

**Review of  
*Parapsychology: A Handbook for the 21st Century, 2015,*  
edited by Etzel Cardeña, John Palmer, and  
David Marcusson-Clavertz**

James E. Kennedy  
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*Parapsychology: A Handbook for the 21st Century* (Cardeña, Palmer, & Marcusson-Clavertz, 2015) is intended to update the Wolman (1977) *Handbook of Parapsychology*. That is a major undertaking because the Wolman *Handbook* was a milestone for parapsychology and one of the most important books in the history of the field.

A cursory comparison reveals significant differences between the new and previous handbooks. The Wolman *Handbook* was primarily intended for professionals and was not intended to be an introduction that would be accessible to undergraduates or to the general public. The new handbook appears to be intended for a wider audience. Most of the writing would be suitable for undergraduate students or for the interested general public. The topics are generally not covered with the depth found in the previous handbook.

The new handbook was published during a period of dramatic improvements to the methodological standards for research in psychology and parapsychology. Many of the chapters have comments and recommendations in line with the new standards, notably preregistration of well-powered confirmatory research.

These new methodological standards present a dilemma when dealing with previous research. How much weight should be given to studies that were conducted with methodology that is now recognized as providing many opportunities for potential bias? This question has no clear answer. I suspect that some of the less detailed coverage may be due in part to the recognition now that the in-depth discussions in the Wolman *Handbook* were based on questionable assumptions about the validity and precision of the research findings.

The new handbook has 31 chapters that are divided into 5 parts or sections. Each of these is discussed separately below. The preface by the editors includes an introduction that discusses “what is parapsychology” and associated definitions and terminology. The preface also includes a comparison of the topics in the previous and new handbooks.

## **Part One: Basic Concepts**

**In Chapter 1**, Nancy Zingrone, Carlos Alvarado and Gerd Hövelmann point out key developments since 1977 for a wide variety of topics related to parapsychology. These topics include not only various lines of research, but also developments pertaining to sociology, religion, anthropology, philosophy, education, institutions, journals, and funding sources. These discussions are more historical rather than attempts to present the current state of knowledge.

**In Chapter 2**, Ed Kelly provides a summary of the book *Irreducible Mind* that he co-authored (Kelly, Crabtree, Gauld, Grosso, & Greyson, 2007). This discussion places psi phenomena as one of several mental phenomena that cannot be explained with current physicalist assumptions about the human mind. Some of the other phenomena include genius-level creativity, mystical experiences, secondary centers of consciousness, and the unity of conscious experience. Kelly advocates that a philosophy of dualism and associated models as originally proposed by Frederic Myers and William James will be a fruitful approach. He concludes the chapter by suggesting that research on mystical experiences and the various means to facilitate mystical experience are the highest priority. He also refers readers to his new book (Kelly, Crabtree, & Marshall, 2015) that focuses more on philosophy and spirituality.

**In Chapter 3**, Doug Stokes presents a skeptical view of parapsychological research findings. This is a significant chapter that should not be ignored. The common claim within parapsychology that those who are skeptical of psi research are not knowledgeable of the findings is clearly not applicable here. It is safe to say that Stokes knows as much about parapsychology as anyone alive.

After decades of involvement in parapsychology with an open mind, Stokes has come to the conclusion that the research findings are more consistent with bias and fraud than with paranormal phenomena. Stokes does not consider parapsychology as unique for the occurrence of bias and fraud. Rather, he points out the need for improved research standards throughout science and the “recent avalanche of exposed fraud in mainstream biological and psychological science.” He concludes the chapter by noting that evidence for psi could be established by thorough investigation of spontaneous cases and by experiments with good research methodology that can be repeated by the vast majority of competent scientists.

For full disclosure, I should note that I have reached similar conclusions. After working in regulated medical research that had much higher methodological standards than academic psychology and parapsychology, I also came to the conclusion that the existing psi research does not and cannot provide convincing evidence for psi. Research with greatly improved methodology is needed. Like Stokes, I also think that undetected experimenter fraud occurs much more frequently than is generally recognized given that individual researchers can usually commit fraud easily with very little threat of detection (Kennedy, 2014a). If fraud occurs frequently, obvious differences among experimenters would be expected—which has been a traditional characteristic of parapsychological research. Meta-analyses, small p-values, Bayesian analyses, and study preregistration cannot overcome fraud. Other measures are needed and should be a top priority for parapsychology given the disparities among experimenters. I will also note that because of several striking personal paranormal experiences, I am absolutely certain that paranormal phenomena sometimes occur (Kennedy, 2000).

For purposes of this review, the term *methodological optimist* refers to those who believe that undetected bias and fraud are negligible in previous psi research and that recent meta-analyses provide compelling evidence for psi. Similarly, *methodological pessimist* refers to those like Stokes and myself who believe that the small, sporadic, unreliable effects and the differences among experimenters in producing the effects in parapsychology could easily be a result of undetected bias and fraud given past research methodology. Except for Stokes, all of the authors who review experimental results in the *Handbook* appear to be methodological optimists.

## ***Part Two: Research Methods and Statistical Approaches***

**In Chapter 4**, John Palmer provides an introduction to parapsychological experimental methods for those who are not familiar with them. This chapter is suitable for undergraduates and the interested general public.

**In Chapter 5**, Emily Williams Kelly and Jim Tucker discuss spontaneous cases. Although this chapter is in the methodology section of the handbook, it presents findings from research on spontaneous cases as well as descriptions of the methods. The first part of the chapter describes the authors' approach to spontaneous cases and critiques some other approaches. One point the authors emphasize is that the investigation of spontaneous cases has a fundamental and essential role in understanding psi and consciousness. They challenge the view held by some that spontaneous cases are secondary or peripheral to experimental research. I strongly agree with their perspective.

They also clarify that their interest is "in studying the phenomena for what they have to tell us about the theoretical question of the nature of the relation of consciousness and matter." They consider research on the meaning of the experiences in the lives of the people who have them to be a separate more clinical area of research. My view is that consciousness and meaning cannot be separated and that integration of these two approaches is essential for progress in understanding both consciousness and psi.

Their primary recommendations for research are to conduct surveys that are followed up with systematic investigation of cases. They also think the investigations should include a broad range of experiences, including mystical, bereavement, and dying.

**In Chapter 6**, Graham Watkins discusses methodology for investigating macro-PK effects, which are effects that do not require statistical analyses because there is no possibility that they would occur by chance or from random fluctuation. Levitation of objects is the classic example. The chapter begins with a brief review of various macro-PK effects that have been reported. The discussion of methodology notes that these effects tend to be associated with certain individuals and it is a good idea to have a trained magician involved. He also notes that effects that require physical contact such as spoon bending are much more difficult to investigate than effects that can be isolated from contact. One of his more significant conclusions is the warning that "it should be understood that there probably are no demonstrations that will not be dismissed as fraudulent by skeptics and stage magicians."

**In Chapter 7**, Patrizio Tressoldi and Jessica Utts discuss statistical methods for psi research. This starts with a basic introduction to statistics that would be suitable for undergraduates or the interested general public. The later sections discuss topics such as power analysis, meta-analysis, and Bayesian statistics. They describe the criticisms of classical null hypothesis testing that are

popular in psychology now, but do not point out that these problems have occurred primarily because psychologists have ignored the central role of power analysis that has long been described in textbooks on statistics for psychologists (Kennedy, 2015). Their final list of recommendations is a useful summary of the new methodological standards for research.

### ***Part Three: Psychology and Psi***

**In Chapter 8**, Rex Stanford describes his psi-mediated instrumental response model (PMIR) and Jim Carpenter's first-sight model. Both of these psychological models are basically elaborations of the working assumptions about psi at J.B. Rhine's Duke Parapsychology Laboratory in the 1960s when Stanford and Carpenter were there. The working assumptions can be found in the book *Parapsychology From Duke to FRNM* (Rhine and Associates, 1965). "[P]si seems to function as a normal, healthy ability, responsive to motivational drives, and as part of the general unconscious system of the individual" (page 109). The key points were that everyone can be assumed to have potential psi ability that is guided by motivation and that operates unconsciously and can occur without conscious intention and usually without awareness that psi is occurring. It was noted that physiological measures such as blood volume, EEG, galvanic skin response, or other physiological conditions that do not depend on a conscious response may be the best way to detect the occurrence of the unconscious psi process (page 53).

These Rhinean assumptions leave open the question of whether psi occurs basically all the time in the unconscious or only occasionally. That was treated as more of an empirical question than a theoretical question. In this chapter Stanford similarly leaves this question open for PMIR, although the implication is that psi operates frequently. The first-sight model has the explicit assumption that psi operates continuously in the unconscious.

The PMIR and first-sight models, like the Rhinean approach in general, are based on the assumption that psychological factors are the obstacles to reliable manifestations of psi. The troublesome, capricious nature of psi missing, displacement effects, declines, and unexplained loss of psi ability are usually attributed to factors such as conflicted unconscious motivations, loss of motivation, or errors of interpretation in the mediation of psi information into consciousness.

Researchers increasingly recognize that noticeable scientific progress has not occurred with these Rhinean models and that the obstacles to reliable psi may be a fundamental aspect of psi. Ideas such as that psi is radically elusive (Batchelder, 1994), is actively evasive (Beloff, 1994), manifests as a trickster (Hansen, 2001), is unsustainable (Kennedy, 2003), and is constrained to be useless (Lucadou, 1995; Millar, Chapter 13 in the *Handbook*) are increasingly recognized as worthy of investigation. In fact, the field of parapsychology is becoming divided into two camps. One camp, composed largely of researchers with degrees in psychology, focuses on hypotheses consistent with the Rhinean assumptions. Those in the other camp, composed largely of researchers with degrees in areas other than psychology, focus on non-psychological constraints to the operation of psi. Brian Millar (PhD in chemistry) describes one of those models in Chapter 13 in the *Handbook*. Other relevant non-psychological models and theoretical discussions can be found in the recent book on theories of psi edited by May and Marwaha (2015).

Stanford's discussion of the first-sight model is divided into two sections. The first part attempts to present Carpenter's key ideas in a way that is easier to understand than in Carpenter's

admittedly difficult writings. The second part corrects various mistakes Carpenter made in discussing Stanford's research. Although Stanford does not explicitly address this point, the mistakes he discusses do raise the possibility that similar mistakes may be present in Carpenter's discussions of other research.

**In Chapter 9**, Etzel Cardeña and David Marcusson-Clavertz review studies on states, traits, and cognitive variables. The topics include personality, attitude, previous psi experiences, creativity, altered states of consciousness, dissociation, and need for control. Given this wide range of topics, each topic is not discussed in great detail. They conclude that their review “does not reveal one or more psychological variables that can be used to consistently and strongly predict performance in psi tests, yet there is some regularity to the findings of various areas of research.” They note that some areas, particularly research on personality and psi, are at a stage where preregistered confirmatory studies are needed.

**In Chapter 10**, Serena Roney-Dougal reviews research on meditation and psi. She notes that in many of the early studies psi-missing in the control condition contributed as much to the results as high scoring in the meditation conditions—which may be fulfilling the wishes of the experimenter more than revealing the relationship between psi and meditation. She concludes that the research has shown that meditation is a psi-conducive condition. She also adds that most meditation research has been “with relatively untrained people who show a minimal level of very unreliable psi” and that “these initial tentative explorations into this area with Western practitioners, who by Eastern standards are just beginners, does show potential.” This chapter does not inspire attempts to confirm any particular previous study.

#### ***Part Four: Biology and Psi***

**In Chapter 11**, Richard Broughton discusses biological aspects of psi research, including evolution. He first briefly reviews research pertaining to possible psi by animals and various studies involving psi and physiological and brain measures. He then proposes an evolutionary framework for psi. His idea is that receptive psi (ESP) is linked to the human ability to imagine the future. He discusses several factors that are related to this ability and that appear similar to findings about psi. He recognizes that unknown factors apparently limit psi ability, but suggests that sufficient benefit is obtained for evolution to have developed weak psi ability.

Efforts to place psi in an evolutionary framework are much needed. However, Broughton's ideas appear to emerge from the Rhinean assumptions. To a great extent, the Rhinean assumptions are ad hoc speculations that psi has some benefit even though the capriciously unreliable results for experimental research do not demonstrate useful benefits. Thus, psi is assumed to be an unconscious process that produces benefits that are not apparent.

An alternative approach to understanding the potential benefits of psi is to look in the more natural setting of spontaneous cases. The starting point is to ask people who have had spontaneous experiences what were the effects of the experiences. Rhea White (1997a, 1997b) was the primary pioneer with this approach, but similar findings have been reported by others (e.g., Kennedy, 2000; Kennedy & Kanthamani, 1995; Palmer & Hastings, 2013).

This research gives a very different worldview than the Rhinean assumptions. *Tangible benefits related to needs, survival, and motivation, as assumed by the Rhinean models, are rare.* For example, a dream about the distant death of a loved one does not prevent the death as would

be expected if psi were guided by motivation, but does inspire a sense that there is more to life than just the physical senses. The effects of paranormal phenomena are frequently described in terms of an increased sense of meaning in life and spirituality. As noted in Kennedy (2000, p. 139), “The question at this point is not whether psi can inspire a more spiritual worldview, but whether it does anything else.” The benefits involving meaning in life and spirituality result from the experiences being specifically recognized as paranormal—which is in sharp contrast to the Rhinean speculations that psi is generally not noticed. The spontaneous cases as a whole do not provide support for the Rhinean assumptions (Kennedy, 2000).

The Rhinean assumptions and associated experimental research were a necessary scientific step for psychical research. Experimental research was based on the parsimonious assumptions that psi is guided by the motivations of the persons directly involved in an experiment or experience, and that other possible sources of psi can be ignored. These assumptions can be presumed to be valid to the extent that scientific progress is made with research programs based on them.

The lack of scientific progress with experimental research may be an indication that it is time to consider less parsimonious assumptions. Hansen’s (2001) ideas that psi must be understood in terms of cultural and sociological factors rather than individual psychology are one alternative approach. In fact, the Rhinean assumption that psi is a normal ability that can fulfill a person’s motivations without conscious intention or awareness logically leads to the expectation that a psi effect is typically the result of a network of motivations and influences from many people, rather than an effect attributable solely to the designated subject in an experiment. One can argue that the typical interpretations of experimental research are actually inconsistent with the working psychological assumptions about psi.

The lineage of dualistic ideas discussed by Kelly (Chapter 2 of the *Handbook*) that include a non-physical realm of mind or consciousness also pose a more complex and challenging concept of psi. Psi effects may depend more on factors in the non-physical realm than on factors in the physical world. These dualistic ideas often explicitly or implicitly place psi outside of biological evolution. Attributing paranormal effects to a separate realm of beings and powers that are not under direct human control has been the most common explanation for the phenomena throughout human history.

**In Chapter 12**, David Luke discusses psychedelic drugs and psi. He describes various neurochemical models that could be related to psi ability and summarizes the extensive anecdotal reports that associate psychedelics and psi. He found published reports for 23 experiments, but notes that these were exploratory and are difficult to assess. The chapter concludes with many useful recommendations and ideas for further research.

### ***Part Five: Physics and Psi***

**In Chapter 13**, Brian Millar discusses quantum theory and psi. This chapter points out that significant developments in quantum theory have made some earlier ideas about quantum physics and psi outdated. Millar describes an updated version of an observational theory for psi that has testable predictions. According to this model, some type of feedback is required for psi to occur. The model can be tested by manipulating feedback.

The model is based on quantum entanglement, which is a precarious state that breaks down if efforts are made to transmit a signal or to make use of psi. Like other manifestations of entanglement, correlations can be observed, but signals cannot be sent—which means that repeatable experiments and useful applications of psi cannot be expected. He points out that those who propose that psi can be explained by nonlocal entanglement and also propose that useful applications of psi will be developed are missing the point of entanglement.

Millar also gives experimenter psi a prominent role in research. Even if his theoretical ideas prove to be incorrect, he makes a strong argument that both physics and psychology must be considered if progress is to be made in parapsychology.

**In Chapter 14**, Adrian Ryan discusses physical factors that could affect psi performance. The most extensively investigated factor is geomagnetic activity. He concludes that there is strong evidence that telepathy and clairvoyance are stronger during periods of low geomagnetic activity, but precognition and PK may be stronger during periods of high activity. However, these studies appear to be post hoc correlational analyses of previously collected data.

Ryan's recommendations for research appear to be exploratory. He recommends separate analyses for: "psi-hitting and psi missing studies/conditions/ participants", real-time and time-displaced trials, and males and females. He also recommends recording "whether participants are seated, standing or supine and the direction that they face during experiments." He makes no mention of correction for multiple analyses. He notes the need to correct for trends in the geomagnetic data, but does not address this technically—which gives the impression that this is another exploratory factor. There is little in the recommendations that would inspire confirmatory research consistent with the new methodological standards. However, this area of research badly needs a more prospective, confirmatory approach.

### ***Part Six: Psi Phenomena: Anomalous Cognition, Perturbation and Force***

**In Chapter 15**, Johann Baptista, Max Derakhshani and Patrizio Tressoldi provide meta-analyses and recommendations for research methodology for ganzfeld, forced-choice, remote viewing and dream studies. Those who are seeking a technical review of the current state of knowledge about psi research and/or recommendations about methodology for future research will find this chapter useful.

The authors are methodological optimists and their meta-analyses are conducted and interpreted accordingly. Although methodological pessimists will not find their meta-analyses and associated extensive post hoc data explorations as convincing evidence for psi, all will agree that their analyses will be highly useful in planning the confirmatory research and prospective meta-analyses that are now needed.

Their methodological recommendations include efforts to prevent fraud in addition to well-powered, preregistered confirmatory experiments. However, fraud prevention is addressed as a general recommendation rather than with a detailed discussion of fraud prevention methods. They note the value of making raw data available for independent analyses by others. However, methods such as duplicate records and experimenters double checking each other are not described.

They also provide a useful section pointing out state-of-the-art meta-analysis methods that would improve parapsychological meta-analyses. One point they do not address is that

retrospective meta-analyses have high potential for bias because methodological decisions are made after the results of the studies are known. Making methodological decisions after the results are known is common for an exploratory stage of research. The optimal strategy for a retrospective meta-analysis would be to include both proponents and skeptics on the team conducting the meta-analysis. A retrospective meta-analysis conducted solely by proponents or solely by skeptics is likely to be significantly biased. A prospective meta-analysis with the methodological decisions publicly registered before the included studies have been conducted is a confirmatory strategy that provides much stronger evidence.

One topic that I think they do not treat optimally is the evaluation of experimenter effects. One strategy for evaluating experimenter effects is to do an ANOVA with each experimenter as a factor level. This strategy has low power and does not utilize available information about experimenter effects. It is the preferred strategy for those who do not want to find evidence for experimenter effects. Another strategy is to categorize experimenters as generally successful, sometimes successful, or generally unsuccessful based on their history with other research. This more powerful strategy is the preferred choice for those who want to find evidence for experimenter effects. In general, statistical power must be considered when drawing conclusions about the absence of an effect.

**In Chapter 16**, John Palmer reviews studies of implicit or non-intentional ESP (anomalous cognition). These are studies that contain an ESP task that is not consciously known or intended by the participant. This type of research was primarily inspired by Stanford's PMIR model and included studies in which participants obtained some type of reward if they did well on an ESP test that was embedded in a task without their awareness that the ESP test was being conducted. Palmer also included studies in which precognition is assumed to facilitate success on a psychological task as in the studies by Bem.

The available studies are diverse and generally not appropriate for a meta-analysis. Palmer summarizes the data with a table that gives counts of the number of studies in the expected direction and the number with significant outcomes. He concludes that the PMIR studies have impressive results, but the Bem studies did not replicate as consistently. This area of research appears to be ready for preregistered confirmatory studies, although Palmer's recommendations for future research do not mention that.

One topic that Palmer notes but does not fully develop is the high likelihood of experimenter effects in these studies. Rhinean models such as PMIR include the assumption that psi is more likely with stronger motivation. In these studies of non-intentional psi, is the participant or the experimenter more motivated for a psi effect?

The possibility of experimenter effects is particularly high in studies that have contrived artificial assumptions. For example, some of the studies were academic exams for students with the answers to some questions in a sealed envelope behind each student's exam sheet. The questions with answers varied among the students. The assumption was that the students would do better on the questions with the answers in their envelope. This assumes that ESP works better if the answer is closer to the student than if the answer is in the envelope of a nearby student. In fact, the answers for all questions presumably are readily available by ESP. A higher success rate for questions with answers in the student's envelope almost certainly reflects the motivations of the experimenter more than the operation of psi by the students.



**In Chapter 17**, Dean Radin and Alan Pierce review studies of presentiment, studies of correlations among brain waves, and studies of brain states associated with successful psi performance. They note that the first presentiment study using physiological measures of precognitive anticipation was a study by Levin and Kennedy (1975) that reported successful results. Unfortunately, they do not discuss the later summary of the research (Kennedy, 1979) that noted methodological problems that raise doubts about the earlier report. That summary also reported findings from three subsequent studies. The overall conclusion was that these exploratory research efforts were inconclusive. As Radin and Pierce note, this line of research was subsequently pursued by Hartwell (1978, 1979) who reported two more formal, but unsuccessful, studies. It is notable that Hartwell's studies were preregistered in accordance with the policy of the *European Journal of Parapsychology* at that time and are some of the first preregistered studies in parapsychology.

This research utilized the contingent negative variation (CNV), which is a slow brainwave that indicates anticipation. We chose this measure because it captures a relatively high level of cerebral processing. We thought that a high level of cerebral functioning might be more likely to show psi effects than lower level physiological processes. However, the presentiment line of research initiated by Radin in 1997 utilized skin conductance with emotional stimuli, and has been reported to be more successful.

Unfortunately, this area of research is confounded by methodological issues. Most of the research used the physiological measures as the dependent or outcome variable. This reverses the long established method for analyzing data in parapsychology. Human responses, whether conscious or physiological measures, cannot be assumed to be independent as is required for the outcome variable for standard statistical analysis. This has long been recognized in parapsychology and is why the traditional analysis uses human responses to predict the random events rather than the random events to predict human responses (Burdick & Kelly, 1977; Kennedy, 2014b). The optimal analysis for presentiment studies would be to develop prediction criteria from previous data and then apply the criteria to predict the random stimuli on new trials (Kennedy, 2013, 2014b). The early CNV studies did not fully achieve this methodology, but were generally closer than the later presentiment studies.

Radin and Pierce claim that permutation tests adjust for dependencies among observations, but I do not see how that is possible for these data. Permutation tests are based on the assumption that the observations are exchangeable (Good, 2005). Permutation tests can be used with dependent data if the data can be placed in blocks that are exchangeable. However, when the participants' responses likely incorporate reactions to feedback on previous trials, the sequence of responses can be expected to depend on the specific sequence of random stimuli to which the participant was exposed, which makes the responses not exchangeable.

Attempts to correct for dependence with simple statistical adjustments or to do post hoc analyses to argue that certain types of dependence did not occur are controversial (Dalkvist, Mossbridge, and Westerlund, 2014; Kennedy, 2013, 2014b). As the early statisticians for parapsychology recognized, dependence problems are better avoided rather than debated.

Preregistration of presentiment studies is very important because these studies have significant potential for bias (Kennedy, 2013). The physiological values in the analysis are typically derived from complex data processing after the researchers know the type of stimulus

on each trial. This gives opportunity for bias to enter into the data processing decisions and likely has greater potential for bias than the dependence problems.

**In Chapter 18**, Stefan Schmidt reviews research on distant intention. In these experiments, an agent attempts to activate or calm the physiology of a remote person or the state of the remote person as evaluated by physiological measures. This research is closely linked to distant healing and related topics. Meta-analyses of the three most common types of studies (electrodermal activity, remote staring, and attention focusing) found small but significant effects for each type of study. The primary obstacle for research is that with the observed effect sizes, 650 sessions are needed to obtain a power of .80 for confirmatory research. This is not considered feasible. Schmidt concludes that the top priority for research is to explore factors that might enhance effect size.

Schmidt does not discuss the striking decline in effect size that occurred with the first studies by Braud and Schlitz (1991), and the associated implications for experimenter effects. The first three studies each had 10 sessions and obtained significant results—which is extremely unlikely if the effect size from the meta-analysis was applicable. Meta-analyses that focus on overall average effect size without considering factors such as experimenter effects and the initial declines for a new line of research that have been common in parapsychology may obscure more than enhance understanding of psi.

Like presentiment studies, these studies use human responses as the dependent variable, which raises potential dependence problems. However, these studies do not have feedback on each trial as occurs with presentiment studies and that greatly increases the potential for bias. Schmidt notes that concerns have been raised about the possible effects of trends for increasing relaxation over a session. Such trends are a common dependence problem for physiological measures. He also notes that various non-standard measures of skin response were used in many studies. As with presentiment studies, the processing of the physiological data needs to be carefully pre-specified and preregistered for confirmatory research to assure that post hoc adaptations are not made during data analysis.

**In Chapter 19**, Stephen Braude discusses macro-PK. This chapter starts with a very interesting discussion of the controversial nature of macro-PK, the common criticisms, and the historical background. However, the discussion of recent developments is not nearly as detailed or thorough as the introduction, and primarily lists references with little summary or evaluation of the evidence provided by the references. This left me hanging without a sense of the current state of evidence or potential for research. Notably, the investigations of sitter groups by Batchelder (1966, 1984) and others provided a wealth of theoretical ideas about conditions needed for the production of macro-PK as well as much to contemplate regarding evidence. I was expecting to find information about the current state of that research. The emphasis in this chapter on the introductory discussions of extremely rare individuals in the past is unlikely to inspire research.

**In Chapter 20**, Mario Varvoglis and Peter Bancel discuss PK experiments with electronic RNGs. This appears to be a relatively unbiased effort to make progress with a very difficult topic. The inconsistent nature of their conclusions reflects the complexity of the topic. They conclude that the existing data provide evidence for publication bias and (with methodological optimism) also for psi, and that more theory-driven research is needed. However, they also say

that the available data cannot be used to estimate effect sizes for properly planning research and that the high likelihood of experimenter effects in PK studies “may be pointing to the need for reconsidering the experimental paradigm of parapsychology altogether.” The conclusions I get from this chapter are that research with RNGs remains at an exploratory stage and that any hope for confirmatory research must be predicated upon selected experimenters and/or selected subjects—and may be too idiosyncratic for meaningful meta-analysis.

One possibility that the authors do not discuss is goal-oriented psi experimenter effects (Kennedy, 1995). This model considers an entire experiment as one very complex PK trial with the probability of success equal to the significance level desired by the experimenter—typically .05. The probability of a successful outcome for conducting an experiment is 1/20, which does not differ conceptually from the probability of a successful outcome for throwing a die of 1/6. For both cases, the PK effect may operate on the desired outcome as a unit.

If this effect occurs, p-values and z values for experiments will be unrelated to sample size and a meta-analysis will find a cluster of outcomes at the criteria for significance—which will typically be interpreted as evidence for publication bias and other methodological biases. This pattern is exactly what has happened with the RNG studies.

Preregistered studies will eliminate the possibility of common methodological biases and may allow recognition of the model of goal-oriented psi experimenter effects. Sample size and effect size are not relevant when planning studies if goal-oriented psi is operating. The specific motivations and psi abilities of those involved determine whether a study has goal-oriented psi experimenter effects, and the motivations may change as a series of experiments are conducted (Kennedy, 1995).

**In Chapter 21**, Roger Nelson discusses the Global Consciousness Project that investigates deviations in the output of a network of electronic RNGs during times of major world events. The RNGs are located around the world. The idea is that a large number of people concentrating on the same world event creates some type of field consciousness that causes deviations in the RNGs. The project has been running for 15 years and investigated over 450 events.

Of course, for those who adhere to a Rhinean or other motivation-driven model of psi (which includes the great majority of those who do psi experiments), any psi effects in this research are assumed to be experimenter effects. This is just another study of non-intentional psi. The intrinsically post hoc nature of the analyses raises substantial methodological challenges. I have not delved into the complex statistical issues to develop an informed opinion about the validity of the claimed results. If there are psi effects in these data, I expect that experimenter effects will remain the most parsimonious explanation for the foreseeable future.

**In Chapter 22**, John Palmer and Brian Millar discuss experimenter effects in parapsychological research. Palmer reviews the main studies that investigated experimenter differences and notes that experimenter psi influence on the experimental outcome would be virtually inevitable with the PMIR model. Millar notes that experimenters can be expected to influence the experimental outcome with either Rhinean or non-Rhinean models of psi. He also discusses cases of experimenters who admitted that they attempt to use psi to influence the outcomes of their experiments. He suggests that experimenters may leave “fingerprints” that can be detected. This is similar to the unintended or incidental signs of experimenter “mind prints” initially suggested by Jule Eisenbud and discussed in Kennedy and Taddonio (1976).

The authors do not discuss one of the best strategies for investigating experimenter effects. That is to conduct experiments in which the participants have motivation for a certain outcome and the experimenter has motivation for a different outcome. The results for such an experiment indicate the relative roles of participant and experimenter psi. This type of situation occurs more frequently than is generally realized as experimenters investigate their pet hypotheses. The studies of psi in academic exams noted above are an example. Studies of majority-vote procedures are another example (Kennedy, 1995).

### ***Part Seven: Psi Phenomena: Research on Survival***

**In Chapter 23**, Julie Beischel and Nancy Zingrone review research and associated controversies involving mediumship. They focus on the findings of research with mediums and put aside the unresolvable debate about whether information from mediums is due to psi by the medium or due to a discarnate being. I found this approach more productive than the usual rehash of the debate about the source of psi. They provide summaries of recent studies investigating the validity of readings by mediums and the associated methodological criticisms. They also summarize research on dissociative and other characteristics of mediums and on the experiences of mediums.

They conclude that a standard proof-oriented protocol would be valuable and make recommendations for that. This area of research appears to be ready for preregistered confirmatory studies. They also recommend more diverse research to understand mediumship and to evaluate the role of mediumship for bereavement.

**In Chapter 24**, Antonia Mills and Jim Tucker provide a summary and update of the research and writings on reincarnation by Ian Stevenson. Stevenson's in-depth case studies provide very interesting results, but also have the well-known limitations of non-experimental research. It might be useful to broaden the inquiry to address questions such as how do these experiences affect the lives of those involved?

**In Chapter 25**, Michaelleen Maher provides a useful review of diverse research and proposed explanations related to ghosts and poltergeists. She concludes that there is no irrefutable evidence that can support the paranormal validity of ghosts and poltergeists, and that the proposed explanations, both normal and paranormal, have not been productive. The matter remains a mystery. She notes that poltergeists, in particular, exemplify the "trickster-liminality connection" described by Hansen (2001), who proposes that ostensible paranormal phenomena often include a mixture of bogus effects and truly paranormal effects that are disruptive and cannot be captured and controlled.

**In Chapter 26**, Mark Leary and Tom Butler review the findings and proposed explanations for electronic voice phenomena (EVP), which are voice-like sounds in the background noise of magnetic tape and digital audio recordings. Instances of ostensible EVP are usually no longer than three seconds and one to five words. The quality and characteristics of the voice-like sounds vary greatly. Leary and Butler note that most of the research on EVP has been by amateur investigators and that scientific research has been rare. They conclude that some EVP effects are difficult to explain by normal processes and that the current research is inadequate to provide conclusions one way or the other.

## ***Part Eight: Practical Applications***

**In Chapter 27**, Rupert Sheldrake discusses psi in everyday life by animals and humans. Most of the discussion is about reports of anecdotal experiences, but experiments are reported for three effects. The first is experiments investigating the possibility that dogs precognitively know when their owners will return home. However these experiments had a poor design that invites controversy. The owners returned home at randomly selected times and the behavior of the dog was evaluated prior to the owner arriving. Unfortunately, this does not provide a clear control condition because the anticipation by the dogs may vary over time when the owner returns at a later time. A simple alternative design would have avoided this controversy. The behavior of the dog would be evaluated at a fixed time each day and the owner would either arrive at that time or arrive later based on a random process. The dog's behavior would be used to predict whether the owner returned at the fixed time. This design also avoids statistical dependence problems.

Sheldrake also reports experiments with people knowing or guessing who is calling them. These experiments did not have obvious methodological problems. Other experiments investigated a person sensing that they were being started at. Unfortunately, many of these experiments apparently had unacceptable designs, such as the two people in the same room and/or the same random sequence used for all participants.

Well-designed preregistered confirmatory experiments are needed for these research areas.

**In Chapter 28**, Martina Belz and Wolfgang Fach discuss clinical counseling for people who believe they are having paranormal or other exceptional experiences and seek understanding of the experiences, or find them distressing, or have underlying psychopathology. Much of the chapter is devoted to describing various classification models for the experiences and associated psychology. The section on intervention notes that counselors need to have an understanding of parapsychology and that each case typically has unique features that prevent applying a standard formula for counseling.

**In Chapter 29**, Paul Smith and Garret Moddel review technological approaches for practical applications of psi. They note that much of their review is based on anecdotal evidence. The topics include remote viewing in intelligence collection, associative remote viewing for financial gain, precognitive dreams, dowsing, ESP in police investigations, ESP in archeology, and divination. Their review is highly optimistic with little discussion of critical evaluations. They appear to assume that the reversals and loss of effects that have sometimes occurred with these investigations are minor problems that will soon be overcome. My observation has been that most people begin parapsychological research with similar optimism, but reliable effects and sustainable applications of psi have not been achieved after 85 years of experimental parapsychology and 130 years of psychical research. As noted in the comments above for Chapters 8, 13, and 25, this situation has resulted in an increasing recognition that some fundamental property of psi may prevent reliable, useful effects. A balanced review of the current state of knowledge for applications of psi would include these alternative views.

## ***Part Nine: To Sum It Up***

**In Chapter 30**, Gerd Hövelmann argues that parapsychology has made important methodological contributions to scientific research. He also provides philosophical discussions

about the relationship between established science and parapsychology as a borderline area of research.

**In Chapter 31**, Etzel Cardeña summarizes his thoughts about the status of parapsychological research in light of this handbook. He believes that the evidence indicates that psi effects do occur, but he also remains adrift in uncertainty and mystery about the nature of the phenomena and about consciousness. He considers the new methodological standards to be an important and necessary step, but suspects that they will result in scientists being forced to confront how little they know more often than providing clear resolution of scientific questions. Cardeña ends the chapter (and book) with a list of recommendations for further research. Notable recommendations include to investigate “are psi phenomena in the lab of a different nature than those in real life,” and to “investigate systematically the social ecology and psychology of the most successful researchers.”

## Final Thoughts

For those who are methodological pessimists, the primary value of this new handbook is to guide future research and to provide a foundation for designing studies that have better methodology than in the past. Many chapters do this well. For those who are methodological optimists, the handbook summarizes compelling evidence that psi occurs in addition to providing a foundation for future research. Unfortunately, given past research practices, this handbook cannot resolve the different perspectives of methodological optimists and methodological pessimists. Hopefully, future research will narrow the gap and the next version of a handbook of parapsychology will have a greater emphasis on what is known compared to what needs to be done.

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