

BOOK REVIEWS

ANOMALOUS COGNITION: REMOTE VIEWING RESEARCH AND THEORY by Edwin C. May & Sonali B. Marwaha. Jefferson, NC: McFarland, 2014. Pp. vi + 434. \$45.00 (paperback). ISBN 978-864-9458-3.

Background

Spying is one of the oldest professions and if psi exists it could be an invaluable tool: If assassination by PK (or anomalous perturbation—AP) proves impractical, then ESP (or anomalous cognition—AC) can be used indirectly to discover enemy secrets (and *then* clobber them)—or to predict future events. This idea was likely first implemented by shamans and later by an impressive array of warlords, right through classical antiquity up to modern times.

Some within the CIA clearly followed parapsychology with Argus eyes and the notorious MKUltra project, initiated in 1953, included parapsychology experiments. Later J. B. Rhine (1957) wrote: “. . . modern war, even more than ever before, hinges mainly upon secrecy and surprise. If these two dangers could be banished, a long lease on world peace could be gained . . .” (p. 249). Harold Puthoff and Russell Targ apparently agreed and in 1972 initiated what is now known as the Star Gate program, which focused on remote viewing (RV): Edwin May joined them in 1975.

Contents

Now 40 years later May, together with colleague Sonali Marwaha, has put together a compendium of articles which he authored or coauthored. This anthology is decidedly not another glossy “salon table” book on the wonders of psi but is for the most part previously published technical work on RV. As Broughton notes in his introduction, May’s is a decidedly “no-nonsense” approach: The reader will look in vain for metaphysical disquisitions. The going is quite tough, with statistics, equations and all. It comprises 26 papers, divided into six sections: Methods, (Electro-)Physiology, Decision Augmentation Theory, Entropy, Miscellaneous, and Challenges.

The seven papers in the Methods section comprise a self-contained guide to RV protocols, target construction, and analysis techniques all conveniently gathered together in one place. The book is worth having for this alone; but it is pointless to try to condense this technical material into a few words. Particularly worthy of attention is the novel “figure of merit,” based on fuzzy sets. This is basically an attempt to “semi-automate” the tedious judging process involved in rating and ranking.

The last paper in this section is of more than purely methodological interest: This is an attempt to determine whether feedback to the remote viewer (RVer) is important for success. Feedback was given over a range of tachistoscopic intensities: Unfortunately only two of the four RVer were able to score under these conditions, so the experiment effectively collapsed to case studies. The results were disparate: For one of them there was a positive but nonsignificant relationship between feedback intensity and AC success, while for the other RVer this was significantly *negative*. We are told (p. 110) that “All receivers believed strongly that feedback was not necessary for success, but they were uncertain about the degree to which feedback might contribute to success.” What is not reported is the kind of thing which would have been examined in a planned case study, for example, did this *particular* RVer strongly disbelieve in the relevance of feedback? I have proposed elsewhere (Millar, in press) that a simplified form of this theoretically important experiment should be performed on a large scale by experimenters and participants with diverse expectations.

As an old electrode buff, the six papers in the Physiology section draw my particular attention. May’s team has looked rather widely (though thinly) at physiological responses instead of the usual con-

scious responses: They used measures ranging from skin conductance to EEG alpha power to state-of-the-art magneto-encephalography. Some promising results were registered. The best (p. 152), in a presentiment experiment with auditory stimuli, yielded an over five standard deviation difference between experimental and control conditions. However, while the skin response measure went up for experimental trials, it went down for the controls. This is of little utility to the participant but does serve the experimenter's goal of getting a significant difference. I have proposed elsewhere (Millar, 2012) that such "mirroring" is a "fingerprint" for experimenter psi (ϵ). In the following study (pp. 158–171) more direct evidence was found for an ϵ mechanism: Here the difference was primarily due to a negative deviation in the control trials. It seems that the (so-called) participant's only function was that of random skin conductance generator!

Review Plan

If parapsychology were normal science, these first two sections, which account for more than half of the book, would contain most of the "meat," definite facts learned about the nature of psi. Investigators of simple physical systems usually have little difficulty in replicating initial results and gradually refine them to more and more decimal places. Parapsychology is just not like that: Typically one experimenter gets result A, while another reports B and never the twain shall meet.

From this point the order of the book is followed less closely: It is approached instead via a number of themes. Because of its nature as an anthology of May's work, the book systematically lacks context. Here it is examined within the background of parapsychology in general. First on the order are theories: Decision augmentation theory and entropy are examined within a historical framework. With these under the belt, the underlying question is tackled: Does RV really work and, if so, is it better than other methods? A critical topic, endemic to parapsychology as a whole, is then taken up: Where or from whom does the psi registered come? Some words are reserved for the promotional aspects of the volume.

Decision Augmentation Theory (DAT)

The "jewel in the lotus" of this book is undoubtedly decision augmentation theory, a phenomenological/mathematical account of psi, which May describes as the earlier psychological psi-mediated instrumental response theory of Stanford (1974) with a math topping: The core is that psi is usually quite unconscious and goes on automatically all the time to satisfy a "need" (or "disposition"). Phenomenal representation, such as having a precognitive dream or seeing an apparition, is quite unusual. DAT is based on precognition. Psi habitually acts on all the trivial choices of everyday life so as to result in a "good" future and avoid the "bad." This frequently involves unconscious precognition-guided timing. For example, Alice wakes up late one morning and misses her usual train, which subsequently crashes into a chemical tanker, crisply toasting all aboard.

The psi failure of the prematurely cremated passengers is *not* accounted for by DAT: The obvious suggestion, though, is that the unfortunate majority just didn't have enough psi—and this is surely May's answer (though it may not be welcomed by Stanford). In any case, it is likely true that most psi is unconscious and little under the control of the percipient/agent—in Rhine's terminology psi is only marginally "dirigible."

DAT applies, in principle, to *any* study in which statistical inference is used and arbitrary (timing) decisions are made by subject or experimenter: Psi enters at just these points. Of importance here is how DAT applies to RNG-PK experiments. In this theory there is no such thing as PK, only precognition: the RNG machine is switched on at just the right moment to catch a chance deviation which is "good" (nominally significant). DAT pseudo-PK is actually unconscious precognition-based selection of the "good seed" with every timing decision.

But is it true? If psi selects out the good seed from a purely chance set, it follows that the "bad seed" is left behind. DAT theorists have looked at the experimental data (good seed) but have not examined whether the positive deviation found is matched by an equal negative deviation in the "control" data. In

fact, these residue/control data have often been potential rather than real. Needed are experiments in which the total set (say 10,000 randomly pre-generated sessions) is actually pregenerated and recorded before separating the data into experimental and control: in this way the *full* prediction of DAT (for both experimental and control) can be put to the test.

A compensating negative deviation in nonobserved data may be expected rather generally for *any* theory that involves *selection* of data sets from a larger “random” set, by whatever mechanism, rather than being specifically characteristic for DAT.

To this reviewer it seems a chicken and egg question whether the basic phenomenon is precognition or (retro-)PK because both involve an influence of the relative future on the relative past. DAT fails to give a clear account of this coupling: Precognition for May seems to be regarded as something basic and unanalyzable. Physically, however, it must be nonlocal in nature, because a *signal* cannot propagate backwards in time. May asserts (personal communication, 2015) “there is NO evidence of retro-PK in physics”: (If this were true) it impacts DAT with equal force because future/past coupling is the same irrespective of the verbal nicety of whether it is called retro-PK or precognition.

DAT pseudo-PK superficially looks like “ordinary” PK. A little bit of math, though, reveals that the DAT version results in a *constant z score*, independent of the number of trials: *an experiment with a few trials (and/or participants, etc.) is as good as a big one!* By contrast, it is well known in psychology that for any trial-by-trial effect, the *z score* increases with the square root (Sqrt) of the number (*N*) of trials. The startling counter-intuitive DAT result comes about because the session (or experiment) is influenced *as a whole*: Only if psi functions on a trial-by-trial level is Sqrt(*N*) dependence to be expected. The apparent paradox is expected also from any other model whatsoever in which the data are psi-influenced together as a single composite unit.

Singled out is the global consciousness project (GCP; pp. 268–277): It is instructive to compare Nelson’s (2015) “world-mind friendly” account with the treatment by Bancel (2015) in the same volume. GCP seemed to follow a constant *z* rather than Sqrt(*N*) dependence, and this was interpreted by May et al. as likely *epsi* (Nelson-psi). However, Bancel calculated that the expected regression slope is so small that the empirical data are statistically consistent with both models. DAT has not (yet) notched up a decisive victory on Sqrt(*N*) grounds. Other evidence May et al. advance, however, seems more persuasive: The GCP events proposed by Nelson himself have significantly higher scoring than those contributed by others: Nelson himself was forced to suggest he may just know by experience what kind of events give the best scoring. Influentially, Bancel (2015) has recanted his earlier support for a real effect of global consciousness and comes out (guardedly) in favor of *epsi*.

A major limitation of the presentation here is that it gives the impression DAT is entirely *de novo*: a good theory-mother just knows his own brain-child is unique. The historical context and relationship to other theories is not explored. DAT has much (but by no means all) in common with the observational theories (OTs) and they have both been treated (Millar, in press) as non-local-in-time theories (NLTs).

In the quantum world systems can exist in several states *at the same time* (superposition), quite unlike the macroscopic world of experience. In the early stages of quantum theory it was even less clear than now where the quantum description ends and classical physics takes over. To dramatize this, Schrödinger imagined a cat in a box together with an apparatus based on radioactive decay, which either killed it or not, with a 50% chance of each: He reasoned that the cat should then exist in a superposition, both living and dead at the same time, until someone opened the box and “collapsed” the state vector to one definite event or the other.

Walker seized on this for one of the several (incompatible?) mechanisms for psi he proposed, “Walker’s Alice”, if you like. On observing a random generator output and wishing for “1”, Alice enters a superposition in which she “sees” both 0 and 1 simultaneously: She goes around in this superposition (which naturally passes unnoticed) until she (or her density matrix) is later collapsed back to ordinary billiard-ball reality, but with the desired *1 in her memory*: All the rest of the world “plays along” as if a real (classical) 1 had been generated in the first instance.

If this sounds like Alice in Wonderland – it is. The majority of (European) parapsychologists in-

volved with OT rapidly chucked this notion like a hand grenade: Only Bierman (perhaps) followed Walker here. Schmidt, ever the gentleman, was silent about Walker's Alice in public. In private he dismissed it as nonsense: Elementary calculation shows that an Alice superposition could not exist for more than about a millionth of a microsecond. However, May took Walker's mechanism seriously enough to subject it to experiment (pp. 339–350), which lent it no support: He could have saved himself a great deal of trouble.

If this was May's idea of what the OTs are all about, it is no wonder he developed a theory of his own, first the intuitive data sorting (IDS) model, which was succeeded by DAT. In the continuing course of development they have recently (Marwaha & May, 2015) started sketching an updated version—the multi-phasic model of precognition (MMPC).

Entropy

Another intriguing section deals with the relation between entropy and psi. There are two kinds of entropy: physical entropy and informational (Shannon) entropy. Physical entropy was first defined purely in terms of heat and temperature, progeny of the steam engine. It later turned out that it is a general measure of organizational disorder: For any (macroscopic) closed physical system entropy always increases (or stays the same).

Shannon entropy was inspired by the telegraph. A communication system must be designed to cater for not just a single message but the whole set of *possible* messages. Shannon characterized an information source by a measure of the available choice, Shannon entropy. This has the same mathematical form as physical entropy but is *not* restricted to any particular physical system. Information (or communication) theory (IT) is not a *substantive* theory: It is a mathematical method. IT may be described as a theory of correlation for categorical data. The beauty of IT is how it expresses things in neat (logarithmic) terms that have some connection to intuitive ideas about information. This correspondence is far from complete: In particular, there is no (direct) reference to “meaning.” IT may prove to be a fruitful method for parapsychology: Already various measures of ES are in use, which are actually (implicit) measures of information.

The first mention of physical entropy in parapsychology was long ago when it was still psychical research. In the context of hauntings and poltergeists, it was conjectured that maybe the energy for the manifestation was drawn from the surrounding air, leaving “cold breezes” behind, in violation of the second law of thermodynamics. Critics found this a “load of hot air.” Donald and Martin (1975) much later presented a formal observational theory (OT) based on statistical thermodynamics. These ideas are conceptual forerunners of DAT's psi-separation of the good and bad seed and the antientropic nature of psi.

In the course of their RV experiments May and colleagues garnered the impression that target events that involve conspicuous physical-entropy change, such as rocket launching, are relatively successful. They then turned to informational entropy (a different beast) in target pictures and found that RV scoring related to what they call the gradient of the Shannon entropy: This is a measure of the diversity within each picture. How is this entropy gradient effect to be explained? Perhaps it has nothing to do with entropy as such. If high entropy-gradient pictures stand out visually then the experiment becomes a (no tachistoscope) test of the importance of sensory feedback “in disguise” (see “Contents” section above).

Importantly they suggest an ingenious equation that relates maximum possible psi scoring to (informational) entropy change: This really warrants a separate review too extensive to incorporate here.

The authors must be lauded for drawing attention to thermodynamic and informational entropies, topics which have largely been neglected in parapsychology; however, it seems to this reviewer that here once more, their own contribution is presented as unique and they fail to set it adequately within the historical background.

Does RV Really “Wash Whiter”?

Although the title “Anomalous Cognition” is likely to attract only those with at least a modicum of curiosity about the field, the readership targeted is primarily the mainstream technical scientist. The work is pitched in a way that is familiar to the researcher: A few may be intrigued enough to try it out for themselves.

Will those scientists who attempt RV experiments get similar overwhelmingly positive results? My own crystal ball on RV tells me that only a few will get convincing outcomes: I very much hope I am wrong. Few current parapsychologists, least of all May, would be so naive as to maintain that psi-scoring can be obtained by simply following a cookbook. Time alone will tell how many experimenters can produce a gourmet meal with his book as guide.

Evaluation of published evidence is influenced to an important (and usually unacknowledged) degree by one's own experience: This is true both for experimenters who obtain substantially positive results and those who do not. Near the end of his long (near uniformly negative) career in experimental parapsychology, Beloff (1994) summarized the field as a "succession of false dawns and frustrated hopes" (p. 7). Like sentiments have been expressed by pillars of parapsychology from Pratt to Rao.

It seems that psi-missing, declines, displacements and like effects that have historically given psi its characteristically elusive character have *not* manifested in May's experience. He emphasizes (p. 21) that no decline is to be seen in his RV data and he has never yet seen psi-missing. However, in one paper addressing the effect of sidereal time on AC scoring (pp. 377–386), highly significant effects were found, which in later work (Spottiswoode, 2015) have (practically) disappeared. Other RV researchers, too, have not experienced the same "plain sailing" as May. For example, when Targ attempted to develop a practical application of RV, psi-hitting capriciously switched to psi-missing (Targ, Braud, Stanford, Schlitz, & Honorton, 1991, pp. 76–77).

Has May discovered the long sought secret of banishing decline, or is it waiting just around the corner? According to Kennedy (2003), a new psi line typically starts with a bang (Beloff's false dawn): Thereafter, scoring of selected experimenters may remain stable for some time but becomes unreliable when they attempt to clinch it, either by formal studies or to apply the effects. Finally it fades away to a limbo, which yields neither convincing evidence for psi nor its absence. The practical application that would provide a compelling common-sense case for psi still remains a "frustrated hope," though persistent attempts continue to be made.

RV is basically "guided imagination," an association exercise steered by the demand characteristics of the experiment. While handbooks have been written on how to do RV and commercial training programs are offered, it is by no means clear that any of this is of real value (Utts, 1996). While opinions in the RV community are mixed, Targ considers that one can learn all one needs to know about RV in 15 minutes.

There does not seem to be anything very specific that distinguishes RV from a plethora of other methods, and it is interesting to explore what elements might account for its success (see also the interesting psychological speculations by McMoneagle and May, pp. 368–376). The most visible difference from more standard psi experiments is that RVers are systematically selected on the basis of past success. Many alternative methods and measures were tried in an effort to forecast RV performance, but none was found to be useful in practice. This is singular, given the manifold claims for substantial correlation of psi with inter alia assorted personality measures.

The best available predictor of psi-scoring, according to May et al., is previous psi scoring: Dumas Père long ago noted "Rien ne réussit comme le succès" (Nothing succeeds like success). RV contrasts strongly with the standard approach, which currently almost eschews subject selection. If the standard recipe begins "Take 20 psychology students," then May agrees with Mrs. Beaton: "First catch your chicken"—a strategy earlier promoted by Honorton.

The conventional approach only makes sense if psi is as widely distributed as most psychological variables, and this was (and still is) the standard assumption. An alternative is the traditional (ecological) belief in the special powers of saints and witches versus the rest of humanity (HAVEs and (nearly) have-NOTs). May himself pays lip service to ". . . the normal continuum of human perceptual abilities . . ." but adds ". . . there are some who are AC gifted, much like giftedness in music, sports . . ." (p. 4), and he makes clear his preference that parapsychology should go for these "biggies." One estimate (Utts, 1996) is that 1% of the general population is fairly successful at RV.

It is to be hoped that May's approach will stimulate the renaissance of empirical investigations into the actual distribution of psi, a topic which has been nearly taboo since the '80s (Millar, 1979). A hopeful bode is that Dobyns (2015, p. 233) has analyzed the PEAR REG database and found the best fit indicates

15% constitute a “talented subpopulation” that was responsible for the psi. Apparently, however, this does not fit their RV data.

Does RV actually offer a higher yield of psi than other methods? The luminous impression from reading selected excerpts is that there is something much more powerful going on here than in the typical psi experiment. There is reason to doubt this: Utts (1996) compared the performance of RV (at SRI) to ganzfeld (at PRL): The effect sizes (ESs) for novices are nearly identical (0.16 vs. 0.17), as are the ESs of experienced participants (0.39 vs. 0.35). It looks as if ping-pong balls may be as effective as RV. The (rather poorly specified) selection procedures employed seem to be about equally good in both cases: ES is near doubled, which corresponds to a major (4-fold) increase of information per trial.

Free response trials typically take much more time than card guessing, so the standard ES (per trial) is inappropriate for comparing the two. Nelson (2006) suggests instead ES per unit time, $z/\sqrt{\text{hours}}$. In a rapid search, I was unable to find any formal comparison of RV studies versus forced-response using his measure. However, if an RV trial takes 30 min and a pack of 25 cards 5 min, the time ratio per trial is about 150 to 1 and the corresponding Nelson-ES ratio is the square root of that, roughly 12 to 1. The empirical ratio of ESs (RV/cards) is perhaps 0.16/0.015 or 11 to 1—nearly the same. This “back of the envelope” estimate suggests that RV may not, in fact, be superior to card-guessing in terms of invested person-hours.

Whodunnit?

The enormous experimenter involvement in RV fairly jumps off the page. One can hardly speak of separate experimenter and participant: It is more like a symbiotic pair. This is very unlike the usual situation in psi experiments, where the participant is “on his own” and the experimenter has a relatively aloof supervisory role. The RV setup seems likely to maximize both psychological and psi contributions due to the experimenter. The authors warn that experimenters should not run too frequent sessions (even with different RVers) or results deteriorate: This is a “warning” sign that the experimenter may be an essential part of the psi system. Already (see above) there are clear suggestions of *epsi* in the electrophysiology experiments.

This is fine if all that is wanted is to demonstrate that “sometimes psi happens”; but what if RV success is dependent on the (exceptional) person of May and his results are mere “selfies”? He recognizes the problem, which is inherent in his cherished theory (DAT), but discussion is largely relegated to a short section “Ideologically-driven Experimenter Expectancy Effects” (p. 394), in a paper on “Research Challenges” tucked away near the end of the book. It is almost as if May suspects he is an essential part of his experiments but promotes RV anyway because it is his own particular working “ritual.” He explicitly proposes (see below) that another successful researcher, Nelson, plays just this kind of role in his own global consciousness project (GCP). *Is parapsychology an “experimenter-specific science”?* May may be right in thinking the first priority is to get a psi effect any which way and only later to narrow down where it is coming from.

The million dollar question: Is it the RV method that works or is it the experimenter/lab? May (and Targ too) has a track record of successful experiments other than RV—strike one against the method. But some of the RVers involved, notably McMoneagle, have scored independently (one all). *I must admit to an intense curiosity whether such RV stars would continue to shine with me as experimenter.*

What have other people made of RV? Baptista, Derakhshani, and Tressoldi (in press) provide a valuable comprehensive summary, though approached exclusively from the traditional “participants dunnit” viewpoint. Schwartz (2015) fills in much fascinating background and his much more positive evaluation (partly based on his own experiments) provides a counterpoint to my own. Apart from the SRI International (and SAIC) work, the major investigation of RV was done by members of the PEAR group, Dunne (and nominally Jahn). This was marred by a pair of severe methodological errors. Hansen, Utts, and Markwick (1992) remark: “The research departs from criteria usually expected in formal scientific experimentation” and they continue: “Many of the issues of remote-viewing methodology were identified by Stokes and Kennedy over 10 years ago” (p. 97). Baptista et al. agree, though they express it more diplomatically. Evidence for RV consequently depends quite heavily on May’s lab. The odds, I wager, remain even.

Polemics and Politics

The banner headline of May's introduction screams: "Science Has Demonstrated That ESP Is Real!" If "science" has demonstrated psi "in spades" to May, she has, in contrast, dealt me a distinctly "thrifty" hand. The role of the scientist is not that of evangelist, to push his own overwhelming experience down the throats of benighted souls: This leaves a sour taste. So long as gross and persistent experimenter differences exist, doubt about the reality of psi remains eminently reasonable.

Concluding Remarks

This book is the result of the personal dedication and determination of Ed May, who took the greatest advantage possible of a temporary window of public funding. Despite conditions not always conducive to science, his decades of application have produced a lasting monument. This is an exciting book with fresh ideas and it deserves a place on the bookshelves of every serious parapsychologist. *Anomalous Cognition* is a "must have" for the professional who intends to do RV experiments. It should, however, be borne in mind that the book is a "one man band" and May's results may be idiosyncratic. It is very much a preliminary sketch of diverse work that should be vigorously followed up by many others. *Anomalous Cognition* can also be recommended for the contemporary scientist interested in psi, though it should be read in conjunction with a more general account of parapsychology.

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ESP WARS by Edwin C. May, Victor Rubel, & Loyd Auerbach. Palo Alto, CA: Laboratories for Fundamental Research, 2014. Pp. xii + 323. \$20 (paperback). ISBN 978-1500743000.

The literature concerning the psychic arms race between America and the Soviet Union is an intriguing mixture of fact and fiction. Many of the claims are bound to exceed boggle thresholds, but some seem more plausible. *ESP Wars* allegedly contains the true stories as narrated by some of the key players.

The book starts with a wide-ranging historical survey with accounts from ancient times, including stories about shamans, yogis, and saints. Alongside well-known mediums, there are also fascinating individuals such as Blavatsky and Rasputin who make an appearance. Messing does not appear, but Hanussen, “Prophet of the Third Reich,” does, in a section concerning Nazi occultism, a subject which, like the named individuals, is itself surrounded by myths. When Hitler came to power he began to persecute those involved in the occult, and something similar occurred under Stalin’s regime: “Books on occultism were removed from shelves all across the country, members of esoteric groups were sent to camps and shot” (pp. 44–45). Officially, in the late 1930s occultism and psi research in the Soviet Union ceased to exist.

The history of psi research in the West and the East is also outlined. Oddly, neither the declassified reports nor the accounts by parapsychologists who had been in the Soviet Union are cited or even mentioned. Project MKULTRA, initiated by the CIA in 1953, is briefly covered. According to the authors, the CIA’s interest in psi was due to its “. . . potential both as a means of mental manipulation and as a method of covertly sending and receiving information” (p. 53).

The well-known story about the telepathy experiment involving the U.S. submarine Nautilus, which allegedly took place in 1959, is naturally also covered. The authors appear oblivious to Martin Ebon’s (1983) investigation: in short, this suggests it to have been a hoax by the author Jacques Bergier, who may have been deliberately fed disinformation. The story did function as an ignition spark for the psychic arms race—Soviet parapsychologists successfully used it to argue that psi research should start anew. With minor exceptions no psi research has officially been carried out there since the late 1930s. It could thus be argued that the U.S. had a head start.

The Soviets’ attempt to catch up, combined with lack of reliable information and the sensational book *Psychic Discoveries Behind the Iron Curtain* (Ostrander & Schroeder, 1970), created the need for a threat assessment. The zeitgeist in the 1970s presumably also contributed to the U.S. intelligence agencies’ increased interest. Later, in addition to Soviet émigrés’ claims (Starr & McQuaid, 1985), popular books and articles concerning a psychic arms race emerged (McRae, 1984; White, 1988)—the impact of all this is not discussed.

The limits of psi are not known, and this worried the U.S. intelligence agencies. The need for a threat assessment resulted in what is now known as the Star Gate program, which focused on remote viewing. The program can in hindsight be said to have been initiated in 1972 by the CIA, which for a few years supported the research (Kress, 1977/1999; Richelson, 2001). Some in the CIA were clearly impressed by the initial research, but they also became aware of the still unsolved problem concerning how to separate fact from fiction in the remote viewing data. In addition, there was some unwanted publicity about the intelligence agencies’ interest in psi (Wilhelm, 1976, 1977).

Given some stunning results and the lack of reliable information about the development in the Soviet

Union, a military program was nevertheless initiated in 1978 at Ft. Meade.

The authors claim that approximately 3,000 intelligence personnel worldwide were screened with regard to their potential participation in the program. Declassified documents, however, reveal that 251 personnel were considered, 117 initially interviewed, and 6 individuals eventually selected (CIA-RDP96-00788R001100020001-8); one of them was Joseph McMoneagle. Both Edwin May and McMoneagle contribute autobiographical sections; although interesting, they seem somewhat redundant given the book's focus. More interesting are the accounts about search tasks involving remote viewers. One account is unverifiable, but the others can be scrutinized.

One search task (Project 8916) was initiated in order to locate Drug Enforcement Agency agent Charles Jordan. The authors mention only remote viewer Angela Dellafiora Ford's participation, yet 17 sessions focused on finding Jordan and several viewers were involved—their impressions differed from one another. The authors also fail to note that Ford's preferred method is referred to as written remote viewing—like a trance medium, she channeled information from entities (Smith, 2005). In addition, it is far from clear if her impressions really led to the capture of Jordan as the authors claim (Graff, 1998, 2000; Nickell, 2004).

Another case of selective reporting concerns the remote viewers' involvement in the search for the kidnapped Brigadier General James Dozier. He was allegedly impressed by some of the remote viewing data, but it should be noted that it was a Red Brigades member, not McMoneagle's impressions, that provided the clues that led to his location (Graff, 2000). Also briefly covered is the remote viewers' involvement in the Iran hostage crisis—more than 200 sessions focused on this. Andrew Endersby's (2014) examination reveals that there was a lot of erroneous information in the remote viewing data.

The authors also claim that McMoneagle was involved in the attempt to locate a Soviet aircraft which had crashed in Zaire (this claim turns up elsewhere as well). The plane, however, crashed in March 1979, before he had completed his remote viewing training. That said, data from two remote viewers, Rosemary Smith and Gary Langford, made the search team shift their focus, which caused them to encounter natives who had found the plane (Graff, 2000; Smith, 2005).

Relatively little is said about the U.S. research on remote viewing. The reader is reminded of the researchers' inability to find out why some individuals are good remote viewers and others are not. In passing, the remote viewing experiment involving the submarine *Taurus* is also covered. Of most interest are the comments about the psychic Ingo Swann, who was tasked with developing a method to train remote viewers. According to May, he had a brilliant mind and worked 12 to 14 hours each day for years, but it is stressed that he was not a scientist and that the method he developed was fundamentally flawed. The main problem was that Swann was not blind to the target and provided feedback to the remote viewers during training sessions: "Assuming no psychic ability whatsoever, a person could arrive at the correct site via clever responses, conscious or unconscious" (pp. 140–141). May also claims that Swann instilled an anti-science attitude in his trainees, which contributed to the creation of a chasm between the researchers and the remote viewers at Ft. Meade.

Naturally the eventual closure of the Star Gate program in 1995 is also covered. May has expressed his views about this before (May, 1996), but he provides some additional information about what occurred behind the scenes. For more than a decade after the closure he and McMoneagle tried to start a new program—all their efforts were in vain. Nevertheless, it is still rumored that an active remote viewing program exists (e.g., Margolis, 2013, pp. 108–110).

So far, this review has barely touched on psi research in the Soviet Union, despite the fact that the sections concerning this are presumably of most interest. The claims that these sections contain are much harder to assess and are bound to exceed boggle thresholds, although the authors for their part seem to take most of the claims at face value. Due to the frustrating lack of details it is difficult to grasp the scope of psi research after the disintegration of the Soviet Union, but it seems as if in the 1990s, the focus was on applied psi rather than research.

Some of the stranger claims that reached the U.S. concerned psychotronic generators—hardware devices, often with nothing under the casing that ostensibly utilized psychic energy. In the Soviet Union dozens were constructed but most of them never worked. That said, according to Major General Nikolai Sham, the few that did ". . . were, and still remain, utterly unique designs that were frequently ahead of

their time and which created the foundation for future technologies” (p. x). May, however, claims that Sham told him that although he had funded 40 different institutes specifically to develop psychotronic weapons they just could not get them to work. When the Cold War ended development of psychotronic weapons was discontinued.

Psychics were, however, still used, but according to the Russians their intelligence agencies only sporadically consulted psychics and primarily in crime investigations or counter-espionage operations. The authors note: “The KGB held two opposing views of psychics at the same time: Officially, the KGB prosecuted them. Unofficially, they secretly used them” (p. 281). Several examples are given, including some recollections by the psychic Tofik Dadashev (concerning him, see Gris & Dick, 1978). The Russians also, allegedly, had psychics on Dickson Island in the Kara Sea who “. . . studied the American military satellites by means of remote viewing, even to the details of their designs” (p. 170).

Major General Boris Ratnikov and Major General Georgii Rogozin consulted psychics concerning threats to Boris Yeltsin. On some occasions Ratnikov even changed Yeltsin’s schedule based on information from psychics. They also used psychics to protect important politicians from psychic influence and mind reading, but no details are given. Ratnikov relates that a psychic informed him that Yuri Skokov, Secretary of the State Security Council, was subject to psychic influence during his and Yeltsin’s visit in the U.S. in 1992: “Naturally, we tried to protect Skokov from this psychic influence and to block the leak of information through extrasensory methods” (p. 189). Ratnikov clearly believed such things could occur and has previously claimed that psychics read former U.S. Secretary of State Madeleine Albright’s subconscious mind in 1999 (Smolchenko, 2007).

Equally mind-boggling is Lt. General Alexei Savin’s account about military unit 10003. Savin comes across as an exceptional man who after three near-death experiences in childhood became psychic. Friends in high places ensured that his extensive program, initiated in 1989, was well-supported. Initially his staff consisted of 10 people, but by the year 2000, it included more than 50 people. Groups of psychics were trained (unfortunately no details are given about their training) and applied their abilities in a variety of ways. My impression is however that Savin would agree with Major General Nikolai Sham: “But most importantly, unique techniques of developing extraordinary human abilities and qualitatively increasing intellectual and spiritual levels were developed and carefully tested in practice” (p. xi). The program remained active until 2004.

In conclusion, it should be admitted that the book lacks an index, is somewhat poorly organized, and references are rarely given. Parapsychologists know where to search for more detailed information, but the book is clearly aimed at laymen. They would likely also appreciate a timeline with key events. Whether the true stories have really been provided can be disputed, but it is an interesting book. Most of the information concerning the Star Gate program is already in the open literature (e.g., May, 2014), but especially May’s recollections provide some new glimpses behind the scenes. The authors also managed to get the Russians to share some new thought-provoking information that would fit well in a science fiction novel.

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