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THE SKEPTICAL POSITION: IS IT TENABLE?

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Abstract

A distinction is made between the de facto skeptic who demands stronger evidence for paranormal claims and the absolute skeptic who dismisses all such claims as inherently preposterous and incredible. It is with the former that we are here mainly concerned. It transpires that the sticking-point for the de facto skeptic is an insistance on repeatability-on-demand, a position which enables the skeptic to ignore the entire corpus of historical evidence to which parapsychology has given rise. Whether parapsychology will ever be in a position to meet such a criterion remains doubtful. In the meanwhile the skeptical position, so defined, will remain tenable. Whether it is reasonable, still less compelling, is, however, another matter.

By the `Skeptical Position' I shall here mean the view that there is no evidence, as yet, that would justify acknowledging any phenomenon as `paranormal'. On this view parapsychology is, at best, a potential science, concerned as it is with claims that still await authentication. Furthermore, given the fact that attempts to clinch such claims have now been going on for at least a century, the prospect that any phenomenon will in due course be universally recognized as paranormal must be considered increasingly unlikely. The `skeptical position', so defined, is today widely held among the scientific community with, inevitably, unfortunate practical consequences for parapsychology, especially when it comes to funding. It is the aim of this paper to consider whether or not this position is, philosophically, defensible.

The skeptical position, as I have stated it, represents what I shall call `de facto' skepticism as distinct from what I shall call `absolute skepticism'. A `de facto skeptic' is an empiricist who is willing, in principle, to abide by the evidence but is not satisfied that the evidence in this case is coercive. Paul Kurtz voices the attitude of the de facto skeptic when he writes: "Philosophers have analyzed the coherence between physical theories and alleged extrasensory functions in order to determine whether the latter are consistent with physical laws. I submit that one should be dubious about purely a priori formal methods of evaluation, because if the phenomenon is found to be genuine it is the antecedent conceptual system that will have to be modified. The data must not be sacrificed at the altar of preconceived notions of logical coherence." [my emphasis] (Kurtz 1992 p.145). His subsequent remarks, however, reveal that his open-mindedness is actually confined to `anomalies'. Parapsychological findings may, Kurtz concedes, in due course be taken at face value but always with the tacit understanding that they can eventually be reconciled with the physicalist world view. Hence he specifically rejects the term `paranormal' if this is taken to imply any kind of "spiritual, mental or idealistic dimensions"(p.149). Kurtz, we might say, is a `de facto' skeptic about the paranormal in the weak sense of anomaly but an `absolute' skeptic about the paranormal in the strong sense in which it has inspired so many parapsychologists. His position, I may add, is by no means idiosyncratic; it is, on the contrary, widely shared by scientists and philosophers.

Absolute Skepticism

For an `absolute' or `a priori' skeptic, evidence is simply irrelevant. It would make no difference what evidence the parapsychologist might adduce; an absolute skeptic could always nullify it by invoking some general principle such as consistency with known physical laws, as in Kurtz' example, or some other cherished criterion. There are not many among our critics, I may say, who profess absolute skepticism in this sense yet, in practice, it provides a fall-back position when de facto skepticism begins to look shaky. Although absolute skepticism could be described as a form of dogmatism, we must not dismiss it as irrational. All of us, after all, are absolute skeptics about certain claims which overstrain our credulity. Speaking for myself, I would have to confess to being an absolute skeptic when it comes to claims that extraterrestrial aliens have visited the Earth. No matter how many eyewitness reports might be pushed under my nose, no matter how many abduction cases may come to light (and they are numerous) I would still decline to credit them. For it would always strike me as more likely that the reports were misleading than that the claims were valid. Of course, even absolute skepticism is never absolute in the strict sense, inasmuch as we could always envisage some hypothetical example that would force us to change our mind. No doubt if I myself were to have a close encounter with an extra-terrestrial alien even I would have to admit that I was wrong. But so long as I refuse to envisage such a contingency as anything more than an academic hypothesis, I could still count as an absolute skeptic with respect to claims of this sort. Equally, even the most dismissive of skeptics would presumably change their tune in the face of some overwhelming manifestation of the paranormal but, that aside, they would still qualify as absolute skeptics with respect to the current parapsychological evidence.

The pioneer of absolute skepticism with respect to what he called `miracles' was, of course, David Hume (Hume 1777). His argument was simple. A miracle, by definition, contravenes our past experiences. At the same time we all know to our cost that liars and deceivers abound. Hence, even if miracles did occur - and, as a strict empiricist, Hume could not deny that they might it would always be more rational to doubt the testimony of the witnesses than to accept the miracle as a fact. We must note, however, that there is a subtle difference between a `miracle', as Hume understood the term, and what we would now call a `paranormal event'. A miracle merely contradicts certain universal expectations as to what can or cannot happen. A paranormal event, on the other hand, must be anomalous with respect to the entire conceptual framework of science as we know it. Hence, to call something paranormal is, as Kurtz rightly insists, to call into question the universality of the prevailing scientific world view. Hume conceded that, if a miracle could be regularly repeated, we would be forced to believe in it but then it would ipso facto cease to be a miracle! A paranormal phenomenon, on the other hand, would still be paranormal even after the umpteenth repetition so long as no scientific explanation was forthcoming. Repetition is, of course, a key issue for the skeptical position and we shall be returning to it later. Antony Flew (1978) endorses Hume with respect to all past claims of a paranormal kind and so could be classed as an absolute skeptic with respect to the historical record but even he concedes that, if parapsychologists were able to satisfy repeatability on demand, we would then, indeed, have to revise our assumptions.

Some critics, who may be de facto skeptics with respect to parapsychology as a whole, may be absolute skeptics with respect to certain classes of psi phenomena. For example, some who are open minded about ESP may take exception to pure clairvoyance, others who are prepared to accept micro-PK may draw the line at macro-PK. Flew has, throughout his long career as a skeptic, consist ently rejected precognition as nonsensical (cf.Flew 1987). He does so on the grounds that it implies that a cause may occur *after* its effect. And he is perfectly correct, of course, in saying that precognition implies backward causation. Where he goes astray is in thinking that backward causation is somehow *logically* inadmissible - however offensive it may be to our commonsense (see Brier 1974; Beloff 1990, pp.78-87). If one were to follow Flew one would have to dismiss all traditional beliefs in prophecy and foreknowledge as not just false but literally meaningless - which seems odd to say the least!

Even odder is the fact that, faced with one such instance - let us suppose Flew were to dream consistently the winner of tomorrow's horse-race - he could not call his dreams `precognitive', he could at most express surprise at his continuing run of luck! Coincidence has, of course, always been a *logical* option where any psi phenomenon is concerned but the science of statistics has enabled us to put a value on the degree of coincidence we need to posit once we reject a causal explanation. An absolute skeptic has to be prepared to dismiss as coincidence what others would accept as evidence for the paranormal no matter what the odds might be. We may note, in passing, that even those who accept the paranormal in general have raised what they consider logical difficulties over precognition as traditionally understood (see Braude 1986, Chap.5; Griffin 1993 pp.270-275) but they at least could invoke alternative paranormal explanations which Flew could not.

Perhaps the most audacious absolute skeptic of recent times is Nicholas Humphrey, current holder of the Perrott-Warrick Research Fellowship at Darwin College Cambridge. He alone. to my knowledge, has pronounced psi phenomena *as such* to be *logically* impossible. This is indeed startling because, in the ordinary way, nothing whatsoever, no matter how extraordinary or incredible, can be pronounced *logically* impossible unless, of course, it involves a contradiction. For, even God, who was thought to be omnipotent, could not, theologians agreed, do what was logically impossible. Humphrey now protests that he was misreported and concedes that it is not phenomena qua phenomena that can be dismissed as impossible, only certain explanations of those phenomena. However, since what makes a phenomenon paranormal lies precisely in its interpretation, i.e. in the assumption that it is inexplicable on any known physical hypothesis, he is, in effect saying that psi phenomena as such are indeed logically impossible.

What induced Humphrey to stick his neck out to this extent? Until he publishes his definitive treatise all I can go by is some personal correspondence and some miscellaneous pronounce ments. But the gist of it seems to be that, notably in the case of PK but, by extension, to psi in general, a given output appears to be more complex than its input. Thus, in a typical micro-PK experiment, the intention on the part of the subject may be simply to score by shifting a pointer, producing a click or whatever. At the same time, the physical processes required to produce this result are always very complex. Thus one is confronted with the paradox of an input containing minimal information issuing in an informationally rich output. As Humphrey puts it, the problem is "how the supposed psychic powers can have the `targeting' and `indexical' properties they must have if they are to do their job'(Humphrey 1993).

This feature of the psi process, to which Humphrey rightly draws our attention, haslong been familiar to parapsychologists under the rubric of the `goal oriented' aspect of psi. The concept of goal orientation, however, is by no means confined to psi phenomena. When I write something down, for example, I think and intend only the words that I wish to write, better still, only the meaning I am trying to convey, confident that my fingers will automatically execute the relevant movements on the page or the keyboard as the case may be. What makes writing a normal, rather than a paranormal, activity is, of course, that there is a physical connection between my brain and my hand. Now, to an epiphenomenalist, my *intention* to write is of no consequence. Provided that my brain is in the appropriate physical state, the relevant movements of my hand and fingers will duly follow, just as when the computer is in the appropriate state it will trigger the appropriate display on the screen. My conscious intention must be regarded as no more than a passive reflection of what my brain is doing and would still, indeed, be doing were I never to be conscious at all! Hence, the fact that I, the agent, remain entirely ignorant as to what goes on in my brain and nervous system when engaged in voluntary activity is irrelevant. I suspect that what actually disturbs Humphrey is not this goal-oriented aspect of psi phenomena which, as I say, is common to all normal voluntary activity, but rather the fact that, in the psi case, there is, ex hypothesi, no mechanical connection between the subject and the target and so it cannot be subsumed under the epiphenomenalist view of mind. In other words, it is the concept of the paranormal as such, the idea that anything at all could operate outside the universal physical system that

really bothers Humphrey as it bothered Kurtz. To a dualist- interactionist like myself, on the other hand, this analogy between what goes on in normal voluntary behaviour and what goes on in the case of PK is both suggestive and illuminating. In both instances, one could say, physical processes are brought into play *in order to* fulfil a specific wish or intention. If that is `magical thinking' then so be it; such an hypothesis is not logically inconceivable and the evidence from parapsychology demands no less.

So much for absolute skepticism. The attempt to show that psi phenomena involve some kind of logical fallacy has not succeeded and cannot do so. Alternatively, to dismiss such phenomena out of hand as too absurd or fantastic to warrant serious consideration (as I myself unashamedly did with respect to extraterrestrial visitations) though still an option, becomes increasingly threadbare when one examines the best experimental evidence of recent years to which we must now turn.

The Empirical Evidence

Let us now consider the more serious challenge which comes from those I have called the *de facto* skeptics who claim to have an open mind but who demand stronger evidence than anything that has so far been produced. Ray Hyman, James Alcock, Persi Diaconis and the other well known associates of CSICOP exemplify this category. The report of the National Research Council, *Enhancing Human Performance* (Druckman & Swets 1988) is an expression of this outlook while the critique of it by Palmer, Honorton and Utts (1989) shows why it fails to convince parapsychologists.

Perhaps nothing has done more to bring the skeptical position into focus than the advent of meta-analysis. Comprehensive and painstaking meta-analyses have now been carried out on all the major phenomena of experimental parapsychology. Each has yielded a combined z-score reprresenting astronomical odds against chance. In no case, moreover, is there the suggestion that the weaker studies contributed unduly to this overall significance. It was Honorton's own meta-analysis of the ganzfeld studies (Honorton 1985) that sparked off the memorable ganzfeld debate in the pages of the Journal of Parapsychology between Honorton and Hyman culminating in their `joint communique' (Hyman and Honorton 1986) and eliciting contributions from various authorities. A more recent attempt to draw upon the metaanalytic evidence in support of the ganzfeld evidence was the paper by the statistician, Jessica Utts, in Statistical Science (Utts 1991). Yet it was clear from the invited comments on the Utts paper that the skeptics remained skeptics. Since it is now obvious that meta-analysis has not turned the tide we must ask why? What can still be lacking?

The answer, in a phrase, is: `repeatability on demand'. The data-base for the meta-analytic studies shows that by no means every experiment that uses a particular procedure produces a significant outcome. Indeed, in the case of the ganzfeld studies which now may be said to represent state-of-the-art parapsychology, only 12 of the 28 studies considered by Honorton in his 1985 analysis (i.e.43%) (Honorton 1985) were significant at the 5% level of confidence and, although much will depend on the number of trials attempted, it suggests that even for the keen parapsychologists who provided these data, there was less than a one in two chance of a successful outcome. Given the dedication necessary to carry out a decent ganzfeld experiment, where it may take as much as two hours to run just one trial, we can see that such a technique is still a long way from meeting the criterion of routine repeatability. We might suppose that micro-PK would afford a more promising approach than free-response ESP, given the enormous number of trials that can be run in a single session using a random event generator, but any illusions we may have on that score would soon be dispelled by an examination of the data which Robert Jahn and his associates at the PEAR laboratory at Princeton have amassed since 1979 (Jahn & Dunne 1987). There the practice has been to use as subjects anyone willing to put in time at the laboratory. Unfortunately very few such subjects attain even a minimal level of significance at the termination of their sessions.

In asking, as I do in this paper, whether the skeptical position is *tenable*, one is, in effect, asking whether the criterion of `repeatability on demand' is reasonable. If it is, then not only must we forfeit psi but, equally, we must discard any psychological phenomenon that is not robust enough to pass this test. One can never know, of course, what discoveries may yet alter the situation but, if history has anything to teach us, it is, surely, that psi is inherently elusive and evasive. Given the notorious decline effect, given the experimenter effect and the critical importance of situation and atmosphere, any prospect of arriving at a formula for routinely producing psi effects can only seem hopelessly quixotic. On the other hand, there can be no question that, in the exact sciences, in biology, chemistry, physics etc. repeatability on demand by duly qualified experimenters is, rightly, accepted as the ultimate test of any challenging new claim. Thus, when two scientists recently claimed to have produced cold nuclear fusion they created a furore, not just because this would be theoretically so upsetting, but, because others who tried their method failed to confirm it (though, I gather, we may not yet have heard the last of this particular controversy - there is now even a Cold Fusion magazine!).

The answer to our question, then, is that, by the standards of conventional science, the skeptical position is reasonable. Meta-analyses are important as guidelines for the future but they cannot serve as a substitute for repeatability on demand. But, at this point, a further question arises. Is proof, by the conventional scientific method, the sole valid test of truth? We have only to ask this question to realize that this cannot be so. No sane person would deny that there is such a thing as historical truth. Yet, in the nature of the case, every past event is unique and so unavailable for inspection. Yet we have no problem authenticating historical claims. We do so on the basis of documents, artefacts or other relevant evidence. Science, it has been said, deals with what happens; history (and, we may add, jurisprudence) is concerned with what has happened. Experimental parapsychology purports to be a science, inasmuch as it deals with ongoing claims or hypotheses but parapsychology as such - what used to be called `psychical research' - embraces the entire gamut of human encounters with the paranormal. This includes innumerable spontaneous personal experiences, for which there is a rich store of well documented cases going back to the late 19th century, as well as unique investigations of mediums or gifted subjects by experienced investigators. In an otherwise brilliant book on the nature of science, Lewis Wolpert (1992 p.136) dismisses parapsychology with the curt remark: "While there are numerous reports of paranormal phenomena, they are, almost without exception, anecdotal". Wolpert gravely underrates the strength of experimental parapsychology. All the same, we can admit that parapsychology is as much one of the humanities as one of the sciences - as, indeed, is psychology.

Assessing the Historical Evidence

The position of the skeptic with regard to historical and anecdotal evidence fluctuates between ignoring it as irrelevant and debunking it as flawed. The singularity of so much of the past evidence and the fact that it surpasses anything that we can lay our hands on today inevitably fuels suspicion. Furthermore, the fact that cheating of varying degrees has been a recurrent feature of parapsychological history further encourages the belief that, given time and patience, *all* such evidence will eventually yield to the charge of trickery or deceit. Some critics even jump to the conclusion that, if a psychic is once caught cheating, all the evidence from that source is tainted and can be summarily dismissed. But, however beguiling such an inference, it is logically unsound. If an *experimenter* is caught cheating then, indeed, *all* the evidence for which that experimenter was responsible must be treated as suspect - as J.B.Rhine rightly treated the work of his protege, W.J.Levy, after the latter's exposure. In science we have to rely, *pro tem*, on the veracity of the experimenter and so to invent or to falsify data is rightly considered the ultimate sin for a scientist and it is doubly so in parapsychology where replication is such a problem But, where the *subject* is concerned, the evidence can only ever be as good as the test conditions allow. If, when conditions are lax, the subject cheats, as Palladino used to cheat when she could get away with it, that may tell

us something about the *morals* or foibles of the subject in question but it tells us nothing whatsoever about the authenticity or otherwise of evidence obtained when conditions were rigorous. Just as it would be manifest nonsense to authenticate paranormal claims simply because the subject in question had an impeccable reputation, so it is equally ludicrous to reject them because the subject is found to be untrustworthy. Indeed, one could even argue that evidence emanating from a subject known to be a cheat should, if anything, carry more weight with us inasmuch as the experimenters involved could be presumed to have been that much more vigilant!

The only acceptable way of disposing of the historical evidence is to provide a possible normal counter-explanation and some episodes from the past, that had long stood the test of time, have indeed succumbed to such treatment. The late Trevor Hall was, perhaps, the best known exponent in recent times of this line of attack. However, for all his pertinacity and ingenuity, he sometimes failed to see that his counter-explanations ran up against certain fatal objections. Thus, his contention that the reason for Edmund Gurney's suicide was the discovery that his collaborator, G.A.Smith, had been deceiving him in their joint experiments, in which Smith had acted as hypnotist with subjects whom he had himself introduced, (Hall 1964) falls down, not so much because there is every reason to think that Gurney's death was accidental and not a suicide (Coleman 1992) but because it is inconceivable that someone as conscientious as Gurney would not first have alerted Myers and the others at the SPR who were still using Smith!

Trevor Hall's most celebrated feat of debunking, however, is to be found in his earlier work, The Spiritualists, (Hall 1962/ 1984) in which he argues that William Crookes colluded with the medium, Florence Cook, who, in 1873, was purporting to material ize the phantom, `Katie King', as a *quid pro quo* for her sexual favours. He bases this on an alleged confession Florence is reported to have made to a lover of hers many years after the event (the so-called Anderson testimony). Now, whatever one may think of this scenario, it ignores the fact that a second medium, Mary Showers, was invited to participate in some of these sessions. But, if the sessions were just a cover-up for his affair with Florence, the last thing in the world he would have wanted was the involvement of a second medium - with all the possibilities for blackmail which this would open up. More still to the point, having thus implicated Miss Showers, why would he then have accused her of just the sort of impersonation which Hall now attributes to Florence? How, moreover, would he have dared to threaten Miss Showers with exposure? It is she who would have threatened him!

We shall never know the truth of this extraordinary episode and we are not obliged to accept the paranormality of `Katie King' but, if only for the reason given, we can be as sure as we can be about anything in the past that it was *not* a case of collusion. And yet, on the basis of Hall's far-fetched speculations, skeptics such as Antony Flew feel fully entitled, whenever the occasion presents, roundly to declare: Crookes was a crook!

Having disposed of Crookes and of the early work of the SPR associated with Gurney, Hall, in the last book he published before his death, took on perhaps the most formidable challenge that confronts the historical debunker: the case of D.D.Home. Although his book `The Enigma of Daniel Home: Medium or Fraud?' (Hall 1984) purports to solve the mystery by demonstrating that Home was no more than an artful trickster, he nowhere gets to grips with any of the more impressive and intractable evidence. He has much to say, for example, about the Ashley House episode but, as Dingwall points out in his review: "Hall has made no attempt to quote or analyze any of the more striking examples of Home's mediumship such as his experiments with Crookes, the sittings in Holland in 1858, and above all the accounts related by very many sitters as to the lighting conditions which in many cases but not all make many of Dr Hall's speculations untenable" Dingwall (to whom Hall pays fulsome tribute in his preface) concludes his review with the words:"The chief lesson to be learnt from this book is that the enigma of D.D.Home remains an enigma, and there is no sign of it being resolved". (Dingwall 1987).

The hard fact, against which would-be debunkers can only bang their heads, is that, for more than twenty years, Home gave regular sittings, sometimes more than one a week, at which in good illumination (usually gaslight) a large table would be levitated to shoulder height or higher and that in no case was he ever detected, by any of the hundreds of sitters who attended these seances, using sleight-of-hand. Even Robert Browning, who loathed Home and whose satirical poem is still tiresomely cited when the name of Home crops up, confessed that he, for one, had no idea how it was done. Yet, unless all these witnesses, including the hostile ones, were concealing certain facts, or were, as James Alcock suggests "not thinking about what he was actually doing" (personal communication) the question still remains: how *did* he do it? The art of conjuring has made considerable strides since those days. Why has no contemporary conjuror recreated such a sitting for our entertainment and edification? I have indeed watched table levitations in TV studios that I cannot explain but Home, let us not forget, operated in private parlours or hotel sitting rooms often at short notice.

Historical skepticism still has its devotees. Ruth Brandon's *The Spiritualists*, subtitled `The Passion for the Occult in the 19th and 20th Centuries'(Brandon 1983), is a fair example. I myself was once foolhardy enough to challenge skeptics to upset the Feilding Report of 1908 on Palladino (Beloff 1985). Sure enough my challenge was, in due course, accepted and we now have Richard Wiseman's account of how her phenomena *might* have been faked if she had arranged for a removable panel to be fitted to the door of the seance room, at the Hotel Victoria in Naples, through which an accomplice could enter and exit (Wiseman 1992). His ingenious hypothesis failed to convince those who were persuaded, on the strength of the general body of evidence, that Palladino was genuine. Nevertheless, in principle, this is the sort of thing that we need if historical skepticism is to be taken seriously and I salute Wiseman's skill and ingenuity both in rising to my challenge and in facing up to his critics. Even if he did not succeed in nullifying Palladino, he did at least draw attention to some serious omissions in the Feilding Report hitherto regarded as a model of its kind.

Conclusion

We have seen that if what I have called `The Skeptical Postion' is to be tenable it must apply not only to the latest experimental evidence but also to historical cases, some of which may indeed tax the credulity of those who would defend the former. In asking if the skeptical position is tenable, therefore, we must consider *both* sorts of evidence. We have already pointed out that if repeatability on demand is taken as the only safe assurance that we are dealing with a genuine phenomenon, parapsychology not only cannot and may never be in a position to meet that goal and hence skepticism with respect to the ongoing experimental evidence will always be an option. When we turn to the historical evidence, on the other hand, the mere failure of attempts at debunking does not constitute a guarantee that the phenomena in question were genuine; at most they imply a failure on the part of the historian to hit upon a tenable normal explanation. It is worth pondering that the late Eric Dingwall, who knew more, perhaps, than anyone about the history of this field, died an embittered man never able to make up his mind whether he was a believer or a skeptic. For, when all is said and done, one can never prove a negative. The fact that we cannot explain something can never be taken as proof that there is no normal explanation - it could just be that we have not yet hit upon the solution. In the end we each have to decide for ourselves and then try to remain tolerant towards those who think differently. To sum up, to the question: is the skeptical position tenable? the answer is: yes. To the question: is it plausible, the answer is that it depends on one's presuppositions. If you are Robert McConnell (1993) you will insist that not only is it implausible it is downright irresponsible! Finally, to the question: is it more rational to doubt the parapsychological evidence than to credit it, the answer is a resounding NO!

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AN AUTOMATED FREE RESPONSE SELF TEST IN THE SUBJECT'S OWN ENVIRONMENT: FIRST RESULTS WITH SECURITY MEASURES AND A SELECTED SUBJECT

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Abstract

The implementation of an automated free response experiment that allows for unattended self testing is described. The 80 visual targets are stored on disk. One of them is randomly selected as the target while another one is selected as the decoy. Two ASC induction procedures, hypnosis and EEG entrainment, are also implemented and integrated into the free response procedure.

The safety measures are discussed, most notably a remote login to the University network that might result in a practically fraud-resistant protocol. The description of this system and especially the security aspects ae the focus of this paper[1]. Some recommendations for further work along these lines are given.

A first pilot experiment with this set up was conducted with a promising subject. Feedback was provided for 13 trials of the 22 trials. Overall scoring was non-significant (hit rate was 45.5% while MCE = 50%). There were 5 trials with ratings 50% or more of the maximal rating, i.e. where the subject showed some confidence. These trials had a 80% hit rate.

Mean rating difference between target and decoy was +1.27 (t= 0.45, df=21; n.s.). For sessions with feed-back the rating difference was +1.23 versus +1.33 for non-FB sessions. The non-significant mean rating difference following the EEG-entrainment induction was +0.8 (N=5), following hypnosis -18.0 (N=2) while for the trials where the subjects used his own ritual the mean rating difference was +4 (N=15). The subject suggested some improvements that are discussed.

Introduction

Ganzfeld (GF) research has been claimed to be a very successful paradigm in psi research (Bem & Honorton, 1994). The estimated true effect size is considerable larger than the effect sizes typically found in forced choice or micro-PK experiments. It can, however, be doubted if this is specifically due to the ganzfeld induction procedure. Indeed, similar, if not greater, effect sizes have been found in dream studies (Kanthamani, 1992). Whatever the underlying reason, the general rule across paradigms seems to be that the more time is invested in a session or trial the larger the effect size. This tentative rule may imply that simply increasing the session rate will result in decreasing effect size while exhausting the experimenter.

[1] The software is not described in detail, e.g. no screendumps are given because the software is freely available with the first author.