• *ABSTRACT*

The scientific literature can be used to construct facts or to deconstruct them. The formal journals construct by presenting maximally 'demodalized' accounts of experiment. The more popular journals are licensed to present more than this, but usually, where they do provide contingent details of scientists' work, these are details of life away from *the laboratory bench. Sometimes, popular journals use their license to present contingent details of work* at *the laboratory bench, and this has a deconstructing effect on the scientific results presented. This analysis emerges from a consideration of the role of the literature from an active 'construction of scientific knowledge' perspective. The work of the 'Committee for the Scientific Investigation of the Claims of the Paranormal' and its journal are then analyzed with these themes in mind. The Committee sometimes presents itself as revealing the results of its own experiments, and sometimes uses its journal to deconstruct others' work. The cases of 'Remote Viewing' and the astrological 'Mars Effect' are discussed. The analysis bears out the 'active' view as regards the scientific literature. Also, members of the Committee are seen to take up the active view as their experience of controversial science grows.*

Private Science and Public Knowledge: The Committee for the Scientific Investigation of the Claims of the Paranormal and its Use of the Literature

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The most readily available public model of science sees scientists as in dialogue with a fixed natural world. The key feature is that given the appropriate circumstances, the data can speak, if only to falsify an over-presumptuous theory. Scientists are the talented high priests who intercede between an authoritative 'Nature' and a respectful laity. This forms what might be described as the ideology of science.

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Studies of scientific practice have, over the last decade, increasingly called into question this and the related, more philosophical, 'standard model' of science.[1] The 'failures' of the standard view of science have shown up in particular during episodes of scientific controversy. During controversies Nature's voice appears indistinct. Humankind is seen to have a more active role in the construction of knowledge. In controversy many features of experimentation that would be thought of as purely contingent under the standard model gain salience even for the scientists involved.[2] The active model of science dissolves the dichotomy between mankind and nature. Our concepts of the natural world emerge from the artful construction and negotiation of scientific knowledge.[3] In this model, knowledge about nature is treated as rather like knowledge about art, or politics, or the law.

The Literature

The mechanisms and style for the transmission and diffusion of knowledge, when seen through the perspective of the standard model, are designed to aid the process of revealing the truth about the natural world. Results are presented in esoteric journals. The language is self-effacing, suggesting that the experimenter played the role only of facilitator, or 'amplifier' of Nature's voice. Even when controversy arises this passive language should sooth the emotions of critics, allowing a dispassionate debate on the merits of the work and further appraisal by others through more experiments done in cool blood. First results for some new effect are presented modestly, with a provisional air. The proprieties are that the investigator presents him or herself as fallible — an automaton as far as possible, but a fallible automaton. As for the less formal journals, under the standard model, their purpose is simply that of popularization. They disseminate scientific knowledge and information in a readily understandable form.

In times of controversy the formal literature becomes conspicuous for what it does not include. Biographical details of the authors are not to be found in the formal journal article. Details of the experimenter's health, the date of the experiments, the motives, interests and emotions of the experimenters, are also absent. The failures, preliminary runs, aggravations, breakdowns, financial difficulties, family and time pressures are not reported.

The journal article presents itself in this way because the construction of scientific facts is bound up with their presentation in language. Latour and Woolgar have suggested that the facticity of a claim changes in concert with changes in statement type.[4] Thus, early claims are made in the form: 'Peter [ref.] has suggested that in goldfish the hypothalamus has an inhibitory effect on the secretion of TSH'.[5] Whereas, as the claim becomes fact, its expression in print moves through a number of stages until it reaches a form something like: 'In goldfish the hypothalamus inhibits the secretion of TSH'.[6] Or it even becomes embedded in another statement: 'Bearing in mind the inhibitory effect of the hypothalamus on the secretion of TSH . . . .'[7] These changes are described as the removal of 'modalities' from the statement.

The removal of modalities is, we would suggest, an example of a more general feature of the construction of facts achieved through the presentation of results. The same piece of laboratory work is open to a variety of descriptions. Some descriptions increase facticity, some reduce it. Thus the astute critic of a knowledge claim need do no more than honestly redescribe an experiment in all its contingent detail to dissolve the scientific potential of the experimental findings. In an earlier work we describe at some length the construction and writing of a report concerning experiments on 'psychokinesis', and show how the same data could have been presented in a way that would not lend support to the conclusions that we drew from it.[8] Likewise we show how others redescribed the report in ways that supported their scientific interests, and how, in general, critics of the paranormal present accounts of positive experimental work in a degree of detail which is not associated with reports in the formal journals. These accounts work by invading the privacy which surrounds reports of laboratory activity in the formal journals and therefore make the reports appear less fact-like and more like the everyday activity of You and I — open, of course, to all the familiar biases and pitfalls.

In the normal way, even in the more popular scientific press, journalists are not generally privy to the research site; they make no attempt to recover the social dimensions of laboratory life. 'Human interest' may be injected into science news, but here the contingent facts concern the scientist caught *away* from the research. It may be that the Professor is absent-minded, or loves a dog, or rides a shabby bicycle, or struggles against a disability, but these human qualities only add wonder to the infallibility of the

scientist *at the bench*. During controversy, however, the informal media can use their licence to discuss the contingent features of scientific activity at the laboratory bench. The privacy necessary for the predominance of *fact*-like accounts then breaks down.

In this paper we exemplify a number of these features of scientific life through the work of the Committee for the Scientific Investigation of the Claims of the Paranormal (CSICOP). First we examine their journal — *The Skeptical Inquirer*. We show that this has been used sometimes after the manner of a formal journal to give claims a fact-like quality, and sometimes like a popular journal to 'deconstruct' scientific facts through the invasion of experimental privacy. We exemplify the latter process in their handling of certain experiments on 'remote viewing'. Next we examine the Committee's handling of an astrological claim — the 'Mars Effect'. This case re-makes the point about uses of the literature. However, on this occasion it was *Fate*, a popular magazine, that invaded the privacy of the *Skeptical Inquirer* and deconstructed the Committee's attempts to refute the Mars Effect 'scientifically'. Also, this case demonstrates — at least as revealed by the writings of members of the Committee in the *Skeptical Inquirer* — that substantial experience of controversy does indeed lead to a shift from the 'standard view', of science to an active 'negotiated' view.

The Committee and the *Skeptical Inquirer*

The debate over the reality and scientific status of paranormal phenomena is longstanding. Despite over a century of research, the existence of the paranormal seems to be no better established today than when the Victorian scientists formed the Society for Psychical Research. However, the phenomena will not go away. Every few years some new outcropping of interest seems to develop. The most recent upsurge, which has occurred during the late 1960s and early 1970s, has left its mark, if only in that it has engendered the formation of a remarkable organization of sceptics. This organization was founded in 1975 in an atmosphere in which phenomena such as those produced by Uri Geller seemed to promise a much more visible and researchable manifestation of paranormal powers than had previously seemed possible.[9]

The Committee was originally part of the American Humanist

Association (AHA) (formal links between the two organizations were severed soon after the Committee was formed). The renewed interest in the paranormal and other occult sciences in the late 1960s provoked several critical articles in the AHA magazine, the *Humanist*. This criticism took a new turn in 1975 with the

FIGURE 1
The Statement on the Back Cover of the *Skeptical Inquirer*

The Committee for the Scientific Investigation of Claims of the Paranormal

The Committee for the Scientific Investigation of Claims of the Paranormal attempts to encourage the critical investigation of paranormal and fringe-science claims from a responsible, scientific point of view and to disseminate factual information about the results of such inquiries to the scientific community and the public. To carry out these objectives the Committee:

- Maintains a network of people interested in critically examining claims of the paranormal.
- Prepares bibliographies of published materials that carefully examine such claims.
- Encourages and commissions research by objective and impartial inquirers in areas where it is needed.
- Convenes conferences and meetings.
- Publishes articles, monographs, and books that examine claims of the paranormal.
- Does not reject claims on a priori grounds, antecedent to inquiry, but rather examines them objectively and carefully.

The Committee is a nonprofit scientific and educational organization. The SKEPTICAL INQUIRER is its official journal.

FIGURE 2
The Front Cover of the *Skeptical Inquirer*

the Skeptical Inquirer

Gerard Croiset:
The Case of the Claimed
Clairvoyant Detective

Horoscope Inaccuracy / Planets and
Radio Disturbance / Psychic Hucksters
Bermuda Triangle 1981 Style

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publication in the *Humanist* of the statement 'Objections to Astrology', which was signed by 186 scientists, including 18 Nobel Laureates.[10] This statement was widely circulated amongst the media by the *Humanist* editor, Paul Kurtz. Encouraged by the ensuing publicity, Kurtz became convinced that the time was ripe for a more active crusade against parapsychology and other pseudo-sciences. In the summer of 1975 Kurtz heard of the activities of Marcello Truzzi, a sociology professor at Eastern Michigan University. Kurtz and Truzzi were to become founding members and co-chairmen of the Committee.

Truzzi had a long-standing interest in the sociology of the occult and edited a newsletter, *The Zetetic*, which was circulated amongst interested academics.[11] In 1975 Truzzi considered expanding his newsletter into an information bank of resource material on the paranormal.[12] He had discussions with science journalist, Martin Gardner, psychologist, Ray Hyman, and magician James 'the Amazing' Randi. Kurtz persuaded Truzzi jointly to form a new organization, CSICOP, with a revamped *Zetetic* as its official publication, and with Truzzi as its editor. Gardner, Hyman and Randi all became 'Fellows' of the new Committee.

The initial style of the *Zetetic* was near to that of a formal journal. However, this style lasted for only two issues. At a meeting of the executive council of the Committee held in 1976 it was decided, against the wishes of Truzzi, that the *Zetetic* should be turned into a semi-popular journal. Such a change of format was inspired by the appearance in *Readers Digest* at this time of a pro-paranormal article. It was felt that the *Zetetic* with a readership of a few thousand could not hope to compete with the mass-circulation *Readers Digest*.[13] Truzzi resigned from the editorship and this made way for the *Zetetic* to become the more down-market *Skeptical Inquirer (SI)*. Kendrick Frazier, a former editor of *Science News*, took over as editor. The journal has continued in this more popular style to this day (it now has a circulation of 12,000). In order that the reader can get a feeling for this rather obscure journal, we will present a brief description of its form and content.

The *SI* is a cross between a formal scientific journal and a popular magazine. Its glossy, 'arty-style' cover alerts us that it is not a typical learned journal, yet the layout and print style is closer to that which might be found in a serious academic journal than a popular one. The respectable scientific pedigree of the journal is

reflected in the sober-minded statement of aims and objectives printed on the back cover (see Figure 1). This contrasts with the newspaper-style headings and photograph to be found on the front cover (see Figure 2). Both inside covers add to the apparent scientific weight of the journal. We learn from the inside front cover that *SI* is the 'official journal of the Committee for the Scientific Investigation of Claims of the Paranormal'. There is an accompanying list of 'fellows of the Committee' and in this list we find names such as B.F Skinner and W.V. Quine.[14] The inside back cover also adds to the scientific standing of the journal by presenting an impressive list of 'Scientific and Technical Consultants'. International sections of the Committee, and various 'sub-committees' responsible for detailed investigations of particular phenomena, are listed.

Sandwiched between the glossy covers are to be found a contents page, occasional editorials, news items giving details of the activities of sceptics or believers, short 'put-downs' of proponents of the paranormal (often achieved merely by repeating some of their more 'ludicrous' claims), articles, book reviews, bibliographies, letters and even the occasional cartoon. In the Fall 1981 issue, the articles dealt with 'psychic sleuths', horoscopes, planetary influence upon radio interference, the Bermuda Triangle, and psychic healing. Some of the articles were straightforward journalism — for instance, an account of a visit to an anthropology seminar where psychic healing was to be demonstrated. Others were more technical, and one article included graphical presentation of statistical evidence: appended to this article was a detailed list of references as might be found in a learned journal. All the articles in this particular issue either 'debunked', or called into question, the fringe-science phenomenon under discussion.

The *Skeptical Inquirer* appears to covet the authority of scientific standing, as its back-cover shows (see Figure 1), but it feeds its readership a spicier fare than is normally served up in a technical journal.

The Case of Remote Viewing

'Remote Viewing' is the claimed ability to obtain visual information of a remote location by paranormal means. In the early 1970s, experiments concerned with this phenomenon were

conducted at the Stanford Research Institute (SRI) by two physicists, Russel Targ and Harold Puthoff. The results were first published in *Nature* in 1974 as part of a report of a wider series of experiments[15] (including experiments with Uri Geller[16]). We reproduce the relevant section of Targ and Puthoff's *Nature* paper as Appendix 1. The 'remote viewing' phenomenon soon became a centre of critical attention, and a number of articles and a book appeared proposing 'normal' explanations for the phenomenon. One such article was written by David F. Marks in the *SI* of summer 1982.[17] The section which refers to Targ-Puthoff *Nature* experiments is reproduced as Appendix 2.

From Appendix 1, it can be seen that Targ and Puthoff's description of the experiments is typical of the style to be found in the formal journals. The motives for carrying out the work are presented in depersonalized terms: 'A study by Osis'. The language is passive and self-effacing: 'This experiment . . . consisted of . . .'; 'Several descriptions yielded significantly correct data . . .'; '. . .the experimental results were subjected to independent judging'. The impression given is that Targ and Puthoff played a minimal role as facilitators in letting Nature speak. Furthermore, their results are presented in a modest manner: 'Among all possible analyses, the most conservative is . . .' Indeed, unless one follows the technical argument carefully, the importance of their results can be missed in the bald statement that 'under the null hypothesis (no remote viewing and a random selection of descriptions without replacement), this outcome has an a priori probability of $P = 5.6 \times 10^{-4}$. . .'

If we now look at the redescription of the experiments in the *SI* (Appendix 2), we find that Targ and Puthoff become 'humanized' and subject to all the personal biases of human observers. For example, their emotional state is referred to; they are described as being 'highly delighted with the results'. They are also now no longer portrayed as anonymous scientists who could be working anywhere, but part of the 'SRI research program, promoted as well — controlled science . . .'. Targ and Puthoff become like the rest of us — they are approachable; they are people who can be visited. Similarly, the anonymous independent judge becomes identifiable as SRI analyst, Arthur Hastings. Hastings is portrayed as manfully struggling with his conscience, caught between his loyalty to his SRI colleagues and his role as an informant. In the end his loyalty wins out.

As the description continues, Targ and Puthoff become less and less automaton-like. They withold transcripts; they fail to reply to letters; generally they act in a difficult manner. When they do respond to letters their reply becomes a crucial piece of information revealing supposedly vital clues about the performance of the experiment. Targ and Puthoff are made the subject of the moral force of public pronouncement: 'I would like to publicly ask Targ and Puthoff . . .' Thus even when personal access is no longer possible, the experimenters become personalized by being presented as people who might be swayed by public opinion. Our attention is focused on the familiar ordinariness of the experiment by the use of the active tense. Phrases, such as, 'Targ's most blatant cues' present us with a *particular* picture of a scientist carrying out an experiment (in this case incompetently).

In this description the shadowy experimenter of the formal-journal article has been replaced with a real live person with all the familiar human failings. As the minutiae of what Targ, Puthoff and Hastings did on particular occasions is described, the experiment increasingly takes on the appearance of being subject to the contingencies of everyday life. That special appearance which makes it science has been lost. What we see here is the *SI* using its popular status to reduce the facticity of paranormal claims by invading the privacy of the laboratory, and presenting the contingent details of work at the laboratory bench.

The Mars Effect

The engagement of the Committee with the 'Mars Effect', as we shall see below, led them to experiment on a highly controversial scientific phenomenon. The Committee, previous to their involvement, had set out their view of science in their statement of aims and objectives (see Figure 1). These aims correspond with the 'standard' view of science. However, if it is true that close involvement with controversy precipitates a more active 'negotiated' view of science,[18] then we should expect some revision of the views of Committee members, at least on a temporary basis. There is evidence that this happened. The Committee in effect used themselves (albeit unwittingly) as a test of this idea.

A Test of Astrology or Philosophy?

Much of the work of French psychologists Michel and Françoise Gauquelin has been aimed at debunking traditional astrology, but they have also gathered some of the most compelling scientific evidence in support of astrological-type ideas. Over the last thirty years they have generated data which indicates the existence of correlations between the positions of the planets at the time of birth and certain human characteristics. The strongest correlation is known as the Mars Effect. This is a correlation between the position of the planet Mars at time of birth and sporting prowess. According to the Gauquelins, Mars appears in the relevant sectors of the sky at 22 percent of the births of sports champions, compared with 17 percent for non-champions.

The Committee's involvement with this issue came by way of a challenge to the Gauquelins issued by a statistician, Marvin Zelen, in the *Humanist* magazine.[19] This challenge, known as the 'Zelen test', concerned the statistical validity of the Gauquelins' findings.[20] Zelen was unhappy with the figure of 17 percent for the incidence of the Mars Effect amongst the population of non-champions.[21] He proposed that this issue be settled empirically by collecting data on a new sample of non-champions born in close spatial and temporal proximity to a representative sample of champions. If the Gauquelins were correct, then 17 percent of this population of non-champions should exhibit a Mars Effect. A larger percentage would call into question the statistical significance of the Mars Effect amongst the champion group. The Gauquelins were agreeable to the implementation of the test, and they proceeded to collect data on a large European sample of non-champions who were born in close spatial and temporal proximity to a smaller group of champions (chosen from their original sample). Committee members checked the way that the Gauquelins implemented the test.

This test, then, had every promise of being a definitive test of the Mars Effect. Both parties seemed to have agreed upon the appropriate procedures, and now it seemed to be merely a matter of inspecting the results. The Committee fully expected that this foray into the field of astrology would clear up the matter. For instance, Zelen wrote: 'We now have an objective way for unambiguous corroboration or disconfirmation.'[22] Similarly another Fellow of the Committee, George Abell, who has been closely involved with

the Mars Effect debate, described it as a 'very definitive test'.[23]

The results were published in two papers in the *Humanist* of November-December 1977. One paper, written by the Gauquelins, sets out their view that the Zelen test had confirmed the Mars Effect.[24] The other article, written by Zelen, Kurtz and Abell, questioned this interpretation.[25] Accompanying the two articles was an introduction to the Mars Effect by the *Humanist* editor, Paul Kurtz. In his introduction Kurtz put forward the Committee's verdict: '. . . the Committee does not think that the Mars Effect has been uniformly confirmed by the Zelen test'.[26] In their analysis the Gauquelins claimed that there was a difference between the incidence of the Mars Effect amongst champions and non-champions that was significant with a probability of 0.03. The Mars Effect appeared to occur significantly only amongst the champions, and not in the population at large. The Gauquelins made good use of Zelen's previous claims for the definitiveness of the test. They quoted his own words back at him:

> It is suggested that this simple method of data collection will result in scientifically valid data, which will not be subject to the criticisms made of Gauquelin's work . . . if the sports figures' hours of birth fall into either of these sectors (the key sectors) in greater proportion than do those of non-sports figures, we must accept Gauquelin's conclusions.[27]

They went on to write:

> Under rigorous control conditions, the data gathered for the Zelen test have brought various and coherent proofs that the 'Mars Effect' appears with sports champions and does not appear with non-sports champions.[28]

This interpretation of events was not shared by Zelen and his Committee colleagues. Their attack came on several fronts, the main one being to claim that the statistical significance of the purported effect was 0.04 rather than the 0.03 claimed by the Gauquelins. This reduction in significance was achieved by dropping female athletes from the sample. They claimed that this was legitimate because there were special difficulties in female athletes becoming champions, as evidenced by the small numbers of female champions. As the overall rationale of the test was to compare 'like with like', they felt justified in excluding another possible disturbing variable — gender. Thus they compared male champions with male non-champions. Having reduced the significance of the Mars Effect, they then went on to point out that

if there had been one less champion born in a key sector then the overall probability would have fallen to 0.07. They wrote:

> Thus the judgment on the overall statistical test is critically dependent on one key-sector birth. Clearly this is not conclusive scientific evidence of the existence of a statistically significant difference between champions and nonchampions for the overall group.[29]

After questioning the statistical significance of the results, Zelen and his colleagues next turned their attention to the geographical distribution of the Mars Effect. By sub-dividing the sample they found that most of the significant effect came from one geographic region — Paris. They went on to suggest that the sampling technique may have led to large cities, such as Paris, being over-represented. Because the Gauquelins had not been able to find enough birth records of non-champions born at the same time and location as the champions, they had been forced to restrict the sample to somewhat large areas in order to obtain enough data. This meant, however, that the overall Mars Effect could arise from significant contributions from a very few localities. This would tend to bolster the interpretation that the Mars Effect was just a statistically rare event. The implications of the analysis were clear:

> If the Mars Effect is real, why can it not be demonstrated over a larger geographical locality than Paris? Another possible interpretation of the Paris results is that indeed one has observed a rare event. In looking at many data sets one will occasionally conclude the existence of a real difference when in fact none really exists . . .[30]

It thus seemed that Zelen and his colleagues had been able to argue that the Mars Effect was a statistically weak effect, possibly a fluke, and that it was restricted to just one locality.

The Gauquelins and others soon responded to this interpretation of their results. It was pointed out that the rationale for removing female athletes from the sample was unclear, especially since three of the female champions dropped had Mars in the key sector.[31] It was only *after* the women had been dropped that the significance of the result could be said to turn on one athlete. Another point which has been raised by several critics of Zelen has been the post-hoc sub-division of the sample. The sub-samples, being much smaller, could hardly be expected to show any statistical significance.[32] It has also been pointed out that the purpose of the Zelen test had

changed.[33] As originally conceived, its purpose was to test the incidence of the Mars Effect amongst non-champions (the test of the 17 percent incidence). Since Zelen and his colleagues did not appear to have challenged the Gauquelins' data on this point, they should have at least acknowledged that the test, as first formulated, had gone in the Gauquelins' favour.

Ad-hocery, Replication, and the Salience of the Active Model of Science

The debate over the Zelen test did not end there. Arguments back and forth continue.[34] Eventually the Committee decided to attempt a replication test of the Gauquelins' study.

Part of the standard model of science is that the 'real' experimental results are capable of replication by independent groups. Thus, in the hope of clearing up this troublesome matter once and for all, the Committee embarked upon a replication based on a US sample. This was drawn up by Committee chairman Kurtz, and was analyzed by Dennis Rawlins, a Fellow of the Committee. The results were published in the *SI*.[35] No evidence for a Mars Effect was found. However, the Gauquelins immediately produced a rather different analysis of the Committee's data.[36] Their main criticism was that the Committee had failed to base its samples on the 'very greatest' champions. It was only amongst this latter élite group that the Mars Effect was expected to occur. The effect had been diluted by the inclusion of lesser sportsmen. Kurtz had been unable to obtain a large enough sample initially because of problems encountered with State Registries who refused to release records. The sampling was thus conducted in two stages and in the latter stage lesser athletes (according to the Gauquelins) were used. They re-analyzed the data using only what they regarded as the very best champions and found, they claimed, a statistically significant Mars Effect. Kurtz and his Committee colleagues refused to accept such a post hoc analysis, claiming that the Gauquelins had agreed in advance to the use of these sources for sampling.[37] The Gauquelins, in turn, denied this accusation of 'post-hocery', and demanded written documentation that such an agreement had been reached.[38]

There is a nice symmetry between this debate and the earlier one over the Zelen test. The Committee on that occasion had

questioned the construction of the sample, and the Gauquelins had charged the Committee with post-hoc analysis. Now it was the Gauquelins who were questioning the construction of the sample, and it was the Committee who were claiming that the Gauquelins were guilty of post-hoc analysis. This reversal of argument strategies is a striking illustration of the flexible use to which technical arguments can be put in the course of a scientific dispute.

The replication issue subsequently became even more murky after Michel Gauquelin, in 1979, embarked upon his own replication of his earlier studies — a project in which the Committee declined to participate. Positive results were reported. However, Kurtz's group have refused to accept these results and have again questioned the criteria upon which the sampling was based![39] The attempts at experimental replication seem to have been no more definitive than the Zelen test in resolving the issue. In 1980 Kurtz and his colleagues summarized their work as follows:

> An effort at independent replication of the 'mars effect' among 408 US sports champions shows no evidence that such an effect exists, nor does Gauquelin's new study of 432 European sports champions make the case any more convincing.[40]

On the other hand, Michel Gauquelin's own conclusions to his most recent study is somewhat different. He writes:

> In conclusion, it appears more and more obvious that the Mars Effect with famous sports champions is an indisputable fact, that we should now try to understand and explain.[41]

It can be seen that the debate over the Mars Effect (at least that part which we have documented here) has followed a path made familiar in studies of other scientific controversies.[42] Since experiments inevitably rest upon a web of taken-for-granted assumptions and practices, if anyone wishes to dispute the outcomes of experiments all they have to do is question one or more of the assumptions. The data generated in such experiments can be shown to lack compulsion and the performance of more experiments will not necessarily resolve the dispute, since the new data can in turn be questioned. Both sides can maintain their position indefinitely so far as the data are concerned. The outcome of the dispute can be described as a matter of 'negotiation'.

At the beginning of this paper, we claimed that close *involvement*

with a controversial science will at least temporarily engender in the participant a new view of scientific method; we can now see that this appears to be the case. For example, in 1976, Zelen reproduced the rhetoric of the 'standard view' when he referred to his test as 'an objective way for unambiguous corroboration or disconfirmation'[43]; and similarly, Abell described the Zelen test as a 'very definitive test'.[44] But in their very first comment on the results, Zelen, Kurtz and Abell wrote:

> . . . statistical analysis cannot directly prove (or disprove) the existence of the Mars Effect, only that certain models are consistent (or inconsistent) with the data.[45]

And, in 1981, in reply to the allegation that they had been guilty of post-hoc analysis of data, Abell, Zelen and Kurtz wrote:

> . . . such individuals [who make this type of criticism] regard the experiment as a contest with rules which are fixed in advance. This is *not the way science is conducted* nor the way a data analyst analyzes data.[46]

This last comment is particularly ironic since, as we have tried to show, the Committee's whole approach towards the debate appeared to be founded upon the assumption that there were such clear-cut rules. Finally, in a recent 'Reappraisal' (written almost in a confessional tone), Abell, Kurtz and Zelen, in referring to their own earlier statements expressing optimism about the definitiveness of the Zelen test, have written:

> So great was our respect for the Zelen test that two of us have published careless statements about its power.[47]

The conclusion to the 'Reappraisal' also hints at a much more realistic attitude concerning the progress of controversial science:

> We regret that at the outset we had not the foresight to exercise a great deal more care in our experiments and in reporting them. Had we done so, we might have been able to reach conclusions more convincing to others. On the other hand, *it is doubtful if anything we could have done would have settled the matter.*[48]

Still more recently, Kurtz and Abell have waived aside congratulations from Gauquelin on their courage in admitting their errors. They appear to adopt a mantle of almost Olympian magnanimity in remarking:

> . . . we don't know that it is courageous to admit one's errors or revise one's interpretations; this is part of the ongoing process of scientific inquiry.[49]

Fate Lends a Hand

There is, however, another lesson to be learned. Rumours of a cover-up over the Committee's handling of the Mars Effect had been circulating in the scientific community for a number of years — rumours which eventually led to an acrimonious row amongst Committee members. The 'Reappraisal' only appeared after a number of exposés of Committee activities. These developments were precipitated by an extraordinary article in *Fate* magazine, written by ousted-Committee Fellow and Mars Effect investigator, Dennis Rawlins.[50] The article levels a number of charges against the Committee — the most serious of them being that a Watergate-style cover-up was instigated once the results of the Zelen test became apparent. The debate has subsequently been fuelled by a reply from the Committee,[51] and a number of articles which have appeared in the *Zetetic Scholar*[52] (a journal Marcello Truzzi edited after his earlier resignation from the Committee).[53] The net result has been that some Fellows (although as far as we can establish only a small number) have resigned, and that there was considerable pressure to produce the much less dogmatic 'Reappraisal'.

The point at issue in the 'cover-up' story is the matter of experimental privacy, discussed earlier. It is not that the Committee's original interpretation of events was simply wrong, it is just that their interpretation became untenable once the cloak of privacy had been swept aside. They were exactly as wrong as Robert Millikan was wrong when he discovered the charge of the electron by ignoring certain entries in his notebook! But Millikan's notebook was private (until recently[54]). If Millikan's work had been subject to the sort of scrutiny that the Committee regularly provides for the work of parapsychologists, then his results would have not counted as the measurement of the charge of the electron, because he would not have been allowed to discard the problematic data. Likewise, if the Committee had allowed themselves a little more privacy, or had been allowed it by Gauquelin and Rawlins, then the Zelen test need not have supported the Mars Effect. After all, they could just have discarded the Zelen result, as Millikan had disregarded his recalcitrant results. It was their publicly trumpeted procedural rules that prevented this.

It is the canonical model of science which does not allow for a failed experiment. It was Rawlins' honest cleaving to the standard model, in the face of his erstwhile colleagues' dawn of

understanding, that led to the Zelen test's downfall. Experienced scientists know that the prediction-test-statistical-analysis model of science just does not work, because experiments are so unpredictable in difficult areas.

The wonderful irony of the whole incident, as far as the Committee and its journal are concerned, is that the *Skeptical Inquirer* did not itself draw aside the veil of privacy from the Zelen test. From the beginning, *SI* and the Committee as a whole have been ambivalent — indeed uncertain — about their role. Sometimes the Committee seems to feel that it can conduct its own tests and report them — acting as though it were entitled to constitute new knowledge (see, for example, the report by the Committee on their spoon-bending test[55]). Sometimes the Committee acts solely in a popularizing role. The most recent statements claim that there are no such things as Committee tests.[56]

In the example of Targ and Puthoff's remote viewing experiments, we saw the *SI* acting in its role as a popular journal in order to personalize the presentation in *Nature*, and dissolve the facticity of the claims. Naturally, as regards the Zelen test, the Committee and its journal presented themselves as disinterested automata competently constituting facts. But big fleas have little fleas upon their backs to bite them! *Fate*, a pulp magazine sold at news-stands, and advertising horoscopes and lucky charms, was the little flea in this case. *Fate* played popular forum[57] to *SI*'s role as scientific journal. It was only *Fate* that made it possible for Rawlins to personalize, and thus dissolve, the Committee's pretentions.

Conclusion

In this paper we have tried to show the part played by the formal and informal literatures in the social construction of scientific knowledge. The formal literature maintains the privacy of laboratory activity, as does, in the normal way, the popular literature. However, the popular literature can be used to invade this privacy and present science as a much more human, contingent activity. In this process facts, previously established by their presentation in the formal literature, can be deconstructed. Substantial involvement with scientific controversy reveals the 'negotiated' character of science and ought to make it easier to see the way that different media and their uses can affect the outcome

of scientific debate. To some extent, involvement in controversy reveals the active face of science even to the scientists themselves.

As regards the Committee itself, and similar scientific-vigilante organizations, there are lessons to be learned. The Committee's main platform for attack upon parapsychology and astrology has been the standard, or canonical, model of science. This is a strategy that can only be used in complete safety by organizations that do not engage in controversial science themselves. Only by avoiding having to face up to the problems of doing controversial science, and by avoiding the changed consciousness concerning scientific method which accompanies such engagement, can an attack from the canonical model be sustained without difficulty. It was the engagement with astrological research which forced the Committee members to re-appraise their understanding of scientific method. Even then, provided they had been prepared to endorse the canonical model in public while operating in a rather different way in private, they could have maintained their position. But once the observational privileges that they demanded for themselves as regards others' scientific work were extended to their own activities — not exactly voluntarily in this case — then the problem of reconciling public rhetoric about scientific knowledge with private science became too great.

In the last resort, however, Committee members have sacrificed themselves to maintain the public image. Abell, Kurtz and Zelen regret their own lack of foresight, their carelessness and their errors, to account for the unfortunate incidents. No systematic new understanding or re-appraisal of scientific method has been endorsed by the Committee, only personal failure. The Committee's new position is that it will continue to fight the battle from the platform of the canonical version of science — preserving the ideology as it does so — while sensibly keeping its own hands clean and avoiding the risks of doing any experimental science itself. Under these circumstances, there should be no further trouble for this and similar organizations.

Excerpts from the *Nature* Paper by Targ and Puthoff

REMOTE VIEWING OF NATURAL TARGETS

A study by Osis[8] led us to determine whether a subject could describe randomly chosen geographical sites located several miles from the subject's position and demarcated by some appropriate means (remote viewing). This experiment carried out with Price, a former California police commissioner and city councilman, consisted of a series of double-blind, demonstration-of-ability tests involving local targets in the San Francisco Bay area which could be documented by several independent judges. We planned the experiment considering that natural geographical places or man-made sites that have existed for a long time are more potent targets for paranormal perception experiments than are artificial targets prepared in the laboratory. This is based on subject opinions that the use of artificial targets involves a 'trivialisation of the ability' as compared with natural pre-existing targets.

In each of nine experiments involving Price as subject and SRI experimenters as a target demarcation team, a remote location was chosen in a double-blind protocol. Price, who remained at SRI, was asked to describe this remote location, as well as whatever activities might be going on there.

Several descriptions yielded significantly correct data pertaining to and descriptive of the target location.

In the experiments a set of twelve target locations clearly differentiated from each other and within 30 min driving time from SRI had been chosen from a target-rich environment (more than 100 targets of the type used in the experimental series) prior to the experimental series by an individual in SRI management, the director of the Information Science and Engineering Division, not otherwise associated with the experiment. Both the experimenters and the subject were kept blind as to the contents of the target pool, which were used without replacement.

An experimenter was closeted with Price at SRI to wait 30 min to begin the narrative description of the remote location. The SRI locations from which the subject viewed the remote locations consisted of an outdoor park (Experiments 1, 2), the double-walled copper-screen Faraday cage discussed earlier (Experiments 3, 4, and 6–9), and an office (Experiment 5). A second experimenter would then obtain a target location from the Division Director from a set of travelling orders previously prepared and randomised by the Director and kept under his control. The target demarcation team (two to four SRI experimenters) then proceeded directly to the target by automobile without communicating with the subject or experimenter remaining behind. Since the experimenter remaining with the subject at SRI was in ignorance both as to the particular target and as to the target pool, he was free to question Price to clarify his descriptions. The demarcation team then remained at the target site for 30 min after the 30 min allotted for travel. During the observation period, the remote-viewing subject would describe his impressions of the target site into a tape recorder. A comparison was then made when the demarcation team returned.

Price's ability to describe correctly buildings, docks, roads, gardens and so on, including structural materials, colour, ambience and activity, sometimes in great detail, indicated the functioning of a remote perceptual ability. But the descriptions contained inaccuracies as well as correct statements. To obtain a numerical evaluation of the accuracy of the remote viewing experiment, the experimental results were subjected to independent judging on a blind basis by five SRI scientists who were not otherwise associated with the research. The judges were asked to match the nine locations, which they independently visited, against the typed manuscripts of the tape-recorded narratives of the remote viewer. The transcripts were unlabelled and presented in random order. The judges were asked to find a narrative which they would consider the best match for each of the places they visited. A given narrative could be assigned to more than one target location. A correct match requires that the transcript of a given date be associated with the target of that date. Table 2 shows the distribution of the judges' choices.

Among all possible analyses, the most conservative is a permutation analysis of the plurality vote of the judges' selections assuming assignment without replacement, an approach independent of the number of judges. By plurality vote, six of the nine descriptions and locations were correctly matched. Under the null hypothesis (no remote viewing and a random selection of descriptions without replacement), this outcome has an *a priori* probability of $P = 5.6 \times 10^{-4}$, since, among all possible permutations of the integers one through nine, the probability of six or more being in their natural position in the list has that value. Therefore, although Price's descriptions contain inaccuracies, the descriptions are sufficiently accurate to permit the judges to differentiate among the various targets to the degree indicated.

The Beginning of Marks' *SI* Paper

Data Suppression

Science separates itself form pseudoscience along a number of dimensions. One of these dimensions is accessibility of the data. Scientific data are consensually validated by open inspection of the recorded observations or through replication of the relevant phenomena. Following publication of major observations it is an accepted practice in science for researchers to allow colleagues who are doing serious research in the same field to have access to their original data. When researchers consistently refuse to allow colleagues such access, something important is being signaled. Of course data may get lost or destroyed or be difficult or costly to retrieve in the form required. Or they may be classified information or have commercial value that a scientist may wish to exploit prior to their general release. However, when none of these considerations is applicable, a refusal to supply a copy of a set of data leads to the unpleasant inference that something is wrong, that the data do not support what is claimed for them, that the data are an embarrassment following an extravagant claim that cannot be substantiated.

Over the past few years I have made frequent requests to Puthoff and Targ for copies of their remote viewing (RV) transcripts obtained in their experiments reported in *Nature* (1974), *Proceedings of the Institute of Electrical and Electronic Engineers* (1976), and *Mind-Reach* (1977). Targ and Puthoff have consistently refused to supply this important information, as they have to all others I know who made this request. The only concession has been to supply a single transcript from the Price series (Experiment 7), which is published in *Mind-Reach*. Normally no explanation or reply follows such requests. However in May 1979, while preparing the manuscript of *Psychology of the Psychic* I did receive the following explanation from Puthoff:

> With regard to your request, at the present time we are subjecting the transcripts to a number of blind judging procedures in order to assess which of several approaches constitutes the best way to handle free-response text in remote viewing studies. As a result we are not ready to release the materials, since premature disclosure would prevent further blind analysis work. I am sure you can appreciate the fact that after spending years building up a data base, we don't want to do anything that would jeopardize our options with regard to blind analysis of this data pool.
>
> By the way, this work is proceeding quite well. We have several research analysts breaking transcripts down into concepts which are then individually rated against concepts generated for the targets. . . .
>
> *When our blind analysis work has been completed, you may of course have access to the raw data.* I do not think it will be in 1979, however, as our analysis contracts extend beyond that; thus, I doubt it will be in time for your book." [Italics mine.]

Three years later I'm still waiting! Surely "several research analysts" are not still "breaking transcripts down into concepts." *I would like to publicly ask Targ and Puthoff to release all of their remote-viewing transcripts in their original unedited form.* This would enable members of the more skeptical scientific community to evaluate the data for themselves.

Targ and Puthoff supplied John Wilhelm with a single transcript (Price series, Experiment 4, Redwood City Marina), reproduced in Wilhelm's book *The Search for Superman* (pp. 213-18). Although this was purported to be a complete transcript, it actually had two pages missing. A copy of the whole transcript given to me by Arthur Hastings (the SRI judge) contained nine pages. Pages 6 and 7 are missing from Wilhelm's version, and one can only speculate on the implications of the fact that one of Targ's most blatant cues ("Nothing like having 3 successes behind you") was on the missing page 7. Who could avoid the conclusion after reading this cue that this experiment was number 4 in the series?

The refusal to release the data-base for their controversial claims is a disturbing feature of Puthoff and Targ's remote-viewing project. The task of presenting an accurate and fair evaluation of the research is made none the easier as a consequence.

Fortunately, the main judge in the SRI research team, Arthur Hastings, was more helpful. Hastings loaned me a complete set of nine transcripts from the Price series and showed me six of the nine Hammid

transcripts. However, Hastings said he felt "paranoid" about releasing this information and soon requested their return as they had become "confidential." How much longer these data will remain confidential is anybody's guess. It may be forever.

The 1970s saw the emergence of an astonishing psychic phenomenon that the principal investigators called "remote viewing." This refers to an alleged ability to perceive information from remote sources not available to any known sense. Actually this product of the paranormal is not a new one—it is really good old faithful ESP in a new package and with a different brandname. The main promoters of remote viewing are two physicists at SRI International (formerly the Stanford Research Institute), Russell Targ and Harold Puthoff. The remote-viewing effect could apparently be obtained by anybody and it required no special training or unique abilities. The results were allegedly reliable and repeatable. In fact, remote viewing was every parapsychologist's dream come true.

This is how one observes remote viewing. Someone (let's call him the traveler, T) gets into a car and drives to a place some distance away (e.g., a park, a church, a city hall, a railway station, or a golf course). Someone else (let's call him P) waits with the subject (S) with a tape recorder and a drawing pad. At a prearranged time, when T will have arrived at the target location, S tries to describe the location using his or her imagination. Experimenter P may assist S by asking various questions to clarify the description. S may also draw a picture of the target location. T allegedly acts as a kind of "psychic beacon" beaming back information from the target using some unknown sensory modality. After a prearranged interval (say 15 or 30 minutes) of remote viewing by S, T returns and escorts S to the target site to provide feedback on how well S has done. The experiment is repeated a number of times (e.g., 9).

Generally speaking, S, T, and P are all highly delighted with the results and all kinds of matching elements are found between S's descriptions and aspects of the target sites. I can personally vouch for the reliability of the remote-viewing effect when evaluated by simple inspection of the target site immediately after S has produced his response. I have found the remote-viewing experience compelling and direct and, on occasion, eerie. However it is obviously important to validate the effect on a more objective level. To this end, S's taped descriptions are transcribed and, together with any drawings that may have been produced, the transcripts are given to a judge who tries to match them against the series of targets. The judge normally visits all the sites and ranks the transcripts at each site in the order of their degree of matching.

Targ and Puthoff (1974, 1976, 1977) have reported extremely successful results from this judging process, and so a compelling subjective phenomenon has apparently been quantified numerically in the form of probability values. The implications of remote viewing (if real) for science are enormous. Textbooks of psychology, physiology, anatomy, and physics would all need to be completely rewritten, as our knowledge of the bases of perception and psychophysics built up over the ages would have to undergo major revision. Before this revolution in science begins, however, it would seem prudent to examine the phenomenon of remote viewing more closely to determine whether there is any possibility of artifacts, flaws in the methodology, experimenter bias, or some other parsimonious explanation.

With this in mind I began an investigation in 1975, in association with Richard Kammann, the results of which were published in our coauthored book *The Psychology of the Psychic*. Like many other investigators we were unable to replicate the effect claimed by Puthoff and Targ, as none of our judges could match transcripts accurately. Following visits to SRI and multiple discussions with Targ, Puthoff, and their key judge, Arthur Hastings, it became clear that the SRI research program, promoted as well-controlled science, was actually a massive litany of fallacies and flaws. To avoid duplication, this article will take up the story more or less where our book left it two years ago. This review relates to the two series of experiments with Price and Hammid, and various attempted replications by other investigators.

• NOTES

We would like to acknowledge the help of Susan Hemmings, who provided us with much background material on the Committee and its origins. We are also grateful to Marcello Truzzi for much useful information and documentation and to the editors and publishers of the *Skeptical Inquirer* and *Nature* for permission to use extracts from their journals in this article.

1. For an account of the 'standard model' and its demise, see M. Mulkay, *Science and the Sociology of Knowledge* (London: George Allen & Unwin, 1979).

2. For example, see H.M. Collins, 'The Seven Sexes: A Study in the Sociology of a Phenomenon, or the Replication of Experiments in Physics', *Sociology*, Vol. 9 (1975), 205–24.

3. For an account of the emergence of this model, see H.M. Collins, 'The Sociology of Scientific Knowledge: Studies of Contemporary Science', *Annual Review of Sociology*, Vol. 9 (1983), 265–85.

4. B. Latour and S. Woolgar, *Laboratory Life* (London and Beverly Hills, Calif.: Sage, 1979).

5. Ibid., 79.

6. Note that we have constructed this statement to exemplify Latour and Woolgar's point — it is not to be found in their data.

7. Again we have manufactured this statement to make the example work.

8. H.M. Collins and T.J. Pinch, *Frames of Meaning: The Social Construction of Extraordinary Science* (London: Routledge & Kegan Paul, 1982). For a model of the social negotiation of scientific facts which makes particular reference to the use of the literature in the 'constitutive' and 'contingent' forums, see H.M. Collins and T.J. Pinch, 'The Construction of the Paranormal: Nothing Unscientific is Happening', in R. Wallis (ed.), *On the Margins of Science, Sociological Review Monograph* No. 27 (Keele, Staffs.: University of Keele, 1979), 237–70.

9. For a detailed account of the 'Geller Effect' and the atmosphere at the time, see Collins and Pinch (1982), op. cit. note 8.

10. B.J. Bok, L.E. Jerome and P. Kurtz, 'Objections to Astrology', *The Humanist* (September-October 1975), 4–5.

11. For example, Collins received the newsletter pre-1975.

12. See *Zetetic Newsletter*, Vol. 3, No. 2 (1975), 1–2.

13. For Truzzi's own account of events, see J. Clark and J.G. Melton, 'Marcello Truzzi Talks about the Crusade Against the Paranormal — Part 1', *Fate* (September 1979), 70–76.

14. The active members of the Committee tend not to be the distinguished 'Fellows'.

15. R. Targ and H.E. Puthoff, 'Information Transfer Under Conditions of Sensory Shielding', *Nature*, Vol. 251 (18 October 1974), 602–07.

16. For more information on these experiments, see Collins and Pinch (1982), op. cit. note 8.

17. D.F. Marks, 'Remote Viewing Revisited', *Skeptical Inquirer*, Vol. 6, No. 4 (1982), 18–29.

18. H.M. Collins, 'The Place of the "Core-Set" in Modern Science: Social Contingency with Methodological Propriety in Science', *History of Science*, Vol. 19 (1981), 6–19.

19. M. Zelen, 'Astrology and Statistics: A Challenge', *The Humanist* (January-February 1976), 32–36.

20. The involvement of the Committee with the Mars Effect has itself become somewhat of a controversial issue with claims being made that the Zelen test was not a *Committee* activity as the Committee, as such, was not in existence when Zelen first published his challenge. However, that it soon became sanctioned by the Committee can be seen from the writings of Kurtz, where he refers to the test as a Committee activity and talks about the Committee's view of the test. See P. Kurtz, 'The Mars Effect and the Zelen Test', *The Humanist* (November-December 1977), 29.

21. This figure had earlier been called into question by a Belgian group (Commite Para). They had attempted to replicate the Gauquelins' findings. After obtaining what seemed to be positive results they claimed that the test was inconclusive because of the ambiguity as to the expected theoretical frequency of the Mars Effect amongst non-champions.

22. Zelen, op. cit. note 19, 33.

23. G.O. Abell, A.A. Abell, M. Gauquelin and F. Gauquelin, 'A Test of the Gauquelin Mars Effect', *The Humanist* (September-October 1976), 40–45, at 44.

24. M. Gauquelin and F. Gauquelin, 'The Zelen Test of the Mars Effect', *The Humanist* (November-December 1977), 30–35.

25. M. Zelen, P. Kurtz, and G. Abell, 'Is there a Mars Effect?', *The Humanist* (November-December 1977), 36–39.

26. Kurtz, op. cit. note 20.

27. Zelen, quoted by the Gauquelins, op. cit. note 24, 35.

28. Ibid., 35.

29. Zelen, Kurtz and Abell, op. cit. note 25, 37.

30. Ibid., 38.

31. This point has been made by P. Curry, 'Research on the Mars Effect', *Zetetic Scholar*, No. 9 (1982), 34–53; and by R. Kammann, 'The True Disbelievers: Mars Effect Drives Skeptics to Irrationality', *Zetetic Scholar*, No. 10 (1982), 50–65.

32. See E. Tarkington, 'Gauquelin's Travels', *Phenomena*, Vol. 2 (1978), 18–20; and Kammann, op. cit. note 31.

33. This point has been made by D. Rawlins, 'sTARBABY', *Fate*, Vol. 34, No. 10 (1981), 67–98; and Curry, op. cit. note 31.

34. A useful review of the whole debate is to be found in Curry, op. cit. note 31.

35. P. Kurtz, M. Zelen and G. Abell, 'Results of the US Test of the "Mars Effect" are Negative', *Skeptical Inquirer*, Vol. 4, No. 2 (Winter 1979–80), 19–26; and D. Rawlins, 'Report on the US Test of the Gauquelins' "Mars Effect"', ibid., 26–31.

36. M. Gauquelin and F. Gauquelin, 'Star US Sportsmen Display the Mars Effect', *Skeptical Inquirer*, Vol. 4, No. 2 (Winter 1979–80), 31–40.

37. P. Kurtz, M. Zelen and G. Abell, 'Response to the Gauquelins', *Skeptical Inquirer*, Vol. 4, No. 2 (Winter 1979–80), 44–63.

38. This point is made in a widely circulated letter from M. Gauquelin to Paul Kurtz, 1 June 1981.

39. This latest round of the debate is describe in Curry, op. cit. note 31.

40. P. Kurtz, M. Zelen, and G. Abell, 'The Contradictions in Gauquelin's Research: Rejoinder by Kurtz, Zelen and Abell,' *Skeptical Inquirer*, Vol. 4, No. 4 (Summer 1980), 62–68.

41. M. Gauquelin, 'The Mars Effect and the Sports Champions: A New

Replication' (Paris: Laboratoire D'Etude des Relations entre Rythmes Cosmiques et Psychophysiologiques, Series D, Vol. 6, 1979), 24.

42. See, for instance, the collection of studies in H.M. Collins (ed.), *Knowledge and Controversy, Social Studies of Science*, Vol. 11 (1981), 3–158.

43. Zelen, op. cit. note 19.

44. Abell, op. cit. note 23.

45. Zelen, Kurtz and Abell, op. cit. note 25, 36.

46. G. Abell, P. Kurtz and M. Zelen, 'Status of the Mars Effect', document available from CSICOP, 4 (our emphasis).

47. G. Abell, P. Kurtz and M. Zelen, 'The Abell-Kurtz-Zelen "Mars Effect" Experiments: A Reappraisal', *Skeptical Inquirer*, Vol. 7, No. 3 (Spring 1983), 77–82, 78.

48. Ibid., 82 (our emphasis).

49. P. Kurtz and G. Abell, ' "Paul Kurtz and George Abell Respond": (A Response to a letter by Michel Gauquelin)', *Skeptical Inquirer*, Vol. 8, No. 1 (Fall 1983), 87–88, at 87.

50. Rawlins, op. cit. note 33.

51. See P.J. Klass, 'CRYBABY', unpublished document, circulated amongst interested parties in 1981.

52. See, for example, Curry, op. cit. note 31; Kammann, op. cit. note 32; and R.A. McConnell and T.K. Clark, 'Guardians of Orthodoxy: The Sponsors of the Committee for the Scientific Investigation of Claims of the Paranormal', *Zetetic Scholar*, No. 10 (1982), 43–49; and M. Truzzi, 'Personal Reflections on the Mars Effect Controversy', ibid., 74–81.

53. See note 13.

54. See G. Holton, *The Scientific Imagination: Case Studies* (Cambridge: Cambridge University Press, 1978), for discussion of the Millikan Case. See also A.D. Franklin, 'Millikan's Published and Unpublished Data on Oil Drops', *Historical Studies in the Physical Sciences*, Vol. 11 (1981), 185–201.

55. James Randi, 'The CSICOP Conducts a Test of Psychokinesis', *Skeptical Inquirer*, Vol. 2, No. 2 (Spring-Summer 1978), 36-39.

56. See the statement 'Policy on Sponsoring Research, Testing Individual Claims, and Conducting Investigations of Alleged Paranormal Powers and Phenomena', *Skeptical Inquirer*, Vol. 6, No. 3 (Spring 1982), 9.

57. Collins and Pinch, 'Construction of the Paranormal', op. cit. note 8.

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