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Commonalities in Arguments Over Anomalies

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Abstract—There are a number of features that seem to be common to controversies about claimed anomalies. Foremost perhaps is the very fact of controversy. Typically, the anomaly runs counter to the expectations of established orthodoxy, and there is often a populist tone to the argument. Questions concerning the demarcation of science from pseudoscience and of epistemology in general are typically raised. It becomes important to distinguish between the pros and cons of a particular claim and what is said by the disputants; an examination of the ways in which belief and disbelief are distributed among various groups can be useful in clarifying the issues. It is also vital that one distinguish between the occurrence and the reporting of events. As with interdisciplinary work, it is problematic to establish what parts of existing knowledge might be relevant; and anomalies bring to attention large and sometimes unsuspected areas of ignorance. There are pitfalls in assuming that anomalies with superficial similarities have any functional or necessary relation to one another. The manner in which anomalies are perceived is clearly influenced by contemporary science and by contemporary societal beliefs. For many reasons that go far beyond the possible reality of any given anomalous claim, then, the study of anomalies can be interesting and enlightening.

Introduction

There seems to be agreement over the existence of a class or category of "anomalous phenomena"—the sort of subject with which the Society for Scientific Exploration (formed for the study of anomalous phenomena) is concerned. But it is no simple matter to define precisely what it is that makes a particular topic a member of that class.

The attempt to enumerate these subjects shows that the class is a large one: e.g., abominable snowman, absent healing, acupuncture, astral projection, astrology, Atlantis, auras, automatic writing, ball lightning, biorhythms, clairvoyance, dowsing, flying saucers, Loch Ness monsters, Wilhelm Reich. The list is long indeed and has spawned a number of bibliographies, compendia, and encyclopedias (for example, Cavendish, 1974; Clarie, 1978, 1984; Corliss, n.d.; Shepard, 1978; see also such periodicals as *Fate, Skeptical Inquirer, Zetetic Scholar*).

The attempt to enumerate illustrates one corollary of the lack of a good definition: controversy over the inclusion of particular items. Although most people seem to feel that such a category of topics exists, and would also agree over the inclusion of many such subjects as those listed above, there is no unanimity over all the topics to be included. Thus ball lightning, earthquake lights, hypnotism, and the kraken exemplify subjects now regarded as fit for scientific inquiry but not so regarded in the past—though even today's opinion is by no means unanimous on these matters. On the other hand, fairies, Martian canals, and unicorns, say, were once taken quite seriously but no longer are—except by a few, that is.

Those last statements sharpen the point: perhaps the most significant feature common to all these matters is lack of unanimity or even consensus, and therefore there is *controversy*. Other commonalities will become evident as one considers some suggested definitions of the class of anomalous phenomena.

Attempts at Definition

The sort of subject with which we are concerned would better be called not simply anomalies but rather *alleged* anomalies (Westrum & Truzzi, 1978). Not everyone agrees that the phenomena actually exist, and at least some of those who insist that they do also believe that there is nothing actually anomalous (in the sense of unnatural) about them.

That the allegations of anomalies meet with disbelief illustrates that they are in some manner surprising (Westrum & Truzzi, 1978): they appear to contradict accepted knowledge. The degree of contrariness varies from case to case: those matters that Truzzi (1977) has called "crypto" merely assert the existence of unexpected objects—Loch Ness monsters, say—whose actual demonstrated discovery would demand very little adjustment of current theory; "para" matters, on the other hand, assert unexpected relationships and would demand major change in existing theory—for example, psychokinetic effects.

Some attempts to define the class of anomalies are implicit and not explicit; thus one or another topic will be described as "Fortean," or "occult," or "pseudoscience," say. In the following discussion, the aptness of such terms is considered.

Enigmas was a term used by Rupert Gould (1890–1948), who wrote about a number of these subjects (Gould, 1928, 1929, 1930, 1934, 1944). But the term implies a real phenomenon that awaits explanation, missing the point that disagreement often subsists over whether a claimed anomaly is even real. Further, an enigma may be an isolated event or fact, whereas a significant aspect of "anomalous phenomena" is the claim that something of generality is at stake—not just a single deformed animal, say, but an unexpected *species*. Again, data quite within the mainstream of a given science can be "enigmatic," whereas the anomalous phenomena that concern us are typically outside any single discipline, be it because of the subject or because of the views of the disciplinary practitioners. So "enigma" is too broad and misses the points that make anomalies *non grata*.

Fortean refers to Charles Fort (1874–1932) who published several volumes describing alleged anomalies (Fort, 1919, 1923, 1931, 1932). As with any attempt to name a field after an individual (compare "Darwinism"!), this is at once too restrictive and too vague: too restrictive if interpreted only as those things about which Fort actually wrote; too vague and subject to disagreement if understood nonrestrictively as all the things and ideas with which Fort would concern himself if he were active today. Apt, however, is the connotation of matters commonly ignored or regarded as intellectually unrespectable by authoritative opinion; and also apt is the implication that those who take these matters seriously believe that there is considerable wheat among the chaff. While Fort himself did not venture far into theory or attempted explanation, many Forteans are less inhibited (see, for example, *Fortean Times*).

Occult is quite commonly applied to some of these matters. But that implies some hidden or secret system of understanding or power, whereas some alleged anomalies are merely claims that a particular sort of thing exists, say, a Sasquatch. Where "Fortean" may stress too much mere phenomenology, "occult" places too much emphasis on the metaphysical. "Occult" is apt, however, in the implication of disagreement; though some devotees use the term proudly, in more common usage it has a not quite respectable connotation, of beliefs that are outdated, inappropriate or downright wrong.

Oddities was another term used by Gould, unsatisfactory for the same reason as "enigmas" (see above); oddities can be "mere," whereas an important aspect of anomalous phenomena is their alleged significance as well as reality.

Popular fallacies captures important features: the topics are outside the scientific mainstream and often enjoy wide public acceptance; a couple of classic books in this genre base their titles on these features (Gardner, 1957; Mackay, 1841). "Populist" might be even better than "popular" because proponents often stress the antagonism of the establishment and the fact that anyone among the lay public is free to become an advocate or investigator. But "fallacies" is not an appropriate general label. Though some of such subjects have become discredited (fairies, Martian canals, unicorns, etc.), others have moved to respectability (meteorites being perhaps the classic instance). Moreover, a characteristic aspect of anomalous phenomena is the willingness of a significant number of serious people to take them seriously, at least provisionally; otherwise, in point of fact, these subjects would not be so visible. "Fallacies" is part of the rhetoric of these controversies, not an appropriate definition.

Pseudoscience is a commonly applied term; it is apt in the respect that an invariable feature of the controversies is arguing over whether or not the matter is science or scientific, not apt in prejudging that question.

These attempts to label or define illustrate the difficulties and why one often resorts to drawing up a list rather than to venturing a definition.

However, the partial aptness of the labels points to some common features of anomalous phenomena: there is disagreement over the truth and significance of the subjects; the established intellectual disciplines disdain them; though most of them may be fallacious, history teaches that at least a few of them harbor a kernel of truth and are likely to attain respectability in the future. Also, these subjects raise the questions: What is science? What is pseudoscience? How are they to be distinguished?

Disagreement

Disagreement over anomalies tends to be quite sharp: people seem to be very sure, either that anomalies are real and significant or that they are not real. Not many are both actively interested in such a subject and largely neutral over its reality; and those few "zetetics" or "true skeptics" tend to be sniped at from both sides (Truzzi, 1987).

Some of these disagreements have persisted over a very long time, about astrology or various psychic phenomena, say; other disagreements are only a few decades old, such as those about Loch Ness monsters or UFOs (though, of course, the proponents believe in a much longer history of what underlies those alleged phenomena); and other disagreements can be very recent, for example those concerning matters of holistic health. But what might seem to be a long persistence of some of these matters may be partly an illusion. As in science, philosophy or any other intellectual concern, the matter is not *precisely* the same now as it was then: the reasons for belief and for disbelief change (for instance over heliocentricity); the purported attributes alter (Greek "atoms" are not John-Dalton "atoms" and the latter are not 1987 "atoms"); the misfit of the anomaly with the wider society is different ("disembodied spirits" were not always as unthinkable as they are now). That sort of change over time vitiates an otherwise and theoretically attractive way of winnowing wheat from the chaff: a widely agreed characteristic of pseudoscience is that is does not progress, whereas science does; in practice, however, the attempt to apply this criterion to a specific instance usually fails as the disputants also disagree over whether or not there has been *significant* progress as opposed to mere *ad hoc* adjustments or retreats to vaguer positions.

There is no agreed procedure or venue for settling these disagreements: they are waged in the media, in specialist publications and meetings, and only rarely and episodically in mainstream scientific journals. The literature is unorganized, and individual items vary enormously in reliability.

Each anomaly tends to have its own set of enthusiasts, the number of whom varies widely: it is tiny for something like Flat-Earth theory, whereas belief in some form of extrasensory perception is a majority rather than a minority view. For many anomalies, belief is widespread among the public or the laity so that the proponents actually represent the majority while at the same time they are a minority with respect to established expert opinion in the relevant fields. The disbelievers, on the other hand, have prevailing expert opinion on their side but often feel themselves to be a struggling minority with respect to the public and the media.

Many devotees of a particular anomaly have little or no interest in other anomalies: most UFOlogists have little interest in cryptozoology, and vice versa. Some people, though, have a catholic taste in anomalies—a few as believers, others as zetetics or as students of the "meta-phenomenon" of "concern with anomalies." Amongst the disbelievers, on the other hand, indiscriminate catholic rejection of all anomalies is more the rule than the exception (Abell & Singer, 1981; Cazeau & Scott, 1979; Gardner, 1957; Sladek, 1973; *Skeptical Inquirer*).

Societal attitudes toward believers cover a wide range. Most people suspect that "something is wrong" with Flat-Earth proponents, whereas they tend to regard Christian Scientists just as people who happen to have a particular religious belief. Here again one sees the difficulty of generalizing: one has to distinguish the average popular or lay view from the average intellectual view: medical people and intellectuals tend to hold more drastic opinions about Christian Science than does the public at large. Occasionally, an anomaly may display a peculiar mixture of acceptance (or toleration) and rejection: thus, chiropractic is intellectually anathema but legally sanctioned, and those who have recourse to chiropractors include some who reject the theoretical claims made for it. Again, though the metaphysical notions pushed by Rudolf Steiner have had almost no adherents for quite some time, private schools based on Steiner's precepts for education have long continued to enjoy respectability.

For many anomalies, the proponents are not at all homogeneous in other respects: Nessie hunters, UFOlogists, or parapsychologists, for instance, encompass all social, intellectual, educational, religious, and national categories. For some anomalies, however, there are typical correlations: between, on the one hand, political liberalism and agnosticism or atheism, say, and on the other hand, holistic medicine, organic gardening, and health faddism in general. Occasionally, there are quite tight correlations: for instance, between Creation Science and religious fundamentalism.

In examining the disagreement over any given anomaly, then, one has to inquire separately into a number of issues: the division of opinion, differences in opinion among different groups, and possible correlations with other social or intellectual factors.

Belief Distributions

Since there is no single, compelling authoritative view and no unanimity over anomalous matters, it could be useful (Bauer, 1987) to describe disagreements by means of *belief distributions* which show how widely held among various groups are varying degrees of belief in a given anomaly. Thus, with respect to Flat-Earth theory, almost everyone is sure that the belief is wrong, but a very small group holds the opposite, and almost no one is neutral about it; among scientists the minority view is missing altogether; but if opinion had been sampled a couple of millennia ago, or were sampled nowadays among Stone-Age peoples, the distributions would be quite different. On the other hand, the distribution of belief over clairvoyance would show a high degree of belief, a high degree of undecidedness, and little utter disbelief; and that distribution will have changed over the centuries much less than has the Flat-Earth one, and is likely to be less different among different cultures; however, within western society the distributions would be markedly different among various groups, more so than over Flat-Earthness.

The point is that meaningful enquiry into anomalies is difficult unless one can obtain some information about who are the believers and who are the disbelievers, what attributes correlate with belief and with disbelief, and how belief has changed over time. On topics that are in the mainstream, if one says, for example, "It is known that . . . ," or "It has been shown that . . . ," others either accept the statement through their own knowledge or recognize that it can be validated in texts or reference books or the primary literature. However, if one makes a similar statement about a claimed anomaly, one immediately encounters objections and such questions as, "*Who* knows? *Who* has shown?". On topics in the mainstream, the interested neophyte or outsider readily finds authoritative works; on anomalous topics, by contrast, he finds disagreements and had better look into who the disputants are, what the division of opinion is among different groups, and so forth.

Where belief distributions are available, they can afford some unexpected insights. Thus, in the matter of Loch Ness, a content analysis of the literature revealed the following (Bauer, 1988), more or less counter to intuition: within the scientific literature, the average opinion is neutral and the belief distribution is almost the "normal" ("bell-shaped") one; the daily press has shown little change of opinion over the years, whereas in magazines there was a marked shift from disbelief toward belief beginning in the 1950s. The analysis also showed marked variations in the degree of polarization of opinion: during periods when little fresh news came from Loch Ness, strong belief and strong disbelief decreased and the degree of undecidedness grew.

One superficially tempting definition of the class of anomalous phenomena might be, subjects for which both proof and disproof are lacking. But a consideration of belief distributions reveals how unworkable in practice that theoretically apt definition would be: who has the authority to decide what is sufficient proof and what suffices as disproof? In principle, one might argue that where opinion is strongly divided, Q.E.D.: the issue has not been proven one way or the other. In that case, however, the belief distribution —if it reflects "objectively" formed opinions—should be concentrated around neutrality or undecidedness, whereas in practice the belief distributions on such things are much more polarized than that (see Bauer, 1987, 1988): some people maintain that belief has been proven correct while others insist that disbelief has been proven to be warranted.

The distinction between what the facts might demonstrate and what interested people believe ought to be made, but commonly it is not made. In assessing the possible merits of any given anomaly, one needs to distinguish between, on the one hand, the case that might ideally or disinterestedly be made and, on the other hand, the case that is actually made by the proponents. Also, in assessing the criticisms levelled by disbelievers, one needs to distinguish between those that speak to (or rather against) the purported evidence for the anomaly and those that merely rebut, in debating fashion, points ventured by believers. Something that is quite true can be propounded so incompetently and by such dubious characters that one is misled into not believing; or, people of excellent character might utterly flub making a proper, logical case; or, trustworthy people can in good faith make a convincing case for something that happens not to be so. That some proponents of some anomaly happen to be fraudulent or logically incompetent does not (in itself) invalidate the anomaly; that the proponents are in many ways demonstrably wrong, even, does not inevitably entail that their anomaly altogether fails to exist (Clark, 1983). None of that, of course, is intended to avoid the reasonable view that the burden of proof for demonstrating an anomaly rests on the proponents, that no one is obliged to take them seriously or to take any interest in the matter unless and until that burden of proof has been accepted and discharged. At the same time, one needs also to recognize that, no matter how compelling the evidence, there will always remain some people who refuse to alter their preexisting, demonstrably false beliefs.

Relevant Knowledge

Always at issue with anomalous phenomena is the relevance of the accepted categorical knowledge held by the various disciplines. The investigation and discussion of anomalies take place largely outside the intellectual mainstream: journals of biology rarely feature sea serpents or Nessies; journals of astronomy or physics pay almost no attention to Velikovsky or his ilk. The literature expounding specific anomalies consists chiefly of books from commercial, nontechnical publishers—often potboilers or even worse—as well as articles in popular magazines, in specialist periodicals, and in small-circulation bulletins, newsletters and the like, often unavailable in libraries and frequently short-lived. Thus, there is little if any contact with academic disciplines that might have something useful to say about a particular anomalous claim.

More fundamentally, as long as something remains an anomalous phenomenon, one does not know which disciplines might be relevant. In the case of astrology, for instance, it is typically assumed that astronomy is the relevant science that embodies information needed to assess astrological contentions. But astronomy deals only with planetary and stellar phenomena, whereas astrology makes assertions about human behavior as well. The fact that even Nobel laureates in physics and astronomy assert that there is no conceivable mechanism by which astrology could work need not be taken as decisive (Westrum, 1976); those of empirical bent want first to know, do any correlations exist that seem to parallel astrological conjectures? It turns out that some such correlations appear to exist (Gauquelin, 1983), and to explore the validity and significance of these data calls for expertise in social science and in statistics, not in astronomy or in physics. How adequately were the samples of individuals chosen for whom the correlation was found? How appropriate was the statistical analysis used? Are there any factors whose periodicity is at all similar to that of the planetary positions—even only approximately for a couple of generations, the period covered by the lives of the people sampled? And that last question takes one—as all such series of questions do, sooner or later—to the limits of human knowledge: there exists no discipline that knows about periodicities as such, and the attempt to decide whether or not a given set of data shows periodicities easily becomes controversial (Hoffman, 1985; Raup, 1986). (So far as biological periodicities go, or such correlations as between health and weather, the effects spoken of by individual meteorologists and doctors still remain to be codified.) The point is not only that astronomy is not *per se* qualified to pronounce upon astrological claims: it is not even clear that any combination of disciplines could presently mobilize all the needed understanding.

In practice, of course, what one takes to be relevant to a given anomaly is influenced by one's preconceptions, about that anomaly and also stemming from one's training: physicists tend to regard psychokinetic claims as calling for careful observation and measurement of objects, whereas psychologists or magicians tend to regard psychokinetic claims as calling for careful observation and control of claimed psychics.

The investigation of anomalies, therefore, suffers some of the dilemmas and difficulties inherently involved in any interdisciplinary effort (Bauer, 1984, pp. 288–295). One can never be certain that some field or other does not know something that could be directly relevant; one can never be sure that a deeper understanding of some subject might not provide an answer; one cannot know which experts in which fields to approach. To give just one instance: "angel hair" purportedly associated with UFOs turned out to be spiders' webs (Story, 1981, pp. 95–98), but who other than an arachidnologist knew—or needed to know, for that matter—that certain spiders migrate thousands of feet up in the atmosphere? How many biologists of various specializations knew that? And why should a UFOlogist ever feel the need to consult a biologist in the first place, let alone an arachidnologist?

Thus, with anomalous phenomena, serendipity is an even more desirable eventuality than it is in science. Further, the study of anomalies—as interdisciplinary efforts in general—can usefully bring to our attention the substantial areas of ignorance that subsist at the edges and interstices of established knowledge.

Classifying Anomalies

Just as one cannot know, for a given anomaly, what disciplines might be relevant, neither can one be sure which anomalous phenomena might be related to one another. It follows that there are potential dangers (as well as advantages) in asserting that certain anomalies are related to one another.

Cryptozoology encompasses the possibility of unexpected creatures of all sorts—Bigfoot, Nessie, sea serpents, and so forth (Greenwell, 1985; Heuvelmans, 1982, 1986). That grouping can be heuristic, since all such searches have to be concerned with human testimony and with purported references in folklore, and expertise about those matters can by usefully shared. On the other hand, such association could also mislead: despite similarities in *sorts* of evidence, specific items pertain only to a single anomaly. If Nessie turns out to exist and thereby to vindicate her eyewitnesses, that will not mean that eyewitnesses for Bigfoot should then be thought to have been vindicated; or that the legendary kraken turned out to be the real giant squid does not entail that other creatures described in similar detail in similar literature must also exist or have existed.

In some cases, the implicit assumption that certain anomalies are related amounts to accepting them as real and even asserting, albeit only vaguely, an underlying explanation or mechanism or theory; that parapsychology is taken to include clairvoyance, telepathy, and psychokinesis appears not to be disputed, yet even the unquestioned existence of each of those effects would not demonstrate that they are sensibly related—no more than sight and hearing are. Again, that some people include spiritualism or reincarnation within parapsychology bespeaks a preexisting belief about the nature of those matters.

The cultural Zeitgeist obviously influences what is regarded as being anomalous at any given time (heliocentricity, say), but it also influences how a particular anomaly is thought or talked about (Hufford, 1982). Thus UFOs led to speculation about airships in the late 19th century whereas after World War II they have supported speculation about spaceships; there are myths appropriate to the space age (Cohen, 1965). When everyone was a religious believer, anomalies were interpreted as miracles; in the age of science, anomalies are seen in the light of science—as "alternative," valid science by some and as pseudoscience by others. And every spectacular advance in science seems to spawn unorthodox claims as well as countertheorists: during the 19th century, electrical and magnetic bases were asserted for quack medicines; at the turn of the century, the discovery of X-rays and radioactivity was accompanied by spurious discoveries of radiations and quack radiation cures; Newton provoked antigravitationists, and Einstein produced antirelativists.

Attempts to find neutral characterizations for anomalies could help in the study of specific ones. Thus, it is well to be aware at the outset that the strength of evidence called for increases in proportion to the improbability of the claimed anomaly (that extraordinary claims call for extraordinary proof is continually stressed by Truzzi, 1987, for example); and it is salutary to recall just how stringently that applies within science (Trefil, 1983). The distinction between "crypto" and "para" claims is germane here, between mere claims of the existence of unexpected objects and by contrast assertions of unorthodox theoretical connections (Truzzi, 1977). A particular class of crypto-type phenomena is that of "hidden events" (Westrum, 1982): certain things happen but are not spoken or written about, which makes it possible to believe that they do not occur; the battering of children is a remarkable instance. In those cases in particular as well as with anomalous matters in general, the frequency of occurrence bears little relationship to the frequency of reporting (Westrum, 1982, and references therein): the publication of reports is influenced by societal attitudes that are quite apart from the phenomenon itself. In those respects, the study of anomalous phenomena offers similar difficulties to those encountered by students of mental illness or sexual behavior, for example.

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Remote Viewing and Computer Communications—An Experiment

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Abstract—A series of remote viewing experiments were run with 12 participants who communicated through a computer conferencing network. These participants, who were located in various regions of the United States and Canada, used portable terminals in their homes and offices to provide typed descriptions of 10 mineral samples. These samples were divided into an open series and a double-blind series. A panel of five judges was asked to match the remote viewing descriptions against the mineral samples by a percentage scoring system. The correct target sample was identified in 8 out of 33 cases: this represents more than double the pure chance expectation. Two experienced users provided 20 transcripts for which the probability of achieving the observed distribution of the percentage score by chance was 0.04.

These results confirm earlier reports of successful remote viewing experiments while extending them to cases in which participants were thousands of miles away from each other and in which the targets were mineral samples of potential economic significance, with control of communications provided by a computer network.

Research Questions in Remote Viewing

Among other human parapsychological abilities, the phenomenon of remote viewing has been the subject of intensive study in recent years because it is amenable to standardized experimental protocols and to formal replication. Indeed, the pioneering work of Hal Puthoff and Russell Targ (1976) at the Stanford Research Institute has now been extended and verified by several teams in various parts of the world (Jahn, R. G., 1982).

Examples of early remote viewing research noted by Puthoff and Targ (1976) include experiments performed by members of the Society for Psychical Research in London, reported by Sir Oliver Lodge in the 1922 book *Outline of Science* (1922). In these experiments one person kept a record of impressions, at a certain time each day, of what the other person actually saw while travelling hundreds of miles away. These researchers reported a striking similarity between the descriptions and the actual sites and they did not find any decrease of accuracy with increasing distance.

According to Russell Targ and Keith Harary (1984), the first researcher to "see the pattern" of remote viewing phenomena was Rene Warcollier whose book *Mind to Mind* (1963) contained not only actual experiments but a theory of "secondary elaboration" that described the nature of the "mental noise" interfering with perception of the targets.

In contrast with this earlier work, in many of the SRI experiments no one was actually at the site. The viewer was simply asked to describe "the target" or a location designated by a certain longitude and latitude.

The observation that human subjects, under certain conditions of training and of operational environment, are able to provide accurate descriptions of remote sites that are only designated by an address (such as a set of coordinates) or by an abstract keyword opens up a series of interesting questions.

One would want to know, in particular, whether or not distance from the site is a factor of success; whether sensory deprivation enhances or hampers performance; and whether it is true that group communication amplifies or stimulates remote viewing ability. Similarly, it seems important to establish whether or not telepathy plays a role in remote viewing; that is, all other conditions being the same, are the viewers more successful when another human being has conscious knowledge of the site or target to be viewed?

Over 10 years ago a series of experiments were designed, executed and analyzed in order to explore these questions. Our results have only been published until now in a very terse and summary form (Vallee, 1981; Vallee, Hastings & Askevold, 1976). It may be of interest to members of the Society for Scientific Exploration to review how the sessions were planned and what was learned from them.

Operations of the Computer Network

At the time when the initial SRI remote viewing work was in progress, the present author served as principal investigator for DARPA and NSF in the development of the first teleconferencing system based on a computer network. Hence we had access to advanced software permitting an arbitrary number of remote users to connect themselves under a series of extended electronic meetings where social structure could be controlled by a leader designated as "Organizer," and where access could be secured through keywords.

It was thus possible for the first time to run conferences whose styles ranged from the very formal (with voting and anonymous polling) to the completely open, as in brainstorming sessions. Furthermore, interaction could be simultaneous, in which case any entry made by any user was instantaneously transmitted to all others, or delayed, in which case users could come and go, read the entries that had accumulated, and respond at their convenience. In this system, which has been used for numerous other business and scientific applications, the files of entries are encrypted in storage and cannot be changed once released by the sender.

All communication was typed on terminals equipped with modems and connected to the network by local telephone calls. The system features and human factors have been described in greater detail by the author in the book *Computer Message Systems* (Vallee, 1984).

The availability of this system enabled us not only to conduct the first remote viewing experiments that used computer communications as a medium, but to document the advantages of this form of group interaction. Among these advantages we noted the ability to capture unobtrusively the date, time, duration, and text of every comment, thus greatly facilitating monitoring and analysis; the convenience of having access to the entire conference at any time of day or night; the ability for each viewer to perform experiments while staying in familiar, comfortable surroundings, rather than enduring the frustration of tiring trips to a remote laboratory; and the remarkable feeling of connectedness created by the computer network itself —a feeling that Arthur Hastings has aptly called "an altered state of communications."

Among these factors, the enhanced control and monitoring structure of the medium is perhaps the most important at this stage of remote viewing research: computer conferencing eliminates non-verbal and subliminal cues and places the entire group in conditions where all communications can be documented without the intervention of a human monitor.

Planning of the Experiments

Having agreed on the principle of the experiments we secured private funding and the necessary equipment to enable a group of 12 users to interact over a four-week period, from mid-June to mid-July 1975. The formal remote viewing experiments were scheduled for the period of June 29 to July 3.

Given the pioneering nature of the work, it was not difficult to enlist the participation of an enthusiastic and dedicated group of seasoned researchers. Indeed the team included some of the star performers in the SRI work as well as interested observers located throughout the United States and Canada. All participants donated their personal time to this effort.

The choice of mineral samples as targets for these experiments is another factor that distinguishes this work from other remote viewing efforts that have concentrated on geographic sites. While the result of such work is often striking, the variability and the ambiguity of terrain, complicated by real-time changes in weather, makes judging a very difficult task, open to many criticisms in terms of protocol. It seemed to the author that it would be preferable, given the objectives of this particular experiment, to select targets that were simpler and more easy to standardize than locations on the earth.

Another advantage we saw in the use of rocks as targets was the undeniable economic value of the accurate recognition of minerals by psychic means, if it could be demonstrated and perfected. Accordingly we selected as the targets a set of 11 mineral samples, listed in Table 1 below.

The remote viewers were told only that they would be working with rocks

TABLE 1
Mineral samples used as targets

A =	Rare earth sample of bastnosite and Europium
B =	Not used in experiments
C =	Vein filling of galena (silver ore) and quartz
D =	Precious Opal from Virginia Valley, Nevada
E =	Gold ore contained in a section of quartz vein
F =	Halite (salt crystal) from Nevada
G =	Realgar from Utah, aggregated with orpiment
H =	Barite from Dugway Proving Grounds
I =	Cinnabar from Alaska (mercury mineral)
J =	Magnetite from British Columbia, strongly magnetic
K =	Cobaltite from Alaska (cobalt ore)

from North America. The minerals themselves were selected by the author and by Gerald Askevold, a geologist with the U.S. Geological Survey. They came from the collections of the USGS or from private collections. They were selected for their uniqueness in terms of composition, origin, physical properties, or esthetic appeal. They had not been polished, cut or otherwise altered by man after extraction. For security reasons the samples remained in the custody of Mr. Askevold.

After selection the rocks were sealed in envelopes and the envelopes were labelled from A to K (one sample, labelled as "B" was not used). A geologist who was not a member of the experimental group wrote a one or two-page description of each mineral sample, and these descriptions were filed for later reference. Appendix 2 gives an actual example of such a description as it was made available to the judges.

The Computer Conference

From June 14 to June 28 the participants began to interact in a series of discussions about current issues in psychic research. The exchanges were warm and friendly. They enabled the group members to become comfortable with the equipment, the system and with each other.

The formal experiments began on Sunday June 29 and lasted until Thursday July 3, 1975. They were conducted as follows: Five of the envelopes containing samples were pulled out at random, and enclosed in larger blank envelopes. An assistant was then asked to come into the room and, left alone in the room, to randomize these unmarked envelopes. They were then labeled "Sunday" through "Thursday" and constituted the "double-blind pool." The remaining samples were referred to as the "open pool."

The experimental protocol was then conducted as follows: Each day at 7:30 a.m. and 7:30 p.m. (California time) a geologist sitting alone at his home terminal selected any one of the envelopes from the "open pool," extracted the sample and held it in his hand for 30 minutes. At this point he simply announced that the session could begin. Anyone entering the conference during this period could volunteer a remote viewing description which was recorded by the computer with a date and time stamp. The entry was available in hard copy and could not be edited after it was sent. The geologist closed the session by providing a feedback statement about the sample, which was then removed from the pool.

Each morning we also took the envelope bearing the name of that day from the double blind pool. We placed it at a designated office location where it was available for remote viewing for eight hours. At that location the envelope was in full view of the professional and office staff during the entire day, providing additional security. Any participant coming into the computer conference during that day could type in a description of the sample contained inside the double sealed envelope. At the end of the day the envelope was returned to the geologist, who added it to the "open pool" but provided no feedback for these targets.

Remote Viewing Data

Upon completion of the experiments the transcript contained 33 descriptions of the 10 samples from six active remote viewers. Thirteen of these descriptions were under double blind conditions and 20 under open conditions. Four specimens had been run both as double blind and as open targets.

A review of the computer transcript illustrates both the process and the product of the experiments, witness the following description by viewer "B" located in Florida:

I was getting something that looked like a big Malaysian penny for a minute, all copper/brass, and there suddenly what to my wondering eyes should appear but a small donut of quartz crystals, like a keyring had fallen into a supersaturated salt solution overnight.

Entry No. 238, the same day at 7:46 p.m. The viewer is "S" in New York City:

I have the impression I could "look" right through it. My analytical overlay is providing lots of alternatives. Damn, wish it would keep still. Crystal, crystal, crystal ball, glass, crystal clear crystal.

Entry No. 239, two minutes later, the same viewer:

I think I'll settle for a chunk of crystal of some sort, formed by dripping and evaporation. Location by specific state: Northern Nevada?

Indeed, the actual target that inspired these descriptions was a semi-transparent salt crystal. The feedback entry (No. 251), was provided the same day at 8:00 p.m. by geologist Gerald Askevold in Menlo Park, California:

The sample (F) is a beautiful specimen of crystalline halite, which is salt, and in this almost pure form is practically transparent (in fact, looks very much like quartz). It has beautiful cubic cleavage on part of the sample, and I can see through it. This sample was taken in St. Thomas, Nevada. Halite is formed from sedimentary evaporite beds.

In the above exchange, many of the familiar aspects of remote viewing are illustrated. The same is true in the description of the next target sample made by "B" the next day:

Why do I keep getting greens? I see a medium size green wedge. I don't see a pure emerald crystal, much as I would like to. It is flecked, and connected to a coarse rock edging. A non-geologist would say it is not metallic: it looks to me like it was poured, a heavy liquid green plastic (the green becoming blue-green at the edges of the sample), and if fractured it would be in one clean smooth break of glassine purity.

The stone in question was an opal and the independent geological description we had in our files read: "A small and irregularly angular fragment that is roughly pyramidal in shape. Most of the specimen is a pale brown, aphanitic, siliceous rock with a conchoidal fracture. It is transected by thin discontinuous veinlets that are translucent and have a play of delicate colors including deep green blue and milky white. The specimen was selected to represent the material in the veinlets, precious opal."

These few extracts provide a sense of the richness and precision of the descriptions we obtained. They also convey the spirit of the experiments, an attitude of positive team work and an atmosphere of deep trust. The participants commented frequently on this atmosphere, which they subjectively felt was conducive to good performance on their part. The realization that the software effectively bridged time and space was specially stimulating. At times participants from Canada, New York, Florida, and California were making entries into the system simultaneously.

At the end of the conference we were able to retrieve the experimental descriptions and to subject them to a formal judging process.

Statistical Findings

Of all the phases of the remote viewing process, judging is the most complex. It calls for the matching of actual descriptions with a set of known targets. Such matching is to a large extent subjective: how can we be sure that two judges will perceive the same similarities and the same differences when pairing a description with a target?

To alleviate this problem the SRI protocol uses a panel of several judges who are asked to rank the targets for each description. We modified the protocol by asking our five judges to assign a "score" as a probability of match (rather than a rank) to each description. This percentage score reflected the judge's certainty of the "match."

The judges were professionals: a sociologist, a librarian, an editor, an administrative assistant and a physicist. They had not taken part in the experiment and had not read the transcript. They were provided with the geologist's objective description of the ten rocks and with the 33 remote viewing statements that had been retyped eliminating any indication of date, authorship or experimental conditions. They were also provided with the 10 mineral samples.

The task of each of the five judges was to independently assign one or more probability estimates to each description, reflecting the match between that description and every mineral sample. In other words, each judge independently rated each transcript against each of the 10 samples and assigned a numerical value to the goodness of fit in such a way that the sum of all the values equaled 100, possibly assigning a non-zero value to the category "other" if the fit with the given samples was felt to be poor. Appendix 3 shows the actual instructions as they appeared in the introduction to the booklet given to each judge, and Appendix 4 shows an actual transcript rating form as it was filled out by one of the judges.

When the judging process was completed these numbers were added to provide a score of possible matches for every one of the 33 descriptions, resulting in the figures in Table 2, where the top five matches have been listed for every description. In Table 2 the circled entries indicate a correct match and a number in parentheses gives the aggregate score assigned to that mineral sample by the panel.

We see, for instance, that given description #1 the judges felt it was most representative of sample J (with a total score of 134) followed by F and C. The actual target was C. In description #3 the judges overwhelmingly designated F as the best match (score of 212) and F was indeed the target.

To facilitate in-depth analysis of these results, Appendix 5 gives the actual individual figures assigned by each of the independent judges in our panel, and Appendix 6 shows the aggregate figures, obtained when the ratings were summed across the five independent judges.

The actual target was assigned the highest score (that is, it was "correctly identified") in 8 out of 33 cases. This frequency is more than double a pure chance expectation of 3.3. By chance it would occur less than once in 100 trials.

For a more detailed analysis, which accounted for the distribution of percentages among several targets for each description, the percentage scores were computer processed with the Statistical Package for the Social Sciences (SPSS). A one-tailed *T*-test was used to determine the probability that the assigned percentage scores for correct and incorrect targets were due to chance.

For all 33 transcripts the probability of achieving the observed distribution by chance was 0.08. For the 20 transcripts provided by the two most experienced viewers in the group, the *T*-test indicated a 0.04 probability score. In fact, the results for the whole group are due entirely to these two "most experienced" viewers.

TABLE 2
Analysis results (Correct matches are circled)

Remote Description			Panel Selection and Scores Among the 10 Possible Targets					
No.	Open/ Blind	Author	Best Match	2nd Match	3rd Match	4th Match	5th Match	Actual Target Was:
1	Open	S	J(134)	F(60)	C(50)	G(44)	D(30)	C
2	Open	H	A(100)	I(60)	F(52)	H(50)	C(45)	C
3	Blind	S	F(212)	G(50)	C(10)	D(10)	H(10)	F
4	Open	S	J(100)	E(76)	F(55)	K(30)	A(10)	G
5	Blind	H	K(70)	H(62)	G(60)			D
6	Blind	B	D(284)	J(10)				D
7	Blind	T	A(205)	I(90)	G(80)			D
8	Blind	S	I(210)	J(40)				D
9	Open	H	H(150)	F(70)	J(50)	C(20)		I
10	Open	S	D(50)	J(50)	K(50)	E(48)	C(30)	I
11	Open	B	I(208)	D(30)	C(20)	H(20)		I
12	Open	H	F(45)	E(20)	D(17)			E
13	Open	B	F(188)					E
14	Blind	B	K(110)	D(56)	J(30)			H
15	Blind	H	A(180)	D(10)	F(10)			H
16	Blind	S	H(166)					H
17	Open	B	D(100)	F(90)				F
18	Open	S	F(246)	D(120)	I(20)			F
19	Open	S	G(104)	C(100)	J(84)			J
20	Open	H	F(30)	K(20)	D(6)			J
21	Open	B	J(56)	I(50)	D(30)	A(10)	C(10)	J
22	Blind	B	H(62)					K
23	Blind	H	D(52)	G(10)				K
24	Blind	Ba	C(40)	D(14)	J(10)			K
25	Open	B	D(72)	J(10)				D
26	Open	V	J(80)	D(25)				D
27	Open	H	D(55)	E(5)	F(5)	K(2)		D
28	Open	B	D(126)					H
29	Open	H	D(222)	F(10)				H
30	Open	S	J(58)	C(30)	H(22)	A(10)	I(10)	H
31	Blind	H	H(32)	E(30)	G(10)	I(6)		A
32	Blind	B	C(130)	G(124)				A
33	Open	B	I(60)	D(16)	J(15)	K(6)		A

Lessons Drawn From the Experiment

We were encouraged by the results of this experiment. Accurate and significant remote perception occurred under test conditions that placed the most successful participants 2,500 miles away from the targets. Also of interest is the result that the "double blind" and the "open" conditions provided equally correct descriptions, suggesting that the ability under study also functions on information not known to others. The computer conference system allowed control of the test conditions, with complete recording of all messages among participants.

About two-thirds of the transcripts contained descriptive elements that corresponded with the correct target specimen, but often these were mixed with non-corresponding elements, and it was not possible to reduce the information to a coherent single identification. The characteristics most often identified correctly were the color of the sample, the shape, relative weight, presence of crystals, type of material (for instance, metallic), and geological formation process (for instance, volcanic).

Attempts to specify location were usually in error as were descriptions of the size of the samples and their exact substance. In particular we were disappointed that the sample of europium (A) had been missed by the viewers; this particular target came from a unique mine in Mountain Pass, California, that has the potential of cornering the world's rare earth market. A unique location could therefore have been provided. We were also disappointed that the magnetic properties of sample J had not come through. We do not know if these patterns are due to the participants or to the nature of the information transfer process, or to insufficient structuring of our experiments. We suggest that further studies should select targets that are easily discriminated (i.e., widely different) along the "most perceived" characteristics. In order to stimulate such studies, and to facilitate replication, a formal Protocol is attached here as Appendix 1.

The fact that several of the specimens were composite and contained mixed materials made this an especially complex (though realistic) test situation, perhaps more demanding than the conditions that prevailed in the SRI studies, at least for non-geologists. Our results tend to validate Puthoff and Targ's experiments and strongly indicate that remote viewing techniques are deserving of further scientific attention.

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Appendix 1

A 17-Step Protocol for Remote Viewing Experiments

This formal Protocol employs computer-based communications media capable of supporting message exchange either in synchronous (simultaneous) manner or in asynchronous (delayed) manner among a geographically disseminated group of users. The system must capture uniquely and unobtrusively the name of the sender and the date and time of a message, and it must preclude editing once an entry is made.

Step 1. A series of 10 to 12 mineral samples are selected by two principal investigators. They have similar characteristics in terms of bulk, such that they can fit in a standard letter-size mailing envelope. It is advisable to select a few more samples than actually required.

Step 2. An independent geologist writes a one-page statement covering the appearance, density, composition, history and physical properties of the samples. These statements are set aside in a secure place for later use by the judges panel.

Step 3. The principal investigators enclose the samples in sealed envelopes labelled with letters of the alphabet for unique designation. This designation is kept confidential.

Step 4. The samples are divided arbitrarily into an "open pool" and a "double-blind pool," each pool composed of five or six samples. The selection is made by someone who has not seen the samples and only has access to the envelopes, which are of similar bulk and weight.

Step 5. The "double-blind pool" is randomized as follows: the envelopes containing the samples are placed inside larger, unmarked envelopes that are left on a table by the experimenters together with a marker pen. The investigators leave the room. A person who has no knowledge of the experiment and is not intended to be either a subject or a judge is sent into the room under supervision by another independent person, with instructions to move the envelopes around in any order and then to write on them the names of the days of the week.

Step 6. One of the principal investigators takes custody of the "open pool" while the other takes custody of the "double-blind pool."

Step 7. A group of viewers in remote locations has been selected. This group is trained in the use of the computer system and briefed on the following process (steps 8 through 11). They are told that their task is to provide a description of mineral samples that have not been refined or processed. The experiments extend over a specific five-day period.

Step 8. On each day of the experiments, at a designated time (such as 8:00 a.m.) the "open pool" experimenter logs on to the system, makes his or her presence known to the group, opens any envelope at random and takes the sample in hand for 30 minutes, declaring simply the session has begun. Any viewer who chooses to provide a description during this time period is free to do so. Descriptions so entered are seen by anyone who is logged on. At the end of the 30-minute period the principal investigator types in a "feedback" statement describing the sample in his or her own words.

Step 9. The same procedure is repeated with a new sample at a fixed, predesignated time at the end of the day—for instance at 8:00 p.m.

Step 10. Also on each day of the experiments, the "blind pool" investigator takes the large envelope marked with the name of that particular day and places it in a designated location (such as "on the metal tray next to my terminal"). He or she makes an entry into the system announcing that the "target" is available. Anyone logging in at any time during the day is free to provide a description of the sample contained in the envelope.

No feedback is provided. At the end of the day the envelope is removed and turned over to the second investigator, who extracts the small envelope from the large one and adds it to his "open pool."

Step 11. At the end of the last day of the experiments feedback statements are provided for all "blind pool" targets and the viewer group is disbanded.

Step 12. A panel of judges is assembled. The panel is composed of five persons who have experience, either from professional practice or personal background, in using judgment in qualitative decisions. They are open-minded about psychic abilities, without being strongly committed to any particular theory about such abilities. They do not know the viewers personally.

Step 13. All viewer statements are retyped from the computer transcript in a standard format with names and dates removed. These anonymous statements are also randomized and are turned over to the panel of judges who are provided with all mineral samples and the independent geologist's prior descriptions. The judges are not given the "feedback" statements written by the experimenters, which were written after the fact and may therefore be biased.

Step 14. Taking each viewer statement in turn, it is the task of an individual judge to assign to each mineral sample a rating "score" representing its perceived match with respect to that particular statement. The scores for the given statement must add up to 100. For instance, the judge may feel that the statement matches sample "E" perfectly, giving a 100 score to E and zero to all others. Alternatively, the 100 points might be spread among two or more samples and a category called "other." The scores are turned over to the principal investigators.

Step 15. For each viewer statement, the scores are added together and the five best matches are retained to provide a summary table similar to Table 2.

Step 16. Using standard, widely-available computer software such as the SPSS package, one uses a one-tailed *T*-test to determine the probability that the assigned scores for correct and incorrect targets are due to chance.

Step 17. It is then possible to go back over the set of experiments and examine the results obtained separately for the open and blind pools, for specific targets, distance, or physical characteristics, and for specific viewers, thus providing information to guide future experiments.

Note. The same protocol would naturally apply, with minor adaptation, to any collection of similar objects. It is not restricted to mineral samples.

Appendix 2

Example of Independent Geological Description

Specimen A—Locality: Mountain Pass, California (southern California near Nevada border)

General Appearance

The specimen is a somewhat flat, angular fragment ($8 \times 7 \times 2$ cm.) that is splotchy pale red, moderate pink, and light brownish-gray in color. It is a rock composed of several non-metallic minerals, the most conspicuous forms moderate pink crystals to 2 cm. across scattered through the specimen and in thin veinlets (bastnosite). These crystals are only moderately well-formed and have many irregular boundaries. It is light in density.

Mineralogy

The specimen contains the rare mineral bastnosite—a fluorcarbonate of the rare earth elements and is especially rich in cerium, lanthanum, reodymium, and praseodymium. Europium is a minor constituent.

Occurrence and Use

Europium is used in color televisions.

The specimen comes from a unique mineral occurrence at Mountain Pass, California. Here bastnosite occurs in cartherote-rich veins that are spatially and genetically related to a potash-rich intrusive shankinite-syenite complex. Other minerals commonly associated with it are calcite, dolomite, ankerite, and siderite.

This single mine has the potential to corner the world's rare earth market.

Appendix 3

Actual Instructions to Judges

This booklet contains ten target descriptions, labeled A through K (sample B was not used), printed on yellow sheets and providing a photograph of each sample together with an accurate summary of its properties written by a geologist not associated with the experiments. Read these target descriptions first.

It also contains 33 unlabeled verbal "remote perceptions" of these targets by a team of experimenters (pink sheets numbered 1 to 33). Your task is to match each remote perception of the experimenters against the 10 targets.

We would like you to find what you would consider the best match for each of the descriptions. Your matches will be expressed in terms of probabilities, and for this purpose we have provided a list of the samples, in the margin of each remote perception, in order for you to allocate percentages to possible choices. A category called "other" has been provided and should be used when the remote perception does not match well with any of the samples such that the sum of all percentages allocated to samples A through K adds up to less than one hundred.

Another way to think of this assignment is: If you had to bet the sum of $100 on a given remote perception corresponding to the ten target samples, how much money would you be willing to bet and how much would you place on each letter?

Thank you for helping us in analyzing this experiment.

Appendix 4

Example of Actual Judge's Response For a Given Transcript

Remote Perception No. 6		Percentage Allocated to:
Black shiny hardness, like a crystal	A	
of obsidian projecting from more	C	
basic coarse rock. A strong sense of	D	70
this two-naturedness; two definitely	E	
different aspects to the one sample.	F	
The polished part looks like a min-	G	
iature black Washington Monu-	H	

Remote Perception No. 6

ment, except six or eight-sided, with a pyramidal point at the end. About 4–5 inches long. Comes from Central New Mexico.

Percentage Allocated to:

I ________________

J _______10_______

K ________________

Other _______20_______

Total: 100

Appendix 5

Results From Individual Judges

Description No.	Author	Target Sample: Hand Held	Target Sample: Double Blind	Individual Rating of Transcripts: Judge 1	Judge 2	Judge 3	Judge 4	Judge 5
1	S	C		24G + 24J	30D + 50J	50C + 50J	10H + 10J	60F + 20G + 20I
2	H	C		12F + 40H	40C + 20I + 40J	100A	25E	5C + 40F + 5G + 10H + 40I
3	S		SUN (F)	12F	70F + 10H	100F	20F	10C + 10D + 10F + 50G
4	S	G		6E + 40J	10A + 10C + 5F + 60J	30K	20E + 20F	10D + 50E + 30F
5	H		MON (D)	2H	50H	70K		60G + 10H
6	B		MON (D)	24D	90D	100D		70D + 10J
7	T		MON (D)	30A	70I	100A	75A	80G + 20I
8	S		MON (D)	30I	30I	100I		50I + 40J
9	H	I		40H	70F	100H	10H	20C + 50J
10	S	I		28E + 2F	50D + 20F	30C + 20E + 50K		50J
11	B	I		20C + 48I	30D	100I		20H + 60I
12	H	E		12D		5D + 45F		20E
13	B	E		8F		100F	10F	70F
14	B		TUE (H)	6D	30J	100K	10K	50D
15	H		TUE (H)	20A	60A	100A		10D + 10F
16	S		TUE (H)	6H		100H		60H
17	B	F		30F	60F	100D		
18	S	F		86F	40D + 20I	100F	10D + 40F	70D + 20F
19	S	J		24G + 24J	60J	100C		80G
20	H	J		6D		10K	10F	20F + 10K
21	B	J		6J	10A + 10C + 10D + 10I	20D	10J	40I + 40J
22	B		WED (K)	12H				50H
23	H		WED (K)	12D	10D + 10G			30D
24	Ba		WED (K)	4D	10D + 10J			40C
25	B	D		12D	10D		10J	50D
26	V	D		30J		25D		50J
27	H	D		2K	5D + 5E + 5F			50D
28	B	H		36D	40D	10D		40D
29	H	H		52D	40D	100D	20D	10D + 10F
30	S	H		12H + 18J	10A + 10I + 10K	10H	30J	30D + 10J
31	H		THU (A)	12H + 6I			20H	30E + 10G
32	B		THU (A)	24G	30C	100C	40G	60G
33	B	A		6D + 6K		10D + 15J		60I

Appendix 6

Results for Judges Panel as a Whole

Description No.	Author	Target Sample: Hand Held	Target Sample: Double Blind	Aggregate Panel Results for Specimen: A	C	D	E	F	G	H	I	J	K
1	S	C			50	30		60	44	10	20	134	
2	H	C		100	45		25	52	5	50	60	40	
3	S		SUN (F)		10	10		212	50	10			
4	S	G		10	10	10	76	55				100	30
5	H		MON (D)						60	62			70
6	B		MON (D)			284						10	
7	T		MON (D)	205					80		90		
8	S		MON (D)								210	40	
9	H	I			20			70		150		50	
10	S	I			30	50	48	22				50	50
11	B	I			20	30				20	208		
12	H	E				17	20	45					
13	B	E						188					
14	B		TUE (H)			56						30	100
15	H		TUE (H)	180		10		10					
16	S		TUE (H)							166			
17	B	F			100			90					
18	S	F				120		246			20		
19	S	J			100				104			84	
20	H	J				6		30					20
21	B	J		10	10	30					50	56	
22	B		WED (K)							62			
23	H		WED (K)			52			10				
24	Ba		WED (K)		40	14						10	
25	B	D				72						10	
26	V	D				25						80	
27	H	D				55	5	5					2
28	B	H				126							
29	H	H				222		10					
30	S	H		10	30					22	10	58	10
31	H		THU (A)				30		10	32	6		
32	B		THU (A)		130				124				
33	B	A				16					60	15	6

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Is There a Mars Effect?

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Abstract—The so-called "Mars Effect" is discussed in a larger context. The phenomenon refers to a significant tendency for champion athletes to have been born at the time of either the rise or the upper culmination of the planet Mars. The populations and samples, methodology and its development are described along with earlier and more recent findings. Control studies and replications by others are reported in some detail. Particular attention is paid to certain basic and procedural criticisms and the problem of bias or artifacts. The current scientific status of the issue is reviewed in light of several kinds of empirical evidence that has accumulated over the past three decades. The question raised in the title of the paper is answered in the affirmative.

Introduction

I began empirical studies in the 1940s, initially focusing on the claims of astrology. The verdict of my statistical evaluations was not at all favorable to those claims. Thus I found no truth whatever behind certain major tenets of the horoscope, including the alleged influence of the signs of the zodiac, the reality of the astrological "aspects," the reported role of the "houses," or the prediction of future events. I also analysed in detail the statistical evidence offered by some well-publicized astrologers (e.g., Paul Choisnard, Karl E. Krafft); was forced to emphasize the lack of a sound methodology, and was generally unable to replicate their findings (Gauquelin, 1955, 1978). Over the years, and even recently, I made further attempts to test the validity of zodiacal signs or "aspects." In spite of more refined approaches and larger samples I still failed to obtain positive results (Gauquelin, 1980, 1981, 1982, 1985).

Nevertheless my labors were not entirely in vain: In the process, from 1951 on, I recorded the birth times of French men and women who were particularly successful in a variety of occupations or professions, and it became obvious to me that the distribution of certain associated planetary positions diverged sharply from the averages. These results could not just be written off as chance, and would be deemed "very significant" by statisticians as well.

I published my observations in a first book, *L'Influence des Astres,* complete with the 6,000 birth data items on which they were based (Gauquelin, 1955). It was in this book—unfortunately not yet available in English—that I described what has become known as the "Mars effect." This is the marked tendency for champion athletes to be born when the planet Mars has either risen over the horizon or passed its upper culmination. This particular pattern is seen far more frequently around the birth of outstanding athletes than for low ranking ones. As some readers are aware, the Mars effect referred to here has been under skeptical scrutiny by experts for many years. Additional details regarding this two-decade long controversy are summarized later.

It is important to emphasize, however, that the Mars effect for sports champions is merely one among my many findings concerning famous individuals. For instance, Jupiter, in analogous fashion, was found linked to success in politics, cinema, theater, and journalism; Saturn, with accomplishment in science; the moon, "favorable" in the case of writers. Besides athletes Mars also played much the same role for military leaders, chief executives, physicians, and so forth. Very generally, planetary position at birth—in term of the rise and upper culmination—was found associated with outstanding professional accomplishment. Results obtained in France have been successfully replicated through records of 18,000 other notable Europeans. Details were given in my second book, *Les Hommes et les Astres* (Gauquelin, 1960). In 1970 my laboratory published six volumes comprising all the birth and planetary data assembled since 1949 (Gauquelin, 1970). This enables interested scientists to verify the materials and the conclusions. Recently, I carried out additional replications, with positive outcome, on 1,400 eminent Americans (Gauquelin, 1982) and on new European samples, mostly French (Gauquelin, 1979, 1984), again making available the data base for inspection.

It is logical that scientists are most reluctant to accept findings of such an extraordinary nature. Indeed, biases or errors seem the most reasonable explanation. It is, therefore, necessary to describe my methodology in greater detail.

Methods and Procedure

My chief purpose was establishing an objective method that could be verified at every step: (1) the gathering of data; (2) astronomical computations; (3) statistical analysis. This seems to be the only way to establish the validity of the observations. The main problems to be solved here are discussed in what follows.

1. Gathering Birth Data

Biographical Dictionaries. The names of eminent individuals were culled from biographical directories and similar sources. These publications commonly list the date and place of birth of everyone included. In the framework of my research, they satisfy three important criteria:

- Objectivity: The dictionaries were compiled by individuals other than myself and for a different purpose;
- Homogeneity: All members of the group listed have in common that they had achieved success in the same occupation or profession;
- Large number of cases: Directories tend to afford access to the names of many hundred or thousands of successful individuals.

However, as I have been pointing out since the beginning of my work, success in a professional activity is merely a convenient criterion of analysis and cannot figure in the direct explanation of the observed statistical relationships (Gauquelin, 1955, 1960, 1973).

The search for biographical works, whether in France, other European countries, or in the United States, often entailed serious difficulties. Therefore, the relative abundance or paucity of data in respect to specific professional groups also reflect the relative comprehensiveness of the sources I was able to locate. Whatever was found was used, and none was arbitrarily omitted.

It was also necessary to avoid an arbitrary selection among the records collected. Whenever possible, all Subjects listed in the dictionaries were included in my investigations. Some of these sources, however, contained so many entries that the criterion of true notability or renown could not have been met. In such instances, clearly outstanding individuals had to be differentiated from the more obscure. Objective criteria of selection were accordingly defined and, once adopted, were maintained throughout the research phase in question.

Information from Birth Registries. The observed statistical relationships evidently involve the planets' movement and position at birth. It was, therefore, necessary also to know the *hour* of each birth. This information together with the date and place is recorded in the official birth registries. I would, therefore, write to the registry office of each place of birth given in the directories in order to confirm the date and to obtain the precise hour. All the responses received are kept in files in my laboratory and in their original envelopes. There they are available for inspection (Kurtz & Gauquelin, 1977; Dean, 1987; Ertel, 1987). Of course, I did not receive the information in every instance, but in each case where the record was thus incomplete, an explanation or justification is added (for additional details see Gauquelin, 1955, 1960, 1970, 1979, 1982, 1984). The chief limitations were due to the following:

- Incomplete documentation (generally omission of the hour of birth);
- Name of the individual sought is not on record in the registry office of the birth place listed in the directory;
- Refusal to give out information (seldom, except in West Germany and, even more so, in the U.S.A.);
- No reply from the office (very rare).

Let me finally note that the relative degree of confidence in the information obtained from registries in Europe was the subject of a special study by historical epoch and country. The associated reliability was proven to be sufficient; that is, it would permit statistical effects like the ones I observed to be manifested, provided such did exist. For example, as far as birth hours are concerned, a study of data originating between 1850 and 1940 revealed a margin of error of only 20 minutes (Gauquelin, 1959, 1960, 1971; Reverchon, 1967). In the U.S.A. the corresponding precision tended to exceed that of the European records (Gauquelin, 1982).

The Data Base. The number of Subjects overall is in excess of 30,000. The records were gathered in France, Italy, Germany, Belgium, and the Netherlands; and later, in the United States. They span the time from 1793 to 1950, with the majority of births dating to the second half of the 19th and the beginning of the 20th century, respectively. The time when the registries were first established (and birth hours becoming a matter of record) varies from country to country. In France it was 1793; Napoleon also introduced the system, within a few years, in Belgium, the Netherlands, the West bank of the Rhine (Germany) and Naples and Sicily in Italy, respectively. On the other hand, in most of Italy the system was not put into effect until 1866; and in most of Germany, not until 1876. In the United States considerable variation existed across the states. Of course, the more recent the records, the fewer relevant data one can expect to gather for the present purpose. (For specifics, see Gauquelin, 1955, 1960, 1970, 1982.)

2. Astronomical Data

Correlations I observed here involve the position of bodies of the solar system relative to the terrestrial horizon and meridian, that is, two selected positions of the daily movement.

Daily Movement

The celestial bodies appear, over the same period of time and with a uniform movement, to describe a circle parallel to the celestial equator, with the axis defined by the geographic poles.

As seen from the earth those bodies always rise on the Eastern horizon, reach their culmination, and set on the West. They thus occupy all the possible positions on their circular path (e.g., like the sun). This apparent motion is due to the 24-hour rotation of the earth on its axis. I examined the positions of those various bodies during the daily movement in relation to the birth time of each Subject of the professional groups mentioned earlier. Calculating these positions does not present fundamental difficulties. Indeed they have long been available as tabulations or in yearbook format.

Thus, assuming we wish to know the trajectory of Mars in the sky over Paris on 24th of May, 1956, we need only consult a yearbook to find that on this date the planet rose at 0:44, reached its highest point at 05:33, and set at 10:22. Determinations like these are readily available. Let us now imagine that a certain child was born in Paris on May 24th, 1956. If he was born at 1 a.m., then Mars would just have appeared on the horizon. If birth occurred at 6 a.m., Mars would just have culminated in the sky of the city and begun its descent.

Dividing the Planetary Trajectory Into Sectors

In our circumstances we cannot, however, be content with such general descriptions. In order to assign usable probabilities to the planet's positions we will in practice divide its daily path into sectors which can serve as reference. In my research I have employed a division into 36, 18, or 12 sectors, respectively (Figure 1). In a sense, this creates a cosmic roulette wheel numbered from 1 to 36 (or 1 to 18, or 1 to 12), always counted from the planet's rise. At the time of a person's birth, each planet is located in one

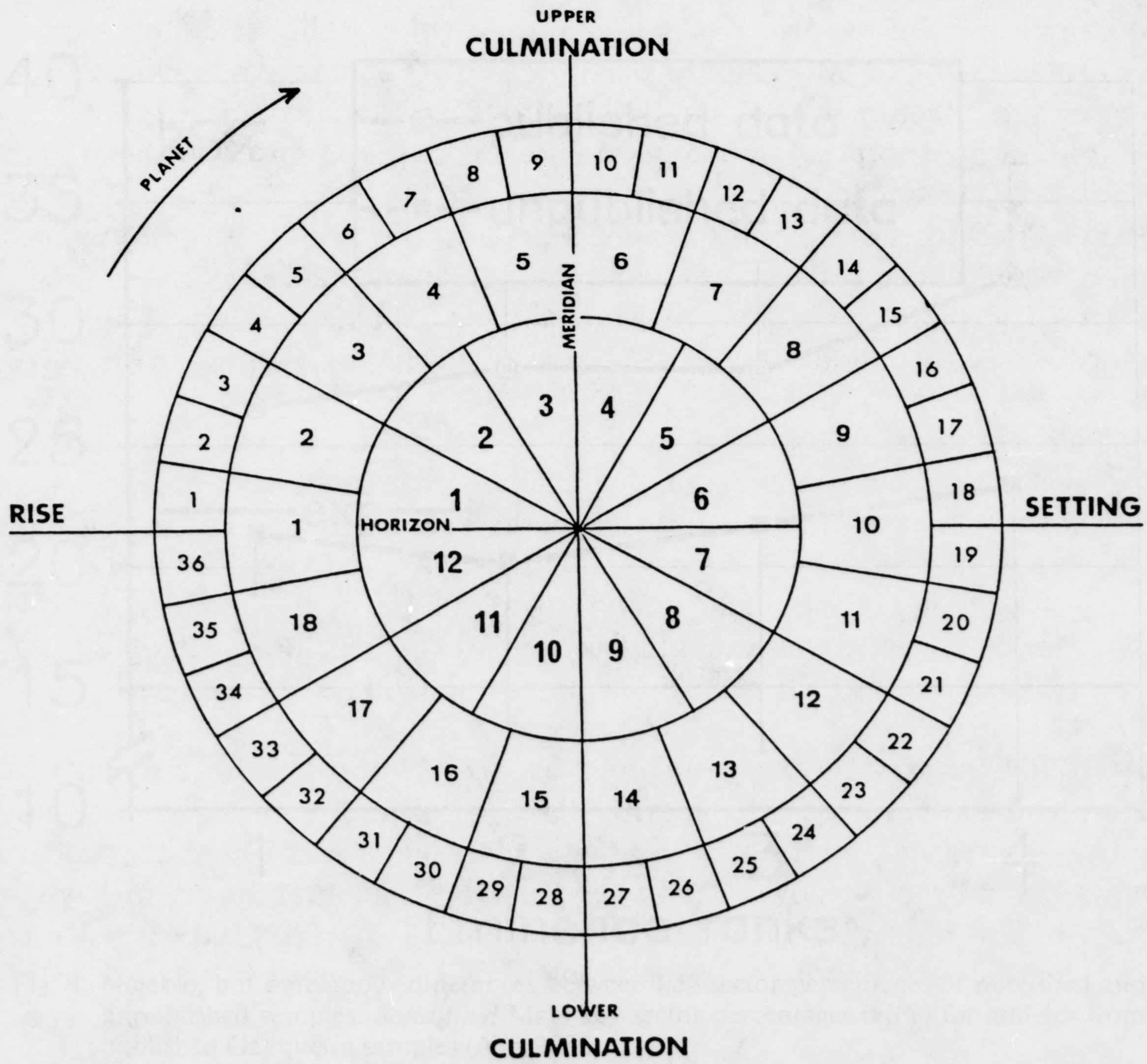

Fig. 1. The three divisions of the daily movement.

of the 36 sectors on the celestial "dial." If we have a hundred or a thousand birth records, we can quite reliably estimate probabilities from the frequency with which a planet had appeared in sectors No. 1, 2, . . . , 36. Indeed there is a resemblance to the situation at a casino table where the gambler makes note of the numbers that come up after each spin of the roulette wheel. In this manner we can generate distributions for each planet and each population of Subjects. Thus the distribution of Mars across the 12 sectors has been determined for the time of birth of 2,088 sports champions (Gauquelin, 1972).

Calculating Expected Frequencies

At first thought, the mapping of the sky I adopted might make it appear as if each planetary body would be found equally likely in any one of the N sectors defined. In general, however, the figure differs from such an average. The actual incidence for each sector is, to a greater or lesser degree, a function of specific astronomical and demographic factors. The demographic variance, for example, is primarily accounted for by the nonrandom distribution of births over the 24 hours (see Section B, below). Therefore, it is necessary for a statistical analysis to calculate the expected frequencies taking those factors into account. Numerous problems arise in these calculations, and a thorough individual analysis is required for each planet and each Subject population.

Consider, for example, the planet Mars and the 2,088 European champion athletes born between 1880 and 1945 (Gauquelin, 1972):

(A) The daily movement of Mars during that period, given a mean Northern European latitude of 47°N, was such that:

1. The probability for Mars to occupy either a diurnal or a nocturnal sector was nearly the same; Σ diurnal arcs/Σ nocturnal arcs = 1.02, slightly favoring the day time segments over the night. The minor difference is primarily due to the eccentricity of the Mars orbit and interacting zodiacal parameters (Gauquelin, 1957).
2. Mars is observed four times as frequently in conjunction with the sun as in opposition, which plays an important part in (B), below.

(B) Births are not evenly distributed over the 24 hours of the day. Rather, there is a maximum in the morning and a minimum in the afternoon. Furthermore, and referring to (2) above, Mars had a slightly greater probability (by 4%) to occupy a sector near its rise than near its setting, with culmination intermediate (−1%) (Gauquelin, 1955, 1957). Given a large number of births, distributed over a long period of time—as is the case with the athletes—the above analysis will enable us to create, in approximation, a "model sample" for the expected probability of Mars to occupy each of the sectors, and as determined by the several relevant astronomical and demographic conditions noted (Gauquelin, 1957, 1960, 1988). On the other hand, when the actual distribution of sector appearances and transitions differs substantially from the expected pattern—and this, again, is the case with the champion athletes—it is advisable to examine the circumstances surrounding every birth so as to ascertain whether idiosyncratic or "Subject variables" could account for the findings. We employ, therefore, the following procedure (Gauquelin, 1957, 1960, 1972, 1979, 1988).

Individual Births. For a given day and geographic locale the expected incidence for a planet to be in a specific sector is determined by several parameters, primarily:

1. The length of the semi-diurnal arc (or semi-nocturnal arc as the case may be) which is itself a function of the planet's declination and the geographic latitude; "semi-diurnal" or "semi-nocturnal" naturally refer to the distance between the rise and relevant (upper or lower) culmination.
2. The incidence of births from the moment of the planet's entry into, and until, its exit from that sector. *Example:* What is the probability for Mars to be in some specified sector on the day of birth of the sports champion, Robert Accard? The Subject was born in Lisieux, France, on November 26, 1897. On that day and in the particular location Mars rose at 7:27 a.m.; culminated at 11:41 a.m.; and set at 3:56 p.m. We can describe the planet's apparent movement on that date in terms of the times of its crossing each of the (here 12) sector boundaries. On the 24-hour clock we have, for Accard's birth specifications:

Sector No.	1	2	3	4	5	6	7	8	9	10	11	12
Mars entering sector at	0727	0852	1017	1141	1306	1431	1556	1831	2106	2341	0216	0452

The times spent in the respective sectors correspond to the associated astronomical probabilities. Taking into account as we need to do, the daily distribution of birth as such, we obtain the results of Table 1. This outcome is based upon many thousand births and the percentages with which "notable" persons were born over the 24 hours, each hour broken down into six-minute intervals. These figures, then, are the demographic probabilities in question. We can also make use of the table for determining the percentage of births normally occurring during the time that Mars occupied a specified sector. Again with Accard's birth data and place we have:

Sector No.	1	2	3	4	5	6	7	8	9	10	11	12
Expected percentage of births	6.25	6.10	6.62	5.67	4.94	5.33	9.94	9.62	10.46	10.19	12.46	12.34

The tabled outcome requires no further explanation. It indicates the astronomical *and* demographic likelihood for Mars to be in the various sectors, for every person—including Robert Accard—who was born in Lisieux on November 26th, 1897.

Aggregate Births. The procedure is repeated for all 2,088 sports champions. The expected probabilities for Mars to be in a given sector are obtained by summing the 2,088 individual probabilities calculated for that sector. The expected 12-sector distribution of Mars is the result of these calculations (Table 2).

It is also necessary, however, to ascertain that the 24 hour-pattern of births in the sports champions corresponds to the natural (general population) demographics. Actually, the more recent obstetric procedures tend to modify the natural (circadian) cycle of labor and birth (Gauquelin, 1959, 1971). Fortunately, the athletes were not born that recently, and their births still reflect a spontaneous pattern (Gauquelin, 1957, 1972).

TABLE 1
Cumulative percentage of births of notable individuals during 24 hours, by six-minute intervals

	Minutes									
Hours	0	6	12	18	24	30	36	42	48	54
0	0	0.15	0.29	0.44	0.59	0.74	0.88	1.03	1.18	1.32
1	1.47	2.05	2.63	3.22	3.80	4.38	4.97	5.55	6.13	6.71
2	7.29	7.81	8.32	8.83	9.35	9.86	10.37	10.88	11.40	11.91
3	12.42	12.88	13.34	13.80	14.25	14.71	15.17	15.63	16.09	16.55
4	17.00	17.47	17.95	18.42	18.90	19.37	19.85	20.32	20.80	21.27
5	21.75	22.24	22.73	23.22	23.71	24.20	24.69	25.17	25.66	26.15
6	26.64	27.13	27.61	28.09	28.58	29.06	29.55	30.03	30.51	31.00
7	31.48	31.92	32.36	32.80	33.24	33.68	34.12	34.56	35.00	35.44
8	35.88	36.32	36.77	37.21	37.65	38.09	38.54	38.98	39.42	39.86
9	40.30	40.72	41.14	41.56	41.97	42.39	42.81	43.23	43.65	44.06
10	44.48	44.96	45.43	45.91	46.39	46.86	47.34	47.81	48.29	48.77
11	49.24	49.71	50.17	50.64	51.11	51.57	52.04	52.51	52.97	53.44
12	53.91	54.30	54.69	55.08	55.48	55.87	56.26	56.65	57.04	57.44
13	57.83	58.16	58.50	58.83	59.17	59.50	59.83	60.17	60.50	60.84
14	61.17	61.54	61.92	62.29	62.66	63.04	63.41	63.78	64.16	64.53
15	64.90	65.28	65.66	66.04	66.42	66.79	67.17	67.55	67.93	68.31
16	68.68	69.09	69.49	69.89	70.30	70.70	71.10	71.50	71.91	72.31
17	72.71	73.09	73.46	73.83	74.21	74.58	74.96	75.33	75.71	76.08
18	76.46	76.83	77.21	77.58	77.96	78.33	78.71	79.08	79.46	79.83
19	80.21	80.56	80.91	81.26	81.61	81.96	82.31	82.66	83.01	83.36
20	83.71	84.10	84.50	84.89	85.28	85.67	86.06	86.46	86.85	87.24
21	87.63	88.01	88.38	88.75	89.13	89.50	89.87	90.25	90.62	91.00
22	91.37	91.75	92.13	92.52	92.90	93.28	93.66	94.04	94.43	94.81
23	95.19	95.67	96.15	96.63	97.11	97.59	98.07	98.55	99.03	99.51

It was empirically demonstrated that the daily distribution for ordinary people is quite similar to the distribution for notables. No appreciable difference in percentages is found between the two distributions (Gauquelin, 1972, p. 47).

Control Groups

Of course, the expected sector frequencies of Mars can be determined rationally (theoretically) as well as estimated empirically. For the latter purpose, we gathered 24,961 records of ordinary births from the same countries and time periods as the sports champions. These data have been published in their entirety as well (Gauquelin, 1970, 1972). The observed distribution of Mars for these control births does not significantly differ from the expected frequencies calculated by the procedure described above (Table 2).

Results

1. Statistical Evidence

The evidence for a "Mars effect," that is, the tendency for sports champions to be born more frequently when Mars is in Sector 1 (rise) and in Sector 4 (culmination) of the 12-sector division, can be cast in the form of a 2×2 contingency table (number obtained from Table 2):

	Mars in Sectors 1 & 4	Mars in Sectors Other Than 1 & 4
Champion births	452	1,636
Control births	4,296	20,665

For this table, $\chi^2 = 26.2$ which with one degree of freedom, yields $p < 10^{-6}$. Figure 2 is a graphic illustration of this highly significant result. (Note that this is the equivalent 18-sector mapping.)

This observation pertains to Mars and sports champions *only;* yet, however significant, it would not have been sufficient by itself to conclude that there is a correlation between planetary motion and time of birth of famous individuals. In fact, as briefly mentioned in the introduction, several other statistical analyses showed significant results not only for Mars but also for Jupiter, Saturn, and the moon.

As Prof. I. J. Good, Dept. of Statistics, Virginia Polytechnic Institute, remarks in the review of my account (Gauquelin, 1983):

> Among other striking results, $\chi^2 = 24.4$ for the birth times of outstanding physicians and men of science during the rise and culmination of Saturn, $\chi^2 = 29.2$ for military leaders and Jupiter, and $\chi^2 = 21.6$ for outstanding writers and the moon. In Good (1982) I tried to dwindle the Mars effect, partly by allowing for "special selection" of planet and attributes, and managed to get Bayes' factor down to about 60; but faced with the Saturn, Jupiter, and moon effects, the approach will clearly not undermine Gauquelin's results. (Good, 1987)

TABLE 2
Mars: Control group versus sports champions, 12-sector distribution

SECT	Obs	Exp	Obs	Exp
1	2234	2224.0	240	186,0
2	2210	2171.6	173	181,7
3	2116	2134.2	163	178,5
4	2062	2061.8	212	172,5
5	2113	2029.3	152	169,8
6	2025	1994.4	135	166,8
7	1886	1959.4	162	163,9
8	2023	1961.3	176	164,1
9	1975	2031.8	185	170,0
10	2017	2084.2	165	174,3
11	2214	2151.6	158	180,0
12	2086	2156.6	167	180,4
N	24961		2088	

Left: Observed and expected frequencies of Mars at the birth of 24,961 ordinary individuals. Right: Observed and expected frequencies of Mars at the birth of 2,088 sports champions (from Gauquelin, 1972).

As a matter of fact, the results mentioned by Good are extremely significant, and there are additional observations, not mentioned by him, that have emerged in the course of my studies (see Figure 3).

2. *"Key Sector" Boundaries*

I am frequently being asked in correspondence about the more precise pattern of "influence" of a planet along its path of motion. For instance, how does the effect increase with the births' proximity to the rise, or to culmination? At my request, Thomas Shanks, Research Director, Astro-Computing-Services, San Diego, computed the distribution of planets in 72 sectors for each professional group (my previous publications listed distribution primarily for 36 sectors).

The results published (Gauquelin, 1984) show that the two significant zones of the sky (insofar as the relationships are concerned) begin about 10° before the rise or the upper culmination; extend through the ends of sectors 1 and 4 (in the 12-sector mapping) and even slightly beyond, then rapidly lose their prominence. Since the significant zones exceed somewhat the Sector 1 and 4 boundaries, I now speak of "enlarged key sectors" or "plus zones." In the 36-sector arrangement these comprise four sectors surrounding the rise (nos. 36, 1, 2, and 3) and four at the upper culmination (nos. 9, 10, 11, and 12), respectively. Figure 1 should be self-explanatory. Investigators who have been examining my findings more recently generally work with the "enlarged key sector" definition for good reason since this procedure accounts for a greater proportion of the variance (Ertel, 1986, 1987, 1988; Müller, 1986).

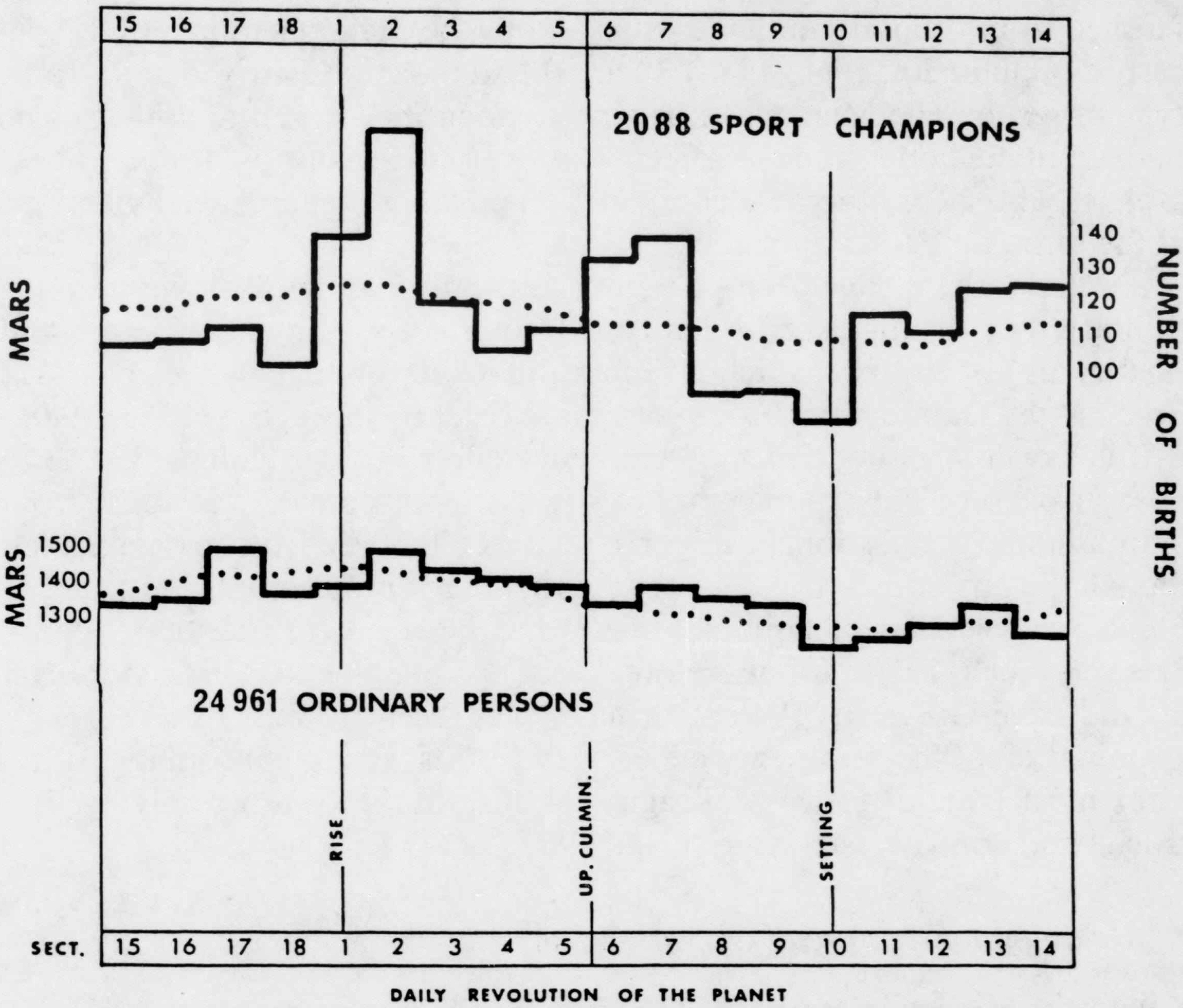

Fig. 2. Mars' distribution in 18 sectors for 2,088 sports champions (top) and 24,961 ordinary persons (bottom). Solid line: observed frequencies; dotted line: expected frequencies. Sports Champions were born significantly more often after the rise and the upper culmination of Mars; ordinary persons were not (from Gauquelin, 1972).

3. *The Eminence Effect*

Another fundamental finding discussed, as early as in my first book of 1955 might be referred to as the "eminence effect." For example (Gauquelin, 1973), I stated, "It is essential that a certain measure of success be achieved, that a certain threshold of fame be reached before positive results can be observed. Moreover, the greater the heights reached by an individual in his chosen profession, the more likely he is to have been born in 'planetary conformity' with his peers." As a case in point, consider the athletes. Along with the material on 2,088 sports champions, I assembled 717 lesser known athletes who were born during the same period of time. This group is comprised primarily of Italian soccer players who had participated in First Division games ("calcio Italiano") but never played in the national league. For this population Mars was calculated to have been in sectors 1 and 4, at birth, 124 times instead of 121.2 theoretically expected, and extremely close fit to the latter value. Athletes then who are not actually of champion caliber, even professionals like the Italian soccer team, cannot be distinguished from ordinary individuals insofar as the Mars phenomenon is concerned (Gauquelin, 1960, 1988). In another replication I arrived at the same type of result: The Mars effect in a newly identified group of athletes who had attained fame; and no effect whatever in a group of lesser known athletes who were otherwise comparable and whose records were obtained at the same time (Gauquelin, 1979).

Similarly, there have been only nonsignificant findings for lower ranking military, less distinguished scientists, "minor artists," and for actors and politicians not enjoying a major reputation. (Relevent details are given—in French—by Gauquelin, 1955, 1960; more recently in English, 1973, 1988.)

It is worth stressing that the "eminence effect" in particular is prone to raise suspicions. The criteria employed in distinguishing the "famous" from "nonfamous" professionals might be subject to biases on my part even though the procedures are well documented in my publications. As a case in point, my selection of famous athletes has been put in question (Kurtz, Zelen, & Abell, 1980). It is therefore a very positive step that Professor Ertel of Göttingen University (West Germany) has recently taken by a thorough examination of the "eminence effect." His results tend to confirm its reality. I am most grateful to this colleague for his efforts toward resolving that crucial and controversial issue (Ertel, 1987, 1988).

4. *Meaningful Structure of Overall Results*

Another important feature of the findings lies in the fact that the "profession versus planet" relationships are not scattered about in some "anarchic" fashion, as it were, but exhibit an internal or underlying "structure" that must be taken into consideration for a proper understanding of these results. (The existence of such a structure has been independently demonstrated by Ertel, 1986.) Figure 3 gives an overview of what is meant here by structure of results. Intuitively "similar" professions or activities tend to manifest comparable planetary arrangements as well. "Antagonistic" professions tend to have opposing planetary arrangements. For example, the "artists" can be contrasted with the "scientists"; scientists here are physicians, physicists, astronomers, chemists, and so forth. As a group, they tend to be born when Mars, or Saturn, had just risen or culminated. The artists comprise painters, musicians, actors and, to an extent, writers. As a group the eventual artists tend *not* to be born when Mars or Saturn are in the positions noted. Other traditionally antagonistic groups are soldiers and musicians, respectively. In our statistics, there are no other distributions of Mars so distinctly opposed as those of soldiers and musicians. On the other hand, there are professional populations which are mutually compatible in our sense. Such is the case with sports champions and soldiers. Consider that, in every area, sports has served in a somewhat preparatory function for war: Boxing, javelin throw, and archery remain popular evidence of the connection to this day. Now we find that champion athletes and soldiers are born under the same conditions of Mars' progression as well. Similarly, actors and politicians, both of whom function in a "representing" capacity and make headlines particularly often, tend to exhibit much the same, in this case Jovian birth schedules. Such a patterning of the results does seem to be meaningful although hardly transparent. In fact, it is necessary to examine the mentalities behind the simple occupation labels.

There is an interrelationship between personality and success. Many psychologists have made note of this connection. Character is an important part of success, and every profession has a typical psychological profile.

It would therefore, be fruitful to search for a connection between planetary position and personality traits which are typical of successful people. We know that folklore has Mars associated with energy and war. Folklore also associates Jupiter with extroversion. Such a correlation then would not surprise the believers of traditional astrology. On the other hand, I "wanted to prove scientifically that the true correlation lay not in the relation between planet and profession, but in the relation between planet and personality; and I also needed to find a scientific way of describing these planetary personality factors. To achieve this twofold goal, I intended using biographies of the outstanding professional people, from whom I had already collected all the birth and planetary position" (Gauquelin, 1983). I called

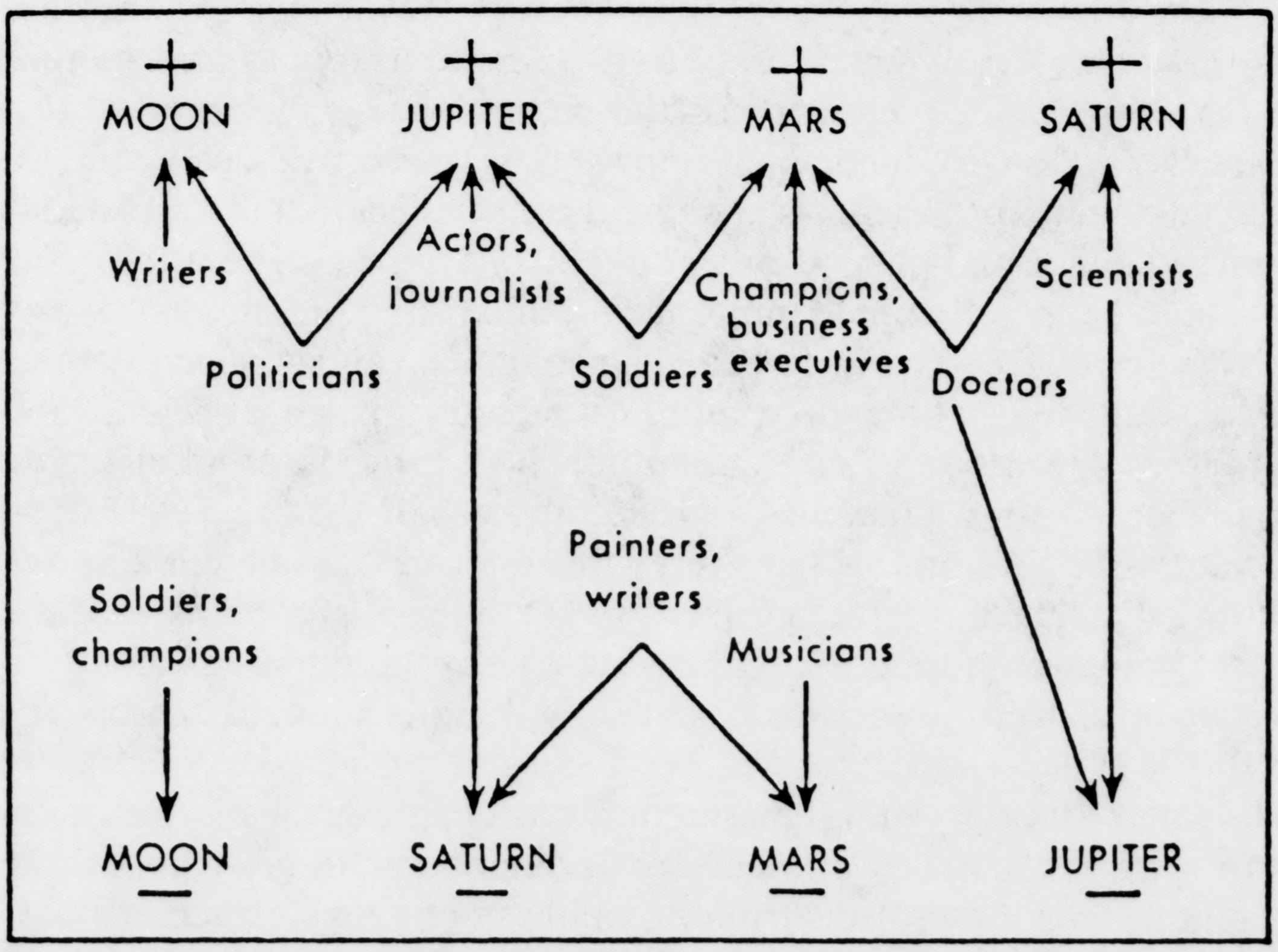

Fig. 3. Structure of the planet ≠ profession results. Plus signs (+): *maximum* incidence of births in the sectors after rise and culmination. Minus signs (−): *minimum* incidence of birth in those sectors. Arrows depict the characteristic bonds observed between professions and planets, respectively (from Gauquelin, 1955, 1973, 1988).

the methodology which I gradually developed from 1967 on, the "character trait method."

It is not the purpose of the present article to describe this method and its results. That would necessitate a separate paper. Suffice it to say that a 10-year study enabled us to create a catalogue or inventory of more than 50,000 personality trait units, specified item by item in our *Psychological Monograph Series* (Gauquelin, 1973–1977). The analysis based on this material reveals that the correlations between planets and personality traits exceed in strength these between planets and professions (Gauquelin, 1972, 1975, 1980, 1983; Gauquelin et al., 1979, 1981).

Response of the Scientific Community and Control Studies

Two general questions could be raised concerning my work: First, is the methodology sound? Second, do the results replicate? Over the years, and with few exceptions, control studies centered on the much-discussed Mars effect at the birth of champions athletes. Let me pursue this topic further in what follows.

Birth Data

These data have been scutinized several times, since the files of my laboratory are open to inspection. The observers concerned were apparently satisfied. Let me quote:

—Professors Abell, Kurtz, and Zelen: "One of us (Kurtz) did spot-check the data Gauquelin presented for the champions . . . Kurtz found that Gauquelin's files were meticulous and well organized, and on June 24, 1977 Gauquelin and Kurtz signed a statement to the effect that the files had been examined and found in good order" (Abell, Kurtz, & Zelen, 1983).

—Dr. Geoffrey Dean: "I personally visited Gauquelin's laboratory in Paris for a couple of days in 1981 and again in 1983 and was most impressed by the excellence and organisation of his records" (Dean, 1987).

—Professor Suitbert Ertel: "The author spent three days and nights in the Paris laboratory. Gauquelin was absent about half the time. All data files were accessible. Additional files were looked for in Gauquelin's absence, as he himself might not have recalled the location of everything at the moment. (None were found.) Circumstances are regarded as sufficiently conducive to discovering fraud or bias if something of the sort had occurred" (Ertel, 1987).

Note that data on groups other than sports champions have been submitted to scientific scrutiny as well. For instance, Professor Arno Müller of Saarland University (West Germany) carefully checked our records of German physicians. He wrote directly to the original registry offices, once again requesting the birth times in question. According to his results (the evident precision of the match) the possibility of fraud on my part—that is, a "manufacturing" of records—can be ruled out (Müller, 1986).

Computation of Parameters

The results released until 1972 were based upon planetary positions calculated manually since computers were not then available to me. Under the circumstances, of course, mistakes would be all too easy to make. Was I, then making such mistakes? In fact there have been several independent checks of such a possibility.

The Belgian Para Committee carried out computer checks of my (hand) calculations for their independently selected 535 champion athletes (see below); and "was unable to discover any mistake or error in Gauquelin's calculations nor [sic] the results he claimed" (De Marré, Comité Para member, 1982).

An American astronomer, Owen Gingerich, had my Mars calculations spot-checked for about 2,000 of the 16,756 nonchampion controls in the Zelen test (discussed later); and "no discrepancy was found" (Abell, Kurtz, & Zelen, 1983).

In 1980, Professor Abell, with the assistance of Albert Lee, calculated the Mars sector positions for our experimental population of 2,088 sports champions. Their finding was that "we differ from you only slightly, and the Mars effect clearly shows up on both sets of data" (Abell, 1980).

This suggests that, if I did make any errors, those are not major and cannot really affect the results. I recently carried out a complete check of my hand calculations on computer and again found no appreciable discrepancies (Gauquelin, 1984).

Expected Frequencies

Some critics have claimed that my results, the Mars effect in particular, merely reflect some astronomical or demographic artifact (e.g., Jerome, 1973). However, independent assessments of this issue, too, have taken place over the years at the hands of a number of investigators. These can be divided into theoretical analysis and empirical tests, respectively.

Two skeptics reviewed my methodology from a theoretical perspective:

—Dr. Jean Porte, Administrator at the Institut National de la Statistique et des Études Economiques (INSEE), Paris, carefully examined my approach to the problem for Mars and sports champions. He then stated, in his foreward to our methods volume, "I have looked for errors in the present work—and I have found none" (Porte, 1957).

—Twenty years later, a skeptical astronomer, Dennis Rawlins, wrote a memorandum in which he discussed the chief methodological objection to the Mars effect, that is, the demographic problem. Rawlins called it "the dawn factor" problem. Rawlins then rejects that objection on the grounds of astronomical and mathematical arguments: ". . . therefore, one concludes that Gauquelin has made fair allowance for the effect under investigation" (Rawlins, 1978).

Of course, all our tests and assumptions about the expected frequencies did have an empirical basis to begin with. Skeptics were generally unaware of my own numerous checks and controls, or else—understandably—they were not persuaded by those. They wanted to carry out control studies of their own and with their own procedures. It is here that the Para Committee's experiment and the Zelen test came into being.

Para Committee Replication

The Belgian Committee for the Scientific Investigation of Alleged Paranormal Phenomena (Para Committee) is composed of scientists including astronomers, demographers, and statisticians. This committee is extremely skeptical of, and strongly opposed to, the recognition of any paranormal phenomena. Unconvinced by my statistical documentations, the Committee decided to gather a new group of 535 sports champions; and in fact they obtained results quite similar to mine.

There have been many misunderstandings regarding the Para Committee's successful replication. Therefore, I would like to reproduce here the table published in the Committee's own report (Para Committee, 1976) (Table 3).

The Mars distribution observed for the new sample of the Committee's 535 champions is associated with a value for χ^2 of 26.66 which, with 11 degrees of freedom, is significant at the .01 level.* The Committee's report furthermore contains this unequivocal statement:

> The distribution of the actual frequencies of Mars is far from uniform: They display the same general pattern found by M. M. Gauquelin with samples of other sports champions, the main characteristics being a clear predominance in sector "1" (rising) above all the others. The Comité therefore gives its agreement on this point with the results of M. M. Gauquelin. (Para Committee, 1976, p. 331)

The Para Committee was, of course, greatly surprised at their own result. Jean Dath, a professor of engineering at the École Royale Militaire of Brussels, and Jean Dommanget, astronomer at the Brussels Royal Observatory, both of whom had worked actively on the project, subsequently began to question my methods even though they had agreed with those six years earlier. A discussion ensued regarding the calculation of expected frequencies; according to the Para Committee, a more adequate procedure would eventually reveal a fault or artifact—likely of demographic origin—such that the Mars effect could be accounted for by some "normal" cause. To its credit the Para Committee then undertook several counter experiments. The most significant of those is described in what follows.

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* It is worth stressing that this is a result that has actually been weakened by the Committee's format of a complete 12-sector breakdown. In view of the predictions made ahead of time it would have been technically preferable to test the significance of the observed frequencies in key sectors 1 and 4 (pooled) against the *sum total* of the other 10 (and $df = 1$).

TABLE 3
Para Committee replication. Mars' distribution at the birth of 535 sports champions

Classe (i)	Fréquence observée $f^{o}_{(i)}$	*Fréquence calculée* $f^{th}_{(i)}$	$\Delta f = f^{o}_{(i)} - f^{th}_{(i)}$	$\frac{(\Delta f)^2}{f^{th}_{(i)}}$
1	68	47,7	+20,3	8,64
2	47	46,9	+0,1	0,00
3	36	45,3	−9,3	1,91
4	51	44,0	+7,0	1,11
5	36	43,2	−7,2	1,20
6	30	42,7	−12,7	3,78
7	36	41,6	−5,6	0,75
8	50	42,2	+7,8	1,44
9	53	43,7	+9,3	1,98
10	54	45,2	+8,8	1,71
11	40	46,2	−6,2	0,83
12	34	46,4	−12,4	3,31
Total:	535	535,1		$26{,}66 = \chi^2$

Reproduction of Table 1 from the Para Committee report. Legend/translation, from left to right: 1st column: Classe = Sector; 2nd column: Fréquence observée = Observed frequency; 3rd column: Fréquence calculée = Expected frequency; 4th column: difference between observed and expected frequency; 5th column: square of the difference divided by expected frequency. For comments, see text (from Para Committee, 1976, p. 330).

Para Committee Counter-Experiment

A crucial test for evaluating hypothetical demographic or astronomical biases is to create a distribution of births which corresponds statistically to that of the champions' (i.e., the same year, month, day, place, and time of birth); but "shuffling" (systematically rearranged) the *hours* of birth: Each champion would keep, as it were, his actual birth date and place, but would be assigned the birth hour of, for example, the athlete preceding him in the alphabet. Exactly the same demographic and astronomical conditions, therefore, pertain to the group thus constituted as to the champions' population with its factual birth hours.

The Para Committee repeated this procedure nine times, each time systematically shifting the birth hours by a predetermined number of (alphabetical) steps. For example, in the first test, champion No. 4 keeps his real birth *date* and *place* but "receives" the birth *hour* of champion No. 3; and so forth for the others. In the second, champion No. 4 is assigned the birth hour of champion No. 2, No. 3 the one actually identified with No. 1; and so forth. In the third test, No. 4 receives the birth hour of No. 1, No. 3 now has that of No. 535; and continuing in this manner.

When the procedure is completed, the results are those shown in Table 4, which is taken from Dommanget (1970); *cf.* also Gauquelin (1972, 1982).

The distributions of Mars for the nine counter-experiments differ significantly from the distribution obtained with the real times of birth of the champions. Our conclusion is accordingly, that the Mars effect, again replicated by the Para Committee, cannot be considered a (procedural) error or demographic artifact. Moreover, the values in Table 4, column $f_{1,9}$ are very close to the theoretical (expected) values I calculated by my methodology and which were previously used by the Para Committee itself (see Table 3, third column).

That was not, surprisingly enough, the final conclusion in the Para Committee's report. Actually, the Para Committee discarded the results of their own counter-experiments. According to their rationale, it is "impossible" to calculate any expected frequencies for Mars because the problem is too complex. Without being more specific the report claims that I surely must have made some methodological mistake somewhere. Now it was the merit of the Zelen test to clarify the situation.

The Zelen Test

Professor Marvin Zelen of the Department of Biostatistics, Harvard University, suggested another experiment, later known generally as the "Zelen test" (Zelen, 1976). In Zelen's view that experiment should either prove or disprove the existence of the Mars effect. His rationale was as follows:

TABLE 4
Para Committee's counter-experiment for sports champions

					Classement Alphabétique						
cl	f_0	f_1	f_2	f_3	f_4	f_5	f_6	f_7	f_8	f_9	$f_{1,9}$
1	68	45	55	44	44	56	38	47	50	40	46,6
2	47	50	43	38	46	37	52	49	45	56	46,2
3	36	46	47	52	46	43	45	51	45	42	46,3
4	51	58	44	50	45	54	49	32	53	42	47,4
5	36	35	42	40	42	31	54	44	44	50	42,4
6	30	38	35	50	41	41	31	43	43	46	40,9
7	36	31	48	34	37	44	33	50	37	36	38,9
8	51	36	34	40	52	46	40	44	50	39	42,3
9	53	48	51	52	48	51	46	38	42	40	46,2
10	53	48	45	48	38	40	53	53	40	39	44,9
11	40	54	48	34	49	46	49	42	37	41	44,4
12	34	46	43	53	47	46	45	42	49	64	48,3
	χ^2	33,0	24,9	36,1	32,2	21,6	40,8	43,1	25,8	60,4	25,4
	p	—	0,8%	—	—	3%	—	—	0,6%	—	0,7%

Explanations and comments: "Classement alphabétique" is alphabetical order. From left to right: *cl* = Mars sectors; f_0 = actual distribution of Mars at the birth of the champions; f_1 through f_9 = distributions for the nine counter-experiments; $f_{1,9}$ = means of the nine counter-experiments, by Mars sector. The bottom rows marked χ^2 and p designate the chi-square statistic and its probability under the null hypothesis, respectively. Values are obtained by comparing the actual distribution, f_0, with the respective distribution of each counter-experiment, $f_1, f_2, \ldots, f_9$. All nine differences are significant: Those between f_0 and f_1, f_3, f_4, f_6, f_7, and f_9 are significant at $p < .001$. The remainder range from $p < .05$ to $p < .01$. The overall comparison between f_0 and $f_{1,9}$ (last column) yields $p < .01$ (after Dommanget, 1970).

Supposing the Mars configuration at the birth of champions is nothing but the consequence of an artifact, then all nonchampions born on the same day and in the same place as the former ought to exhibit the same phenomenon —that is, they, too, should have been born more frequently at the rise and culmination of the planet (the "key sectors"). One merely needed to contact the registry offices of the birth places of the champions and request the *hour* of birth of everyone born on the same date and thus under identical astronomical and demographic conditions as those. Calculations of the positions of Mars at the hour of these additional births would yield the answer desired.

I agree to carry out the test under the close supervision of Zelen, Kurtz and Abell, managing to gather 16,756 birth hours of nonchampions born in the same week (i.e., ±3 days of the target date) and in the same places as 303 sports champions. The latter were drawn from the total of 2,088, using an objective procedure of which Zelen had been apprised beforehand.*

I then sent photocopies of all birth records received from the registries to Paul Kurtz, chairman of the Committee for the Scientific Investigation of Claims of the Paranormal (CSICOP). Results of the test were published (Gauquelin, 1977). They provide an unequivocal answer within the framework of Zelen's reasoning: It is that Mars occupies "key sectors" significantly more frequently at the champions' births than is noted for the large number of other individuals, whose births occurred on the very same days and in the same places as the former. Table 5, reproduced from the Zelen test report, gives the main empirical evidence in a numerical format; Figure 4 is a graphic analog of this Table (when rotated 90° clockwise).

Eventually, the three CSICOP members involved in the Mars control studies, Professors Abell, Kurtz, and Zelen, would acknowledge that ". . . Gauquelin adequately allowed for demographic and astronomical factors in predicting the expected distribution of Mars sectors for birth times in the general population" (Abell, Kurtz, & Zelen, 1983).

Discussion

What is the present status of the Mars effect? In its favor are the considerable statistical significance, the satisfactory checks of the procedures, the independent replication by the Para Committee, and the results of the Zelen test. So then, is there really a Mars effect?

It is only fair at this point to mention that Kurtz, Zelen, and Abell conducted still another study on a fresh sample of 409 (U.S.) athletes, this time with negative outcome (Kurtz, Zelen, & Abell, 1979/1980). Personally, I do not consider this finding a real setback since the investigators failed to take the factor of eminence into adequate account: This factor, however, is

* "Michel Gauquelin had long before sent him (Zelen) three detailed descriptions of the sampling procedure which were entirely straightforward and barred Gauquelin himself from influencing the data" (Professor Richard Kammann, 1982).

TABLE 5
Results of the Zelen test

Each of the Seven Days Taken Separately						
	Nonchampions		Champions			
Day	*N* Number of Births	*k* Mars in Key Sectors	*n* Number of Births	*y* Mars in Key Sectors	*p'* 303 (*k*/*N*)	CR
−3 days	2,302	347	—	—	46	3.2
−2 days	2,354	382	—	—	49	2.7
−1 day	2,485	436	—	—	53	2.0
0 day	2,341	373	303	66	48	2.8
+1 day	2,460	422	—	—	52	2.1
+2 days	2,449	395	—	—	49	2.7
+3 days	2,365	390	—	—	50	2.5

Comments: For each of the seven days centering on the champion's birth the observed frequency of Mars for champions (=66; column *y*) is significantly higher than the expected (empirical) figures for the nonchampions. The latter are listed in column *p'* and range from 46 to 53. The probability under the null hypothesis, of the difference between 66 and each of the expected frequencies (46, 49, etc.) can be determined by way of the respective critical ratios (CR). The seven values of CR (last column) correspond to *p*-levels ranging from .002 to .05, two-tailed. The outcome for "0 day," the champions' exact birthdate, is most interesting in the context: The appearance of Mars in key sectors exceeds the expected one to a particularly remarkable extent. The "expected" value is of course based on the nonchampions born in the same locale and on the same day. The likelihood of that difference under a hypothesis of "no effect" is less than .006. The overall results would seem to be the best confirmation that methodological errors cannot explain the Mars effect (from Gauquelin, 1977).

of paramount importance in the phenomenon at issue. Kurtz, Zelen, and Abell have maintained, on the other hand, that their sample does represent successful athletes sufficiently well. Professor Ertel on his part recently demonstrated that in the U.S. sample too many lower ranking athletes were aggregated with two few exemplary ones. He also showed that the more renowned those American Subjects, the more prominent also the Mars effect (Ertel, 1987).

Consequently, I believe that the U.S. study, too—although of limited significance in itself—tends to substantiate the Mars effect for outstanding champions (Gauquelin, 1979/1980). This assessment is shared by reviewers of the American tests (Curry, 1982; Eysenck, 1983).

May I conclude by saying how well I understand the skepticism of scientific investigators confronted with a claim like the Mars effect? I myself cannot but agree with the late astronomer, George Abell, as he wrote in his foreword to my *Dreams and Illusions of Astrology* (Gauquelin, 1979):

To be honest, I am highly skeptical of Gauquelin's findings and his hypothesis. The main reason is I cannot imagine a mechanism whereby the effect can be produced.

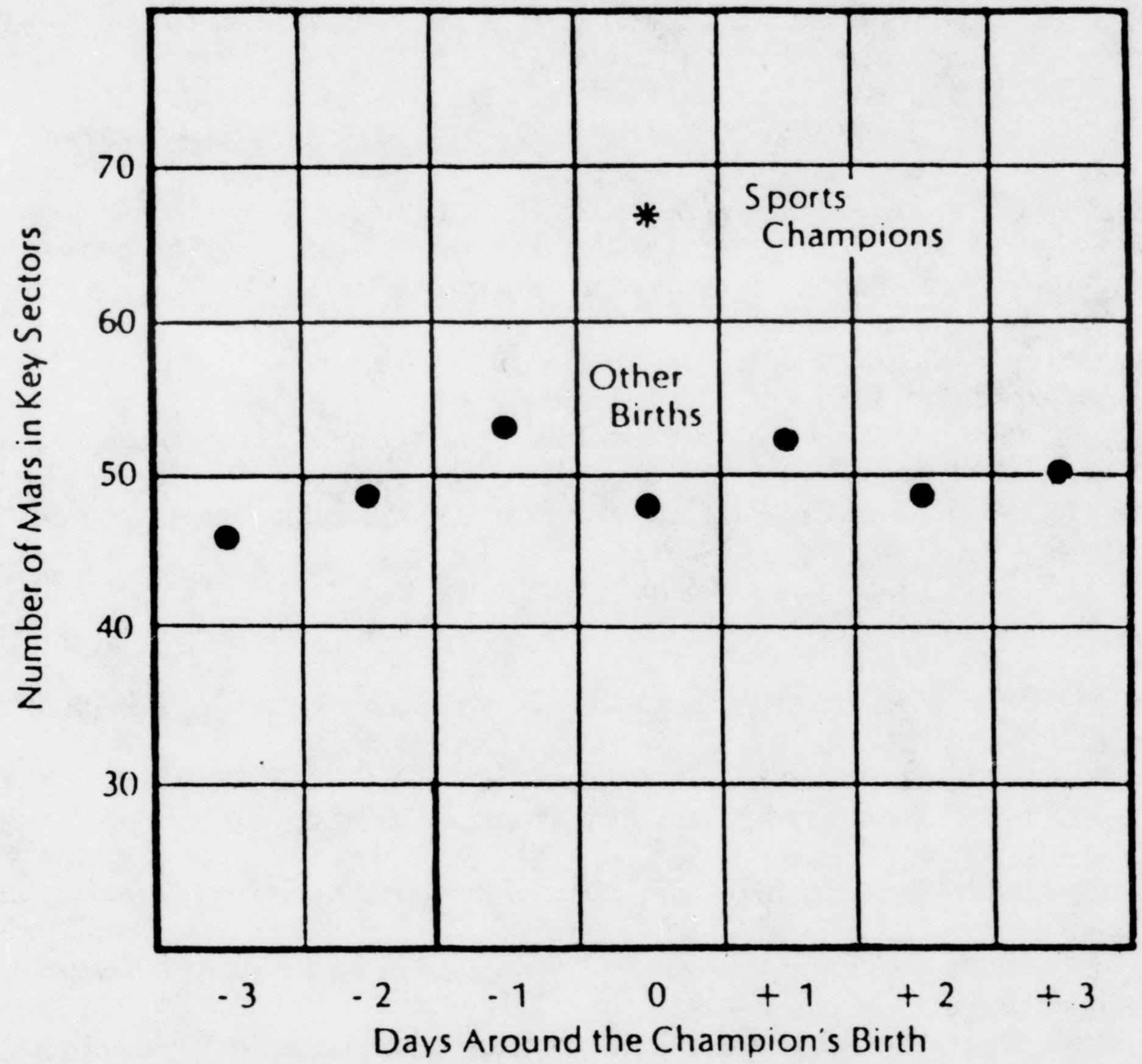

Fig. 4. Zelen test. Mars in key sectors for sports champions versus other births. The observed frequency (= "number" in graph) of sports champions' births with Mars in key sector (*) is significantly higher than the expected number calculated from nonsports champions born in the same places, relative to each of the ±3 days considered (from Gauquelin, 1977).

However, I do not *know* that the effect is not there; my skepticism cannot be considered closed-mindedness, any more than a gullible acceptance of astrology should be regarded as open-mindedness. If the planetary effects suggested by Gauquelin *are* real, then his discovery is of profound importance. Consequently, I think the Gauquelin evidence, based on a great mass of data collected over many years, deserves to be checked out. (Abell, 1979)

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Raising the Hurdle for the Athletes' Mars Effect: Association Co-Varies With Eminence*

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Abstract—By 1955, Michel Gauquelin had begun to publicize the claim that famous athletes are born with frequencies far beyond chance at times when Mars is rising over the Earth's horizon ("key sector I") or when the planet crosses the meridian ("key sector II"). Critics did not succeed in refuting this claim empirically: The "Mars effect" survived three such attempts. It was largely doubts over the impeccability of M. and F. Gauquelin's data base, however, which kept researchers from pursuing the problem further. The present study incorporates the entire repertoire of birth data of athletes available to date (N = 4391). The objective is to test the alleged planetary correlation as a function of degree of sportive eminence, the latter being determined by citation counts. It is contended that this procedure is superior to Gauquelin's own; and that the predicted eminence function could hardly be expected to materialize in case his former results were due to biased data treatment. Findings corroborate the eminence prediction: The proportion of athletes born at Mars key sector hours increases from the lowest to the highest of five ranks of sporting eminence; the trend is highly significant ($p < .005$) by several criteria. It is concluded that Gauquelin's hypothesis, after having passed this crucial examination, deserves the most thorough attention.

Introduction

From 1955 on, Michel Gauquelin has been claiming to have evidence for a perplexing "astro-psychological" relation: Frequencies of births of eminent professionals are said to deviate from chance expectation in particular ways. Athletes, for example, are allegedly born more frequently than expected by chance when Mars is rising over the Earth's horizon (i.e., when the planet transits "key sector I") or when it passes the meridian ("key sector II," see

* *In memoriam* George A. Abell († Oct. 7, 1983) whose "sincerity, honesty" and whose "respectful manner" of treating the principle victim of the previous Mars effect drama was explicitly noted by Piet H. Hoebens. And, *in memoriam* Piet H. Hoebens († Oct. 22, 1984) who wished that CSICOP would soon get "a chance to prove that the Mars effect fiasco has indeed been an isolated lapse."

"There are lessons which I am sure all of us involved have learned very well: When investigating an allegedly paranormal claim, we must take it seriously, think of it thoroughly, use professional care, as we would in real science . . ." (G. A. Abell in his "epilogue," addressed to "actors, producers, and drama critics," May 1st, 1982).

Figure 1). Gauquelin offers a detailed account of this anomaly as well as of the methods used to ascertain its existence in this same issue of the Journal (Gauquelin, 1988). The present contribution deals with the second in a series of attempts to test the supposed association in a more rigorous manner. (For the first study, see Ertel, 1986.)

For what follows, it is necessary to divide Gauquelin's assertion into two. The first is the more general one, namely, that there are relations between planet key sectors and certain professionals' births at all. The second specifies that the association becomes more pronounced as professional achievement increases. The latter proposition is stronger than the former and more consequential, thus preferred for a critical test. If substantiated, it would, by implication, be tantamount to confirmation of the first claim as well. Likewise, its disconfirmation would seriously weaken the first claim to which the second has been closely linked. Testing the first hypothesis without regard for the second could not yield equally convincing results.

A critical survey of previous investigations by skeptic observers of Gauquelin's work may attest to the usefulness of this reasoning.[1]

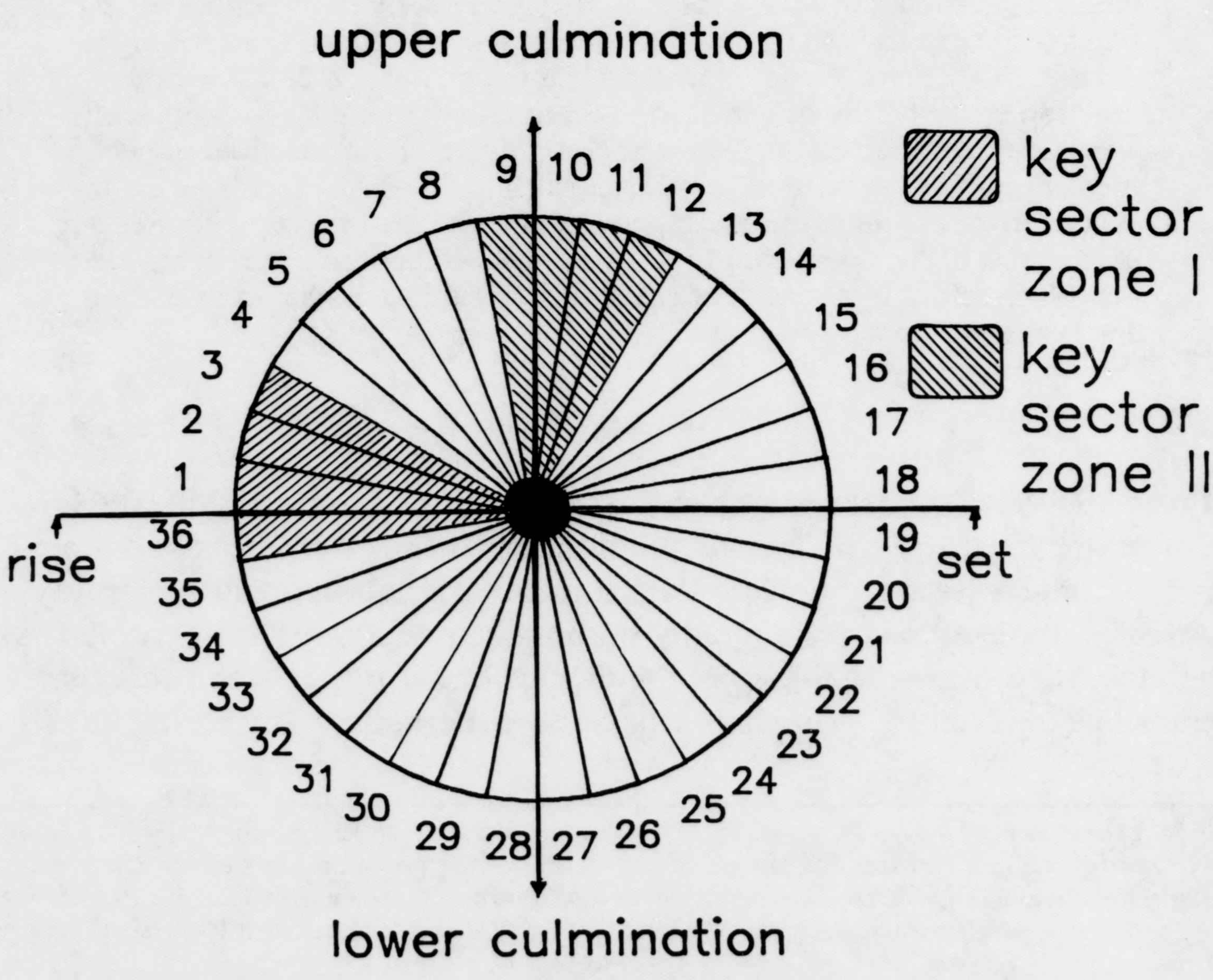

Fig. 1. A 36-sector arrangement for plotting the apparent daily movements of heavenly bodies. Sector nos. 1–18 define motion from rise over the Earth's horizon to setting; sector nos. 19–36 apply to motion beneath the horizon. Sector nos. 36, 1–3 (zone I) and sector nos. 9–12 (zone II) are called key sectors; planetary positions in these zones are claimed to be correlated with birth frequencies of certain professionals.

Evaluation of Previous Attempts

The Zelen Test

The first of Gauquelin's claims has been subjected to the "Zelen test." Zelen's approach circumvented certain statistical complications, in the context of obtaining theoretical key sector frequencies, which had been elaborated by the Belgian Para Committee (1976). Following Zelen's design, birth hours of controls drawn from the general population were tallied ($N = 16{,}756$). The controls had been matched for date and place of birth with 303 athletes drawn from the data pool of Gauquelin's champions ($N = 2088$). For the subsample of 303, the proportion of athletes born during key sector passage of Mars (= key sector-proportion, kS) was ${}_kS_e = .218$; ("e" for experimental subsample). This value was thus representative of key sector proportion of the total (${}_kS_E = .217$; "E" for total experimental sample; see Gauquelin & Gauquelin, 1977).

Zelen's test sought to obtain one empirical information only: ${}_kS_C$, that is, the key sector proportion for the matched control group ("C" for control). Comité Para and those who took their side in this investigation, predicted ${}_kS_C = {}_kS_E = .217$. After having equalized astronomical and demographic conditions of C with E, and in the absence of Mars correlation, ${}_kS_C$ should not differ from ${}_kS_E$. Gauquelin, on the other hand, using his standard procedure for calculating theoretical key sector-proportions (${}_kS_G = .167$; "G" for Gauquelin procedure) predicted ${}_kS_C = {}_kS_G = .167$.

Zelen obtained ${}_kS_C = .164$ for the controls, that is, Gauquelin's procedure of calculating theoretical kS proportions had stood the test. To quote A-K-Z: "The results of the Zelen test suggest that Gauquelin adequately allowed for demographic and astronomical factors in predicting distribution of Mars sectors for birth times in the general population" (A-K-Z, 1982, p. 82).

Michel and Françoise Gauquelin concluded that the Mars effect had been corroborated; the difference of key sector proportions between experimental and control group ${}_kS_E - {}_kS_G = .218 - .164 = .054$, which was shown to be highly significant, could no longer be attributed to some hidden astronomical-demographic mediation.

A-K-Z, on the other hand, were not inclined to endorse the Gauquelin interpretation at this point—the reason being that, "We were not sure that the sample selected by Gauquelin was unbiased" (A-K-Z, 1982, p. 80). It is not unreasonable to regard the Gauquelin interpretation as not compelling since Zelen's test was to find evidence for an artefact of the kind Comité Para had suspected. It did not, at the same time, rule out another alternative which could possibly explain the Mars peculiarity, namely selection bias and/or data fraud.

The athlete population ($N = 2088$) from which the subsample of $N = 303$ had been drawn, contained 1553 champions selected entirely by Gauquelin himself (the remainder under the supervision of Para Committee). Although inspection of Gauquelin's archives (by P. Kurtz), and some scrutinizing of his statistical methods (by R. Chauvin, E. Scott, D. Rawlins) had not supported suspicion of problems in this regard, the immense weight of the Gauquelin claim which—to use Abell's words—"would lie beyond anything that science can at present understand" (Abell, 1982, p. 7) is ample grounds to maintain, as long as possible, less incredible explanations of those results: For example, "We can imagine ways that bias could have entered without intentional cheating" (Abell, 1982, p. 11). The point is clearly phrased in Kurtz and Abell's final comment on the Gauquelin issue: Uncertainty remains concerning the objectivity of his original data selection. (And) "that is why we cannot confirm the significance of his statistical analysis supporting the 'Mars effect'. This is . . . the crux, and perhaps the Achilles heel of the 'Mars Effect' dispute" (Kurtz & Abell, 1983, p. 88).

In their first report on the Zelen test, however, A-K-Z did not yet address the essentials. Instead, they were preoccupied with doubts concerning the objectivity of Gauquelin's selection of his experimental subsample. They pointed to some seemingly disturbing variability of kS proportions within this group of 303 athletes.

The question of sample homogeneity, although meriting a study of its own, was actually beside the point. To the extent that it is deemed relevant at all, the total population of athletes should be divided into meaningful subgroups in order to look for internal variance. Moreover, heterogeneity in the athletes' sample chosen—if it was heterogeneous—does not necessarily mean lack of sampling objectivity.[2] Finally, inferential statistics were applied *ad hoc* and not always with the necessary care. Criticisms were justified: Twenty-one contributions to the Mars effect dispute were published in *Zetetic Scholar,* Nos. 9 (1982), 10 (1982), and 11 (1983). The majority raised objections. In their reappraisal six years later, A-K-Z did their best to set the records straight.

The U.S.-Athletes Study

There are two approaches to the bias or fraud problem in accounting for the Mars anomaly. The first is Gauquelin-independent replication. This strategy led to K-Z-A's study of U.S. athletes. The second is Gauquelin-independent analysis of Gauquelin data. This strategy led to the study reported below.

K-Z-A's study with U.S. champions was a hopeful next step. However, the researchers committed errors of their own, the most critical being that they did not take Gauquelin's eminence tenet seriously enough. They did not try convincingly to select highest ranking athletes. No "bickering" (Abell) would have occurred as to whether or not the individuals listed in the four *Who's Who* volumes consulted there, were sufficiently eminent if CSICOP's data assemblers had tried in earnest to optimize their sample (see Ertel, in press-b).[3] A-K-Z, in their reappraisal, regret that they "had not obtained in advance a clear understanding with the Gauquelins on exactly what they were predicting and what directories of famous sports champions would be satisfactory according to their hypotheses . . . there were no written agreements" (A-K-Z, 1983, p. 81). The authors could have regretted still more: They did not give the alleged effect the best possible opportunity for revealing itself—if it does exist—or for being shown a product of Gauquelinean data shuffling if that was really what it was. K-Z-A failed to come to firmer conclusions because they focused on the first, that is, the less specific of Gauquelin's claims. Testing the second claim with appropriate precision, therefore, would appear to be a more promising approach.

The Eminence Study

The Study Objective

The hypothesis to be tested is this: The relative tendency for athletes to be born more frequently during kS-passages increases monotonically with their level of sporting eminence.

Gauquelin himself did not scale athletic success beyond two levels. In four studies (1955, 1960, 1979, 1982) he distinguished at most between more and less generally successful figures. He dichotomized the groups by *ad hoc* criteria which did not always meet the standards of objectivity. Moreover, he would change criteria from one study to the next—this procedure was also criticized by A-K-Z in their "Contradictions" article (1980).

Gauquelin's lack of rigor in determining eminence, however, turns out to be an advantage for the present purpose. The rationale here was to create conditions that would logically exclude an explanation of positive results in terms of Gauquelin's data handling. Eminence will be objectively defined by frequencies of citation. If the Mars effect is real and if eminence is adequately reflected in citation counts, then a systematic increase of kS-percentages (kS%) with eminence must be observed.

Such an increase of kS% with eminence could hardly be attributed to Gauquelin's selection technique. One may of course still imagine that the "effect" can be obtained, in principle, by fudging. A cheater might secretly count citations and then "inject" carefully dosed data of individual athletes with appropriate Mars positions, while discarding others with inappropriate positions. Accordingly, kS%-values could be intentionally altered so as to "properly" co-vary with citation counts.

The technical requirements for such a fraud would be immense, however. At the time of Gauquelin's athletes work there were, for one, no personal computers. More important still would be the psychological contradictions inherent in such an operation. Supposing a fraud was in fact committed, the cheater would have gone public with its outcome and would hardly have waited for decades hoping for someone else to come across the product of his surreptitious activity. It is difficult enough to give credence to a "Mars effect" to begin with. Yet to assume that any investigator would spend a lifetime faking data while at the same time trying to camouflage the very masterpiece of this undertaking is hardly less unbelievable.

The Data

All athletes data available were gathered for the study, that is, birth and associated Mars sector positions, the total amounting to $N = 4391$. Considering the atmosphere of suspicion in Mars effect discussions, particular transparency of method is called for in this instance. The following description of the present material is, therefore, rather detailed. Specific features of the samples are noted in Table 1. (For their contributions in terms of numbers see column G.) Asterisked entries involve notations. The letters in parantheses in the following paragraphs refer to Table columns.

Comments on Entries of Table 1

1: "First French." (E–G) The data were first published without planetary sectors data, in an appendix to IA, 1955. The section was headed "570 sportifs" (the number of athletes actually listed was 568). The data were again published, with planetary sectors, in A1 (1970), with one deletion (an erroneous birth date; M. Gauquelin, personal communication, May 10, 1986).

(J) No subdivisions by sporting success are found in this study; rather, Gauquelin dealt with distinctions among sporting categories or areas, including: cyclists, boxers, team athletes, soccer players, and "other athletes," for special analyses.

2: "First European." (D–G) Gauquelin analyzed a sample of 915 non-French European athletes in his HA (1960) study. The data, however, were included only in the A1 collection (1970). Between 1960 and 1970, casual data gathering for European athletes continued ($N = 274$). Subsample No. 2 increased in size to $N = 1189$.

(J) Success was considered here. Seventeen biographical sources were used. Fourteen of those were regarded as listing athletes of renown. Athletes taken from three sources (German) were regarded as less renowned ($N = 117$) ("because criteria for selection were missing" p. 262). For one source (the Italian soccer *Almanacco*), Gauquelin used a breakdown; Those who played at least once on the national team were classified as renowned ($N = 98$), and those who never advanced to the national team were judged less renowned ($N = 600$). This information came directly from *Almanacco.* Apparently, the selection was objective.

3: "Italian Soccer." (E) The data of "less renowned" players were not published. Gauquelin gave this author permission to take the original index cards back to Göttingen University for manual transcription.

(J) (see comments on Italian soccer in 2-J, above).

4: "German Various." (E) Comments to 3-E, above, apply here.

(J) The criterion for regarding athletes as "not renowned" is weak. Athletes from this source were separated for lack of reliable criteria (see comment to 2-J, above).

TABLE 1
Description of the athlete populations (I).
Components of the total ordered chronologically by date of collections

A	B	C	D	E	F	G	H	I	J
No.	Name of the Sample	Responsible for the Collection	Time of Data Collection	Year(s) of Publication U = Unpublished	Publication source or archive	N	National-ities of Athletes	Sport. Categ.	Eminence Criteria
1	"First French"	GAUQ	1951–1955	1955, 1970*	IA, A1*	567 (568)*	F	V (28)	N*
2	"First European"	GAUQ	1955–1960 (70)*	(1969), 1970*	(HA), A1*	1189 (915)*	I, G, B, N	V (9)	S, Do*
3	"Italian football"	GAUQ	1956	U*	GL	600	I	F (1)	Do*
4	"German various"	GAUQ	1955–1960	U*	GL	117	G	V (5)	S*
5	"French occasionals"	GAUQ	1955–1975	U*	GL	204	F	V (24)	J*
6	"Para champions"	Para Committee	1962	1970	AI (PC)*	332 (535)*	F, B	V (24)	Do*
7	"Para lowers"	Para Committee	1962	U*	LG	76 (241)*	B	F (1)	Do*
8	"CSICOP-U.S."	CSICOP	1978/79	1979/80, 1982	SI, D10	192 (409)*	U	V (18)	S, J*
9	"Second European"	GAUQ	1978/79	1979	D6	450 (435)*	F, I, B, G, N, S	V (19)	Do, S, J?*
							Sc, L	C (1)	S*
10	"Italian cyclists"	GAUQ	1968/69	U*	GL	24	U	V (23)	Do, J?*
11	"Lower French"	GAUQ	1978/70	U*	GL	455 (432)*	F	V (21)	S*
12	"GAUQ-U.S."	GAUQ	1981/82	1982	D10	158 (351)*	U	V (16)	J?*
13	"Plus-specials"	GAUQ	1982–1986	U*	GL	27	F		
			Total published		2888				
			Total unpubl.		1503				
			Sum Total		4391				

Note: Column C: GAUQ = M. Gauquelin, partly with assistance by F. Gauquelin.

Column F: IA = L'influence des astres (1955). HA = Les hommes et les astres (1960). A1 = Birth and planetary data, Series A, Vol. 1 (1970). D6 = Scientific Documents, Vol. 6 (1979). D10 = Scientific Documents, Vol. 10 (1982). SI = The Skeptical Inquirer, Winter 1979/80, pp. 60–63. GL = Gauquelin Laboratory at Paris (LRRCP). PC = Para Committee at Brussels (for details see References).

Column H: B = Belgian. F = French. G = German. I = Italian. L = Luxemburgian. N = Dutch. Sc = Scottish. S = Spanish. U = U.S.-American.

Column I: F = Football (Soccer). C = Cyclists. V = Various (numbers indicated).

Column J: N = No breakdown by eminence. S = Sources are considered as listing predominantly less renowned or renowned athletes. Do = Athletes drawn from a source are dichotomized using objective criteria, such as whether a football player had or had not been selected for a national team. J = Personal judgement, no sufficient indication of objective criteria which would allow for a replication.

Asterisks refer to "Comments on Entries of Table 1."

5: "French Occasionals." (E) The data were not included for publication in Gauquelin's 1970 (A1) sample of athletes. The author copied them manually at Gauquelin's laboratory, from the latter's original index cards.

(J) These athletes, Gauquelin said, had been taken from heterogeneous sources (newspapers, lists of teams, etc.) and were judged as "low-low-ranking" by him.

6: "Para Champions." (F, G) The number of renowned athletes selected by the Para Committee was 535. Since Gauquelin had already 203 athletes from the Para sample in his earlier studies (1955, 1960), only 332 are gained towards the present totals.

(J) Comité Para selected athletes, using objective criteria of success (see also 7-J, below).

7: "Para Lowers." (E) These data (Belgian soccer) were neither in the Para Committee's file nor listed or published by Gauquelin. The index cards were deposited in Gauquelin's archive; the author transcribed the information manually.

(G, J) N = 241 soccer players had not been included in the "Para champions" sample because they had participated in fewer than 20 international events. The athletes had been ranked in the archive by participation at international championships. It was for only 76 out of those 241 discarded players (rank Nos. 1–76), that Mars sector information was indexed; for the remainder (N = 165) no planetary data were tabulated.

8: "CSICOP-U.S." (G) The number of U.S. athletes in this study was 409. K-Z-A published Mars data using a 12-sector scale. Since the present study required 36-sector scaling, only those K-Z-A athletes were suitable from this source whose data had later been published by Gauquelin (N = 192) using the 36-sector division. For the rest of the K-Z-A-sample (N = 217) more precise Mars sector data were not obtainable.

(J) Considering the "bickering" over selection of U.S. athletes referred to earlier, the criteria of selection remain doubtful. They may have changed between categories S (all champions of a source are considered) and J (subjective judgements led to inclusions or deletions).

9: "Second European." (G) Gauquelin analyzed the data of 435 champions. In an appendix to D6 he listed 15 additional athletes whose birth dates had been received too late for inclusion. They were added to the present pool.

(J) Much effort is devoted in this study to describing the sources and criteria used for selection (see appendix, pp. 25–28, of D6 [1979]). The main source was *Dictionnaire des Sports,* from which 82% of the athletes of the final sample were drawn. Gauquelin regarded all non-French European athletes listed in the *Dictionnaire* as renowned and included them in the sample unless they had already been used in previous investigations (HA 1960, Para Committee). In order to increase the sample of non-French athletes, Gauquelin consulted 12 additional sources. The numbers extracted there are not given individually; the criteria of selection, however, are briefly described in most cases. Since the main source (*Dictionnaire*) contains a majority of French athletes, with a greater likelihood of the less renowned to be included, Gauquelin discarded many of these, referring to objective criteria where he could. For individual sports he retained only French winners of Olympic medals or of World and European Championships. In respect to French team sports, he kept soccer players who had played at least once on the national team. More than 10 participations with French national teams was set as a requirement for other team sports (basketball, handball, rugby).

10: "Italian Cyclists." (E) The data were copied manually at Gauquelin's laboratory.

(J) Cyclists listed only in the Italian *Velo 1968 and 1970* journal but in no other sources, were not included in DG (1979), as Gauquelin considered them low ranking.

11: "Lower French."(E) The data were copied manually at Gauquelin's laboratory.

(G) The original number of French athletes who were discarded was 432. However, the data of 23 additional athletes of lesser rank had been collected by 1986, raising the sample N of this category to 455.

12: "Gauq-U.S." (G) Gauquelin's U.S. Sample consisted of 351 athletes. As a subsample of 192 had already been used in the K-Z-A-study (see sample no. 8), 159 newcomers were added to the present pool.

(J) Gauquelin made an effort to secure outstanding individuals. Ten sources were used with *Who's Who* directories not regarded as appropriate. After having decided that a given source was to be used, Gauquelin said, no entries were discarded. The reasons for the low final number of athletes are described in detail. They appear to be circumstantial and not related to eminence.

13: "Plus Specials." (E) Gauquelin mailed these sets to the author after his return from Paris. Inadvertently they had not been handed over. There are sports figures from former French colonies and from Paris arrondissements where birth records were difficult to obtain. Gauquelin indicated he tried hard to obtain the data since the athletes in question were famous, and he was successful in 27 cases.

(J) The basis for selection according to Gauquelin: "From criteria of *Dictionnaire des Sports* I consider them as high ranking" (personal communication, May 10, 1986).

Overall Assessment of Data Base

Four evaluative statements will summarize the issues:

1. The total sample of athletes ($N = 4391$) is large enough for breakdowns by degree of eminence.

2. The number of unpublished athletes data is considerable ($N = 1503$). Six unpublished samples are Gauquelin's own (nos. 3, 4, 5, 10, 11, 13), one is the Para Committee's (no. 6). As a rule, it was the data of less eminent athletes that had been excluded. The Gauquelins have reported totals of four unpublished samples (nos. 3, 4, 10, 11) giving rationales for the exclusions. On the other hand, two other unpublished samples (nos. 5, 13) were not known to exist until now. Moreover, in the case of two published samples—nos. 2 and 9—the totals have grown since publication. Finally, Gauquelin has reported no results at all for unpublished samples nos. 5, 7, and 13. A skeptic might suspect that all of the above leaves some room for manipulation. The likelihood of coming upon respective evidence has increased.
3. Eminence criteria used by Gauquelin for distinguishing top athletes from lower ranking ones are not consistent. He did apply objective criteria, but these changed over time. There are also instances of nonrepeatable ratings. Informing the reader about criteria of selection does not rule out the possibility of bias. Moreover, discarding individuals entirely without stating a principle of selection—as occurred in no. 4—seems dubious practice.
4. Gauquelin did not hesitate to make available his unpublished data, including those of whose existence the author was not aware. The data was copied from his files, printouts were returned to him for verification of accurate transcription. Gauquelin thus supported the author's attempt of gathering all existing records irrespective of their previous use. Three days and nights were spent in the Paris laboratory, with Gauquelin absent about half of the time. All his files were fully accessible. Additional athlete records were looked for in Gauquelin's absence—with his permission—as he himself might not have recalled the location of all at the moment (none were found). The author believes he would have detected traces of manipulation if Gauquelin had in fact made special attempts to "make" Mars related to the athletes' births.

Citation Technique

Citation frequency is an objective criterion for renown or eminence. In scientific publications, important references tend to be cited more frequently than minor ones. Thus, the Science Citation Index has been used to determine the eminence of scientists or scientific institutions (Elkana, 1978).

In the present study, citation counts are to define the eminence of sports figures. In the process, some difficulties arise, however. Imagine first, ideal conditions: A large number of independent biographical sources are at one's disposal. Every source covers every field of sports, every nation having participated in international championships is represented. Sports categories, nations, and time periods are considered in balanced proportions.

More successful athletes have a greater chance to be included than those of lower ranks.

In a separate study when writers and painters were graded for eminence, the author encountered conditions almost ideal for determining eminence by citations (Ertel, 1987). Sources for athletes were not as satisfactory for this purpose (see Table 2, below, along with the comments there). The main difficulty was that the number of sources with more general scope was small. For individual sports categories, therefore, additional sources had to be screened in order to raise the overall level of citation frequencies. In the Gauquelin data pool, however, sports categories are unevenly represented (see Table 3). Citations for athletes in different fields may thus be affected by the mere number of individuals who happened to be included in Gauquelin's population. It would have been desirable to counterbalance these differences by using differential numbers of more or fewer screening sources

TABLE 2
Description of the screening sources:
Screening sources used to obtain indices of citation frequencies

No.	Author	Title	Year of Publicaton	Sporting Category	Informat. Displ.	N Hits Publ. Sample	N Hits Unpubl. Sample	N Unique Hits
	A	B	C	D	E	F	G	H
1	Le Roy, B.	Dictionnaire des Sports	1973	various	A, I	1029	488	668
2	Garcia, J. P. et al.	La fabuleuse histoire (4 Vol.)	1973/78	various	A	556	33	51
3	Kamper, E.	Lexikon der 12000 Olympioniken	1974	various	A	305	48	60
4	n.a.	Stars des Sports	1970	various	A	229	29	12
5	Faßbender, H.	Sporttagebuch des 20. Jahrhunderts	1984	various	I	181	4	9
6	Soderberg, P. et al.	The Book of Halls of Fame	1977	various	I	140	3	34
7	n.a.	Sporthöhepunkte	1980/83	various	I	134	7	6
8	Umlauf, L.	World Almanac Book	1980	various	I	90	0	32
9	Newman, G.	The concise encyclopedia	1979	various	I	65	0	3
10	Gronen, W. & Lemke, W.	Geschichte des Radsports	1984	cyclists	I	80	3	25
11	n.a.	Dictionnaire du cyclisme		cyclists	A	*	67	16
12	n.a.	World cup 1974	1974	football	T	122	15	53
13	Chambe, R.	Histoire de l'aviation	1980	airplane	I	88	3	91
14	Gordon, R. & Goldman	The Ring	1981	boxing	T	28	0	9
15	Watman, M.	Encyclopaedia of track & field	1981	track & f.	I	144	20	18
16	Cimarosti, A.	Auto-Rennsport	1973	tennis	T	25	6	11
17	n.a.	Tennis-Jahrbuch	1984	tennis	T	27	1	1
18	n.a.	Skiweltmeisterschaften St. Moritz	1974	ski	T	16	5	1

Note. Column A: n.a. = No author's name given.

Column E: A = Alphabetical directory, biographical articles. I = Alphabetical name index, used for the present purpose. T = Table(s) of sports events. Chronological listings and/or sporting records (converted into alphabetical order for the present purpose).

Column F: * = Missing data: The list of published athletes was not available at the time the unpublished athletes were screened.

TABLE 3
Description of the athletes' sample (I).
Ranked frequencies of athletes in sporting categories

No.	Sporting Category	Published Samples	Unpublished Samples	Total	%
1	Football (Soccer)	685	788	1473	33.5
2	Cycling	534	136	670	15.3
3	Rugby	267	147	414	9.4
4	Track and field	231	183	414	9.4
5	Aviation sports	391	6	397	9.0
6	Boxing	248	7	255	5.8
7	Auto-motor sports	49	58	107	2.4
8	Basketball, P.d.B.	90	8	98	2.2
9	Tennis	76	15	91	2.0
10	Swimming	42	20	62	1.4
11	Skiing	42	2	44	1.0
12	Fencing	18	20	38	0.9
13	Golfing	27	4	31	0.7
14	Baseball	25	1	26	0.6
15	Weight lifting	21	3	24	0.6
16	Equestrian sports	14	9	23	0.5
17	Gymnastics	13	10	23	0.5
18	Rowing	8	15	23	0.5
19	Hockey	14	6	20	0.5
20	Wrestling	13	7	20	0.5
	10 add. categories (each < 20)	80	58	138	3.1
	Total	2888	1503	4391	100.0

Note: From KZA-athletes (N = 409) only a subsample of N = 192 could be used whose sporting categories were taken from M. Gauquelin's 1982 publication.

for different sports. In addition, frequencies of citations for athletes of different categories should have been weighted in order to equalize their contributions. Finally, the uneven national contributions (see Table 4) should have been taken into account. Understandably, these desiderata could not all be realized in practice.

The following particulars of Table 2 might seem overly detailed for less skeptical readers. Some aspects of the Mars effect debate, however, make it advisable to forestall the ambiguities which brevity would bring about.

General Comments on Table 2

Screening sources nos. 1–9 each cover numerous sports categories. Sources nos. 10–18 deal with only one each. Sources nos. 1–9 are arranged here, in descending order, with respect to the number of Gauquelin athletes in those entries. Sources nos. 10–18 are similarly arranged with respect to the proportions of sports categories within Gauquelin's athletes' sample. The numbers of sources chosen for individual sports classifications depend

TABLE 4
Description of the athletes' sample (II).
Nationalities for the published, unpublished, and total samples

No.	Nationalities	Published Athletes	Unpublished Athletes	Total	%
1	FRA	1357	683	2040	46.5
2	ITA	703	625	1328	30.2
3	BEL	323	76	399	9.1
4	USA	351	0	351	7.9
5	GER	37	117	154	3.5
6	NET	60	2	62	1.4
7	SCO	43	0	43	1.0
8	SPA	10	0	10	0.2
9	LUX	4	0	4	0.1
Σ		2888	1503	4391	100.0

on the importance of the respective categories within Gauquelin's athletes pool.

Gauquelin athletes can be identified, without error, in the sources containing biographical articles arranged in alphabetical order (A, see Column E). Mere indexes of names are less dependable for identification: they lack birth information, for one, and the identity of such names in Gauquelin's compilation does not always justify assuming an identity of persons. On the other hand, the person doing the matching may also fail to notice factual identity. Uniqueness of a name—the most salient criterion for the identity of an individual with entries in different places—may be misleading. There may be trivial differences in the first names (e.g., Bill rather than William); two initials instead of one; different spelling or certain classes of surnames; double and/or hyphenated surnames during a woman's career, and so forth. The records were consulted where doubts arose, but these were not resolved in every instance.

The citation "hit" figures are given separately for published (N = 2888) and unpublished athletes (N = 1503) (see columns F and G). Gauquelin tended not to publish data associated with athletes of lower achievements. Fewer "hits" in column G, relative to column F, was the result throughout.

"Unique hits" (column H) are recorded in the particular source and not replicated in others. For example, excessively few unique hits occur with source no. 9—this is understandable, since concise encyclopedias of athletes generally reference only the most significant figures; yet those would likely appear in other sources as well. An extremely large number of unique hits was found with source no. 13, that is, airplane sports champions. Even outstanding performers in this category are not generally regarded as members of the general sporting scene. In contrast to ground-, water-, and snow-, or ice-based sports, airspace activities are not typically regarded as an arena of competition.

Comments on Individual Sources (Table 2)

(For full biographical references see Appendix 2.)

1. The *Dictionnaire des Sports* was Gauquelin's main source for obtaining athletes' names, birth dates and birth places. The number of hits are large, not only for published samples, but also for unpublished ones. This observation will be dealt with below. (The book was on loan from M. Gauquelin.)
2. "Editions O.D.I.L." published four volumes of *Fabuleuses Histoires* (for rugby, soccer, cyclists, and track and field). An appendix to each offers a collection of biographical articles in alphabetical order, devoted to the most eminent athletes of the respective fields. ("Gotha Français" and "Gotha international"). (The books were located in Gauquelin's laboratory and were perused there.)
3. Kamper's *Olympioniken* book lists gold, silver, and bronze medal awardees for all Olympic Sports. The book went through several updated editions, and English and French translations exist. An addendum to Kamper's book with the winners of the 1976 Olympics was also used.
4. *Die Stars des Sports* is an ideal screening source. Nearly 6,000 outstanding athletes are listed alphabetically whether they excelled at Olympics or other international championships. A national bias, if at all present, is probably much less pronounced than with the *Dictionnaire des Sports*. (The book is out of print; a xerox copy was available.)
5. The scope of Fassbender's *Sporttagebuch* is a broad one (its chapters deal with 13 sports categories). An obvious intent here was to cover all outstanding achievements and to give balanced historical accounts (At the public library, Göttingen.)
6. "Halls of Fame" athletes are the Best of the Year as elected by local or regional institutions in the U.S.A. and Canada. In each sports field (even angling, billiards, softball, dog racing) the most famous or popular persons are chosen. In rare cases, a foreign champion may also be elected (as was the German boxer Max Schmeling). Since each local group (e.g., "North Dakota Golf Halls of Fame," "Arizona Horse Racing Halls of Fame") has its own "Halls of Fame" selection, the average achievement level of the total pool is comparatively low. (At the Sporthochschule, Köln.)
7. The *Sporthöhepunkte* provides lists of winners of international championships for eight sports fields, in chronological order. (At the Göttingen public library.)
8. The *World Almanac* provides biographical articles on "sports personalities," pp. 209–224. Some of the most outstanding sports figures have been selected. The range is broad in scope with respect to nationality, sports category, and historical period. (The book was borrowed from Gauquelin.)

9. "Newman's concise encyclopedia" is comparable in quality to the *World Almanac.* (At the Sporthochschule, Köln.).
10. *Gronen* offers a narrative chronology of bicycle races (1899–1939); its scope is international. Cyclists listed in the name index were generally considered more outstanding than those not cited. (At the Sportseminar of Göttingen University.)
11. The *Dictionnaire du Cyclisme* is a biographical dictionary on cyclists only. Biographical information was neglected. A large number of names are provided by the index. (The author had located this reference during his final visit to the Köln Sporthochschule. As he planned to screen unpublished athletes in data sets brought from Paris, the list of published cyclists was not needed for that purpose, therefore not at hand. Consequently, there are missing data in column F.)
12. The appendix of *World Cup* identifies all soccer players who took part in the Final Rounds of World Cups 1939–1974. Every one of them was included. (At the public library, Göttingen.)
13. Chambe's *History of Aviation* includes an index with the names of all individuals who made outstanding contributions to the field in terms of records and achievements. (At the Sporthochschule, Köln.)
14. The index of *The Ring,* a voluminous encyclopedia, lists names of all boxers referenced in individual articles of the book. The most successful champions are identified by bold face type, and only these were considered here. A national bias (U.S.) is apparent. It was not possible to locate a comparable source for sufficiently large numbers of European fighters. (At the Sporthochschule, Köln.)
15. Watman's *Encyclopaedia on Track & Field Athletics* contains biographical articles in alphabetical order. For the most part a name index was used. Its coverage is broad and balanced, although with some national bias. (At the Sporthochschule, Köln.)
16. The appendix to *Cimarosti* lists Grand-Prix winners since 1906 in chronological order. All were considered. (At the public library, Göttingen.)
17. From *Tennis Jahrbuch* the names of Wimbledon champions (single, double, both sexes; 1877–1983). (At the Sportseminar of Göttingen University.)
18. *Skiweltmeisterschaften St. Moritz* details the records set in World Alpine Championships (1931–1971). For each year the names of the first ranking skiers were used. For the period 1966/67 to 1972/73, the first 50 ranks of total Alpine achievements are tabulated, and names were likewise included. (At the public library, Göttingen.)

General Evaluation of the Citation Measure

The screening sources used in this study do not, in their entirety, meet the ideal requirements stated earlier. The citation index to be derived will be less reliable than it should be. Its reliability might be improved by utilizing additional sources. More such sources do exist. However, some were unavailable (e.g., the Lincoln Library of Sports Champions). Some accounts, though available, are in languages outside the author's expertise (Russian, Hungarian). In any case, greater investment along those lines would not necessarily pay off. It was decided to discontinue further searches after 18 sources had been assembled. (Four volumes of O.D.I.L. editions are counted as one, but are listed individually in Appendix 2, hence there are 21 entries there instead of 18.)

Regarding procedure, the following applies: (a) No source was rejected once it had been decided upon, and only those athletic books on sports were discarded at the outset which failed to meet minimal criteria. (b) Athletes' names were recorded without omissions. (c) Data were compiled without knowledge of the individual's planetary sectors. (d) Screenings carried out by one person were generally checked by another, except for two directories not accessible in the Göttingen area. (e) Anyone interested can readily check the identification of Gauquelin athletes in the source materials, by requesting printouts with hits noted, from the author. In addition all 14 sources or copies of indexes from nos. 3 to 18, excluding 8 and 11, are available on a loan basis.

In sum: The present procedures are regarded as sufficiently objective. No selection bias can have influenced the number of hits defining eminence. A measure being less than optimally reliable cannot lead to an error that would in turn favor the Mars phenomenon. Rather, it would blur the effect —assuming it does exist. Only if analysis suggests an acceptance of the null-hypothesis (i.e., no Mars effect) would the reliability of the procedure still have to be improved as a safeguard against wrong conclusions.

Analysis and Results

Key Sector Definition

In previous research on planet-birth relations, two definitions for the "key sector" have generally been in use, namely, a 12-sector or a 36-sector definition. Key sector zones of the 12-sector breakdown cover only three fourths of the key sector zones defined by the 36-sector division: that is, key sector I, defined on a 12-sector basis, does not include sector 36, key sector zone 2 does not include sector 9 of the 36-sector division (see Figure 1).

The 12-sector scale was in use throughout the Mars effect debate. In the present study, however, the 36-sector division is preferred. The decision is justified empirically: Frequencies of births for Mars sectors, using the 36-sector scale, were calculated for the total of athletes (N = 4391). As seen in Figure 2, birth frequency in sector 36 (first column, left) is closer to the mean frequency of subsequent key sectors 1–3 than to the mean of the preceding nonkey sectors 33–35 (the last three columns, on the right). Simi-

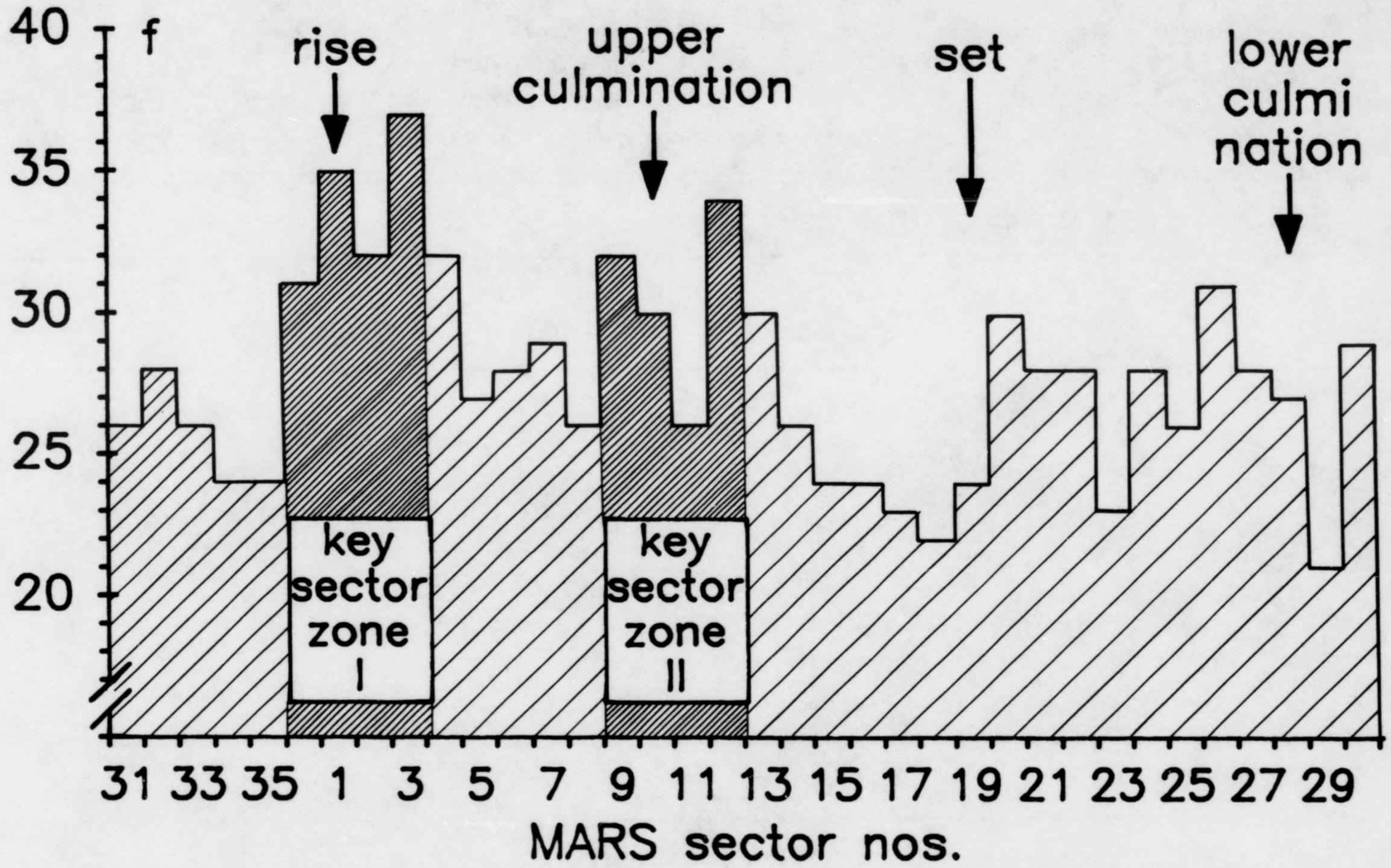

Fig. 2. Birth frequencies of athletes (total sample $N = 4391$) as Mars is crossing sectors 1 through 36. (For key sectors I and II definition see Figure 1.)

larly, birth frequency in sector 9 is closer to the mean frequency of subsequent sectors 10–12 than to the mean frequency of the preceding nonkey sectors 6–8. The advantage of using a 36-sector division has been demonstrated more extensively elsewhere (Ertel, in press-a). The present sector definition by itself does not favor, in some nonlegitimate way, the eminence hypothesis examined below.

Defining Eminence Ranks

Although 18 screening sources were used in this study, the maximum number of citations achievable by an athlete is only 9, since 9/18 sources contained information for just one sports category each. The empirical range of citation frequencies ($c = 0 \cdots 8$) and their distribution for the total is shown in Table 5. The comparisons suggest that Gauquelin's decision to exclude certain samples of athletes in his publications must have been based, indeed, on their low level of eminence.

Rather as expected, as citations become more frequent ($c = 0, 1 \cdots 8$), their occurrence N_c becomes correspondingly less frequent ($N_c = 2271, 1100 \cdots, 3$). To avoid low reliability of rare events, subsamples of frequent citations were suitably combined. Table 5 shows that pooling the counts $N_c = 4 \cdots 8$ results in $N = 232$. Athletes achieving ($c \geq 4$) are assigned the top rank of 5; rank 4, preceding, accounts for 253 cases; and so forth.

In one of the analyses to be presented, subsamples are drawn from the total group of athletes. In these cases ranks 4 and 5 have been combined in order to maintain an acceptable level of reliability (Confidence limits of ${}_kS\%$

TABLE 5
Description of the athletes' sample (III).
Citation frequencies (N_c) for published, unpublished, and total samples and frequencies of ranks (N_r) for total sample

	Frequencies of citations (N_c)				
	Samples:				Frequencies
c	Published	Unpublished	Total	Ranks	of ranks (N_r)
0	1331	940	2271	1	2271
1	657	443	1100	2	1100
2	462	73	535	3	535
3	210	43	253	4	253
4	96	4	100		
5	78	0	78		
6	33	0	33	5	232
7	18	0	18		
8	3	0	3		
Σ	2888	1503	4391		4391

Note: N_c-values are converted into ranks 1 through 5.

values for samples with $N < 250$ are generally too broad. Gauquelin himself objected to Zelen's parsimony in sampling for this very reason.)

The validity of citations as a measure of eminence may be judged to an extent *post hoc* by glancing over the names of 50 top athletes listed by descending rank of citation (see Appendix 2). Readers somewhat acquainted with the history of records in sports should find among the top 50 a large majority of figures well known for successful international competition. More accurate assessment requires expertise, however.

Another test of the validity is to determine, for each level of citation frequency (c), the proportion of Olympic winners of medals (Kamper's *Who is Who in the Olympics,* which is used for identification here, had to be excluded). The percentages of such Olympians at levels of $c \geq 6, 5, 4, 3, 2, 1$, and 0, are as follows: 57.1, 44.8, 46.5, 20.8, 13.8, 7.6, and 2.6, respectively. The corresponding totals are: 23, 58, 99, 228, 521, 1126, and 2331, respectively. It can be seen that, for example, 57.1% of those athletes (absolute count, 23) with six or more citations also distinguished themselves at the Olympics. Likewise, 44.8% of those with at least five citations (absolute number, 58) had demonstrated Olympic excellence. The proportion of Olympic champions in subsamples of still lower citation rates dwindle rapidly. It is a mere 2.6% of Olympic athletes who failed to achieve a single citation as defined, in addition to their being listed in Kamper's *Who is Who.*

Testing the Main Hypothesis

Table 6 shows the main result. Out of 2271 athletes classified as lowest in eminence, $f_{ks} = 555$ were born during Mars key sector passages (i.e., their

TABLE 6
Results: Frequencies of Mars key sector cases (f_{ks}), key sector percentages ($_kS\%$), and deviations from expectancy (kS%-E%), by athletes' ranks

Ranks	N	f_{ks}	Confidence Limits 95%	$_kS\%$	Confidence limits 95%	$_kS\%$-E% E% = 22.2
1	2271	555	±40	24.4	±1.76	2.22
2	1100	275	±28	25.0	±2.56	2.78
3	535	146	±20	27.3	±3.77	5.07
4	253	76	±14	30.0	±5.65	7.82
5	232	75	±14	32.3	±6.01	10.11
Σ	4391	1127		$\bar{X}$ = 25.7		

$_kS\% = 24.4$). This figure exceeds chance expectation by 2.22%, ($_kS\% = 100 * 36/8 = 22.2\%$; an approximation corresponding to the 16.67% approximation used previously for 12-sector scale calculations). Key sector percentages increase, monotonically, with rank of eminence. Deviation from chance level is in fact greatest for the top ranking individuals (i.e., 10.11%, see also Figure 3).

A powerful test for monotonic trend with ranked qualitative data is based on Kendall's tau coefficient (see Marascuilo & McSweeny, 1977). Kendall's tau = .037, z = 2.669, $p < .005$. Level of eminence, ranked 1 through 5, is plotted on X; the key sector status appears on Y.

Finally, a test for monotonic trend of ranked proportions was performed, based upon a Chi square rationale, as suggested by Fleiss (1981, pp. 147–149) who refers to its first description by R. E. Barlow. The Chi^2 statistic for the data is 10.42, m = 5 proportions, and located within the $p < .005$ range with lower boundary $\chi^2 = 9.784$. (The value of χ^2 may not be referred to tables of χ^2, see Fleiss, p. 148.)

There are k*(k-1) = 15 differences among proportions for k = 5 ranks. These were also tested individually using Cohen's arcsine transformation and effect size index h. Significant effect sizes were noted between ranks 1 and 4 (h = .126, $p < .05$); between 1 and 5 (h = .176; $p < .01$); between 2 and 4 (h = .11, $p < .05$), and 2 and 5 (h = .16; $p < .05$); respectively. The associated power values of the normal curve test (1-β) are calculated as .41, .41, .41, .38, respectively (see Cohen, 1977, p. 179ff).

Comparing Unpublished with Published Data

At this point the question of possible bias in data compilation should be reexamined. If Gauquelin had excluded those athletes whose birth hours did not match the kS passages of Mars so as to obtain an eminence trend for kS%, the latter could hardly have emerged in the present instance. The reason is that all athletes whose data had originally been excluded from publication and/or anlaysis have now, too, become part of the study population.

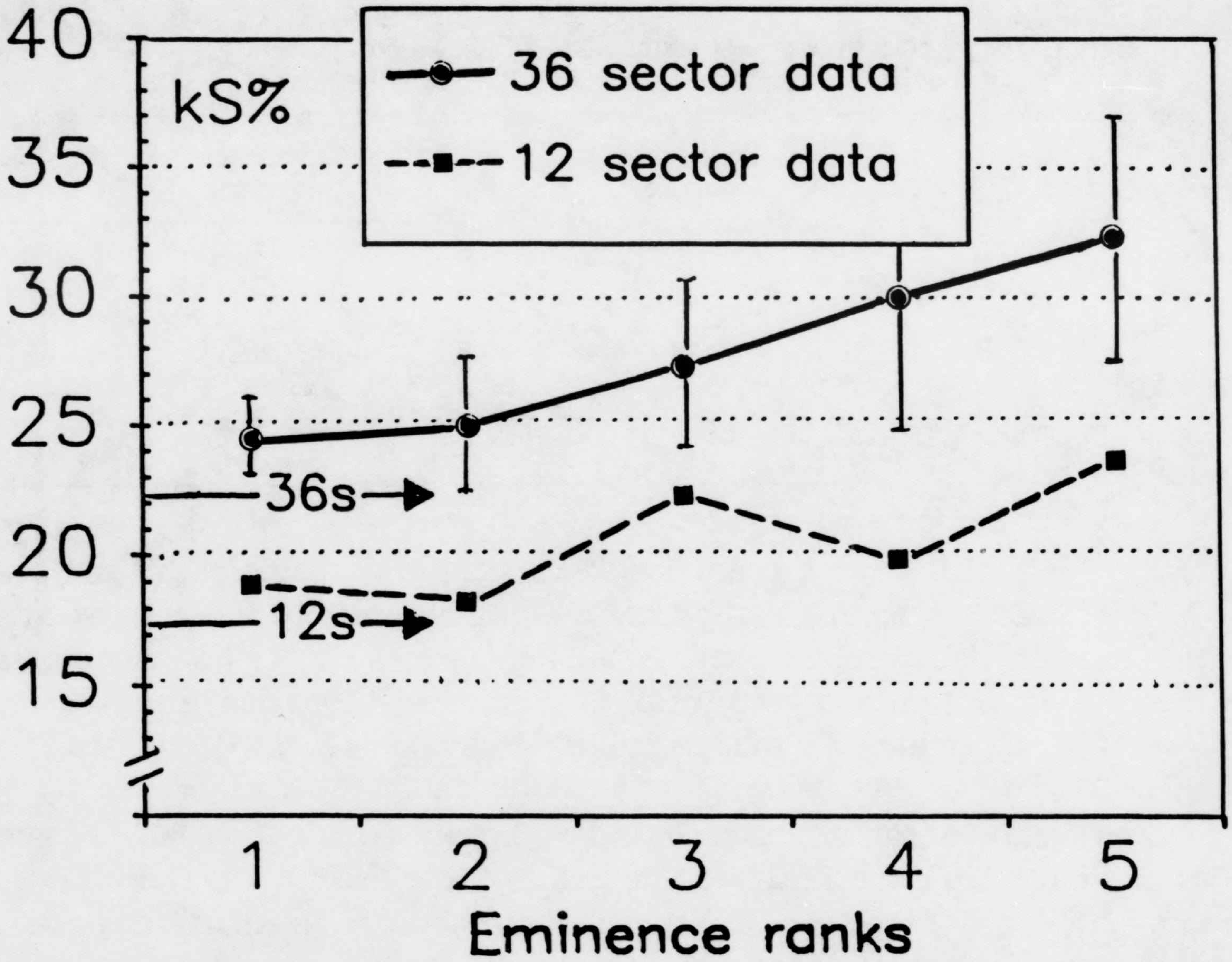

Fig. 3. Main result: key sector percentages increase with eminence. *Solid line:* Mars key sector percentages (kS%), with kS-definition derived from the 36-sector scale for athletes of five eminence ranks (5 = highest rank) based on citation frequencies (N = 4391) (for details see Table 6). The vertical bars show the possible variation for p = .95. (The ranges of confidence increase with eminence ranks, which is due to decreasing number of individuals [see Table 6].)

Dashed line: Mars key sector percentages (kS%) using kS-definition derived from 12-sector scale.

Nevertheless, the question deserves more detailed consideration. Selection bias is more likely to enter if decisions to discard or not to discard individuals are made by someone who is aware of their Mars sectors at birth. Suppose Gauquelin had excluded athletes without knowing their planetary positions, the resulting aggregates of published samples should not differ from unpublished ones with respect to kS%, but only with respect to total frequencies. That is, athletes achieving few citations should merely be more numerous, those with more citations should be less in number in the unpublished samples as compared with published ones.

A difference of absolute numbers has in fact been noted earlier (see comments, Table 5). However, in comparing kS% for published and nonpublished samples, we find, in addition, a substantially lower kS% level among the latter (see Figure 4). The respective differences of kS% for ranks 3 and 4 may be disregarded since among unpublished athletes there are very few with higher ranks. The kS% figures for less eminent athletes, ranks 1 and 2, on the other hand, involve numerous unpublished records. Here we find strong statistical support for a difference in kS% between published and unpublished samples: χ^2 exceeds chance levels at $p = .006$ for rank 1, $p < .005$ for rank 2. The discrepancy does seem to indicate that Gauquelin's knowledge of kS condition at the time he made the decision to segregate high from low achievers had an influence on his sample.

This conclusion, however, is not as certain as it might seem. The difference in question could stem, in part or even entirely, from an advantage of Gauquelin's carefully judging the subjects' achievements as described in their biographies. Directory citation is only more objective in assessing eminence; at the same time it is less sensitive. Gauquelin's taking into account detailed biographical information might have been more appropriate for excluding athletes of lower standard from the study sample. Assuming that the Mars effect exists, it would follow that among a sample of athletes with equal citation count individuals discarded due to relatively poor items in their sports career would yield a lower average kS% compared to those who, after using the same criteria, were selected for analysis and

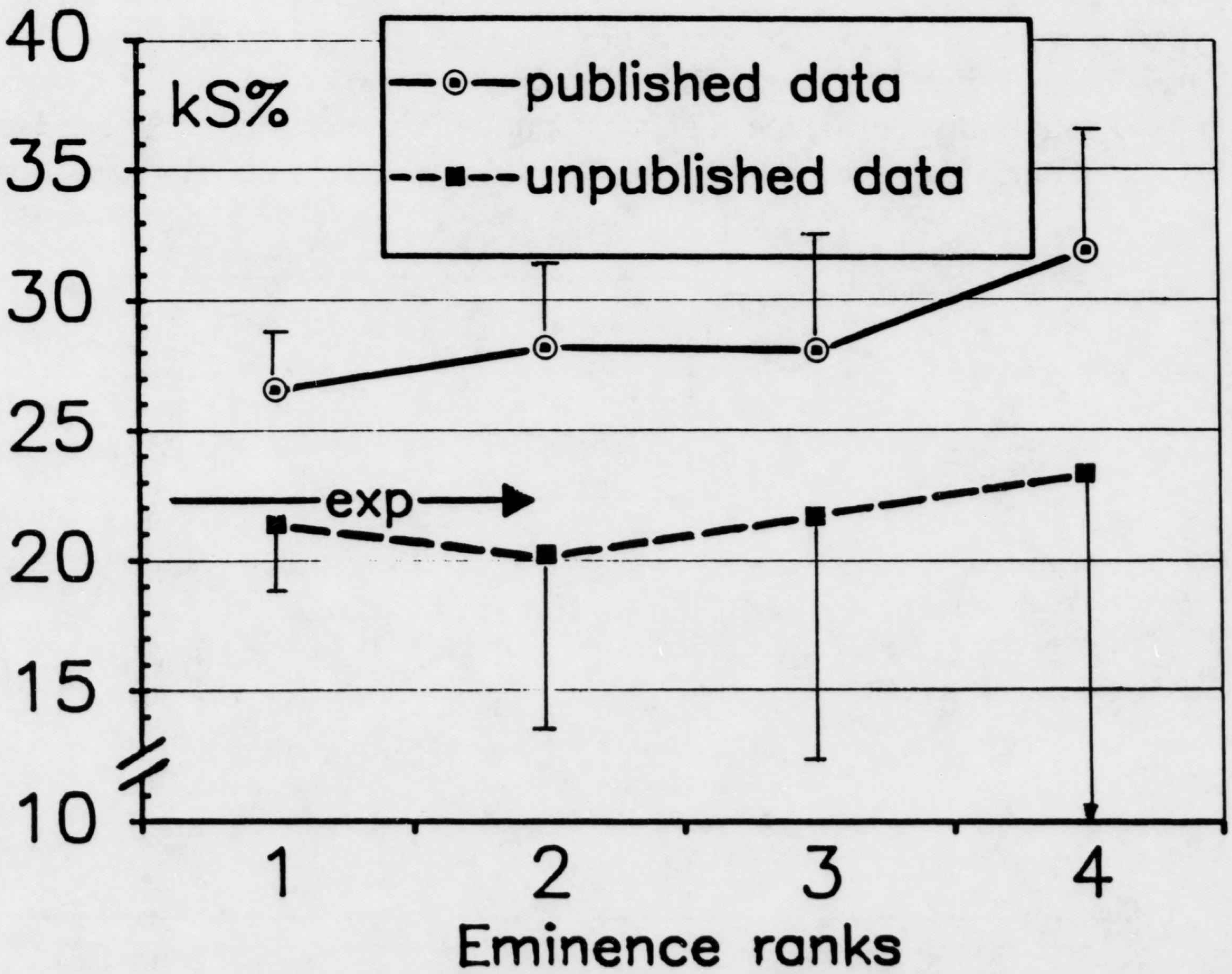

Fig. 4. Notable, but ambiguous differences between key sector percentages of published and unpublished samples. *Solid line:* Mars key sector percentages (kS%) for athletes from published Gauquelin samples ($N = 2888$).
Dashed line: Percentages (kS%) for those athletes from unpublished Gauquelin samples ($N = 1503$).

eventual publication. The conclusion then is that either Gauquelin's sensitiveness concerning biographical content, or his sensitiveness concerning the candidates' Mars sector at birth—or both—could account for the difference between the lines in Figure 4.

Another test was performed to clarify this ambiguity. A subsample of *N* = 659 was drawn from the total of unpublished entries (*N* = 1503). These were athletes chosen by Gauquelin under circumstances presumably more liable to bias than others (samples 5 and 11, see Table 1). Frequencies of Mars key sector passages at birth were then determined using the 36-sector scale and converted into percentages. The resulting graph may be compared with the Mars kS% of the published sample (*N* = 2888) (see Figure 5). The dashed line represents kS% for published data. Notice the upswings following the rise and culmination of Mars: the kS-effect.

What distribution should be expected for those data which Gauquelin had exempted from analysis and/or publication on the grounds that their biographies reported less success? The kS-peaks should be less prominent, the pattern may be washed out; that is, the Mars effect should tend to vanish. Surprisingly, however, the solid curve in Figure 5 still evidences marked deviations around both kS-zones. Even more surprisingly, deviations have the opposite direction. (Reversals of kS-frequencies for unpublished data have been emphasized by shading the respective areas.)

No doubt, this is evidence of bias in Gauquelin's selection procedure. For some of the lower ranking athletes, his acquaintance with Mars position

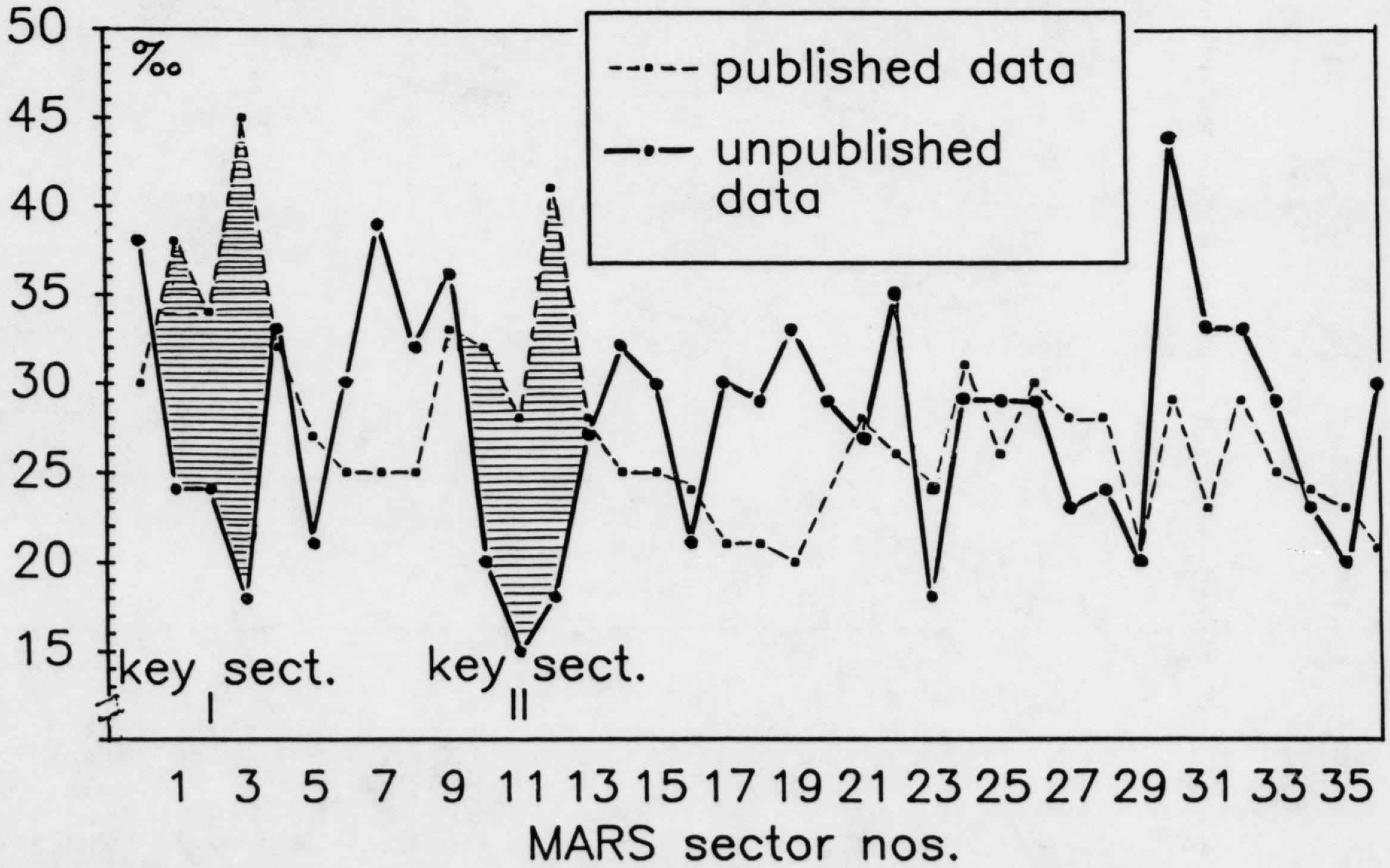

Fig. 5. Gauquelin bias effect: Mars sector frequencies ‰ for published Gauquelin athletes (*N* = 2888), dashed line; and for a subsample of unpublished athletes (*N* = 659), solid line. Marked negative deviations are apparent in key sector areas for unpublished data.

must have played a role, that is, athletes with birth times not associated with kS transitions were more likely to be discarded. Striving for objectivity, Gauquelin may yet have overrated his ability to remain unaffected by his knowledge of planetary positions. Concern over the outcome of the analysis seems to have interfered with the best of his intentions.[4]

The crucial question remaining is as follows: Could the kind of bias noted in Gauquelin's procedure invalidate the outcome of the present study? Could an artefact carried over from original materials raise the risk for wrong conclusions? An answer is afforded in Figure 6: kS percentages of published plus unpublished data, (solid line, $N = 4391$), are compared with those of published data only (dashed line, $N = 2888$). As can be seen, the eminence slope of the former is both lower and steeper than that of the latter. That is, the omission of athletes from experimental samples which occurred here and there on account of Gauquelin's bias had two consequences: (a) It served to inflate the *level* of kS-proportions overall; but also, (b) it weakened the eminence effect. The recombining of the unpublished with published records served to repair (i.e., to lower) the eminence level.

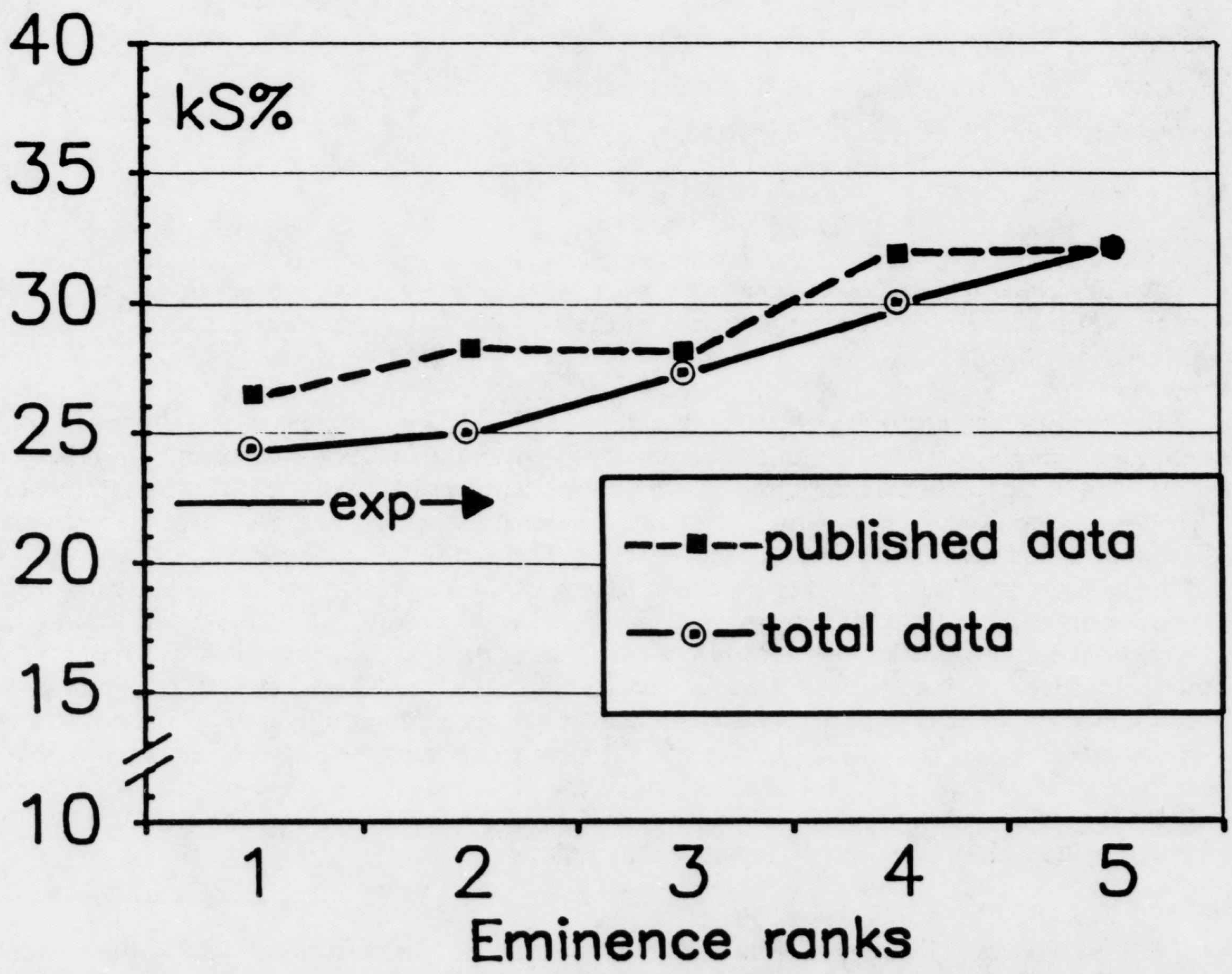

Fig. 6. Gauquelin bias effect: The eminence slope of published data (total minus unpublished) is less steep than that of total data. *Solid line:* Mars key percentages (kS%) for athletes from the total sample ($N = 4391$) (see also Figure 2). *Dashed line:* Mars key sector percentages (kS%) for athletes from published samples only ($N = 2888$) (see also Figure 4).

But at the same time it served to repair its slope (i.e., to make it steeper). The presence of selection bias, therefore, does not weaken the conclusion that Mars' position and the athletes' births are statistically related. Paradoxical though it may seem, this claim has been corroborated due to this bias: Correcting for selection bias by pooling all records *increased* empirical support for the stronger version of this claim; the data have overcome, *in spite of disturbing effects* of bias, the higher methodological hurdle.

Concluding Remarks

Passionate skeptics might continue to argue that it is still easier for a crank scientist to wrap some fabulous features around a fraudulent study than it is for Mars to contribute to an athelete's sports career. They might think that believers in relations between planetary positions and human births violating fundamental laws of physics of today, must err. Those who consider themselves as more liberal may continue to suspend judgement. In the investigator's view, however, the results of the present study have shown that Gauquelin's basic claim, first stated in 1955, is apparently valid. It is an interesting fact in its own right that the first acknowledgments from mainstream scientists required two to three decades to emerge (the first such reaction, if not based upon research of his own was Eysenck's in 1975). More effort directed at a better understanding of this provocative anomaly is now deemed to be highly desirable.[5]

Endnotes

[1] An annotated chronology of the main titles representing the Mars effect debate might be helpful for readers interested in the history of the problem (see Appendix 1).

[2] See the research report by M. & F. Gauquelin (1977). It is not inconceivable, however, that Gauquelin included in or excluded from the experimental collection a certain number of athletes in order to obtain a subsample with kS proportion equal to that of the total sample. G. Abell suspected that Gauquelin might have manipulated the sample at this stage of the research. The point of his suspicion, however, was overrated. For methodological reasons, kS proportions of the experimental sample had to be representative of the kS proportion of the total sample. If the experimental sample, drawn through Gauquelin's procedure, had not shown, upon first inspection, a kS proportion commensurate with that of the total, the investigators should have made improvements in their selection technique. Athletes with Mars in key sectors should have been added or deleted, respectively, so as to assure greater representativeness. Randomness of selection was in fact not called for. Even a completely deliberate selection of an experimental sample having a level of kS% equal to that of the total, could not have fostered an experimenter's goal to obtain a higher (or a lower) kS% level for the controls as compared to results yielded by a random selection of experimental athletes.

[3] Eleven *post hoc* selections of either "more successful" or "less successful" U.S. athletes were made, six by Kurtz-Zelen-Abell (two articles), five by the Gauquelins.

[4] Gauquelin did not dispute a bias of this kind in a public conference (see endnote 5)—its existence, however, does not exclude an additional positive effect due to biographical sensitivity. As referred to before, the difference of kS% levels between published and unpublished

samples, as shown in Figure 5, may still be partially due to a superiority of Gauquelin's weighing biographical information over crude citation counts. Athletes with equally low citation scores may nevertheless differ in achievement. The relative contributions to the difference seen in Figure 5 of a "bias effect," on the one hand, and that of a rating advantage, on the other, remain a matter of debate; but, in view of the present stage of insight into the data structure, that question has lost most of its importance.

[5] The study was supported by a sabbatical grant from the Deutsche Forschungsgemeinschaft (DFG). A travel grant from the DFG permitted participation at the "First Eysenck Research Seminar" at Long Beach, California (1986). A paper presented there referred briefly to the results of the present article. Another paper delivered at the Long Beach conference on further replication tests of the Gauquelin claims was published elsewhere (Ertel, 1987).

Before commencing this study, the author sent a research proposal to the Para Committee (Brussels), CSICOP (Buffalo, N.Y.), to CFEPP (Paris), to some participants of the previous Mars effect debate, as well as to consultants. Some months later a Newsletter apprised the above of the progress of the study. The author received valuable comments and critiques from a number of respondents.

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Appendix 1
Chronology of the Mars Effect Debate by Main Titles (Abbreviated Here)

Codes: H = The Humanist, SI = The Skeptical Inquirer

Stage One: Zelen Test

1975	*Jerome:* "Astrology Magic" (H, Spt/Oct) (Methodological objections are raised against Gauquelin's claims).
1976	*Gauquelin:* "Influence of planets" (H, Jan/Feb) (Rebutting Jerome's objections).
1976	*Zelen:* "Astrology a challenge" (H, Jan/Feb) (An experimentum crucis is proposed).
1977	*Gauquelin, M. & F.:* "Zelen Test" (H, Nov/Dec) (Zelen's test supports the Gauquelin claim).

1977 *Zelen-Kurtz-Abell:* "Is there a Mars effect?" (H, Nov/Dec) (Comments on M. & F. Gauquelin: Zelen test does not support the Mars effect hypothesis).

Stage Two: KZA's (CSICOP's) Replication with American Athletes

1979/80 *Kurtz-Zelen-Abell:* "US test results" (SI, Winter) (Negative results are reported).

1979/80 *Rawlins:* "Report on the US-test" (SI, Winter) (Stresses the negative result of the U.S.-test, but criticizes the adequacy of Zelen's former procedure).

1979/80 *Gauquelin, M. & F.:* "Star US-sportmen show the Mars effect" (SI, Winter) (Criticizes K-Z-A's U.S.-test and its interpretation).

1979/80 *Kurtz-Zelen-Abell:* "Response" (SI, Winter) (Justify their procedures and try to refute the Gauquelins' objections).

1980 *Gauquelin:* "A response" (SI, Summer) (Response to K-Z-A's defense).

1980 *Kurtz-Zelen-Abell:* "Contradictions" (SI, Summer) (Rejoinder to Gauquelin's response).

1980 *Jerome:* "Mars effect" (SI, Fall) (Congratulates K-Z-A for their success in finding no support for the Mars effect).

Third Stage: Conflict and Reevaluation

1981/82 *Rawlins:* "*Remus extremus*" (SI, Winter) (Extreme objections by a former CSICOP council member to the handling of the Gauquelin problem by other CSICOP members. His "sTARBABY" article in *Fate* magazine was intended as a Watergate-like disclosure of objectionable maneuvers).

1981/82 *Abell-Kurtz-Zelen:* "Statement" (SI, Winter) (CSICOP's executive council defends itself).

1981/82 *Abell-Kurtz:* "Response" (SI, Winter) ((Defense of members having been individually accused).

1981/82 *Gauquelin:* "Letter" (SI, Winter) (K-Z-A's "Contradictions" of 1980 are refuted).

1981/82 *Abell-Kurtz:* "Response" (SI, Spring) (The authors maintain their objections).

1983 *Abell-Kurtz-Zelen:* "Reappraisal" (SI, Fall) (Some criticisms are acknowledged, others are not. Result: Mars effect now not proven).

1983 *Gauquelin:* "Comment" (SI, Fall).

1983 *Kurtz-Abell:* "Response" (SI, Fall).

Appendix 2
First 50 Top Athletes Ranked for Citation Frequencies

Data: Sources G:A01, Series A, Vol. 1, G:D10, Series D, Vol. 10,
GAUQ: Nos. in Gauquelin data sources.
KZA: Nos. in Kurtz, Zelen, & Abell's data source (*Skeptical Inquirer*).
Cit. in: Character codes refer to citation sources, Appendix 3.
MARS: Sector no. (36-sector system) of Mars position at the time of birth.
kS: + refers to key sectors.

No.	Data	GAUQ	KZA	Name	Nation	Categ.	Cit. in	Cit.	Fem.	Born	MARS	kS
1	G:D10	185	50	Button, Richard T.	USA	ICES	BDEFHKSX	8		07/18/1929	9	+
2	G:D10	333	91	Dillard, Harrison	USA	TRAC	DEFKOSTX	8		07/08/1923	3	+
3	G:D10	865		Mathias, Robert Bruce	USA	TRAC	BDEKOSTX	8		11/17/1930	13	
4	G:D06	56		Blankers-Koen, Fanny	NET	TRAC	BDFKOST	7	F	04/26/1918	15	
5	G:D10	226		Charles, Ezzard	USA	BOXI	DEFHRSX	7		07/07/1921	21	
6	G:D06	103		Clark, Jim	SCO	CYCL	BCDEFST	7		03/04/1936	12	+
7	G:D10	238	62	Clay, Cassius	USA	BOXI	BDEFHKS	7		01/17/1942	10	+
8	G:D10	257		Connolly, Maureen	USA	TENN	BDFJSTX	7	F	09/17/1934	14	
9	G:D10	426		Fleming, Peggy Gale	USA	ICES	BDFHKSX	7	F	07/27/1948	7	
10	G:D10	437	118	Fosbury, Dick	USA	TRAC	BDFKOST	7		03/06/1947	11	+
11	G:D10	443	120	Frazier, Joe	USA	BOXI	DEFHKRS	7		01/17/1944	22	
12	G:D10	492		Gonzales, Richard Alonzo	USA	TENN	BDEJSTX	7		05/09/1928	4	
13	G:D10	567		Hayes, Bob (Robert Lee)	USA	TRAC	DFKOSTX	7		12/20/1942	16	
14	G:A01	2018		Killy, Jean Claude	FRA	SKII	BDFHKMS	7		08/30/1943	7	
15	G:D10	706		King, Billie J. (Moffit)	USA	TRAC	BDFJSTX	7		11/22/1943	24	
16	G:D10	1003	275	Patterson, Floyd	USA	BOXI	BDFHKRS	7		01/04/1935	22	
17	G:D10	1004	276	Patton, Melvin	USA	TRAC	DFKOSTX	7		11/16/1924	36	+
18	G:D10	1076		Richards, Robert (Bob)	USA	TRAC	EFKOSTX	7		02/20/1926	27	
19	G:D10	1200		Smith, Tommie	USA	TRAC	DEFKOST	7		06/05/1944	29	
20	G:D10	1211	335	Spitz, Marc Andrew	USA	SWIM	BDEFKSX	7		02/10/1950	31	

21	G:D10	1294		Tyus, Wyomia	USA	TRAC	BDKOSTX	7	F	08/29/1945	12	+
22	G:A01	773		Anquetil, Jacques	FRA	CYCL	DFHKOS	6		01/08/1934	3	+
23	G:A01	98		Ascari, Alberto	ITA	AUTO	CDEFHS	6		07/13/1918	2	+
24	G:A01	902		Bartali, Gino	ITA	CYCL	DFHKOS	6		07/18/1914	3	+
25	G:A01	909		Binda, Alfredo	ITA	CYCL	DFGHOS	6		08/11/1902	28	
26	G:D10	147	41	Bragg, Don	USA	TRAC	DKOSTX	6		05/15/1935	36	+
27	G:D06	91		Carnera, Primo	ITA	BOXI	DEFHRS	6		10/25/1906	16	
28	G:D10	270	71	Courtney, Tom	USA	TRAC	DEKSTX	6		08/17/1933	23	
29	G:D10	305	87	Davis, Otis	USA	TRAC	DEFKOS	6		07/12/1932	8	
30	G:D10	396		Evans, Lee	USA	TRAC	DFKOST	6		02/25/1947	12	+
31	G:A01	930		Faggin, Leandro	ITA	CYCL	DFHKOS	6		07/18/1933	35	
32	G:D06	159		Famose, Annie	FRA	SKII	DFHKMS	6	F	06/16/1944	11	+
33	G:D10	434		Foreman, George	USA	BOXI	BFHKRS	6		01/10/1949	22	
34	G:D10	440		Foyt, Anthony Joseph	USA	AUTO	BCDESX	6		01/16/1935	3	+
35	G:D10	474	129	Gibson, Althea	USA	TRAC	BFJSTX	6	F	08/25/1927	3	+
36	G:D10	548		Hansen, Fred	USA	TRAC	DKOSTX	6		12/29/1940	36	+
37	G:A01	46		Jazy, Michel	FRA	TRAC	DFKOST	6		06/13/1936	29	
38	G:D10	661	181	Johnson, Cornelius	USA	TRAC	DKOSTX	6		08/21/1913	4	
39	G:D10	701		Kidd, Billy (William)	USA	SKII	DHKMSX	6		04/13/1943	14	
40	G:A01	2064		Lenglen, Suzanne	FRA	TENN	BDEJKS	6	F	05/24/1899	12	+
41	G:D06	279		Maspes, Antonio	ITA	CYCL	DFHKOS	6		01/14/1932	27	
42	G:A01	857		Michard, Lucien	FRA	CYCL	DGHKOS	6		11/17/1903	9	+
43	G:D10	927		Morrow, Bobby	USA	TRAC	DKOSTX	6		10/15/1935	2	+
44	G:D10	962		O'Brien, Parry	USA	TRAC	DEKSTX	6		01/28/1932	20	
45	G:A01	2024		Perillat, Guy	FRA	SKII	DFHKMS	6		02/24/1940	11	+
46	G:A01	865		Petit-Breton, Lucien	FRA	CYCL	DFGHOS	6		10/18/1882	28	
47	G:D10	1113	300	Ryun, James Ronald	USA	TRAC	BDKSTX	6		04/29/1947	18	
48	G:D10	1138	307	Schollander, Donald	USA	SWIM	DEFKSX	6		04/30/1946	2	+
49	G:D10	1148		Seagren, Robert	USA	TRAC	DKOSTX	6		10/17/1946	15	
50	G:D10	1271	358	Thomas, John	USA	TRAC	DEKOTX	6		03/03/1941	30	

Appendix 3

Screening Sources Applied to Obtain Citation Frequency Indicators

(Character codes as used in the table of "First 50 top athletes," Appendix 2, column "Cit. in.")

1. Chambe, R. (1980). *Histoire de l'aviation.* Paris: Flammarion (A).
2. Chany, P. (1975). *La fabuleuse histoire du cyclisme.* Paris: O.D.I.L. (O).
3. Cimarosti, A. (1973). *Auto-Rennsport. Grands Prix, Wagen, Piloten, Formeln.* Stuttgart: Hallwag (C).
4. Faßbender, H. (1984). *Sporttagebuch des 20. Jahrhunderts.* Düsseldorf: Econ (F).
5. Garcia, H. (1973). *La fabuleuse histoire du rugby.* Paris: O.D.I.L. (O).
6. Gordon, R., & Goldman, H. G. (1981). *The Ring. Record book and boxing encyclopedia.* New York: Atheneum (R).
7. Gronen, W., & Lemke, W. (1978). *Geschichte des Radsports, des Fahrrads.* Eupen: Doepgen (G).
8. Kamper, E. (1975). *Lexikon der 12000 Olympioniken. Who's who at the Olympics* (Mit Supplement für die Olympiade 1976). Graz: Leykam (K).
9. Le Roy, B. (1973). *Dictionnaire encyclopédique des sports, des sportifs et des performances.* Paris (D).
10. Newman, G. (Ed.). (1979). *The concise encyclopedia of sports* (2nd revised ed.). New York: Watts (E).
11. Parienté, R. (1978). *La fabuleuse histoire de l'athlétisme.* Paris: O.D.I.L. (O).
12. Rethacker, J.-P., & Thibert, J. (1974). *La fabuleuse histoire du football.* Paris: O.D.I.L. (O).
13. *Skiweltmeisterschaften St. Moritz* (1974). Zürich: Wyss (M).
14. Soderberg, P., et al. (Eds.). (1977). *The big book of Halls of Fame in the United States and Canada.* New York: Bowker (X).
15. Sporthöhepunkte (1980). München: Pro Sport (H).
16. *Stars des Sports* (1970). Die Stars des Sports von A–Z. Berlin: Habel (S).
17. Sudres, C. (1984). *Dictionnaire du cyclisme.* Paris: Calmann-Levy (Y).
18. Tennis-Jahrbuch (1984). Amtliches Tennis-Jahrbuch des Deutschen Tennis-Bundes (J).
19. Umlauf, L. V. (Ed.). (1980). *The World Almanac of Who. The most important, famous, and interesting people of all times.* Leiceister: Windward (B).
20. Watman, M. (1981). *Encyclopaedia of track and field athletics.* New York: St. Martin (T).
21. *World Cup* (1974). München: Pro Sport (W).

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CORRESPONDENCE

Expanding Ball of Light (EBL) Phenomenon

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An Aurora has been described as a "transient optical phenomenon of the upper atmosphere usually observed in the high latitudes following solar flare activity [in the form of] a single arc or parallel system of arcs with bright lower borders [and extending] a hundred to several thousand meters [and] usually parallel with a geomagnetic latitude circle" (Campbell, 1967, p. 37). Now, a seemingly new phenomenon has been reported which is referred to as an *expanding ball of light* (*EBL*) *phenomenon* but which does not seem to correspond with typical Aurora. EBL reports extend back at least to 1959 and originate from widely divergent geographic locations. Despite the huge size of these visual phenomena, they seem to have received relatively little attention so far. The purpose of this report is to summarize some of these EBL occurrences so that the readers may share their conjecture about the nature of the phenomenon.

Table 1 presents a summary of 15 separate cases for which there is some information. In the last three cases commercial jet aircrew radioed verbal descriptions of the event to authorities on the ground (Alper, 1985; Gelbspan, 1985).

According to several pilot witnesses, the center of the EBL is at relatively high altitude while it is forming. Its color is evenly whitish or yellowish and becomes increasingly transparent to background stars as it expands. As it enlarges it appears to maintain a sharply defined edge. At some point it fades completely from sight. The rate of boundary interface expansion is impossible to determine without knowing its distance from the observer. It is also of interest to note that most EBL events have taken place after dark. If EBL phenomena are associated with an advanced weapons test, one wonders why it would be conducted (a) after dark, and (b) in so many different geographic areas. Both (a) and (b) tend to make EBL phenomena more conspicuous than otherwise. Table 2 presents a list of selected common features of EBL.

A number of explanations has been given for the EBL phenomenon. They include: natural volcanic eruptions occurring directly beneath a cloud (attributed to Walker by Alper, 1985); release of methane in an earthquake; new Soviet long-range weapon test (Bearden, 1984; Possony, 1984); disintegration of meteor striking cloud; an unusual pattern of vertical turbulence in the atmosphere; explosion of liquid hydrogen tanker, even though there are no marine reports of such an event having taken place.

TABLE 1
List of fifteen EBL cases

Date	Time	Duration (min.)	Latitude	Longitude	Place Name	Comments/Details
12/ 1/59	2000Z	?			N. Atlantic	Bright diffused blue light appeared then disappeared very slowly.
6/ 1/61	1800Z	32			Indian Ocean	Two thin, concentric arcs, 150° tip to tip, req'd. 20 min. to reach max. intensity.
6/17/66	2142L	4–5	*36°N	*52°E	Tehran, Iran	Seen by 2 aircrews in flight.
3/20/69	2315Z	10			Caribbean & NW Atlantic Ocean	Milky white, arc top at 50° × 70° wide.
8/22/69	2015L	?	*20°N	*64.5°W	Virgin Islands	White arc.
10/20/69	2345L	15	*33.6°N	*75.5°W	N. of Cuba off USA	Huge, slowly expanding luminous shell seen by 70–100 U.S. Navy seamen on DLG--27.
9/25/72	?	?			N. Atlantic	Expanding luminous area on horizon.
3/24/77	0855Z	7	23.1°N	17.45°W	300 mi. S. Canary Islands	Luminous hemisphere, 3 min. to form.
9/ ?/79	night	?	35–37°N	68–75°E	NE Afghanistan	Huge, luminous hemisphere over USSR
6/22/80	2100L	?			Seen from Kuwait	10 mi. (est.) diam., flat base, probably in Caucasus region.
6/18/82	night	?	*50°N	153°E	N. China	Yellowish-white, 18–27 (est.) km. diam, covered 90° arc along horizon.
6/18/82	2206L	?			N. China	Orig. size of full moon, milky white/greenish, Mig jets communic./navig. malfunctioned.
4/ 9/84	2306L	?	*44°N	*150°E	200 mi. off NE tip of Japan	Sphere enlarged and rose from 14,000 ft to 60,000 ft in 2 min.
7/27/84	1550Z	*10	47.5°N	161°E	N. of Kuril Islands	Huge expanding shell of whitish light, faded gradually, no radioactivity or turbulence.
6/11/85	2240L	2			Lanzhou, China	Huge yellowish glow, expanding, with very intense spot at center, B747 crew CA9333.

* Approximate value.
? Value unknown.

TABLE 2
Selected characteristics that are common to the expanding ball of light phenomenon

Long duration	(typ. 10+ minutes)
Large angular width	up to 40 deg horizontally
Slow rate of expansion	300 to 750 ft/sec (est.)
Generally symmetrical expansion	yes (as a sphere)
Location of center of expansion	high altitude
Ionizing radiation	unlikely
Typical hue	whitish, yellowish
Air turbulence in vicinity	none reported by aircrews
Electromagnetic disturbances	none reported by aircrews
Fireball or flash	none

The Encyclopedia of Science and Technology (Lapedes, 1977) describes a dome-shaped Aurora. The following EBL characteristics would seem to suggest that it is not an Aurora type of phenomenon: (a) the long duration, (b) symmetrical expansion into an apparent sphere rather than concentric arcs, (c) the absence of solar activity during or before the events, and (d) the locations of these events which are not all in particularly northerly latitudes nor parallel with a geomagnetic latitude circle. McKenna and Walker (1986) and Walker, McCreery and Oliveira (1985) have suggested that the EBL phenomenon is preceded by a towering cumulus-like cloud which appears to rise out of the stratiform layer. In most of the authors' cases where weather was indicated, there was no other cloud formation(s) present near the EBL. McKenna and Walker (1986) also point out that the entire sequence of events lasts from 15 to 20 minutes, which is in agreement with the present data.

References and other notes regarding the sightings reported here are available from the author on an exchange basis. Readers having any relevant information are urged to write the author.

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BOOK REVIEW

Origins—A Skeptic's Guide to the Creation of Life on Earth, by Robert Shapiro. New York: Summit Books, 1986, 332 pp., $17.95. Also in paperback—New York: Bantam Books, 1987, $9.95.

Arguably, the existence of life on earth is the anomalous phenomenon par excellence: it indubitably exists even though it is implausible to the *n*th degree (if life were common in the universe, we ought to have known that by now from some of the places where it would have begun several billion years earlier than here). That is as though we had a reproducible parapsychological experiment, or a physical bit of an indubitable UFO, or a Nessie carcase: it would still remain to find a satisfactory way of connecting that datum with the corpus of scientific understanding. *Origins* illustrates how scientists flounder around in such a situation, where needed information is missing: believing that the connections exist, unwilling to leave them unknown for the time being, they build elaborate but conflicting hypotheses on little or no data and argue passionately over their relative merits. The unwarranted interpretation of doubtful evidence is as present here as it is in much of the literature on the evolution of Homo sapiens, on nutrition, on holistic medicine, or on what is commonly called fringe science or pseudo-science. But the media do not discriminate within science between what is properly scientific and what is not, and so some scientific myths can attain much currency: in this case, the myth that the earth initially had a strongly reducing atmosphere, that natural events would produce from that copious amounts of all the species needed for life, that only detail remains to be understood of the subsequent processes of intricate organization involved in the establishment of replication with sufficient (but not quite perfect) accuracy, of information transfer and specialization of systems, of cellular separation from the environment. Shapiro points to the lack of actual evidential support for the postulated sequence; more important and more useful, he shows in detail how the proponents' arguments are mere handwaving, and he makes plain what sort of information we will need to construct a realistic explanation.

Origins begins with exemplars of religious or mythological explanations on the one hand, of scientific on the other; and an excellent discussion of the nature of science and how it differs from mythology and religion. Then comes a well-organized summary of the chemistry of life on earth, and a survey of its history, followed by a revealing account of the Urey–Miller experiments which produced some amino-acids by sparking reduced gases.

There follows the useful chapter, "The Odds," and then attention is directed to the main issue, how a replicating system could originate and what its chemical nature could have been. Later come sections on extraterrestrial origins and on creationism. Shapiro's own guess is that structured clays played a role in the origin of life on earth, and that the first replicator was protein rather than nucleic acid; but the book is valuable for its analysis, not for any particular conclusions.

The controversies over extrasensory perception, UFOs, Nessies, and other cryptobeasts, etc., are intractably messy for a number of reasons: one is that argument over bits of data is incessantly fused and confused with argument over the plausibility of possible interpretations of such bits of data; another is the ever-present red herring, what is science and what is not? or, what is science and what is pseudo-science? This book is germane to both of those points.

To arguing over what is science and what is not, there is no end. There is no agreement in principle about what criteria are valid, and even the most plausible criteria often turn out to be ambiguous in application to specific instances. Shapiro emphasizes the distinction between myth, whose function it is to provide a sense of security by purveying certainty, and by contrast science, which can never legitimately claim a final and positive certainty. Throughout the book, this distinction is not merely asserted but applied and illustrated; moreover evenhandedly, as in the demonstrations that the "origins" theories of several scientists, though often enough published in scientific periodicals, are nevertheless myths and not science, no more true to facts than is the myth of the Scientific Creationists.

Shapiro has a fine gift for finding analogies that are illuminating and evocative, for instance in remarking that the Earth's history can be inferred from its mountains and sediments as can an individual's from his wrinkles and scars. Again, the manner in which the Scientific Creationists attempt to discredit science is beautifully and legitimately compared to an attempt to rewrite a specific and well-known bit of history. And subtleties having to do with probabilities are brilliantly explicated: Shapiro drives home the point that a few billion years is by no means long enough if the postulates are sufficiently unlikely, and he uses in a fresh and cogent way the old analogy of the typist monkeys set to produce bits of Shakespeare.

Primarily, of course, this book is an up-to-date and informed review of what we do not yet know about the presumed origin of life from inanimate precursors; there are incisive accounts of the competing schools of thought, and Shapiro ventures his own speculations as well as suggestions for fruitful lines of inquiry. Then too, as already said, it is an illuminating study of a dispute that has much in common with the disputes over anomalous phenomena. It makes clear at least one significant way that science differs from at least one category of non-science. It is exemplary in the inspired use of pedagogical analogy. And the whole is spiced by matter-of-fact comments on the eccentricities of such well-known people as Fred Hoyle and H. J. Muller.

An earlier book by Feinberg and Shapiro, *Life Beyond Earth,* met very favorable critical response. This book is even better: no more than once or twice a year do I come across a book as worth reading as this one. It affords pleasure in several ways, and it has special interest for those who struggle to make sense of anomalous phenomena.

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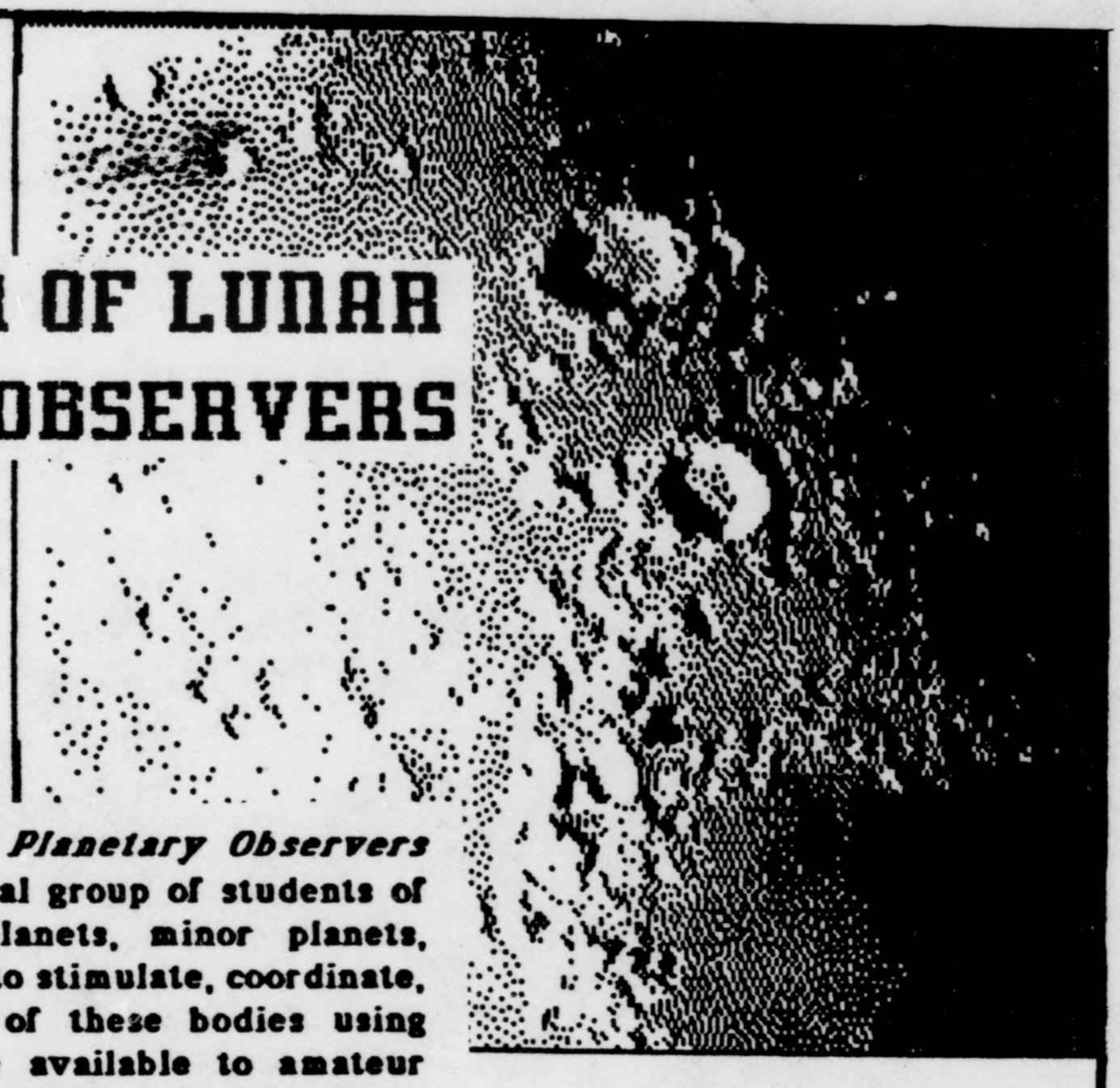

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CONTENTS

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