

The Bulletin of the
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8.1

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Edgar
Mitchell
Issue



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From the Editor's Desk

The Bulletin of the
Parapsychological
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| by ETZEL CARDEÑA,
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Plus ça change, plus
c'est la même chose.

While reading the wonderful book by Deborah Blum (2006) on early psychical research, *Ghost Hunters*, I was struck by the striking similarity between the rhetoric of the anti-psi dogmatics of yore and of the current ones, making me almost a believer in the notion of “the eternal return.” Here are some of the parallels I found. The first president of the American Society for Psychical Research, Simon Newcomb, dis-

missed the data and careful work by Barrett, Gurney, Nora Sidgwick, and others by stating that real scientists knew that telepathy was impossible. This reminds me of the use of the Perrot-Warrick award, whose objective is to further the study of psi phenomena, by Nicholas Humphrey, who proceeded to write an anti-parapsychology tractum (1995) that pretty much ignored all of the relevant evidence.

Then there were the vituperative dismissals of G. Stanley Hall, one of the founders of developmental psychology and a believer of racial eugenics and a strong separation of sex roles. He wrote that consideration of whether the amazing medium Mrs. Piper could communicate with spirits belonged “more to the troglodyte age than our own” and to test her “trance” he proceeded to dip a toxic substance into her mouth that produced blisters for days. He also screwed a weight against her arm, which became incapacitated for days after that pressure (in Blum, 2006, p. 303-304). Nowadays, Hall's example is alive and well in so many anti-psi statements that hide their ignorance of the evidence and lack

of arguments behind scorn and invective (see Cardeña, 2011).

The previously highly valued intellects of academics like William Barrett, William Crookes, and others were suddenly dismissed when they described evidence for psi phenomena, similarly to what has happened to Nobel prizewinner in physics Brian Josephson in our days (see Cardeña, 2015b). And the persecution against those involved in psychical research could be vicious. For instance, James McKeen Cattell, a pioneer in the study of mental tests and individual differences, and another proponent of eugenics, apparently tried to have the president of Columbia silenced and eventually dismiss James Hyslop for his research interest.

Some attacks were not only nasty but deceitful and cowardly as well. William B. Carpenter, who would later become the President of the British Association for the Advancement of Science, wrote an anonymous editorial raising false imputations against Crookes and his collaborator Varley (Blum, 2006, p. 333). This instance has a parallel in the “anonymous” committee that prevented a talk by

I also found out that “Randi’s Prize” was not the first offer of money for a “genuine” demonstration of psychic abilities. Around 1857 the Boston Courier newspaper offered 500 USCy for any medium who could produce psychic phenomena [...]

Rupert Sheldrake from being aired in its original TED slot (see see Cardeña, 2015b). Rupert has also been persecuted for his interest in psi by evolutionary biologist Jerry Coyne, as have I by a cabal of mostly hard-sciences professors at Lund (Cardeña, 2015; see also Hess, 1992).

There were then also the writings of “critical thinkers” such as Joseph Jastrow, who pointed out errors in reasoning concerning some beliefs, while not using the same criteria to their own prejudices (cf. Cardeña, 2014, b, for a recent example of the same process). I also found out that “Randi’s Prize” was not the first offer of money for a “genuine”

demonstration of psychic abilities. Around 1857 the *Boston Courier* newspaper offered 500 USCy for any medium who could produce psychic phenomena (Blum, 2006, p. 23).

To end on a positive note, it was a joy to revisit in *Ghost Hunters* the extraordinary brilliance and courage of the SPR founders, some of whom (e.g., Frank Podmore) started as debunkers but found out later that they could not explain all of their observations through fraud or conventional means. This is what more clearly distinguishes Mrs. Sidwick and other SPR pioneers, who considered all of the evidence and went where it took them, instead of a-priori assuming that they already had the final answer, as did the dogmatic pro-Spiritualists or anti-SPR people. This book has also made to me more endearing the figure of William James, a champion of free inquiry and someone who did not suffer fools or arrogants gladly. It is somewhat comforting to know that the battle for a non-dogmatic science and free inquiry has had such extraordinary champions, but this intellectual freedom requires eternal vigilance (cf. Cardeña, 2014a). As Rosenthal (1994) concluded, censoring research just because we disagree with its possible implications is not only bad science, but bad ethics.

This issue has a strong start in the Presidential Column of Chris Roe, in which he points out how the extant evidence does not support the position that psi research can be explained away by resorting to hypothetical experimenter fraud. In my personal musing in the last chapter of *Parapsychology* (Cardeña, 2015a) I declared that Douglas Stokes’s dismissal of lab psi results as due to experimenter fraud and the file-drawer phenomenon was unpersuasive given the tradition of null-results publication, the scarcity of research in the field, and the number of studies in the drawer that people in the field would need to be hiding. Since I mentioned already *Ghost Hunters*, going further back in history, it is worth mentioning another aspect in which parapsychology researchers have not been given nearly enough credit, namely the unveiling of fraudulent practitioners. Blum describes how Richard Hodgson, from the *SPR*, was a foremost unveiler of fraudulent psychic mediums (for instance of Madame Blavatsky and, perhaps inflexibly so, of all the manifestations of Eusapia Palladino; see also Gauld, 1968). It took the extraordinary communications, under also extraordinary experimental controls, of Mrs. Piper and

This issue is dedicated to Edgar Mitchell, an exceptional individual in so many ways. He was the sixth person to walk on the moon in the Apollo 14 mission and the winner of the Presidential Medal of Freedom, among various honors. [...]

other mediums to convince him and Mrs. Sidgwick that there was psi gold within the muddy dregs containing so much fool's gold.

In his column, Carlos Alvarado gives a lengthy account of the strong anti-psi bias in most traditional psychology history accounts, an area that has also been tackled recently by Andreas Sommer. Two contributions in this issue of *Mindfield* address the issue of potential non-fraudulent but nonconscious acquisition of information through sensory means. Diane Hennacy Powell describes some impressive-sounding tests with a 10-year-old autistic child, but Charley Tart, in comments originally written for an online discussion group, warns that having someone with a knowledge of the targets present in the room

makes the results suspect considering the possibility of conscious or unconscious cueing, something that parapsychology researchers have been aware of for a long time (e.g., James, 1899). Nonetheless, I think that it would be very useful to find out if skeptic researchers, particularly those with expertise on mentalism, can reproduce the preliminary demonstrations with the autistic child. I look forward to reading more about how Diane's research with children with autism progresses.

Bob Rosenthal, a psychological eminence for his many methodological, statistical, and content contributions to psychology, has been a fair and important supporter of psi research, and I am happy to include his *Reflections*. The column of the PA's former Student Rep, Erika A. Pratte, centers on a topic near and dear to me, Exceptional Experiences, and their clinical and scientific implications. Gerd Hövelmman provides as always his useful series of references in non-specialized journals.

This issue is dedicated to Edgar Mitchell, an exceptional individual in so many ways. He was the sixth person to walk on the moon in the Apollo 14 mission and the winner of the Presidential Medal of Freedom, among various honors. The epiphany of interconnectedness he encountered in deep space led him to found the Institute of Noetic Sciences in 1973, which is still going strong

and has supported the research on psi of Marilyn Schlitz and Dean Radin, among others. He was almost certainly the only person to carry an ESP test from space (Mitchell, 1971) and edited a book on related matters (Mitchell, 1974). I regret that I never met him in person but am glad that Marilyn Schlitz can tell us something about him.

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Mindfield has received the following:

Dowding (Lord), Hugh (2013). *The dark star*. Hove, UK: White Crow Books. The Royal Air Force Commander widely cited for the successful British air defense against Göring's Luftwaffe also wrote four books on the possibility of life after death, of which he was convinced. In this volume, he discusses spiritualist ideas about rebirth, astral life, and one that I particularly liked, the mistreatment of nonhuman animals. (Originally published 1951)

Dowding (Lord), Hugh (2015). *In God's magic*. Hove, UK: White Crow Books. In his final book, Lord Downing summarizes the evidence through mediumship of post-death survival, mostly of soldiers in the II WW, and what this told him about the fabric of reality. (Originally published 1960)

Grosso, Michael (2016). *The man who could fly: St. Joseph of Copertino and the mystery of levitation*. Lanham, MD: Rowman & Littlefield. Philosopher and parapsychology author (e.g., *Irreducible Mind*) Michael Grosso reflects on the evidence, provided by many witnesses during decades, that this ecstatic Catholic saint could indeed levitate, along with the implications of such acts for our understanding of the mind/physics relation.

Haraldsson, Erlendur, & Gissurason, L. R. (2015). *Indridi Indridason: The Icelandic physical medium*. Hove, UK: White Crow Books. If you have been impressed by the descriptions of the physical medium D. D. Home, prepare yourself to be at least equally impressed by the Icelandic medium Indridason, who at the beginning of the 20th century produced levitations, dematerializations, and other

outstanding phenomena while being thoroughly investigated by Icelandic scientists.

Poynton, John (2015). *Science, mysticism and psychical research: The revolutionary synthesis of Michael Whiteman*. Newcastle upon Tyne, UK: Cambridge Scholars Publishing. The current president of the SPR discusses the work of Michael Whiteman, erstwhile professor of Applied Mathematics, who crafted a synthesis of mathematical physics, psychical research, classical mystical texts, and his own mystical and other anomalous experiences.

Strieber, Whitley, & Kripal, J. J. (2016). *The supernatural: A new vision of the unexplained*. New York, NY: Tarcher Penguin. The most influential voice in the accounts of alien abduction experiences, Whitley Strieber, teams with the “mutant, academic superhero ;-)” Jeff Kripal, known for his innovative books on religion, popular culture, and paranormality, for a daring book “dialogue” about how the “supernatural” is the new “natural.”

And don't forget the 2016 PA Convention in Colorado. See the relevant information here:

http://www.parapsych.org/section/52/2016_convention.aspx

The Problem of Fraud in Parapsychology

"Oh, what a tangled web we weave, when first we practise to deceive."

Walter Scott,
Marmion (1808)



| by *CHRIS A. ROE*
University of Northampton

In my last article I noted how parapsychology is portrayed in mainstream academic textbooks as characterised by experimenter fraud, with the implication that many positive outcomes can be accounted for in terms of malpractice. For example, Gross (2010, p. 85) quotes Colman (1987) as describing the history of parapsychology as "disfigured by numerous cases of fraud involving some of the most 'highly respected scientists'". But such damning appraisals are not restricted to dyed-in-the-wool sceptics. Douglas Stokes (2015) has claimed that the body of evidence from parapsychological research "conform[s] to the

pattern that would be expected if a small minority of psi researchers has engaged in fraud" (p. 42), and for James Kennedy (2014) the situation is even worse: "Experimenter misconduct has occurred many times in parapsychology and is a constant threat. It detracts from the scientific acceptance of the field and hinders progress by diverting resources to

invalid hypotheses" (p. 9). In this article I'd like to take a closer look at how commonplace fraud has been in parapsychology and how this compares with other scientific disciplines so as to make a judgement about whether such claims have any substance.

Instances of Fraud in Parapsychology

To substantiate his assertion, Kennedy refers ominously but obtusely to 17 cases of fraud in parapsychology of which 15 are derived from Rhine's (1974a) paper, "Security Versus Deception in Parapsychology." In that article Rhine does indeed refer to "a dozen cases to illustrate fairly typically the problem of experimenter unreliability prevalent in the 1940s and 1950s" (i.e., in the wake of the popularisation of Rhine's monograph, *Extrasens-*

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sory Perception, which described research methods that had been developed at Duke University so as to allow replication attempts without the need for specialist equipment or extensive training in research methods). It seems as if the monograph's aim was successful, since these cases sound like reports of experiments received by the *Journal of Parapsychology* from people who were not academics ("seven did not have the doctorate") and not part of the parapsychological community ("several were persons of evident ability but were located (some of them abroad) where research in parapsychology was extremely hard to manage but not nearly so hard to fake"). There is no indication that any of these persons continued to conduct experiments nor that any of their work was published. It seems odd, then, for Kennedy to portray them as if they were typical or representative of the parapsychological community at large. Rhine does refer to four additional cases involving people who were "all better qualified for

psi research than ... the 'dozen'. They all knew the rules and standards that had been developed through the years" (p. 105), and these are of more concern. Unfortunately, all cases are described in general terms, with no information included that might allow those suspected of "experimenter unreliability" to be identified. Their non-adherence to generally accepted security standards is described, though it is not altogether clear that they constitute misconduct. For instance, the first example involves a comparison of performance by participants at psi and non-psi tasks in which two experimenters were responsible for different tests. Rather than ensure that the tests were scored independently while masked to the outcomes from other tests, the experimenters actually exchanged information when participants were scoring particularly well. This could certainly result in an expectancy bias when scoring performance that could inflate any correlation in scores between the tasks, but pales in comparison with the types of fraud discussed later in this article. Examples 2 and 3 present a stronger circumstantial case for experimenter fraud, in which effects only occurred when one experimenter had unsupervised access to raw data records. Importantly, colleagues raised suspicions about the work and none of it had been published when the researchers

withdrew. The final example Rhine includes is a study that adopts the Screened Touch Matching method described by Pratt and Woodruff on the grounds that it had been criticised by Mark Hansel as allowing for fraud by exploiting inadequate matching, and so is included because "trickery was a conceivable possibility" rather than because someone had been caught *in flagrante delicto*. Taken together, these 16 cases (accepting that Kennedy includes only the first 15) do not make a compelling case for Kennedy's assertion that experimenter misconduct is commonplace in parapsychology. He also raises concerns about other researchers in Rhine's laboratory, but these amount to little more than pernicious insinuation of the tittle-tattle variety, and I will not spend time on them here.

Of more concern are two particular cases to which Kennedy (2014) and Stokes (2015) refer that are generally accepted as involving calculated and systematic fraud by the experimenter. The first of these involves Walter J. Levy Jr., a medical school graduate who had joined J. B. Rhine's Institute for Parapsychology and had been so prolific and highly regarded that he had been appointed Director of the Institute; he was widely expected to succeed Rhine on the latter's imminent retirement (Stokes, 2015). Levy was particularly interested in animal psi and had designed

ingenious experiments that tested psi abilities in gerbils, rats, and chick embryos such that successful psi performance would meet the animals' basic needs (avoidance of pain, increase in experiences of pleasure, maintenance of optimal body temperature). One of the great strengths of Levy's experiments was their automaticity — once set up, the apparatus could run independently, monitoring the behaviour of random event generators (REGs) that the animals needed to influence in order to produce the desired outcome, and creating a physical recording of the outcomes for analysis. Three of Levy's research colleagues (Kennedy, Jim Davis, and Jerry Levin) became suspicious, then, of the amount of time Levy seemed to spend in the vicinity of the equipment while the experiments were in progress. They secretly wired up the computer so that it would produce a second record of REG output, and to their consternation discovered that the REG output was perfectly random while Levy's official record showed a deviation in the predicted direction (i.e., the rats were getting more stimulation of their neurological pleasure centres than would be expected by chance). Rhine was presented with the evidence and confronted Levy, who admitted having falsified the confirmatory study but insisted that his original research was sound and the

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data falsification had only begun when the genuine effect could not be repeated (Kennedy, 1975). This line of research had not yet been published (because of the need, in Rhine's view, for replication in order to confirm findings as evidential). Levy defended his other, published, work, pointing out that it had been independently replicated both at the Institute for Parapsychology and elsewhere. Nevertheless, in writing about the affair in the next issue of the *Journal of Parapsychology*, Rhine (1974b) sagely advised "although his single known violation involves only one of his many experimental lines, it unavoidably casts a reasonable doubt on all of his work individually and jointly conducted during the five years he has been in parapsychology" (p. 220). Rhine took comfort from the principle that, in the long run, independent replication would differentiate between sound and unsound findings. He immediately wrote to all those he believed were plan-

ning to refer to Levy's work in designing and writing up their own studies to inform them that the data were suspect, ensuring that this instance of fraud was dealt with swiftly and publicly. Fifteen months later Rhine (1975) gave an update which identified other suspicious practices by Levy and led him to conclude rather reluctantly that, despite double masked and multi-experimenter designs in some cases, no study completely eliminated the possibility of dishonesty, and so there was no option but to write it all off.

The second generally accepted case involves Samuel G. Soal, a mathematics lecturer at Queen Mary College (part of the University of London), who had been one of the principal exponents of forced choice ESP testing in the UK, but the consistent failure of those tests had led him to become among Rhine's severest critics (Beloff, 1993). That is, until (on the advice of Whately Carington) he reanalysed his data to look for displacement effects, instances in which the participant's call corresponded not with the target symbol but with the preceding or subsequent symbol. Two participants who showed evidence of these effects, Basil Shackleton and Gloria Stewart, were invited back for further tests, which resulted in a steady stream of significant results. Soal was a mathematician by profession but achieved his D.Sc. for psychical research. Al-

though his reputation remained intact during his lifetime, suspicions had been raised but these were quickly put down by threats of legal action (Beloff, 1993, p 147). In a detailed account, Markwick and West (unpub.) have described how one of the accusers was Mrs. Gretl Albert, one of the successful agents for Basil Shackleton. She confided to her friend Mollie Goldney (Soal's co-experimenter) that after the test she had seen him altering figures on a score sheet (the records were in the form of the numbers 1 to 5 rather than as symbols). Goldney asked to see the score sheets, but could find no signs of alteration. The nature of the alterations was not obvious from simple scrutiny with a magnifying glass; it required statistical analysis. Scott and Haskell (1974) speculated that if Soal was converting target 1s into 4s or 5s so that they matched participant calls, then there should be an excess of hits on trials where the target was apparently a 4 and 5 and a deficit of target 1s where the guess was 4 or 5. Both effects were confirmed so dramatically as to suggest that most target 1s that fell opposite a guess 4 or 5 had been altered to produce false hits. However, there was no overall deficit of target 1s or excess of target 4s and 5s, which Barrington (personal communication, cited in Markwick & West, unpub.) interpreted as suggesting that the target sheets had been prepared with

manipulation in mind by beginning with too many 1s and too few 4s or 5s. Scott and Haskell acknowledged that their findings could not explain the above chance scoring in most of Soal's sessions with Shackleton, but felt it was more likely that other falsification methods had been used than that results were a mixture of the genuine and the fraudulent. Evidence of those other methods came from Markwick (1978), who found that Soal's pre-prepared lists of random numbers (supposedly drawn from published sources) contained repetitions of sequences of up to 25 digits at a time, sometimes in reverse order. These repeated sequences contained an occasional extra digit, and 75% of these gave hits, which could suggest that placeholder digits (e.g., 1) had been entered with the intention of adjusting them later to match the call. Despite the general suspicions surrounding Soal's reported levels of success (according to Stokes, 2015, J. B. Rhine had long suspected that his research was fraudulent), other aspects of his behaviour are puzzling. Markwick and West (unpub., p. 176) conclude, "our revelation of his further deceptions may boost some sceptics' assumptions that all seemingly convincing claims for the paranormal must be fraudulent. That is not our view. Soal was exceptional in his secretiveness, his resistance to outside interference and his unwillingness

to have his subjects tested by other experimenters."

Cases of Fraud in Other Disciplines

So how does this portrait of fraud in parapsychology compare with other sciences? Rhine himself felt that parapsychology lagged behind: "most other branches of science have already matured to the point where the problem of experimenter trickery causes no great concern. This is partly because deliberate fraud would be too quickly spotted and exposed" (p. 112). This confidence in the checks and balances of mainstream scientific practice has been quite typical. Braud and Wade (1982, p. 11) reproduce testimony by Philip Handler, President of the National Academy of Sciences, before the US House Committee on Science and technology who described the problem of scientific fraud as "grossly exaggerated" by the press, and even when it does transpire "occurs within a system that operates in an effective democratic and self correcting mode". But that system has proven to be imperfect and the collection of cases of scientific misconduct has grown steadily, so much so that the National Science Foundation differentiates between three types: fabrication, falsification, and plagiarism (cf. Gross, 2016, p. 694). Plagiarism is the appropriation of another person's

ideas, processes, results, or words without giving appropriate credit. Falsification is manipulating research materials, equipment, or processes, or changing data or results. Fabrication is making up data or results. To illustrate these three types and to evidence their pervasive occurrence I shall briefly refer to major figures in the history of science who are now generally accepted as having engaged in fraud. This listing is far from exhaustive (for further examples, see Braud & Wade, 1982; Grant, 2007; Judson, 2004; Kohn, 1986).

Perhaps the earliest established case of plagiarism is that of Claudius Ptolemy, whose *Almagest* presents astronomical observations that provide the basis for a mathematical model to describe the movements of celestial bodies around the Earth, and was hugely influential until superseded by the heliocentric model proposed by Copernicus and others. Ptolemy claimed to have made these observations himself in Alexandria, Egypt. However, later back-calculations from the planets' current positions suggested that many of these observations were very poor even by the standard of the day and accorded much more closely with what Ptolemy's predecessor Hipparchus could have observed from Rhodes some 278 years earlier (Newton, 1977). Ptolemy's observations include curious omissions —of the 1,025 stars he documented, none are

from the five degrees band visible from Alexandria but invisible from Rhodes. As Grant (2007, p. 20) summarises, "rather than go out and make observations, it seems Ptolemy spent his time in the Library at Alexandria cribbing many of Hipparchus's results and claiming them as his own."

Isaac Newton may be the most eminent person to be accused of falsification. He was an irascible personality who regularly had spats with contemporaries. His greatest adversary was probably Leibniz, whose natural philosophy was at odds with Newton's theory of gravitation and laws of motion as outlined in his *Philosophiæ Naturalis Principia Mathematica*. Leibniz's influence in continental Europe meant that Newton's theory met with a lukewarm response there. In subsequent editions, Newton's case was strengthened by the adjustment of data, including his calculations of the velocity of sound, the precession of the equinoxes and the measurement of tides, so that they agreed precisely with his theory (Braud & Wade, 1982). Newton's reported measurements are given to six significant digits, a level of precision that is almost impossible even today (Westfall, 1973).

The Augustinian Friar, Gregor Mendel, is credited with making observations of the inheritance of characteristics across generations of pea plants in proportions that suggested a kind of discrete

transmission, so laying the foundation for a science of genetics. However, the proportions that Mendel reported fit so exactly with theoretical expectation that they drew the suspicion of R. A. Fisher, the eminent statistician who was responsible *inter alia* for the analysis of variance (ANOVA) test. Fisher closely examined Mendel's methods and data and found that the data were too good to be true, rather kindly concluding that Mendel's assistants may have adjusted figures in line with expectation. Others have suggested subconscious errors or selection (Judson, 2004, pp. 52-58), or less kindly suggested fudging (Anonymous, 1972).

Galileo Galilei is now thought to have fabricated the results of many of his experiments, which is somewhat ironic for the exemplary empiricist who privileged experimental observations over aesthetic or theoretical concerns. Tales of his testing the action of gravity by dropping objects from the Leaning Tower of Pisa are regarded as apocryphal but were invented later rather than by Galileo himself (Grant, 2007, p. 20). Other experiments that he claimed to have repeated "near a hundred times" with consistent results, could not have given that degree of homogeneity using the materials available at the time — as was found by contemporaries such as Père Mersenne who failed to replicate his findings (Braud & Wade, 1982, p. 26).

Fraud in Psychology

A high profile and extensive case of fraud from psychology involves Diederik Stapel. He was Professor of Social Psychology and Dean of the School of Social and Behavioural Sciences at Tilburg University. He had enjoyed a prolific career, receiving €2.2 million in grants from the Netherlands Organisation for Scientific Research (Enserink, 2012b), and producing 124 journal articles that had been cited a total of 1,756 times (Budd, 2013). One article, published in the flagship journal *Science*, reported an experiment conducted at the train station in Utrecht showing that a rubbish-filled environment tended to bring out racist tendencies in individuals (Stapel & Lindenberg, 2011). The cleaning staff had been on strike just before the summer vacation and this had provided an opportunity to compare responses from people who visited the station during the strike when the platform was unclean with those who were there once the cleaners had returned. He had speculated that expressed attitudes would be more stereotypical (“Brazilians are sexy, British people are polite, New Yorkers are pushy”) in the former, less orderly environment. Cleverly, he also designed a behavioural measure by inviting the predominantly white respondents to sit down on one of six seats laid out

in a row while they completed the measure, which happened to have a black person seated at one end, apparently already participating in the study. Stapel reported that participants did indeed express more stereotypical views in the untidy condition, and interestingly sat farther away from the already seated black person when their environment was messier, indirectly suggesting heightened racism. The problem for these elegant findings is that Stapel was so convinced that this phenomenon was a true property of the real world that he did not think it necessary to actually collect the data, which he instead invented at home so that they would give a cleaner result. He claimed that this was a consequence of his early experience with journal editors who found his real experimental data too complicated, with outcomes that were too messy, often asking him to leave out elements and make things simpler before they would publish — it was simpler to ensure the data were neat and consistent by making them up (Bhattacharjee, 2013).

Soon he was embroiled in an elaborate charade in which he would collaborate with research assistants or Ph. D. students on the development of research materials, such as questionnaires and bespoke equipment, but then (astonishingly) would insist on conducting the studies alone or taking the materials to contacts

of his in schools and colleges to administer. This allowed him to simulate the experiment at home to give a reasonable benchmark score and then create datasets around that figure that would give an unambiguous but believable confirmation of his hypothesis. He would bring the data sets or results of analyses back to colleagues and collaborate with them on the write-up. Stapel may have got away with his fabrications for so long because he ensured that his findings were in keeping with general expectations. “I didn’t do strange stuff, I never said let’s do an experiment to show that the earth is flat ... I always checked ... that the experiment was reasonable, that it followed from the research that had come before, that it was just this extra step that everybody was waiting for” (Bhattacharjee, 2013).

Ultimately his fraud was revealed when his collaborators asked about possible internal analyses (such as sex differences) that had not occurred to Stapel and so had not been concocted, or they asked for the raw data to conduct exploratory analyses but were told that they had been destroyed for lack of storage space. Suspensions were also raised by Stapel’s near faultless record of significance. One colleague was struck by how great the data looked, no matter what the experiment: “I don’t know that I ever saw that a study failed, which is

highly unusual ... Even the best people, in my experience, have studies that fail constantly. Usually, half don't work" (Bhattacharjee, 2013). An investigation by the universities that had employed him produced a final report that found Stapel had committed fraud in at least 55 of his papers, as well as in 10 Ph. D. dissertations written by his students (Levelt, 2012). By 2014, 58 of his published papers have been retracted.

Although Stapel may be labelled "perhaps the biggest con man in academic science" (Bhattacharjee, 2013), he is far from unique among psychologists: lack of space prevents me from discussing well documented cases involving Sir Cyril Burt, who invented data sets (and co-authors) to support the role of genetic inheritance on personality and intelligence (Tucker, 1997), Marc Hauser, who fabricated data and pressured his graduate students to reach his preferred conclusions (Wade, 2010), Dirk Smeesters, who had two high profile papers retracted when their data were found to be too good to be true (Enserink, 2012a), and Karen Ruggiero, who admitted to fabricating five experiments published in two articles and to doctoring research that appeared in a third (Price, 2010).

These detailed exposés can give the impression that they represent highly unusual aberrations perpetrated by individuals who can safely be regarded as

pathological (perhaps made so by stress or overwork). But we must remain wary that doing so could simply represent a defensive strategy that allows us to distance the miscreant from normal researchers and distinguishes their behaviour from normal practice. Braud and Wade (1982, p. 20) describe the genesis of fraud as often involving smaller steps, "those who falsify scientific data probably start and succeed with the much lesser crime of improving upon existing results. Minor and seemingly trivial instances of data manipulation — such as making results appear just a little crisper or more definitive than they really are, or selecting just the 'best' data for publication and ignoring those that don't fit the case — are probably far from unusual in science. But it is only a difference in degree between 'cooking' the data and inventing a whole experiment out of thin air". This resonates with Stapel's own account. In his biography, *Faking Science: A True Story of Academic Fraud*, he begins (p. iii): "I was doing fine, but then I became impatient, overambitious, reckless. I wanted to go faster and better and higher and smarter, all the time. I thought it would help if I just took this one tiny little shortcut, but then I found myself more and more often in completely the wrong lane, and in the end I wasn't even on the road at all. I left the road where I should have

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gone straight on, and made my own, spectacular, destructive, fatal accident."

Recent findings from psychology suggest that these more minor transgressions are relatively common. John, Loewenstein, and Prelec (2012) surveyed over 2,000 psychologists about their involvement in questionable research practices. Worryingly, they found that some practices occurred much more regularly, such as multiple analyses with selective reporting (78%) and optional stopping once significance has been reached (36%), but also practices that fall closer to the conscious fraud end of the continuum such as excluding data once their effects on the analysis are known (62%) and falsifying data (9%). Extending beyond psychology, Fanelli (2009) presented a meta-analysis of survey data on scientific misconduct that gave a pooled weighted mean of 1.97% who admitted to having ever fabricated, or falsified research data, but this figure rose to 14.12%

when asked if they had personal knowledge of a colleague who altered, fabricated or falsified research data, and increased further to 46.24% when misconduct was defined more comprehensively, for example as “experimental deficiencies, reporting deficiencies, misrepresentation of data, falsification of data” (p. 7). When the American Association for the Advancement of Science surveyed a random sample of its members, they found that 27% believed they had encountered or witnessed fabricated, falsified, or plagiarized research over the previous 10 years, with an average of 2.5 examples (Titus, Wells, & Rhoades 2008). Incidences are typically even higher when restricted to the biomedical sciences (Ranstam et al., 2000; Roberts & St John, 2014; Wells, 2008).

One indicator of fraud is the rate at which papers are subsequently retracted from journals. Steen, Casadevall, and Fang (2013) found 2,047 retracted articles indexed in PubMed, with the number having risen sharply in the last decade, both by reason of fraud and of “error” (which included plagiarism). This could not be attributed just to the increase in numbers of publications, but also represented a pro rata increase (though Gross, 2016, warns against over-interpreting these figures, since increased retraction rates might also reflect the greater attention being paid to malpractice or greater willingness to bring instances to the

attention of editors and publishers). Fang, Steen, and Casadevall (2012) followed up by consulting secondary sources to identify the reasons for retraction if none had been mentioned in the retraction notice (as was often the case). They found that 21.3% of retractions were attributable to error while 67.4% were attributable to misconduct, including fraud or suspected fraud (43.4%), duplicate publication (14.2%), and plagiarism (9.8%). It is not uncommon for retractions to be announced ambiguously and with little fanfare, so that they can go unnoticed. Retracted papers continue to be cited even after their retractions (Budd, 2013), including by 18% of the authors of retracted papers themselves, with less than 5% mentioning that the papers were retracted (Gross, 2016). Recently, internet sites such as Retraction Watch (<http://retractionwatch.com>) have been established with the goal of publicising unsound research. Their influence is difficult to gauge, but they may make it easier for dubious practices to be highlighted. Most detection of scientific misconduct is by laboratory colleagues of the transgressor, including their supervisors and students, who work at sufficiently close quarters to notice oddities in behaviour or data (Shamoo & Resnik, 2003). But the consequences for such whistleblowers can be severe. Lubalin, Ardini, and Matheson (1995) found that 47 of 68 complainants whom they surveyed suffered at least one negative consequence,

such as being pressured to withdraw their allegation, being ostracized by colleagues, or suffering a reduction in research support, and 56% believed that whistleblowing stigmatises the complainant. Retraction sites provide an opportunity for concerns to be raised anonymously, thus protecting the complainant, and can ensure that allegations about particular researchers are more widely discussed and acted upon rather than swept under the carpet. To illustrate, the blog site sciencefraud.org published 274 anonymous emails in the period July to December 2012 from bio-scientists claiming research misconduct in studies that had been published. In January 2013, legal threats forced the closure of this site, but a further 233 anonymous emails were submitted that could not be publicised. The fate of the papers referred to in these emails was analysed by Brookes (2014). He found no initial differences in the characteristics of the “public” and “private” cases, but the number of retractions was 650% higher for public cases, and the rate of publishing corrections or other errata were 770% higher. Overall, some kind of corrective action was taken on 23% of the publicly discussed papers, but this was only 3.1% for the unpublished papers.

Conclusions

In conclusion, then, although parapsychology does have two very well substantiated cases of fraud, they seem very typical

for the sector rather than signalling something distinct about the subject area (Levy is very reminiscent of frauds like the Cornell cancer researcher, Mark Spector, who was being groomed to take over as lab director, and Soal's case seems very similar to that of Cyril Burt). Given the recent revelations about misconduct across a range of disciplines that I have touched on here, the incidence of fraud in parapsychology does not seem to be very high when compared with the sector generally (though a per capita comparison could possibly suggest otherwise). It seems to me completely unwarranted, then, for commentators to imply that parapsychology has a particular problem with experimenter misconduct.

Indeed, there are grounds for thinking that parapsychology is actually much *less* susceptible to fraud than other research areas. Although there are as yet no systematic empirical studies of the characteristics of perpetrators of scientific misconduct, Gross (2016) has described the modal fraudulent scientist as “a bright and ambitious young man working in an elite institution in a rapidly moving and highly competitive branch of modern biology or medicine, where results have important theoretical, clinical, or financial implications. He has been mentored and supported by a senior and respected establishment figure who is often the co-author of many of his papers but may have not been closely involved in the research.”

And, based on their extensive primary research, Braud and Wade (1982, p 86) conclude that “the crime rate in science is influenced by three principal factors: the rewards, the perceived chances of getting caught, and the personal ethical standards of the scientist”. (To this I would add the expected consequences of getting caught, since there are a number of instances in which those who have admitted misconduct have been allowed by an institution to leave discreetly so as to not tarnish their reputation or embroil them in lengthy and costly legal and administrative proceedings). In other words, fraud will be more common where it is likely to be lucrative, where one's research can pass relatively unscrutinised (or even unnoticed), and where if discovered one's actions are more likely to be dealt with quietly rather than publicly. I would argue that none of these conditions pertain to parapsychology, which is highly under-resourced, is subject to very high levels of scrutiny whenever findings are positive, and which has a track record of public exposure when fraud is discovered.

This is not grounds to be complacent. Fraud is very difficult to detect without access to raw data. Kennedy (2014) notes that those perpetrating fraud are often reluctant to share raw data for reanalysis (which certainly was the case with Soal), and this may be more so now that the stochastic properties of “real” data are

better understood (for example Benford's Law, which describes the frequency distribution of leading digits in many naturally occurring data sets — see Benford, 1938, Miller, 2016). The case seems to me compelling for the establishment of a data repository so that the evidence on which research claims are made can be scrutinised by anyone who has an interest to, and this seems a natural extension to the design registries and repositories for unpublished research that are already available or are being developed.

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1930–2016

Obituary for Edgar Mitchell

A Final Farewell

Captain Edgar Mitchell was a hero, stepping up to challenges throughout his life with courage and tenacity. Born in Texas on September 17, 1930, Mitchell embraced a changing world at every moment. He often remarked that his grandparents had traveled across the country in a covered wagon and he went to the moon. Anything is possible.

During his illustrious career, Mitchell was an American naval officer and aviator, test pilot, aeronautical engineer, noetic scientist, and NASA astronaut. He was a test pilot during the Korean War, taking off and landing on aircraft carriers. It was, according to Mitchell, like finding a needle in a haystack. He served as the Lunar Module Pilot of Apollo 14. This made him the sixth man to walk on the moon. Mitchell explained to me with dry humor that compared to landing on aircraft carriers in the midst of raging seas and a dramatic war, landing on the moon was a piece of cake.

For Mitchell, it was the journey home from the moon that opened

him to a great epiphany that changed his life. As an engineer, Mitchell trusted the Newtonian paradigm and its emphasis on the physical world. But as he was hurtling through space, watching the sun, the moon, and the earth rising and setting in the vastness of space, he realized that the greatest frontier was not outer space, but the inner space of consciousness. As he explained during an interview for our book (Schlitz, Vieten, & Amorok, 2007):

I realized that the molecules of my body had been created or prototyped in an ancient generation of stars—along with the molecules of the spacecraft and my partners and everything else we could see including the Earth in front of us. Suddenly, it was all very personal. Those were my molecules. (pp. 46-47)

Mitchell described it as an experience of connectedness, bliss and ecstasy. He felt overwhelmed with joy. He realized that our scientific worldview was “incomplete and likely flawed.” Life was suddenly

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different for the visionary, who found himself in search of harmony and love. He founded the Institute of Noetic Sciences in 1973, in order to bring the rigor of science to the mystery of consciousness and our fundamental interconnectedness.

Years later, I again interviewed Mitchell for my book and documentary film, *Death Makes Life Possible*. He explained that questions of death and a possible afterlife are fundamental to our understandings of reality. And yet, he maintained an open mind. With new data coming from sources like the Hubble telescope, Mitchell shared that we are coming to a whole new understanding of the universe and what life is all about in the broadest sense. As a post materialist scientist, he looked to quantum physics and holography to help explain concepts as enigmatic as reincarnation. Equating quantum holography with the ancient idea of Akashic Records,



Mitchell argued, “nature doesn’t lose its experience.”

Pondering his own immortality, Mitchell shared with me (Schlitz, 2015, pp. 195-197) his worldview about death. Rather than focusing on an afterlife, he saw the need to address our lived experiences. In his words:

I think the more important thing for we humans is to learn to feel pleasurable, happy, successful in what we do in this life, and feel that we’re being productive, caring and helpful to each other and to our families. That’s really more important than whether we have all the answers to what happens after this life. Living

this life to the fullest and properly and happily, to me, is far more fundamental.

I am grateful to have known Edgar Mitchell. He was a mentor, a colleague, and a beloved friend. He never failed to ask after my son, referring to him as his “little buddy.” I recall walking with him, my son, and my parents under the unused Apollo capsule at the Apollo Space Museum. Seeing this gigantic rocket brought home to me the man’s enormous courage as well as his qualities of caring for others. I will miss our extraordinary conversations about topics that fall far outside the mainstream. But I can say

with certainty that he lived his life to the fullest — properly and happily. He died on February 4, 2016, in West Palm Beach, Florida at the age of 85. May Mitchell’s spirit live on through the quantum realms of entanglement and that deep interconnectedness that defined his worldview and imbued his personal philosophy.

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Parapsychology and the Study of the MIND

Changing the Historical Record

In a recent article in *Mindfield*, Chris Roe (2016) stated: “A powerful means of imposing scientific dogma is through textbooks, which do not passively and transparently describe a discipline, but instead actively circumscribe it. By the presence or absence of topics and by the way they are represented, the authors determine for the reader the boundaries of legitimate concern and appropriate practice. In this way the boundaries are policed and transmitted from generation to generation” (p. 86). I believe this has affected negatively views of the historical role of parapsychology in relation to psychology and psychiatry, as seen in the traditional historiography of these fields. In the rest of this paper I will discuss this issue, focusing, to a great extent, on some of the articles I have published during the



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last 15 years or so (with apologies for presenting various quotes).

Unfortunately many historians have contributed to perpetuate the view that psychical research was not important to psychology or to psychiatry. An early example was Edwin G. Boring’s (1886–1968) highly influential *A History of Experimental Psychology*, a book that influenced most of our older teachers of psychology and

that was a standard textbook for many years (Boring, 1957). In this book psychical research was considered to be at the periphery of psychology, and it was only mentioned in the book in notes at the end of a chapter (p. 502). The lack of importance of psychical research is also assumed by many other writers who do not even mention psychical research in their writings (e.g., Robinson, 1995), or who mention it in passing as someone’s interest with no details (e.g., Hergenhahn & Henley, 2013, p. 332), underscoring Roe’s comment.

Fortunately there are indications in the last decades that the situation is changing. Perhaps this is related to the attention historians of science and medicine have paid to “marginal” disciplines and movements, some of whom argued that these movements, and the ideas that came from them, contributed to science and to

Fortunately there are indications in the last decades that the situation is changing.

culture at large (see entries about alchemy, astrology, Hermeticism, mesmerism, occult sciences, Spiritualism, and phrenology in Hessenbruch, 2000). Although not all historians agree, many oppose the view that occult and mystical views were a factor that always hindered the development of science. In fact, the opposite has been argued, considering such topics as contributing factors to the development of science (see Applebaum 2005 for an overview).

An important early work bringing such a perspective to psychic phenomena was *The Discovery of the Unconscious* (1970) by Henri F. Ellenberger (1905-1993). Although the emphasis of the book was on the more conventional work of individuals such as Sigmund Freud (1856-1939), Pierre Janet (1859-1947), and Carl G. Jung (1875-1961), which led to the development of ideas about the unconscious mind and psychotherapy, Ellenberger gave a place to ideas coming from mesmerism, psychical research, and Spiritism affecting the study of the mind. Not only did he acknowledge the work of Frederic W. H. Myers

(1843-1901), but he wrote: "Automatic writing, ...was taken over by scientists as a method of exploring the unconscious A new subject, the medium, became available for experimental psychological investigations, out of which evolved a new model of the human mind" (Ellenberger, 1970, p. 85).

Later writers have argued for the importance of the study of psychic phenomena for the development of ideas about non-conscious activities of the mind, thus placing psychical research as a positive influence, not as a mere obstacle in the development of psychology as a science, or as an absurd field. Examples include Adam Crabtree's *From Mesmer to Freud* (1993), Régine Plas's *Naissance d'une Science Humaine* (2000), and Eugene Taylor's *William James on Consciousness Beyond the Margin* (1996), among others (see also Andreas Sommer's forthcoming book).

In her book, Plas (2000) resists the image of psychic studies as an "infantile malady" or as an "amusing bizarreness" of some psychologists (p. 13). Interest in the "marvelous" (including psychic phenomena) shown by psychologists is presented by Plas as an influential force in French psychological studies, particularly in terms of the development of ideas about the unconscious mind.

Of course we have to acknowledge that not everyone accepts this view. But it is encouraging to

see the above mentioned publications, and the fact that some mainstream historians argue that it would be a mistake to exclude psychic phenomena and other "marginal" topics from the canon, and that they "contributed mightily to the constitution of modern psychological medicine" (Micale, 2004, p. 11).

In my own work, consisting of various articles, I have tried to provide information about some of these issues, hoping to influence psychologists and psychiatrists. I *do not* write to defend the existence of psychic phenomena, nor the validity of their research findings, my intention is rather to present psychical research as an agent of influence, of change, just as so many have written about the role of fields such as neurology or concepts such as materialism, on ideas about the mind. The way I see it, the more practitioners and researchers in psychology and psychiatry see papers about psychical research in their journals about issues of historical relevance, the more they will get used to the new way of seeing these topics as part of the histories of psychology and psychiatry. In any case, at least they will be exposed to the topic, and to arguments defending the idea that psychical research is not an example of a peripheral or a useless pseudo-science.

With this purpose in mind in recent years I have published

several papers in the Sage journal *History of Psychiatry*. These are contributions to a section of the journal called “Classic Text” devoted to presenting excerpts or whole articles or chapters relevant, in a broad way, to the history of psychiatry (Alvarado, 2010, 2014a, 2016; Alvarado & Zingrone, 2012). The reprinted text is presented with an introduction that provides contextual, biographical, and other information that justifies the inclusion of such material in the journal. This is not limited to mental illness, but includes much more, such as general psychological topics, and topics of general cultural and social concern believed to be relevant to the study of the mind and behavior. The journal, edited by historian of psychiatry German Berrios, is also open to psychic phenomena. I have never noticed any prejudice against the topic, as judged by my submissions, which to this day have all been accepted. I have presented much information about psychical research in these contributions.

The point of some of my papers has been to identify the psychical research writings of well-known psychologists (e.g., Alvarado, 2009; Alvarado, Maraldi, Machado, & Zangari, 2014). In one of the articles three colleagues and I discussed the work of Swiss psychologist Théodore Flournoy (1854-1920), which included his study of the medium Hélène

Smith, as reported in his famous book *Des Indes à la Planète Mars* (Flournoy, 1900). My colleagues and I wrote:

His main contribution, both to psychology and to psychical research, was conceptual, and referred to the development of the concept of the capabilities of the unseen mind. This he did mainly through his study of Smith’s mediumship ..., but also with a few other case studies ... His contribution of the construction of this idea, while purely psychological, was developed and nurtured in the context of psychic investigations, as were the psychological ideas of Myers, and to some extent, those of others such as Janet and Richet ... From the early days after the publication of *Des Indes* to more recent developments, Flournoy’s investigation of the Smith case has been cited by many to illustrate the capabilities of the subconscious mind for producing fictitious manifestations. It is an example of the vast influence that exemplary cases can have on the development of ideas and research, as seen both in psychology and in psychical research (Alvarado, Maraldi, Machado, & Zangari, 2014, pp. 162-163). ”

Another example is William James (1842-1910; Alvarado, 2016; Alvarado & Krippner, 2010),

who of course has been widely discussed by others. A colleague and I discussed William James as another example of how psychical research contributed to the study of dissociation (Alvarado & Krippner, 2010), with some studies accepting the existence of the supernormal. “Unlike Janet and others, James did not use dissociation to explain mediumship and other phenomena in the sense of reducing everything to suggestion and the workings of a secondary consciousness. Instead he adapted ideas, such as Myers’, that assumed the existence of a secondary consciousness and that were not only relevant to pathology, but to the supernormal and the transcendental. James’ acceptance of the supernormal in the case of Mrs. Piper represents a break with Janet and other conventional explorers of dissociation. It was in fact a plea to study and accept the possibility that dissociation and consciousness in general could transcend bodily limitations” (p. 37).

In my first paper exploring the contributions of psychical research to psychology I focused on the work that early members of the Society for Psychical Research conducted regarding dissociation (Alvarado, 2002). Because I wanted to inform contemporary dissociation researchers, I sent the paper to the *Journal of Trau-*

Mediums, and others such as the hypnotized, “became part of a small group of special individuals who led students of the mind to see invisible regions of the psyche.

ma and Dissociation. In the paper I focused on work about mediumship and hypnosis, and summarized aspects of Myers’s contributions. I concluded that “it is far too simplistic in historical terms to dismiss psychical research as pseudoscientific or as an example of irrational or plainly wrong ideas that have been superseded as psychiatry and psychology have advanced and have become more scientific. Apart from the fact that psychical research deserves serious consideration, we need to realize that in the context of nineteenth-century developments this field made important contributions to the study of dissociation and to the development of the idea of a secondary self... Such considerations remind us that much of our current understanding of the history of dissociation has been itself ‘dissociated’ in the sense of becoming separated from aspects of its origins” (p. 28).

I continued to explore disso-

ciation in other articles. In one I focused on French psychologist Alfred Binet (1857-1911) and his discussion of mediumship to illustrate that, similarly to hypnosis and various cases apparently showing the existence of a secondary consciousness, this phenomenon was used in the psychological discourse of the nineteenth-century to argue for the existence of dissociation as a psychological process (Alvarado, 2010).

Mediumship, I wrote with other colleagues in an essay published in the Brazilian psychiatry journal *Revista de Psiquiatria Clínica*, provided the context for the development of various ideas about the subconscious mind (Alvarado, Machado, Zangari, & Zingrone, 2007). Mediums, and others such as the hypnotized, “became part of a small group of special individuals who led students of the mind to see invisible regions of the psyche. This... had implications for dissociation and for diagnostic matters” (p. 50). An example was the work of Pierre Janet, who did not accept the parapsychological aspects of mediumship, but used the phenomena (and the writings of Myers) to support the concept of dissociation and secondary personalities.

In later papers published in *History of Psychiatry*, I and other colleagues discussed pathological diagnoses informed by mediumship (Alvarado & Zingrone, 2012;

Le Maléfan, Evrard, & Alvarado, 2013). Interestingly, and complicating the issue, there were also several formulations of the relation between dissociation, the subconscious mind, and mediumship, as discussed in another paper in the *Journal of Parapsychology* (Alvarado, 2014b). I wrote in the conclusion of this paper: “Although most medical men held a closed model of the mind (and of dissociation) in which the phenomena were explained mostly by internal resources and a few external influences such as suggestion, few accepted a more open model of mind, such as the one some psychical researchers upheld based on powers that extend sensory and motor capacities beyond the confines of the body. Nonetheless, and as seen in the writings of some such as James... these psychic or supernormal concepts were part of the same general interest in understanding the mind and its myriad of layers as the more accepted ideas of individuals such as Janet... Interestingly, these ideas about the powers or capabilities of the subconscious mind were also connected in some cases to pathology. This was not only the case with those, like Janet... [who] reduced everything to intrapsychic concepts, but also with those like Lombroso... and Morselli... who admitted the existence of the supernormal as a process related to pathologies such as hysteria. But most of the persons discussed

here did not write about pathology” (p. 108).

Together with the authors mentioned above, I have been arguing for a more complete history of psychology and psychiatry. That is, one that represents better the past by recovering from the historical record research and ideas that have been neglected by many representatives of the traditional historiography of these fields. This includes other phenomena and issues not emphasized here, such as the study of hallucinations, hypnosis, eyewitness testimony, institutional developments, and other things. Although we should not forget that the past of these disciplines was influenced by multiple aspects and not only by psychical research, interest in the psychic or supernormal was a factor affecting positively some past inquiries about the mind.

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Robert Rosenthal

This issue's *Reflections* come in the guise of an interview because I had the pleasure of chatting with and interviewing Robert Rosenthal over the phone. Bob, formerly a professor at Harvard and now at the University of California, Riverside, calls himself an “outsider” to parapsychology, but he has been a very eminent defender of the field against unjustified attacks. His CV (a slightly dated one can be found at <http://rosenthal.socialpsychology.org/cv/Rosenthal.pdf>) gives but a small inkling of his importance to psychology (and parapsychology). One of his major contributions was to show that the attributes and expectations of experimenters (and teachers) have a major influence in the outcome of their studies (and students' performance; also known as the *Pygmalion effect*, e.g., Rosenthal & Jacobson, 1968; Rosenthal & Rubin, 1978). Another has been the development of sophisticated statis-

tical techniques that have informed the field for decades (e.g., Rosenthal & Rosnow, 2008). A laudable aspect of his work is that he does not lose track of the general context of science, discussing ethical concerns in the midst of papers on methods and statistics, always written in a graceful prose (e.g., Rosenthal, 1994).

Bob, how did your interest in parapsychology start?

I started doing experiments when I was 14 or 15, at Newtown High School, in the Queens borough of NY. I had a set of Zener cards that Joseph Banks Rhine sent me after I wrote to him following my reading of his *New Frontiers of the Mind*. I became the president of the biology club in high school, and one of the people I tested back then was none other than Russell Targ, who was thus introduced to psi research by me. Although I did not conduct



Robert Rosenthal

statistical analyses on what I did back then, I was impressed by the rigorous methodological and data analytic approaches in an area that studied such phenomena as, for instance, a person mentioning a dream of her son unexpectedly dying precisely at the time of the dream. The probability of these occurrences in everyday life cannot really be ascertained.

How about your contributions to parapsychology?

Although I consider myself an *outsider*, I have been an advocate against those people who reject psi research just because they consider it impossible. I have conducted analyses of the evidence for psi, and used a technique developed by my collaborator Donald Rubin to compare with the same metric psi research in which the number of decoys and target vary, for instance 1 out of 3, 1 out of 4, and so on (Rosenthal, 1986; Rosenthal & Rubin, 1989). I was also tasked to write a review of 5 areas of potential human enhancement, including psi, by the National Research Council (NRC). I found research on parapsychology to be the most rigorously conducted of the techniques evaluated, but was pressured to withdraw our (Monica J. Harris and Robert Rosenthal) positive evaluation of psi so that our analysis of the other areas would be published in a book. We refused to do so (our full report is available at <http://www.nap.edu/read/779/chapter/1#ii>) and I wrote a letter in *Psychological Science* (Rosenthal, 1990) referring to this issue and stating that “we found the typical methodological quality of the Ganzfeld experiments to be superior to the typical quality of the four other areas we considered... and with an average effect size equivalent to the typical effects

[...] the field should continue developing greater precision in the measurement of potential psi phenomena.

found in biofeedback research” (p. 329; Ed.’s note, for the whole sordid history behind the banning of positive reports on psi and the obvious bias in the final NRC report, which had detrimental consequences to psi research, see Palmer, Honorton, & Utts, 1989; see also Cardeña, 2015, for more recent examples of scientific censorship). William Estes, the founding editor of *Psychological Science* who published that letter, was one of the most influential psychologists of the 20th century and while on the faculty at Harvard lived in the same house in which William James had lived.

What are your suggestions for further developments in the field?

You mentioned that you have used in your ganzfeld studies psi z scores (in Marcusson-Clavertz & Cardeña, 2011, after Stanford & Sargent, 1984) instead of binomial distributions, and generally I think that the field should continue developing greater precision in the measurement of potential psi phenomena.

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Student Corner

The terms exceptional experience (EE) and exceptional human experience (regarding an exceptional experience that is also transformative) were coined in 1990 by Rhea White. These terms delineated subjectively anomalous experiences as a class of their own, to be considered within the context in which they occurred and pragmatically. White felt that this demarcation was necessary, particularly because she found that “the behaviorist type of science that was privileged by academic parapsychology” (White, 1999) did not lead to a personal understanding of one’s anomalous experience nor did anything for the person who experienced an EE except often give him a “negative story line” (White, 1994). Proving or disproving an experience is not the point of studying an EE; what it does for the person who experiences one is. However, the study of EE is still an analytical one:

In our approach, we start with the experience, including any predisposing factors and triggers. Then we take a close look not only at the objectively verifiable components and the anomalous ones, but also the physical, physiological, feeling, psychological, and spiritual components. Because of their importance, our main concern has become the aftereffects. If an experience does not have any lasting effect on the experiencer, it remains simply an anomaly, and so can be viewed objectively as a one-time happening, now finished. However, some anomalous experiences become personalized. They become part of the experiencer’s life. They have become exceptional experiences (EEs). (White, 1994)

From here, we see a launching point for the interest in clinical, counseling, and therapeutic approaches to exceptional experiences. More and more publications are being made available in



| by ERIKA A. PRATTE

these areas such as *Exceptional Experience and Health* (Simmonds-Moore, 2012), *Perspectives of Clinical Parapsychology* (Kramer, Bauer, & Hovelmann, 2012), *Varieties of Anomalous Experience* (Cardena, Lynn, & Krippner, 2014), and the *Journal of Exceptional Experiences and Psychology*. The 2015 Parapsychological Association Book Award winner *Para-*

psychology: A Handbook for the 21st Century (Cardeña, Palmer, & Marcusson-Clavertz, 2015) has a chapter entitled, “Exceptional Experiences (ExE) in Clinical Psychology,” written by Martina Belz and Wolfgang Fach. We have also seen a rising of professional research and consultation groups such as the Centre d’Information, de Recherche et de Consultation sur les Expériences Exceptionnelles (CIRCEE), the Institut für Grenzgebiete der Psychologie und Psychohygiene (IGPP), the Parapsychologische Beratungsstelle, and the Instituto de Psicología Paranormal.

In many cases, we see how instantly transformative an EE can be and in surprising ways. For example, in her article “Exceptional Human Experience and the More We Are,” White (1994) states:

I met a woman recently who was in an abusive marriage from which she felt she could not extricate herself because her husband was a violent man who threatened to kill her if she left. She did not doubt that he would. Then she was involved in an automobile accident in which she had an NDE, which left her with the certain knowledge that death is an impossibility. She immediately incorporated this into the story of her life and of human nature. The first thing she did when she returned home

was tell her husband she was leaving. She was even able to laugh at his threats, because of her experience of the ultimate nonreality of death.

White (1994) continues by perfectly explaining why empirical truth is not the focus in exceptional experiences, but personal truth is:

Now you can certainly interpret this story in different ways, several being what I have called negative story lines. You could say the teaching of her NDE was a delusion engineered by various physiological secretions that occur in a life-threatening situation. I say it doesn’t matter what engineered the experience. It is the knowledge she gained that counts. You could say the basis of that knowledge was illusory, yet because she was able to believe it, it altered her attitude, and her new attitude of fearlessness was sensed by her husband, whose violence was depotentiated by it. I say it doesn’t matter how you rationally try to explain it. Here we have an actual life situation. The reality is that the NDE enabled this woman to change her story, and when she did, she was able to change; her life.

The study of exceptional experiences crosses over into many areas, not just parapsychology

or mental health. Creativity is a major theme found amongst those who have had EEs and artwork based on these experiences can be discovered and shared more easily now than ever thanks to the Internet. Archival projects to preserve parapsychological research often include artifacts of people’s exceptional experiences such as art or personal accounts (e.g., the Het Johan Borgman Fonds, IGPP, and William Roll Collection at the University of West Georgia). The *Journal of Exceptional Experiences and Psychology* also publishes personal accounts and creative pieces. Furthermore, exceptional experiences are germane to other erudite areas that are also invested in a person’s lived experiences such as philosophy, anthropology, and various disciplines of psychology (existential psychology, transpersonal psychology, etc.).

The term *exceptional experience* may have come about in the 90s, but truly the pioneers of psychical research at the latter part of the 19th century were heavily invested in studying subjectively anomalous experiences, William James being a prime example. Much of James’s pursuits in psychical research were focused on what and how people experienced anomalous (often referred by him as “mystical”) experiences at the subjective level and the personal, individual truths that a person gained from these experiences.

White (1994) even draws on James' research in her own:

From my study of exceptional human experiences I would say that in such moments, in one way or another, we experience a sense of connection that is accompanied by awe, wonder, surprise, and delight. This is what makes such experiences "exceptional." Perhaps the biggest surprise of all is that there is a quality about these moments with which we cannot help but identify. It is as if a self we experienced as disconnected, small, and unimportant looked in the mirror and saw a radiant being looking back—one who is spontaneously connected to everyone and everything else, which means that automatically the bonds of selfishness, fear, anxiety, greed, envy, and a host of other negative emotions effortlessly fall away. Perhaps most surprising is that, as James points out, with this new self one can identify with what formerly was perceived as being outside. The sense of self is no longer centered on the "me" and is perceived more as a process than as a separate entity. One becomes centered in an interchange between inner and outer that involves one's fullest self and yet seems to be composed of everyone and everything else.

James and the other pioneers were also heavily invested in field research; their curiosity took them to environments where they might witness a spontaneous anomalous phenomenon (e.g., spiritualist events). In the context of field research, one can witness and investigate how a person experiences what may be called anomalous in the context in which it organically occurs. Field research has not been as popular as laboratory research in parapsychology in the last decades, but popular culture has demanded it in one incarnation: ghost hunting. Although generally lacking scientific and analytical vigor, its popularity says something: people become interested in anomalous experiences when they can resonate with them on a personal level and when the story of the experience becomes tangible.

The obvious conundrum is that ghost hunting is not a methodical pursuit of anomalous experiences yet it holds the interest of the public, whereas psychical research offers methodical rigor but cannot claim a similar hold in the public's eye. The field of exceptional experiences may be a bridge to this problem; it brings the focus back to the person and offers a way for experiences to become tangible under the analytical eye. It is accessible and relevant because of the latter but also because of how cross disciplinary it is. It is an inclusive pursuit of subjective anomalous experienc-

es that not only researchers and scholars can get behind, but artists, writers, documentarians, historians, clinicians, and the people who have these experiences. Although experimental parapsychology should of course continue the empirical pursuit of studying psi, the study of exceptional experiences may offer another doorway to psychical research that could bring in more people; with it, we might see a people's parapsychology.

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Evidence for Telepathy in a Nonverbal Autistic Child

This case report is about Hayley, a 10-year-old severely autistic girl who barely speaks, yet often types what her therapists are thinking with perfect accuracy. In May 2014, I investigated claims by her therapists and family that she is telepathic. Hayley's family initially thought she was a mathematical savant because she solved problems involving several digit numbers while not knowing simple multiplication or division. Sacks (1998) described autistic twins who could provide consecutive six digit prime numbers without conscious derivation of them, or even knowing how to do math. By definition, savants have exceptional, almost super-human abilities, in spite of their neurological and cognitive impairments in basic functioning. Savant skills are as mysterious as telepathy, but science accepts them because

they can reliably replicate their skills.

One day Hayley's answers changed from an ordinary numeric format to an exponential one, immediately after the calculator switched to that display. Hayley's therapist (A) asked Hayley how she knew of the change. Hayley typed, "I see the numerators and denominators in your head." Hayley then correctly answered personal questions about A that she should not have been able to, such as A's landlord's name, "Helmut." Months later, another therapist (B) independently suspected Hayley was "reading her mind." B knows German, whereas Hayley and her parents do not, so B asked Hayley to translate "I love you" into German as a test. She was shocked when Hayley passed by typing, "Ich liebe dich."

Hayley's father contacted me because of my research on autistic savants and telepathy. My



| by DIANE HENNACY POWELL

model for the neurophysiology of psi had led me to propose autistic savants as the candidates with the most potential for proving telepathy. Savant skills resemble psi and have the high accuracy rate demanded by science. Also, the autistic brain exemplifies the activity pattern I found to be associated with high reports of psi. (Powell 2008).

Hayley's family sent videos of

A looking at pictures, sentences, and numbers as test stimuli. A asked Hayley what she was looking at, and Hayley independently typed her answers with 100% accuracy. The videos were not acceptable as scientific evidence, because the stimuli were not randomized and the participants were not separated. After the family produced those videos in 2012, Hayley became dependent upon her back being touched to calm her during testing. We successfully weaned Hayley off of being touched and instructed the therapists to work with a divider between them and Hayley.

The cards were handed to the therapists upside down and completely out of Hayley's sight. The therapists turned over the cards one-by-one and wrote their own verbal descriptions of the visual images for comparison with Hayley's answers. Hayley was then asked to "read the therapist's mind"

Changes to an autistic's routine often cause behavioral regression. Hayley reverted to selecting her answers from letters or numbers on plastic stencils before typing them directly into her text-to-speech device.

Hayley also started vocalizing rudimentary sounds. Because we do not know the potential impact Hayley's speech development could have on her "telepathy," testing was done in May 2014 to establish Hayley's baseline. We filmed over six hours of controlled experiments in Hayley's home. The therapist and Hayley were the only ones in the room, and sat side-by-side at a small table with the five-foot by thirty-inch divider between them. To assess for any possible visual and/or auditory cueing, five high definition point-of-view cameras and three microphones were strategically placed to cover the entire experimental space. All cameras were synchronized and time-stamped. Test stimuli consisted of nonsense words created with wordgenerator.net/fake-word-generator. Twenty numbers between one and one billion were chosen on site by an online random number generator (Random.org). The author chose one hundred distinct images from Hayley's decks of flash and playing cards and assigned them numbers from one to one hundred. The online random number generator randomized the selection and order of presenting these stimuli:

cards containing numbers, equations, sentences, or visual images. The cards were handed to the therapists upside down and completely out of Hayley's sight. The therapists turned over the cards one-by-one and wrote their own verbal descriptions of the visual images for comparison with Hayley's answers. Hayley was then asked to "read the therapist's mind" or "tell what the therapist is looking at."

Hayley and the therapists were notably distracted at times during testing, perhaps by the introduction of cameras and the presence of new people in the next room. The ideal experimental setup was not possible. The therapist who knew the answer also needed to hold the stencil. Attempts to change this, such as the insertion of a stencil holder, were too much for Hayley at the time. Nonetheless, the data are impressive, especially when the participants were focused. This is a brief report of the highlights:

Day one, Therapist A: 100% accuracy on three out of twenty image descriptions containing up to nine letters each, 60-100% accuracy on all three of the five-letter nonsense words, 100% accuracy on two random numbers that were eight and nine digits long.

Day Two, Therapist A: 100% accuracy on six out of twelve sets of numbers with 15 to 19 digits each, 100% accuracy on seven out of 20 image descriptions, and

81-100% accuracy on nine out of nine sentences between 18 and 35 letters.

Day Two, Therapist B: 100% accuracy with five out of twenty random numbers of up to six digits in length, and 100% accuracy with five out of twelve image descriptions containing up to six letters.

Reports of telepathy between children similar to Hayley and their caregivers suggest that this is not an isolated case. Ilga K. was a 10-year-old Latvian girl who could not read simple text, but could read even foreign languages if the person next to her was silently doing so (Bender, 1938; Ehrenwald, 1940-1). "Bo," an 11-year-old boy with an IQ of 55, had features of autism, such as repetitive speech and showing no emotion at leaving his parents. He was "thought to be a lightning calculator because no matter what the row of figures given he would immediately give the answer, provided it was in his mother's mind, but he could do absolutely nothing if left alone," and he spontaneously told his mother "words or numbers which she had not overtly expressed." (Drake, 1938, p. 95). Drake (1938) tested Bo for telepathy with ESP cards, controlling for visual cues by blindfolding him and/or placing him on the other side of the room with his back to his mother. On one occasion Bo scored an average of 21 correct hits out of 25, for 14 runs.

Drake (1938) tested Bo for telepathy with ESP cards, controlling for visual cues by blindfolding him and/or placing him on the other side of the room with his back to his mother. On one occasion Bo scored an average of 21 correct hits out of 25, for 14 runs.

Recordon, Stratton, and Peters (1968) tested a boy diagnosed with Spastic Diplegia, congenital cataracts, and mental retardation who had very little vision, but could accurately guess the letters on an eye exam chart, provided his mother knew the answers. Recordon et al. (1968) concluded that their test results were most consistent with telepathy.

All of these cases suggest that highly specific information can be communicated between caregivers and some brain-damaged children. The nature of this information access and its mechanism are unknown. In the case of Hayley, subtle cueing cannot be ruled out at this time. Hayley's parents and therapists appear credible and have no apparent motivation to misrepresent their experiences. If

true, these stories are consistent with telepathy. Further experiments will be done once the experimental protocol can eliminate any possibility of subtle cueing. If Hayley's accuracy remains high, it suggests that an alternative and/or default communication mechanism latent in us becomes more highly developed, and/or expressed, in people motivated by physical dependency and severe language impairment.

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would split my comments into two issues. The first is the actual scientific issue, the second is the public relations issue. But, of course, they do intertwine. As to the actual science, I am happy to do informal things in the same room just to get a feeling for what works and what does not work, but under no circumstances would I attribute results to any kind of ESP, although I might speculate it *might* involve ESP but more testing is needed. It *could* actually indeed be ESP. For more than 50 years I have been convinced by the really rigorous evidence that various forms of ESP exist, but we also know that all sorts of sensory cues are possible when people are in the same room. Even when they are back to back, there could be sufficient reflections in some shiny surface to tell that someone had moved slightly or their facial expression had changed when something was said or done, allowing fishing for information, you might feel slight vibrations of the furniture, or hear very faint sounds that would indicate a pause for a moment in breathing, and the like. The very possibility of these, in terms of the public relations issue, is that any scientist claiming ESP when the two people are in the same room will be immediately dismissed as incompetent. Further, given the “religious” fervor of the pseudo-skeptics, these kind of dismissals will be welcomed by them and publicized as much as possible to show that parapsychologists are indeed totally incompetent. I never studied this, but I heard of

Comments on ESP Testing in Response to Diane's Videos

some claims being made for autistic children using what I think was called facilitated touch, and the possibility of sensory cueing is so obvious there that that really gives the whole area a bad name.

My own approach, which I have taken as basically standard in scientific parapsychology, is that it should be *impossible*, given all we know about the physical universe, for known sensory channels to carry critical information that would provide a simpler explanation of results than ESP. Actually that makes the psychological situation much easier also, because then you can be relaxed and friendly while interacting with percipients and not have to be really uptight to not reveal something, since you do not know it.

In terms of public relations, unless known sensory channels are *impossible* to invoke, other testing will probably be a waste of time. In terms of the actual science, if it gives some clues as to optimal psychological conditions to making things happen, that may be a step forward, especially if what is



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learned there is validated under rigorous conditions.

But given that the media thrive on controversy, expect them to jump on the telepathy part and then debunk it. And as long as sensory cues are available from a person knowing the answers being in the same room, they will probably debunk all of these apparent talents. I do not envy you. Your motivation is the best, but you are in a situation in which hostiles are ready to pounce on if you give them the slightest opening.

Relevant

Articles Relevant to Parapsychology in Journals of Various Fields (XX)

In this twentieth part of the regular bibliographical *Mindfield* column that traces and documents references to publications of parapsychological relevance in the periodical literature of various fields, we can add another 70 relevant articles to the ones that were registered before. This raises the total count of relevant mainstream journal articles to 1310 in just a few years.

As always, useful input and valuable suggestions from my colleagues Renaud Evrard, Maurice van Lijstelaar, and Annalisa Ventola are gratefully acknowledged. Hints to other pertinent recent articles are always welcome. Please send them to the author at hoevelmann.communication@kmpx.de.

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