

Is the spirit willing?

A pentologue on parapsychology



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Andrew M. Colman, Susan Blackmore, Robert L. Morris, Richard Wiseman, and Christopher C. French discuss a central dilemma of research into the paranormal.

THIS article started life as a review of *Test Your Psychic Powers* by Susan Blackmore and Adam Hart-Davis (1995). The author of the review, Andrew Colman, felt that one of the issues he wished to raise was not only controversial but also of sufficient importance and general interest to warrant an open discussion incorporating more than one opinion, so he approached the editor of *The Psychologist* with the suggestion of an article-length propaedeutic exchange of views, along the lines of Bronfenbrenner, Kessel, Kessen, and White (1986), in which the fundamental issue could be aired and discussed among several psychologists with an interest in parapsychology. The editor agreed to the suggestion, and this article is the result.

The opening remarks by Andrew M. Colman were sent to Susan Blackmore for a response, and then both Colman's and Blackmore's contributions were sent to Robert L. Morris for further comment, and so on, so each of the separate contributions to the discussion was written in the light of all the ones that preceded it.

Andrew M. Colman

Most subjects become more interesting the more one learns about them, but the paranormal seems - in my experience at least - to be a striking exception to this rule. People who know little about allegedly paranormal phenomena are often intrigued, but the more one learns, the less interesting they seem. They all dissolve under scrutiny, the scientific evidence turns out to be either methodologically flawed (for example, many of Rhine's experiments, Targ and Puthoff's remote viewing experiments, most psi Ganzfeld experiments), or merely fraudulent (for example, the Soal-Goldney experiments, several other experiments from Rhine's laboratory), and nothing of significance is left to explain. That, at least, was the view I formed when I reviewed the evidence

on extra-sensory perception (ESP) some years ago (Colman, 1987, ch. 7), and nothing that I have read or heard since then has persuaded me to change it.

In spite of this, Susan Blackmore and Adam Hart-Davis's (1995) unusual and ambitious new book certainly manages to rekindle interest in the paranormal without sensationalizing or distorting the evidence. It is really a brief introduction to psychical research together with a laboratory manual for home use. It covers telepathy, effects of crystals, dreams and lucid dreaming, dowsing, premonitions and precognition, psychokinesis, ouija and planchette phenomena, palmistry, and astrology. Each phenomenon is introduced with startling anecdotal evidence, followed by a brief though balanced summary of relevant research evidence, and (a surprise feature that makes the book unique) a detailed explanation of how to perform a suitably controlled experiment, together with clear drawings and Blue-Peter-type instructions on how to construct a planchette from a margarine tub and a ballpoint pen, or how to make a deck of ESP cards, a dowsing rod, a box for testing psychokinetic effects on woodlice (no kidding), or other necessary apparatus. The discussions are superb and the experiments so carefully thought out and so clearly explained that this book may well forge general readers into amateur experimenters - a delightful thought.

Now the big question which, like a 70-year-old rug that has been lying on the hearth for too long, deserves a thorough airing. Blackmore and Hart-Davis's (1995) readable and balanced little introduction to the paranormal notwithstanding, what is the point of it all? Why bother carrying out more experiments into these obviously non-existent phenomena? Take astrology, for example. Numerous empirical studies have shown that astrology is humbug - the Gauquelin effects are irrelevant to conventional astrology, may be interpretable as

merely the effects of being born at different times of the year, and may in any event be artifacts - but the studies were pointless anyway, because the theory is self-evidently ridiculous. Mars is associated with aggression because it is red, but we now know that this is not because of the presence of blood but rather of iron-rich clay of the type we have used on earth for building red-brick universities; Venus represents love and beauty because it looks soft and pale, but we now know that its white mist is made of sulphuric acid; the 12 astrological constellations are associated with personal qualities by virtue of their supposed resemblance to a ram, a bull, twins, and so on, but we now know that they would look quite different from another angle; and anyway they cannot have their claimed effects in far northern latitudes because some of the constellations never even rise there. The qualities of the constellations are, in any event, beside the point, because the precession of the earth's axis has caused all of them to slip out of place during the centuries since astrology was devised by the Babylonians; at the Spring equinox, for example, the sun is supposed by astrologers to be in Aries but is nowadays actually in Pisces.

What is the point of carrying out more experiments on this nonsense, or for that matter on other allegedly paranormal phenomena? The energies of curious and intelligent researchers, both amateur and professional, could surely be better spent on less futile pursuits, such as investigating why people believe such things - an interesting research area in which the other contributors to this discussion have made important contributions. I don't expect that the other contributors will necessarily agree with my conclusion, but I find it hard to imagine what their counter-arguments will be. If someone seriously hypothesized that the moon is made of Gorgonzola, would we devote resources to testing it scientifically? If not, then why astrology and other preposterous doctrines and theories of the paranormal?

Susan Blackmore

Why bother? My answer is the reason we wrote the book. Twenty-five years ago I was convinced that telepathy was real, astral bodies could fly, and scientists were denying the truth. The reason I changed my mind was not the lack of laboratory evidence, nor the sceptics on TV. It was doing experiments for myself and finding out I was wrong.

Imagine you have learned to dowse, and felt the twigs mysteriously jump in your hands as you approach water. It is not unreasonable to conclude that dowsing works. But when you do an experiment with five empty buckets

and one full one you learn something new. With no lids the twigs still jump over the water; put lids on and they don't. The inevitable conclusion is that it works only when you know the answer in advance - and now you have really learned something about unconscious muscular action!

Similarly, experimenting with horoscopes and palmistry reveals our willingness to accept generalized personality readings, and just thinking through a controlled test of crystal power may be enough to make you laugh at the temptation to part with good money for a pretty lump of quartz.

What if some psychic claims are valid? Then doing experiments is the best way to find out. I hope Andrew Colman is right, and this book forges a few more amateur experimenters. Science is not just for scientists. Everyone can benefit from thinking experimentally - especially about something they care about.

Finally there are all those psychology students who hate experimental design and statistics. Using parapsychological practicals I have watched the arguments change from 'Yes it is!' 'No it isn't!' to 'Could she have heard something?' or 'Might he be guessing?' to 'How can we find out? ...'. This is why I bother.

Robert L. Morris

Andrew Colman appears to argue that studies of an effect are pointless if the theory behind it 'is self-evidently ridiculous'. Unfortunately, it is not always easy to achieve a consensus on the absurdity of a theory, and also, I don't think we should confuse an effect with its interpretation. The evidence for an effect is independent of a given theoretical interpretation of the effect. This is why researchers such as ourselves at Edinburgh prefer a bottom-up approach, starting with the available data and evaluating various alternative interpretations of those data.

We start with the consensually accepted finding that many people have experiences, or observe events, which they cannot explain conventionally. Our research includes evaluating conventional



interpretations, including self-deception or innocent cognitive mistakes, as well as strategies of deception and exploitation by clever frauds. We also investigate the various factors that affect the formation, development and maintenance of belief systems. Our approach thus overlaps considerably with Sue Blackmore's and our other colleagues in this debate.

We also research the possibility that there are genuinely new means of environmental interaction still to be uncovered. This is done by exploring some of the most promising evidence from other researchers and doing our best to improve upon it, and we are encouraged by our own results as well as those of other research units. Methodology continues to improve, as does our ability to deal with potential fraud interpretations. Our reading of the recent literature appears to differ from Andrew Colman's. One problem of course is that opinions about how well or poorly a set of studies has been conducted or what constitutes a serious rather than trivial flaw can be somewhat subjective, as any editor, reviewer or reviewee knows all too well. This is especially a problem in studies of complex systems, and we have much to learn in this area. I have yet to read a study with no flaws at all. Perhaps readers can suggest one.

It is clear that if any new means of interaction do exist, they are not readily observed in our daily life. Thus much of our work is aimed at examining special circumstances reported to be conducive to their manifestation. We look at individual differences and situational

factors, we vary conditions to examine their effects upon results, and we try to develop testable models such as the noise reduction model that lies behind some ESP research such as the Ganzfeld studies.

If any truly new means of interaction do exist, we shall learn most about their nature through a multidisciplinary approach with diverse but complementary methodologies, and through research procedures that have meaning for the participants. If no new means exist, then such an approach is also necessary, to build a clear picture of how we have been misleading ourselves or have been misled by others. And this picture must be clear to the public as well as the sophisticated researcher. In order to be persuasive, any research programme must give the purported effect the best opportunity to occur, yet under adequately controlled conditions. This has rarely been done. The failure of trivial but well controlled experiments is no more persuasive to the public than the success of exciting but poorly controlled studies.

One last point. We prefer to avoid terms like 'paranormal'. However some researchers (e.g., Irwin, 1994) define paranormal events as those that are presently unexplained but are assumed to be eventually explainable through scientific advances in our understanding of the natural world. By this definition, scientists should find exploration of paranormal events of considerable potential importance. If we choose to ignore such effects or dismiss them prematurely, then we yield the floor to the charlatans and the gullible, we all remain truly ignorant, and science has failed the public. I think Andrew Colman would agree with this, and I suspect that some of our apparent disagreement may arise because we employ different definitions of the term 'paranormal'.

Richard Wiseman

I believe that parapsychological research is worthwhile for two reasons. First, it represents an excellent opportunity to evaluate and develop the methods used by science to decide upon the reality of reported phenomena. For example, criticisms of some psi studies have helped uncover subtle methodological and statistical artifacts which can cause spurious results in both parapsychological and psychological studies - see, e.g., Rosenthal's (1978) discussion of possible sources of bias in data collection/recording. At a sociological level, Collins and Pinch (1982) have examined scientists' reactions to claims of psychic ability and used these to illustrate how science deals with novel and unusual ideas.

Second, I believe that the debate concerning the existence of psychic

abilities cannot be solved by arguing about whether such phenomena are theoretically possible. Instead, I agree with Bob Morris's comments on the difficulties of achieving a consensus on the absurdity of a theory and believe that the best way of establishing whether such abilities are genuine is through carrying out well designed experiments. Psi proponents argue that these data have already been collected and do indeed support the existence of some form of 'anomalous cognition' (e.g., Bem & Honorton, 1994). Although I do not endorse these conclusions, the data are certainly intriguing and deserving of further investigation.

Christopher C. French

I too disagree with Andrew Colman's claim that further parapsychological research is pointless, even though I suspect that paranormal forces do not exist. The simple truth is that I may be wrong. The list of 'preposterous doctrines and theories' which the wider scientific community initially rejected only to subsequently embrace is a long one, e.g., Wegener's theory of continental drift, Mayer's law of conservation of energy, and Boltzman's kinetic theory of gases. Although this list may be long, however, the list of apparently crackpot ideas which have not been subsequently validated is much longer.

On 31 December 1995 the *Independent on Sunday* published a vitriolic attack on astrology by Richard Dawkins. Judging by the following week's letters page, many readers clearly felt that Dawkins was rejecting astrology simply on the grounds that he personally thought it was nonsense. It was easy to see how such impressions could arise. Dawkins wrote: 'How do I know that there is no truth in astrology? Well, of course I don't know. I can't prove that there is nothing in horoscopes' (p.17). Dawkins would have helped his case considerably by referring to the numerous studies which have shown that astrologers simply cannot do what they say they can. The Chair of Public Understanding of Science did little to enhance the public understanding of science and much to reinforce the view of scientists as arrogant and closed-minded.

The idea that we can reject claims without considering the empirical evidence is simply not good science. There is plenty of good empirical evidence that the moon is not made of cheese, but some of the more recent evidence presented in parapsychology is not that easy to dismiss, as recognized by well-informed sceptics like Ray Hyman. The history of parapsychology, however, leads me to be cautious in accepting it at face value. I was taken in by the Soal-Goldney data as an undergraduate. I

am not implying that the autoganzfeld studies (Bem, 1994; Bem & Honorton, 1994; Hyman, 1994), say, are tainted with fraud but other subtle methodological problems are a real possibility. I will wait and see.

Andrew M. Colman

Bob and Chris say that we can't always reach a consensus about which theories are absurd, and Richard says that the reality of paranormal phenomena cannot be decided by theoretical debates. I agree, though I would maintain that demonstrably self-contradictory theories can and should be rejected out of hand, but I don't agree that the lack of consensus forces the conclusion that we should therefore always do the research. We have no option but to choose which problems to pursue. It would be bad science to treat all hypotheses, even absurd or illogical ones, on an equal footing; in fact, it would be impossible, because we have limited resources. The sensible approach is to pursue promising hypotheses and to ignore worthless ones. Chris is of course right that today's preposterous doctrines are sometimes tomorrow's accepted facts, but that merely shows how hard it is to form a judgement, not that judgement should or could be avoided. We have no alternative but to choose what hypotheses to test - we do it all the time.

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