Journal of Scientific Exploration, Vol. 17, No. 2, pp. 271–282, 2003 0892-3310/03

Parapsychology: Science or Pseudo-Science?

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Abstract—Do paranormal or parapsychological investigations meet the criteria often said to characterize pseudo-science? Mainstream and non-mainstream research is compared through content analysis of selected samples of mainstream journals from several fields and of non-mainstream (''fringe'') journals. Oral communication processes were studied at an annual meeting of the Parapsychological Association. Though certain quantitative differences were noted, qualitative distinctions were not found that could justify classification of parapsychology as pseudo-science. To warrant that, other criteria to define science would need to be established.

Keywords: parapsychology — paranormal research — pseudo-science — parapsychology in France — scientific method

Introduction

The idea for this study came from a polemic by the French historian and philosopher, Bertrand Méheust (1996). His chief points, which relate mainly to the situation in France, are:

> Paranormal phenomena used to be investigated and debated by prominent scientists (astronomer Camille Flammarion, physicists William Crookes and Oliver Lodge, Nobelists Charles Richet, Pierre and Marie Curie) toward the end of the 19th century. The debates were intense and of high standard. The work was commonly published in mainstream journals. The field used to be called ''Métapsychique.''
>
> Nowadays these matters are completely dismissed by the scientific community, which pretends that the debate is over, superseded, absurd. It has become taboo, ''one of the most powerful bans of modern times.''

However, the debate was never really resolved.

This essay[1] discusses the questions: Is there any international research on the paranormal that could be regarded as scientific? Are French scholars right to dismiss such investigation as pseudo-science? These questions are addressed by analyzing communication within the community investigating paranormal phenomena. The focus will be on parapsychology, the closest modern term to what Bertrand Méheust refers to as ''Métapsychique.''

Definitions

Many efforts have been made to set up ''necessary and sufficient'' criteria for science (Chalmers, 1999), but no consensus has been reached. Why not? One

might argue there are no such things as ''epistemological invariants'' for science, that each discipline (or even each lab) could have its own rules and methods (see Zingrone, 2002). However, even if there is no clear criterion for science, there must be a way to distinguish it from pseudo-science if the latter term is to have any meaning.

One possible way to identify science is to look at its results: It works or it does not work. There are three useful weapons that science commonly uses in its war against ignorance: falsifiability, reproducibility and predictability. Thanks to falsifiability, science rejects what does not work; thanks to reproducibility and predictability, it can welcome and accept what works. These three features are essential for science to get its results and to progress.

A second way to identify science is by examining its method. The two main bases of scientific methodology, induction and deduction (i.e., gathering empirical evidence and dealing with theory) have been extensively described by Chalmers (1999). The cohabitation of these two processes reflects an essential component of scientific methodology: relentless confrontation of theories and facts. On the one hand, science cannot escape facts; on the other hand, its goal is to go beyond facts, to find underlying mechanisms.

The scientific attitude is defined by the question, ''How does it work?'' One looks, and guesses; *and the answer is never taken for granted*, so one looks again and guesses again. Questioning facts and theories is the fundamental attitude of the (true) scientist. And the scientist has not only to address his own observations and experiments, he must address those of other scientists as well.

Selected Criteria for Pseudo-Science

Criteria had to be chosen for the present study.

Distinguishing science from pseudo-science by examining results will not work:

1. Predictability, falsifiability and reproducibility are not essential criteria for science (see Stevenson, 1999).
2. That results do not fit a mainstream theory can be an indicator of the emergence of a new paradigm in science (Kuhn, 1970). This is the whole problem of how anomalies are handled. They can trigger a major scientific discovery (a ''revolution''), or on the contrary constitute a basis for pseudo-science; and it is often impossible to predict the future of an anomaly at a given time. Examples are numerous in the history of science, where aberrant results inconsistent with mainstream theories ended up as major discoveries. One of the most famous is the anomaly in the classical theory of light, widely debated, which finally revealed the quantum aspect of light.

These arguments are obviously extensively used by researchers of the paranormal to defend themselves against attacks from mainstream scientists criticizing their lack of well-confirmed theories and practical results. Others

besides Stevenson (1999) have proposed a re-definition of scientific criteria, for example Jahn and Dunne (1997) define a ''neo-subjective'' science that retains logical rigor and empirical/theoretical dialogue.

This type of defense is dismissed derisively by skeptics without offering any good reasons. I would rather agree with Lakatos' view, summarized by Steven E. Phelan (n.d.) as follows:

> The existence of anomalies makes falsification untenable as a doctrine. In place of falsifiability as a demarcation criterion, Lakatos has proposed distinguishing between 'progressive' and 'degenerative' research programs (RPs). A progressive research program makes a few dramatic, unexpected, stunning predictions. An RP that ceases to make novel predictions is degenerating. Scientists tend to move to progressive programs and away from degenerating programs although Lakatos does not condemn those trying to turn a degenerating program into a progressive one.

Thus predictability and reproducibility usually bring results; results bring consensus and acceptance by mainstream science. However, this process is the final objective. A science in the making may not yet have gone through these different stages. That does not mean that it will not, that it is not science. That is why the criterion of assessing what is science by its results is not reliable.

The methodological approach appears to be more consistent. It emphasizes confrontation of facts and theory, both from one's own work and from one's peers', *continual questioning* as opposed to *knowledge being taken for granted*, which is the true invariant of pseudo-science. Indeed, the philosopher and physicist Mario Bunge (1984) once suggested that, rather than dividing disciplines into ''sciences'' versus ''non-sciences'', we ought instead to characterize them as ''research fields'' or ''belief fields.'' The criteria used in this article for distinguishing science from pseudo-science, based on the methodological approach (Bunge, 1984; Strahler, 1999; Thagard, 1988), are shown in Table 1.

The written communication process was compared in three mainstream journals (*British Journal of Psychology, Experimental Physiology, Journal of Physics B: Atomic, Molecular and Optical Physics*) and four ''fringe'' journals (*Journal of Scientific Exploration (JSE), Journal of Parapsychology, Journal of Psychical Research, Revue Française de Parapsychologie*). Comparisons were based on[2]:

1. content analysis, to assess the empirical and theoretical approach;
2. citation analysis, to assess interaction with peers; and
3. survey of peer-review practices and observations at a conference to complete this assessment.

Scientific Criteria are Met

Induction and Deduction

Pseudo-science *neglects empirical matters,* yet 43% of articles in the fringe journals deal with empirical matters and almost one-fourth report laboratory experiments.

TABLE 1
Criteria to Differentiate Science from Pseudo-Science

Scientist	Pseudo-scientist
Induction and deduction	
Gathers or uses data, particularly quantitative data	Neglects empirical matters
Seeks empirical confirmations and disconfirmations	Suppresses or distorts unfavorable data
Uses correlation thinking (e.g., A regularly follows B in controlled experiments)	Uses resemblance thinking (e.g., Mars is red, red is the color of blood, therefore Mars rules war and anger)
Relies on logic	Formal background modest, little mathematics or logic
Proposes and tries out new hypotheses	Over-reliance on testimonials and anecdotal evidence
Questioning and confrontation	
Admits own ignorance, hence need for more research; finds own field difficult and full of gaps	Does not admit own ignorance and need for more research
Consistent with scientific work in other fields	No overlap with another field of research
Seeks critical comments from others	Falls back consistently on authority
Practitioners care about evaluating theories in relation to alternative theories	Practitioners oblivious to alternative theories (pseudo-scientists make little attempt to solve problems with the theory or evaluate the theory in relation to other alternatives)
Communication strategy	
Writes papers than can't be understood by everyone	Uses obscurantist language

Pseudo-science *suppresses or distorts unfavorable data*, whereas science *seeks empirical confirmations and disconfirmations*: but almost half of the fringe articles report a negative outcome (disconfirmation). By contrast, no report of a negative result has been found in my sample of mainstream journals.

Science *relies on logic* and *uses correlation thinking*; pseudo-science is supposed to have a *poor formal background*, to use *little mathematics or logic* and to prefer *resemblance thinking*: On the contrary, my two samples use similar statistical tests. All the articles that aim to gather new empirical evidence, whether in fringe journals or in mainstream journals, use statistical analysis.

Science *proposes and tries out new hypotheses* whereas pseudo-science *relies too much on testimonials and anecdotal evidence*: But 17% of fringe articles deal with theory and propose new hypotheses. Two-thirds offer explanations consistent with mainstream theories (the others offer such explanations as spiritual entities).

On all counts, this sample of fringe journals satisfies the methodological criteria for proper science.

Questioning and Confrontation

Scientists, but not pseudo-scientists, *admit their own ignorance and the need for more research and find their own field difficult and full of gaps:* Yet 29% of the fringe-journal articles (''reflection articles'' in Table 2) discuss progress of research, problems encountered, epistemological issues. This kind of article is completely absent from the mainstream sample. Thus fringe journals fit the ''science'' criterion closer than do mainstream journals. They do not dismiss negative results and do question their work and their results.

Scientists *seek critical comments from others and care about evaluating theories in relation to alternative theories:* Fringe articles include an average of 20 other-author citations. Sixty-four percent of the works cited are taken from scientific journals or edited compilations of articles, which are usually peer-reviewed. The fringe articles *don't fall back consistently on authority* as is expected from pseudo-science. In this respect, they meet the criteria of seeking critical comments and evaluating theories in relation to alternative theories.

Science is *consistent across disciplines,* whereas pseudo-science *does not overlap with other fields of research:* 64% of the citations are found in paranormal publications, but also well represented are mainstream journals in psychology, general science, neurosciences, and physics. In the mainstream sample, more than 90% of the citation references are from the same field (up to 99% in physics). Here again, the pseudo-science criterion *no overlap with another field of research* appears to apply more to the mainstream articles than to the fringe ones.

Communication Strategy

Epistemologists disagree as to the nature of the vocabulary that characterizes pseudo-scientists. Bunge (1984) considers that they use ''*obscurantist language,*'' whereas Strahler (1999) thinks they ''*write papers that can be understood by everyone*'' (which is not the case in ''real'' science). In any case, this is not much of an issue: both types of papers are present in both fringe and mainstream journals. Some papers are necessarily very technical—say, several of the theoretical papers in *JSE* that deal with physical calculations—and some are not.[3]

To conclude: all the selected criteria that purport to characterize science are met by the fringe publications. Empirical evidence and theoretical explanations are sought and confronted with those of other researchers. The main feature that in my opinion reveals true science—never to take things for granted and always to question the validity of one's findings or even of one's research—is definitively found; note in particular the large number of ''reflection articles.'' Interestingly, some so-called ''scientific'' criteria—reporting negative results, openness to critics, reflections on the progress of the research, interdisciplinary approach and overlap with other fields of research—are more common in the fringe articles than in the mainstream articles.

TABLE 2
Comparison of Fringe and Mainstream Journals

	Fringe	Mainstream	Significant difference*
Induction and deduction			
Empirical data[a]	43%	64%	$p < 0.05$
Experiment	24%	57%	$p < 0.01$
Theory	17%	29%	
Confirmation	21%	71%	$p < 0.001$
Disconfirmation	19%	0%	
Rational explanation	31%	83%	$p < 0.001$
Questioning and confrontation			
Reflection articles	29%	0%	
References from outside the field	36%	<10%	
Number references per article[b]	20	32	$p < 0.05$
Percent auto-citation	12%	10%	$p < 0.05$
Citation of books[c]	36%	6%	$p < 0.001$

[a] Experiments, case studies and surveys.
[b] References which are not auto-citations.
[c] "Single author books"; not edited compilations of articles.
* Chi-squared test (except one with ANOVA)[b].

Thus there is no qualitative difference between fringe and mainstream under these criteria; both appear to be science. There is, however, a significant quantitative difference in the extent to which the criteria are met.

Fringe Science is Different from Mainstream Science

Statistical comparisons reveal significant quantitative differences between fringe and mainstream journals (Table 2).

Induction and Deduction

The tests for proper science, especially *seeks empirical confirmations* and *proposes and tries out new hypotheses,* are quantitatively less fulfilled, to a significant extent, in fringe journals than in mainstream ones (except for the already noted lack of disconfirmations in the mainstream journals).

Although statistical tests were, in general, similarly used, the fringe journals emphasized chi-square tests (24% of the 14 articles studied), whereas the mainstream journals used ANOVA more frequently (27% of the 31 articles studied). This could reflect a greater amount of qualitative data more suited to chi-square test in the fringe journals, and more quantitative measures in the mainstream journals. Thus the "real science" criterion: "Gathers or uses data, particularly *quantitative* ones" tends to be less fulfilled in the fringe journals.

Questioning and Confrontation

Fringe articles include significantly fewer references and have a higher proportion of auto-citations.

The nature of the references also shows significant differences: the proportion of references to books, in contrast to scientific journals, is higher in fringe articles. The significant point is that books are not usually peer-reviewed as stringently as work published in a journal. Thus the references to other researchers in fringe journals are more often to general opinion than to peer-reviewed work. Here again, the science criteria *Seeks critical comments from others* and *Practitioners care about evaluating theories in relation to alternative theories* are not met to the same extent in fringe journals as in mainstream journals.

Are These Results Indicators of the Smallness of the Community of Researchers?

A possible reason for these quantitative differences is the meager resources dedicated to paranormal research—few researchers and little funding. The Parapsychological Association (PA) has 300 members, of whom only a small proportion (around 50)[4] are doing full-time laboratory research on a wide range of phenomena (not only "psi" but also such matters as UFOs and homeopathy). A scientist working in such an area has few studies to refer to other than his own work. This accounts for the smaller number of other-author citations and the greater proportion of auto-citations and references to non-peer-reviewed popular journals and general books (in fact, a few books stand out as standard references and are cited in many different articles). This reliance on a few authors could be interpreted as following the criterion *Falls back consistently on authority,* if these authors were not themselves researchers exhibiting a properly questioning attitude.

However, one could also interpret these features as reflecting a field that is not progressing rapidly.

Are These Results an Indicator of the Progression Rate of Parapsychology?

The tendency to handle qualitative rather than quantitative data, illustrated by the prevalence of chi-squared tests, is likely to hinder the development of paranormal research. It is obviously harder to build and check hypotheses on material that can't be quantified:

> When you can measure what you are speaking about, and express it in numbers, you know something about it; but when you cannot measure it, when you cannot express it in numbers, your knowledge is of a meager and unsatisfactory kind: it may be the beginning of knowledge, but you have scarcely, in your thoughts, advanced to the stage of science, whatever the matter may be (Lord Kelvin, 1889).

The large number of epistemological articles (29%) mainly accounts for the

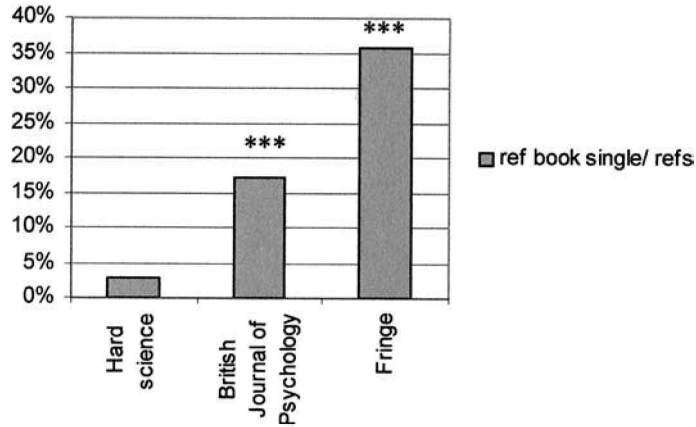

Fig. 1. Proportion of single-author book citations to the total amount of citations in different journals. *** Differences significant at $p < 0.001$.

smaller proportion of experimental works and also partly explains the greater amount of book references. This kind of essay deals more with ideas and opinion than with specific scientific work. The controversial status of this kind of research obviously inspires reflection on epistemological questions. However, it may also be that the small amount of successful experimental work and empirical data causes editors to publish other material. Added to the generally smaller number of references, this could foster the view that parapsychology is not very progressive.

However, the quantitative overall differences between fringe and mainstream science become less compelling when comparisons are made with individual mainstream disciplines. Thus the *British Journal of Psychology* stands in the middle as far as book references are concerned (Figure 1); evidently psychology relies more than hard science on general books compared to specific work published in journals. As pointed out by Remy Chauvin (1999, p. 319), the progress of psychology has not been impressive: ''Everybody knows that in a number of cases, a drug in a syringe gives a much more rapid result than a lot of psychological therapy.''

A low proportion of experiment reports, too, is not restricted to fringe science. Indeed, the proportion of experiments in the physics journal (37%) is not significantly different from that in the fringe journals (24%) and it is significantly smaller than in the other mainstream journals (74%) (Figure 2). Perhaps this reflects the difficulty of conducting experiments in this particular field of physics.

Conclusion

The choice in this article of a more qualitative assessment, based on general attitude and methodology rather than on actual advances, seems justified by the

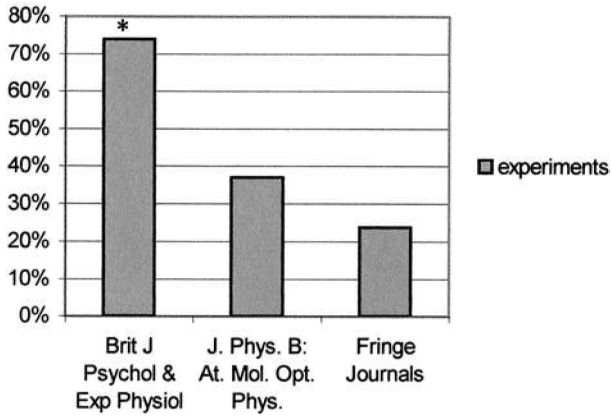

Fig. 2. Proportion of laboratory experiments in different journals. * The difference is significant at $p < 0.05$.

equivocal results of the quantitative comparisons. Moreover, judgments about the progress of parapsychology seem to be quite subjective. Skeptics usually claim that there has been absolutely no progress after a century of research (Alcock, 1991), whereas many parapsychologists assume that progress in their field cannot be questioned. For example:

As a working scientist in this discipline, it is obvious to me that we have made an enormous amount of scientific progress since the founding of the Society for Psychical Research in 1882, particularly given the persistent lack of funding, institutional support, and personnel . . . I agree with Henry Sidgwick, and with Dean Radin who quoted Sidgwick a few years back in his Presidential Address: The time when we needed to debate whether or not the phenomena we study exist is long past. There is an anomaly here. The shape of the natural world that is embodied by that anomaly is becoming clearer and clearer with every methodological refinement, every theoretical advance. The day is coming when the social, psychological, and political surround will not be able to distort the process of observation or the resulting interpretation (Zingrone, 2002, p. 18).

In terms of the substance of our field, I have seen a number of exciting discoveries in these 50 years. Among them are the remote viewing procedure, which seems to give some of the best psi yields in the field, as well as the ganzfeld procedure. Both of these approaches also show that we have learned a lot about handling free-response data in an objective fashion, insofar as evaluating whether psi is present . . . Further, I have been impressed by the geomagnetic and sidereal-time correlates of ESP findings, by various studies using physiological responses to detect psi, and by psychic healing research, as well as many other findings. In spite of the progress in learning more about psi, however, our field is not accepted (Tart, 2002).

Yet this enthusiastic attitude may be owing to the difficulty of admitting lack of progress in one's field. On the other hand, parapsychologists are the best informed; many skeptics do not bother to examine the details of parapsychological studies. Sometimes they note a lack of practical applications, but this is not the same as a lack of new knowledge.

I completed the analysis of written communication with an attempt to evaluate

the peer-review process. I concluded that fringe journals practice peer review in the same general way as mainstream journals. Experience of the 45th convention of the PA was, again, no different from what is experienced at mainstream meetings; researchers questioned and criticized each other's work, albeit perhaps not to the same extent as at mainstream conferences. A less competitive and more friendly atmosphere could be partly explained by the unusually large range of subjects dealt with compared to the smallness of the community (the ninety-five attending people included psychologists, philosophers, historians, neuroscientists, and physicists). Few researchers would be competent enough to argue in all these areas. On the other hand, this interdisciplinary atmosphere was intellectually very stimulating.

To conclude, the contemptuous attitude of French scholars regarding research into the paranormal does not appear to be justified. This research fulfills most of the scientific methodological criteria that characterize "real" science. Communication among researchers in parapsychology reflects the essence of a scientific attitude: they constantly question their work, confront theories and facts, and seek critical comments from their peers. As Collins entitled one of his articles (1979): "The construction of the paranormal: Nothing unscientific is happening here."

Their particular status with respect to the mainstream scientific community nevertheless accounts for a good number of significant quantitative differences from the orthodox communication process. These are not only negative ones, far from it. Mainstream science could learn from the diversity of their interdisciplinary approach (whereas most sciences tend to over-specialization) and the richness of their epistemological reflection (completely ignored by most scientists). Mainstream scientists could also learn from the generally extreme rigor of their experimental approach which aims to address any kind of possible criticisms and which is necessary to separate a very elusive phenomenon from the background noise. They could learn from their concern to publish unsuccessful experiments, whereas mainstream scientists often neglect to report negative data although it can be very useful. Finally, they could learn from their tolerance and open-mindedness, which are usually not prejudiced by authority or personal credentials or by individual ambition (if it were, they would be doing something else).

Yet the quantitative differences found also have negative aspects. Researchers of the paranormal have a tendency to quote their own work, they rely overly on books of general opinion and popular magazines, they publish fewer experiments and tend to use qualitative rather than quantitative data. These significant differences reflect some of the difficulties parapsychologists encounter: the smallness of the community, the specific character of their subject, the lack of resources, their diversity and lack of common focus. Even though they consider that they produce results (see the quotations from Zingrone and Tart), these results are not solid enough to be built upon. Coming back to the introduction, they lack the three tools that make a science successful: reproducibility, falsifiability and predictability. Stengers (n.d.), a French philosopher of science, explains their

status with a parallel taken from mathematics. There are theorems of existence and theorems of construction. Parapsychology is a science of existence, whereas a successful science is a science of construction. Indeed, parapsychologists attempt to prove the existence of an anomaly. Unless this anomaly is at last completely specified, nothing can be built upon it and no consensus will be reached within the scientific community.

Mainstream scientists use many epistemological criteria to dismiss parapsychology as a pseudo-science. It turns out that these are only rhetorical tools, which they would find difficult to apply to their own work. The main criterion is more straightforward: they will accept it if they can use it.

It may be useful to warn against such an attempt, however, because it may slow down the acquisition of fundamental knowledge. If on the one hand a science needs large resources to develop itself, and if resources are given only to a successful science which is developed enough to provide useful material (the others being conveniently regarded as pseudo-science) on the other, there is not much space available for original and innovative ideas to bloom. And one has to be very pretentious or very clairvoyant to assess with certainty that no promising applications should ever be expected from investigation of the paranormal: after all, psychic healing, remote viewing, psychic location of archaeological sites, and psychic help in criminal investigations have been claimed effective in some cases.

In fact, failing to provide useful material may not be the only reason why parapsychology is regarded as a pseudo-science. It does not account for the strong taboo that exists in the French academic community. This taboo seems rather based on a prior conviction that its basis is nothing but popular belief and superstition, whereas science inherently rejects those. Scientists have to overcome a very strong prejudice to consider the paranormal as a possible research subject. As pointed out by Francis Bacon (1625): "There is a superstition in avoiding superstition." Their attitude is thus close to the attitude of the pseudo-scientists they despise: it is based on beliefs rather than on real investigation. According to Rémy Chauvin, this conservative and dogmatic attitude appears to be particularly strong in France. It is not the first time that French scholars have been reluctant to accept new scientific concepts: they were still deriding continental-drift theory when the rest of the world had already accepted it (Chauvin, 2002).

Thus, Bertrand Méheust and other French sociologists or philosophers, such as Pierre Lagrange and Isabelle Stengers, appear to be right on that point. The disregard of the "Métapsychique" issue is political. It is not justified by genuine scientific criteria. On the contrary: "Science must begin with myths, and with the criticism of myths" (Popper, 1957).

Acknowledgments

I wish to thank all the people that helped me with their ideas and comments during the discussions I had with them: Franck Martial, Yves Lignon, Helena

Sheehan, John Palmer, Mario Varvoglis, Paul-Louis Rabeyron, Isabelle Stengers, Bertrand Méheust, Marie-Monique Robin, Gilles Missonier, Jerome Daltrozzo, Stefan Schmidt. I also wish to thank Julian Gillespie and Edward Bach for helping me with the English.

Notes

[1] Based on Marie-Catherine Mousseau, ''Science, research into the paranormal, and irrational belief: what is the link?'' Master's Thesis in Science Communication, Dublin City University, 2002.

[2] Details of the methodology—sampling, statistical tests, etc.—are in references and can be supplied by the author on request.

[3] The editorial policy of JSE is to select articles that can be understood by someone with a general scientific background, unless technical reasons preclude it (Bauer, 2002): ''Because the Journal is intended to be *read* by its subscribers, who on the whole do not expect to find in it material that is so arcane, or so confusingly presented, that only a few individuals (at most ...) could make head or tail of it.'' I believe this is a common-sense attitude for any editor, whether of a fringe journal or not.

[4] Mario Varvoglis, President of the PA, personal communication, 2002.

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