

## AS IT OCCURRED TO ME: LESSONS LEARNED IN RESEARCHING PARAPSYCHOLOGICAL CLAIMS<sup>1</sup>

By Chris A. Roe

### Introduction

I have been involved in parapsychology for a little over 25 years (including my time as a PhD student), so in preparing this paper I've welcomed the opportunity to reflect on that experience to see if any of it is worth sharing with the wider community. In particular, I wondered if there were lessons I have learned the hard way that could be of benefit to early career researchers, so that their trajectory might be a little more smooth or fruitful than my own has been. I have selected some things that have occurred to me both in the sense of events that have happened and in the sense of realisations I have had based on those experiences. I hope they represent useful insights into what I regard as the art of scientific practice in parapsychology. They have shaped the kind of researcher I have become and perhaps explain some of my preoccupations and biases.

### Early Beginnings

My first love is the scientific method. I first became interested in parapsychology when aged about 14, not through any powerful or vivid personal experience, or through a family history of paranormal beliefs or practices, but by discovering scientific research on the subject. In the UK a magazine was published called *The Unexplained* that promised to explore all sorts of fantastical phenomena, from alien visitations to spontaneous human combustion, and these unsurprisingly were very attractive to a young teenager with a fairly rich imagination but also a skeptical disposition. These tales were sobering in reminding us how much there is still to learn about nature and the phenomena that it permits. However, I quickly became disillusioned by a tendency for reports of investigations to end with the conclusion that the phenomenon remained a mystery, rather proudly declaring that things were inexplicable in terms of current scientific principles, and so cocking a snook at the hubris of the scientific mainstream by demonstrating that it didn't, in fact, know everything, but adding very little to what we did know. Surely demonstrating an anomaly was the *beginning* of the scientist's story rather than the end of it, indicating as it did the most potentially rewarding or revealing areas for future investigation? Thankfully, other articles in the magazine were more measured in tone, particularly, I recall, those highlighting Ed Cox's minilab experiments and Carl Sargent's ganzfeld studies at Cambridge,<sup>2</sup> and these adhered to more recognizably scientific methods that might be superficially less exciting, making slower progress and reporting only modest gains, but ultimately be more satisfying in providing trustworthy accounts of how things really are. And so, aged 14, I determined to become a parapsychologist—how difficult could that be?

Three years later, in 1984, I needed to decide to which universities I would apply. I had a meeting with my school careers adviser and mentioned my interest in parapsychology, and he remarked that he had just seen a press release about the appointment of a new Professor of Parapsychology at Edinburgh University. I can recognise an omen when I see one, and so it was that I applied to study psychology at Edinburgh. My original application to study for a Bachelor of Arts degree was returned to me, with the admissions tutor recommending that if I wanted to become a parapsychologist then I would be better to take a Bachelor of Science pathway, with its greater emphasis on training in experimental work. This was effectively a biological sciences degree with a major in psychology, so that I continued to study physics, chemistry and biology (my A-level subjects) alongside zoology and psychology.

<sup>1</sup>A version of this paper was presented at the Parapsychological Association 35th annual convention, Boulder Colorado, June 19–24, 2016.

<sup>2</sup>See, for example, Cox (1984) and Sargent (1980), but also Hansen (1985).

### The Sociology of Science

So it was that my education to that point was wholly in the natural sciences and had turned me into a philosophically naive positivist researcher—I had been taught that the scientific method was simply a process of disciplined observation that guarded against what Francis Bacon (1620/2015) called idols (of the Tribe, the Cave, the Marketplace, and the Theatre); that is, the ways in which we might deceive ourselves when making observations and when drawing inferences from those observations. From that perspective, nature might seem inscrutable, but it would give up its secrets with appropriate effort and diligence on my part. Science, I thought, is a process of increasing refinement and exactitude, particularly in measurement, and all meaningful properties of the world can be measured objectively and consistently if we are careful enough. I was soon to discover the limits of this philosophy when I came to conduct my own research.

I had a foretaste of things to come when I included in my undergraduate programme modules on the philosophy and sociology of science with members of Edinburgh's influential Science Studies Unit. Scholars such as Barry Barnes, Steve Shapin and David Bloor<sup>3</sup> had a huge influence on me. I was introduced to the notion that even in the natural sciences scientific knowledge can be a function of its social and political time, that the scientific elite is inherently conservative and suppressive, and that, in Thomas Kuhn's (1970) terms, scientific practice is only rarely edging toward revolution and is more commonly engaged in "normal science" with its concomitant aggressive policing of the border between the legitimate and the illegitimate. The work of some of these philosophers and sociologists shows that, for most of the mainstream, parapsychology lies on the wrong side of that border, and is a victim of those processes (e.g., Collins, 1985; Collins & Pinch, 1983; Wallis, 1979).

This perspective informed my undergraduate dissertation project, which looked at how the quality ratings of the methodology of a parapsychology research paper (intended to simulate the journal review process) depends not on the described method itself but on whether the outcome and conclusions are congruent or incongruent with the assessor's own prior beliefs. In a classical instance of cognitive dissonance, participants were able to relieve tension brought about by being confronted with counter evidence by simply dismissing the evidence as invalid. I replicated the effect with students from St Andrews University and published the results in the *British Journal of Psychology* (Roe, 1999)—still my only publication in the flagship journal of the British Psychological Society.

This work drove home to me the extent to which people use their rational faculties for *post facto* justification rather than for genuine decision making, and it made the Jonathan Swift quotation<sup>4</sup> "It is useless to attempt to reason a man out of a thing he was never reasoned into" one of my reference points. In my election statement for PA President I mentioned that I thought we spent too much time and energy engaging with sceptics whose reputations are too strongly associated with the counteradvocate position for there to be any realistic prospect of a shift in their public pronouncements, whatever the quality of methods or data we present. It is interesting to note, for example, that the substantive arguments offered by Ray Hyman (2010) and James Alcock (2010) in Krippner and Friedman's edited book, *Debating Psychic Experience*, are essentially the same as those they offered 20 years earlier in books such as *The Elusive Quarry* and *Science and Supernature*, respectively (Alcock, 1990; Hyman, 1989). That intransigence in the face of quite significant shifts in the methods and evidence base of parapsychology suggests—to me, at least—a preference for rhetoric over genuine engagement with the field. It seems that little has changed since Charles Honorton's (1993) damning critique of "the impoverished state of scepticism," which remains the most incisive criticism of the counteradvocate position.

I am not recommending that we ignore our sceptical colleagues but that we are clear in what we are aiming to achieve when we do respond to them. For example, in 2010 I wrote a rejoinder for *The Skeptic Magazine* to an article by Ray Hyman in which he asked the leading question "is Parapsychology dead or alive?" and I have appeared regularly with Chris French in the UK to discuss parapsychological claims before public audiences. One such event, a panel debate that also included Richard Wiseman on

<sup>3</sup> For examples of their approach, see Barnes (1985), Barnes, Bloor, and Henry (1996), and Shapin (1996).

<sup>4</sup> Attributed to Swift in *Scientific American*, Vol. 7 (Munn & Company, 1851), p. 338, but disputed and possibly apocryphal.

the apparent crisis in psychology and parapsychology at the UK skeptics' conference was attended by perhaps twice as many people as attended the joint meeting of the Parapsychological Association and Society for Scientific Exploration in 2016. My objective in each of these encounters was not, of course, to facilitate a shift in the sceptic's position, but to speak through them to a broader audience, some of whose opinions hopefully had not yet ossified, providing them with a more balanced picture of parapsychology's discoveries and emphasising that its practice was scientific "business as usual." Given the virulent way in which the Wikipedia entries on parapsychology are mismanaged, we have a difficult but important task ahead in ensuring that interested but discriminating members of the public can get access to accurate and balanced information about the state of the field.

### **Considering Normal Explanations**

After my undergraduate degree I went on to study for a PhD at the Koestler Parapsychology Unit, and my research topic reflected the unit's emphasis on crossdisciplinary approaches, and on determining "what looks psychic but isn't" in order to better understand the processes that can lead to an attribution of paranormality. I explored the technique of cold reading by getting access to a set of arcane publications that represent how-to guides for would-be pseudopsychics and by spending time with a practitioner who had been working the circuit for over 30 years. Malcolm<sup>5</sup> agreed to give readings to people he had never met before. The clients were asked to rate the accuracy of the readings and to make a judgement as to whether we should study him further, in more formal research. Two of the three sitters were very impressed by their readings and gave unequivocal recommendations. We videotaped the interactions and then recorded Malcolm as he watched the video and explained the stratagems he had been applying.

In analysing Malcolm's account and synthesising the descriptions from cold reading manuals, it became obvious to me that the technique was actually a set of techniques that varied according to how much information leakage was required from the client and how specific the reading material could be: the more leakage, the greater the specificity (see Roe, 1991; Roe & Roxburgh, 2013a, 2013b). This model argues strongly against the "heads I win, tails you lose" explanation offered by some sceptics, whereby unimpressive readings from mediums and psychics provide evidence that psi does not occur, whereas impressive readings from mediums and psychics are seen as evidence of the widespread use of cold reading and so also show that psi does not occur. The intention of the work was to show that we need to take into account the prevailing conditions when assessing whether communications of the specificity observed could be achieved through cold reading alone. It's interesting to note here that explanations in terms of cold reading make some assumptions about client behaviour, including their tendency to recall only the hits and forget the misses, and to elaborate on given material in ways that make the recalled version more specific to them; surprisingly, the only attempts to test these assumptions that I am aware of have been conducted by me (Roe, 1994)—rather than seeking experimental evidence for cold reading, sceptical researchers have been content to apply the method after the fact to given "real-world" data in a manner that would be scorned if done by a parapsychologist (e.g., Greasley, 2000; Underdown, 2003).

### **The Dynamic Interpersonal Nature of Research With a Sentient Subject and the Effect This Has on Outcome Consistency**

As part of my PhD project I wanted to test whether general statements recommended by pseudopsychics were successful because they acted as Barnum statements (Roe, 1995). Barnum statements are statements that most people accept as true of themselves, but importantly do not recognise they are likely to be equally true for others, and so regard them as uniquely or especially pertinent. The classic Barnum statements were coined by psychologist Bertrand Forer in 1949 and were derived from a newsstand astrology book. They include items such as "At times you have serious doubts as to whether you have made the right decision or done the right thing," and "Disciplined and self-controlled outside, you tend to be worrisome and insecure inside." To assess whether the statements recommended by pseudopsychics work in a similar

<sup>5</sup> A pseudonym used to protect his identity.

way I needed to use a standard clinical setting, similar to those used in hundreds of published tests of the Barnum effect, so I recruited participants to provide personal validation data on a new way of assessing results from the House Tree Person Test. This test unsurprisingly entails the participant being given a blank sheet of paper and a set of coloured pens and pencils and asked to draw a house, a tree, and a person. These drawings are passed on to an analyst who generates a personality description based on their interpretation of the images. This description is given to participants the following week so they can evaluate the analysis. Of course, everyone receives the same feedback, which consists of statements recommended in pseudopsychic manuals, and these show the same levels of acceptance as Barnum statements. Because the study contained an element of deception, I took great care during the debrief period to discuss the experimental design and to ask for their thoughts about participating; I was astonished to discover that quite a few of my participants *had been conducting their own experiments within my experiment!* They hadn't studied the House Tree Person Test before, but they suspected that certain features (such as large eyes, large people relative to the house size) might have particular meanings (such as a tendency to paranoia, or grandiosity) and so they included these elements to see if they would translate into their feedback. Many reported with satisfaction that they had—even though everyone received the same feedback, demonstrating people's capacity to see what they expect to see. More importantly for my thinking about research in general, and contrary to my experience of experimentation in the natural sciences, I realised that participants each created their own version of the experiment that I had designed. Any pretensions I had had to produce a consistent set of conditions across all the trials now seemed naively optimistic. Variations in outcome that I saw could have been a consequence of these variations in the psychological conditions under which each trial or study was conducted. But how the participants constructed the experiment was not a random, uncontrollable factor; rather, expectations about the study and its meaning were in part a consequence of the interpersonal dynamics between researcher and participant. These subtle but important effects seem to be especially relevant to parapsychological research, where the phenomena already seem quite sensitive to conditions (I discuss this in more detail in Roe, 2016b).

To illustrate, I led a project that was intended to tease out the similarities and differences in performance on ESP and PK tasks using a common platform. We developed a greyhound racing game that allowed us to include a condition in which participants acted as gamblers and simply had to choose which dog they thought might win the next race, so we were testing for ESP. In a second condition they acted as owners so didn't have any choice over which dog was theirs, and this allowed us to test for PK. Whereas the movements of dogs in the ESP condition were determined by a fixed list of random numbers drawn in advance of the study from tables, and so was potentially available by ESP, movements in the PK condition were determined in real time by reading data from a random number generator, and so the greyhound "movements" were potentially open to psychokinetic influence. In order to control for expectancy effects, we had half the PK trials in fact test for ESP and half the ESP trials test for PK. We thought the task was intuitive and engaging and were very hopeful that it would be effective. Unfortunately, across three formal studies (Roe, Davey, & Stevens, 2003, 2004, 2005) we found little evidence of overall ESP or PK effects, but—as seems usual in parapsychology studies—there were enough secondary effects to suggest that we weren't looking just at random noise in the data that we had collected. For the final study in the series (Roe, Davey, & Stevens, 2006) we speculated that the null results might reflect a kind of experimenter effect. To that point all the data had been collected by Russell Davey, a graduate research assistant of mine who was a bright and able student, but relatively inexperienced at running parapsychology experiments. In this last study, Davey was again responsible for the recruitment and scheduling of participants, but this time he ran only half of the sessions and the other half were run by me. I was more experienced and more invested in the goals of the project, having spent a lot of time developing the ideas and securing funding. Again, Davey's participants scored at or below chance expectation, but my participants scored significantly better in the disguised ESP and PK conditions and suggestively better overall. Most intriguingly, straight after the initial briefing we rated our interactions with each participant (that is, while the trial was being completed, when we did not know anything about the participant's actual performance) and our confidence of success correlated quite strongly with the outcome (Spearman's  $\rho = -.43$ ,  $p = .007$ ).<sup>6</sup> We took this to suggest that there was some

<sup>6</sup> Scoring on the task was such that a negative correlation indicates that greater confidence was associated with better performance.

ineffable quality of the interaction between experimenter and participant that cued us to the trial's likely success, though this was not some particular property we could identify more exactly or manipulate to ensure successes in subsequent trials. We have since conducted similar studies to look for experimenter-participant interaction effects in dream ESP, ganzfeld, and PMIR studies (Hitchman, Pfeuffer, Sherwood, & Roe, 2016; Roe, Sherwood, Farrell, Savva, & Baker, 2007; Sherwood, Roe, Holt, & Wilson, 2005), and although the findings are as yet equivocal, there are some grounds for optimism, particularly when looking at fluctuations of experimenter mood and expectations of success as predictors of performance. This reaffirms my suspicion that despite our best efforts, there are lots of ways in which one trial differs from another even when superficially we are keeping conditions constant.

This brings to mind Heraclitus's famous observation, "No man ever steps in the same river twice; for it's not the same river and it's not the same man." In the wake of the latest crisis in psychology brought about by the Open Science Collaboration's failure to replicate more than 36% of a set of findings originally reported in high calibre journals, I think it would be useful to consider whether it might be unrealistic to expect replication success to be simply a function of effect size and study power—as it might be in the natural sciences—since here we are working with subtle interpersonal factors and drawing on tacit knowledge. I talk about this at more length in a recent issue of *Mindfield* (Roe, 2016a).

### **The Tension Between the Need for Clarity and Control Versus Authenticity and Ecological Validity When Designing Studies**

In my experimental work I had prided myself that my designs were reasonably sophisticated and did quite a good job in controlling for extraneous variables so that significant outcomes, if there were any, could be more unambiguously attributed to the variables I was manipulating. But I was becoming concerned that in my efforts to contrive an experiment with elaborate controls I had lost sight of how the work linked with people's everyday lived experience. This concern was driven home for me by Tony Lawrence, who during a talk at a conference of the Society for Psychical Research confessed to his sense of embarrassment when in social situations he met people who heard of his interest in parapsychology and volunteered accounts of rich and vivid psychic experiences or abilities of their own. He recognised that this sense of embarrassment reflected *his* shortcoming, not theirs. He was comfortable with psi as an abstract construct, and even with the occurrence of psi in experiments—so long as it was strong enough to produce significance levels that encouraged further research but not so strong that it challenged one's worldview. When he met people for whom psi was sufficiently rich to be an integral part of their sense of self and of their everyday lives, he found that he had nothing to say to them because his research didn't speak directly to their experience. That way of thinking about and managing research was clearly dysfunctional. Tony's conference presentation reminded me that as a psychologist, what I do in the refined conditions of experimental work *must* generate discoveries that can speak to people's lived experiences of psi, and for that to happen it must be founded on those lived experiences.

A case in point is telephone telepathy. Rupert Sheldrake has argued that telephone telepathy is among the most common types of parapsychological experience reported by the general public, based on surveys that posed the question "Have you ever heard the telephone ring or picked up the telephone and known who was on the other end without any possible cue, before they had spoken?" (e.g., Sheldrake, 2000). Such experiences are dismissed by the sceptically minded as reflecting the attribution of meaning to simple coincidence, implicit learning of behaviour patterns that might suggest when people are likely to call, and selective recall of confirmations and forgetting of disconfirmations; all of these are plausible given our susceptibility to such errors. To control for these explanations Sheldrake developed a simple protocol (Sheldrake & Smart, 2003a, 2003b) in which the callee is separated from a group of potential callers and one is chosen at random to make each call. The callees make their guesses once the phone rings but before they pick up the receiver, and their likelihood of success just by chance should be in proportion to the number of potential callers. Sheldrake has had some success with this protocol and it is an attractive way to introduce some of the principles of the scientific method to the general public, but I can't help feeling that he has missed the point of telephone telepathy in his rush to the laboratory, and this may account for some

of the difficulties that others have had in replicating (Schmidt, Müller, & Walach, 2004).

J. B. Rhine had some pertinent advice to those who hoped to study psi in the laboratory: “If you want to have rabbit stew, first catch the rabbit” (cited in Stanford, 1993, p. 129), and in order to be able to catch the rabbit then we need to know something of its behaviour and preferences in its natural environment or else we’ll be constantly chasing its tail. In other words, if we wish to study the action of psi in the laboratory then we need to ensure that the laboratory situation reflects the circumstances under which psi will ordinarily appear in the natural world. With only the most meagre sketching of the phenomenon of telephone telepathy as it occurs in situ, there is a real danger that sceptical accounts will fit insofar as one vague thing can be mapped onto another vague thing, and difficulties in replicating will arise because we have developed no real understanding of the necessary or sufficient conditions for its occurrence.

In this context I thought it important to ask people about the circumstances of their telephone telepathy experiences and of how they made sense of them. We conducted focus groups with people who had had such experiences and we thematically analysed their discussions (Roe & Smith, 2011). Among the themes to emerge were that there were often palpable emotional or physical changes during the experience that signalled to them that this wasn’t merely a coincidence (and by dint of which women were thought more likely to have the experience on the basis that they were more trusting of their emotional responses); also that the experiences usually were an expression of a sense of connection with particular people (that is, the telephone telepathy experience was a verification of something they already “knew” by other means); but most importantly that they saw these experiences as a minor subclass of phenomena that formed part of a cluster of experiences that confirmed for them that they were interconnected spiritual beings. They wanted to talk about instances of telephone telepathy in which they were the caller rather than the callee, about occasions when they felt a growing sense of foreboding or concern for someone close to them, whom they felt compelled to call, and who turned out to be in trouble or unwell. They saw this as an affirmation of their bond and obligation to each other. They also wanted to relate other experiences that they interpreted in terms of their lives forming part of some cosmic design or as reflecting the influence of some benign overseer. In that context the experiences were recognised as trivial in themselves, and attempts to reproduce them in formal experiments seemed facetious.

In an effort to overcome the kind of embarrassment that Tony Lawrence has described, I began to seek opportunities to spend time with practitioners, including healers who were part of the Confederation of Healing Organisations in the UK, but especially mediums who were members of the Spiritualist National Union. Along with my colleague Elizabeth Roxburgh, I attended church services with platform demonstrations, participated in development workshops, and talked with practitioners off the record about their abilities, how they experienced them, and importantly how they managed them. Qualitative methods had been quite alien to me given my background in the natural sciences and my research record over the first 15 years, which consisted almost exclusively of experimental work. However, it became clear that for some phenomena qualitative methods were a better fit for the rich, idiosyncratic and nuanced nature of parapsychological experience. They also appealed in privileging experiencers rather than researchers as expert concerning their own experiences, which I saw as an expression of the humility that I had always thought should be at the heart of empirical enquiry.

Our primary research focus has concerned wellbeing. Mediums report experiences that in other circumstances could lead to a diagnosis of pathology—they see things that others cannot see, hear things that others cannot hear, have embodied experiences that they attribute to the influence of (discarnate) others—and yet they present as more psychologically healthy than spiritualists who don’t have these experiences, and in fact also better than norms for the general public (Roxburgh & Roe, 2011). Roxburgh and I explored this paradox in interviews with practitioners, asking them about their life history, their practices and the experiences they led to (Roxburgh & Roe, 2013, 2014). Many reported seeing apparitions in childhood, and the reaction of significant others to these was key in determining how the experiences were processed and assimilated. It was important for them to be part of a community that not only accepted their experiences but valued them. They needed to have an interpretative framework that made sense of the experiences, gave them a sense of order and—importantly—access to ways of working with spirit communicators that made their experiences manageable, and not simply an indicator of psychosis. Thus the voices they heard weren’t

unwanted intrusions but were collaborators who could be negotiated with and whose time could be rationed so that they were able to have uninterrupted time to do mundane things. Some of these concerns are nicely illustrated in Sarah's account:

The first memory that I actually have was hearing voices after my father died . . . one night I went to bed and I woke up and I'd had these voices talking to me saying that my dad was fine, he was living, there wasn't a problem, he wouldn't want me to be upset and I thought I was dreaming, so I thought "pull yourself together" and as I turned over to go back to sleep the voices were still there . . . so I thought "I'm losing it, I'll go down and make a cup of tea" [this was an English person, and so making a cup of tea is the solution for most things] so I went down and all the while I was making this cup of tea these voices were still talking to me . . . so I went to the Doctor's and I told him what had happened, I said "I must be having a nervous breakdown," so he gave me some pills, as they do, told me to go away for a few days and just try and chill and relax . . . never took the pills because I don't take tablets, I don't believe in that sort of thing . . . I thought "Right this is me and now I need to cure myself to get better" so I just pulled myself together, blocked absolutely everything out, thought I've just really got to get back on track and I did that probably for about 6-7 years. [But then . . .] I started to talk to . . . [a medium] . . . and we sat just chatting about things and that is when my interest started because they were explaining things to me because that was my first real knowledge that somebody was talking to me (. . .) Steph and her sister were starting to explain all these things to me and all the little things that had happened over the years which you just put down as "Oh that must be that and that must be that" so you know, things just started making sense.

I have found that concerns about pathologisation and ridicule are quite widespread. Its consequences were impressed upon me by one experience in supervising an undergraduate project some years ago. I was approached by a mature student who wanted to collect new cases of near-death experience. She interviewed one woman, "Emily," who described an NDE that occurred after complications following childbirth perhaps 15 years earlier. Her NDE contained classic features, including an out-of-body element, an encounter with a loving light, and meeting deceased relatives. After the interview, my student was able to explain that these were typical of many NDE cases, and the experient's relief in having her experience validated and normalised was palpable. She disclosed that although the experience had happened so long ago, she had not shared it with anyone—not her husband or other family members, and certainly not any medical staff—for fear that she would be "packed off to the looney bin." She felt like she had been carrying a burden that had finally been lifted from her. It was clear to me that parapsychology has an obligation to support people such as Emily so that they are reassured that many so-called paranormal experiences are not abnormal, but reflect the normal range of human experience.

Our work with mediums has been part of an ongoing initiative to strengthen links between researcher and practitioner communities. When we first approached the Spiritualist National Union there was an understandable suspicion that our intentions were to prove that they were at best misguided and at worst fraudulent. It has taken time to demonstrate our sincere intentions and to build trust. Our strategy has involved a two-way exchange, attending and running workshops that have enabled us to learn about mediumship development and practice, and for practising mediums to learn about the principles of the scientific method in ways they can use for self-reflection and further development. This collaboration advanced significantly in 2014 when we were donated space at Stansted Hall to convert into a permanent on-site research laboratory. The Stansted Hall estate was gifted to the Spiritualists' National Union by Arthur Findlay in 1954 with the intention that the buildings be used to establish a "College of Psychic Science." It was renamed The Arthur Findlay College and has established an international reputation for its educational programmes in mediumship practice and philosophy. But despite spiritualism being among the most evidence-based of the major religions, these programmes have not always sought to engender a scientific approach to the evaluation of empirical evidence gathered during demonstrations, and the establishment of the laboratory was intended to rectify that.

Refurbishing the lab space was made possible by generous support from the Society for Psychical

Research, Friends of Stansted Hall, and local spiritualist groups, showing that this is a genuine collaboration. These donations have enabled us to create a facility that will be made available to serious researchers to work closely with a broad spectrum of practising mediums who visit the AFC. The monetary investment is quite modest but hopefully will have a significant impact on the amount of serious research with mediums that will take place in the UK.

### **The Negative Impact of our Funding Model Upon the Way in Which Parapsychology Is Organised**

Speaking of investment, I'd like to acknowledge that all the research I've just described has been made possible by the generous support of a variety of funders, to whom I'm very grateful. But it's clear to me that the marginal status of parapsychology has made accessing funding much more difficult than for other, more mainstream topics, and I think this has had consequences for the kind of research that we see.

Firstly, because there is very little money to go around, relatively few people can be fully employed in research. The actual community of active researchers is remarkably small, so very little new research is conducted each year—barely enough to make publication in our journals as competitive as it should be. And those people who attract most of that funding tend to be innovators who have been successful in developing new protocols or adapting methods from other areas and in demonstrating “proof of principle” by reporting significant psi effects using such methods. We also have a number of “early adopters” who are quick to seize on new approaches and technologies and are responsible for the first wave of independent replications. However, relatively quickly the innovators lose interest in simple confirmations and move on to develop yet more methods and approaches, and the early adopters soon follow suit. I am sure that this pattern also occurs in other disciplines, but with their greater numbers they also include many able technicians who are willing to conduct modest replication extensions that allow them to do research in order to pay the mortgage. I suspect that parapsychology cannot afford many able technicians, so that interest in an original protocol (or effect) seems to wane as the caravan moves on. This gives parapsychology the appearance of a “butterfly science” that flits en masse from protocol to protocol as it falls in and out of “fashion” much as a butterfly flits from flower to flower (Roe, 2012). At best this is frustrating in diverting resources away from potentially fruitful avenues of research; at worst it looks suspicious to the outsider, who expects to see continuing and systematic work using a particular method for so long as it is productive, particularly when great claims were initially made for it. Why are there now so few micro-PK studies? So few ganzfeld studies? Has Hyman's prediction of an as yet undiscovered fatal flaw been fulfilled and hushed up? As a community we need to better coordinate our efforts to give rise to a more systematic and enduring programme of research, one that goes beyond proof of principle and the first wave of independent replications—the excellent Bem replications are a notable example of what can be achieved when critical mass is achieved (Bem, Tressoldi, Rabeyron, & Duggan, 2015).

Secondly, I think exaggerated competition for funding requires us to be creative in making the most of the limited resources available. As a senior academic in my university, I am simply too expensive these days to be employed directly in conducting research (the support of the Perrott-Warrick Fund is a notable exception). Instead, I have tried to invest the funds I have been awarded in young researchers. Professional development for those interested in a career in parapsychology is still extremely difficult in the UK, with very limited funds available to support one's study for advanced qualifications (MSc and PhD) that are necessary prerequisites to university employment. It seems inevitable that we are losing very talented individuals who simply cannot afford to opt for parapsychology when other branches of psychology can offer bursaries to defray living and research expenses. Employing a bright student to collaborate on a project, ostensibly as a research assistant but in practice as a co-experimenter who contributes to all stages of the research cycle from design to conference presentation and even journal publication, can be just the boost needed to maintain their commitment to parapsychology during times of adversity. Beneficiaries of this approach include people who have since received their doctorate, such as Nicola Holt, Sophie Drennan, and Glenn Hitchman, and people currently in PhD programmes such as Charmaine Sonnex, Callum Cooper, David Saunders, and Andrew Hodrien, names that may be familiar to you or will be in years to come.



I think that as a community we can do more to nurture young talent and give them the tools to become assimilated into the academy. Parapsychology will gain acceptance not by persuading established researchers to become involved in parapsychology, but by seeding the next generation with people who have direct experience of its rigour and caution, and are more open to consider the implications of its findings. That's why I think Bob Morris' strategy during his time as Koestler Professor has been so important for the field and why I think it is essential to continue it in his stead. When he was first appointed to the Edinburgh Chair, he confided to Jim Carpenter (2004, p. 425) that he intended to

take the long view with this post, investing the time and effort to build good relations with other academic disciplines, and developing a quality programme that could generate excellent scholars who would then go on to take academic posts at other universities, seeding the intellectual landscape of Britain and Europe with parapsychological experts in a way that had not yet proven possible in the US.

How successful was he in that endeavour? By my count he was able to mentor 26 graduate students to receive their PhDs, many of whom are still active in parapsychology, having been able to establish themselves in academia in university departments or private facilities (see Figure 1). Of these, 6 have supervised or are supervising their own PhD students, adding another generation to the "family tree." And among that generation 4 are themselves supervising PhD students, making 27 "grandchildren" and a further 9 "great grandchildren." By the standards of mainstream psychology this family tree is unexceptional, but in terms of ensuring the viability of parapsychology I think it makes a crucial contribution to the size and calibre of our research community around the UK. We need to find a way of replicating this in other countries.



Figure 1. Family tree of PhDs supervised by Bob Morris (representing three generations). Persons in bold have gone on to supervise doctoral students; italics indicate persons who have not yet graduated.

At a more local level, such mentoring can also contribute to the vibrancy of the research culture. I am a great believer in the importance of critical mass, having seen its consequences for myself. When I first joined the University of Northampton in 1995 I had colleagues who were supportive of my work but

knew little about it and were not interested in becoming more involved. As an isolated academic, progress was very slow. Things picked up when Christine Simmonds-Moore joined me to start her PhD under my supervision, and suddenly I had a knowledgeable person to bounce ideas with and hatch research plans; a task shared was a task halved. Slowly we were able to draw more people to us, so that today, Northampton has one of the largest research groups devoted to parapsychology and transpersonal psychology, and the diversity of interests and approaches ensures that each day with them is surprising and rewarding.

### Conclusion

I started this paper by wondering whether my experience as a parapsychologist might have any value for those who are in the early stages of their career. What advice would I offer to the young researcher? What misconceptions could I point out? When I was starting out I think I had an unrealistic expectation about the degree to which parapsychology could emulate practice in the natural sciences. I would advise you not to be dismayed if exactitude of findings and consistency of replication in your own work is not what you expected. Recognise that our object of study (human experience and behaviour) is inherently more complex than is allowed by that model, given that it includes a subject matter with thoughts and expectations of its own that can have a dramatic effect on the outcomes, and that are highly sensitive to cues we give as researchers through our interactions with them. Alongside the sophistication of the research designs you develop, keep in mind that the art in scientific practice depends on your interpersonal skill to enable your participants to feel sufficiently comfortable with you to behave naturalistically, and to accept at an emotional level the possibility of a psi effect. Don't be beguiled by cleverness in research design and recognise the importance of authenticity; ensure that your work connects with lived experience and that it can have impact for real people outside the academy, particularly in addressing experiencers' concerns about ridicule and pathologisation. This requires you to understand the phenomena as they occur in the real world and to privilege the experient's account, unsullied as it is by exposure to the blinkers of previous research and theory. Recognise that although it is important to contribute to our research base, it is essential to invest in human resources. Strive to build community, to feel a civic responsibility to support others and to acknowledge the value of critical mass in ensuring the long-term survival of our discipline.

Above all else, don't be worn down by criticism of yourself or the field. Appreciate that the antipathy towards parapsychology is at least partly driven by social and psychological forces that are unlikely to be mollified by reason and further evidence. We are collectively engaged in interrogating phenomena that have the potential to cast light on our deepest and most fundamental capacities as human beings. The insights we gain might represent the merest of glimpses but their implications can be genuinely profound.

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*The University of Northampton  
Park Campus  
Northampton NN2 7AL, UK  
Chris.roe@northampton.ac.uk*