

PARAPSYCHOLOGY: THE GOOD, THE BAD, AND THE UGLY

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DURING THE LAST THREE to four years, I have been a member of what might be called the 'Bob Morris Psi-Show' with this year's major event taking place at the International Congress of Psychology in Stockholm and with some solo performances by its members at the Perrott-Warrick Conference at Cambridge University. The show, which has enjoyed a considerable degree of success, includes an overview of current research in parapsychology presented by Professors Morris and Delanoy along with contributions from Ed May, the former director of the CIA-financed SRI Laboratory, and Jan Dalkvist, parapsychology researcher at Stockholm University, who also was the 'manager' for the Stockholm venues. One of the earlier performances in Stockholm even featured a guest appearance by Richard Wiseman (demonstrating his now standard number on the Indian Rope Trick). The use of the word 'show', while openly frivolous, is not entirely vacuous, since such a presentation of what parapsychology has to offer requires that participants work effectively together to ensure that each presentation complements and builds on the previous one.

In discussing our respective roles in the group, one of the participants made me aware of my role as the rebellious youngster (presumably more to do with my style of not always adhering to the good advice of Professor Morris than my age). I would like to give myself, in what follows, the freedom to develop this role a little further by picking up on what I have learned from taking part in the above performances and my contact with parapsychologists and their critics.

In categorising my experiences as "the good, the bad and the ugly", I am borrowing a phrase from Ed May. He actually restricted its use to describing the antics of critics but here I am choosing to widen its application to the field as a whole. In doing so I am well aware that many of the views being put forward will be hotly contested by some individuals as being opinionated personal attacks and be considered by others as thoughts that are better

left unsaid. Nevertheless I see the strength of these columns as promoting an open discussion of wider issues that would not normally enter the journals. Indeed, I firmly believe that while most of what I have to say is already generally known, it concerns issues that tend to be more or less muted. To preserve the vitality of our subject, some of these issues may need to be brought out in the open, discussed, and hopefully resolved.

Although these views have been developing for some time, the final impulse to express them came in the form of what now seems to be a relatively trivial incident. Christine Simmonds writing in the *Paranormal Review* (July 2000) reported on the presentations of para-psychologists and their critics at the above-mentioned Cambridge Perrott-Warrick Conference. In her diligent efforts to be fair and impartial, she unwittingly perpetuated one of the confusions in thinking that befell some of the critics at that conference.

This concerned the Gothenburg ganzfeld work which, as the reader may recall, suggests that by synchronising audio- and video-recording tapes, the real-time recordings of the receiver's mentation follow quite dramatically the changing sequence of events in the target film-clips as they are being viewed by the sender. Christine Simmonds stated in her review of this work that "it looks impressive but it still might be the one in four hit which one would expect by chance". Since our overall results are highly significant and closer to a one in two than one in four, and most of our illustrative examples of real-time hits were chosen from these series, the one in four dismissal is clearly a mixing up of wrong quantitative statistics with right qualitative data. Qualitative and quantitative findings have different purposes and should not be confused. Instead of providing any absolute evidence that psi occurs, qualitative data can, in the first instance, re-assure us that (despite the high significance of the quantitative data) we are not dealing with a statistical anomaly. However, they do give us potentially much more than

this. Indeed, qualitative data have recently attained a high degree of importance and status in psychology by providing valuable insight into just the kind of mental processes that concern parapsychology.

Using the ganzfeld this way can then be a way of studying the *psi-process* in the laboratory and learning something about it. This of course assumes one is willing to first accept psi as a *working hypothesis*. I had hoped nevertheless that critics would agree with my contention here and see it as a way out of the never-ending debate over whether psi occurs or not: if psi is real it will show real relationships. In presenting the qualitative data, I deliberately choose not only examples of subjects accurately identifying film images but also the before-and-after sequences which appeared to show us something about how psi emerged into consciousness. Some of these suggest that the psi-derived image develops like that of normal perception: it can be vague and ambiguous before more information or memory images enable it to become accurate in identifying the object. For instance one description reads "something at two levels" before identifying these as a train and a racing car that are travelling at two levels parallel to each other.

Unfortunately, the critics at the Perrott-Warrick conference seemed to ignore the many accurate identifications and selectively remember and pick up only on the ambiguous descriptions. I suspect this was because it enabled them to seek refuge in cognitive psychology's newest *modus operandi* (MO) for everything incongruous. This MO is what is now called 'subjective attribution'. Subjective attribution is really not so new but it is essentially a re-naming of confirmation bias or, simply, seeing what you want to believe. Its current over-use has become somewhat reminiscent of Freud's concept of projection as defence mechanism and it suffers from the same weakness. It assumes the exclusive right of the analyst or cognitive psychologist to determine where reality lies and to interpret any deviation from this as projection or in this case, attribution.

Although common sense says to me that this hypothesis is scarcely credible, given that our control recordings do not show this pattern and given the relatively frequency (between one in five and six hits) of these good-quality matches, we are naturally interested in finding

ways of formally quantifying our qualitative data.

To evaluate this kind of real-time correspondence using control recordings is not an easy task since dependencies occur between the units of responses: Having correctly identified a scene in a film clip (say, a city), then a lot of things might be expected to follow, such as, for instance, buildings, cars and people. Obviously, what we found to be impressive were not at all this kind of commonly occurring content but sequences which were very unusual and followed unexpected twists and turns. Nevertheless the methodological difficulty of proving this in terms of statistics remains in principle the same. Although I believe we have now on our own initiative solved the problem of how to evaluate the subjective validation hypothesis, regrettably I was left wondering, if parapsychologists have to develop methodologies to evaluate both psi and non-psi theories, then what do we need our well-funded critics for?

The learning experience for me has been that critics who today merely dismiss the field as artefact, although having earlier played an important role in the development of standards, are increasingly irrelevant to contemporary research needs. Given that most professional parapsychologists have a good training in methodology (as evidenced by Rupert Sheldrake's findings that they have higher standards of controls in their experiments than other specialities) the input of critics often detracts from the more serious research efforts.

The reader might however be easily led into thinking that I am advocating a form of doctrinal insularity. Indeed, the new SPR president, Professor Bernard Carr, in his inaugural speech (summarised in the *Paranormal Review*, October 2000) spoke of the need for pluralism in parapsychology. Traditionally pluralism has been a strength and a hallmark of a developing field. Yet pluralism without a positive thrust forwards can be self-defeating in the long run. Many outsiders depict our field as an exercise in methodology without solid findings or theoretical content. In my experience, grant-giving bodies no longer want to sponsor more replications but, quite reasonably after 70 years of experimental research, look for a starting point in theories and facts. Even some of the more responsible

critics such as Alcock and Hyman accept that we have come as far as we can towards proving there is an anomaly and are of the opinion that, if psi is a meaningful anomaly, it will show relationships to other psychological phenomena. Rather than spending an inordinate amount of time debating with critics, it is these relationships I believe we should be exploring.

This is not, however, the situation today. No other science wastes as much time as parapsychology does in back-tracking. Recently much ado was made about the Schlitz and Wiseman findings concerning the presence of an experimenter effect in their work. The results led Richard Wiseman to publicly comment that "either something weird is happening or Marilyn cheated". Given Wiseman's high profile as a security expert and the number of times the experiment was run, a cynic could of course interpret this merely as a self-declaration of incompetence.

Yet there is nothing new here. Differential experimenter effects have been noted ever since the first issues of the *Journal of Parapsychology*. By the 1970s experimenter effects had become so commonplace that Rhea White published a series of three lengthy review papers discussing the possible mechanisms involved. These reviews go virtually unnoticed by today's critics and parapsychologists.

By the same token, I am also at fault. At the SPR conference in York, when I first presented our material on the way in which apparent psi-derived imagery entered consciousness, Guy Playfair remarked on how we were merely re-discovering the wheel. Some time later I became reacquainted with the work of Upton Sinclair, which at least in some respects had found evidence seventy years earlier of the same kind of top-down processes that we are recording.

All this points to the need to go forward with more persistent research designs that have a basis in not only dry experimental parapsychology but also in the rich nature of spontaneous cases that form the literature of psychical research. Accepting psi as a working hypothesis frees us from having to evaluate each and every case of apparent ESP as though it were the one and only evidence for the phenomenon, and to look more positively at what we can learn about the process. In this respect I like to think that current work may

reveal something new because it is closer to real life. By using real-time recordings, we can actually study more or less natural psi events as they occur in the laboratory,

This brings me to the wider context of my impressions of current research in the field. When I began at Edinburgh as the first doctoral student in parapsychology in modern time, I could not have foreseen that the trend which would follow would be as productive as Matthew Smith recently reported (*Paranormal Review*, April 1999 and Perrott-Warrick Conference, 2000). Some 20 or more doctorates have been awarded in parapsychology, and research activity in the subject now takes place at 13 UK universities. At the time of my doctorate during the late seventies, I was still being discouraged from making a career in parapsychology, and it was estimated that there were not more than 20 persons in the world working professionally in parapsychology.

Today, if we include the numbers working at Hans Bender's former Institute, the IGPP, the figure easily may well be closer to two hundred. (Currently a radical re-deployment of resources is taking place at the Freiburg Institute that will substantially reduce the numbers.) Given this apparent increase in productivity, is there some discernible progress in our knowledge about psi? At a recent conference I asked my former doctoral examiner, Professor Donald West, how the field has changed. Donald West is one of the most experienced and respected academics in our field and one of the first people to encourage this 'academicisation' of parapsychology. His comments confirmed my impression that while there is more activity on paper, there is little if any progress, perhaps because very few individuals are actually doing work on the study of paranormal phenomena in terms of the psi-hypothesis. Normalisation and acceptance have happened but to a large degree at the cost of denying our subject-matter and this would clearly fall under 'bad' if we think in terms of the above terminology. The impression gained from the papers presented at international SPR conferences and PA conventions and from mass-media reports (where the Perrott-Warrick researchers have had high profiles) is that most of UK parapsychology is concerned with what looks like psi but is not psi, and with re-educating the

public towards not believing in psi. Parapsychology, which should be an exciting subject, has become staid, boring, and self-defeating.

This is not intended as a critique of Professor Morris's enormous success in spreading research beyond Edinburgh but is directed towards his protégés, by suggesting that a more confident approach in seeking phenomena and potentially gifted individuals may now be appropriate.

The paradoxical aspect of this situation was recently made concrete for me in the form of the controversy surrounding Geller. Despite having lived in the UK for more than 20 years, it took a journalist in the form of Jonathon Margolis to make a critical evaluation of his claims of possessing a paranormal ability making an in-depth study based on childhood accounts, film records, and critical evaluations by magicians and psychologists.

Lest I be labelled a Geller supporter, I should add that my correspondence with one of the world's leading Geller-experts, Marcello Truzzi, has given me some reservations about the final qualified positive conclusion Margolis arrives at — that Geller does use a mixture of both tricks and genuine psi-effects. Nevertheless in view of this possibility, one can only welcome recent signs that Geller may be trying to 'come clean' in allowing his name to be included in a list of top 100 magicians.

To my knowledge no UK-university based researcher has shown an active interest in gaining Geller's co-operation with a view to investigating his continuing, highly vocal, claims of producing, at least on some occasions, genuine PK-effects. A possible exception is Dr Susan Blackmore, who visited Geller, swam in his pool, went out in his boat, and then apparently without further ado wrote a scathing dismissal of him as a mere trickster (*Paranormal Review*, January, 1999). According to Geller, this episode ended naturally any goodwill on his part to work with the SPR (with which he identified her). My view is that it would be damning if in this age of technical equipment the Geller case is left as enigmatic as that of Daniel Home.

The reason for at least some of this hostility amongst academics towards investigating became obvious when I arranged a visit to Geller's home accompanied by Dr John Beloff. I was then warned by several colleagues that I

risked not only ending up myself as a digitalised exhibit on his web-site but even leading an esteemed figure in the field to the same ignominious fate. Although John Beloff has had the courage to submit an article on Geller to the *European Journal of Parapsychology*, needless to say this fear has proved to be ill-founded. Like clinicians having to deal with psychotic people, parapsychologists may have to accept that part of their subject-matter has to be dealing with the narcissistic needs of psychic claimants.

This apparent over-concern with public image can naturally be seen as belonging to the 'ugly' aspects of the current situation but I would prefer to reserve this category for the more vulgar aspects of academic life, namely the search for funding, fame and fortune. It is often said that the major impediment to progress in parapsychology is lack of funding. However, given that the number of professionally educated researchers working actively on the psi-hypothesis is still small and given the amount of funding which has been *testamentated* to psi research, there should *in principle* be no shortage of funding. Major funds with this kind of stipulation include the Koestler Foundation, the BIAL Foundation, the Perrott-Warrick Fund, and the Holler Stiftung in Munich. Additionally there are many smaller funds (such as the Hodgson Fund at Harvard, the Coover Fund at Stanford, the John Björkhem Fund in Stockholm, the Parapsychology Foundation, and the lesser known Swedenborg's funds in London and at the Swedish Royal Academy). The problem lies not in the total amount of financing available but rather in many cases the appropriation of funding that was testamentated to parapsychological research to support non-psi research.

In the UK, the obvious example of the misuse of funds concerns the Perrott-Warrick Fund, administered by Cambridge University. During a four-year period Nicholas Humphrey used this fund merely to write a popularised book dismissing all belief in the paranormal and religion as nonsense, and in the process receiving not only funding for his propagandist activities but also the subsequent proceeds from the sale of the book. Thanks to the efforts of Donald West, Bernard Carr, and Brian Josephson, the Perrott-Warrick fund has been used during the five years since then largely to

support the research activities of Doctors Susan Blackmore and Richard Wiseman. Some additional relatively minor support was also given to other university researchers, including, amongst others, our own project at Gothenburg.

But all is not well. Although Wiseman's "Perrott-Warrick Unit" at Hertfordshire University is mainly funded by the *Perrott-Warrick Fund for Psychical Research*, the psychical research part of the name is conspicuously absent from the name of the unit. Moreover the web-site makes no mention of parapsychology or psychical research at all, but states that the unit is run on a "prestigious five year (now stretching to nine years or more) Perrott Warrick Fellowship administered through Trinity College Cambridge to research a wide range of psychological phenomena and communicate the methods and results of this work to the public". As well as the much-touted "psychology of the indian rope trick", these psychological phenomena are defined to include "the *psychology* of the paranormal (my emphasis), lying and deception, intuition and false memory syndrome".

There can be little doubt that some, if not most, of the research carried out at Hertfordshire could be seen as a violation of the terms of the Perrott-Warrick Will and Testament which states specifically that money is used exclusively for psychical research. Thanks to the public note by Professor West (*Paranormal Review*, April 1999) we know that the terms of the Will and Testament define this subject quite explicitly as promoting research into paranormal ability and the survival problem. On the basis of Professor West's statement there would appear to be no legal room for arbitrary re-interpretation of the above kind.

To be fair, this is not to say that all the research done in the Hertfordshire unit has been remote to parapsychology. Although it is unclear as to how much of the Wiseman's Perrott-Warrick-funded time was actually spent on this particular project, one of the topics that could be considered to be truly in the spirit of the terms of the Will was the much-heralded "Mind Machine Project". Regrettably, it is just this project, involving as it did portable kiosks with a computer coin-flipping game, which has been heavily criticised by professional parapsychologists and others for showing at

best a naïvety towards and at worst a blatant disregard for previous findings on psi-conducive test conditions.

Another heavily controversial contribution from this unit came in the form of the Milton and Wiseman meta-analysis of the ganzfeld work published in the *Psychological Bulletin*. Perhaps one of the more ugly aspects of academic life in the United Kingdom concerns the allocation of departmental grants to universities on a competitive basis in accordance with publication rate. A paper in *Psychological Bulletin* is considered to be a highly sought-after high-status publication. Apparently the Milton-Wiseman paper was accepted on condition that it was to be accompanied by a reply. It was finally decided this would be from by Professor Morris. Wiseman has given varying accounts of what then happened during the delay caused by Professor Morris's subsequent period of illness but one account is that the authors pressed for publication. In any event, the outcome was a publication without a reply from Professor Morris. Worse, Milton and Wiseman were by then in possession of unpublished data which should have tempered their conclusions. It did not. Their bottom line reads: "This failure to replicate could indicate that the autoganzfeld's results were spurious..." True, they did add a rider that the failed replications might nevertheless be due a lack of psi-conducive conditions but the reader may well ask if psychologists would give attention to this. The answer came recently in one of the most widely used international psychology textbooks (Passer & Smith's 2001 edition of *Psychology*). Although the book does give the *JSPR* a much needed citation (by mentioning the Gothenburg work), this is the company of the Amazing Randi, and Milton and Wiseman, citing the latter to discredit everything else in parapsychology that has gone before. To do so italics are even added to the Milton and Wiseman's "not replicable" pronouncement.

Was the Milton and Wiseman publication then as claimed by many, irresponsible and premature? Since Milton's own later analysis (which then included more of the Edinburgh and Gothenburg work) showed overall significance and a paper presented at this year's Parapsychological Association convention suggested that a more refined analysis showed the effect size to be replicable, there

were problems should that have been resolved before going public.

Another project that does fall inside the specification of the Perrott-Warrick funding concerns the series of studies that Wiseman carried out with Marilyn Schlitz. It was these that showed an experimenter effect suggesting that Schlitz was psi-conducive and Wiseman, psi-inhibitory. As was pointed out earlier, for parapsychologists there was in fact nothing new in this, but at least it held promise of revealing something important had it been followed up. There was no lack of further funding so one is left wondering why was this, apparently Wiseman's only major finding on psi during the five years, never followed up? In view of the eagerness to publish the Milton and Wiseman data, and to go public to the mass media on the equally premature findings on the Sheldrake work, the question can also be raised as to why this study never received any of the same publicity? It may be going too far to say that the selective nature of the mass-media publications and journal publications suggests that the Perrott-Warrick Fund was financing a dangerous mixture of showmanship and science, but the reader might be forgiven for thinking there is at least some truth in this. Undaunted by the danger of being seen as a latter-day Harry Price, Wiseman has included among his most recent projects the attempt at debunking the Hampton Court ghosts and trying his hand at ganzfeld by attempting the "world's greatest ESP experiment". This scene on this occasion was described by Mick O'Neill as being "more like a press conference than an experiment to test many senders" (see *ForteanTimes*: <http://www.forteanimes.com/esp/crit/esp.crit.html>).

On first impressions, the same critique concerning the use of the Perrott-Warrick fund cannot be made of the other major research project the fund supported, namely that carried out under Dr Susan Blackmore's leadership. Blackmore's "Perrott-Warrick Project" (again the psychical research is dropped from the title) at the University of the West of England was heavily funded during a 5-year period to investigate her idea that borderline states of consciousness such as sleep paralysis, false awakenings, and hallucinations, could be psi-conducive. Some of these hypotheses may seem a little bizarre and off the main track but it appears evident from her declaration that she

is intending to pursue investigations which assume psi to occur.

In principle then, this would seem at least to relate vaguely to the terms of the Perrott-Warrick Fund. I will skip a detailed analysis of her reports that might however question whether this was really so in practice, and instead choose to illustrate the general attitude which has permeated her investigations. In 1996, Blackmore presented a paper at the Tucson II Conference on Consciousness entitled *Why psi tells us nothing about Consciousness*. She ends by being quite categorical in her answer: "I would like to be able to provide a fair and unbiased assessment of the evidence for psi, however briefly. This is simply impossible. Many people have tried and failed. In some of the best debates in parapsychology the proponents and critics have ended up simply agreeing to differ — for the evidence can be read either way (e.g. Hyman and Honorton, 1986). The only truly scientific position seems to be to remain on the fence, and yet to do science in practice you have to decide which avenues are worth pursuing. I do not think psi is." As for the role of consciousness, she states also her view quite explicitly and again categorically: "our ordinary view of ourselves, as conscious, active agents experiencing a real world, is wrong — an illusion."

Many things could be said here about the content of the above statement. First, Hyman and Honorton never simply chose to disagree and go their own ways but Honorton took account of Hyman's criticisms and his subsequent work remains one of the few classic examples in psychology where an exchange has led to a constructive resolution. Indeed, Hyman is on record as now saying that an anomaly does appear to exist. Then it should be said that scientific debates and conferences can sometimes be worked up to emotional events which allow some presenters to bring to bear skills that once again seem to relate more to showmanship than real science. Blackmore is then able to help the academic audience out of any dilemma or dissonance created by the evidence for psi, by simply saying that *as long as people disagree*, they, the audience, can always do like her and choose not to accept or be bothered by the evidence. This argument is as elegantly seductive as it is suspect, simply because if there is one thing one can be certain

of, no matter how good evidence becomes, there always will be some disagreement about psi (just as there is about everything in psychology) and many people want categorical answers. There is however some irony with using this argument because in view of the above-mentioned declaration of intent in her Perrott-Warrick-funded work, it must leave Blackmore as a psi-researcher with some feeling of dissonance. If her view is sincere, then was she not on her own account wasting her time and others' money with the above Perrott-Warrick project? Naturally, it might be considered a little crass to insist, as J. B. Rhine did, that only psi-conducive experimenters should do parapsychological research, but given the overwhelming evidence for experimenter effects, was the above not cause enough for the Perrott-Warrick electors to at least insist on Blackmore working together with researchers who believed in what they were doing?

Ultimately, the final judgement of these Perrott-Warrick-funded ventures must be in terms of the progress in the field that the donors intended their money be used for. For me, unless I allow myself to become entirely cynical about the career motives, it remains an enigma as to why two gifted researchers, enjoying the privileged position of being well-funded to do research in parapsychology, actually chose to ignore so much of what is known about psi-conducive conditions and then, in the apparent absence of psi, to spend so many of their resources on experiences other than the fascinating and challenging ones they should have been investigating.

Of course it can be said that this is all in the past and Professor Carr (*Paranormal Review*, October 2000) does give us a more positive picture of the outcome of the current Perrott-Warrick awards. Major support was last year actually given to professional parapsychologists, with Professor Delanoy at University College Northampton being named the Perrott-Warrick scholar.

Considering that Prof. Carr and Prof. West occupy a minority viewpoint on a board dominated by critics, one can also have sympathy for their predicament and see this success as a distinct improvement on the earlier situation. Nevertheless, I am still left wondering if British Law is so vague and committees only answerable unto themselves,

that Wills and testaments become cases for arbitrary re-interpretation? (This is not the case in Sweden, where the public access to such information is legally guaranteed.) As well as the above awards, substantial awards were given again to the Wiseman Unit and also once again to a non-parapsychologist, Dr Robin Wooffitt, whose proposed linguistic study while it may be of interest to sociology, is of no relevance whatsoever to mainstream parapsychology or psychical research. To merely reason that this is an improvement over the past situation is like saying, last year we had £150,000 stolen but this year it is only £50,000.

Like many others in this field we could gladly put to use funding that was intended for parapsychology which is being used for other purposes. Lack of funding during the last year has virtually eliminated our possibility of carrying out active research and there is no certainty we will exist as a unit after the middle of June. We survive only because of some support from the BIAL fund and as an internal project of the Freiburg Institute.

I like to think that one of my achievements while in Sweden has been to make the John Björkhem Fund (which by comparison is a very small fund) open to foreign applicants. Naturally some priority is given in practice to good-quality Swedish applications but we have supported many foreign projects. In this age of international co-operation, I would urge the Perrott-Warrick Committee to adopt a similar policy of at least considering projects that are designed as international ones.

In a small field with limited resources and different centres of expertise, much can be gained by encouraging collaborative international projects. It is clearly 'bad' if, out of rivalry, several groups are independently developing the same technique and making the same mistakes. For instance, there are now projects underway to develop the digital ganzfeld at Edinburgh and at Liverpool to which we, the team at Gothenburg, readily offer our expertise based on the two years spent developing this technique.

Some of the difficulties encountered in this field may not just be due to the difficult subject-matter but rather to the egos of the researchers that the field tends to attract. This more 'ugly' aspect leads many newcomers either to choose a career in debunking or to

believe that just they are the appointed ones who will make the breakthrough that previous generations failed to make. In this respect one of the most prestigious groups, carrying out parapsychological research for the last thirty years, is the Princeton Engineering Anomalies Research Laboratory (so-called PEAR lab). Unfortunately, aside from the efforts of Roger Nelson in maintaining contact with mainstream parapsychology, nearly all of the work of this group is carried out in isolation. Running a long-term project without the benefit of feedback from peers entails the risk of developing an insensitivity to sources of error and of a gradual departure from science. Rather than publishing in parapsychological journals, their work is published in the *Journal for Scientific Exploration* along with articles on all sundry kinds of truly anomalous phenomena. Professor Ian Stevenson's important work on reincarnation that should have major implications for parapsychology is also published in this *Journal*. The reason for this is that Stevenson resigned, understandably, from the Parapsychological Association (PA) many years ago because of its then rather negative attitude towards the importance of spontaneous cases. Despite the passage of time, efforts by others (including myself) have failed to achieve a reconciliation with the PA. This may say more about the career-oriented insularity of the PA than Professor Stevenson in that during all this he has maintained positive relationships with both the SPR and the Swedish SPR.

Given that there is hardly an organisation in this field that does not have latent animosities towards other groups, then these unresolved conflicts become not only a serious hindrance to progress in the field, but they also detract from a unified image that might, at least in some measure, reduce the exploitation of the field.

In this respect one of the most worrying signs is the current suggestion, backed by some leading researchers, to rename the Parapsychological Association and its associated *Journal of Parapsychology* as some form of 'Anomalies Research'. This suggestion arose because of the exploitation of the name 'parapsychology' by various New Age and occult groups and the wish amongst American academics to avoid being associated with this label.

Any further attempt at re-naming of the field may however only detract from parapsychology's plausibility as a consistent scientific endeavour. In fact repeated re-naming of phenomena has been quoted in itself as a sign of bogus science. It can be likened to playing hide and seek with our feet showing from behind the curtain. As Charles Tart says in his introduction to his book *Mind, Body and Spirit*, renaming ESP and PK as 'anomalous cognition' and 'anomalous perturbation' fools no-one. Moreover calling psi 'anomalous' is surely a misnomer, given that more than 50% of the general population believe they have had these experiences. As well as there being a danger of losing the knowledge that has been accrued about psi, the anomaly label also allows psi to be classified along with a host of other phenomena, many of which may be at best irrelevant, and at worst illusory.

In voicing this opinion I am aware that I am dissenting from the opinion of Ed May, one of the most eloquent speakers in the Bob Morris Stockholm team. Indeed Ed May is one of parapsychology's most persuasive and gifted orators, so much so that even critics seem, at least for the duration of his talks, to be rolled over by his extraordinary findings relating psi to astronomical (sidereal) time. These findings are based on an observation by James Spottiswoode that experimentation at the right astronomical time can actually increase effect sizes by as much as 3½ times. What is being implied here is that there is a possible causal link of psi to the position of the earth as measured in terms of its orientation to the centre of the galaxy.

When Ed May requested access to our own ganzfeld data with the times of testing, I arranged for an uninformed graduate student to locate and send him these. Despite this precaution and my scepticism, the plot did closely fit the predicted one. Does this mean I join what has now become the throng of parapsychologists supporting of this theory? No, for me a more feasible explanation may ironically be found in May's earlier work — in what he calls *decision augmentation theory*. The theory supposes that ESP operates as a form intuitive 'opportunism' by knowing just where and when to make the right response. If the above effect is valid, then in terms of this theory we should be looking not towards the stars but at the psi abilities of James

Spottiswoode. By being able to pull out the right data sheets with curves (and evidentially Spottiswoode did pull out quite a few, before being successful) that at least for a time would have predictability, he may simply have succeeded at a psychic task. This is not so strange as it may sound. The real significance of the Spottiswoode and May data may be that human consciousness, when faced with what should be random data, can somehow impose meaningful relationships. There seem to be many instances of this and Bible Codes (if they have statistical validity) may be a further example of this. Moreover, there is a little-known paper by Brian Millar published in the 1979 *European Journal of Parapsychology* which concluded that while true psi ability is actually rare, it is grossly over-represented amongst parapsychology's psi-conducive experimenters. In this context it is worth mentioning that when the successful ganzfeld experimenter, Kathy Dalton, took part in our ganzfeld experiment, she was able herself to produce an impressive hit.

What are then the current strengths and 'good' aspects of contemporary parapsychology? In working as part of the above-mentioned 'Morris-show', it became apparent that while each of us became skilled at presenting a pedagogically persuasive talk, a cohesive theme linking our talks was still lacking. It is easy to say that this merely reflects the reality when there is no accepted theory linking findings together in parapsychology. Yet many of the hypotheses in parapsychology have at least as much validity as those accepted with psychology and psychiatry as facts. Consequently, instead of trying to get psychologists to swallow psi as a supposedly harmless, mere anomaly that they do not need to pay much heed to, we need to do the opposite and relate it to other psychological phenomena. Artistic and musical abilities are the most obvious ones for which a fairly convincing case of a link with psi could be made both on the basis of experimental and anecdotal experiences. A current example is to be found in the *Beatles Anthology* where Ringo Starr is quoted as saying when the group played at its best, it was a telepathic experience but as soon as they stopped to analyse their contributions, this fell apart. Creativity and intuition are perhaps the most natural areas to which psi phenomena can be related and given

the growth of feminist psychology with its interest in these, it would be strategic to take advantage of the priority given to such neglected areas.

Beyond this we know for instance that psi-experiences occur most often and convey most information during altered states. One of the more consistently replicated and yet least cited findings is that psi occurs not during a specific state of consciousness but rather during a rapid change in state when presumably psychological 'filters' may not be so effective.

We know that research has shown that even the experimenter is an important variable and that the psi-effect appears to be multi-determined by all participants and not confined to the formal restraint of the experiment. Some findings from the field of psychical research suggest that expectancy of an effect and more open world-views may be crucial to this. The success of various mediums including some psychic claimants (possibly on occasion even Geller) may be due to their ability to manipulate this and give permission for a miracle to happen. The well-known findings of Kenneth Batchelder and the less known corroboration by Orjan Björkhem (presented at the PA in 1994) support this.

There is also evidence linking psi to mental effects on bodily functions, findings which have implications for psychoimmunology and may provide a scientific basis for healing claims. The research of Professor Delanoy relates well to this area. Neuropsychology is an area that has gained status and prominence in research and accordingly a presentation of modern parapsychology would do well to include the work of Michael Persinger relating psi and altered states to the activity of specific areas of the temporal lobe.

These observations can be used in a positive and confident way to improve our public relations. There are many fascinating phenomena and findings, a growth in research institutions and in the number of doctoral students. With a sensible allocation of funding, there is little doubt that rapid progress will be made. We have a basis of knowledge on which to stand and to leave self-indulgent forms of scepticism behind.

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