

# GUEST EDITORIAL

## Technical Challenges for the Way Forward

BY EDWIN C. MAY

We are, in some sense, victims of our own success. There is now incontrovertible evidence for a statistically based information transfer anomaly we currently do not understand.<sup>1</sup> Not everyone in our field agrees with that statement 100%, but it does raise a very important technical definitional question. The reason that some informed skeptics do agree with it is that the statement does not say or even imply that anomalous cognition (AC)<sup>2</sup> exists. Rather, it simply claims that some people have the capacity to acquire information (i.e., cognition) in ways we do not currently understand (i.e., anomalously).

On the other hand, this statement does raise an important question: What is anomalous cognition? This is one of the technical challenges we face. At this point in our discipline, we do not have a positive definition of AC. Many of us have our own versions of a definition, and here is mine: *The acquisition, by mental means alone, of information that is blocked from the ordinary senses by shielding, distance, or time.*

### *Definitional Problems*

Essentially the definition above means that we define anything that happens when it shouldn't according to the known senses, as AC—a negative definition to be sure. Moreover, this definition has profound implications that are rarely discussed, at least directly, for experiments. It is rather straightforward and relatively inexpensive to design a protocol that meets the requirement in the above definition.<sup>3</sup> For example, the target material can be separated from the participant by hundreds or even thousands of kilometers, and the intended, randomly selected target can be generated in a blind fashion after the participant has completed her/his response—a precognition approach. This protocol will satisfy most of us as foolproof.

However, even here there is a significant problem that is lurking behind the scenes. It takes on many forms and is especially problematic for field research. That is, if we cannot think of a “normal” way to account for an observation, then it must have happened by “paranormal” means.

<sup>1</sup> In this editorial I will focus my remarks mostly upon the ESP part of psi and leave the discussions about the PK side to others.

<sup>2</sup> I will use the term anomalous cognition instead of the more familiar terms of ESP, remote viewing, and so forth, because in my view this term is a description of the observable and avoids an implication of mechanism.

<sup>3</sup> I will leave out of this discussion the issues of all types of fraud.

Unfortunately, this is more a comment about the researcher than it is about the phenomenon. I will illustrate the point where I personally had fallen into this seductive trap.

During the SRI International years of the psi project, I went undercover to a metal-bending workshop in South Lake Tahoe, California, that was organized and directed by a then-recognized name in metal-bending circles as a qualified practitioner. In advance of the workshop, I had gone to a local cutlery store and purchased a number of high quality soup spoons with shafts that were shaped in such a way as to make them especially difficult to bend. I was unable to do so mechanically beyond a slight curve. I asked an SRI technician to place a small surreptitious mark on the underside end of the handles so that I could be assured the spoons had not been replaced. Off I went to Lake Tahoe and arrived for a Friday evening session.

Friday evening passed, so did all day Saturday and Sunday, and there was no attempt to bend anything either by the group (about 10 people) or by the instructor. Rather, in my opinion, we were subjected to food, sleep, and logic deprivation. Each day's activity had hardly any food breaks and the sessions went far into the early mornings of the next day. The "instruction" included every possible new age idea, alien abductions, UFOs, and even weirder things. Remember that I am from Northern California, so for me to say that something is weird, it is *really* weird. A possible serious point here, however, is that all this activity put the participants in some kind of altered state and significantly depressed any tendency for logical analyses. Perhaps this is a requirement to allow PK to happen.

By 3 a.m. Monday, we finally began to bend forks and spoons. We were all seated on the floor with a large pile of cutlery that was easily reachable. By then, we had all been sitting for hours, and I was in a very strange state indeed. To begin with, my body had fallen "asleep" from the waist down and my knees ached from sitting cross-legged far too long. I was bending cutlery with no problem; I could weave the tines of forks as if they were made of soft candy. Somehow I had the presence of mind to pick up one of my special SRI soup spoons and hand it to the instructor. I told him that I could not bend it, and he said "no problem" and proceeded to rapidly wind the bowl around the handle in a tight knot. As a result, I was completely convinced of the reality of PK metal bending and that I had witnessed it first hand and up close. After all, I could not bend our spoons by any normal means that I was aware of.

Upon returning to SRI, I was telling the story to my colleagues, one of whom, as it turns out, was a metallurgist. He asked me to get one of the spoons and promptly wound the bowl around the handle by "normal" means, not PK. He taught me the simple secret behind how to accomplish this, and for years after I could be the hit of any party.<sup>4</sup>

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<sup>4</sup> There is a substantial literature on what is called "shock deformation of metal" online. Shock deformation is the key; you conduct a quick snap of the bowl of the spoon and con-

I am not accusing the instructor from the workshop of fraud; he too fell into the same trap as I. We could not think of a way in which the bending could happen by normal means and thus assumed it happened by PK.

This is just one example of the consequences of a negative definition. In the PK world the problem is especially acute. Professor Robert Morris often taught about things that look like psi but aren't. PK may be a solid exemplar of this idea.

I will illustrate this problem further with a single example from a piezoelectric strain gauge experiment we conducted at SRI during a two-year period. We arrived at this experiment after a careful examination of the macro-PK literature which convinced us that this would be the best candidate to research the properties of macro-PK. We spent over \$500,000 over two years on just this single experiment. Most of the money was spent on engineering design and labor costs. At the end of this effort, we did not see anything that would qualify as a PK effect, but we did see a host of things that could and did mimic PK.<sup>5</sup> For example, if a sensitive device is within a meter of a wall containing cables for the electric mains, then effects upon the device can easily result from very short power surges in the mains that radiate energy to the PK target device. Similar problems arise with acoustic pressure waves, even from a simple knock on a closed door, or micro-movements from the building shaking slightly, and so on.

One way to control for these things is to monitor potential normal effects directly, but then engineering costs grow rapidly, as they did in our experiment. Another way which is often done is to have randomly interspersed effort and control periods where it is assumed that external non-PK effects will appear on both conditions and cancel out in the analysis. But if the PK results require inferential statistics to demonstrate an effect, the alternative hypothesis to the cherished PK one is the potential of a psi-mediated experimenter effect (a.k.a., Decision Augmentation Theory).

Thus, until we can begin to define what anomalous cognition *is* rather than to provide a long list of things that it *is not*, and deal with the trap that we tend to ascribe to psi things we shouldn't, then our discipline has a fundamental structural problem that is difficult to resolve—at least in the PK domain.

### *When/Where/How Long Does Psi Happen?*

As I said above, we are victims of our own success. Except for instructional purposes, there simply is no reason to conduct exclusively evidentiary experiments again with the ganzfeld, remote viewing, or with

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tinue pushing with mild force. The experience is that the metal feels soft. However, if there is a moment's hesitation of this force, then the metal "freezes" and will bend no further.

<sup>5</sup> It is important to realize that one cannot prove the null hypothesis, so just because this study failed to show evidence of PK does not imply that PK does not exist.

random number generators. Rather we must conduct process-oriented studies to figure out how psi works.

But here is the major challenge for us all. At this time in our understanding of psi, we have no confidence in knowing when psi happens, where it happens (physiologically speaking), or how long psi lasts when it does happen. As experimentalists, these are fatal problems in moving forward. For example, consider a complex, carefully designed but hypothetical EEG experiment designed to look for central nervous systems correlates to psi. You ask the world's leading participant to take part in the study. But what you are asking of this poor participant is to accomplish something that is currently beyond his/her skill level; that is, be psychic on demand in the effort period but not in the controls.

A properly designed psychophysiological study with a psi component must include an independent (of the psychophysiology) channel to assure the experimenter that psi actually happened in the trial. However, the problem is, as our talented participants say, that all the psi that is going to happen may have happened outside the laboratory setting, for example, in the parking lot. If this is the case, then the EEG experiment is a complete waste of time and resources. This is an example of the "when" question in this section.

The "where" question is equally problematic. If we extend the "where" question beyond the participants' physiology to include the experimenter, the problem is obvious. On whom should we paste the electrodes? Our own research of skin conductance prestimulus response to startling acoustic stimuli illustrates the point. As we have noted in our publications and conference presentations, even our  $Z = 5$  result is most likely experimenter psi and not because the participant is responding in advance to a future surprise—our cherished hypothesis. Clearly, in this case the skin conductance electrodes should have been on the experimenter and not on the participant!

Fortunately, there are a number of ways in such experiments to sort out the issue of experimenter or participant precognitive effects from causal interaction effects. Except for those studies in which the participant initiates each trial—problematic in its own right on this question—few if any of the studies have incorporated a DAT protocol that could in principle address this question quantitatively.

How long does psi last? On the surface this sounds almost like a silly question. But if we are searching for correlates with either the autonomic nervous system or the central nervous system itself, it is a critical question that can drive what techniques should be used in such studies: fMRI studies, for example. That technique is based upon tracking hydrodynamic blood flow—an inherently unresponsive technique. For example, suppose a psi "insight" happens as a trigger lasting only 1/4th of a second. If that were true, fMRI studies would become a huge waste of resources. If it turns out that psi is by its very nature a fleeting

phenomenon, then most psychophysiology studies are nearly doomed at the onset.

What all this actually says is that unless we have some idea of the properties of psi itself, we are relying on luck to crack open this problem. It would be similar to understanding the thermal signatures of an ill medical patient by using a gamma-ray camera—clearly the wrong instrument for the job. At this juncture of our discipline, it is difficult to know what are the proper instruments for the job.

Since we do not know what the proper instruments are, we should look with as many of them as possible; however, that is extremely inefficient, but more to the point, it becomes prohibitively expensive.

### *Publications*

If we are in agreement that evidentiary experiments are no longer needed, then the next steps for research are by their very nature much more complex and challenging to an interdisciplinary field such as ours. As far as publications are concerned, I will use myself to illustrate two problems.

Over the life of the U.S. government's Star Gate program, we had the luxury of having sufficient resources to carry out rather complex process-oriented studies. I am now in a position to write up these studies for publication in the peer-reviewed journals of our discipline. There are two challenges I face in attempting to submit up to 50 or more papers. First of all, let me admit, I write in a stiff, exceptionally geeky style that is difficult to follow even by trained experts in an appropriate discipline. In jest—sort of—one of my dear colleagues wanted to know who was it that writes my papers for me, given that this colleague thinks of me as an outgoing, casual person who presents complex things in an amusing and understandable way to general-audience conference attendees. The good news for this problem is that it is, in principle, fixable with a change of my writing style.

The second challenge is more structural and is a general problem in interdisciplinary studies of any kind. It is difficult, if not impossible, for me as a physicist to understand a complex psychology model of personality and its relationship to psi. What is clear to the authors of such models, and perhaps to other psychologists as well, may be a total mystery to me. Some of the model diagrams look to me like Google directions gone haywire! The reverse is also true. My writing on entropy and its relationship to psi is absolutely clear to physicists but causes others' eyes to glaze over.

As our experiments become more sophisticated and complex, which they must do if we are to make progress in understanding the mechanisms of psi, so too will their descriptions. For Dick Bierman to adequately describe the results of his most clever fMRI studies, he should not be required to "popularize" them for our journals. The same idea holds for all of us conducting experiments on the edges of physics, neuroscience, and psychology.

One solution I am trying with our backlog of papers is to write an executive summary that contains popular descriptions of the experiments with take-home, nontechnical messages of the study outcomes and with an implication that the nontechnical reader can stop there. This is not easy, but perhaps it is one approach to a solution to the problem.

However, as our experiments become more technical and sophisticated, the tendency will be to attempt to publish the results in journals of the appropriate disciplines rather than in the psi journals. This, of course, is a double-edged blessing. On the one hand, it portends the end of our own journals, but on the other hand, it will move our discipline directly into the mainstream where, perhaps, it should reside anyway.

### *Grant Proposals*

Many of the challenges I outlined above for publishing carry over to our grant proposals. For the last several years, including 2010, I have served on the technical review committee for the BIAL Foundation—a major source of funding for our field. Over the last few biannual funding cycles, the BIAL Foundation has been accepting proposals for non-psi, mostly psychophysiology research. In the 10 or so proposals I review each cycle, about half are from the parapsychology research community and the other half mostly from mainstream scientists. I am terribly saddened to report that for the most part the psi proposals are far below standard and in some cases are absolutely dreadful. Part of the problem is that many of our psi research colleagues are simply not trained nor have experience in writing competitive proposals. This, of course, can be fixed with proper instructions and guidelines.

However, a deeper problem is revealed by the near amateurish ways in which the more complex studies are described, and when compared to the proposals from the “professionals” from the mainstream, it is very difficult for me to recommend that the BIAL Foundation provide much, if any, of their limited resources to my colleagues. Please understand that I am not saying that our psi research experimenters are incompetent. Rather, this is an understandable problem that is embedded within interdisciplinary research, especially when our colleagues do not have much access to researchers across these disciplinary boundaries, or to institutional resources.

There is a related problem that arises in grant writing which is more connected to the topic of psi rather than to some infrastructural problem. Let's face it; our discipline is primarily a hobby, at least in one respect. While there are a number of dedicated people in our field who have been more or less active in psi research for most of their careers, I may be the only one I know who has had a 20-year, full-time job at industrial-scale wages with full medical and retirement benefits and who has had no responsibilities in this job other than to study psi. I am not bragging; rather, I am illustrating

the problem. What young and talented graduate student would choose a full-time career in psi, where there are no jobs, no respect, and very little financial compensation?

As a result, some, if not many, of us are simply not trained in the necessary techniques in many disciplines to make progress. Those researchers who are good for such work choose “real” careers in the mainstream. Again, I will use myself to illustrate the point further. Ed May as a physicist, maybe even a talented one, should not be conducting psychophysiological experiments, developing complex psychological models of personality, or speculating upon matters of deep philosophy. But I have done them all, competent or not! I do not doubt that some of my written musings cause the skin to crawl of the researchers in these disciplines. But my skin crawls when I read the writing of my psychology colleagues about the quantum mechanical nature of consciousness or psi when they do not know even the basics of Quantum 101.

### *Discussion*

I confess that I am pessimistic for the future of our field. At the Winchester PA conference, I suggested that perhaps parapsychology should lie fallow for some period of time to allow younger, more flexible minds to grapple with the most fascinating problem of understanding psi. I wish I could conclude with some specific solutions to the challenges I have outlined here, but beyond some of the obvious ones of writing and such, I cannot offer any advice. The when/where/how-long questions above have me stumped. It seems to me that the researchers who will shine light on the answers to these questions will be mighty damn lucky indeed. But if we stand back and examine the history of science, we learn that luck has had a major role to play, so maybe that is not such a bad thing after all.

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