOs." This recommendation led eventually (in October 1966) to an Air Force contract to the University of Colorado. The director was Professor Edward U. Condon, a very distinguished physicist and a man of strong and independent character.

Work on this contract was carried out over a two-year period with a substantial scientific staff. Since this study is the only unclassified investigation[1] of the UFO phenomenon carried out by an established scientific organization under contract to a U.S. federal agency, the report of this study (Condon & Gillmor 1968; usually referred to as the "Condon Report") constitutes a landmark in the study of the UFO phenomenon, to which all later work must be referred. For instance, any review of the UFO phenomenon to be published in one of the mainstream scientific journals must begin with a discussion of the Condon Report (CR) explaining where and why the author disagrees with the findings of that report. Even more important, any proposal to the Air Force or any other federal agency, requesting funds for UFO research, must begin by explaining why the Condon Report is not to be accepted as the last word on the problem.

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[1] But see the "Postscript" section of this present analysis.

There has, in fact, been considerable development in UFO research since the Colorado Project: at that time, APRO (Aerial Phenomena Research Organization) and NICAP (National Investigations Committee for Aerial Phenomena) were in existence. Since that time, MUFON (Mutual UFO Network) has emerged as an even larger organization for UFO research; CUFOS (Center for UFO Studies) has been founded by J. Allen Hynek; and CNES (Centre Nationale d'Études Spatiale, the French equivalent of NASA) has set up a small group (GEPAN: Group d'Étude des Phénomèmes Aérospatiaux Non-Identifiés) with the charge of studying UFO reports, most of which are channeled to GEPAN by the gendarmerie according to a well defined and well functioning procedure.

Two studies that were initially classified but have since been declassified deserve special mention. One of these was conducted by a panel comprising Luis Alvarez, Lloyd Berkner, Samuel A. Goudsmit, Thornton Page, and H. P. Robertson (chairman), with Frederic C. Durant and J. Allen Hynek serving as associate members. This panel was convened by the Central Intelligence Agency for a period of five days in 1953 to consider the question whether UFOs constitute a threat to national defense. The panel concluded that there was "no evidence that the phenomena indicate a need for the revision of current scientific concepts" and that "the evidence . . . shows no indication that these phenomena constitute a direct physical threat to national security" (Jacobs, 1975).

The other study was conducted by the Battelle Memorial Institute, under contract to the Air Force, from 1951 to 1953. It was primarily a statistical analysis of the conditions and characteristics of UFO reports, but it also included transcripts of several notable sightings. The report of this study (Blue Book Special Report No. 14), which was initially classified but subsequently released, contains a wealth of information and arrives at the notable conclusion that the more complete the data and the better the report, the more likely it was that the report would *remain* unidentified (Jacobs, 1975).

The Condon Report is not a *committee* report: it is not a document signed by a number of scientists, each making his own appraisal but coming—as a group—to a common position and recommendation. It is a *project* report, containing contributions from the scientific staff and an overview by the project director. This fact is crucial and helps one to understand the contents of the report.

Section[2] CR I and CR II, the "Conclusions and Recommendations" and the "Summary of the Study," are written by Condon himself. Condon's summary is followed by six summaries of different aspects of the research, written by staff members, together with a summary of opinion polls conducted by the American Institute of Public Opinion, more familiarly known as the Gallup

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[2] Sections of the Condon Report are referred to as "Section CR I," etc., to distinguish them from sections of the present article. Page references to the Condon Report are denoted by the prefix "CR."

Poll. The staff summaries are followed by 240 pages of case studies. The entire report, with supplementary and peripheral material, is almost 1,000 pages in length.

The general impression given by Condon's summary is that there is nothing unusual or significant in the UFO phenomenon. This view gains significant additional weight from the fact that the Condon Report was reviewed by a panel of eminent scientists of the National Academy of Sciences who endorsed both the methodology and findings of the report (Condon & Gillmor, 1968, pp. vii–ix). We shall consider the NAS Panel Report only briefly in Section V.

The attitudes of scientists towards the UFO problem will be discussed in Section II. An overview of the Condon Report then follows in Section III. In Section IV, we compare Condon's "Summary of the Study" with the six staff summaries, and then proceed to compare each staff summary with the case summaries on which it was based. Section V is devoted to a discussion of scientific methodology, and Section VI is given to a discussion of the present analysis. At the time of revising this article in accordance with the referee's report, I have taken the opportunity to add a short postscript based on material released by the Central Intelligence Agency after this analysis was first prepared.

Some readers may be interested in reading other reviews of the Condon Report. Soon after the Report was published, *Icarus* carried two reviews, one by McDonald (1969) and the other by Chiu (1969). Hynek (1972) and Jacobs (1975), in their books on the UFO problem, each devote a chapter to the Condon Report.

Scientists and the UFO Phenomenon

Although, as indicated in Section I, the scientific community has tended to minimize the significance of the UFO phenomenon, certain individual scientists have argued that the phenomenon is both real and significant. Such views have been presented in the Hearings of the House Committee on Science and Astronautics (Roush, 1968) and in the book by Hynek (1972). It is also notable that one major national scientific society, the American Institute of Aeronautics and Astronautics, set up a subcommittee in 1967 to "gain a fresh and objective perspective on the UFO phenomenon." This subcommittee published a position statement (Kuettner, 1970) and sponsored the publication of analyses of two UFO cases (McDonald, 1971; Thayer, 1971), each of which was considered also by the Condon team. The AIAA versions of these cases are more detailed than those found in the Condon Report and are clearly based on more extensive data.

In their public statements (but not necessarily in their private statements; see Sturrock, 1978), scientists express a generally negative attitude towards the UFO problem, and it is interesting to try to understand this attitude. Most scientists have never had the occasion to confront evidence concerning the UFO phenomenon. To a scientist, the main source of hard information (other than his own experiments or observations) is provided by the scientific journals.

With rare exceptions, scientific journals do not publish reports of UFO observations. The decision not to publish is made by the editor acting on the advice of reviewers. This process is self-reinforcing: the apparent lack of data confirms the view that there is nothing to the UFO phenomenon, and this view works against the presentation of relevant data. If a bizarre phenomenon is reported—sometimes in colorful and emotional terms—in the popular press, and if sober accounts of the same phenomenon are never documented in scientific journals, scientists may understandably come to believe that the reports are spurious: at best, misperceptions of familiar objects and phenomena, and at worst deliberate hoaxes perpetrated on an uncritical public.

Any scientist who spends a small amount of time investigating the subject will soon realize that many of the simpler and more credible reports can, indeed, be interpreted as mirages, weather balloons, and other familiar natural phenomena and technological devices. Further study may turn up dramatic reports that, if they are to be believed, indicate that the earth is being visited by members of a very advanced civilization, which is therefore *presumed* to be alien and extraterrestrial, traveling in craft that behave in a fantastic manner. When faced with such a possibility, the scientist tends to look at the implications of such a hypothesis. In making his deductions, he has available a great store of information concerning the solar system, the universe, laws of physics, and conditions in which living organisms can survive. Using this information, the scientist may well conclude that the hypothesis must be rejected.

An example of this type of argument is advanced by Condon himself (Condon & Gillmor, 1968, p. 28). Starting from the assertion that an alien civilization must originate on a planet of the sun or some other star, Condon argues that a civilization based on a planet attached to a nearby star would not set out on a journey to earth until the civilization knows that an advanced technology has been established here. From this consideration he estimates that there is no possibility of such a civilization visiting earth in the next 10,000 years. Concerning the solar system, Condon takes the view that only Venus and Mars might provide possible abodes for life and argues that our knowledge of these planets provides no evidence for the existence of advanced civilizations on these planets.

It appears, therefore, that the difference in attitude toward the UFO phenomenon on the part of scientists and members of the public may to some extent be understood in terms of the stricter demands of evidence and proof required by the former, and in part by the large amount of information available to the former that tends to argue for interpretation in familiar terms and against explanation in terms of alien civilizations. However, there may well be other factors influencing scientists' attitudes, such as the fear of ridicule.

Further comments may be advanced concerning the attitudes of particular groups of scientists. For instance, physicists appear to attach importance to single conclusive cases: the evolution of physics is marked by such milestones as Thomson's demonstration of the particle nature of cathode rays, Davisson and Germer's demonstration of the wave nature of the electron, etc. This

attitude is, in fact, capsulated by Condon's hypothetical case that would convince all scientists that UFOs are spacecraft from an alien civilization (Condon & Gillmor, 1968, p. 26):

> The question of ETA (Extra-Terrestrial Actuality) would be settled in a few minutes if a flying saucer were to land on the lawn of the hotel where a convention of the American Physical Society was in progress and its occupants were to emerge and present a special paper to the assembled physicists, revealing where they came from and the technology of how their craft operates. Searching questions from the audience would follow.

By contrast, information concerning astronomical phenomena is typically accumulated more laboriously. Furthermore, the picture emerging from astronomical data may for many years remain inconclusive and perhaps contradictory. When one also notes that astronomical observations represent a passive activity, essentially different from the design and operation of experiments, one might conclude that study of the UFO phenomenon bears more similarity to astronomical research than to laboratory studies in the physical sciences.

The above contrast between the attitudes of physicists and astronomers has been overdrawn in order to emphasize a point. Quantum mechanics emerged from many years of patient and unspectacular studies of atomic spectra, and a very short run of radio observations were sufficient to establish the existence of a new class of astronomical objects now called "radio pulsars" (Hewish, Bell, Pilkington, Scott, & Collins, 1968).

Overview

The Condon Report, presenting the results of the Colorado Project on a Scientific Study of Unidentified Flying Objects, does not give the impression of a tightly integrated research program. The total budget over a two-year period was $500,000, but the report lists 37 members of the staff project and a number of other individuals were consulted in addition. It is clear that the Air Force was receiving a very high return of scientific manpower for its money, even though most of the staff must have been contributing only a small fraction of their time to the project. One would have expected that such a large research effort would have been organized into teams led by the other principal investigators or by members of the full-time staff, but there is no indication that such a structure was set up.

Professor Condon is listed as the "Director" of the project. The following are listed as "Principal Investigators": Stuart W. Cook, Professor of Psychology; Franklin E. Roach, Professor of Astrogeophysics; and David R. Saunders, Professor of Psychology; in addition, William A. Scott, Professor of Psychology, is listed as "Co-Principal Investigator"; all were at the University of Colorado. Mr. Robert J. Low, with degrees in Electrical Engineering and Business Administration, was the "Project Coordinator." In addition, there were five "Research Associates": Norman E. Levine (PhD, Engineering), Ronald I. Presnell

(MS, Engineering), Gerald M. Rothberg (PhD, Physics), Herbert J. Strentz (MA, Journalism), and James E. Wadsworth (BA, Behavioral Science).

The hard core of the report is Section CR IV, which presents 59 cases. In this work, the Director took no part; one Principal Investigator worked on two cases, another Principal Investigator on one case; the Co-Principal Investigator took no part; the Project Coordinator worked on eight cases; one Research Associate (Dr. Levine) worked on eight cases; Dr. Rothberg on one case; and Mr. Wadsworth on 17 cases. Important contributions to case studies were made by Roy Craig (PhD, Physical Chemistry) and William K. Hartmann (PhD, Astronomy), who are listed simply as "staff members." Craig and Hartmann each worked on 14 cases.

The next most important section is Section CR III, which presents six summaries of the work of the Colorado Project, together with a review of opinion polls by Aldora Lee (PhD, Social Psychology). None was written by the Director, one by a Principal Investigator (Roach), none by the Research Associates. Three chapters were written by Craig, one by Hartmann, and one by Gordon Thayer (BS, Physics).

Section CR V, dealing with historical aspects of UFO phenomena, comprises three chapters, and Section CR VI, dealing with "The Scientific Context," comprises 10 chapters. Of these 13 chapters, one was written by the Director. The remaining 12 chapters were written by staff members not previously listed in this discussion.

Concerning Sections CR III to V, it is seen that a substantial contribution was made by one Principal Investigator (Roach) and by the Project Coordinator. The remainder of the staff made no contribution to the Report or only specialized contributions. Section CR I, "Conclusions and Recommendations," and Section CR II, "Summary of the Study," were written by the Director. This breakdown is summarized in Table 1.

Another important part of any scientific study is the definition of the scope of the study and definitions of the principal terms involved. Condon states (Condon & Gillmor, 1968, p. 1) that "The emphasis of the study has been on attempting to learn from UFO reports anything that could be considered as adding to scientific knowledge." His conclusion (Condon & Gillmor, 1968, p. 1) was that "nothing has come from the study of UFOs in the last twenty years that has added to scientific knowledge. . . . Further extensive study of UFOs probably cannot be justified in the expectation that science will be advanced thereby."

The key definition is given by Condon as follows:

> An unidentified flying object (UFO, pronounced OO-FO[3]) is here defined as the stimulus for a report made by one or more individuals of something seen in the sky (or an object thought to be capable of flight but seen when landed on the earth) which the observer could not identify as having an ordinary natural original, which seemed to him sufficiently puzzling that he undertook to make a report of it to the police, to

[3] The pronunciation originated with Condon and appears to have been used only by him.

TABLE 1
Breakdown of activities among staff

		Condon report			
		Sec. IV 59 cases	Sec. III 7 summaries	Sec. V, VI 13 chapters	Sec. I, II
Condon	Director	0	0	1	2
Cook	Principal Investigator	1	0	0	0
Roach	Principal Investigator	2	1	0	0
Scott	Co-Principal Investigator	0	0	0	0
Low	Project Coordinator	8	0	0	0
Levine	Research Associate	8	0	0	0
Presnell	Research Associate	0	0	0	0
Rothberg	Research Associate	1	0	0	0
Strentz	Research Associate	0	0	0	0
Wadsworth	Research Associate	17	0	0	0
Craig	"Staff"	13	3	0	0
Hartmann	"Staff"	14	1	0	0
Lee	"Staff"	0	1	0	0
Thayer	"Staff"	0	1	0	0
Others		⩾30	0	12	0

govenment officials, to the press, or perhaps to a representative of a private organization devoted to the study of such objects. Defined in this way, there is no question as to the existence of UFOs because UFO reports exist in very large numbers, and the stimulus for each report is, by this definition, an UFO. The problem then becomes that of learning to recognize the various kinds of stimuli that give rise to UFO reports.

Most scientists who study UFOs adopt a more restricted definition that rules out reports that are readily explainable (see, for instance, Hynek, 1972, pp. 3, 4). Furthermore, some members of the project staff must have adopted a different definition of "UFO" since one finds on Condon and Gillmor (1968) p. 248 the statement "The preponderance of evidence indicates the possibility of a genuine UFO in this case" and on Condon and Gillmor (1968) p. 256, "The probability of at least one UFO involved appears to be fairly high."

In most scientific research, investigators have in mind one or more considered hypotheses. Condon specifically mentions the following:

> The idea that some UFOs *may* be spacecraft sent to Earth from another civilization, residing on another planet of the solar system, or on a planet associated with a more distant star than the Sun, is called the Extra-terrestrial Hypothesis (ETH).

It is somewhat confusing that Condon also introduces the term "Extra-terrestrial Actuality" (ETA), which apparently represents the belief that ETH is true. Condon's finding (Condon & Gillmor, 1968, p. 25) is that "No direct evidence whatever of a convincing nature now exists for the claim that any UFOs represent spacecraft visiting Earth from another civilization." In reach-

ing this conclusion, Condon takes the position (Condon & Gillmor, 1968, p. 19) that "If an UFO report can be plausibly explained in ordinary terms, then we accept that explanation, even though not enough evidence may be available to prove it beyond all doubt."

In assessing the basis for Condon's conclusion, we may refer to the staff summaries that comprise Section CR III; the staff summaries are based, in turn, on the case studies. In the next section, we shall consider the evidence as categorized in the staff summaries, referring to specific cases as seems appropriate. In the remainder of this section, we shall categorize cases according to the conclusions drawn by the project staff.

In addition to three observations by astronauts (which will be discussed separately in Section IV), 59 cases are listed in Section CR IV. One of these (Case 14) involves six separate events. Another (Case 38) is a discussion of "over 800 sightings of UFOs." The appraisals were as follows:

1. No event; one case (19).
2. Inconsistent data, possible hoaxes, or otherwise of no probative value; 17 cases (4, 7, 14.4, 22, 23, 24, 26, 32, 33, 39, 42, 44, 48, 52, 53, 56, 58).
3. Identified (anything from "conclusively" to "inconclusively"); 25 cases (3, 9, 11, 14.3, 14.6, 15, 18, 20, 25, 27, 28, 29, 35, 36, 37, 40, 41, 43, 45, 49, 50, 51, 54, 55).
4. Not identified; 14 cases (1, 5, 6, 8, 10, 12, 13, 14.2, 17, 21, 32, 34, 47, 59).
5. Delusions; 2 cases (16, 38).
6. Not appraised due to evasion by Air Force; 1 case (30).[4]
7. Possible UFOs; 2 cases (14.1, 57).
8. Probable UFOs; 2 cases (2, 46).

Evaluation of Evidence by Category

We now consider evidence by category, drawing from both Section CR III (staff summaries) and Section CR IV (case studies) of the report.

Narrative Evidence

Craig states (Condon & Gillmor, 1968, pp. 72, 73):

> While the current cases investigated did not yield impressive residual evidence, even in the narrative content, to support an hypothesis that an alien vehicle was physically present, narratives of past events, such as the 1966 incident at Beverly, Mass. (Case 6), would fit no other explanation if the testimony of witnesses is taken at face value.

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[4] This case is discussed further in the "Postscript."

Case 6 is described on Condon and Gillmor (1968) pp. 266–270. The abstract of this case (Condon & Gillmor, 1968) p. 266 is as follows:

> Three adult women went onto the high school athletic field to check the identity of a bright light which had frightened an 11-year-old girl in her home nearby, and reported that one of three lights they saw maneuvering in the sky above the school flew noiselessly toward them, coming directly overhead, 20–30 ft. above one of them. It was described as a flowing [*sic*], solid disc-like, automobile-sized object. Two policemen who responded to a telephone message that a UFO was under observation verified that an extraordinary object was flying over the high school. The object has not been identified. Most of the extended observation, however, apparently was an observation of the planet Jupiter.

Photographic Evidence

In his summary of this category, Hartmann (Condon & Gillmor, 1968, p. 86) describes a "residual group of unidentifieds" which "is not inconsistent with the hypothesis that unknown and extraordinary aircraft have penetrated the airspace of the United States," although "none yields sufficient evidence to establish this hypothesis." A little later, Hartmann remarks:

> After investigation, there remains a small residual of the order of 2 of all cases, that appears to represent well recorded but unidentified or unidentifiable objects that are airborne—i.e., UFOs. . . . The present data are compatible with, but do not establish either the hypothesis that (1) the entire UFO phenomenon is a product of misidentification, poor reporting, and fabrication, or that (2) a very small part of the UFO phenomenon involves extraordinary events.

As examples of the "small residual" cases, we may refer to Cases 46 and 47. Concerning Case 46 (McMinnville, Oregon, May 11, 1950), Hartmann reaches the following conclusions (Condon & Gillmor, 1968, p. 407):

> This is one of the few UFO reports in which all factors investigated, geometric, psychological, and physical appear to be consistent with the assertion that an extraordinary flying object, silvery, metallic, disc-shaped, tens of meters in diameter, and evidently artificial, flew within the sight of two witnesses. It cannot be said that the evidence positively rules out fabrication, although there are some factors such as the accuracy of certain photometric measures of the original negatives which argue against a fabrication.

Hartmann describes Case 47 (Great Falls, Montana, August 15, 1950) in an abstract as follows:

> Witness I, General Manager of a Great Falls baseball team, and Witness II, Secretary, observed two white lights moving slowly across the sky. Witness I made 16 mm motion pictures of the lights. Both individuals have recently reaffirmed the observation, and there is little reason to question its validity. The case remains unexplained. Analysis indicates that the images on the film are difficult to reconcile with aircraft or other known phenomena, although aircraft cannot be entirely ruled out.

It is interesting to compare Hartmann's report and case studies with Condon's two-page summary of "Study of UFO Photographs" (Condon & Gillmor, 1968, pp. 35–37). Only one paragraph is clearly based on Hartmann's work. This reads:

> Hartmann made a detailed study of 35 photographic cases (Section IV, Chapter 3) referring to the period 1966–1968, and a selection of eighteen older cases, some of which have been widely acclaimed in the UFO literature. This photographic study led to the identification of a number of widely publicized photographs as being ordinary objects, others as fabrications, and others as innocent misidentifications of things photographed under unusual conditions.

In fact, Hartmann discusses 14 cases, of which six are from the period 1966–1968. Concerning the McMinnville, Oregon, case (Case 46), Condon refers not to the analysis made by Hartmann, but to an analysis made by Everitt Merritt, who was not a member of the project staff, but a photogrammatrist on the staff of the Autometrics Division of the Raytheon Company of Alexandria, Virginia. Merritt found that "the UFO images turned out to be too fuzzy to allow worthwhile further parametric analysis." Condon reports at length Merritt's analysis of another case (Zanesville, Ohio; not discussed anywhere else in the report) that was considered to be a hoax, and also discusses two photographs published in *Look* magazine, quoting the analysis of Staff Sergeant Earl Schroeder of the Wright-Patterson Air Force Base. Schroeder is not listed as being affiliated with the Colorado UFO Project, and the case he analyzed was not considered by the project staff.

Apart from generalizations, Condon devotes only one and one-half pages to discussion of photographic evidence. Of this one and one-half pages, 60% is devoted to the work of Merritt, 30% to the work of Schroeder, and only 10% to the work of Hartmann. Further, as we have seen, Condon's summary of the work of his own staff member (Hartmann) was quite inadequate and—for whatever reasons—misleading.

Radar-Visual Cases

Special importance may be attached to cases in which both visual and radar observations were made, and in which these observations were consistent. Such cases will typically involve several witnesses: they involve observations made at two or more "channels" of the electromagnetic spectrum; and the radar observations provide distance measurements and possibly height measurements also. Such cases are discussed in two staff summaries: Section CR III, Chapter 2, "Field Studies" by Craig (Condon & Gillmor, 1968, pp. 51–75), and Section CR III, Chapter 5, "Optical and Radar Analysis of Field Cases" by Thayer (Condon & Gillmor, 1968, pp. 115–176).

Thayer, in his summary of radar-visual cases, states (Condon & Gillmor, 1968, p. 175): "There is a small, but significant, residue of cases from the radar-visual files (i.e., 1482N, Case 2) that have no plausible explanation such

as propagation phenomena and/or misinterpreted man-made objects." Earlier in his summary (Condon & Gillmor, 1968, pp. 163–164), Thayer makes the following comment on this case, which he then identifies as "Lakenheath, England, August 13–14, 1956, 2230–0330 LST": "The probability that anomalous propagation of radar signals may have been involved in this case seems to be small." Later, he adds: "The apparently rational, intelligent behavior of the UFO suggests a mechanical device of unknown origin as the most probable explanation of this sighting."

Case 2 (listed, rather oddly, as "Greenwich, summer 1956") is presented in the Condon Report (Condon & Gillmor, 1968) pp. 248–256. The abstract reads as follows:

> At least one UFO was tracked by air traffic control radar (GCA) at two USAF-RAF stations, with apparently corresponding visual sightings of round, white rapidly moving objects which changed directions abruptly. Interception by RAF fighter aircraft was attempted; one aircraft was vectored to the UFO by GCA radar and the pilot reported airborne radar contact and radar "gunlock." The UFO appeared to circle around behind the aircraft and followed it in spite of the pilot's evasive maneuvers. Contact was broken when the aircraft returned to base, low on fuel. The preponderance of evidence indicates the possibility of a genuine UFO in this case. The weather was generally clear with good visibility.

This case has been further described by Thayer (1971) as one of the AIAA cases. It is interesting to note the conclusion given by Thayer, at the end of this article, which reflects his view after further intensive study of this case:

> In conclusion, with two highly redundant contacts—the first with ground radar, combined with both ground and airborne visual observers, and the second with airborne radar, an airborne visual observer, and two different ground radars—the Bentwaters-Lakenheath UFO incident represents one of the most significant radar-visual UFO cases. Taking into consideration the high credibility of the information and the cohesiveness and continuity of accounts, combined with a high degree of 'strangeness', it is also certainly one of the most disturbing UFO incidents known today.

The other case of special interest is Case 5 (Condon & Gillmor, 1968, pp. 260–266) listed in the Condon Report as "South-Central, Fall, 1957." This case is reviewed by Craig (Condon & Gillmor, 1968, pp. 56–58). He emphasizes that "No report of the incident was found in Blue Book files or in the files of NORAD Headquarters at Ent AFB." [The reason that no report was found is that the project staff had incorrectly dated the event as September 19, 1957, whereas it actually occurred on July 17, 1957.] Craig, in describing the phenomenon, stated:

> It disappeared suddenly and reappeared at a different location both visually and on airborne and ground radars. Since visual and radar observation seemed to coincide, reflection of ground radar did not seem a satisfactory explanation. Other explanations such as airplanes, meteors, and plasma also seem unsatisfactory.

Craig concludes (Condon & Gillmor, 1968, p. 57): "If the report is accurate, it describes an unusual, intriguing, and puzzling phenomenon, which, in the absence of additional information, must be listed as unidentified.

The case is also discussed extensively by Thayer in his summary (Condon & Gillmor, 1968, pp. 136–139). Thayer attempts an explanation in terms of "anomalous propagation" (AP) echoes and an unidentified ground light source, but adds, "There are many unexplained aspects to this sighting, however, and a solution such as given above, although possible, does not seem highly probable." The reader is urged to assess this statement by reviewing the case (Condon & Gillmor, 1968, pp. 260–266) and by reading the more extensive AIAA-sponsored account of McDonald (1971), who determined the correct date of this event and so obtained Air Force records that the Condon staff had been unable to track down. The AIAA case is, therefore, more complete, more detailed, and more reliable than the study presented in the Condon Report. The summary of this case, as given by McDonald, is as follows:

> An Air Force RB-47, equipped with electronic countermeasures (ECM) gear, and manned by six officers, was followed by an unidentified object for a distance of well over 700 mi. and for a time period of 1.5 hr., as it flew from Mississippi, through Louisiana and Texas and into Oklahoma. The object was, at various times, seen visually by the cockpit crew as an intensely luminous light, followed by ground-radar and detected on ECM monitoring gear aboard the RB-47. Of special interest in this case are several instances of simultaneous appearances and disappearances on all three of those physically distinct 'channels,' and rapidity of maneuvers beyond the prior experience of the air crew.

Condon, in his "Summary of the Study," devotes almost three pages to discussion of radar sightings of UFOs, but his comments on the case studies of the Colorado Project are confined to two short paragraphs comprising only 10% of Condon's discussion of radar sightings. As an evaluation of these case studies, he quotes from Thayer's summary: ". . . there was no case where the meteorological data available tended to negate the anomalous propagation hypothesis. . . ." This is, at best, an unfortunate quotation, implying that Thayer regards the anomalous propagation hypothesis as offering a plausible explanation of every case. A more complete quotation of Thayer's remark (Condon & Gillmor, 1968, p. 172) is as follows:

> The reader should note that the assignment of cases into the probable AP cause category could have been made on the basis of the observational testimony alone. That is to say, that there was no case where the meteorological data available tended to negate the anomalous propagation hypothesis, thereby causing that case to be assigned to some other category.

In the table (Condon & Gillmor, 1968, p. 173) to which Thayer is referring, we see that for only 19 of the 35 cases does Thayer regard anomalous prop-

agation to be the "most likely or most plausible explanation." Thayer's assessment is perhaps presented more clearly by a later quotation (Condon & Gillmor, 1968, p. 174): ". . . where the observational data pointed to anomalous propagation as the probable cause of an UFO incident, the meteorological data are overwhelmingly in favour of the plausibility of the AP hypothesis." Thayer has clearly concluded that a substantial fraction of radar observations are probably due to anomalous propagation effects; but it is equally clear that he does not ascribe *all* radar observations to this phenomenon. The impression given by Condon's summary concerning radar-visual cases is, therefore, at variance with Thayer's summary and with the cases on which Thayer's summary is based.

Condon's account of radar cases is very similar to his account of photographic evidence: very little of what he writes makes reference to the work of his staff, and what he does write about his staff's work is misleading.

Radar Detection Without Visual Detection

Both Craig and Thayer attach special significance to Case 21 (Condon & Gillmor, 1968, pp. 310–316) [Colorado Springs, Colorado, May 13, 1967] in which clear and consistent signals were shown by *two* airport radars, with no corresponding visual observation. The abstract of this case [identified on Condon & Gillmor (1968 p. 310) only as "South Mountain (Location A), Spring, 1967"] is as follows:

> Operators of two airport radars reported that a target equivalent to an aircraft had followed a commercial flight in, overtaken it, and passed it on one side, and proceeding [*sic*] at about 200 knots until it left the radar field. No corresponding object was visible from the control tower. On the basis of witnesses' reports and weather records, explanations based on anomalous atmospheric propagation or freak reflection from other objects appear inadequate. The case is not adequately explained despite features that suggest a reflection effect (see Section CR III, Chapter 6).

[Section CR III, Chapter 6, is devoted to "Visual Observations Made by U.S. Astronauts" and contains nothing relevant to this case.]

Craig, in his summary of "Field Studies," makes the following comment on this case (Condon & Gillmor, 1968, p. 72): "Of the current cases involving radar observations, one remained particulary puzzling after analysis of the information, since anomalous propagation and other common explanations apparently could not account for the observation. . . ."

In his summary of "Optical and Radar Analysis of Field Cases," Thayer devoted over one page (Condon & Gillmor, 1968, pp. 170–171) to this case. He remarks: "This is a radar-only case, and is of particular interest because the UFO could not be seen, when there was every indication that is should have been seen." He points out that, although no object was seen from the ground, from the landing Braniff plane, or from a following Continental Airlines plane, the UFO followed "precisely the correct procedure for an over-

taking aircraft, or one which is practicing an ILS approach but does not actually intend to touch down." In Thayer's opinion, "A ghost echo seems to be ruled out." He concludes that

> This must remain one of the most puzzling radar cases on record, and no conclusion is possible at this time. It seems inconceivable that an anomalous propagation echo would behave in the manner described, particularly with respect to the reported altitude changes, even if AP had been likely at the time. In view of the meteorological situation, it would seem that AP was rather unlikely. Besides, what is the probability that an AP return would appear only once, and at that time appear to execute a perfect practice ILS approach?

Condon makes no reference to this case in the section of his summary dealing with radar sightings of UFOs.

Miscellaneous Evidence

Brief mention only will be made of some of the other types of evidence considered in the report. Section CR III, Chapter 6, concerns "Visual Observations Made by U.S. Astronauts" as studied by Professor Franklin E. Roach (Condon & Gillmor, 1968, pp. 176–208). The final paragraph of Roach's "Summary and Evaluation" is as follows:

> The three unexplained sightings which have been gleaned from a great mass of reports are a challenge to the analyst. Especially puzzling is the first one of the list, the daytime sighting of an object showing details such as arms (antennas?) protruding from a body having a noticeable angular extension. If the NORAD listing of objects near the GT-4 spacecraft at the time of the sighting is complete as it presumably is, we shall have to find a rational explanation, or alternatively, keep it on our list of unidentifieds.

Condon, in discussing these observations (Condon & Gillmor, 1968, pp. 42–43), quotes Roach's remark that the three sightings are "a challenge to the analyst," and goes on to remark that "nothing definite relating to the ETH aspects of UFOs has been established as a result of these rather sporadic observations."

Concerning "Direct Physical Evidence," Craig (Condon & Gillmor, 1968, pp. 94–97) attaches special significance to "metal fragments that purportedly fell to earth at Ubatuba, São Paulo, Brazil, from an exploding extra-terrestrial vehicle. The metal was alleged to be of such extreme purity that it could not have been produced by earthly technology." Investigation by the Colorado staff showed that a sample of triply sublimed magnesium, supplied by the Dow Chemical Company, had a smaller impurity level than that of the "Brazil UFO." The analysis, however, showed that the fragments contained traces of both barium and strontium, which are not usual impurities in the production of magnesium; these metals were undetectable in the Dow sample. Craig remarks, "The high content of Sr was particularly interesting since Sr is not an expected impurity in magnesium made by usual production methods, and

Dr. Busk [of Dow Chemical Company] knew of no one who intentionally added strontium to commercial magnesium." It was found that Dow Metallurgical Laboratory had made experimental batches of magnesium alloy containing 0.1% up to 40% of strontium, which is to be compared with the level of 500 ± 100 parts per million of strontium in the Brazil sample. Although the lowest value in this range is twice the value found in the Brazil sample, Craig states that Dow had "produced a . . . batch of magnesium containing nominally the same concentration of Sr as was continued [*sic*] in the Ubatuba sample."

Craig also makes the following remarks: "Metallographic examinations show large, elongated magnesium grains, indicating that the metal had not been worked after solidification from the liquid or vapor state. It, therefore, seems doubtful that this sample had been a part of a fabricated metal object." This is a very curious remark, implying—as it does—that no fabricated object has ever been made of cast metal.

Condon, in his summary, remarks that "the magnesium metal was found to be much less pure than the regular commercial metal produced in 1957 by the Dow Chemical Company . . . (and) therefore it need not have come from an extra-terrestrial source. . . ."

Once again, Condon's statement does not give an accurate representation of the work of his staff. The staff describe the comparison sample simply as "magnesium produced by known earthly technology" (Condon & Gillmor, 1968, p. 96). Condon describes it as "regular commercial magnesium." As Craig states (Condon & Gillmor, 1968, p. 95), the Dow Chemical Company has "supplied on request samples of triply sublimed magnesium." These samples represented a laboratory production, not "regular commercial magnesium." Furthermore, the samples of triply sublimed magnesium supplied by the Dow Chemical Company had not been annealed (annealing would introduce further impurities), so that their metallurgical properties were grossly different from those of Brazil magnesium.

However, the most regrettable aspect of the Colorado Project investigation of the Brazil magnesium is that the investigation was confined to a rather limited laboratory analysis of the sample. It is a basic rule of UFO research that one must assess the *total* evidence, which always includes the narrative evidence. According to this rule, another investigator (fluent in Portuguese, or accompanied by a translator) should have been sent to Brazil to track down any evidence of events that might have been related to the Brazil magnesium sample.

The last category of evidence considered is "Indirect Physical Evidence," reviewed by Craig (Condon & Gillmor, 1968, pp. 97–115). In presenting his conclusions, he states:

> Of all physical effects claimed to be due to the presence of UFOs, the alleged malfunction of automobile motors is perhaps the most puzzling. The claim is frequently made, sometimes in reports which are impressive because they involve multiple independent witnesses. Witnesses seem certain that the function of their cars was affected by the

unidentified object, which sometimes reportedly was not seen until after the malfunction was noted. No satisfactory explanation for such effects, if indeed they occurred, is apparent. (p. 115)

The discussion of this evidence, both by Condon and by other members of the project staff, is of special interest. It is argued that, if automobile motors are stopped, it must be attributed to magnetic fields associated with UFOs (Condon & Gillmor, 1968, pp. 38, 101, 380). For the one case studied by the project, it was determined that the automobile had not been exposed to a strong magnetic field. Craig (Condon & Gillmor, 1968, p. 380) concludes: "The case, therefore, apparently did not offer probative information regarding UFOs." We shall return to discussion of this argument in Section V.

Scientific Methodology of the Colorado Project

The title of the Condon Report is "Scientific Study of Unidentified Flying Objects." The great weight attached to this report by scientists, by the public, and perhaps by officers of the Federal Government, is based on the presumption that the study was, in fact, scientific.[5] This has been disputed by a number of individuals, notably McDonald (1969) and Hynek (1972), who make specific criticisms of the methodology of the project. These criticisms will not be repeated here. The following comments are more general in nature.

Whether or not there is a well defined "scientific method" applicable to all scientific problems, the fact is that the practices used by scientists vary from one subject to another. In research areas where the background noise and/or the inherent variability are high, such as epidemiology and meteorology, it is necessary to develop and use appropriate statistical techniques of data analysis. Where the experimental situation is well controlled and where the results are faithfully reproducible, it may suffice and may be desirable to analyze a single experiment in meticulous detail.

It was stressed in Section II that physicists tend to look for an outstanding experiment that, taken in isolation, conclusively proves or disproves some hypothesis. It is perhaps not surprising, therefore, that this is the approach adopted by Condon in appraising the information reported to him by his staff. To some extent, it reflects also the attitude of the scientific staff. For exceptions to this rule, one might cite the recently quoted paragraph by Craig (Condon & Gillmor, 1968, p. 115), concerning "Indirect Physical Evidence," which clearly reflects judgment based on an *accumulation* of evidence. It is also worth pointing out that, if the staff had indeed been searching for one or two cases to prove conclusively one hypothesis or another, it would have been necessary to devote far more time, attention, manpower, and resources to those cases than appears to have been given to any one case.

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[5] And also on the presumption that it was a free and open investigation with no secret aspects such as a hidden agenda or undisclosed involvement with sources of classified information. For further comments on this aspect, see the Postscript.

The UFO problem is perhaps closer to astronomy than physics. No single observation of the position of a single planet establishes Kepler's law. No single observation of the position and magnitude of a single star establishes that the sun is in a disc-shaped galaxy. Nor can data concerning a single star confirm a proposed theory of stellar evolution. In discussing astronomical problems, it is essential to combine evidence derived from many observations. The strength of the observational facts may become significant only when very large numbers of observations are combined.

Following astronomical practice as a guide, one would infer that a crucial first step in the scientific study of UFOs would be the compilation of a catalog. This would have the immediate consequence of drawing upon information already accumulated (in many cases with great effort and great care) by other organizations. For instance, organizations such as APRO (Aerial Phenomena Research Organization), CUFOS (Center for UFO Studies), MUFON (Mutual UFO Network), and NICAP (National Investigation Committee for Aerial Phenomena) have compiled extensive files of UFO cases using careful screening and evaluation techniques. One valuable collection of data, which the project could have used, was that produced by the Battelle Memorial Institute, under contract to the Air Force, and issued as Blue Book Special Report No. 14. This was certainly available to the project, since it was declassified in 1955.

There is, indeed, great advantage to be derived from using more than one source of data. Data derived from one source only might be spurious, or partly spurious, and the same might be true for another source of data. If both sources of data yield distinct and irreconcilable patterns, one would suspect that at least one of the two sources has been subject to biased reduction and possibly even to deliberate fabrication. If one of the sources of data is from one's own scientific staff, one might conclude that the fault lies with the other group, or one might choose to check carefully the methods used by one's own team.

On the other hand, patterns that appear *consistently* in data derived from several sources are far more significant than a pattern that shows up in the data of one source but not in the data of other sources. "Strong" facts of this type can be obtained only by careful cataloging of data from as many responsible sources as one can find. After a catalog has been compiled and patterns supported by the weight of evidence in the catalog have been established, one can then begin the comparison of evidence and hypotheses. (An outstanding example of this process is the construction of the Hertzsprung-Russell diagram in astrophysics, which provides the crucial test for any theory of stellar evolution.) This procedure is complex, calling for a careful organization of theoretical work and data reduction. A "bookkeeping" procedure for organizing the many judgments involved in this stage of scientific research has been proposed elsewhere (Sturrock, 1973), with application to astrophysical problems in mind. Some subsequent comments on the scientific study of UFOs are based in part upon this article.

In assessing a phenomenon, it is essential to "filter" the available evidence. A key filtering procedure is represented by the *definition* of the phenomenon. In this respect, Condon's definition, which has already been quoted, suffers from the defect that it allows a great deal of "noise" to accompany whatever "signal" there may be in the data. Most students of the UFO phenomenon would adopt a more restrictive definition such as that adopted by Hynek (1972), who recommends that a "UFO report" be defined as "a statement by a person or persons judged responsible and psychologically normal by commonly accepted standards, describing a personal, visual, or instrumentally-aided perception of an object or light in the sky or on the ground and/or its assumed physical effects, that does not specify any known physical event, object, or process or any psychological event or process." However, the definition of the phenomenon is only one filtering procedure. In discussing a complex phenomenon such as the UFO phenomenon, it should be followed by further "filters" that may comprise restrictions on allowable evidence, classification schemes, etc. The staff summaries, indeed, provide a breakdown of evidence into categories, but this is only a rudimentary scheme of analysis.

Another important point of scientific methodology is that, if one is evaluating a hypothesis (such as ETH), it is beneficial to regard this hypothesis as one member of a complete and mutually exclusive set of hypotheses. This point also seems to have been clearly recognized by Thayer (Condon & Gillmor, 1968, p. 116), but it was apparently ignored by Condon and by other members of the project staff.

Finally, in evaluating a hypothesis, one must avoid procedures of data reduction that depend upon the truth or falseness of that hypothesis. Put another way, one must avoid "theory-dependent" arguments. This requirement, above all, makes the appraisal of the UFO phenomenon very difficult: if we entertain the hypothesis that the phenomenon may be due to an extremely advanced civilization, we must face the possibility that many ideas that we accept as simple truths may, in a wider and more sophisticated context, not be as simple and may not even be truths.

As a specific example, one may draw attention to the argument (Condon & Gillmor, 1968, p. 143) that a supersonic UFO should produce a sonic boom. This is certainly true of every supersonic object that man has constructed. But we should *not* assume that a more advanced civilization could not find some way of traveling at supersonic speeds without producing a sonic boom. Petit (1986) has paid special attention to this aspect of UFO reports and has proposed a procedure involving magnetohydrodynamic processes whereby the shock wave of a supersonic object would be suppressed.

Although it is simple to state this requirement concerning data reduction, it is by no means simple to put it into effect. It may, indeed, be necessary to proceed by trial and error: whenever one runs into an impasse, a situation in which it is impossible to reconcile the established data with any explicitly considered hypothesis (including that of ETH), one may need to review the

process of data reduction to see if the relaxation of an implicit hypothesis will lead to a situation in which the evidence can be reconciled with at least one explicit hypothesis.

A further example of this type of situation is the discussion of "Automobile Malfunction and Headlight Failure" (see Craig in Condon & Gillmor, 1968, pp. 100–108), which was discussed in Section IV. As we have noted, the position taken by Condon and other members of the project staff is that, if automobile motors are stopped, this phenomenon must be due to magnetic fields associated with UFOs. Condon and other members of the staff apparently do not consider the possibility that an advanced civilization may know of and use physical processes with which we are now unfamiliar. [Yet this possibility is perhaps the most intriguing reason a scientist would be interested in studying the UFO phenomenon.] The discussion of sonic booms and of automobile engine malfunction by the Condon staff provide two prime examples of theory-dependent arguments.

Discussion

The evaluation of evidence by category, presented in Section IV, seems to show that each staff summary is a fair and justifiably cautious summary of the relevant case material. By contrast, Condon's summary bears little relation to the work, analyses, and summaries of his own staff. Hence, a minimal criticism that one might make is that the efforts of many individuals found no satisfactory integration.[6]

This failing may have been due in part to a faulty initial conception of the nature of the phenomenon. If, as the Director may have believed, the phenomenon could be tackled as a straightforward problem of physical science, there might now be little disagreement among the scientific community regarding the validity and conclusions of the Report. The UFO phenomenon appears instead to be more akin to some of the enigmatic phenomena of modern astronomy, such as the sources of gamma-ray bursts. Concerning these strange objects, we do not know where they are, we do not know what they are, and we can only speculate on how they function; but these limitations, severe as they are, by no means deter astronomers and astrophysicists from studying them as intensively as possible.

Concerning UFOs, we are not sure whether they are hoaxes, illusions, or real. If real, we do not know whether the reality is of a psychological and sociological nature, or one that belongs in the realm of physics. If the phenomenon has physical reality, we do not know whether it can be understood in terms of present-day physics, or whether it may present us with an example

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[6] When I showed an early version of this analysis to one of the Principal Investigators of the Colorado Project, he remarked, "You should have seen the first draft that Condon wrote. It was much worse. After I pointed out a lot that was wrong with the first draft, Condon rewrote it and improved it considerably."

of 21st century (or 30th century) physics in action. If one is, indeed, facing a problem of this magnitude, it is necessary to devote the utmost care to the scientific methodology involved in the project.

In sum, it is my opinion that weaknesses of the Condon Report are an understandable but regrettable consequence of a misapprehension concerning the nature and subtlety of the phenomenon. It is also my opinion that there is much in the Condon Report that could be used in support of the proposition that an analysis of the totality of UFO reports would show that a signal emerges from the noise and that the signal is not readily comprehensible in terms of phenomena now well known to science. If this is so, then the Report makes a case for the further scientific study of UFO reports. It appears that this opinion is, in fact, shared by certain members of the Colorado Project staff. For instance, Professor David R. Saunders, who left the project in unfortunate circumstances, has published a book (Saunders & Hawkins 1968) challenging the findings of the Condon Report. Gordon D. Thayer also has continued his interest in the phenomenon, as is evident from his report on the Lakenheath case for the journal *Astronautics and Aeronautics* (Thayer, 1971).

In conclusion, it is necessary to comment briefly on the review of the Condon Report by the National Academy of Sciences Panel (Condon & Gillmor, 1968, pp. vii–ix). This distinguished body reviewed the report and fully endorsed its scope, methodology, and findings. In Section IV, we have noted the discrepancies between facts and views advanced by the Colorado Project staff and those advanced by the Director. In comparing these with the NAS Panel Review, it is clear that some of their information is taken from the Director's "Summary of the Study," even where the content of this section is contradicted by material presented in Sections CR III and CR IV of the report. For instance, in discussing photographic cases, the Panel asserts that "35 photographic cases were investigated . . . none proved to be real objects with high strangeness." This statement is entirely compatible with Condon's discussion of photographic evidence in Section CR II of the report; but, as we have seen in Section III, Condon's statements are not compatible with material presented by Hartmann, who carried out the photographic analysis: Hartmann discussed 14 cases, not 35; and, in his summary (CR 86), Hartmann states, ". . . after investigation, there remains a small residual of the order of 2% of all cases, that appears to represent well recorded but unidentified or unidentifiable objects that are air-borne—i.e. UFOs. . . ."

The Condon Report has also been studied by the UFO Subcommittee of the American Institute of Aeronautics and Astronautics, as part of their appraisal of the UFO problem (Kuettner et al., 1970). The Subcommittee states that "not all conclusions contained in the Report itself are fully reflected in Condon's summary." The subcommittee also points out that "Condon's chapter, 'Summary of the Study,' contains more than its title indicates; it discloses many of his personal conclusions."

Condon's most important recommendation was perhaps that concerned with future activity. He states that "further extensive study of UFOs probably

cannot be justified in the expectation that science will be advanced thereby" (Condon & Gillmor, 1968, p. 1). The NAS panel concurred in this recommendation. On the other hand, the AIAA UFO Subcommittee "did not find a basis in the report for his prediction that nothing of scientific value will come of further studies."

The NAS panel, which was appointed in late October and early November 1968, began their initial reading of the report on November 15, 1968. The panel convened on December 2 and again on January 6, 1969, to conclude its deliberations and to prepare its findings. Seven weeks is a very short time for the panel members to digest a report on what was probably an unfamiliar subject.[7] This is especially true when there are gross discrepancies between the report and its summary, which readers are unlikely to expect. By contrast, the views of the AIAA Subcommittee were crystallized late in 1970, allowing more time to appreciate the subtleties of the problem and to digest the massive report.

This re-examination of the Condon Report and my comparatively brief quotations from the reviews by the NAS panel and the AIAA subcommittee may cast doubt on some of the findings of the report and some of the opinions and recommendations of the Director. The following quotation shows that such dissent was foreseen, and even encouraged, by Condon himself:

> Scientists are no respecters of authority. Our conclusion that study of UFO reports is not likely to advance science will not be uncritically accepted by them. Nor should it be, nor do we wish it to be. For scientists, it is our hope that the detailed analytical presentation of what we were able to do, and what we were unable to do, will assist them in deciding whether or not they agree with our conclusions. Our hope is that the details of this report will help other scientists in seeing what the problems are and the difficulties of coping with them. (Condon & Gillmor, 1968, p. 2)

Postscript

The first draft of this article was prepared in 1974, but has recently been extensively rewritten. In the intervening years, new information has come to my attention that raises serious questions about the Colorado Project.

In the Introduction, I pointed out that the importance of the Condon Report is due to the fact that the study "is the only unclassified investigation of the UFO phenomenon carried out by an established scientific organization under contract to a U.S. federal agency." By contrast, documents that were originally classified and have since been released (such as reports arising from Projects Sign, Grudge, and Blue Book) make almost no impact on the scientific community. The reason for this may be understood from a remark of Condon

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[7] I have learned from private conversation with one of the panelists that, in fact, all of the panelists were not as happy with the Condon Report as the panel report would indicate. He told me that he had had concerns and reservations about the Condon Report but did not press them in the panel discussions because he "did not want to rock the boat."

himself, who writes: "Where secrecy is known to exist, one can never be absolutely sure that he knows the complete truth" (Condon & Gillmor, 1968, p. 522).

This remark of Condon's was made in relation to the proposition that "some agency of the Government—either within the Air Force, the Central Intelligence Agency, or elsewhere—knows all about UFOs and is keeping the knowledge secret. . . . We decided not to pay special attention to [this hypothesis], but instead to keep alert to any indications that might lead to any evidence that not all of the essential facts known to the government were being given to us. . . . We found no such evidence."

The above statement, that the Colorado Project found no evidence that the government might be withholding information about the UFO problem, should be compared with the account of Case 30 (Condon & Gillmor, 1968, pp. 341, 342). The abstract of this case reads: "A civilian employee at an AFB confirmed an earlier report that base personnel had made an UFO sighting, although official sources denied that such an event had occurred." The background reads: "A rumor was relayed to this project by a source considered to be reliable, reporting in the fall, 1967, six UFOs had followed an X-15 flight at the AFB. It was suggested that motion pictures of the event should be available from the Air Force." There follows an account of the investigation that includes the following remarks: "The rumor persisted, however, with indications that official secrecy was associated with the event. If reports of the event had been classified, no record would appear on the operations log. . . . A responsible base employee . . . had reassured our source that there was a sighting by pilots and control tower operators. . . . His replacement . . . is quoted as saying that there apparently was something to it because 'they are not just flatly denying it.' " Attempts to learn more about the reported event from the PIO [Public Information Officer] were met with apparent evasion from that office. [The PIO was never available for telephone conversation and never returned telephone calls, even when a Pentagon officer transmitted a request to the base Director of Information that he telephone the Project Investigator and clarify this situation.] "(The source) was contacted later . . . and asked for clarification of the incident. He responded only that the Director of Information had told him to 'stay out of that.' " The conclusion of this case reads as follows: "Although it is true that the report of this incident was never more than a rumor, it is also true that project investigators were not able to satisfactorily confirm or deny that an UFO incident had occurred. Attempts to investigate the rumor were met with evasion and uncooperative responses to our inquiries by base information."

If Condon was familiar with the details of this case, as he certainly should have been, it is hard to understand that he would state without qualification or comment that "We were assured that the federal government would withhold no information on the subject. . . ." (Condon & Gillmor, 1968, p. 8).

In the late 1970s, the Freedom of Information Act made it possible to request from federal agencies information that had been classified. On learning

that the Central Intelligence Agency had released some information related to the UFO problem, I requested copies of this information from the CIA and received it in 1979. Some of the documents refer to the Colorado Project. On February 7, 1967, a memorandum for the Deputy Director for Intelligence reports on the U.S. Air Force contract with the University of Colorado to investigate the UFO situation. It reported arrangements between Brigadier General Ed Giller (USAF) and Dr. Thomas Ratchford (AFOSR) with Arthur C. Lundahl (Director of the National Photographic Interpretation Center [NPIC] of the CIA), which provided for NPIC to provide photographic services to the Air Force in support of the Colorado Project. Arrangements were made for Condon, Low, Saunders, William Price (ex-director of AFRST), and John Coleman (listed as "ex-director of the National Academy of Sciences") to visit NPIC. All five visitors were cleared for at least USAF secret. Lundahl had told the USAF representatives that he could "have no part in writing whatever they might conclude on this UFO phenomena [*sic*]."[8] Lundahl goes on to day, "I might be able to preserve a CIA window on this program for whatever use DRS&T might want to make of it."

A memorandum for the record, dated February 23, 1967, concerns the planned visit to NPIC, which occurred on February 20, 1967. Dr. Condon was accompanied by "Dr. Richard Lowe [this must refer to Mr. Robert Low], Dr. David Saunders, Dr. William Price, and Dr. Thomas Ratchford. The clearance level was secret. It was agreed that NPIC would assist Dr. Condon on the understanding that this assistance would not be identified as work accomplished by the CIA. NPIC presented briefings on their analytical capabilities and on their results "on the second UFO project." There followed "a general discussion on UFOs."

A document dated March 24, 1967, is entitled "Guidance to UFO Photographers" and comprises a list of ten recommendations to photographers who have an opportunity to photograph a UFO event, and an information sheet that the photographer should complete. This document was prepared by NPIC and approved by Dr. Arthur C. Lundahl, Director of NPIC. On May 1, 1967, the Colorado Project issued a press release calling for "pictures of unidentified flying objects from private citizens," and it gave a set of recommmendations to the photographer and a list of items of information that the photographer should prepare. This press release is simply a rewrite of the NPIC document.

A "memorandum for the record," dated May 8, 1967, concerns a "UFO briefing for Dr. Edward Condon, 5 May 1967." Those listed as being in at-

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[8] We must hope that Lundahl had no firm basis for his assumption that the conclusions of the study were to be written by Air Force staff rather than by the Director of the study. On the other hand, when we find phrasing such as "Paralleling the official government interest, was a burgeoning of amateur interest . . ." (Condon & Gillmor, 1968, p. 13) in a section ostensibly written by Condon, a master of scientific prose, and when we contrast Condon's detailed and apparently accurate history of Air Force involvement (pp. 502–552) with his sparse and inaccurate account of the work of his own team (pp. 7–50), it seems highly likely that he had received some help from official quarters.

tendance were (in addition to CIA staff) Condon, Low, Hartmann, Ratchford, Dr. Charles Reed of the National Research Council, and someone whose name is suppressed in the released document. This unknown person presented a briefing on photogrammetric analysis he had carried out on a UFO case, and his briefing impressed Condon and his group very favorably. At that meeting, preliminary arrangements were made for contractual arrangements which would enable the unknown person to carry out analysis for the Colorado Project. It was agreed that that unknown person would submit his report on the analysis of the Zanesville photography through certain channels so that it would reach Condon. It is clear that the unknown person must have been Dr. Everitt Merritt of the Autometrics Division of the Raytheon Company of Alexandria, Virginia. As the reader will remember, Condon referred extensively to the work of Merritt when dealing with photographic evidence, including Merritt's analysis of the Zanesville case. Hence, the CIA releases explain how Condon "became acquainted with Everitt Merritt" and why and how he "made arrangements with Merritt for his services." During the May 5, 1967, meeting, Condon "indicated he wished to keep a channel open into our organization [CIA/NPIC]." It was agreed that Dr. Merritt's report on his analysis of the Zanesville photography would be forwarded, for distribution to Dr. Condon, through an office, the name of which has been deleted from the document.

The facts that Condon and some members of his staff had secret meetings with some members of the Central Intelligence Agency, that the CIA contributed to the work of the Colorado Project, and that these facts are not revealed in the Condon Report, raise troubling questions. If Condon and some members of his staff received secret briefings from the CIA, did they also receive secret briefings from the Air Force and perhaps from other agencies? If they did receive secret briefings from the Air Force, can one accept at face value Condon's statement (Condon & Gillmor, 1968, p. 8) that "The contract provided that the planning, direction and conclusions of the Colorado project were to be conducted wholly independently of the Air Force"? If there were no secret briefings, why was it necessary to arrange Air Force secret clearance for some members of the Project staff? Was there a "hidden agenda" for the Colorado Project? Would knowledge of the hidden agenda, if it existed, help one to understand the gross mismatch between Condon's summary and the work of his own staff? Why did Condon attach so much more weight to the work of Merritt, whom he met through the good offices of the CIA, than he did to the work of Hartmann, who was a member of the staff of the Colorado Project? Finally, given the importance attached by the scientific community to the subsequent review of the Condon Report by a panel of the National Academy of Sciences, what is one to make of the presence at a meeting early in the project between Condon and his staff and CIA staff, of a scientist identified in the CIA record as "ex-director of the National Academy of Sciences"?

It is conceivable that these concerns are groundless, that there were no secret meetings other than those already referred to, and that these had no

impact whatever on the policy guiding the conduct of the Colorado Project. It may be that the discrepancy between Condon's summary and the work of his own staff was the result of an innocent lack of activity and lack of perception on Condon's part. Nevertheless, to repeat an earlier quotation from Condon, "where secrecy is known to exist, one can never by absolutely sure that he knows the complete truth" (Condon & Gillmor, 1968, p. 522).

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