

The Elusive Agenda: Dissuading as Debunking in Ray Hyman's *The Elusive Quarry*

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ABSTRACT: Ray Hyman has been a prominent rhetorical critic of parapsychology for more than a decade. He is a member of the Executive Council of the Committee for the Scientific Investigation of Claims of the Paranormal and has served as a consultant to various government bodies. His book, *The Elusive Quarry*, reprints nearly all of his major papers on the paranormal. Hyman plays two roles as critic. One role is that of scientific, technical critic; the other is that of prosecutor trying to deny the scientific legitimacy of parapsychology. In his role as technical critic, Hyman has provided useful insights, but he also has made serious technical errors. As a rhetorical critic, Hyman occasionally acknowledges that a number of research programs have produced results that have not been explained within current scientific frameworks. He grants the existence of anomalies, but much of his writing is spent advocating that other scientists need not consider these anomalies. Hyman's approach and strategies are discussed herein.

Ray Hyman can be considered *the* preeminent, outside critic of parapsychology for more than a decade; as such, his recent book (Hyman, 1989), *The Elusive Quarry: A Scientific Appraisal of Psychical Research*, requires our detailed attention. In this article I will provide some background information on the author, briefly outline the book, and then discuss the roles that Hyman plays as critic. The discussion will be illustrated with writings from the book and other sources.

Hyman is a professor of psychology at the University of Oregon. He serves on the Executive Council of CSICOP (Committee for the Scientific Investigation of Claims of the Paranormal) and chairs its subcommittee on parapsychology. Hyman has been a professional magician, has published in conjuring magazines, and is well known in that field as well. In fact, his picture appeared on the cover of the October 1986 issue of the *Linking Ring*, probably the magic magazine with the largest circulation in the world. He also served as chair of the parapsychology subcommittee of the National Research Council (NRC) study for the U.S. Army on enhancing human performance (Druckman & Swets, 1988; for a response, see Palmer, Honorton, & Utts, 1989).

It is clear that Hyman has had an enduring interest in psychic research. His first published critique on parapsychology was a review of Soal and Bateman's *Modern Experiments in Telepathy* that appeared in 1957 (reprinted in *The Elusive Quarry*). That review displays a sophisticated grasp of the technical and philosophical issues of the field. Hyman must have been following parapsychology closely. During this early period, he also collaborated with anthropologist Evon Vogt in research on dowsing. With the founding of CSICOP in the mid-1970s, Hyman publicly reemerged as a critic. Although he appears to have published little in the interim, his

interest must have been known because he was called on by the Department of Defense to help evaluate the Stanford Research Institute psi experiments in the early 1970s. In recent years, a major portion of his professional writings has been devoted to criticizing parapsychology.

The Elusive Quarry is a compilation of nearly all Hyman's major published articles on parapsychology. It is arranged in four parts, each with a brief introduction. The first section is the most technical, and it includes papers reprinted from the *Journal of Parapsychology*, *Proceedings of the IEEE*, and *Experientia*. His writings on the Ganzfeld controversy are included, as are some writings on eminent historical scientists involved in psychic research. Several book reviews are reprinted. There is considerable overlap among the articles.

The second part focuses more heavily on eminent historical scientists' involvement in psychical research, specifically, Hare, Crookes, Wallace, and Zöllner. Hyman suggests that these scientists did not investigate the phenomena effectively. This section is tiresomely repetitious.

The third part is titled "Psychic Phenomena" and covers dowsing,¹ occult healing, remote viewing, and a few other topics. The fourth section is the shortest and discusses psychology of belief, cold reading, and includes an article entitled "Proper Criticism," which was composed for local skeptics' groups.

In order to fully appreciate Hyman as a critic, one must recognize two separate aspects. One role is that of the scientific, technical critic (i.e., on methodology and statistics); the other is that of a prosecutor arguing the case against the scientific legitimacy of parapsychology. The roles are so subtly blended that even the careful reader may miss the distinction. I will address each aspect in a separate section in order to clarify the difference.

HYMAN AS TECHNICAL CRITIC

At this point a few words might be said about the distinction between "outside" and "inside" critics. These can be distinguished by the periodicals in which they primarily present their arguments. The inside critics publish their work in the refereed parapsychology journals and in books such as the *Advances in Parapsychological Research* series. Insiders would include Charles Akers, Irvin Child, J. E. Kennedy, Betty Markwick, John Palmer, Rex Stanford, and Douglas Stokes, among others. The outside critics typically publish in more popular, unrefereed forums. Outsiders include James Alcock, Martin Gardner, Edward Girden, C. E. M. Hansel, Ray Hyman, David Marks, and James Randi. At one time, Christopher Scott might have been designated an insider, but in recent years he could be classified as an outsider.

Of those persons who are identified as outside critics of the field,

¹ As I have pointed out elsewhere (Hansen, 1982), Vogt and Hyman ignored the experimental work supporting dowsing, even though Hyman claims that they covered "just about every aspect of the subject" (Hyman, 1989, p. 321).

Hyman is easily one of the most scientifically competent. He is well versed in parapsychology's current literature as well as its history. In his overview of critics of parapsychology, Child (1987) wrote: "Hyman has much more to contribute than fellow critics who share his position of extreme doubt about the possibility of psi phenomena but have apparently not read the research" (p. 206). The only other outside critic in Hyman's class might be Christopher Scott;² no other CSICOP members come close to the technical competence of these two. Of course, a number of researchers inside the field are superior to Hyman and Scott and have made more incisive criticisms.

Hyman's major technical contributions involve free-response ESP methodology and statistical tests. His critique of the Ganzfeld work is probably the best known (it is reprinted in *The Elusive Quarry* as the first major chapter). In his review, he pointed out statistical errors, raised security issues, and discussed randomization. Honorton (1985) produced a detailed rebuttal, and the exchange generated commentaries, which were printed in the December 1986 issue of the *Journal of Parapsychology*. Honorton and Hyman collaborated on a joint communiqué (also reprinted in *The Elusive Quarry*), which listed reporting and procedural guidelines for future Ganzfeld research.

In 1977 Hyman authored two articles for *The Humanist* (reprinted in the book) in which he described subtle problems of statistical independence in remote-viewing experiments (e.g., with closed-deck judging procedures, subjects might systematically avoid responding with characteristics of earlier targets when feedback has been given in earlier trials). A number of his subsequent articles elaborated on this topic and are the clearest explanations of the issues available. Even though Hyman identified the problems over 10 years ago, I have encountered a number of major researchers in the field who, even recently, have failed to grasp the thrust of his points. His 1977 articles also pointed out the problems of not using duplicate target sets for sending and judging in Ganzfeld work (now a typical procedure because of his insight).

Some of the flaws detected by Hyman have only trivial consequences. Honorton convincingly demonstrated that multiple analysis could not account for the Ganzfeld result (which Hyman now admits). Christopher Scott (1986) declared Honorton the winner of the debate. Even fellow critic David Marks (1988) seems to have objected to one of Hyman's "flaws" in remote-viewing research.³

Hyman's comments on random number generator (RNG) research are

² Although Scott's comments are usually to the mark on technical issues, it perhaps should be explained to readers that Scott has candidly admitted how embittered he has become regarding the field (see Blackmore, 1989, p. 260). As a result, some of his writings contain highly emotional polemic.

³ In *The Elusive Quarry* (pp. 149–150) credit to Marks is omitted, giving the impression that the statement was Hyman's.

not as extensive. He has suggested that long control runs might obscure local nonrandomness in the output of a generator. This has not been shown to be a problem in psi research, and in any event the Kolmogorov-Smirnov test has been used to check generator operation (e.g., Psychophysical Research Laboratories, 1985). This test is specifically geared to check both local and global nonrandomness. Hyman also reprints his criticisms of C. E. M. Hansel's attack on Helmut Schmidt's work. He found Hansel's complaints off the mark.⁴

Some of Hyman's pointed criticisms have not had the swift impact on research procedure that they warranted. Hyman himself is partly to blame for this. Although his technical criticisms are usually direct and appropriate, he has chosen to publish many of them in popular, unrefereed periodicals (even two in a quasi-religious magazine). These periodicals have frequently carried exceptionally poor-quality critical articles with glaring technical errors intermixed with emotional diatribe. As a result, many scientific researchers do not find it worthwhile wading through all the drivel to find the useful nuggets (though Hyman's own articles contain little overt emotional polemic).

Despite his contributions to the understanding of methodological issues, Hyman's work is not flawless. He has made a number of mistakes on technical matters, some quite serious. This is ironic because he has incessantly complained about the technical errors of others, and he has billed himself as having a special interest in "human error, especially 'mistakes' made by highly competent individuals."⁵

Examples can be found in the several versions of Hyman's Ganzfeld criticism. Honorton (1985) has pointed out that Hyman's initial critique, presented at the 1982 Parapsychological Association (PA) convention, contained numerous errors in ascribing flaws to studies. Hyman made changes in it in November of that year, but errors still remained. In his first presentation, Hyman claimed an almost perfect correlation between degree of success and number of flaws in a study. He has been forced to dramatically retreat from that claim; in fact, he now admits that he cannot "support any firm conclusion about the relationship between flaws and study outcome" (Hyman, 1989, p. 65). In the published version of his critique, which appeared in 1985, he reported a chi-square statistic calculated in order to find evidence for biased reporting. However, as pointed out by Honorton (1985, p. 63), not only was his calculation incorrect, but he seriously violated the underlying required assumptions of the test. Statistician Jessica Utts (1986) has noted other serious errors with his interpretation of that statistic. Hyman's critique was reprinted in *The Elusive Quarry*, but he neglected to correct his errors and did not inform his readers of the mistakes.

⁴ It is amusing to note that in his recent book, Hansel (1989) criticizes Ganzfeld research but does not mention Hyman. Hyman is not even listed in the index!

⁵ In a biographical sketch in *Proceedings of the IEEE*, 1986, 74, 886.

Also in his Ganzfeld critique, Hyman conducted a factor analysis in order to study the effect of flaws. Saunders (1985) discovered important errors in Hyman's analysis and demonstrated that Hyman's findings were meaningless. In the introduction to the first section of the book, Hyman complained about Saunders' paper, but he gave no specific points of rebuttal. I wrote to Hyman requesting details; he did not reply. In any event, Harvard psychologist Robert Rosenthal performed several similar analyses that failed to support Hyman's conclusions (Harris & Rosenthal, 1988, see postscript of document).

One of Hyman's reprinted papers was a response to Scott Rogo in a debate published in *The Humanist*. In discussing the early statistical controversies in the card guessing experiments, he wrote: "I don't know . . . what the Institute of Mathematical Statistics is" (Hyman, 1989, p. 166). This should be an embarrassing statement for someone who publicly described himself as "primarily a statistician" at the 1982 PA convention. The Institute of Mathematical Statistics has been in existence since 1935, has approximately 4,000 members, and publishes several scholarly statistical journals.

I don't cite all these mistakes in order to disparage Hyman's competence as a critic, for he is truly quite capable. I only mention them to illustrate that *all* scientific work (even criticism) has the potential for flaws. He has made such statements as "the parapsychological community must be concerned to discover that their best experiments still fall far short of the methodological adequacy they themselves would profess" (Hyman, 1989, p. 157). A similar statement might be made with regard to the critics.

Hyman apparently has reported only one foray into empirical psi research,⁶ and in that, his involvement seems to have been minimal. The work was almost totally conducted by James McClenon (the paper is included in *The Elusive Quarry*). This gives Hyman a distinct advantage in the rhetorical arena. Sociologists Pinch and Collins (1984), in their paper specifically addressing CSICOP, pointed out that CSICOP's tactics

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. (p. 539)

In fact, they specifically suggested that the critics not engage in empirical research if they were to be effective in promoting their agenda. They pointed out that various research findings and interpretations in controversial science conflict, and a large component of establishing scientific

⁶ That remote-viewing study "fall[s] short of scientific acceptability" under Hyman's (1989, p. 381) own criteria because it was not published in a refereed journal.

knowledge involves human negotiation and not just “consulting the facts.”

HYMAN AS PROSECUTOR

The idealistic view of the scientist is that of one who coolly examines the facts and theories and then dispassionately judges the evidence. When new evidence refutes one's earlier position, the scientist readily admits the mistake. This idealistic view has been promoted in the popular media by CSICOP members. In fact, the back cover of the *Skeptical Inquirer* proclaims that CSICOP encourages “research by objective and impartial inquiry” and “does not reject claims on a priori grounds, antecedent to inquiry.” By effectively selling this claim to the public, they enhance their own credibility as scientists. This grants power to further advance their own agenda. When one is able to project the image of “dispassionate scientist” while in reality playing the role of prosecutor, the effectiveness of the prosecution is much enhanced.

The prosecutorial aspect can best be seen by comparing positions presented in scientific arenas contrasted with those in popular forums. In the scientific literature, it is typical practice to allow those criticized to respond. The readership is then in a position to decide just who made the stronger case. In books and popular media, those attacked have little or no chance for rebuttal. The readers see only one side of a case. An enlightening example appears in *Science and the Paranormal*, an anthology edited by Abell and Singer. Hyman (1981) contributed a chapter entitled “Scientists and Psychics” (an earlier, previously unpublished version is printed in *The Elusive Quarry*). In that chapter, Hyman clearly and dispassionately describes problems scientists might encounter when testing psychics. His presentation is moderate and would appear to many readers as eminently fair and balanced. In the very last paragraph of the chapter, Hyman states:

I have no quarrel with any scientist who wants to investigate the claims of an alleged psychic. Indeed, the willingness of such men to risk their reputation and to face ridicule is probably a good thing for the growth of science in the long run. What seems to be lacking is a recognition on the part of such scientists of what it will take to put such an investigation onto a scientific footing. Standardized procedures, instrumentation, variables, controls, concepts, data analyses, and other necessities of scientific inquiry will have to be developed, tested, debugged, and validated from scratch. This will not be easy and probably cannot be done by one or two men working alone. (Hyman, 1981, pp. 140–141)

A footnote at the beginning of that essay reported that the writing was supported by the National Science Foundation. Thus, to the naive reader, the above passage would have high credibility. The reader unfamiliar with parapsychology would not realize that it was completely untrue. No men-

tion was made that ongoing, controlled, experimental research had been underway since the 1930s. The article cited 43 references; none were to the *Journal of the American Society for Psychical Research* or the *Journal of Parapsychology*. There was only one citation to the *Journal of the Society for Psychical Research*, and that was a letter. This is a most effective strategy for dissuading the interested reader from pursuing information on long-term, scientific research on psychic phenomena. The clear implication is that nothing like that exists.

One of the credibility-building themes that recurs in a number of Hyman's major articles is that psychic claims have been unfairly attacked in the past. In fact, a whole dialogue on this issue appeared in *Zetetic Scholar (ZS)* (reprinted in *The Elusive Quarry*). The theme can be seen in at least four of the major articles in this book. Such a ploy would give the reader a sense of confidence that Hyman's criticisms, at least, will be fair. It takes a sophisticated reader to see through the stratagem. In the *ZS* dialogue, historian of science Seymour Mauskopf noted:

Hyman calls for a “more appropriate and rational” response from the scientific community to deviant or “pathological” scientific claims than the usual crude, ill-supported *ad hominem* accusations and innuendos. But he does so not in the interest of really open-minded discussion of unsettling assertions but rather the more effectively to lay them to rest: to disarm the recalcitrant deviant, to show him the error of his folly, and to admonish the naive who might be similarly tempted to go astray. Whatever else “more appropriate and rational” might mean, the phrase, as used in this context, clearly means “prejudged” response. (Mauskopf, 1980, p. 58)

Both Stephen Braude (1980) and Mauskopf (1980) describe aspects from Hyman's article that illustrate his preformed opinion.

In some instances, Hyman has acknowledged that several long-term research programs have produced results for which no normal, reasonable explanation has been given. This was admitted for modern day research as well as for studies conducted over 100 years ago. Hyman has stated: “It is true that no one who has studied the reports of seances by Home or Crookes's accounts of his tests on this medium has come up with plausible ways he could have cheated” (Hyman, 1989, p. 286). Hyman thus admits that the Home mediumship was a true enigma for which no satisfactory scientific explanation has been given.

Speaking of the best experiments of modern research (primarily Ganzfeld and RNG work), Hyman has acknowledged that the critics have not “demonstrated a plausible alternative” (1989, p. 157). He admits that neither he nor other critics have provided a conventional explanation for the results. Yet he insists that other scientists need not pay attention. Indeed, he has admitted that he attempts to “justify withholding any attention to the claims for the paranormal on the part of orthodox science” (Hyman, 1989, p. 206). This is a direct, candid admission of his agenda.

In order to rationalize his position, Hyman frequently goes on at great

length to say that the psychic researchers need fully repeatable experiments, lawful relationships, etc. before they should receive the attention of science. Until that time, he claims, there is no explanation needed. In one long epistle on the topic, he asserts that there must be a "minimal set of criteria for deciding if an anomalous result justifies further consideration and attempts at explanation" (Hyman, 1981, p. 137). He defines the minimal criteria as follows:

In addition to a community of shared concerns and paradigms with respect to a given problem, the observations must be made with standardized and proven procedures, the observers and their instruments must be reliable, the data must be reported according to conventional categories and attributes, and the settings and tasks must be ones in widespread use or ones that have gone through preliminary checks and standardization. In addition, especially if the reported results are anomalous or at variance with current theories and presuppositions, they must be systematically studied under a wide variety of conditions, and they should be repeatable by investigators in independent laboratories. (Hyman, 1981, p. 136)

Child (1987) commented:

This is a very fine statement of what might be theoretically desirable. In practice, it seems to offer a recipe for guaranteeing that anomalies will never be studied. For it prescribes that no one in the scientific world should pursue the study of an anomaly until a large number of scientists have already pursued it at great expense. The preliminary work required by this statement of principles might well require many times the budget of all the existing parapsychology laboratories and many times the number of trained scientists ever to have worked on the problem of psi. But none of these scientists should start working on the problem until after the large-scale preliminary work has been completed. This seems to be a Catch-22 statement of principles. (p. 223)

Many reported findings on biological influences of electromagnetic radiation would not meet Hyman's criteria as deserving further study! Numerous other instances of leading-edge science could be cited as well. Nevertheless, the strategy of long-winded rationalizations is effective. It obscures the point that psi effects have been consistently found in long-term research programs but have not yet been explained.

Hyman's perceived position as a "responsible critic" of parapsychology has placed him in a position of some influence. He was appointed to the National Research Council committee on enhancing human performance for the U.S. Army. He served as chair of the parapsychology subcommittee, which concluded that there was "no scientific justification from research conducted over a period of 130 years for the existence of parapsychological phenomena" (Druckman & Swets, 1988, p. 22). This NRC report has been widely read by people in a position to fund psi research. Rather surprisingly, not long before his appointment, Hyman co-signed a fund-raising letter for CSICOP (March 23, 1985) that stated:

"Belief in paranormal phenomena is still growing, and the dangers to our society are real . . . in these days of government budget-cutting the Defense Department may be spending millions of tax dollars on developing 'psychic arms' . . . Please help us in this battle against the irrational. Your contribution, in any amount, will help us grow and be better able to combat the flood of belief in the paranormal." This strikingly illustrates his prejudice. In the section on parapsychology of the NRC report, there is no mention whatever of the conclusions of the NRC-commissioned work by Robert Rosenthal; that work was not even cited. Rosenthal's findings diametrically contradicted the opinion of Hyman's subcommittee; this was a clear cover-up. Even after all of this, in his 1988 *Experientia* article, Hyman claims to give parapsychology a "fair and unbiased appraisal" (in Hyman, 1989, p. 141)! Writing of some of Hyman's earlier work, philosopher Stephen Braude presciently and pungently stated: "Hyman professes one set of attitudes and beliefs, and betrays another. One's dagger may be brandished openly or concealed under one's cloak. Real malevolence may be served either way" (1980, p. 43).

HYMAN AS PSYCHOLOGIST

In his introduction to the section "Psychic Phenomena," Hyman calls for an attempt to understand the psychology of "believers." He specifically implies that James McClenon's view that believers and skeptics look at psi from different paradigms is not altogether rational. In later chapters he goes on to condescendingly describe the errors of the ways of the "believers." He provides no similar analysis to enlighten us on the psychology of skeptics. One might speculate that, in his own case, there may be a degree of cognitive dissonance because he admits that there has been no satisfactory explanation of the best psi research results. Or maybe he simply views himself as serving a constituency, an attorney arguing the best case for the prosecution. Or perhaps he responds to social pressure within CSICOP. For a time he may have been a rather low-status character within CSICOP because he was "too easy" on the "believers." Hyman has chosen to speculate on the psychology of his opponents, although he should be in a better position to provide insight into the psychology of skepticism.

CONCLUSIONS

In summary, if one has carefully followed Hyman's attack on scientific parapsychology in the last 15 years, one will find little that is new in *The Elusive Quarry*. As such the book will be of minimal value. It is simply another of Prometheus Books' slap-dash productions of skeptical books. They didn't even care enough to include an index. On the other hand, if one is new to the psi controversy, this is probably the second most impor-

tant skeptic's book of recent years (the first being Kurtz's [1985] edited volume, *A Skeptic's Handbook of Parapsychology*). Hyman's work more consistently is of a better intellectual quality than that of most of Kurtz's contributors. Also, his articles on free-response ESP methodology are required reading for anyone contemplating work in that area.

Over a period of decades, Hyman has expended considerable effort in trying to explain away the results of parapsychology. He admits that this endeavor has been unsuccessful. His specifically stated tactic now is to dissuade the scientific community from giving serious attention to the field. The dust jacket of the book acknowledges that one of Hyman's major themes is that "the best way to proceed in the hunt for the 'elusive quarry' of psi is to improve the communication between parapsychologists and their critics." This appears to be yet another credibility-building tactic of the debunker. Research scientists have little to gain in trying to communicate with polemicists who engage in no research themselves and who have already decided the issue. As pointed out by Richard Feynman in his (unpublished) banquet address at the 1984 PA convention, the field has good internal critics and does not need outsiders to do that job.

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