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EDITORIAL

> *Man is a rational animal who always loses his temper*
> *when he is called upon to act in accordance with the dictates of reason.*
> —Oscar Wilde

I've often noticed how debates within the SSE community sometimes parallel debates in the political arena, perhaps especially with respect to the passion they elicit and the intolerance and condescension sometimes lavished on members of the "opposition." Occasionally, of course, the debates in the SSE are nearly indistinguishable from those in the political arena—say, over the evidence for human-caused climate change. But what I find most striking is how the passion, intolerance, etc.—perhaps most often displayed by those defending whatever the "received" view happens to be— betrays either a surprising ignorance or else a seemingly convenient lapse of memory, one that probably wouldn't appear in less emotionally charged contexts. What impassioned partisans tend to ignore or forget concerns (a) the tentative nature of both scientific pronouncements and knowledge claims generally (including matters ostensibly much more secure than those under debate), as well as (b) the extensive network of assumptions on which every knowledge claim rests.

So I'd like to offer what I hope will be a perspective-enhancer, concerning how even our allegedly most secure and fundamental pieces of *a priori knowledge* are themselves open to reasonable debate. A widespread, but naïve, view of logic is that no rational person could doubt its elementary laws. But that bit of popular "wisdom" is demonstrably false. And if that's the case, then so much the worse for the degree of certitude we can expect in more controversial arenas. Let me illustrate with a few examples.[1]

Consider, first, an empirical context in which some people have tried to deploy a logical law. In philosophical discussions of the nature and structure of the self, many writers invoke some version of the *law of non-contradiction* to argue for the existence of distinct parts of the self. This strategy is at least as old as Plato and may be more familiar to *JSE* readers in the form it took with Freud. Ironically, though, these arguments highlight just how insecure this dialectical strategy is (for a more detailed account, see Braude, 1995, Chapter 6).

Consider: In debates about the nature of multiple personality/dissociative identity disorder (MPD/DID), many argue that because different alter personalities/identities can apparently have different and even conflicting

epistemic states, that there must be distinct parts of the self corresponding to the conflicting states. So Kathleen Wilkes writes:

> We break this law [of non-contradiction] as soon as we permit ourselves to say that one and the same entity both knows and does not know that *p*, for nothing can, at time *t*, be said to φ and not to φ. (Wilkes 1988:142)

Of course, to those without any philosophical axe to grind, cases of DID might suggest that one can indeed be said to φ and not to φ at the same time. Since that could easily be taken to suggest that the law of noncontradiction has some hitherto unacknowledged limitation, and since one must always be open to the possibility that logical laws have limitations of one sort or another, let's examine the status of the law which some dissociative and other phenomena appear to violate.

Notice, first, that what logicians generally consider to be the law of noncontradiction is either (a) the formal, syntactic law "~(*A* · ~*A*)," usually rendered more informally as "not-(*A* and not-*A*)," or else (b) a claim in logical semantics about truth-value assignments, namely, "no sentence can be both true and false" (or alternatively, "the conjunction of any sentence *p* and its denial not-*p* is false"). But the first of these is not violated by dissociative conflicts, and the second is not even clearly a law.

Consider the syntactic law first. It concerns the *form*, rather than the content, of strings of symbols within a formal system. It takes any compound expression of the form "not-(*A* and not-*A*)" to be a theorem, for any well-formed formula "*A*". But strictly speaking, the law does not pertain to sentences of any actual natural language. The syntactic law of noncontradiction does nothing more than sanction a particular arrangement of expressions within a certain set of formal systems. And although one can easily determine which symbolic expressions are theorems, those logical systems do not, in addition, offer a decision procedure for determining which sentences in a natural language are true or false. On the contrary, the relationship of formal to natural languages has to be both stipulated and investigated. And ultimately, the utility of a formal system of logic has to be evaluated empirically, by seeing whether or how well it applies to various domains of discourse, for example by seeing whether the truth-values it would assign to actual sentences matches our independent judgments about what those truth-values should be.

In fact, formal logical systems don't even specify which expressions in a natural language count as legitimate instances of a simple (i.e. noncompound) formula "*A*", hence, which natural language expressions are instances (or violations) of its theorems. Although logicians generally

agree that the simple formulae of the systems should represent declarative sentences, there's considerable debate over which particular *kinds* of declarative sentences are suitable. Interestingly, many would say that as far as the purely formal laws of logic are concerned, "*A*" could stand even for sentences whose truth-value or meaning are uncertain, such as "unicorns are compassionate," "the square root of 4 is asleep," and "Zeus is insecure." But then it seems as if the uninterpreted formal law of noncontradiction is simply irrelevant to the cases under consideration. At best, those cases appear to challenge a semantic counterpart to the formal law, either

(NC$_1$): The conjunction of any sentence *p* and its denial not-*p* is false

or

(NC$_2$): No sentence can be both true and false

We needn't worry at the moment about whether (or to what extent) either of these versions of the law of noncontradiction is satisfactory. What matters now is that even if the law of noncontradiction turns out to be a viable principle of logical semantics, it may still have a variety of significant limitations. In fact, the utility of formal logical laws varies widely, and the interpretation of those laws has proven to be a notoriously tricky business. As with all formal systems, no system of logic determines in which domains (if any) its expressions may be successfully applied. Students of elementary logic learn quickly that there are differences between the logical connectives "and" and "or" and many instances of the words "and" and "or" in ordinary language. Similarly, not all "if . . . then . . . " sentences are adequately handled by the material conditional in standard systems of sentential logic, although that logical connective is undeniably useful in a great range of cases. Moreover, varieties of nonstandard and "modal" logics have been developed in attempts to represent types of discourse resistant to standard logical systems.

But even more relevantly, in most standard systems of logic, the formal law of noncontradiction, "not-(*A* and not-*A*)," is demonstrably equivalent to the *law of the excluded middle*, "*A* v ~*A*" (i.e. "*A* or not-*A*"). Like the formal law of noncontradiction, the law of the excluded middle concerns the form rather than the content of expressions. It takes any compound formula of the form "*A* or not-*A*" to be a theorem (or logical truth), no matter what formula "*A*" happens to be. Now the semantic sibling of that syntactic law is called the *law of bivalence*, which states that every sentence is either true or false. But the law of bivalence has faced numerous challenges throughout

the history of logic (in fact, since the time of Aristotle). Many people have argued that it fails for sentences in the future tense and sentences whose singular terms refer to nonexistent objects. Moreover, some logicians consider these difficulties sufficiently profound to warrant the development of logical systems that retain the syntactic law of the excluded middle but reject the semantic law of bivalence (see, e.g., van Fraassen 1966, 1968, Thomason 1970). Now granted, these same logicians don't also reject the semantic version of the law of noncontradiction. Nevertheless, their reservations concerning bivalence should give us pause (especially in light of the caveats noted above regarding the limitations of formal systems generally). The debate over bivalence illustrates an important point, namely, that *the relative impregnability of a formal logical law may not be inherited by its semantic counterpart (i.e. one of its interpretations)*. But at the very best, it's only the semantic counterpart of non-contradiction that rests at the center of the Platonic/Freudian arguments for parts of the self. And in fact, as far as Plato's argument for the parts of the soul is concerned, the argument turns on an even more exotic interpretation of non-contradiction. See Braude (1995) for details.

But before we leave this topic, it's important to note that

(NC$_1$): The conjunction of any sentence *p* and its denial not-*p* is false

and

(NC$_2$): No sentence can be both true and false

are likewise problematical, and probably more so than most *JSE* readers appreciate. First of all, (NC$_1$) has numerous counterexamples familiar to students of logic and the philosophy of language. For example, it seems to fail for sentences such as the aforementioned "unicorns are compassionate," "the square root of 4 is asleep," and "Zeus is insecure," which seem to lack truth-value. Many people (but, notably, not all) would say that when a sentence lacks truth-value, the conjunction of that sentence and its denial also lacks truth-value.

The somewhat more common (NC$_2$) has similar problems. Most notoriously, perhaps, it fails for the self-referential sentence "this sentence is false," as well as for kindred expressions that don't seem even remotely suspicious inherently. For example, it fails for the innocent "the sentence on page 42 is false," when that sentence happens to be the only sentence on page 42. If these sentences have any truth-value at all, it seems as if they will be both true and false.

Furthermore, (NC$_2$) apparently fails for quite mundane present-tense sentences. For example, "Socrates is sitting" may be true at one time and false at another. Of course, one standard response to such cases would be to claim that the sentence "Socrates is sitting" contains an implicit reference to its time of production, so that it's not really the same sentence that's true at one time and false at another (i.e. those nonsimultaneous sentences would allegedly differ in meaning or express different propositions). For reasons too complex to be explored here, it seems to me that this particular maneuver creates more problems than it solves. Indeed, I've argued that the standard Aristotelian notion of contradictories (stated in terms of opposing truth-values) fails conspicuously for a tensed natural language, and that tensed contradictories can have the same truth-value (see Braude (1986) for a discussion of these issues). Although I recognize that my position is most definitely a minority view, I submit that there are additional serious reasons here for challenging the straightforward application of (NC$_2$) to a real natural language, hence, for questioning its inviolability outside of the highly artificial or overly simplified linguistic situations to which logical laws apply easily. In any case, this nest of issues illustrates again the kinds of concerns involved in evaluating the apparently uncertain status of what are considered to be our most cherished logical principles.

Please note that my point is not that the semantic law of noncontradiction is useless as a philosophical tool. And the moral is not simply that logical laws (like formal laws generally) may not hold in all domains (although that's certainly true and relevant here). Rather, the point is also that logical laws hold *in real life* only for sentences we regard as *acceptable* (or *legitimate*) and *appropriate*, or as understood in certain ways rather than others. But these interpretations and classifications of linguistic entities are *practical* decisions, made as part of a much larger network of interrelated philosophical commitments. Accordingly, those decisions don't stand or fall in isolation from others in various areas of philosophy and logic. In fact, they will continually be open for reassessment in light of apparent difficulties arising at numerous points in our overall system of commitments.

One further example reinforces that last point; it concerns what many regard as a fundamental principle about what philosophers typically call *numerical identity*. Many people have argued that it's an indisputable rational principle that everything is identical with itself. However, it turns out that the concept of numerical identity is not so straightforward.

To see this, consider first the expression

(x)(x = x)

usually interpreted as "anything x is such that it's identical to itself," or more colloquially, "everything is self-identical." The acceptability of this alleged law of identity is not something we can decide by considering that law alone, and it's certainly not something that's immune from debate among reasonable and well-informed persons. Regarded merely as a theorem of a formal system, it has no meaning at all; it's nothing more than a sanctioned expression within a set of rules for manipulating symbols. But as an interpreted bit of formalism, it's acceptable only with respect to situations in which we attempt to apply it. And perhaps more interesting, it's *intelligible* only as part of a larger network of commitments. That is, what we mean by "everything is self-identical" depends in part on how we integrate that sentence with other principles or inferences we accept or reject.

To see this, consider whether we would accept as true the statement

(1) Zeus = Zeus

To many people, no doubt, that sentence seems as unproblematically true as the superficially similar

(2) Steve Braude = Steve Braude

However, in many systems of deductive logic containing the rule of Existential Generalization (EG), from the symbolization of (1), namely,

(1′) z = z

we can infer

(3) $(\exists x)\, x = z$

which we typically read as

(4) Zeus exists.

And of course, many people consider that result intolerable.

Not surprisingly, philosophers have entertained various ways of dealing with this situation. One would be to taxonomize different types of existence and interpret the rule of Existential Generalization as applying only to some of them (for example, prohibiting its application to cases of mythical or fictional existence). Another approach would be to get fussy about the concept of a *name*. We could decide that "Zeus" is not a genuine name

and that genuine names (like "Steve Braude") pick out only real existent individuals, and not (say) mythical or fictional individuals. (Readers might be especially surprised to learn that some people have actually endorsed the view that we should not consider "Hamlet" or "Zeus" to be names if they pick out fictional or mythical characters.) In any case, both these approaches concede certain (but different) sorts of limitations to standard predicate logic and the way or extent it connects with ordinary discourse. Others prefer to tweak the logic directly, either syntactically or semantically. For example, some people simply reject the rule of Existential Generalization and endorse a so-called (existence) *free* logic. Alternatively, some retain EG but adopt a substitutional interpretation of the quantifiers "(x)" and "($\exists$x)", so that instead of reading (3) as

(3′) There is (or exists) some x such that x is identical with z (Zeus)

we read it as

(3″) Some substitution instance of "x = z" is true.

The latter, they would say, is acceptable and carries no existential commitments.[2]

Now the reader needn't understand all these options. The moral, however, should be clear enough. All these approaches raise concerns about what should be regarded as a *thing* in certain contexts. The statement "everything is self-identical" is not as clear or indisputable as one might think, and even more important in the present context, it's not simply true *no matter what*. Its truth (and indeed, meaning) turn on a number of other decisions as to which other principles or inferences are acceptable, and that whole package of decisions can be evaluated only on pragmatic grounds. Moreover, it's perfectly respectable to decide that some solutions to this conundrum are appropriate for some situations and that other solutions are appropriate for others. We're never constrained to select one solution as privileged or fundamental.

The reason why I've gone on at such length about these matters is that they should serve as a cautionary note to those who all too easily display intolerance and condescension in empirical (or political) debates. It's completely clear that reasonable and informed people can disagree (and have disagreed) over the nature and status—and, indeed, the *meaning*—of what we take to be fundamental logical laws. Of course, scientific (and political) debates rest not only on logical assumptions but on various empirical, methodological, and other conceptual assumptions as well. So presumably

they're even more contentious and vulnerable to reasonable challenges than disputes over the foundations of logic. But then one would expect to find even more room there for reasonable and informed disagreement. Ideally, then, one would expect participants in empirical debates to be particularly open-minded, tolerant, and respectful of opposing views. So the next time you find yourself tempted to dismiss or deride with a disdainful flourish someone with whom you disagree over a matter of science (or politics), I encourage you to remember how venerable and substantive are the serious debates over the very foundations of our conceptual framework.

Notes

1 I'm indebted to Aune (1970) for much of what follows.
2 For more on free logic, see Lambert (2004), Morscher & Hieke (2001), and van Fraassen (1966). And for an accessible review of many of the issues concerning nonexistent objects, see Reicher (2016).

—Stephen E. Braude

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Statistical Parapsychology as Seen by an Applied Physicist

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Abstract—An attempt is made to recognize a system behind the statistical psi effects that are evaluated in terms of hit rates. For this purpose, I formulate five rules that appear to apply at least to studies of good quality with the most common chance hit rates $p = \frac{1}{2}$ or $\frac{1}{4}$. A problem in the evaluation of the results arises from the fact that the hit rate h cannot be smaller than 0 or larger than 1. This implies that the z-scores of an experiment, i.e. the ratio of deviation to standard deviation, and their mean values $<z>$ can be limited as well. The true effect size should in principle be unbounded, but its standard definition by $<z>$ may be expected to fail whenever h is near one of its boundaries. In order to deal with such a situation, most likely if an experiment consists of a single decision between hit or miss, an effect size is needed that is unlimited but for $(h - p) \rightarrow 0$ merges with $<z>$. Two such effect sizes are derived here from models of psi effects. Moreover, on the basis of a sixth rule, as yet preliminary, the scattering of the effect size, a common but little-explored phenomenon, and its possible consequences for the hit rate are dealt with. The comparison of the ratio of the $<z>$-scores of two extensively investigated psi effects with that of the corresponding conjectured true effect sizes helps to decide between the models. Another such comparison may suggest insufficient separation of (ganzfeld-psi) experiments.

Introduction

On one hand, parapsychology deals with rare phenomena that very likely are anomalous and in general are not repeatable. They lack the reproducibility characteristic of the natural sciences. On the other hand, roughly since the middle of the last century there have been numerous investigations of psi effects of a very different, nearly opposite kind: The result of a single experiment remains within random noise, but the statistical analysis of a large number of equal or similar experiments proves the existence of the psi effect. The chance probability of the overall result tends to zero as the number of experiments increases, even though the size of the effect may vary

from study to study, including excursions to negative values. Accordingly, statistical psi effects are considered not reproducible but replicable. In a typical study, the mean of a stochastically fluctuating quantity is shifted by the influence of psi in the desired direction. Examples are the increase of the hit rate above its chance value when faces of playing cards are guessed (ESP) or dice are thrown (PK). Many people believe they have had experiences of this kind when they played games of chance. Statistical parapsychology provides evidence that such anomalies actually take place, even in studies conducted under controlled conditions in the lab. Without dwelling on details of mathematical analysis, Schmidt (2014) recently gave a survey of experiments on statistical psi effects. An introduction to their evaluation was published by Tressoldi and Utts (2015).

The statistically detectable psi effects appear to be everyday occurrences. They have been found at different places and by different investigators and participants. There are no regions or populations in the world that are known to be devoid of anomalous occurrences. Therefore, psi abilities are likely to be universal. However, the averages such as the hit rate of a particular study more often lie outside the confidence limits of similar studies than is allowed by chance. Obviously, the effect size scatters, which adds to the scattering of z at fixed effect size. This may seem to make it appear hopeless to search in statistical parapsychology for laws like those governing the natural sciences. Nevertheless, as in physics, measurements are taken and analyzed, and the data of the numerous investigated statistical psi effects could obey some rules. Unlike laws, these rules would allow for deviations and exceptions. Leaving aside the very rare reports on persons producing with some reliability a psi effect that far exceeds random noise, it may be worthwhile to think about a possible systematics behind the statistically detected psi effects.

The approach taken in the following is that of an applied physicist who wishes to orient himself in statistical parapsychology. In the next section, **Conjectured Rules**, I present a set of conjectured rules that possibly hold for all statistical psi effects, no matter whether it is psychokinesis (PK) or extrasensory perception (ESP). Only the data from hit-or-miss experiments with chance hit rates $p = \frac{1}{2}$ and $\frac{1}{4}$ are considered, because they are particularly numerous and allow a simple and reliable analysis that can be adapted to other psi experiments whose evaluations are similar. A number of experiments on the same psi effect make up a study. The rules are based mostly on meta-analyses covering many studies of the same kind. Each single step in an experiment is a decision between hit or miss, to be called a trial in the following. An experiment consists of an arbitrary predetermined number of trials down to one. Its separation from equal experiments in a

study will be of central importance in the formulation of the rules. Studies in which the separation is obviously unclear are disregarded.

Apart from the exceptions encountered in the **Calculations** section, the rules are formulated in terms of z-scores. The z-score of an experiment is defined as

$$z = \frac{k - pn}{\sqrt{p(1-p)n}}.$$

(1)

The nominator of the fraction is the deviation of the number k of hits from its expectation value pn, where p is the chance value of the hit rate and n the number of trials in the experiment, while the denominator is the standard deviation of k from pn. Averaging over a sufficient number N of equal psi experiments, to suppress scatter, it may be expected to result in a reasonably stable value of the actual hit rate

$$h = \sum_{j=1}^{j=N} k_j / (nN)$$

(2)

Using h, a mean value by definition, one may express the mean value of z by

$$<z> = \frac{h - p}{\sqrt{p(1-p)}} \sqrt{n}$$

(3)

The mean z-score is identical to Cohen's d that often, as in the present paper, serves as the definition of the effect size of an influence pushing z away from zero, its mean value for the null effect, that is in the absence of psi. The standard deviation of z from $<z>$ is taken to be that of the null effect, which is $<(z - <z>)^2> = 1$. It is in general augmented by a scattering of the effect size.[1]

In the **Conjectured Rules** section, apparently common properties of the statistical psi effects are sorted out from available data and the simplest possible rules for them are formulated. Calculations associated with the rules are assembled in the four sections under **Calculations**. The first subsection, ***Unlimited Definitions of the Effect Size***, addresses the problems that may arise from the fact that the hit rate h is restricted to the interval $0 \leq h \leq 1$. Because of Equation (3), this implies that the $<z>$-score is also limited, the

allowed range expanding with $\sqrt{n}$. Since the effect size per trial should in principle be unbounded, its standard definition by $<z>$ is likely to fail when $<z>$ is near one of its limits. The chance of h being so will be seen

to be greatest in one-trial experiments. In order to be able to deal with such cases, a definition of the effect size is desirable that is unlimited and at small enough values of $h - p$ merges with $< z >$. Two definitions satisfying this condition and based on different models of psi effects will be proposed to convert $< z >$ into a conjectured "true" effect size and vice versa.

The two subsections *Scattering at Small Effect Sizes per Trial* and *Scattering at Large Effect Sizes per Trial* deal with the scattering of the effect size, which seems typical of psi effects. I will distinguish between weak and strong effect size scattering. It is called weak when the limits of h do not enter the calculation of shift and widening and it only widens the z-score normal distribution without affecting the shift $< z >$, while it is called strong when the limits need to be taken into account. In the case of strong scattering, the primary quantity that can be measured and calculated is the averaged hit rate, h_{av}. It is understood as the integral over a new variable h_{qu} from $-\infty$ to $+\infty$ of the product of h and its probability density, both as functions of h_{qu}. The independent variable h_{qu} will be defined by extrapolating the case of weak effect sizes. The averaged mean z-score $< z >_{av}$ is calculated from the averaged hit rate h_{av} by means of Equation (3). Based on a small dataset, the quantitative treatment of scattering is speculative. Normal distributions of the conjectured effect size will be assumed as they are common in statistics and convenient in calculations. The only histograms of effect sizes I found in the literature more or less represent a normal distribution that is wider than that of the null effect and can be explained by weak scattering. Finally, I calculate the ratios of $< z >_{av}$ in the case of strong scattering (i.e. for n = 1) to $< z >$ in the quadratic approximation (i.e. for $n >> 1$), which in most of the examples considered are less than one. In the subsection *Comparisons with Experimental Data* under the main Section **Calculations**, two comparisons are made between ratios of experimentally determined $< z >$-scores and the ratios of the corresponding conjectured true effect sizes, and cautious conclusions are drawn from the results. The **Conclusions** section presents a discussion of the rules and an argument as to why the small size of statistical psi effects might make sense for philosophical reasons and, if so, can probably not be substantially increased.

In a previous paper by the author, it was argued that the sizes of all psi effects are roughly equal (Helfrich 2011). However, the difference between the $< z >$-scores of PK experiments on the binary random noise generator and dream-psi, both with $p = \frac{1}{2}$, was found to be so large (0.65 versus 0.182, see Rule 5 below) that it was tentatively attributed to the fact that the former are many-trial and the latter one-trial experiments. This guess is examined in the subsection *Comparisons with Experimental Data* under the main section **Calculations** of the present paper.

Conjectured Rules

For the sake of clarity, the rules are numbered. Of course, they could be differently arranged and in part decomposed or combined. They are based mainly on meta-analyses or reviews. The experimental results given here in support are in most cases not complete but a small number of examples.

Rule 1: The effect size is independent of spatial distances.

The size of psi effects does in principle not depend on the distance between participant and object or recipient and sender. There are PK studies on binary random number generators (RNGs) that show this for terrestrial distances (Dunne & Jahn 1992, 1995). The independence of distance has also been checked in ESP studies (Steinkamp 2005). In one of them a decrease of the effect size was found at large separations. No significant decrease with distance was noted in studies of remote viewing (Dunne & Jahn 2003).

Rule 2: The effect size is independent of temporal distances.

The size of psi effects does in principle not depend on differences in time between participant and object or recipient and sender. In their meta-analysis of precognition studies, Honorton and Ferrari (1989) found a dramatic decrease of the hit rate with increasing delay, which largely takes place within the first day. However, no such decay was observed in studies with selected participants. In the studies of Bem (2011), cards were guessed with the target being randomly selected only after the guess. This was interpreted as a retroactive psi effect, but PK as another possible explanation was not ruled out. In their PK studies on binary RNGs, Dunne and Jahn (1992, 1995) found no significant influence of the time shift between mental aiming and operating the RNG. The time of aiming varied from 73 hours before to 336 hours after the generation of the RNG data. The transition from PK to a kind of retroactive psi effect produced no significant break in the scatter plot of z-scores. An independence of temporal distances was also registered with remote viewing (Dunne & Jahn 2003).

Decreases of psi effects with increasing spatial and temporal distances are possibly due to a diminishing emotional relationship of the participant to the object or of the recipient to the sender. While the emotional relationship of a participant to an object is difficult to quantify by normal means, the decisive role of a close bond between sender and receiver was observed by Hinterberger (2008) who measured psi-induced physiological correlations at mostly large distances. Studies of the effect size as a function of distances and other parameters are much more demanding than proofs of existence of

a psi effect. This is because the number of experiments has to be divided among the data points and the error of differences combines the errors of two data points.

Rule 3: The effect size is independent of the number of participants.

It does not matter much how many persons take part in an experiment, actively or passively. To employ several senders or recipients is what most physicists and engineers tend to suggest first when being told how weak psi effects are. Evidently, the lack of success of such attempts is the reason why group experiments have early on ceased to be of interest. Disappointing group studies of precognition and ESP in general were mentioned by Honorton and Ferrari (1989) and Steinkamp (2005), respectively. Dunne and Jahn (1995) found in their PK experiments that the success of pairs of participants decreased when they were equal in gender but increased when they were opposite, as compared with the success of single participants. The effect size was found to be four times larger than that of single participants when the pairs of opposite gender were "bonded," as were seven pairs in this study.

Rule 4:   The effect size per experiment is independent of the predetermined number of trials in the experiments. This is on condition that on the one hand the experiments are closed, i.e. without breaks, and on the other hand clearly separated from equal or similar experiments.

The separation seems to be assured in two kinds of experiments consisting of a single trial. One of them is dream-psi (Sherwood & Roe 2003), where the temporal distance between experiments is at least a day. The other is ganzfeld-psi (Williams 2011), where the time interval between experiments is about an hour. This may not be long enough, but it seems that in addition usually the participant was changed between experiments. When an experiment consists of more than one trial, the conditions for Rule 4 seem to be well-satisfied if in a study each participant performs a single experiment that consists of a compact series of trials. Such sessions at a binary RNG that comprise roughly 20 to 50 trials are today the method of choice in many studies. In remote-viewing studies, which in addition to a recipient often involve an observer who may function as a sender, the temporal distance between experiments seems in general large enough for a clear separation, but a change of the participants would be safer. Interestingly, Baptista, Derakshani, and Tressoldi (2015) recommend that no more than one or two experiments of this kind should be carried out per day by the same

participant in order to avoid fatigue or boredom. Another problem with these studies, discussed in detail by Dunne and Jahn (2003), is bringing the perception tests into the shape of yes-or-no questions with known chance probabilities.

An indirect confirmation of Rule 4 is the change in the common definition of the effect size over the course of several decades. In the beginning, it referred to the single trials, regardless of their number in an experiment, whose *z*-scores were thought to be independent of *n*. The smallness and the extreme scatter of the mean *z*-score thus obtained gradually led to a redefinition. Today the effect size practically always refers to whole experiments. How strictly $<z>$ is independent of *n* has rarely been checked. An early form of Rule 4 put forward together with experimental confirmation is the data augmentation theory (DAT) of May, Utts, and Spottiswoode (1995). According to this theory, clairvoyance unconsciously recognizes and selects rows of trials of reduced entropy in PK experiments (May et al. 1995). Numerous references to the DAT model are given in a book edited by May and Marwaha (2015).

The most convincing confirmation of Rule 4 is provided by the meta-analyses of Radin and Nelson (1989, 2000) of PK experiments with binary RNGs. They took the data from about 150 English-language references including papers published in conference proceedings, thus collecting nearly 600 experiments. In the absence of psi, the RNGs produced zeroes and ones with equal probability. The aim of the psi experiments was to mentally influence the PC so that it generates more ones than zeroes or vice versa. A most remarkable feature of these studies is the enormous range of the number of trials per experiment reaching from about 20 to 10^8. The deviation in the nominator of Equation (1) is the number of hits minus $n/2$, its chance expectation value. Without the psi effect, the scattering of the *z*-score results in a standard normal distribution. For this so-called null effect, the standard deviation of the *z*-score is $\sigma_0 = 1$, and the expectation value $E(z) = 0$, which was checked and confirmed by a histogram of 200 such experiments. With a somewhat "smoothing" assumption, which of the assignment "insignificant" made a truncated null effect distribution, Radin and Nelson in their first meta-analysis of the PK effect (1989) found a normal distribution of *z*-scores. Its histogram is not only shifted to $<z>\ = 0.65$ but also widened by a factor $\alpha = 1.5$ with respect to the null effect. In addition, there are a few outliers, while none are visible in the null-effect histogram. They were partially suppressed by a homogenization before $<z>$ and α were calculated. (However, the differences between the values of $<z>$ and α calculated after homogenization and those taken directly from the histogram seem to be small.) Knowledge of $<z>$ and α allows the computing

of the entropic energies of displacement and widening, respectively. For the combination of < z > = 0.65 and α = 1.5, they turn out to be practically equal (Helfrich 2011). The energies will be derived again in the subsection *Scattering at Small Effect Sizes per Trial* in the **Calculations** section, and their equality generalized in the subsection *Scattering at Large Effect Sizes per Trial* in support of Preliminary Rule 6.

A problem of PK experiments with binary RNGs is the separation of the experiments from one another: "In general, within a given reviewed report, the largest possible aggregation of non-overlapping data collected under a single intentional aim was defined as the unit of analysis (hereafter called an experiment or study)" (Radin & Nelson 1989). One would like to know if interruptions like a pause or a change of participant were excluded in these experiments. They could cause a breakup into several separate experiments. With n' being the number of effective breaks, the z-score of an experiment increases by the factor $\sqrt{n'}$ according to Stouffer's formula (see next paragraph). Therefore, breaks could be a reason for the widening.

In their second meta-analysis which in addition contained 175 new or newly found experiments, Radin and Nelson (2000) cumulated the 258 z-scores taken from PEAR (Princeton Engineering Anomalies Research) into a single one, using Stouffer's formula, $z_{\mathrm{cum}} = \sum_{j=1}^{j=N} z_j / \sqrt{N}$. (This approximation becomes exact, apart from scattering, if $< z_j >$ can be taken to be the same for all experiments.) Was this done because decomposing the PEAR data into experiments was particularly difficult? In their second histogram of the PK effect, and in the histogram of Schub with a wider range of shown z-scores, the cumulated z-score is not marked and the indication "insignificant" is rendered simply by $z = 0$. Apart from the concentration of scores at $z = 0$ and a greatly increased roughness, there is little difference between the old and new histograms of Radin and Nelson.

The meta-analyses of Radin and Nelson (1989, 2000) were severely criticized by Bösch, Steinkamp, and Boller (2006a, 2000b) as well as by Schub (2006), who in their papers rejected its central result, an overwhelming proof of the existence of the PK effect. Radin et al. (2005a, 2005b) defended the result. In the opinion of critics, the shift of the normal distribution of hit numbers is due to a publication bias. They overlooked the fact that the widening and the outliers produced data points on both sides of the spectrum that independently of the shift drastically reduce the probability of obtaining Radin and Nelson's histogram of the psi effect by chance (Helfrich 2007).

How to be convinced that the mean shift $< z >$ is independent of the number n of trials per PK experiment at the binary RNG? First of all, the

huge range of n-values leaves little room for other inferences. A partition of 377 carefully selected experiments into four practically equal blocks according to the magnitude of n provides a kind of check (Bösch, Steinkamp, & Boller 2006b). With increasing n, the authors found the decreasing $<z>$-scores 1.05, 0.75, 0.56, and 0.41. The differences, though small, may be taken to mean that the widening found by Radin and Nelson results from a superposition of normal distributions centered at different $<z>$-scores. The reason for the maximum of $<z>$ at the smallest n could be a relatively large number of interrupted experiments. This seems paradoxical, but most of the experiments with small n probably took place at a time when computer technology was nonexistent or only at its beginnings.

It should be mentioned that three PK studies with an extremely high frequency of trials (2,000,000 per 0.2 sec, once every second) produced exceptional $<z> \approx -2$, which is three times larger than what is measured at the usual 200 trials per 0.2 sec, once every second, and of the wrong sign (Ibison 1998, Dobyns et al. 2004). These results are significant but in conflict with Rule 4.

The overall effect size of the PK effect on binary RNGs obtained by Radin and Nelson (1989, 2000) in their PK meta-analyses, $<z> = 0.65$, lies on the upper border of mean z-scores of psi experiments. However, the same value was found by Honorton and Ferrari (1989) in a meta-analysis of precognition experiments. The number of experiments covered was extremely large, but in contrast to the PK studies they were quite heterogeneous. An experiment was defined as the data measured between subsequent changes of the conditions. Again, the question arises if pauses or a change of the participant occurred within an experiment because a possible breakup into shorter experiments would have caused the measured $<z>$ to be above its true value. Like Radin and Nelson, the authors found an increase of the standard deviation by a factor α as an accompanying psi effect. Before a homogenization discarding 10% of the z-scores as outliers, they obtained $<z> = 0.65$ and $\alpha = 2.48$, thereafter $<z> = 0.38$ and $\alpha = 1.45$.

Rule 5: The effect size is equal for all psi effects. Its fluctuations among studies are about as large as the average size. (However, one of the unlimited definitions of the effect size to be proposed in the following predicts a dependence of $<z>$ on the chance hit rate p according to which $<z>$ has its maximum at $p = \frac{1}{2}$ and tends to zero for $p \to 0$ and $p \to 1$.)

All psi effects, at least those with the most common chance hit rates $p = \frac{1}{2}$ and $p = \frac{1}{4}$ have similar effect sizes $<z>$. They lie preferably in or near

the interval $0.2 < <z> < 0.3$. Values below 0.1 or above 0.8 are extremely rare. Such cases call for a check if perhaps the confidence interval reaches into the preferred range. The rule applies to all modifications of ESP and PK, including retroactivity. Between studies of the same type, $<z>$ can easily change by a factor of 2 or more, covering altogether a range whose boundaries differ by a factor of 4. In general, the limits of the confidence interval are placed at $z - <z> = \pm 1.96 \, \sigma_0$, so that the integral of the chance probability density over one or two tails of the normal distribution outside this range equals 0.025 or 0.05, respectively. In proofs of existence of a psi effect, these are the limits of significance.

Rule 5 is based on numerous meta-analyses, especially those of Radin and Nelson (1989, 2000), Honorton and Ferrari (1989), Dunne and Jahn (2003), Sherwood and Roe (2003), Williams (2011), Baptista, Derakshani, and Tressoldi (2015), Utts et al. (2010), Schmidt (2012), and Mossbridge, Tressoldi, and Utts (2012). The two last-mentioned meta-analyses deal with unconscious, physiologically detected psi effects, whose statistical evaluation was more complicated than the simple hit-or-miss scheme. We also use a comprehensive article by Bem (2011) as a source of data, even though it is not a meta-analysis. It describes nine studies, each with usually about 100 participants, of various retroactive psi effects, that were guessing tasks with the targets being randomly selected after the guessing. A recent meta-analysis by Bem et al. (2015) covers these studies and, as a check, 81 similar ones. According to the authors, the effect sizes of the additional studies as a whole, are smaller than or practically equal to Bem's values, depending on the method of analysis. I do not further discuss them because not all of them are based on hit-or-miss trials and effect sizes are expressed in a different measure (Hedge's *g*).

Moreover, it appears appropriate to include the *z*-score of Nelson's Global Consciousness Project (Nelson 2001, Nelson et al. 2002). The *z*-scores z_{gcp} are expressed in terms of a sum of the type

$$\sum_{i=1}^{n} (z_i^2 - 1) / \sqrt{<(z^2 - 1)^2 > M} = \sum_{i=1}^{M} (z_i^2 - 1) / \sqrt{2M},$$

where M is a very large number that increases with the time elapsed since the start of the experiment. On the basis of 500 experiments, Nelson reports $<z_{gcp}> = 0.3269$, which is in the typical range of effect sizes (as of December 2015, see GCP updates online, noosphere.princeton.edu). In these equations, z_i and z designate measured and chance values, respectively, the average being taken over the latter.

In their meta-analysis of dream-psi, Sherwood and Roe (2003) distinguish two periods. The first comprises the experiments carried out at

Maimonides Medical Center Brooklyn from 1962 to 1978. It is characterized by many exploratory experiments and the preferred use of telepathy as ESP channel. The hit rate of the 450 experiments was 63% instead of a 50% chance probability. The second period, called post-Maimonides, lasted from 1977 to 2007. The 820 additional experiments differed in location and method, and their effect size was smaller than that of the first period. In some cases, a large number of recipients simultaneously received the same dream content by telepathy from the same sender. The number of experiments was equated to that of the number of recipients. However, according to Rule 4, it should rather be one because there was a single sender. For this reason, we prefer $<z> = 0.26$, the value of the first period, over $<z> = 0.182$, the value calculated by Radin (2006) for the total of 1,270 experiments.

The $<z>$-scores of Bem's studies (2011) varied within the range given above. In most studies, a test consisting of two questions distinguished between stimulus-seeking and other participants. The $<z>$-scores were computed for both groups and for the total of participants in a study. The stimulus-seekers were clearly more successful than the others, the averaged effect sizes of the groups being $<z> = 0.43$ and 0.10, respectively. The overall effect size was $<z> = 0.22$.

How to optimize psi effects with respect to size and replicability is the main subject of a meta-analysis of Baptista, Derakshani, and Tressoldi (2015). They consider ganzfeld-psi, card guessing, remote viewing, and dream-psi. The most important precondition for large effect sizes appears to be selection of the participants. Belief in the existence of psi effects, experience with psi experiments, success in previous such experiments, and training in meditation all are helpful. The $<z>$-scores of Bem's studies show that being a stimulus-seeking person can be enough to achieve above-average effect sizes. The aforementioned small but significant decrease of $<z>$ with increasing n, as noted by Bösch, Steinkamp, and Boller (2006b) in the data of Radin and Nelson, could be explained not only by breaks but alternatively (and less likely) on the basis of Rule 5 by a predominance of enthusiasm in the shorter, early PK experiments and a predominance of routine in the longer, later ones.

Preliminary Rule 6: The z-scores of very large numbers of experiments carried out by different groups and over a long period of time tend to end up in normal distributions.

The size of psi effects is not constant but undergoes fluctuations from study to study. Ganzfeld-psi represents a well-investigated example of the type $n = 1$, as demonstrated, e.g., by the meta-analyses by Williams

(2011) and Baptista, Derakshani, and Tressoldi (2015). The $< z >$-scores of these studies more often are outside the limits of confidence of similar studies than would be expected on the basis of null-effect scattering. The easiest way of recognizing fluctuations of the effect size is to look at the experimental standard deviation σ of z which in their presence exceeds that of the null effect, i.e. $\sigma > \sigma_0 = 1$. If there is a widening of the standard deviation, additional effort is required to gain information on the effect-size distribution causing it. Although indications of a scattering of the effect size have often been observed, there seem to be no systematic investigations of the affected z-score distribution functions, apart from Radin and Nelson's (1989, 2000) meta-analyses and Schub's critique thereof.

In the subsections ***Scattering at Large Effect Sizes per Trial*** and ***Comparisons with Experimental Data*** in the **Calculations** section, I will presuppose normal distributions of the scattered effect size, thereby permitting a lowering of the effect size by homogenization and elimination of outliers. Calculations with an acceptable effort are possible only with normal distributions. From the experimental point of view, the assumption that they are at least reasonable approximations can be inferred only from Radin and Nelson's meta-analyses, the criticisms of which have been pointed out above. The same applies to the assumption that the energies of shifting and widening the distribution of the z-scores are equal or proportional to one another.

Calculations

Unlimited Definitions of Effect Size

The starting point of all calculations is the binomial distribution. I consider n equal trials of the same chance hit rate p. It does not matter at this point whether they belong to a single experiment with n trials or a series of n equal one-trial experiments. The possible total numbers of hits are $k = 0, 1$ $2, \ldots \ldots, n$. The probability of exactly k hits may be expressed by the term $B_{n,p}(k)$ of a binomial distribution:

$$B_{n,p}(k) = \frac{n!}{k!(n-k)!} p^k (1-p)^{n-k} \tag{4}$$

The sum of these terms over k satisfies for all n

$$\sum_{k=0}^{n} = \frac{n!}{k!(n-k)!} p^k (1-p)^{n-k} = 1. \tag{5}$$

It is, as may be said, normalized to unity.

The expectation value of the hit number is np. The term $k = np$ (or the one next to np) is the term with the largest probability. According to the DeMoivre-Laplace theorem, the binomial distribution asymptotically approaches, for $n \to \infty$ but some fixed x in $k = np + x\sqrt{np(1-p)}$ a normal distribution of the probability density

$$w_{n,p}(k) = \frac{1}{\sqrt{2\pi p(1-p)n}} \exp\left[-\frac{(k-np)^2}{2p(1-p)n} \right]. \tag{6}$$

Its integral over k is equal to unity. This holds exactly only if the integral reaches from $-\infty$ to $+\infty$. In the present case it is restricted to the interval $0 \le k \le n$. Equation (6) can also be read as a discrete probability function, the sum over all integers k tending to unity for $n \to \infty$. Insertion of Equation (1) into Equation (6) leads to the standard form of the normal distribution

$$w(z) = \frac{1}{\sqrt{2\pi}} \exp(-z^2 / 2), \tag{7}$$

where z is usually regarded as a continuous variable.

The chance probability of obtaining, at the chance hit rate p with n trials nh hits, is given by $B_{n,p}(nh)$. The quantity of interest is the ratio $Q_{n,p}(h)$ of the probabilities of this state to that of the ground state, i.e. the most probable state. All states $B_{n,p'}(nh')$ with arbitrary $0 < h' < 1$ lend themselves as ground states. The probabilities of these ground states are not exactly equal but differ by the factor $1/\sqrt{h'(1-h')}$. The natural choice may seem to be $h' = p$ so that ground state and excited state, i.e. state of lower probability, have the same chance hit rate p. However, the more convenient choice is $h' = h$ because then all the factorials cancel each other. This leads immediately to

$$\frac{B_{n,p}(nh)}{B_{n,h}(nh)} = \frac{p^{nh}(1-p)^{n(1-h)}}{h^{nh}(1-h)^{n(1-h)}}. \tag{8}$$

It does not matter that the binomial terms $B_{n,p}(nh)B_{n,h}(nh)$ belong to different binomial series, because the sum of each series is normalized to unity. Taking logarithms, one may write

$$\ln \frac{B_{n,p}(nh)}{B_{n,h}(nh)} = -n\eta(p,h), \tag{9}$$

where

$$\eta(p,h) = (1-h)\ln \frac{1-h}{1-p} + h\ln \frac{h}{p}. \tag{10}$$

This function of h and p has its minimum at $h = p$ with $\eta(p,p) = 0$. If one chooses $B_{n,p}(np)$ instead of $B_{n,h}(nh)$ as the ground state, one has to add on the right side of Equation (9) the term $(1/2)\ln\{p(1-p)\,/\,[h\,(1-h)]\}$. This follows from Stirling's formula applied to $B_{n,h}(nh)\,/\,B_{n,p}(np)$ as well as from the factors preceding the exponentials of the probability normal distributions. The additional term does not depend on n, thus being negligible at large n. It disappears if instead of the probability ratios between the individual states of maximum probability one considers the ratios of the products of this probability and the respective standard deviation (or a fraction thereof). Moreover, it is completely avoided when Equation (10) is deduced on the basis of a single-trial approach, as will be done below (see Equation (18)). Equation (10) without the second term can also be derived in terms of the physics of the isothermal ideal gas[2]. In a slightly modified form, it is a special case of the Kullback-Leibler distance or relative entropy. Accordingly, the formula of the probability ratio to be employed in the following is

$$Q_{n,p}(h) = \exp[-n\boldsymbol{\eta}(p,h)\} \tag{11}$$

In statistical thermodynamics, the probability of a state being occupied is proportional to the Boltzmann factor, another exponential function. Its exponent is $-E/k_B T$, where E is the energy of the state, k_B Boltzmann's constant, and T the absolute temperature. Obviously, $-\ln\eta(p,h)$ may be interpreted as the entropic free energy of a state minus that of the ground state, both per trial and divided by $k_B T$. A temperature dependence of psi effects is not known. If $E/k_B T$ is independent of temperature, E must be proportional to $k_B T$. For convenience, $\eta(p,h)$ will sometimes simply be called energy.

The function $\eta(p,h)$ represents the energy per trial required to bring the hit rate from the chance value p to the actual value h. It can be expanded into a power series of $(h-p)$. Omitting the terms of higher than quadratic order in $(h-p)$, one obtains

$$\eta_{qu}(p,h) = \frac{(h-p)^2}{2p(1-p)}, \tag{12}$$

i.e. the quadratic approximation of $\eta(p,h)$. Comparison of Equation (12) with Equation (1) leads to

$$\eta(p,h) = \frac{1}{2} < z_{n=1} >^2, \tag{13}$$

the subscript $n = 1$ indicating one-trial experiments. The function $\eta(p,h)$ is defined only in the interval $0 \le h \le 1$ where it is finite everywhere. However, the derivatives of $\eta(p,h)$ with respect to h diverge at the limits of h. The first two derivatives are

$$d\eta(p,h) / dh = \ln[h(1-p) / p(1-h)], \tag{14}$$

and

$$d^2\eta(p,h) / dh^2 = 1 / h(1-h). \tag{15}$$

Also of interest will be the first derivative of $\eta_{qu}(p,h)$

$$\begin{aligned} d\eta_{qu}(p,h) / dh &= (h-p) / p(1-p) \\ &= \; < z_{n=1} > / \sqrt{p(1-p)}. \end{aligned} \tag{16}$$

For small enough $|h - p|$, the functions $\eta_{qu}(p,h)$ and $\eta(p,h)$ are practically identical. The definition of the effect size by $<z_{n=1}>$ becomes questionable to the extent that $\eta(p,h)$ and its quadratic approximation $\eta_{qu}(p,h)$ differ from each other. Any redefined effect size should be unlimited but merge with the quadratic approximation at small sizes. Two modified definitions of the effect size satisfying these requirements are proposed next. They are based on physically inspired concepts of the psi effects that might be called field model and momentum model.

Beginning with the field model, let me imagine the psi effect to be caused by the psi field

$$\kappa(p,h) = \frac{d\eta(p,h)}{dh}, \tag{17}$$

In equilibrium, a psi field $\kappa > 0$ shifts the minimum of the total one-trial energy $\eta(p,h) - \kappa(p,h)h$ from $h = p$ to some $h > p$. The quantity $\kappa(p,h)$ is reminiscent of a physical force. However, a force can be defined as the negative derivative of energy with respect to length, while $\kappa(p,h)$ is the positive derivative with respect to hit rate. The dimension of $\kappa(p,h)$ is again energy in units of k_BT as h is a dimensionless quantity. Its meaning becomes apparent by expanding the fraction in Equation (17) by a sufficient number n of trials and writing the result as an equation of differences, $\Delta(n\eta(p,h)) = \kappa(p,h)\,\Delta(nh)$. The number of hits must be a natural number between 0 and n. A decrease of this number by 1, i.e. $\Delta(nh) = 1$, is accompanied by the release of the energy $\kappa(p,h)$ which at equilibrium is exactly what is absorbed by the system when a miss is converted into a hit. The fact that κ is a released energy implies that the ratio of the probability of a trial being a hit to that of being a miss or, in other words, the rate of hits to the rate of misses, is $pe^\kappa/(1-p)$, where $e^\kappa = \exp[-(-\kappa)]$ represents the "Boltzmann factor" of the energy, $-\kappa(p,h)$. This results in the following formula for h:

$$h(p,\kappa) = pe^\kappa / [pe^\kappa + (1-p)] = 1/(1 + \frac{1-p}{p}e^{-\kappa}). \qquad (18)$$

Solving Equation (18) for $\kappa(p,h)$ that is subsequently substituted by Equation (17), leads, in fact, back to Equation (14) and finally Equation (10). The last form of Equation (18) serves to show that it is easier to compute h as a function of κ than the other way round.

The limited hit rate h is a one-to-one function of unlimited $\kappa(p,h)$ or $\sqrt{p(1-p)}\kappa(p,h)$ that merges with its quadratic approximation (see Figure 1 below). In the quadratic approximation, the psi field $\kappa(p,h)$ takes the form

$$\kappa(p,h) = \frac{d\eta(p,h)}{dh} = \frac{h-p}{p(1-p)} = \; <z_{n=1}> /\sqrt{p(1-p)} \qquad (19)$$

that is bounded because of the limits of h. In an x/y plot, the psi field shifts the parabolic potential $\eta_{qu}(p,h)$ over the (horizontal) distance $(h-p)$, thereby lowering without deforming it.

The effect size can be expressed by $\sqrt{p(1-p)}\kappa(p,h)$ or by the field $\kappa(p,h)$. Although direct use of the psi field κ as effect size may seem attractive (and is made in Figure 1), practical reasons argue for keeping $\sqrt{p(1-p)}\kappa(p,h)$, as long as it is not known which of the two variants, if any, is the correct one. Otherwise, all effect sizes $<z>$ reported in the literature would have to be magnified by the factor $1/\sqrt{p(1-p)}$ to convert them into κ.
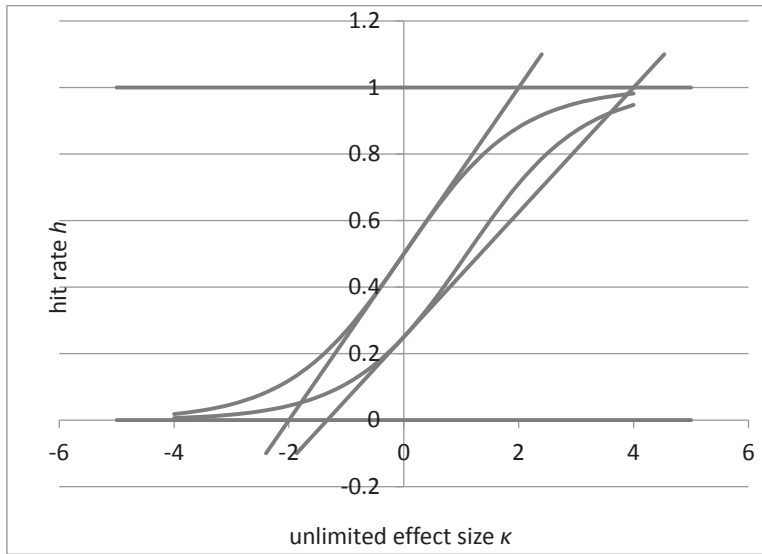
Figure 1. Hit rate *h* as a function of the psi field $\kappa = d\eta(p,h)/dh$. The curved lines represent the dependence of *h* on κ when the exact energy $\eta(p,h)$ is used. In the approximate calculations, each curved line is replaced by three pieces of straight lines. The central one of them derives from the quadratic approximation $\eta_{qu}(p,h)$ of the exact energy, the horizontal ones represent the limiting hit rates 0 and 1. The two structures refer to $p = \frac{1}{2}$ (left) and $p = \frac{1}{4}$ (right). The psi field κ equals $(h_{qu} - p) / p(1 - p)$ (see main text).

A more comfortable alternative would be to use $\kappa/2$ as the new effect size. The correction factor would then be $1/[2\sqrt{p(1-p)}]$, so that the numbers do not change for $p = \frac{1}{2}$, the most often investigated case. Whenever $p \neq \frac{1}{2}$, both κ or $\kappa/2$ are larger by this factor than their values at $p = \frac{1}{2}$. For $p = \frac{1}{4}$, the factor is 1.15.

In the other model allowing effect sizes of unlimited size, the psi effect is caused by a momentum *s* that is the new effect size. It could be carried by a particle with the kinetic energy $s^2/2m$ hitting the system at a particular trial. The mass *m* is equated to unity so that the maximum transferable energy is $\frac{1}{2} s^2$. At the beginning, the system is thought to be in the ground state with the potential $\eta(p,p) = 0$. The momentum excites the *z*-score of the trial in the direction of its sign and is assumed to be completely absorbed by the system if the hit rate associated with the energy does not exceed the limits $h = 1$ or $h = 0$. Within this range, *s* is defined by the equation

$$s = \pm\sqrt{2\eta(p,h)} \ . \tag{20}$$

If the quadratic approximation holds, the momentum obeys

$$\frac{1}{2}s^2 = \eta_{\text{}}(p,h) = \frac{(h-p)^2}{2p(1-p)} = \frac{1}{2} < z_{n=1} >^2 \tag{21}$$

Beyond its mergers with one of the limits of h, $h(p,s)$ is assumed to continue on one of the straight lines representing $h = 1$ or 0, depending on whether s increases or decreases, respectively (see Figure 2 below). This implies trimming away any parts of $h(p,s)$ that exceed the limits of h. A questionable simplification of the momentum model is the disregard of the null-effect fluctuations. Despite the apparent superiority of the field model, I will continue to consider both models, because it is an entirely open question how psi works. Neither model removes the mystery from psi, they only move it to an earlier moment in the chain of events.

When could it be necessary to go beyond the quadratic approximation? Probably only when the hit rate h associated with κ or s is near its limits at 1 and 0. From now on, I will distinguish between the number of trials, n, and the number of experiments, N. The condition just stipulated is certainly not satisfied by experiments consisting of many trials. According to Rule 4, the effect size per experiment, $< z >$, may be expected to break up into an effect size per trial of $< z > / \sqrt{n}$, a quantity that rapidly decreases with the number of trials. A rise of n from 1 to 2 already makes a great difference. Therefore, the limits of h interfere the most in one-trial experiments, the only case of low n to be considered in the following. Incidentally, fields and momenta varying with $1 / \sqrt{n}$ result automatically if the total values are decomposed into n equal components in n-dimensional space. This might be interpreted as a physical explanation of Rule 4.

The dependences of the hit rate on the effect sizes κ and s in the case $n = 1$ are shown in the two figures for the two most common chance hit rates, $p = \frac{1}{2}$ and $p = \frac{1}{4}$. Figure 1 depicts the functions $h(p,\kappa)$, their curvilinear plots approaching the limits of h without ever reaching them. Figure 2 depicts the functions $h(p,s)$. Their plots are curvilinear as long as the energy $\eta(p,s)$ associated with s can be fully absorbed by the system. They are assumed to change to the horizontal straight lines representing the limits of h at 1 and 0 where they merge with them rather abruptly. This is the basic version of the momentum model; two more complicated variants will be briefly considered in the subsection ***Scattering at Large Effect Sizes per Trial***. For small $|h{-}p|$
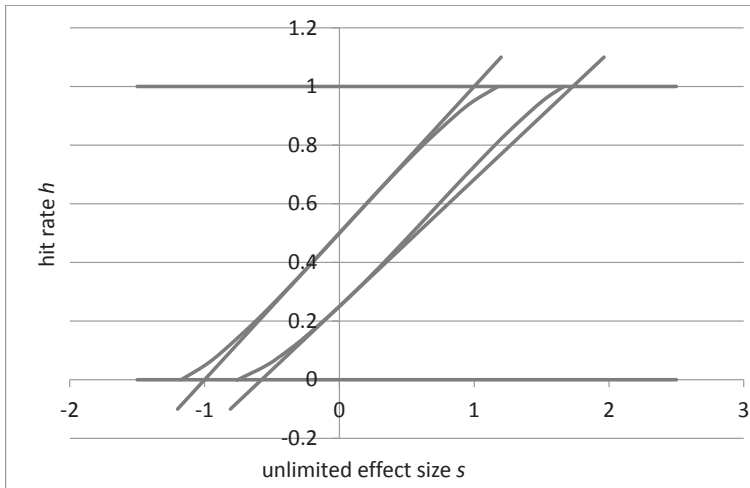
Figure 2. **Hit rate *h* as a function of the psi momentum *s*, obtained by plotting** $s(p,h) = \sqrt{2\eta(p,h)}$ **as a function of** *h*. The curved lines represent the dependence of *h* on *s* when the exact energy $\eta(p,h)$ is used. Their mergers with the horizontal lines $h = 1$ and $h = -1$ are too abrupt to be resolved in the Figure. The straight-lined approximations of $h(p,s)$ correspond to those in Figure 1. The two structures refer to $p = \frac{1}{2}$ (left) and $p = \frac{1}{4}$ (right). The psi momentum *s* equals $(h_{qu} - p) / \sqrt{p(1-p)}$ (see main text).

the curvilinear functions are seen to merge with the corresponding linear dependences of the quadratic approximation. According to Figures 1 and 2, it is only near the limits of *h* that the new effect sizes deviate appreciably from $< z_{n=1} >$, the measured mean *z*-score. For $p = \frac{1}{2}$, the deviation in the field model is circa +15% at $(h - p) = 0.3$ and circa +40% at $(h - p) = 0.4$, in the momentum model it hardly exceeds +10%. For $p = \frac{1}{4}$ in the field model it may be negative, reaching circa −20% at $(h - p) = 0.5$, but from there it rises to the positive side of $< z_{n=1} >$. The positive or negative deviations in the momentum model are in general smaller than those in the field model. However, beyond the mergers the hit rate $h(p,s)$ does not respond to further increases of the effect size, while in the field model the limited hit rate is a one-to-one function of the unlimited effect size.

The effect sizes of most one-trial experiments reported in the literature are so small that to a good approximation they can be expressed by $< z_{n=1} >$ as measured. However, a substantial downward or upward deviation of the conjectured effect size from $< z_{n=1} >$ is still possible if the effect size scatters so widely that part of its spectrum lies outside the range of validity of the

quadratic approximation. Dealing with the scattering of the effect size is the next and final task. Two cases will be distinguished: weak scattering that can be treated within the quadratic approximation and strong scattering that cannot.

The scattering of the effect size is probably composed of three parts. In the first place, the psi-ability or psi-sensitivity has been found to vary considerably among participants. Also, personal sensitivity can change over the course of time and with the circumstances. The investigators and checkers may exert another influence. In addition, there may be fluctuations of the effect size caused by external influences that do not depend on the persons involved and may be inexplicable. Technical irregularities can arise from errors in the counting of the experiments. A clear distinction of these sources is not possible. Therefore, the total scattering will be represented by normal distributions in the following calculations.

Scattering at Small Effect Sizes per Trial

The meta-analyses of Radin and Nelson (1989, 2000) start from the standard normal distribution of *z*-scores for the null effect at large *n*, as described by Equation (7). With a simple mathematical ansatz, one can reproduce the unintended widening of this distribution by a factor α that emerges in the meta-analysis of the PK effect in addition to the intended shift. It is sufficient to assume that the effect size scatters and that the scattering obeys a normal distribution (Helfrich 2011)

$$w(\zeta) = \frac{1}{\tau\sqrt{2\pi}} \exp(-\zeta^2 / 2\tau^2), \tag{22}$$

where ζ is the variable part of the effect size expressed in units of *z*. Combining Equation (7), i.e. the standard normal distribution associated with the scattering of the null effect, and Equation (22), one obtains the normal distribution

$$\begin{aligned} w(z) &= \int_{-\infty}^{\infty} \frac{1}{2\pi\tau} \exp[-\frac{(z-<z>-\zeta^2)}{2} - \frac{\zeta^2}{2\tau^2}] d\zeta \\ &= \frac{1}{\sqrt{2\pi(1+\tau^2)}} \exp[-\frac{(z-<z>)^2}{2(1+\tau^2)}], \end{aligned} \tag{23}$$

The standard deviation of effect-size scattering, τ, is still unknown, but comparison of Equation (23) with the result of Nelson and Radin's meta-analysis immediately leads to

$$\alpha^2 = 1 + \tau^2. \tag{24}$$

Insertion of $\alpha = 1.5$ yields $\tau = 1.18$. Evidently, the normally distributed effect size scattering does not affect the measured values of $< z >$ and h, despite the fact that they are averages of mean values in its presence.

How large is the probability ratio $\varphi(< z >, \alpha)$ per experiment for the transition from distribution Equation (7) to distribution Equation (23)? The probability ratio of a transition from $z = 0$ to a particular z-score in the widened distribution is $\alpha \exp(-(z^2 - 1) / 2)$. The factor $\alpha > 1$ takes into account that more states, i.e. k-values, are available per standard deviation in the new than in the old distribution, provided the z-scale is retained. The z^2 term is averaged over the new distribution and $< z^2 >$ substituted by means of the well-known relationship $\alpha^2 = < z^2 > - < z >^2$. The result is

$$\varphi(<z>, a) = \exp[\frac{\ln \alpha^2 - (\alpha^2 - 1) - < z >^2}{2}]. \tag{25}$$

The number 1 in the nominator of the exponent ensures that $\varphi(<z>,\alpha) = 1$ if new and old distributions are identical. Evidently, the last term in the exponent is the energy of shifting the normal distribution, while the preceding terms represent the energy of its widening. Both are, of course, energies in units of $k_B T$. They happen to be practically equal for the combination of $< z > = 0.65$ and $\alpha = 1.5$, the values of PK on binary RNGs taken from Radin and Nelson's (1989) meta-analysis. Insertion into Equation (25) yields $< z >^2 / 2 = 0.22$ and $[\ln\alpha^2 - (\alpha^2 - 1)] / 2 = 0.21$. Exact equality means

$$(a^2 - 1) - \ln \alpha^2 = < z >^2. \tag{26}$$

The derivation of the left side of this equation with respect to the widening $(\alpha - 1)$ at the point where both sides of Equation (26) are zero leads to the linear relationship

$$(\alpha - 1) = < z > / \sqrt{2}, \tag{27}$$

Over a surprisingly wide range of $< z >$, it is a good approximation to Equation (26), the deviation of $(\alpha - 1)$ as calculated from Equation (27) relative to the value obtained from Equation (26) reaching hardly +5% at $< z > = 1$. One may wonder whether widening and shift, i.e. $(\alpha - 1)$ and $< z >$, are equal or proportional to each other rather than the associated energies.

Another question is whether τ should perhaps be 1 instead of 1.18, so that the standard deviations of null-effect and effect-size scattering are equal. This would be exactly valid, e.g., for the combination of $<z> = 0.65$ and $\alpha = 1.46$. The uncertainties of the reported experimental data, including those related to homogenization, allow for many hypotheses. Additional problems may arise if according to some external criterion the participants in a study or meta-analysis can be divided into groups with different $<z>$-scores. In dealing with the scattering of large effect sizes, I will ignore all of these possibilities and adhere to the assumption that the energies of shifting and widening the normal distribution of the null effect are equal.

Scattering at Large Effect Sizes per Trial

The aim of these especially speculative and approximate calculations is to predict the averaged hit rate h_{av} and the averaged mean z-score $<z>_{av}$ computed from h_{av} by means of Equation (3). The subscript av serves to distinguish these theoretical numbers from measured hit rates and effect sizes. For this purpose, an idea of the statistics governing the effect sizes κ and s is needed: Let me simply assume the quadratic approximation to continue beyond the limits of h, calling the new, unlimited variable h_{qu}. The approach seems plausible because externally controlled psi fields or momenta should not depend on the properties of the system on which they act. For the reasons given above, only one-trial experiments will be considered. The associated energy is

$$\eta_{qu}(p, h_{qu}) = \frac{(h_{qu} - p)^2}{2p(1-p)} \ .$$

(28)

The independent variable h_{qu} practically coincides with h as long as the quadratic approximation is applicable. According to Equation (28), the conjectured psi field is

$$= \frac{d_{qu}(p, h_{qu})}{dh_{qu}} = \frac{h_{qu} \quad p}{p(1 \quad p)}$$

(29)

while the conjectured psi momentum is

$$s = \sqrt{2\eta_{qu}(p, h_{qu})} = \frac{h_{qu} - p}{\sqrt{p(1-p)}}.$$

(30)

Note the linear relationships of h_{qu} with κ and s. The new variable, and thus κ and s, are thought to be normally distributed. The central value h_0 of the h_{qu} distribution can be determined experimentally, if it is possible to do equal or similar many-trial experiments with n so large that the quadratic approximation holds. According to Rule 4 the effect sizes of one-trial and many-trial experiments should be equal, which implies $h_0 - p = < z_{n \gg 1} > \sqrt{p(1-p)}$. In analogy to Equation (23), the probability density of h_{qu} is expressed by

$$w_p(h_{qu}) = \frac{1}{\tau\sqrt{2\pi p(1-p)}} \exp[-\frac{(h_{qu} - h_0)^2}{2p(1-p)\tau^2}]. \qquad (31)$$

The averaged hit rate h_{av} is the integral over h_{qu} from $-\infty$ to $+\infty$ of the product of this function and $h(p,\kappa)$ or $h(p,s)$. While $h(p,\kappa)$ is rendered in explicit form by Equation (18), there is no explicit form of the function $h(p,s)$. The actual hit rate as a function of h_{qu} will be designated $h_p(h_{qu})$, the model to which it applies following from the context.

In view of the speculative character of the models and the uncertainties of the experimental data, it seems reasonable in a first, approximate calculation of the averaged hit rate to prefer transparency over mathematical rigor. Therefore, $h(p,\kappa)$ and $h(p,s)$ are replaced by the three straight sections representing the straight-lined approximations of the models (see Figure 1 and Figure 2). Coming from $h_{qu} = -\infty$ on the straight line $h = 0$, one changes at the intersections to the straight lines of the quadratic approximation and from there to the line $h = 1$ on which one continues up to $h_{qu} = +\infty$. The resulting function is $h = h_{qu}$ in the interval $0 \leq h_{qu} \leq 1$, while it is 0 for $h_{qu} \leq 0$ and 1 for $h_{qu} \geq 1$. The functions $h(p,s)$ of the basic version of the momentum model deviate only in their curvilinear parts from this approximation, while the functions $h(p,\kappa)$ differ everywhere.

In the case of the momentum model, one has to make assumptions on how to deal with the momenta that cannot be fully absorbed by the system. Three simple choices are to be considered: Excessive values of h_{qu} are either trimmed off to the next absorbable value, thus becoming $h = 0$ or 1, as was done above to define the basic version of this model, or they are lost and the loss is compensated so that the integral of the probability density remains unity. Trimming is the simplest method and more compatible with physics than the other two. Compensation is achieved by assuming for the lost parts of the spectrum the value of the null effect, $h = p$, or by multiplying what is left of the spectrum by a renormalization factor. The two variants

of the momentum model with lost but compensated excessive momenta are included because they yield the strong reduction of $< z >$ in single-trial experiments that was originally inferred from the experimental data. However, they require modifications of the function $h(p,s)$ representing the basic, i.e. trimmed, version of this model. If the null effect serves as compensation, h cannot remain at the values 1 or 0 once these limits are attained. Instead, both values have to be substituted by p beyond the mergers or, in the rectilineaer approximation, intersections with the lines $h = 1$ or 0. Such a breakdown of the psi effect beyond its extrema would mean that excessive momenta pass the system without leaving a trace. While this appears unlikely, it cannot be entirely ruled out on the basis of presently available data. The renormalization variant requires modifications even less acceptable from the physics point of view.

Three integrals of the probability density, Equation (31), which can be regarded as areas, are needed for the calculation of h_{av}, the averaged hit rate. The areas between $h_{qu} = -\infty$ and $h_{qu} = 0$, $h_{qu} = 0$ and $h_{qu} = 1$, $h_{qu} = 1$ and $h_{qu} = +\infty$ will be called A_L, A_M, A_R, respectively. For instance,

$$A_R = \int_1^\infty \frac{1}{\tau\sqrt{2\pi(1-p)p}} \exp[-\frac{(h_{qu} - h_0)^2}{2(1-p)p\tau^2}]dh_{qu}. \qquad (32)$$

Of course, the sum of the three areas equals unity. The integrals A_L and A_R are the probabilities of the hit rates $h = 0$ and $h = 1$, respectively. The integral of the product of h_{qu} and the probability density, Equation (31), from $h_{qu} = 0$ to $h_{qu} = 1$,

$$h_M = \int_0^1 h_{qu} \frac{1}{\tau\sqrt{2\pi(1-p)p}} \exp[-\frac{(h_{qu} - h_0)^2}{2(1-p)p\ \tau^2}]dh_{qu}, \qquad (33)$$

is the (unrenormalized) contribution to h_{av} of the interval between $h = 0$ and $h = 1$. An elementary integration yields

$$h_M = -\sqrt{\frac{p(1-p)\tau^2}{2\pi}} \left\{ \exp[-\frac{(1-h_0)^2}{2p(1-p)\tau^2}] - \exp[-\frac{h_0^2}{2p(1-p)\tau^2}] \right\} + h_0 A_M. \qquad (34)$$

Combining the contributions, one obtains in the field model and the momentum model with trimming the averaged hit rate

$$h_{av} = h_M + A_R, \qquad (35)$$

where the second term represents the contribution of the straight line $h = 1$. In the variants of the momentum model where excessive momenta are lost, one finds

$$h_{av} = h_M + p(1 - A_M), \tag{36}$$

if the loss is compensated by the null effect, and

$$h_{av} = h_M / A_M , \tag{37}$$

if it is compensated by renormalization.

Two sets of averaged hit rates calculated from Equations (35) to (37) and ratios $R = \dfrac{h_{av} - p}{h_0 - p} = \dfrac{<z_{n=1}>_{av}}{<z_{n\gg1}>}$ where $<z_{n=1}>_{av} = (h_{av} - p) / \sqrt{p(1-p)}$ is the averaged $<z>$-score, are listed in Table 1. The ratio R serves

as the correction factor of $<z_{n\gg1}>$ that brings it down (or up) to $<z_{n=1}>_{av}$, it is unity in the quadratic approximation. The calculations refer to the value pairs $<z> = 0.65$ with $\alpha = 1.5$ measured at $n \gg 1$ and $<z> = 0.4066$ with $\alpha = 1.3$. The first pair nearly and the second one exactly satisfy Equation (26). Only positive $<z>$-scores at the chance hit rates $p = \frac{1}{2}$ and $\frac{1}{4}$ are considered. For $p = \frac{1}{4}$, the corrections are little, i.e. R remains close to unity. There are small reductions (R < 1) for $<z> = 0.65$ and small enhancements (R > 1) for $<z> = 0.4066$. The latter are due to the fact that for $h_0 < 0.5$, a larger part of the scattering spectrum lies on the left of the range $0 < h_{qu} < 1$ than on the right. More interesting are the results for $p = \frac{1}{2}$, where R is generally reduced, apparently tending to unity with decreasing $(h_0 - p)$. The reductions listed in Table 1 are particularly distinct in the case $<z> = 0.65$, ranging from 0.60 in the approximate field model and the momentum model with trimming to R = 0.13 in the momentum model with compensation by the null effect.

In the field model, the hit rate $h_p(h_{qu})$ deviates markedly from the straight-lined approximation. Therefore, exact integrations over h_{qu} of the products $w_p(h_{qu})h_p(h_{qu})$ were done in addition (by online integration). The values of h_{av} and R thus calculated are in the last column of Table 1, next to those obtained for the field model and the trimmed momentum model in the straight-lined approximations. For $p = \frac{1}{2}$ the averaged hit rates in the last column are smaller by about 10% than those of the approximation. For $p = \frac{1}{4}$ they are larger by up to 20% in the range between $h_{qu} = \frac{1}{4}$ and $h_{qu} = 1$, probably because of the pronounced upward bulge of $h_p(h_{qu})$ relative to $h = h_{qu}$ that is visible in Figure 1.

TABLE 1

Calculated Approximate Values of h_{av} and R = $<z>$ / $<z_{n\gg1}>$ = (h_{av} – p) / (h_0 – p) for the Three Variants of the Momentum Model and the Field Model

		Momentum model with approximate rectilinear $h(h_{qu}) = 0$, h_{qu} or 1			Field model* with exact curvilinear $h(h_{qu})$
		Excessive effect sizes			
		lost but compensated by		trimmed	
		null effect	renormalizing		
$p = 1/2$	h_{av}	0.542	0.576	0.696	0.677
$<z> = 0.65, \alpha = 1.5$	R	0.13	0.23	0.60	0.54
$p = 1/2$	h_{av}	0.557	0.580	0.653	0.634
$<z> = 0.4066, \alpha = 1.3$	R	0.28	0.39	0.75	0.66
$p = 1/4$	h_{av}	0.431	0.510	0.522	0.551
$<z> = 0.65, \alpha = 1.5$	R	0.64	0.92	0.97	1.07
$p = 1/4$	h_{av}	0.427	0.464	0.439	0.475
$<z> = 0.4066, \alpha = 1.3$	R	1.00	1.21	1.07	1.28

* The values of the momentum model with trimming in the next-to-last column apply, as approximations, also to the field model.

One would like to know which of the proposed models, if any, is correct or at least makes the best predictions. From the physicist's point of view, the field model appears much more attractive than any of the three variants of the momentum model. The variant with trimming is the most plausible choice among them. However, the excessive parts of the conjectured true effect size are trimmed away and not detectable. The following comparisons of theory and experiment are an attempt to find the most likely model.

Comparisons with Experimental Data

As already mentioned, in an earlier paper by the author the question arose whether the limits of the hit rate at 0 and 1 can diminish the measured $<z>$ in comparison to what it would be without this limitation (Helfrich 2011). The reason was the great difference between $<z> = 0.65$ as obtained in the meta-analyses of PK experiments on binary RNGs by Radin and Nelson (1989, 2000) and $<z> = 0.182$ as calculated by Radin (2006) from the dream-psi data of Sherwood and Roe (2003), both belonging to the class $p = \frac{1}{2}$. Their ratio is 0.28, calling for a correction factor R of about this size to reduce $<z_{n\gg1}>$ = 0.65 to $<z_{n=1}>$ = 0.182. The momentum model with losses compensated

by renormalization would almost exactly and that with losses compensated by the null effect more than fulfill this requirement. However, the ratio R differs much less from unity if from the PK data only the quarter of the experiments with the largest values of n is taken into account for which $<z> = 0.41$ (Bösch, Steinkamp, & Boller 2006), and at the same time the result for dream-psi is elevated to the Maimonides value $<z> = 0.26$ for the reasons given in the section **Conjectured Rules**. In order to obtain $<z_{n=1}>$ from $<z_{n\gg1}>$, the latter is now multiplied by R = 0.75, the correction factor for $\alpha = 1.3$ and $<z> = 0.4066$ applicable in the momentum model with trimmed momenta and in the rectilinear approximation of the field model in the case $p = \frac{1}{2}$ (see Table 1). In view of the small difference between $<z_{n=1}> = 0.26$, which is a measured value, and $<z_{n=1}>_{av} = 0.31$, the calculated value for non-existent one-trial PK experiments, one could speak of good agreement between theory and experiment. The agreement is even better for the exact field model with R = 0.66 leading to $<z_{n=1}>_{av} = 0.27$. These considerations suggest that the reduction of the measured $<z>$-score can indeed be explained by the limitations of the hit rate, even without assuming compensated losses. The numbers slightly favor the exact field model over the momentum model with trimmed momenta.

Another comparison concerns the distance between the $<z>$-scores of dream-psi ($p = \frac{1}{2}$) and ganzfeld-psi ($p = \frac{1}{4}$). In both cases, the experiments consist of a single trial ($n = 1$). Radin (2006) adopted for dream-psi $(h - p) = 0.091$ and for ganzfeld-psi $(h-p)=0.07$, which correspond to $<z>=0.182$ and 0.162, respectively. This difference could well be explained by the decrease of 15% to be expected if instead of $<z>$ the psi field κ is independent of the chance hit rate p. However, in the preceding paragraph the larger mean-score of dream-psi deduced from the experiments at Maimonides, $<z> = 0.26$, was preferred. With this value, the effect size of ganzfeld-psi would be 35% less than that of dream-psi. All these z-scores are measured $<z_{n=1}>$ values. For a comparison in terms of many-trial experiments, one has to multiply these values of $<z_{n=1}>$ by the associated inverted correction factors $1/R$ to obtain $<z_{n\gg1}>_{re}$, another calculated quantity (the subscript re means reversal). Taking the values of R for $<z> = 0.4066$ from Table 1, one finds that regardless of the model the procedure would add slightly to the size of the discrepancy. I cannot rule out that the large difference is due to an effect-size fluctuation, but consider this to be not very likely. In order to escape a breakdown of Rule 5, let me invoke a possible reason that is the opposite of counting too many trials. The temporal distance of about an hour between ganzfeld experiments may be too small for their reliable separation, and as a consequence two experiments might fuse into a single one whenever the participant does not change. Studies without such a change are mentioned

in the literature (see, e.g., Bem & Honorton 1994), but in general there is no attention paid to the problem of separation. Incidentally, Radin's value of the ganzfeld hit rate, $h = 0.32$, is corroborated by other authors. Williams (2011) obtains $h = 0.31$ in his meta-analysis, while Utts, Norris, Suess, and Johnson (2010) and Storm, Tressoldi, and DiRisio (2010) find $h = 0.334$ and 0.32, respectively. The meta-analyses differ by the selection of the data. Radin's value lies in the middle of the others.

The comparisons just made may be regarded as naïve because they neglect the large standard errors and wide confidence intervals linked with the statistical error of the null effect. For instance, with 200 experiments, the 95% confidence interval (two-tailed) of the $< z >$-score remaining after N experiments, $\pm 1.96 \sqrt{\frac{p(1-p)}{N}}$, is ± 0.06 at $p = \frac{1}{4}$ and ± 0.07 at $p = \frac{1}{2}$. The scattering of the effect size makes standard error and confidence interval even larger. Without effect-size scattering, the confidence interval of the $< z >$-score of the 450 Maimonides dream-psi experiments, $< z_{n=1} > = 0.26$, would be 0.046.

In the article referred to at the beginning of this section, an attempt was made to explain the extraordinarily high $< z >$-scores found in the ball drawing test of Ertel (2005), a case of ESP with $p = 1/5$, and PK-influenced dice throwing with $p = 1/6$ meta-analyzed by Honorton and Ferrari (1989). They were $< z > = 0.79$ and $< z > = 0.917$, respectively, both measured at large n. For this purpose an ad hoc model was proposed in which $< z >$ diverges as p tends to zero. The rules proposed in the present paper do not permit a divergence of $< z >$, which would be strange anyway. Provided Rule 4 applies, it seems much more likely that the experiments in question were interrupted quite frequently.

Conclusion

In the present paper, six rules presumably or conjecturally holding for statistical psi effects have been formulated. Mainly experiments consisting of hit-or-miss trials, with the chance hit rates $p = \frac{1}{2}$ or $p = \frac{1}{4}$, were taken into consideration. The first three rules relate to the absence of dependences on spatial and temporal distances and to the impossibility of markedly increasing the effect size by a collaboration of more than one participant where only one is needed; these three rules appear to be firmly established. The same applies to the fourth rule, but clear specifications of what is needed for the separation of two subsequent experiments with regard to temporal distance and change of participant or participants between experiments are still missing. The fifth rule claims that all statistical psi effects, at least

those analyzable in terms of hit rates, are of roughly equal size and that the scattering of the effect size is about as strong as the size itself. The two parts of the rule were combined because the scattering emerges in measurements of the effect size. More data on effect sizes and in particular on effect-size scattering are required for a more precise formulation of this rule and for a final judgement on its validity. There seem to be only three meta-analyses in the literature that present histograms of the z-score distributions (Radin & Nelson 1989, 2000, Schub 2006). The sixth rule is preliminary and speculative, because its claim that these distributions tend to be normal ones is based on the results of essentially first two of these meta-analyses.

The primary purpose of the calculations was to deal with the possibility of effect sizes per trial being too large to be defined by the mean z-score $< z >$ that like the hit rate h cannot transgress upper and lower limits. Two unlimited effect sizes were derived from different models of the psi effect on condition that they merge with $< z_{n=1} >$ as $h - p$ approaches zero. In the field model the limited hit rate is a one-to-one function of the unlimited effect size. The deviations of the conjectured true effect size from $< z >$ in this model were estimated from Figure 1 to become substantial in one-trial experiments at hit rate $p = 1/2$ when $< z_{n=1} >$ rises above 0.5. From Figure 2 it was deduced that they are relatively small in the momentum model up or down to the points where h as a function of the conjectured effect size merges with the lines $h = 1$ or $h = 0$. Any further increase of the effect size beyond these points cannot be detected in the momentum model. A direct check of these predictions to identify the right model is as yet impossible because of the scattering of the effect size and a lack of data suitable for meaningful comparisons. For an indirect check, I included this scattering in the calculations, assuming normal distributions of effect sizes. The first of two comparisons between theory and experiment suggests that the field model is the better choice. The numbers differ too little to allow a final decision. The second comparison deals with a possible dependence of the effect size on p in the field model and seems to lead to a conflict with Rule 5, i.e. the hypothesis of the approximate equality of effect sizes. An experimental reason for this disagreement may be insufficient separation of the ganzfeld-psi experiments resulting in a decrease of their actual number.

The effect sizes of statistical psi are of a magnitude that permits them to noticeably influence the outcome of an experiment, but not large enough to prove the action of psi by a single experiment. A philosophical and at the same time practical reason for this constraint is obvious. Stronger effects could permit us to mentally control (Helfrich 2007) and supervise each other. This would be in conflict with our freedom. We like to believe in the power of good wishes and prayers, but at the same time insist on our

autonomy and privacy. In a sense, the uncertainties of psi might reconcile these contradictory demands.

Statistical psi effects are independent of physical laws but do not directly violate them. They utilize the randomness of non-equilibrium states that continually allows choosing among different paths of development. How psi works and the path toward a goal it selects remain mysteries. One may assume that it avoids detours and prefers paths of high probability (which might be another rule). Physics took a long time to recognize the failure of determinism and replace certainties with probabilities. Parapsychology questions this achievement by allowing an influence on randomness. Perhaps parapsychology reveals a bridge between the mind and the material world, in particular the brain. It is astonishing that philosophy and the official churches take hardly any notice of these perspectives. The more rules parapsychology can be shown to obey, the more readily it will be respected by the exact sciences. Therefore, one may hope that it will prove worthwhile to look for rules and with this purpose in mind even to speculate to some extent where the experimental data are still vague or seem to turn out to be incomplete.

Acknowledgments

Note

[1] The scattering of z about $<z> \neq 0$ at the psi-induced hit rate h may be expected to differ from that about $<z> = 0$ at the chance hit rate p, the

standard deviation being $\sqrt{h(1-h)n}$ instead of $\sqrt{p(1-p)n}$, an effect that to my knowledge has not yet been observed. The reference state in calculations of deformational energies and chance probabilities of psi-affected z-scores and their distributions is always the ground state with the chance hit rate p.

[2] Equation (10) can also be derived by means of a thought experiment with an ideal gas. An ideal gas consisting of n particles (taking the place of trials) in a cylinder of volume V can be divided by a circular impermeable wall into the partial volumes Vh and $V(1 - h)$. Temperature and, in the beginning, pressure are the same in both. Then the wall is shifted

at constant temperature until the partial volumes are Vp and $V(1 - p)$, respectively. The total work of compressing one partial volume (positive) and dilating the other (negative) is

$$E = nk_BT\left[(1-h)\ln\frac{1-h}{1-p} + h\ln\frac{h}{p}\right] .$$

This is identical to Equation (10).

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RESEARCH ARTICLE

Eisenbud, Smias, and Psi

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Abstract—This paper explores contributions made by Jule Eisenbud pertaining to the substantive interrelationship of psi functioning and probabilistic behavior. It is argued that Eisenbud came very close to articulating a construct that may prove useful to psi theoreticians and researchers. That construct is "smias," which was first articulated by researchers examining the foundations of probability theory in connection with an investigation of the gambler's fallacy. It is shown that the smias construct fits Eisenbud's theorization very well indeed, and that the construct offers psi researchers and theorists an account of the Law of Large Numbers that can accommodate empirically observed convergence while providing a metaphysical alternative to the metaphysics underlying the conventional view of the empirical functioning of the Law. An empirical test is outlined that might provide support for an Eisenbudian conception of the role of psi in connection with probabilistic behavior.

The psychiatrist and parapsychologist Jule Eisenbud was a renegade among renegades. Not only did he research what was considered by many of his colleagues to be highly controversial psi subject matter; he also, in the course of this research, launched incisive attacks on core assumptions underlying probability theory. Exactly what is involved in probabilistic behavior, and how probability theory is best seen as relating to psi theory and research, are issues that Eisenbud meditated upon at length. Readers might agree that his conclusions on these scores are quite intriguing.

One upshot of Eisenbud's theoretical and empirical explorations led him to seize the psi bull by the horns, so to speak, by arguing, rather startlingly, that a psi linkage among events is ultimately the only type of linkage between events that there is. On this issue, here is Eisenbud in 1956 (Eisenbud 1992):

> This author saw in the paradox of the fact that probability had laws at all, and that it worked, what many metaphysicians had already grasped in terms of ontological and cosmological necessity—that nothing in the universe would work, neither atoms nor animals nor astral systems, in the absence of

what amounts to a communicating dependency of each event upon every
other event and upon the whole . . . [i]n his exhaustive study of "what makes
probability run," Marbe was led to deny the statistical independence of suc-
cessive trials in a coin-toss series and came to the same conclusion in regard
to every other type of statistical series: i.e. that in a sense nature *does* have a
memory (at least he suspected that the answer to the riddle lay somewhere
in the psychological sphere, just where he could not finally say). (Eisenbud
1992[1956]:36)

In addition to rendering contributions surrounding the bold claim
positing a very intimate and *substantive* interrelationship of psi to
probabilistic behavior, Eisenbud made related claims and contributions
addressing the important issue as to why psi can, at least sometimes and
wholly aside from shortfalls in statistical power, be so difficult to detect
and/or replicate in laboratory settings. This paper explores Eisenbud's ideas
relating to probability theory's relationship to psi and holds that he came
close to articulating a construct that may prove useful to psi theorization
and research. That construct is "smias" and was first formulated by Baird
and Otte (1982) in the context of an examination of the foundations of
probability theory that was prompted in part by their reflections on the
"gambler's fallacy" and the Law of Large Numbers. We begin the analysis
by comparing the conventional treatment of the Law of Large Numbers
with what Eisenbud had to say on the matter. We conclude the analysis
with recommendations for empirical testing of whether the Law of Large
Numbers operates in an Eisenbudian manner in nature.

The Law of Large Numbers: Framing the Issues

The mathematician David Hand's informal but serviceable definition
of the Law of Large Numbers states that the Law "says that the average
of a sequence of numbers randomly drawn from a given set of values is
likely to get closer and closer to the average of that set" (Hand 2014:64).
Hand is highly skeptical as to psi phenomena, but his view on the Law
is representative of the nearly universal and conventional approach to the
issue—an approach that we are going to criticize.

Grinstead and Snell (2003) meticulously show that the Law of Large
Numbers can be logically and mathematically derived with respect
to probability mass as well as density functions by employing basic
constructs such as variance, expected value, Chebyshev's Inequality, and an
independently and identically distributed (hereinafter iid) random variable.
Neither Eisenbud nor the current author have any qualms with the formalism
of the Law of Large Numbers. The operative question for Eisenbud and the

current author is why, *empirically*, the average of a sequence of numbers tends to converge around the average of the population set. As we will see later, there are two different ways convergence can happen empirically. These two ways, left undefined for now, are "dilution" and what might be called "balancing." The former, which supposes that conditions can be achieved wherein iid can establish a guaranteed foothold in nature, is the conventional view. The latter, which, in spite of empirical phenomena such as convergence, supposes that iid may never really hold in nature, is the Eisenbudian perspective. Coinciding with this distinction are two very distinguishable conceptions of the level at which probability operates. The conventional view holds that probability is invariably grounded at the level of individual events. The Eisenbudian view, which we will argue is a "smias" type view, denies that this is so.

We should add that it would seem to be true that an *empirical* failure of the Law of Large Numbers is not a logical impossibility, and it might not be a metaphysical impossibility either (see Coates 1947, 1956, for a rather unsettling characterization of a possible world wherein the Law fails to hold). This is just to say that the formalism of the Law of Large Numbers might be no more self-executing with respect to the empirical world than any other formalism, and that the issue of whether, and if so how, the Law performs empirically is *not* of the order 1+1=2.

We need not deny that events empirically converge in accordance with the Law in order to question whether the Law's formalism really accounts for what transpires in the empirical realm. It may be, for example, that the iid assumption might not really figure into an explanation of empirical convergence. This might occur, for example, if an empirically operative process conceals such violations in a mathematically valid way. In short, we are going to explore the possibility that the empirical convergence implied by the formalism of the Law of Large Numbers might instead be compliance with a substantively very different process. That process is "smias." Intriguingly, we will see that when the smias construct is put together with the balancing conception alluded to a moment ago the prospect of statistical confirmation of the Eisenbudian view emerges even though convergence occurs.

Eisenbud on the Law of Large Numbers

In keeping with the above, Eisenbud (1982) contends that the Law of Large Numbers amounts to a question begging tack-on if it is invoked as a self-executing explanation of the empirical tendency of sample results to increasingly converge around expected values as sample size grows:

What has puzzled more than one logician is how events that are sup-
posed to originate independently of one another manage to transform
themselves into statistical aggregates with the profoundest respect for order.

Unlike other versions of the divine principle which it has replaced in
many quarters, probability does not operate with trifling scrupulosity. The
fall of a single sparrow may well go unnoticed, but let a thousand fall and
the matter is given the strictest attention in order that a certain *espirit de
corps* be maintained. Take the familiar coin toss. Since the coin can fall only
heads or tails, the probability of either coming up—as true on the thou-
sandth throw as on the first—will be ½. But if every throw is independent
of every other, how is it that the ratio of heads to tails will always tend to
approximate one on any lengthy series (the longer the series, the closer the
approximation)? The coin, say the mathematicians, has neither memory nor
consciousness. Then how is the auditing done? And what has induced a
hundred or a thousand individual events to waive their rights to fall out as
they please and to make common cause in a universe that then takes on the
appearance of lawfulness and predictability?

Mathematicians, begging the question, are apt to invoke the law of
large numbers—which, of course, is precisely what has to be explained. Or
(still begging the question) they insist that the coin does what it does out of
logical necessity. (Eisenbud 1982:212)

Here, Eisenbud expresses the view that the independence assumption
(as, say, applied to coin flips) is fundamentally at odds with the sort of
convergence of sample results around population expected values that
the Law is supposed to explain. He seems to think, for example, that the
increasing convergence, as a function of sample size, of tosses of a fair coin
to a 1:1 ratio of heads and tails no matter what has occurred up to a given
point in any particular sequence is suggestive of a kind of "memory" or
"auditing" process whereby successive outcomes in the sequence of tosses
are, contrary to the independence assumption, in some way sensitive to
previous results in the sequence. We are going to argue later that Eisenbud
may indeed have been correct in his assertion that independence never
really holds in reality, but that he barely missed the correct specification as
to why this may be so.

First, however, let us examine more closely the above contentions
of Eisenbud in order to see where they fall short. Let us suppose that
Eisenbud was merely speaking loosely when he erroneously stated that the
probabilities of heads and tails must be .5 and .5 respectively since there are
only two possible outcomes to consider; accordingly, let us operate under
the supposition that Eisenbud simply meant that we should just suppose
a random variable with two possible values, the realization of which is
such that each value is held to be equiprobable. With this in mind, the main

problem with Eisenbud's perspective as expressed above is that he appears to want to have things both ways; that is he wants to hold the probability of heads and tails constant across trials, but he also wants to claim that, in order to reliably obtain results that are compatible with the Law of Large Numbers, it must be the case that events can "waive their rights to fall out as they please."

Contrary to Eisenbud on this point, however, it is unclear what "waiving their rights to fall out as they please" can mean other than that outcome probabilities on certain trials are altered as a function of what has transpired on previous trials—which would of course militate against what Eisenbud appears to have already granted in the form of constant probabilities across trials. We should add that insofar as Eisenbud is interpreted as holding that statistical independence (formally considered) along with the rest of the formalism of the Law of Large Numbers cannot *possibly* account for the falling out of empirical events in accordance with the Law, he is wrong and the conventional view of the Law is correct. However, an Eisenbudian retort to this point would be that such a world would be one in which our "explanation" for the empirical conformity that we have been discussing would have to run along the rather vacuous lines of "that's just the way things are" or, similarly, "the Law of Large Numbers is writ into the empirical nature of things and that's that."

Where we should take issue with the conventional view of the Law of Large Numbers is with respect to the idea that this is the *only* view of the Law it is rational to hold. In fact, we can adopt a view of the Law of Large Numbers that is fully consistent with empirical convergence but which nonetheless drops the Law's iid assumption. It is true that one could then argue that a Law of Large Numbers without an iid assumption is no Law of Large Numbers at all. The response, which hopefully does not belabor the point, is that if the forthcoming smias argument in this paper is sound, we can shake hands with the conventional theorist as to events empirically converging, but part ways as to whether, or conceivably how often, iid holds—so that each party can subscribe to the Law with respect to convergence but with differing metaphysical commitments. In sum and in regard to an Eisenbudian approach to the Law of Large Numbers, what is needed at this point is a mathematically valid account of sequences that empirically conform with convergence that is also compatible with a metaphysical perspective that can in principle reject the iid assumption. These considerations lead us to a discussion of smias by way of the gambler's fallacy.

Conventional View of the Law of Large Numbers and the Gambler's Fallacy

Turning once more to Hand (2014), we see the argument that granted a variable with equiprobable outcomes and iid trials, results over numerous trials that comport with the Law of Large numbers are exactly what is to be expected, no matter what values have been realized previously in the sequence and regardless of, and indeed contrary to, the operation of some analogue of memory because:

> What actually happens is that the imbalance is diluted, so that over time the proportion of heads gets closer and closer to one-half. One half is just the average of 0 and 1. This is simply the law of large numbers. (Hand 2014:65)

In effect, Hand (2014) asserts that when Eisenbud speaks of events "waiving their rights to fall out as they please," he is in fact committing the gambler's fallacy, which can be stated as:

> [t]he mistaken belief that an initial imbalance in the proportion of coin tosses that come up heads would be counterbalanced by an excess in the other direction as we made more and more throws. (Hand 2014:65)

Clearly, the gambler's fallacy (if such it is) does involve the mistaken imputation of a sort of memory to the system.

However, we must not make the mistake of supposing that the dilution effect Hand speaks of applies empirically simply because that is what the formalism requires. Such a position would be on the order of a brute metaphysical assertion masquerading as an empirical claim. It is worth noting that Hand offers neither an empirical test of dilution nor a citation to a test of dilution even though he does offer, as countless others have, results from an empirical test of convergence. In fact, this author has been unable to locate any empirical tests of the dilution construct. Perhaps this is because conventional theorists believe that no such test is needed. If so, those theorists are wrong. We return to this issue at the conclusion of the analysis, but pause to ask why conventional theorists do not test dilution empirically given that they test convergence empirically and very often at that.

It is easy to see that the balancing that Hand refers to could produce convergence at least as well as dilution; we could suppose that, for example, 5 heads might follow 5 tails as a consequence of the failure of iid. To amplify what was mentioned earlier, no logical contradiction would be involved if a coin with empirically indistinguishable sides and flipped

in empirical conditions as close to ideal as possible continually came up heads over a very large number of trials and then tails over a very large number of trials so that a Bayesian might conclude that a failure of iid was in play. Furthermore, it is by no means obvious that this would amount to a metaphysical impossibility. Thus, the fact that the Law of Large Numbers is logically and mathematically valid in and of itself says nothing about its explanatory power with regard to empirical phenomena. To get that power, the conventional theorist must, almost by sleight of hand, smuggle in the metaphysical assumption that the Law is somehow inscribed in empirical nature.

The conventional position (which *is* a metaphysical position) as to the functioning of the Law in empirical nature may in fact be sound. However, what is conceivably very important work by Baird and Otte (1982) raises very serious difficulties for the conventional account. As Baird and Otte (1982) point out, it is never possible to rule out the possibility that probabilities in fact attach only to ensembles of events. This entails, in turn, that empirical events can never be conclusively established as iid. It also entails that we can never be sure that the Law ever in fact achieves "footing" at the level of individual units, and that we can never be sure that the gambler's fallacy is in fact a fallacy. It is easily shown that these truths are quite compatible both with Eisenbud's overarching theoretical perspective on the role of psi in the empirical world as well as his skepticism toward the conventional view of the Law.

One interesting upshot of Baird and Otte's findings as applied to Eisenbud's work is that the occurrence of psi events can be analogized to committing the gambler's fallacy but getting away with it in that the probabilities of events can be altered at any given point in a sequence in such a way that the iid assumption of constant probabilities across trials and the independence of trials appears not to have been disturbed with respect to convergence—even though, in the sense Eisenbud may well have had in mind during his ruminations on psi and probability theory, they have been. In turn, the potential for the sort of statistical masking described by Baker and Otte's smias construct implies that the *absence* of statistical significance in psi studies may be *neither here nor there* with respect to the presence or absence of psi effects.

Smias, Bias, and Preliminary Application to Psi

Baird and Otte's (1982) potentially pivotal concept of "smias" is perhaps best approached initially by contrasting it with the more familiar term "bias." Bias functions the same way in Baird and Otte's framework as it does in many other instances in which statistical testing is involved; that is

it simply refers to probabilities assigned to different individual realizations of a random variable or variables together with the standard iid assumption. Thus, to take an example of Baird and Otte's (1982) that is felicitously in line with Eisenbud's coin example, bias can be understood to refer to the hypothesis that each individual coin toss realizes a value of "head" with a probability of .5 and realizes a probability of "tail" with a probability of .5 with each flip iid (any other assignment of probabilities to the two alternatives would involve bias, too, since the key consideration is that bias-type probabilities are probabilities that are assigned to the potential values of individual trial outcomes).

Smias, on the other hand, involves the assignment of well-specified probabilities only to a series, or ensemble, of coin tosses rather than individual coin tosses, and correspondingly adopts a modified independence assumption under which only successive ensembles of a given size are independently distributed. So, for example, and again following Baird and Otte, we can hypothesize that the probability that a coin "will land heads five or more times in 20 flips" (Baird and Otte 1982:173) is .9941, and in addition suppose that sets, or ensembles, of 20 flips are independent of one another while dropping the standard independence assumption that applies between each successive flip.

One of Baird and Otte's key observations is that hypotheses formulated on the basis of bias can appear to be interchangeable with hypotheses formulated on the basis of smias. Thus, if we adopt the bias-type hypothesis of a fair coin along with bias-type iid ($p(H) = .5$, $p(T) = .5$ on each flip), we are also necessarily, by way of the binomial distribution, implying the smias-type hypothesis that the probability of 5 or more heads in an ensemble of 20 flips is .9941. An important point about smias, though, is that the converse does not hold—so that the supposition of smias need not imply anything at all about bias. For example, if we suppose only a smias-type ensemble probability in the 20-flip scenario, in all but degenerate cases each of the 20 individual outcomes within the sequence will, at the outset of the flipping process, be compatible with a range of different bias-type probability assignments—assignments that can, as the process unfolds, change in violation of bias-type independence. As Baird and Otte note in linking smias to Hacking's (1980) emergent probabilities construct, the application of smias to the bias-rooted gambler's fallacy issue is clear:

> Hacking says that such emergent probabilities make no difference to statistical inference, but they do. If there is an adequate bias model, then reasoning about the remaining 5 flips of a 20-flip sequence on the basis of the previous 15 flips commits the gambler's fallacy. However, if there is no bias model "underneath" a smias model, then such reasoning is sound. Given

> 15 tails in the first 15 flips of a 20-flip trial, if we assume smias of .999 and no bias underneath, we can legitimately infer that the next five flips will be heads. This is how to commit the gambler's fallacy and get away with it. (Baird & Otte 1982:174)

In theory, then, granted knowledge of the smias system and its parameter or parameters, one could say either as Baird and Otte do that one could commit the gambler's fallacy and get away with it, or, what amounts to the same thing, that under such circumstances no gambler's fallacy exists at all in that the system really does rebalance itself so as to conform with smias constraints.

Similar considerations apply with respect to the Law of Large Numbers. Once more, from Baird and Otte:

> When a statistical uniformity appears in a population, there are two ways to account for it. One way is to ascribe probabilistic properties to individuals and use results such as the law of large numbers to explain stable regularities in ensembles of individuals. Another way is to claim that the uniformity in the population does not arise out of any probabilistic facts about the individual members of the population, but rather that the probabilities are manifest only at the level of an ensemble. (Baird & Otte 1982:171)

Thus, smias is quite capable of yielding, in a mathematically valid way, the *same* empirical convergence the conventional view of the Law of Large Numbers anticipates—but with a very different metaphysical basis. With the preceding in mind, it is interesting to contemplate what happens if we synthesize Eisenbud's contemplations on the Law (quoted above) with Baird and Otte's (1982) smias construct. If we do, we can take Eisenbud to have been suggesting that psi functions at the smias level and that the Law operates in a "smiasing" fashion and therefore, at least in certain instances, due to psi. In addition, for the reasons specified above, the integration of smias with Eisenbud's thinking would absolve him of the charge of committing the gambler's fallacy.

Another intriguing implication of Baird and Otte's (1982) smias construct as it may pertain to psi theory and research revolves around the idea that smias-derived shifting, or perhaps even fixing, of probabilities and violations of independence can be very difficult and perhaps even impossible to detect with full confidence and can therefore function in ways that are practically indistinguishable from bias-type processes:

> We simply are in a position of not knowing for certain whether smias properties are always, ever, or never grounded from below by bias properties. In some cases we may be able to gather some pertinent data. We can test for

independence of flips. Such tests are not, however, conclusive. Ultimately our belief in grounding probabilities from below rests on a metaphysical assumption. This intuition may be very plausible but it is metaphysical, nonetheless. Consequently, we can never be certain whether the gambler's fallacy is really a fallacy, since it also rests upon this metaphysical assumption. Perhaps there is some consolation in this. After all, how could so many gamblers be so wrong? (Baird & Otte 1982:178)

We will see later that while the proposed smias-based empirical test alluded to at the outset does not offer the prospect of conclusive support for the smias perspective (of course no statistical test could), it does offer the possibility for compelling statistical evidence of an Eisenbud-style, smias-based balancing effect in contravention to conventional, Handian bias–style dilution.

Turning now to a related potentially significant research implication of smias, readers might agree that the properties of smias could conceivably have a role to play in terms of helping to account for the difficulty of securing replicability of experimental psi results for the simple reason that smias-style psi can masquerade as bias—especially if one does not keep a close eye on the dilution versus balancing question. In any event, smias properties certainly do dovetail nicely with Eisenbud's views on the difficulties of detecting, and especially replicating, psi functioning. Braude (1979) offered remarks that are particularly apposite here:

> Jule Eisenbud has suggested that parapsychology's failure to design experiments that are as reliably repeatable as those in other areas of science may be a function of large-scale or cosmic constraints on psi-functioning. Although he regards dramatic laboratory evidence for psi as a kind of chance occurrence, he does not deny the existence of psi functioning. Rather, he argues that psi functioning may be such that it tends to operate in all of us, but unobtrusively, and that occasional dramatic occurrences of psi in the lab are random fluctuations in what, *by its very nature*, is a non-dramatic range of phenomena. (Braude 1979:70)

Braude continues:

> I think this general position deserves to be taken seriously, although Eisenbud and Crumbaugh (especially Eisenbud) state this view in a way that makes it seem as if there were some mysterious feature of psi itself which makes psi difficult to pin down. Some might see this as an effort to explain the mystery of psi by reference to an even greater mystery. (Braude 1979:70–71)

If we grant to Eisenbud smias-type support for his views on pervasive yet typically unobtrusive psi-functioning, there is a way to reconcile at least the thrust of Eisenbud's conjectures with Braude's concerns. We can say, for example, that to the degree that psi is difficult to replicate it is because of the comparatively quotidian reason that psi tends to be associated with the maintenance of smias, or ensemble-level, probabilistic features—features which, since they can function while preserving the illusion of bias-type properties such as independence and constant probabilities across individual trials, are easily disguised.

We might add, though—and this, too, touches on the empirical test to be proposed shortly—that if purportedly random systems, such as random number generators (RNGs), really do function in accordance with smias and balancing, it could be that larger psi effects would be observed if psi efforts were directed at securing a perfect 50/50 balance in system outcomes. The theory here would be that if there is a psi-based tendency for such systems to balance, efforts to disrupt that balance would be confronting an opposing "force." It might be thought that efforts aligned with the force could conceivably produce larger effects. Though the direction of outcomes toward a 50/50 balance might seem counterintuitive, there is nothing to suggest that such a procedure would be a statistically invalid way to proceed since the possibility clearly exists of obtaining 50/50 results at rates greater than chance standards would lead us to expect.

Some Remarks on Smias and Eisenbud's General View of Psi Functioning

It was shown at the outset, by way of the Eisenbud quote referencing Marbe, that Eisenbud's theorization of probability as it pertains to psi functioning posited a sort of psi-type "memory" associated with probabilistic processes in nature whereby without this "memory" function the laws of probability would not function. So far, we have suggested that Eisenbud's "memory" conjecture is compatible with, and might be better construed as, smias functions or laws and an associated balancing mechanism. We have seen that one virtue of the smias approach is that it provides a straightforward explanation as to why psi functioning can be challenging to assess statistically without condemning entirely the statistical investigation of psi and without necessarily falling back on an account of psi that supposes it to function at cross-purposes with itself. Here, with smias in mind, we turn toward a more detailed examination of Eisenbud's provocative claim that psi processes function as the "glue" that ensures that the laws of probability hold, as well as his related claim that psi might be understood as a sort of psychologically mediated and hierarchically organized control system.

It may well have been that Eisenbud was operating with a view of the interrelationships of probability, randomness, and the law of large numbers that was propounded by Bertrand Russell. Baird and Otte (1982), in the course of advancing their smias construct and questioning the metaphysical basis of probability theory, offer the following quote from Russell:

> The theory of probability is in a very unsatisfactory state, both logically and mathematically; and I do not believe that there is any alchemy by which it can produce regularity in large numbers out of pure caprice in each single case. If the penny really chose by caprice whether to fall heads or tails, have we any reason to say that it would choose one about as often as the other? Might not caprice lead just as well always to the same choice? . . . [W]e cannot accept the view that ultimate regularities in the world have to do with large numbers of cases, and we shall have to suppose that the statistical laws of atomic behavior are derivative from hitherto undiscovered laws of individual behavior (Russell 1935:168).

Russell appears to be arguing that if we interpret randomness as caprice there is no satisfactory way to derive the Law of Large Numbers. So a 10,000 consecutive heads sequence and something like a 5,000 tails and 5,000 heads sequence must each be considered equally indicative of chance, or randomness, since in each case, according to Russell, we would have to say "that's just the way things are; it's an arbitrary process." Thus, as the end of the preceding quote shows, Russell adopted determinism at the individual unit level in order to secure the empirical results typically associated with the Law.

It may have been with respect to the metaphysical frailty of the Law on touchy issues such as the correct specification of randomness that Eisenbud was motivated to state, in terms that resonate nicely with smias:

> Thus if we imagine an experiment in which coins, sticks, or needles (or billiard balls in a shuffling machine, molecules in solution, or gas particles in a chamber, to take classical examples) could, through some means, be shielded from all observers and all mental influence, we would find, according to our supposition, that the laws of probability would no longer hold. They would not work by themselves. In the absence of the observer, pure indeterminate chance would reign, chance unfettered by the "laws of chance," chance that would never give rise to any kind of order. Such a thought experiment is at best, of course, a kind of myth. But it makes more sense than than the updated version of the immaculate conception that sees the key to the universe in atomic events in which causality fades out like the Cheshire Cat and only an abstract and impotent probability, suspended in nothingness, is left to do the world's work. It avoids the difficulties, moreover, of leaving to each individual inanimate event (coin, stick, atom) the privilege

(and the necessity) of making up its own mind about how it will behave. (Eisenbud 1982:214–215)

It would seem reasonable to take Eisenbud to have accepted, along with Russell, a view of randomness as caprice. If we do, we can balance our books by supposing that where Russell plumped for conventional determinism as a way out of caprice/randomness, Eisenbud instead opted for psi-style determinism that can be constructed along smias lines.

Along these lines, Eisenbud stated:

. . . in the underlying system of psi-mediated probabilistic bookkeeping by which events are kept from getting too much out of whack along one axis or another, a breakthrough in one sector must sooner or later be balanced by a tightening up or oppositely signed trend in another. (Eisenbud 1992[1963]:163)

Furthermore, we have Eisenbud contending:

With the observer now seen also in his creative aspect, we might envisage control as effected through some sort of a collective unconscious clearing house (not to be confused with Jung's Collective Unconscious), where myriad individual behavioral vectors (arising from dynamic contexts such as those seen in the case studies presented earlier) are sifted, sorted, and graduated hierarchically into the effective determinants of large-scale events on a group level. Particular segments of existence, as I have elsewhere suggested (Eisenbud 1966, 1967) might be the case in the areas of ecology and evolution, would be responsive to individual decisions (not too unlike those of Margenau's electrons) made at grass roots levels; but these, at the same time, would be subject to boundary conditions ("needs") imposed at higher levels. (Eisenbud 1982:215)

In smias terms, Eisenbud is presenting us with the notion that psi is a mental process that governs nature in such a way that events fall out in ways that are sensitive to the falling out of other, supposedly independent, events. We proceed to adumbrate suggestions pertaining to the empirical investigation of this idea in the next section.

Toward the Empirical Investigation of Smias/Psias

Eisenbud can be read as having advanced the idea that psi is written into the fabric of nature in such a way that events invariably fall out in patterns suggesting some type of inter-communication and hence dependency—just as the conventional theorist/skeptic can be read as supporting the idea that probabilistic events can fall out as individual units and independently of one

another. A label for the Eisenbud perspective on psi and probability might be handy, and so in light of our synthesis of smias with Eisenbud's work on psi, we might consider him to have advocated a "psias" model. In any event, we have seen that the two views can have very different implications with regard to the manner in which the Law of Large Numbers operates empirically.

   Both views are compatible with empirical convergence, but the two views present us with a contest between bias/dilution and smias/balancing. Simple tests involving random number generators should help resolve the dispute. If the conventional/skeptical view is correct, Hand, for example, is right when he asserts that an initial imbalance in heads ". . . is diluted, so that over time, the proportion of heads gets closer and closer to one-half" (Hand 2014:63). This is, after all, exactly what a logical proponent of the bias view who holds that the formalism of the Law of Large Numbers self-executes empirically would say. Notice that Hand says "closer and closer to one half," so that he is acknowledging that in the case of an initial imbalance convergence is expected to be *directional*. Under the Hand view, at any point in the sequence the expected number of heads in the remainder of flips, regardless of how many, will of course be one-half the remaining number of flips. Therefore, Hand is asserting that no matter how many flips subsequent to the initial imbalance we perform, we *must* expect to observe an excess of whatever result was imbalanced at the start of the sequence (heads in Hand's example).

   But *is* that in fact what we observe empirically? Might it be the case that what we observe instead is that RNG systems are, we might as well say now, smiased/psiased so that they tend to rebalance excesses, in accordance with a smias, aggregate level parameter of .5, more completely than the bias/dilution model says will happen? Perhaps, for example, the probability of exactly 5,000 heads in 10,000 RNG "flips" after having obtained an initial 15 tails is significantly higher than the bias/dilution model predicts? Similarly, if results really are "pulled" toward a smias system parameter of .5, we might expect to observe a tightening of sampling distributions in that significantly less variance (and therefore smaller standard errors) will be observed than predicted by the bias/dilution model.

   To be sure, smias/psias effects may be a function of sample size as well as the nature of the RNG mechanism examined. Also, we should mention that the precise relationship, if any, of the magnitude of initial imbalances to the magnitude of the smiasing of individual outcomes in the direction of hypothesized smias-governed balancing is very much open to question. It should not be difficult to test smias/balancing hypotheses; indeed, scrutiny of extant datasets would serve well as a start (although it is logically

possible, but at least to this author quite unlikely a priori, that the absence of the will to test these hypotheses in the past meant that the effects did not show up in the data. The a priori unlikelihood referred to is especially reinforced if Eisenbud is correct in his conjecture that psi is writ in nature).

Discussion and Conclusion

It has been shown that Jule Eisenbud expressed ideas about psi functioning that came very close to capturing that which is expressed by Baird and Otte's (1982) smias construct. Unsurprisingly, then, it was also possible to show that smias and Eisenbud's view on the nature of probability as it pertains to psi have a natural affinity for one another.

Smias is a friend of Eisenbud, and it may be a construct that is worthy of consideration by all psi theorists and researchers. The construct has its virtues in terms of psi research and theorization—it offers a reasonable account, that goes hand-in-hand with psi, of the empirical convergence implied by the Law of Large Numbers; it avoids problems associated with the gambler's fallacy; and it figures prominently in a reasonable argument as to why psi is, at least sometimes, hard to detect and/or replicate statistically. A construct that can do each of these things simultaneously should perhaps not be summarily dismissed.

Furthermore, it has been noted that the conventional view as to why the Law of Large Numbers functions the way it does empirically is in fact a metaphysical view, and not the only metaphysical view on the issue that is rational to hold. Along these lines, a test has been proposed that might empirically substantiate a smias/balancing view of the empirical operation of the Law of Large Numbers as against the conventional bias/dilution view of same. Finally, given the specification of the smias construct, empirical substantiation might be viewed as tantamount to metaphysical support for psi functioning.

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The Mediumship of Carlos Mirabelli (1889–1951)

Stephen E. Braude

Abstract—The case of the Brazilian medium Carlos Mirabelli is one of the most tantalizing and frustrating in psychical research. If his phenomena—especially his psychokinetic manifestations—occurred as reported, he was probably the greatest physical medium of all time. Mirabelli reportedly moved objects (including very large objects) without contact, levitated himself while bound to a chair, and dematerialized and transported objects of all kinds (including himself) to distant locations. Mirabelli also reportedly produced numerous different full-figure materializations in bright daylight, and these were often recognized as deceased relatives, acquaintances, or well-known public figures by those attending the séance. Sitters would watch them form; attending physicians would carefully examine them for up to 30 minutes and report ordinary bodily functions; photographs of the figures would be taken; and then they would slowly dissolve or fade before everyone's eyes. However, Mirabelli was also clearly guilty of fraud on occasion, including his notorious use of a doctored photo ostensibly showing him to be levitating. His case therefore presents an all-too-familiar challenge to psi research—namely, how to assess cases of so-called "mixed" mediumship.

Introduction

The case of the Brazilian medium Carmine (Carlos) Mirabelli[1] is one of the most tantalizing and frustrating in psychical research. To see why it's tantalizing, consider how his story has been introduced in two contemporary surveys of the case. Eric Dingwall wrote:

> I propose discussing a case in which the most extraordinary occurrences are recorded, so extraordinary indeed that there is nothing like them in the whole range of psychical literature. (Dingwall 1930:296)

Similarly, Guy Playfair writes:

> If everything they say about Carmine Mirabelli is true, he was without doubt the most spectacular physical effects medium in history . . . . Mirabelli was surely the medium to end all mediums. You name it, and he is said to have
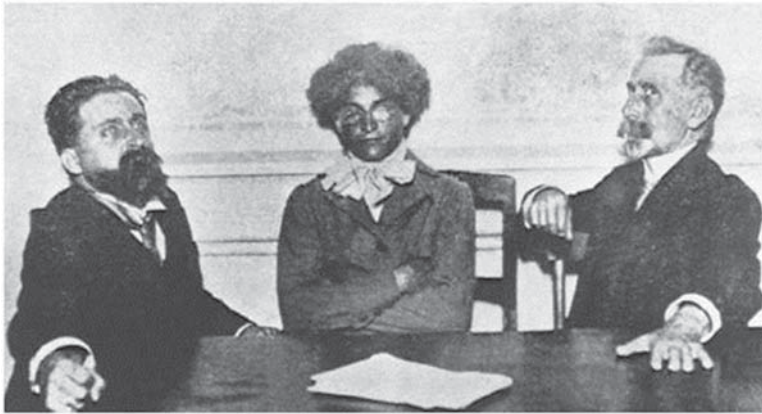
Figure 1. Dr. Carlos de Castro (right) seems alarmed at finding the deceased poet Giuseppe Parini between himself and the entranced Mirabelli.

done it; automatic writing in over thirty languages living or dead, speaking in numerous foreign tongues, materializing objects and people, transporting anything from a bunch of flowers to large pieces of furniture (including levitation of himself even when strapped to a chair), producing impressions of spirit hands in trays of flour or wax inside locked drawers, dematerializing anything in sight, himself included. (Playfair 2011:23)

Furthermore, Mirabelli reportedly produced full-form materializations in bright daylight, and these were often recognized as deceased relatives, acquaintances, or well-known public figures by those attending the séance. Sitters would watch them form; attending physicians would carefully examine them for up to 30 minutes and report ordinary bodily functions; photographs of the figures would be taken (e.g., Figure 1); and then they would slowly dissolve or fade before everyone's eyes. Moreover, Mirabelli reportedly materialized animals as well, such as the dog whose photo appears in the paper by Medeiros (1935).

Unfortunately, however, the case of Mirabelli never received the full scrutiny and documentation accorded Home, Palladino, and some others. In part, this may be due to the prevailing antipathy toward physical mediumship among prominent members of the Society for Psychical Research (SPR) (Inglis 1984:221ff). That antipathy had arguably reached a zenith over the earlier mediumship of Eusapia Palladino (Braude 1997, Dingwall 1962, Inglis 1977). Moreover—and undoubtedly contributing to the problem—there's some evidence of fraud in Mirabelli's case, most notably a

doctored photograph (discussed later) of the medium apparently levitating (Playfair 1992, 2011).

Nevertheless, Mirabelli's phenomena were witnessed by many people, often under conditions apparently sufficient to rule out fraud, and they were often described in great detail. But most of those accounts were written in Portuguese, and for that reason they may have been either ignored or unfairly discounted by Anglo-American and European researchers.[2]

Beginnings

Mirabelli was born to Italian parents in Brazil in 1889, and Playfair writes that "like many sons of immigrants he never quite mastered either his ancestors' or his adopted country's language. He learned some English and possibly also some German, but certainly became no skilled linguist" (Playfair 2011:25).

Mirabelli's history with psychokinesis seems to have begun in his early twenties, with some poltergeist-like outbreaks while he was employed at a shoe store. Legend has it that shoeboxes would fly off the shelves and sometimes follow the fleeing Mirabelli into the street. As a result, many concluded that Mirabelli was insane, and before long he was committed to an asylum. However, the psychiatrists in charge apparently had other ideas, and rather than putting Mirabelli into a straitjacket, they ran some tests and found that he could move objects at a distance. Their conclusion was that although Mirabelli was not normal, he was not insane. In their opinion, the phenomena occurring in Mirabelli's vicinity were "the result of the radiation of nervous forces that we all have, but that Mr Mirabelli has in extraordinary excess" (Inglis 1984:222, Playfair 2011:25). So after a stay of only nineteen days, Mirabelli was released.

Mirabelli's mediumistic career began at this point and very quickly flourished. In response to a rapidly proliferating array of astounding reports, local newspapers began taking sides in the case, some (not surprisingly) accusing Mirabelli of outright fraud and others taking a more sympathetic view of the matter. But of course accusations of fraud come with the territory, and Mirabelli had many credible supporters. Indeed, as Dingwall observed, Mirabelli's "friends and supporters included many from the best strata of S. Paulo society. Engineers, chemists, mathematicians, medical men, politicians, members of the various Faculties of Universities—all testified in his favour and recounted the marvels that they had witnessed in his presence" (Dingwall 1930:297).

Because Mirabelli's feats were so astonishing, eventually a 20-person committee was established to adjudicate the case. The committee concluded that a more formal investigation should be conducted by people

well-qualified to determine the authenticity of Mirabelli's phenomena. And that investigation was carried out by the Cesar Lombroso Academy of Psychical Studies, founded in 1919 for this purpose. However, according to SPR's Theodore Besterman, that Academy consisted only of Mirabelli and his wife, and thus Mirabelli was merely investigating himself (Besterman 1935). But as we will see later, Besterman may not be an entirely reliable reporter in this case. At any rate, the Academy's report was published in 1926, and it was that report which brought Mirabelli to the attention of researchers in the Northern Hemisphere.

Dingwall emphasized one very important feature of Mirabelli's manifestations, which he cautioned might well be "forgotten by those who try to belittle the claims of Mirabelli" (Besterman 1935), and which in fact were apparently forgotten later by Besterman (1935) (discussed below). That important feature is that "the greater part of the phenomena observed with Mirabelli were investigated in *broad daylight,* even the materializations, telekinesis, and levitations. When evening sittings were held, these were undertaken in a room *illuminated by powerful electric light*" (Dingwall 1930:298, emphasis in original).

I should note that Mirabelli also practiced healing, and that his automatisms extended beyond writing to painting and musical performances. According to Playfair,

> . . . he could paint in a number of different styles, produce portraits of dead people which were identified by surviving relatives (fifty paintings of his were once exhibited in Amsterdam), and also conjure musical phenomena out of thin air. Witnesses recall having heard ethereal concerts in his presence, ranging from snatches of opera to military fanfares, while the musically untrained Mirabelli (who was untrained in practically everything else as well, come to that), would sing lengthy arias in a number of languages, often while doing something else at the same time, like writing or painting. (Playfair 2011:31)

The phenomena observed during the Academy's investigation were divided into three categories: (1) automatic writing in 28 different languages including some dialects, as well as 3 dead languages (Latin, Chaldaic, and Hieroglyphic); (2) spoken mediumship in 26 languages including 7 dialects; and (3) physical phenomena including "levitation and invisible transportation of objects: the dematerialization of organic and inorganic bodies: luminous appearances and a variety of rapping and other sounds: touches: digital and other impressions upon soft substances, and finally the materialization of complete human beings with perfect anatomical features" (Playfair 2011).

Mirabelli's linguistic productions, on "a wide range of subjects from medicine, law, sociology, to astronomy, musical science, and literature" (Dingwall 1930:304), are remarkable because, as Playfair noted, "All witnesses I have interviewed agree without hesitation that Mirabelli could not even speak either of his own languages (Italian and Portuguese) correctly" (Playfair 2011:32–33).

The automatic writing was also remarkable for its diversity, quantity, and speed. According to Dingwall,

> we find [mediumistic control] Johann Huss impressing Mirabelli to write a treatise of 9 pages on "the independence of Checho-slovakia" in 20 minutes; Flammarion inspiring him to write about the inhabited planets, 14 pages in 19 minutes, in French; Muri Ka Ksi leading him to treat the Russian–Japanese war in Japanese, in 12 minutes to the extent of 5 pages; Moses is his control for a four-page dissertation entitled "The Slandering" (die Verleumdung), written in Hebrew; Harun el Raschid makes him write 15 pages in Syrian: "Allah and his Prophets," which required 22 minutes and thus down the list, his most extensive work mentioned being 40 pages written in Italian about "Loving your Neighbor" in 90 minutes, and the most odd feature mentioned is an untranslateable [sic] writing of three pages in hieroglyphics which took 32 minutes. (Dingwall 1930:304)

Altogether the Academy reported 392 sittings. They were held at 22 different locations, the majority of them (349) in the facilities of the Academy. Of these 392 sittings, 189 were for spoken mediumship (apparently all positive), 93 for automatic writing (of which 8 were negative), and 110 for physical phenomena (47 of which were negative). So 63 sessions were positive for physical phenomena. And of those, 40 were held in broad daylight and 23 at evening or at night but in bright artificial light. Moreover, in those sessions Mirabelli was clearly visible to witnesses, often sitting tied up in his chair, and in rooms searched before and after. Nevertheless, witnesses reported many occurrences which would seem to be impossible to produce fraudulently under those conditions.

For example, an armchair, with Mirabelli seated in it and his legs under control, rose two meters above the floor, remained aloft for two minutes, and then descended 2.5 meters away from its original place.

On another occasion, a skull rose into the air and began accumulating bones until it became a complete skeleton. Observers handled the skeleton for a while until it began to fade away, leaving the skull to remain floating. Soon thereafter, the skull fell onto the table. Mirabelli was bound throughout the event, which lasted 22 minutes in bright daylight. One of the sitters confessed later that when the skull initially rose into the air, he had mentally asked whether the rest of the skeleton would appear.

The Academy's report also cites a materialization occurring in a room of about 1,000 square meters, with stone walls and locked doors. Three knocks were heard, and then a child's voice called "Papa." One of the investigators said that he recognized the voice of his recently deceased daughter, and then a materialization began to take shape. It was of a young girl, wearing (according to the investigator) the dress in which she had been buried. The weeping investigator embraced the phantom, and a doctor who was there felt her pulse while the figure answered questions 'tonelessly but sensibly' (Inglis 1984:224). The investigators photographed the figure and eventually published it in their report. After that the phantom floated into the air and then, thirty minutes later, dematerialized. All ten investigators testified to what had occurred.

Another materialization is so astounding that Dingwall's description deserves to be quoted in its entirety.

> Phenomena began by an odor of roses which filled the room, and after a few minutes a vague cloudy appearance was remarked forming over an arm-chair. All eyes were rivetted upon this manifestation and the sitters observed the cloud becoming thicker and forming little puffs of cloudy vapour. Then the cloud seemed to divide and move towards the sitters floating over them and condensing while at the same time it revolved and shone with a yellowish golden sheen. Then a part divided and from the opening was seen to emerge the smiling form of the prelate, Bishop Camargo Barros, who had been drowned in a shipwreck. He was wearing his biretta and insignia of office and when he descended to earth he was minutely examined by a medical man. His respiration was verified and the saliva in his mouth examined: even the inner rumblings of the stomach were duly heard and noted. Other sitters also examined the figure and fully satisfied themselves that they were not the victims of illusion or disordered imagination. The Bishop then addressed them and told them to watch carefully the mode of his disappearance. The phantom then approached the medium who was lying in his chair in a deep trance, and bent over him. Suddenly the body of the phantom appeared to be convulsed in a strange manner and then began to shrink and seemingly to wither away. The medium, controlled by the sitters on either side, then began to snore loudly and break into a cold sweat, whilst the apparition continued to draw together until it was apparently absorbed and finally disappeared. Then again the room was pervaded by the sweet odor of roses.[3] (Dingwall 1930:299)

Yet another materialization report is likewise worth noting.

> During the course of a sitting a bell which was on the table rose ringing into the air. The medium awoke from his trance and told those present to look at the figure of an old man enveloped in a white mantle. While he was speak-

ing there was suddenly a loud noise and to the amazement of the sitters they found amongst them an old man as described by the medium. Two of the sitters recognized the phantom as that of a physician recently deceased and photographs were taken while the form was examined for some fifteen minutes by two medical men who stated that it appeared to be a normal human being. After the examination was completed the figure was seen dissolving away from the feet upwards until only the upper part of the body remained floating in the air. One of the medical men who had examined the figure rushed forward exclaiming "But this is too much!" and seized the half of the body floating in front of him. Uttering a cry he sank unconscious to the ground, while what was left of the phantom disappeared instantly. The sitting was closed and the doctor carried from the room and restoratives applied. When he recovered he told the sitters that what he felt was a spongy, flaccid mass of substance and that then he experienced some kind of a shock and fell to the ground. (Dingwall 1961:81)

At another sitting conducted in good light, Mirabelli, tied to his chair with bonds sealed, disappeared from the séance room and was found later in another room, "though the seals put on his bonds were intact, as were the seals on all the doors and windows of the séance room" (Inglis 1984:226). Moreover, the bonds remained in the room from which Mirabelli disappeared. They simply fell to the floor after Mirabelli disappeared.

Perhaps the most famous of Mirabelli's disappearances was his apparent spontaneous transportation from São Paulo's Luz train station to São Vicente, about 50 miles away. According to witnesses, he simply vanished from the platform, where he had been standing among friends. After about 15 minutes, those concerned friends got through by telephone to the home where they had all been heading, and were told that Mirabelli had been there for the past 15 minutes (mentioned several times in de Goes 1937, and also Dingwall 1930, Inglis 1984, and Playfair 2011).

Mirabelli was a polarizing figure for Brazilian Spiritism, especially because he was somewhat flamboyant, self-aggrandizing, and accepted substantial fees for his services. It's worth noting, then, that some of the testimony in Mirabelli's favor was provided by witnesses predisposed against the medium. Perhaps the most important account is that of Carlos Imbassahy, a highly respected figure in the orthodox Brazilian Spiritist community, and the author of the 528-page *O Espiritismo à Luz dos Fatos* (Spiritism in the Light of Facts), a history of psi phenomena (Imbassahy 1935). Imbassahy was clearly not an admirer of Mirabelli. He considered the medium to be "either a vulgar fraud, a skilful [sic] conjuror, or at most a medium who had got mixed up in the wrong company, both incarnate and discarnate" (Playfair 2011:47).

Imbassahy was at home one day with a businessman friend, Daniel de

Brito, when another friend arrived along with Mirabelli. Imbassahy reports that there was nobody he less wanted to see than Mirabelli. Characteristically, the medium made himself comfortable and started speaking in "detestable Italian mixed with Portuguese and Spanish words" (quoted in Playfair 2011:47, from Imbassahy 1935), purportedly from Cesare Lombroso. After that, he turned to de Brito and "proceeded to give the startled businessman an account of his life from the cradle onwards. Brito had never met him before, and was not a well-known figure himself, but the medium seemed to know all there was to know about him. Imbassahy was reluctantly impressed" (Playfair 2011:47).

Then, when Mirabelli learned that someone in Imbassahy's house was ill, he asked for some bottles of water, which a maid promptly brought and placed on a table four or five meters away from the medium. Mirabelli often would "magnetize" water as part of a ritual for his many efforts at mediumistic healing. The four men joined hands to form a "current"; light in the room was provided by two 100-watt bulbs; only the maid touched the bottles; Mirabelli had no time to prepare a trick; and his hands were held during the phenomena that followed. Imbassahy reports:

> Immediately, in full view of us all, one of the bottles rose half way up the height of the others, and hit them with full force for five or ten seconds, before returning to its place. We thought they must have been cracked. This was clearly seen and heard, with no shadow of hesitation. People in the next room also heard it, and the patient became extremely alarmed! (Quoted in and translated by Playfair 2011:47)

Imbassahy reluctantly concluded that Mirabelli had genuine mediumistic gifts, although he continued to disapprove of him personally.

When Playfair visited Brazil in 1973, he interviewed Mirabelli's son Regene, 'a businessman and accomplished amateur hypnotist with a keen interest in the scientific rather than the spiritual side of psychical research' (Playfair 2011:33). Playfair recorded some of Regene's fascinating recollections.

> I was sitting on the arm of a heavy renaissance-style sofa. Father liked me to stroke his hair, and I was doing this when the sofa simply began to move, with both of us sitting on it. Then I clearly saw the shadow of a figure on the floor in front of us; there was sunlight coming through a heavy glass window beside the sofa. Then the door of the cupboard across the room opened and a quill pen came out and was shot into the wooden floor like an arrow.

That incident sent Regene rushing from the room in terror, screaming for his mother. But,

> Out in the hallway there was a heavy brass cuspidor that had fallen over, blocking the passage. We heard loud bangs and crashes coming from a room beyond, and when I rushed in, there was Mother lying on the floor with every piece of the furniture in the room on top of her. She wasn't hurt because "they" had the consideration to place a thick mattress over her first! (Playfair 2011:33)

On another occasion Regene and the rest of the family joined a dozen friends for a session to help a bedridden invalid in another room. Regene reports:

> Father told us all to form a current, and he said not to worry about any phenomena that might happen. I was sitting about two meters from a table where there were three bottles of water, corked. This was to be "fluidized" and used to treat the sick man. We all sat there, and suddenly the bottles rose into the air, about thirty centimeters, and we heard three clinks as each struck the other. Then the bottles slowly began to turn over in mid-air, and stayed like that, upside down for a moment or two. I could see them very clearly, and the water inside them seemed to have gone solid, for it stayed in position, with a gap just under the cork. Then all the bottles fell hard onto the table and rolled about, although they did not break. (Playfair 2011:33–34)

It's also worth mentioning that investigators often closely monitored Mirabelli's physical condition during his various manifestations. Dingwall summarized their observations.

> [Mirabelli's] temperature, it was found, varied from 36.2 to 40.2: the pulse rate from 48 to 155; and the respiration was extremely various, sometimes being fast and stertorous and at others short and almost imperceptible. At times the body became rigid with cold sweats and abundant salivation was remarked, whilst occasionally there was general muscular contraction with tremors, glassy eyes and contracted pupils. (Dingwall 1930:298)

European and American Investigations

Eventually, news about Mirabelli began to spread more widely beyond the borders of Brazil, and at that point veteran American and European researchers began taking an active interest in the case. In August 1928 philosopher and SPR president (1926–1927) Hans Driesch sat with Mirabelli, and later wrote a letter recounting his experiences (Driesch 1930).

Driesch was clearly unimpressed with the linguistic productions he observed. Mirabelli spoke Italian (in which Driesch was fluent) as if the medium's father were speaking through him. But Driesch wrote: "There was not the slightest idea of a 'trance' and I believe the whole affair was

not at all genuine, but a comedy" (Driesch 1930). Later, Mirabelli seemed to speak Estonian to a young Estonian girl he had brought with him, but Driesch could not believe that the girl's father was really speaking through the medium. He assumed instead that Mirabelli had probably learned some Estonian.

However, Driesch was somewhat more sympathetic regarding Mirabelli's physical phenomena. As the company entered the hostess's dressing room, "Mirabelli cried and said some prayers and then, suddenly, a small vase on one of the tables began to move and finally fell down. I could not observe any sort of mechanical arrangement such as a wire or string or otherwise" (Driesch 1930).

Driesch was highly suspicious of several apports that occurred on this occasion, especially since Mirabelli wore a large overcoat "with enormous pockets" (Driesch 1930). But there was more. For example, Driesch, Mirabelli, and their hostess stood on a veranda whose windows were closed (and therefore on which there was no wind), and other members of the company stood inside the adjacent drawing room. Mirabelli began to pray for a sign, and then the open folding doors between the veranda and drawing room slowly closed. "This was seen at the same time by the persons in the drawing room and those on the veranda. It was rather impressive, and no mechanical arrangements could be found" (Driesch 1930). But Driesch added, cautiously, "Mirabelli had been in Pritze's villa already about an hour before we arrived, alone with Frau Pritze. He *may* have made some arrangement before we came—I do not say that he did" (Driesch 1930).

In January, 1934, SPR member May Walker had sittings with Mirabelli and published a short and favorable report soon after (Walker 1934). For the first sitting,

> There were four phenomena in all, witnessed in good white light sufficient to see each person clearly and also all the objects in the room. My camera, with which I had just taken a photograph of the medium, was lying on a long wooden table at some distance from where we were standing holding Mirabelli's hands. It began to move about on the table and jumped on to the floor. A small fan laid on my upturned palms, began to wriggle about as if alive, then falling off. In this case, Mirabelli's fingers were near my hands but not touching them and it almost seemed as if some magnetism issued from his fingers, causing the fan to move.
>
> My hat, a large straw one, turned completely round on the table and three tall glass bottles filled with water all shook together. Later one of them fell over on its side. There was an interval of some minutes between each phenomenon. (Walker 1934:75–76)

The second sitting took place in a private garden, "owing to the fact that so many things in the house had been broken by psychic means" (Walker 1934:76). It was held in the evening, "well lit by electric lamps" (Walker 1934), and most of the phenomena were apports, which Walker found moderately persuasive. However, she wisely preferred indoor phenomena, and the next evening her wish was granted.

The third sitting began with some object movements and an apport, the authenticity of which Walker was not prepared to endorse. But, she said,

> Of the last phenomena, however, I had no doubts. All of us adjourned to the back room, where, on a table against the far wall, were about a dozen large wine bottles filled with water.
>
> We formed a chain in a semi-circle at the other side of the room, Mirabelli being at one end of it, but a considerable distance from the table. He asked for a sign that the water had been magnetized—which I understand he thinks is done by his father, who has passed over.
>
> Immediately came the jingling together of the bottles;—then a loud noise which shook them still more, as if some one has rapped on the table. After a slight pause, one bottle fell over on its side. (Walker 1934:77)

Regrettably, Walker doesn't indicate why she was certain that Mirabelli hadn't prepared the bottles somehow in advance. In any case, she concluded that Mirabelli had presented her with "the best telekinesis I have ever seen" (Walker 1934:78).

Later the same year (in August), SPR's Theodore Besterman visited Mirabelli. By this time Besterman had already established himself as critically cautious but open-minded with regard to at least moderate-scale demonstrations of physical mediumship. For example, his often-cited study of slate-writing showed that under certain (rather poor) séance conditions and for certain kinds of small-scale ostensibly paranormal phenomena, subjects can err in their observations and sometimes report events that never occurred (Besterman 1932b). But Besterman was also prepared to endorse the carefully obtained evidence for Rudi Schneider's ability to deflect an infrared beam at a distance (Besterman 1932a).

However, when it came to Mirabelli, it seems that something simply rubbed Besterman the wrong way, right from the start. In fact, it may be that he was predisposed to distrust Mirabelli, because four years earlier he had skeptically reviewed the published accounts that were available at the time (Besterman 1930).

At any rate, during his visit to Brazil, Eurico de Goes, "one of Brazil's first serious psychical researchers" (Playfair 2011:24), took minutes of the several sessions (at least 5) that Besterman attended. According to those minutes,

flowers materialized, bottles on a table jumped around, one even hopping onto the floor, a picture left the wall to float in mid-air and land abruptly on someone's head, a chair slid along the floor for about ten feet, the front-door key drifted out of its lock, and Mirabelli came up with a learned written discourse in French, writing nearly 1800 words in 53 minutes. (Playfair 2011:27)

Initially at least, Besterman seemed to be impressed. At least that's how he presented himself to his Brazilian hosts. de Goes quoted him in English as having written "Mr Mirabelli's phenomena [are] of the greatest interest. . . . Many of them were unique of their kind" (Playfair 2011). Notice that this quote does not endorse the phenomena as authentic, and it does not contradict his earlier skeptical review of the published accounts of Mirabelli. So it's not really surprising that by the time Besterman wrote his 1934 report for the SPR *Journal*, he showed little if any enthusiasm for what he'd observed in Brazil. Indeed, in his often sarcastic and condescending report he accused Mirabelli of fraud and provided some examples of phenomena he believed to have been faked.

Significantly, in Besterman's sessions, Mirabelli didn't allow the sorts of controls reported in some of the most striking cases mentioned earlier—for example, binding Mirabelli to an armchair and sealing the bonds. Besterman reported that it was clear he was allowed to be no more than a spectator, and he remarked, "No sort of control was at any time exercised, suggested or asked for by any sitter other than myself, and then without success" (Besterman 1935:144). Séances were held in the evening, with illumination varying from complete darkness to bright electric light from seven or eight uncovered bulbs.

The largest group of phenomena witnessed by Besterman were apports, which Besterman claimed "were undoubtedly all faked" (Besterman 1935:145) and facilitated by obvious methods of distraction and occasionally by darkness as well. Besterman also reported moving bottles of "magnetized" water, similar to what Walker had reported months earlier. However, in Besterman's case, the phenomenon occurred in darkness. Not surprisingly, Besterman conjectured that Mirabelli looped a black thread around the moved bottle (rather than attaching it to the bottle) so that it could be easily retrieved.

After briefly mentioning and dismissing some other minor physical phenomena, Besterman then reported two other examples in detail. The first does, indeed, seem to have been a simple conjuring trick, as Besterman noted. Besterman described the performance as follows:

[Mirabelli] went into another room accompanied by [one of the sitters], there, we were told, [he] held the coin in his open palm, with the sitter's open palm over it. The coin then vanished, Mirabelli returned to the room in which we were sitting, and asked me where I wanted the coin re-materialised. I elected for my own pocket and in a moment or two Mirabelli announced that the coin had been precipitated into my breast-pocket; there I duly found it. This performance was repeated with each of the male sitters present, with success, except that on one occasion I ventured correctly to forecast to my neighbour where the coin would be found. It must be noted that at no time during the progress of this phenomenon did Mirabelli approach within three yards of the main body of sitters.

As Besterman correctly observed,

The way this trick was done was simple in the extreme. At a given moment, before the lecture, Mirabelli asked the male sitters one by one into an adjoining room, where he examined them "magnetically," making passes over them, etc. While doing so he slipped a coin into the pocket of each "patient." The vanishing of the coin is of course elementary palming, and the rest is obvious. All that is required is unlimited impudence and a sufficient number of similar coins. What first aroused my suspicion was this: when asked to examine the 1869 coin I *did* examine it and made a mental note of its characteristics. When I found the coin in my breast-pocket I immediately saw, from minute characteristic marks, that it was not the same one, and the rest was then obvious. Again, every coin was found in an outside breast-pocket except X's, who had his materialised into his hip pocket, and X had been the only "patient" who had been asked to take his jacket off, as I happened by chance to notice. (Besterman 1935:148)

Besterman claimed that only one phenomenon during his sittings was "really impressive." This was the turning of a blackboard placed on the top of a bottle, occurring in bright light sufficient for filming the event, and with the medium and sitters holding their hands over the board. This occurred twice, and Besterman was unable to duplicate the effect by blowing on the board. He was also certain that no threads were used. He wrote:

I am still puzzled by this phenomenon; taking into account the good light, the fact that Mirabelli performs the phenomenon completely surrounded by standing "sitters," who seem to have complete liberty of movement, and the fact that he expressed no objection whatever to the filming, although I strongly emphasised the fact that the camera and the film were very special ones and would show every detail, the fact that Mirabelli allowed me on each occasion to arrange the *mise en scène* and did not precipitate himself on the board as it fell, the fact that the room, the table, and the bottle were all different, though the board was the same, all these circumstances make

the hypothesis of threads practically impossible, while any other fraudulent method is difficult to conceive. (Besterman 1938:148)

Besterman's report elicited a sharply critical response from Dingwall (Dingwall 1936), claiming that Besterman was merely "bringing back stories of silly tricks" (Dingwall 1936:169). His remarks criticized not only Besterman's negative appraisal of Mirabelli, but his positive views as well, and are worth excerpting.

> Mr Besterman has come to a surprising conclusion. He thinks that there is a *prima facie* case that Mirabelli may possess some paranormal "faculty," and this is based on the fact he was unable to detect the *modus operandi* of a revolving blackboard effect. Apart from the fact that there was no reason why he should have been able to understand it, are we expected to believe . . . that because . . . [Mr Besterman] could not and cannot discover how certain conjuring tricks are done there is a *prima facie* case for the successful performers possessing "paranormal" faculties? It is this that makes psychical research ridiculous, and rightly so.
>
> In my account of Mirabelli, which was printed in 1930 by the A.S.P.R., I described certain phenomena and named the parties who were said to have been present. . . . Did Mr Besterman interview any one of these persons? Did he talk to any of the sitters who are recorded as being present at the alleged materializations of Bishop Barros, Prof. Ferreira, or Dr de Souza's daughter? To say that their testimony "is of relatively little value" is beside the point. It is as valuable as that of Mr Besterman, since what they record is quite as striking as anything with D. D. Home. Do these witnesses exist? Were they present at these sittings? Were they lying or are they made to record phenomena which never took place at all? Or must we admit that certain "*events took place which were described by those who witnessed them in the terms we have read*"? What were those events?" I wrote these words in 1930. No answer has been attempted. Yet in 1934, at heavy cost to the S.P.R., Mr Besterman goes to South America ostensibly to inquire into what he terms Mirabelli's "astounding feats" and comes back with tales of revolving objects which puzzled him.
>
> The problem of Mirabelli is the same as that of Home. In the latter case the witnesses are dead and cannot now be interviewed: in the former case they are living and can be seen and cross-examined. Signed statements by Dr G. de Souza, Dr Moura, or Dr Mendonça describing in their own words what they saw on certain occasions as recorded in *0 Medium Mirabelli* would be worth far more than stories of revolving blackboards and jumping cameras which puzzled observers who would be equally puzzled by 90% of conjuring tricks performed by even moderately skilled artistes. (Dingwall 1936:169–170)

To this, Besterman responded simply that Dingwall's criticisms called

"for little comment" (Besterman 1936:236). But Dingwall was justified in complaining that Besterman made no effort to follow up on the most intriguing eyewitness reports of dramatic phenomena under good controls. Fortunately, but much later, Playfair was able to interview some of the surviving sitters at Mirabelli's séances, and that material informs his detailed account (Playfair 2011). Playfair also generously concedes: "It must be said that little useful research can be done in two or three weeks in Brazil even today, and even when one speaks Portuguese, as I do and he did not" (Playfair 2011:44).

So readers should keep in mind that Besterman claimed never to have observed the most dramatic phenomena on which Mirabelli's fame largely rests, and it should be mentioned again that he never observed the medium submitting to the seemingly good controls so often reported by others during those events. This is somewhat reminiscent of a feature of the case of Eusapia Palladino, whose most impressive phenomena often occurred under the most stringent controls (see for example Feilding 1963, Feilding, Baggally, & Carrington 1909), and who had few if any reservations about cheating when conditions were looser, or when she disliked her investigators, or when she was lazy, or when the "force" was weak (see the discussion of Palladino in Braude 1997).

However, as Playfair noted, Besterman may indeed have witnessed something more spectacular and less amenable to charges of chicanery. He may have intentionally failed to report an apparently impressive materialization. This was evidently not a full-figure materialization, but rather "radiations . . . on a corner of the table" (de Goes 1937:125). Playfair reports:

> At the very first meeting, according to the minutes [of the séances], Mirabelli announced that he could see an entity named Zabelle, whom he described in detail. Besterman said he had known a lady of that name in London who was now dead, and when he asked for a sign of her presence, bottles began to jump around on a table, one of them even falling on to the floor at his request. Besterman mentions the bottles, but not the mysterious Zabelle.
>
> At the second meeting, Zabelle again dropped in and became visible enough for Dr Thadeu de Medeiros to take a photograph of her. This is reproduced in de Goes's book, and is one of the more credible materialization photographs I have seen. . . . According to the minutes, which de Goes reports Besterman as having signed, Zabelle performed a number of feats to prove her presence.
>
> In the minutes of the third meeting, we are told that Besterman examined the photograph of Zabelle and declared that there was a strong resemblance to the lady he had known. The face on the photograph is extremely clear, more so than in most pictures of this kind. [See Figure 2]

Figure 2. Apparent materialization of Zabelle.

Besterman's failure to mention these incidents is certainly surprising. de Goes's minutes claim that at the first of the three meetings "Besterman . . . confessed that he had never seen anything so interesting" (de Goes 1937:105). Playfair correctly observes,

> It is surprising that Besterman makes no mention of this episode. It is clear from his lengthy published report that he was anxious to miss no opportunity to discredit Mirabelli's powers, and if the Zabelle story were untrue, here was an excellent opportunity to do so.
>
> If, on the other hand, it was true, then Besterman is guilty of suppressing strong evidence in favour of the medium. (Playfair 2011:45)

The Phantom Ladder

However, if one wants to find evidence of Mirabelli cheating in connection with his more spectacular manifestations, one need only consider the famous (or at least notorious) photograph of Mirabelli allegedly levitating (see Figure 3a and 3b). This photo was published outside of Brazil for the first time in the first (1975) edition of Playfair's *The Flying Cow*. And in that book Playfair noted that he was unable to authenticate the photo, and that it might be faked.

Confirmation came in 1990, when American researcher Gordon Stein found an original print of the photo in the SPR archives in the Cambridge University Library, showing clearly that the image had been retouched to remove the ladder upon which Mirabelli was standing. It's unclear whether the original negative had been retouched, or whether a print was manipulated and then re-photographed. But in any case, the damning evidence is clear (see Figure 4), and Stein was undoubtedly justified in claiming that Mirabelli "knowingly passed off a fraudulent photo of himself as genuine"
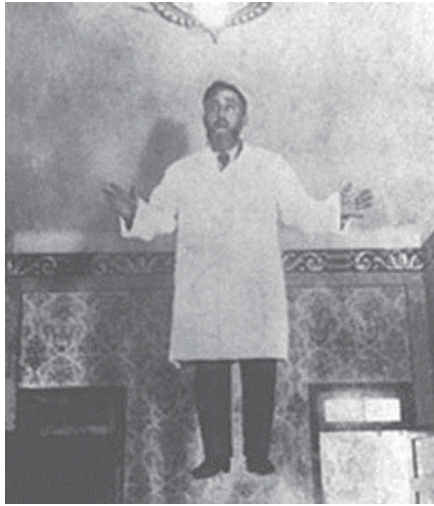
Figure 3a. Mirabelli apparently levitating.
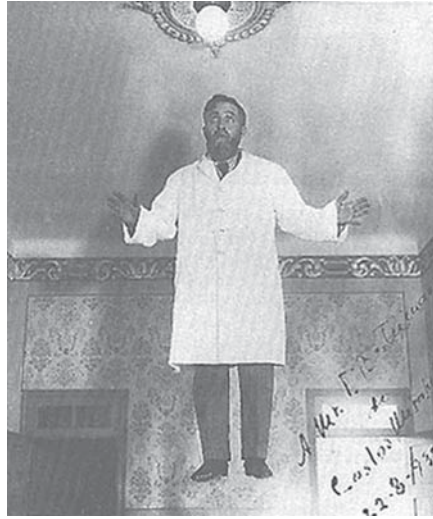
Figure 3b. Phantom ladder photo inscribed to Besterman.

Figure 4. The signs of retouching the photo to hide the ladder.[4]

(Stein 1991). Curiously, Mirabelli had signed the print and inscribed it "To Mr Theodore Besterman." And equally curiously, Besterman—clearly no fan of Mirabelli—failed to seize the opportunity to mention the obvious fraud in his report. At any rate, Playfair was also quick to publish a paper discussing the discovered fraud, and he updated the account of Mirabelli in a later edition of his book (Playfair 1992, 2011).

Conclusion

Obviously, the case of Mirabelli must be regarded, *at best*, as one of so-called "mixed mediumship"—that is combining fraudulent with genuine phenomena. Equally obviously, and as the case of Palladino illustrates clearly, one can't plausibly argue that a person who cheats once will cheat all the time. Indeed, as noted above, there can be obvious (and perhaps even defensible) reasons for a medium cheating occasionally. In fact, an irony of the Palladino case is that her willingness to cheat when allowed set the stage for the most convincing and stringently controlled séances in her career—the 1908 Naples sittings (Feilding 1963, Feilding, Baggally, & Carrington 1909).[5]

But assuming that Mirabelli wasn't fully, exclusively, and honorably devoted to promoting Spiritism, what might his reasons have been to cheat? The most obvious candidates would be money and fame. Now there's nothing inherently scandalous in someone suitably psychically gifted wanting to make mediumship a primary source of income. However, perhaps there's more to the story than that. By all accounts, Mirabelli was not averse to liberal self-promotion; Playfair describes him as flamboyant and vain. He also claims that Mirabelli 'was a big spender, who would think nothing of buying ten suits or a dozen pairs of shoes at a time, only to give most of them away' (Playfair 2011:26). Clearly, that's a lifestyle that somehow needs to be funded.

Besterman's spin on Mirabelli's fiscal profile is somewhat less neutral. He wrote:

> Though he is anxious not to be considered as a professional, in fact he is. Directly or indirectly Mirabelli demands and obtains (as I know only too well) substantial fees, far more substantial, indeed, than any ever asked of me before when attending sittings on a medium's own premises. The procedure is this: Mirabelli founds or causes to be founded an institute, for which he works, on the premises of which he lives, and to which sitters make payment. It was in this way that the Academia de Estudos Psychicos "Cesar Lombroso" was founded at São Paulo in September 1919; and Mirabelli's move to Rio de Janeiro led to the foundation there, in November 1933, of the Institute Psiquico Brasileiro. (Besterman 1935:142)

So if we're to assume that Mirabelli was nothing but a cheat, and that he cheated not only to live comfortably, but to live lavishly as well, how do we account for the reports of his most compelling manifestations, and his materializations in particular? Skeptics might initially appeal to the usual suspects, malobservation, naivete, and collusion among witnesses. But that would seem to require an implausibly large number of gullible, incompe-

tent, or corrupt people, whose otherwise primary disqualification as witnesses is that they weren't fortunate enough to be SPR insiders. Mirabelli's manifestations were observed by more than five hundred people (more than one hundred of them foreigners), often supported by photographs, and as mentioned earlier the phenomena were typically produced in bright light and often under decent controls.

Nevertheless, Besterman condescendingly impugned the competence of the many witnesses who testified to Mirabelli's most dramatic manifestations. He wrote:

> . . . their testimony is of relatively little value. These gentlemen have in most cases had no experience with mediums other than Mirabelli, and they have no notion of the conditions under which psychical research should be conducted. Their testimony, in short, has such value as can be given to evidence put forward by inexperienced and more or less casual visitors, concerning events over which they have no control. (Besterman 1935:143)

However, it's Besterman whose claims appear to be worthless. For one thing, many of Mirabelli's impressive and well-documented phenomena evidently did in fact occur under good controls, including (as we've seen) bright light, sealed binding of the medium to his chair, and holding a materialized phantom as it melted in the observer's grasp. Moreover, in many cases, knowing how to control or properly observe such large-scale manifestations, often in locations at which Mirabelli had no opportunity to prepare a trick, doesn't require a rich prior history of mediumistic investigation. Besides, the phenomena continued for many years. Members of Mirabelli's regular investigations probably learned from experience and became more careful and shrewd with time. Revealingly, Besterman admitted in a footnote to the passage quoted above, that his judgment was based, not on any attempt to meet with and query those whose reports he was impugning, but rather on his very limited experiences with Mirabelli, which (we've noted) were not controlled.

At least Besterman didn't resort to the famously lame hypotheses of collective hypnosis or collective hallucination. I've dealt in detail elsewhere with these last-ditch maneuvers (Braude 1997, 2007), and so I'll just mention a few salient points here. First, regarding hypnosis: There simply is no evidence that the appropriate kind of mass hypnosis has ever occurred—that is, inducing people to issue the same or concordant observational reports in conditions widely recognized as being unfavorable to hypnosis, and (even more important) despite the well-known and great variability in human hypnotic susceptibility. Actually, if a medium could, through suggestion, get different people, of different degrees of hypnotizability, simultaneously to

experience and report the same phenomena, and also do this under conditions unfavorable to suggestion, that ability would arguably be as paranormal as what it's supposed to explain away. In fact, it looks suspiciously like telepathic influence.

The second hypothesis, of collective hallucination, is simply ridiculous. It can't even remotely account for Mirabelli's continued success under good conditions, and often for many years. Since Mirabelli's witnesses weren't engaged in something like mushroom rituals, there would have to be a lot of spontaneous hallucinating going on, over decades, remarkably resulting in people having the same or similar non-veridical experiences. Besides, this hypothesis fails to account for the causal relevance of Mirabelli's presence. If the medium had nothing to do with witnesses' allegedly false observational reports, why were they hallucinating in the first place? But if Mirabelli was responsible, then (since he presumably wasn't dispensing hallucinogens) it looks like this hypothesis is really just one of collective hypnosis, the inadequacy of which we've just noted.

But can the materializations at least be explained away satisfactorily by positing an array of confederates posing as the deceased? Dingwall disposed of that conjecture:

> I will even grant the possibility of wholesale confederacy and assume (for the sake of argument) that the materializations are confederates of the medium or of the sitters. But confederates are human beings and human beings do not usually rise into the air, dissolve into pieces and float about in clouds of vapor. Confederates do not lose half their bodies, feel like flaccid sponges and give violent shocks to people who try to seize them. (Dingwall 1930:301–302)

It's also worth noting that Mirabelli's reported phenomena are not particularly outlandish when compared to lesser materialization phenomena for which there exists good evidence (see, e.g., Braude 1997, Inglis 1977, Weaver 2015). Some are simply more complete, complex, or virtuosic. Besides, in the absence of any kind of credible scale for determining degrees of strangeness, and in view of the abundance of decent evidence for partial materializations (including evidence from the cases of Home and Palladino), we would do well to heed Richet's warning that

> it is as difficult to understand the materialization of a living hand, warm, articulated, and mobile, or even of a single finger, as to understand the materialization of an entire personality which comes and goes, speaks, and moves the veil that covers him. (Richet 1923/1975:491)

The fact remains that many of Mirabelli's apparently well-attested and decently controlled manifestations resist easy—or any—plausible skeptical dismissal. Certainly, Besterman's exposure of and conjectures about conjuring tricks under no controls fails to address the challenge posed by the much more spectacular and controlled physical phenomena reported in Mirabelli's case. So although Mirabelli's manifestations are perhaps not as well-established as, say, the best of D. D. Home, Eusapia Palladino, Kluski, and others, good reasons remain for taking the case seriously, and perhaps for regarding it as indicating just how dramatic PK phenomena can be.[6]

Notes

[1] Mirabelli changed his name when he was young, concerned over the similarity between his name and the woman's name Carmen.

[2] Moreover, because most of the primary material in this case is written in Portuguese, which I do not know (but for which online translation programs provided some help), this report inevitably focuses on the accounts written in English.

[3] See Figure 1 for a photo of a materialized poet.

[4] I found this image in an online search. Evidently it was taken from a BBC program covering the case of Mirabelli, but apart from that I don't know its origin. I should mention that I have a print of the ostensible levitation, and I've seen many others. In all of those, the retouching is not so obvious. Nevertheless, all the prints I've seen show a noticeable difference in clarity between the wallpaper behind Mirabelli's torso and that behind his feet. This can be seen in Figure 3a and 3b.

[5] This series of sittings was criticized toothlessly—in fact, absurdly—by Richard Wiseman. For an analysis of his critique, see Braude 1997:Chapter 2.

[6] I'm very grateful to Carlos Alvarado, Leslie Kean, Michael Nahm, and Guy Playfair for helpful comments on several ancestors of this paper.

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Selected Aspects of Carlos Mirabelli's Mediumship

Michael Nahm

Abstract—In the present issue of this *Journal*, Stephen Braude summarized the enigmatic case of Brazilian medium Carlos Mirabelli, who, like many other physical mediums, was hailed as an outstanding genuine medium by some and regarded as a complete fraud by others. In this article, I present an overview on two aspects of the Mirabelli mediumship. First, I introduce historical material that relates to the context of the somewhat famous visit of biologist and philosopher Hans Driesch at Mirabelli's in 1928, along with a few comments from my side; and I will then present two apparently little-known accounts of sittings that Mirabelli held in 1930 and 1935 in New York.

A Commentary on Hans Driesch's Sitting with Carlos Mirabelli in 1928

After Albert von Schrenck-Notzing published a German summary of a Portuguese book entitled *O Medium Mirabelli* (Mikulasch 1926, Schrenck-Notzing 1927), discussions about Mirabelli commenced in Germany. For example, in a radio talk held on December 13, 1927, Max Dessoir stated that he received a letter from a woman who claimed that Mirabelli, who was now often discussed in periodicals, had already been exposed as a swindler.[1] According to this woman, Mirabelli was caught faking during a sitting her husband attended—but apparently no further details were given. This commentary by Dessoir was cited in an article by Christoph Schröder (1928), then editor of the German parapsychological journal *Zeitschrift für Psychische Forschung*. Schröder, who was on unfriendly terms with Dessoir, then presented a summary of the Mirabelli sitting on August 2, 1928—the "famous" sitting that Hans Driesch had attended when he visited Brazil and Argentina (Driesch 1930, 1951, see also Braude in this issue). Schröder's article contained the rather short séance protocol, the text of which was provided by Bernardo Pritze, a German-born director of the exchange department of the Transatlantic German Bank in São Paulo (de Goes 1937:210), in whose small villa in a suburb of São Paulo the sitting took place (Figure 1). Pritze added a few personal remarks on the events witnessed.

Figure 1. Carlos Mirabelli (left) and Eurico de Goes in the garden of Bernardo and Brigida Pritze's villa, in which the sitting with Hans Driesch took place on August 2, 1928. (Photo taken from de Goes 1937, and digitally revamped by M. Nahm.)

Apparently, Pritze had a keen interest in parapsychology. He visited Schröder and also Driesch earlier in 1928, and was also in contact with Schrenck-Notzing. Thereafter, he aimed at establishing contact with Mirabelli. Obviously he succeeded, and he arranged a rather informal "sitting" with Mirabelli in August for himself, Driesch, and both their wives.

Pritze seemed to be especially impressed by the ostensible apport phenomena that occurred in his home. He briefly recounted several examples in his letter to Schröder, stressing that at least one object must have come out of a locked drawer. Moreover, he confirmed that, as stated in the séance protocol, his wife Brigida Pritze saw an apparition of a woman who appeared at the request of Mirabelli. Although it seemed to be only vaguely visible, Mrs. Pritze claimed that she could clearly distinguish the

moving apparition's dark dress and white overcoat. However, this apparition was only seen by her. Only she and Driesch were in the room in which it appeared, but Driesch stated he didn't see it (without giving possible reasons why). Brigida Pritze published the séance protocol in another parapsychological journal as well, along with a few personal comments from her side (Brigida Pritze 1928). The phenomenon that impressed Driesch most, a seemingly inexplicable movement of two folding doors, was originally not mentioned in the séance protocol, but was added in a footnote by Driesch. He described this event in more detail later (Driesch 1930, 1951). Also, in his autobiography, Driesch stated that after several glass bottles had mysteriously leaned forward and backward a number of times in full light, he immediately stepped between these bottles and Mirabelli, but that there were no threads or wires (Driesch 1951). He had no idea how Mirabelli could have faked these object movements.

In her article, Brigida Pritze (1928) described other telekineses and apport phenomena that were observed on September 21, 1928. For example, during the morning of this day, the spectacles of Mrs. Pritze were transported 10–12 km from their villa to the house of Eurico de Goes, where Mirabelli was staying. He had not been to the Pritzes' house for several days (see also de Goes 1937:63).[2] Later on this day, Bernardo Pritze, Eurico de Goes, Mirabelli and his wife, and the poet and diplomat Sir Douglas Ainslie arrived at the Pritze's villa to hold a sitting. Yet, when they approached its entrance, Mirabelli seemed to become possessed by a spirit, and prevented them from entering the house. He announced that a small clock owned by Ainslie, which had allegedly been dematerialized in Ainslie's hotel room approximately 10 km away the evening before, would materialize in the corridor inside the house. When Ainsley entered the house alone to look for his clock, he indeed found it in the corridor as Mirabelli had announced (see also de Goes 1937:62).

The Pritzes met Mirabelli on a few more occasions, and experienced a number of other puzzling apport phenomena that often entailed the appearance of objects that were usually stored in locked locations. For instance, when they paid Mirabelli an unexpected and unannounced visit at his home (approximately 60 km from their villa), Mirabelli claimed that a revolver of the brand "Browning" was soon to materialize. Shortly after, such a revolver fell down next to the feet of Mr. Pritze, to whom it belonged. It had been stored in a locked cupboard in his home (de Goes 1937:184). Similarly, when they drove in their car together with Mirabelli, various objects would appear inside the car. Some of them had been stored in locked drawers, the keys for which were in the possession of Mr. Pritze (de Goes 1937:212).

Although both Pritzes sympathized with spiritist attitudes, they retained a critical attitude. In a short article, Bernardo Pritze explained why he, in agreement with Pascal Forthuny, with whom he corresponded, didn't think that the personalities communicating through Mirabelli were who they claimed to be, but that they were largely produced by the medium's subconscious mind (Bernardo Pritze 1929). When May Walker, a wealthy member of the British Society for Psychical Research (Walther 1955, 1960), tried to locate Mirabelli in January 1934 and also contacted the Pritzes, they responded that they had not been in touch with Mirabelli for three or four years. Thus, it seems, they were only in contact with Mirabelli for a relatively short time.

In contrast to the Pritzes, Driesch remained skeptical regarding the apports that occurred at their sitting. In his report, he stated that he considered them "far from convincing; for I never saw the path of the apported object in the air, but only saw it when it had reached the ground. And Mirabelli was in a large overcoat with enormous pockets" (Driesch 1930:487). However, judging by the material presented by the Pritzes and Eurico de Goes, his suspicions regarding the ostensible apport phenomena might have been exaggerated.[3] Driesch was known to be cautious when it came to accepting the reality of physical phenomena of mediumship, but some of the dismissive statements in the concluding section of his account (Driesch 1930) seem inappropriate. Apparently, they rested to a considerable degree on his inability to find out who had published the book *O Medium Mirabelli* in 1926, because Schrenck-Notzing didn't include Mikulasch's name in his German summary (Schrenck-Notzing 1927).[4] Driesch (1930) claimed that he asked the Pritzes and an "intimate friend of Mirabelli's" about the authorship of this book, but that they didn't know about it. As a consequence, Driesch mused with regard to the authentication of the records contained therein that Mirabelli might have written the book himself, complained about the weakness of the phenomena he was able to observe in comparison to those described in the book, and stated that "everything must absolutely remain *in dubio*" (Driesch 1930:487).

But, if the authorship of this book was so important for Driesch, why didn't he ask Mirabelli himself about its authorship? And, given that he was interested in the authentication of the available records: Why didn't he seek much earlier before his visit to Brazil to establish contact with persons who knew Mirabelli, or, in case this was too difficult, also later via Mr. Pritze, Mirabelli himself, or the latter's "intimate friend"? It also remains obscure why Driesch built his skepticism additionally on the argument that neither Mr. Pritze nor Mirabelli's "intimate friend" had seen the phenomena described in Mikulasch's book. Regarding Mr. Pritze, it is obvious that he,

like Driesch himself, didn't know Mirabelli before summer 1928. Judging by the sources available, it seems very likely that the sitting on August 2, 1928, was also the Pritzes' first sitting with Mirabelli. And, curiously, the "intimate friend" mentioned by Driesch must have been nobody else than Eurico de Goes, who indeed had known Mirabelli since about 1917 (de Goes 1937), and who frequently cited Mikulasch's book in his own book to be published in 1937.[5] Driesch even met Mirabelli, Mr. Pritze, and de Goes again in October 1928 (Driesch 1951). When May Walker met with de Goes during her visit to São Paulo, she stated that she had a long and interesting talk with him, and that he, as might be expected, related "all sorts of wonders concerning materializations and levitations" to her (Walker 1934:74).

In any case, according to Mikulasch (1926), of 110 sittings held for physical phenomena, 47 were negative, and 35 of these negative sittings were held in the facilities of the Cesar Lombroso Academy of Psychical Studies that was founded to investigate Mirabelli's mediumistic abilities.[6] Apparently, it was not uncommon that Mirabelli's sittings for physical phenomena were rather uneventful even when they were held in a supportive environment. And, according to Bernardo Pritze (Schröder 1928), Mirabelli was sickly for an extended period in 1928, which might have contributed to the production of comparably weak phenomena during this year. Be that as it may, Driesch was well aware that the proven reality of phenomena of physical mediumship would bear an enormous significance for philosophy and natural sciences. Thus, he frequently urged parapsychologists to investigate them under scientifically satisfying conditions. This also applied to Mirabelli. Even though he considered him a partial fraud, he was impressed enough by what he witnessed to recommend further studies with him (Driesch 1930).

Carlos Mirabelli in New York

In the following, I briefly summarize the events of two apparently little-known sittings that Mirabelli held in New York. Unfortunately, particularly the first report is unduly short, as is so often the case with Mirabelli. Still, the occurrences described match the descriptions of other remarkable physical phenomena presented especially by Mikulasch (1926) and de Goes (1937). Still, provided these reports are not completely invented, they show that Mirabelli accepted invitations from foreign academics—apparently including skeptical ones.

The first of the sittings in New York took place in 1930. It was first described in the Italian journal *Mondo Occulto* (Rosacroce 1930), and then reprinted in Italian by de Goes (1937:187). According to this report, Mirabelli was invited by a group of university professors in New York,

among them several physicians, to hold a controlled séance in full light at 3 p.m. in the facilities of an Institute for Chemistry. Photographic equipment was mounted. After a few minutes, during which he looked into a mirror, Mirabelli fell into trance. In the middle of the room, a blurred smoke-like column of about one meter height formed, a few meters away from the medium. This contour-less shape then condensed and formed a single and very white arm, as if it was formed out of the nebulous mass, which slowly vanished. It was the arm of a woman. One finger wore a ring. Suddenly, the ring disengaged from the finger, fell down, and rolled underneath a small table. Immediately thereafter, the arm began to dissolve again into the nebulous shape, which grew ever more transparent and disappeared. This phenomenon took six minutes and was photographed by two cameras. There were no other materializations during this sitting. The ring turned out to be a golden wedding ring with the inscription "J. Irving" on its inside. Indeed, one of the sitters was an industrialist named "John Irving," and he had lost his young wife three years ago in an automobile accident after being married to her for two years. Irving attested that the wedding ring his wife was buried with was identical to the ring that mysteriously dropped from the finger of the materialized arm. It is stated that this sitting left a deep impression on all sitters, including the skeptical ones.

The other sitting Mirabelli was reported having held in New York took place in 1935. Its account was first published by a "special correspondent" from New York in an Austrian periodical, *Neues Wiener Journal*, on March 25, 1935 (Anonymous 1935), and was summarized in Hans Gerloff's book about Mirabelli (1960).[7] The sitting was held in the house of a physician named Dr. Schelders at 3 p.m. in full light, and was additionally attended by the latter's wife and 11 guests who were friends. The salon, in which the sitting was to take place, was cleared except for the necessary chairs, a small table, and two cinematographic cameras. Mirabelli sat in an armchair, chatted with the sitters, and eventually gazed into a small crystal ball he brought with him, until after about five minutes he seemed to fall into a trance-like state. After about ten more minutes, a thin smoke-like column appeared in close vicinity to the medium, as if it came out of the wooden floor next to him. Within a very short time, however, this smoky mass seemed to condense and it assumed the shape of an elderly man dressed in damaged clothing. He appeared embarrassed to find himself among a group people who stared at him, and, vice versa, the sitters appeared embarrassed by the sudden appearance of a man who looked perfectly human—were it not that he appeared among them in a rather unusual manner. The first person who dared to break the uncomfortable silence was an advocate by the name of J. Johnson who asked the apparition for his name and whereabouts.

Uncertainly, he replied that he didn't know how he came here and that he didn't belong here. Yet, he stated that his name was John Ronaldson, and that he was born on February 23, 1875, in Saint Louis, thereafter spending most of his time in New York. He claimed he killed someone named Carlington and was then sent to prison. A judge named Valentin Mewes took a drinking glass from the small table and asked the apparition to leave the fingerprints of his right hand as a souvenir, which he did. When asked where he lived after his time in prison, Mirabelli's body, which had lain motionless on his chair until now, cramped, and the apparition seemed to lose his feet and legs, and to hover in the air.[8] It was then covered by a smoke-like column, which seemed to dissolve after several minutes until nothing was left. Two physicians, Drs. Ercole and Hutchinson, claimed that the phenomenon must have been a collective hallucination. However, the employed cameras captured the entire process of Ronaldson's appearance, and thus proved them wrong.

Valentin Mewes took the glass with the fingerprints with him, photographed them, and asked the police records department for their identification. The reply came after 14 days: They belonged to a certain John Ronaldson who was born in Saint Louis in 1875, and who was sentenced to imprisonment on December 21, 1907, due to holdup murder. He died of pneumonia in prison in 1911. Ronaldson had a lesion on his right thumb, and this lesion was also visible on the fingerprints taken at the sitting.

Concluding Comment

Certainly, such video material would be a valuable documentation of Mirabelli's mediumship. Gerloff (1960:154) stated that he would try to locate it, but, since he never spoke of it again in later publications when mentioning Mirabelli, it seems he was not successful. However, without extensive, rigorous additional documentation, even such video material would be of comparably little persuasive power for those who doubt the reality of physical phenomena of this magnitude. At least Mikulasch and de Goes presented consecutive series of photographs of supposed apparitions along with reports of witnesses, but these seem to exert little impact even in parapsychological circles (e.g., Figure 2). Be that as it may: With regard to judging Mirabelli's mediumship, we are left with only two options: First, his mediumship constitutes the most spectacular fraud within the history of spiritualistic mediumship, because it must have involved dozens of confederates who impersonated false apparitions in full light, from little children to old men and women who originated from various cultural backgrounds, and it also must have required numerous deceitful investigators who came and went throughout the years, and hundreds of
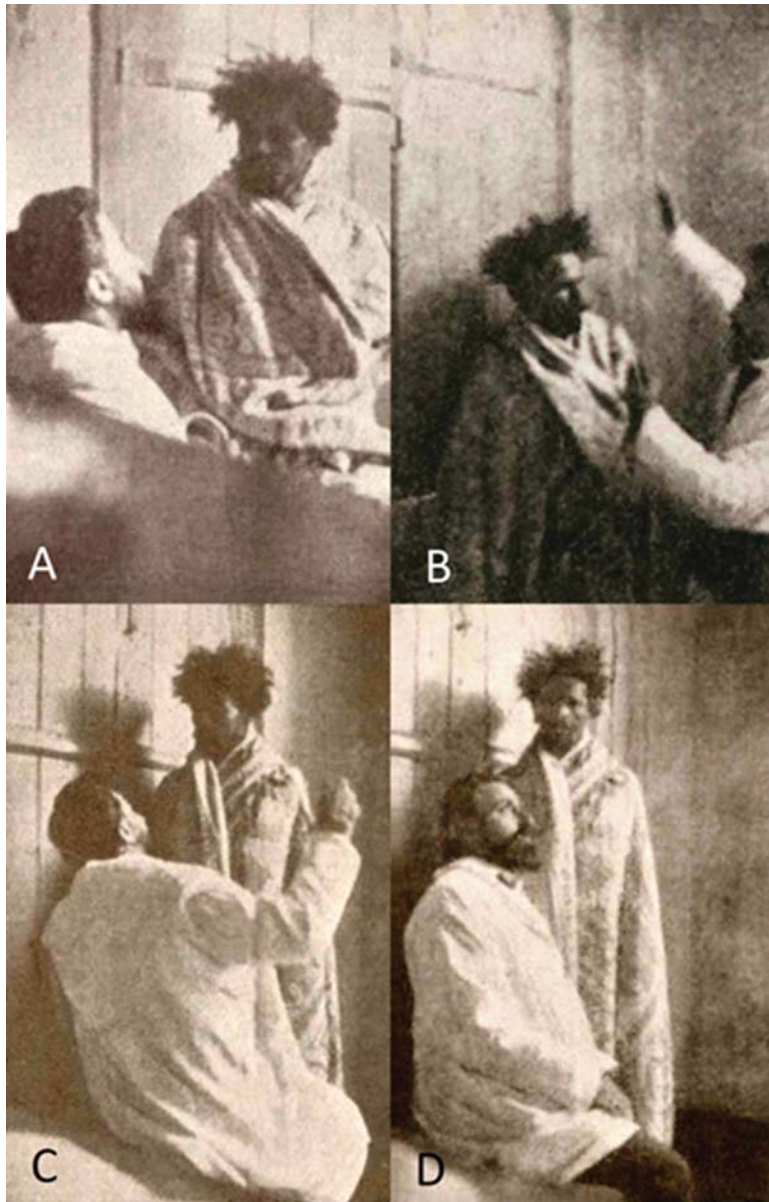
Figure 2. Four photographs of a "Moroccan" apparition. (A), (C), and (D) are taken from Mikulasch (1926) and (B) from de Goes (1937). (The photos were digitally revamped by M. Nahm.)

lying witnesses from all social strata—and all of these people must have acted in mutual agreement at different locations and cities, for decades. Or, second, Mirabelli was the most spectacular medium in the history of spiritualistic mediumship, regardless of whether he resorted to fraud on occasion or not. Given these drastic alternatives, it appears comprehensible if one prefers to sit on the fence, perhaps with a bias to this or to that side, and continue to wonder and ponder about this astonishing man and his abilities.

Notes

[1] Max Dessoir was an influential driver of the development of early psychology in Germany during the late 19th century, and is also known for coining the term "Parapsychologie" in 1889. For more information on Dessoir, see, e.g., Sommer (2013).

[2] de Goes' 1937 book is available at http://bvespirita.com/Livros2-P.html with pagination differing from the original book. The page numbers used in the present article refer to the online version of this book.

[3] The same might apply to Besterman's negative appraisal of Mirabelli's apport phenomena that occurred when he participated in several sittings with him (Besterman 1935; for a summary, see Braude, this issue). Whilst Besterman claimed that they were undoubtedly all faked, especially the coin apports, the original sitting protocols (which Besterman signed) mention, for example, that a coin was seen to levitate from the hand of a Mrs. Olga, and a Dr. Alvaro stated he saw a coin, which ended up in a rear pocket of a Mr. Fry, travel through the air (de Goes 1937:107f, Gerloff 1960:123).

[4] Perhaps Driesch overlooked that the 1927 issue of the *Journal of the Society for Psychical Research*, the Society he was the president of in 1926/1927, referred to Rodolpho Mikulasch as the publisher of this book on page 127, and indirectly also on page 144 (which even contains a reference to Driesch himself, albeit in another context). In any case, on the second title page of the book in question, Rodolpho Mikulasch, a General Secretary of the *Cesar Lombroso Academy of Psychical Studies* (Mikulasch 1926:31, compare Gerloff 1960), is explicitly given as its editor. Later, Mikulasch became the prefect of the city São Vicente (Gerloff 1960).

[5] That this "intimate friend" must have been Eurico de Goes is suggested by his profession. Driesch (1930:487) stated that he was "the overseer of the Town Library of São Paulo" and its "chief librarian" (Driesch 1951:259), which is in accordance with de Goes (1937) and Walker's (1934) descriptions of his profession.

[6] See also the translation by Gerloff (1960:52). In the translated summary

published by Schrenck-Notzing (1927), it is stated that 35 negative sittings were held *outside* the facilities of the Academy, but the original text by Mikulasch (1926) states they were held *within* these facilities.

[7] This treatise about Mirabelli by Hans Gerloff contains a translation of the entire book of Mikulasch (1926) as well as translated excerpts of other Portuguese sources about Mirabelli, among them the séance protocols of the sittings with Besterman, which are included in de Goes (1937). Gerloff was convinced that the phenomena of Mirabelli were genuine, and he advanced severe accusations against Western parapsychologists, most notably against Besterman. He was utterly disappointed that they, in his opinion, carelessly missed the unique chance to scrutinize Mirabelli's mediumship in a seriously scientific manner.

[8] Sometimes, Mirabelli's movements were synchronized with those of the apparitions. For example, whenever the apparition of the little daughter of Dr. Ganimedes de Souza moved (when floating in the air toward the end of her appearance in full light), the arms of the entranced Mirabelli jolted as if in nervous tremor (Mikulasch 1926:56, see also Gerloff 1960:73).

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Eusapia Palladino Anthologized

Eusapia Paladino: Materialisations and Intellergetic Phenomena in Physical Mediumship compiled by Scott Dickerson. Runabout, 2016. 286 pp. ISBN 978-1530915859.

In the last decade we have seen the publication of various books about physical mediums. The authors of these studies have done much to rescue from oblivion the careers of individuals such as Indridi Indridasson from Iceland (Haraldsson & Gisurarsson 2015), William Mumler from the United States (Kaplan 2008), and Franek Kluski from Poland (Weaver 2015). One medium who needs discussion, so as to be reintroduced to new generations, is the Italian Eusapia Palladino, the topic of the present book.

Unlike the above-mentioned studies and overviews of the careers of mediums, the present work, compiled by Scott Dickerson, is more limited in scope. It is an anthology of various English-language articles and sections of books about Palladino. *Eusapia Paladino: Materialisations and Intellergetic Phenomena in Physical Mediumship* is part of a series entitled "Lost Foundations of Parapsychology and Psychical Research." Unfortunately, the book has no information about this series, but its Amazon page states that the series "endeavors to provide the interested public with authoritative investigative accounts from the period of research prior to the rise of the statistical approach to parapsychology championed by J. B. Rhine, coming to prominence in the later 1930s."

Why Palladino?

The mediumship of Eusapia Palladino has continued to be relevant among those who take physical mediumship seriously. Unfortunately, Dickerson does not introduce the book (nor any of the articles reprinted) in any way, limiting himself to presenting the articles with no additional information. Such a course of action does not help those readers who are not familiar with the medium to understand her importance in psychical research, nor to realize from the beginning of the book the variety of phenomena shown by her (Alvarado 2016).

Palladino is important for various reasons. First, her case has long been considered evidential by many. For example, Charles Richet (1922:38–39)

stated in his celebrated *Traité de Métapsychique*, that even assuming that Palladino was the only medium in the world, her séances would suffice to scientifically establish both telekinesis and materializations. A more recent example is Stephen E. Braude, who has written:

> The crucial issue is not whether there are instances in which the medium cheated, but whether there are instances in which the evidence is strong that no cheating occurred. And in that respect, Eusapia's case is exceptionally good. (Braude 2007:47)

The medium's cheating, which was repeatedly reported (e.g., Carrington 1909:182, Courtier 1908:521–540), has created much skepticism among modern parapsychologists, some of whom tend to ignore the case (Irwin & Watt 2007), or present highly imaginative fraudulent explanations for particular séances (Wiseman 1992). Such skepticism is not unique about Palladino, since other past physical mediums also are neglected. Many people appear to have a general distrust about accepting old séance reports as documentation for the reality of physical phenomena.

Second, and as I have argued before (Alvarado 1993), Palladino's case was historically important for reasons other than evidential concerns. In addition to projecting influential negative images of mediumship with some of her fraudulent, flamboyant, and apparently hysterical behaviors, her performances contributed to the development of theoretical concepts, and of research standards involving controls and the use of instruments to record physical phenomena.[1]

Palladino's importance is evident in the coverage of her mediumship in overviews of the history of parapsychology, such as Gutierez and Maillard's *Les Adventuriers de L'Esprit* (2004:82–100, 117–138) and Inglis' *Natural and Supernatural* (1992:Chapters 35–38). There is much information about her in Biondi's history of spiritism and psychical research in Italy (1988:96–100, 121–129, 134–159). Furthermore, there are other examples of scholarship about her such as the work of Gauld (1968:Chapter 10), Giuditta (2010), and Sommer (2012). Palladino has been a particular interest of mine, as seen in various articles I have written about topics such as her influence on psychical research (Alvarado 1993), Julian Ochorowicz's ideas about her

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fraud and dissociation (Alvarado 2010), her biography (Alvarado 2011), and Lombroso's writings about her (Alvarado & Biondi, in press).

But to learn about this medium in detail, it is necessary to go to the

original séance reports. Many examples could be presented. In addition to very early mentions of the medium (Damiani 1872), there are various nineteenth-century reports. These include those about the famous sittings Cesare Lombroso had with the medium (Ciolfi 1891), as well as those conducted at Milan (Aksakof et al. 1893), and elsewhere (e.g., Lodge 1894). The subsequent literature is vast, including the reports of Bottazzi (1909/2011), Carrington (1954), Courtier (1908), Feilding, Baggally, and Carrington (1909), and Morselli (1908).
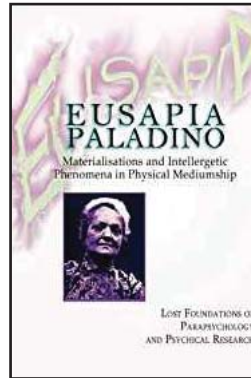
The Anthology

The anthology opens with Cesare de Vesme's article "The Spiritistic and Spiritualistic Explanation of Mediumistic Phenomena" (de Vesme 1907:1–39, these and other page ranges refer to the anthology reviewed here). I wonder why this article was chosen to open the volume since it barely mentions Palladino. It consists mainly of ideas about the study of psychic phenomena in general. Some examples are sections about "a priorism in scientific language," the use of explaining phenomena with one hypothesis, and cases that "appear to be spiritistic." While in principle such issues are related to many phenomena and specific cases in psychical research, including Palladino's, the medium is little discussed in the article.

This is followed by a much-neglected article by physician and student of mediumship Giuseppe (called Joseph in the report) Venzano, "A Contribution to the Study of Materialisations" (pp. 41–138), taken from the *Annals of Psychical Science* (Venzano 1907).[2] This is a good selection because it includes more dramatic materializations than those usually reported with this medium. The following is an example reported in a séance that took place on December 29, 1900, which included various individuals, among them Venzano and astronomer Francesco Porro:

> Suddenly Dr. Venzano, who held the medium with his left hand, she resting her head, visibly to all, on the shoulder of Professor Porro, saw forming to his left, at about a hand's breadth from his face, as it were a globular, vaporous, whitish mass, which condensed into a more decided form, that of an oval, which gradually assumed the aspect of a human head, of which the nose, the eyes, the moustache, and the pointed beard could be distinctly recognised. This form came and touched his face, and he felt a warm and living forehead press against his own and remain there for a second or two. Then he felt the contact of the whole profile of the face against his own,

with a pressure as of a caress, then the imprint of a kiss, after which the mass seemed to vanish into vapour near the curtains. The other sitters, meanwhile, were only aware of a vague luminosity in the direction of Dr. Venzano; but they distinctly perceived the sound of the kiss. (p. 91)

Other observations were even more dramatic, involving the appearance of apparently whole body figures. All of these observations were made in Italy, at the Circolo Scientifico Minerva in Genoa. Several reports of phenomena seen in this group appear in books published in Italian by Ernesto Bozzano (1903), Enrico Morselli (1908), and Luigi Arnaldo Vassallo (1902).

The third chapter, "Eusapia Paladino" (pp. 139–144), is taken from William F. Barrett's *On the Threshold of the Unseen* (1917). He mentions at the beginning the negative conclusions of the investigation of the medium at Cambridge University by members of the Society for Psychical Research (see Sidgwick 1895). Unfortunately, the account does not even begin to acknowledge the magnitude of the controversies created, as seen in Gauld's (1968:Chapter 10) discussion of the subject.

But Barrett discussed other issues as well. He stated:

> Like other psychics . . . , she is most sensitive to "suggestion," even when unexpressed; and in the trance, when her consciousness and self-control are largely inhibited, she is the easy prey of external influences. In the absence of the steadying, though subconscious, influence of a high moral nature, she unblushingly cheats whenever the conditions are unfavourable for the production of supernormal phenomena. We have no right to assume that she is wholly conscious of so doing . . . . If they are due, as some have thought, to an externalization of the nerve force of the psychic, it is not improbable that the degree of this externalization will vary with the favorable or unfavorable mental state of those present. We may even conceive that when this psychic force is restricted or not externalized, it may create movements of the limbs of the psychic which will cause her to perform by normal actions (in perhaps a semi-conscious state) what under good psychical conditions would be done supernormally. This would produce the impression of intentional fraud. (pp. 143–144)

However, these interesting ideas were not original with Barrett. Julian Ochorowicz (1896) discussed this years before, but the lack of contextual information in this anthology does not help the reader realize this.

Some of the work of Enrico Morselli is covered in "Experiments Made with Eusapia Paladino at Genoa by Professor Morselli" (pp. 145–169), taken from a chapter from *Psychical and Supernormal Phenomena* by Paul Joire (1909/circa 1916:Chapter 37), who mainly cites Morselli. From

the so-called Feilding report (Feilding, Baggally, & Carrington 1909), the compiler presents the conclusions of each of the authors (pp. 170–191): Hereward Carrington, W. W. Baggally, and Everard Feilding. Carrington wrote:

> As a result of the ten sittings held by us at Naples, November 21st–December 15th, 1908—being the ten séances attended by me—I beg to record my absolute conviction of the reality of at least some of the phenomena; and the conviction, amounting in my own mind to complete certainty, that the results witnessed by [us] were not due to fraud or trickery on the part of Eusapia. (p. 170)

Carrington's statement is interesting because it shows his conversion to the reality of the medium's manifestations. It is important to remember, and this is something that the compiler does not mention, that Carrington was not only skeptical of Palladino, but of most physical mediums. This is clear in his book *The Physical Phenomena of Spiritualism* (Carrington 1907). But the 1908 Naples sittings made him change his mind about the Italian medium. He stated in the book's second edition: "Since this book was first issued, I have seen Eusapia Palladino, and witnessed what I believe to be many genuine and remarkable physical manifestations" (Carrington 1920:vi).

There were important consequences. As I have documented before (Alvarado 1993:273–274), Carrington became a defender of Palladino, providing much publicity for the medium. Furthermore, Carrington brought the medium to the United States in 1909, something that generated many controversies (Carrington 1954).

The book being reviewed here also has excerpts presenting negative and positive views from Frank Podmore and Cesare Lombroso. The excerpt by Podmore (pp. 193–201) comes from his well-known book *Modern Spiritualism* (Podmore 1902:Vol. 2:198–203), a study showing much skepticism to physical phenomena. In addition to some general background, Podmore stated:

> Finally, if we decide to reject the evidence in favour of Eusapia's supernormal powers, that decision is in the last analysis justified . . . . The justification is that the results attained . . . are not sufficiently free from ambiguity to weigh against the presumption derived . . . from an examination of all previous evidence upon the subject. (p. 200)

Cesare Lombroso's discussion (pp. 203–239), including personal experiences with the medium, was taken from his book *After Death—*

What? (1909). There is no discussion in the anthology of the importance of Lombroso's acceptance of her phenomena, which included publicizing her with his fame in psychiatry and criminology, and bringing her to the attention of others who then conducted many séances with her. For example, the first major investigation of her mediumship, conducted in Milan, stated:

> Taking into consideration the testimony of Professor Cesare Lombroso about the topic of mediumistic phenomena that occur through Mme. Eusapia Paladino, the undersigned gathered here in Milan to do with her a series of studies in order to verify these phenomena by submitting them to experiments and observations as rigorous as possible. (Aksakof et al. 1893:39)

Cesare Lombroso

But Lombroso was also important in another way. His writings about Palladino spread a "mixed" perspective of mediumship, also held by a few others (e.g., Morselli 1908). This view consisted of the idea that Palladino suffered from hysteria but also showed genuine physical phenomena such as movement of objects and materializations (Lombroso 1892, 1909), a topic I discuss with a colleague elsewhere (Alvarado & Biondi in press).

In the excerpt presented in the anthology, Lombroso describes many examples of phenomena. Here is one of them:

> At Naples, in 1895 . . . , I again tried these experiments in a room in our inn chosen expressly for the purpose. And here, in full light, we saw a great curtain which separated our room from an alcove adjoining (and which was more than three feet distant from the medium) suddenly move out toward me, envelop me, and wrap me close. Nor was I able to free myself from it except with great difficulty. A dish of flour had been put in the little alcove room, at a distance of more than four and a half feet from the medium, who, in her trance, had thought, or at any rate spoken, of sprinkling some of the flour in our faces. When light was made, it was found that the dish was bottom side up with the flour under it. This was dry, to be sure, but coagulated like gelatine . . . . When the lights had been turned on, and we were all ready to go, a great wardrobe that stood in the alcove room, about six and a half feet away from us, was seen advancing slowly toward us. It seemed like a huge pachyderm that was proceeding in leisurely fashion to attack us, and looked as if pushed forward by someone. (pp. 217–218)

Perhaps the most dramatic one was the séance in which Lombroso's deceased mother was said to appear. He wrote, after being told she would come:

> I saw detach itself from the curtain a rather short figure like that of my mother, veiled, which made the complete circuit of the table until it came to me, and whispered to me words heard by many, but not by me, who am somewhat hard of hearing . . . I was almost beside myself with emotion and begged her to repeat her words. She did so, saying, "*Cesar, fio mio!*" (I admit at once that this was not her habitual expression, which was, when she met me, *"mio fiol"*; but the mistakes in expression made by the apparitions of the deceased are well-known, and how they borrow from the language of the psychic and of the experimenters), and, removing the veil from her face for a moment, she gave me a kiss.
>
> After that day the shade of my mother (alas! only too truly a shadow) reappeared at least twenty times during Eusapia's seances while the medium was in trance; but her form was enveloped in the curtain of the psychic's cabinet, her head barely appearing while she would say, "My son, my treasure," kissing my head and my lips with her lips, which seemed to me dry and ligneous like her tongue. (pp. 224–225).

Furthermore, the book also includes a summary (pp. 241–253) of a biographical article written by Paola Lombroso (1907b) originally published in Italian (P. Lombroso 1907a). This is a good selection for the anthology because it includes much personal and anecdotal information about the medium's personality.

Finally, the anthology includes some articles published about Palladino's performances during her visit to New York, published in issues of the newspapers *The New York World*, *The New York Times*, and *The New York Herald* (pp. 255–284). All this illustrates the complexity of this case, particularly in the New York séances. In fact, I would argue that this is an episode of the medium's career that deserves further study, particularly using the New York newspapers. I once copied from microfilm (before the advent of modern PDF databases) everything I could find about Palladino's New York séances from 1909–1910 issues of *The New York Times* and can attest to the complexity of the discussions. But one must remember, as seen in the current anthology, that there were several other newspapers at the time in New York covering the séances as well.

Evaluation

I have argued throughout this Essay Review that the anthology is problematic in many ways. To start, the book clearly needs more contextual information to introduce the reader to Palladino, since not all potential readers can be expected to know much about her. This information could have been provided with an initial essay presenting an overview of the medium's career, including biography, phenomena typical of the séances,

controversies (evidence for fraud), and other aspects of this important mediumship case.

It would have also helped to have short introductions to each chapter with information about its authors. After all, not everyone today knows who Barrett, Carrington, de Vesme, Feilding, Lombroso, Podmore, and Venzano were, to name a few. Similarly, the book would have been improved if a bibliography of published primary and secondary literature about Palladino had been included.

It would also have been helpful to readers if essays appearing latter in the volume, such as those of Barrett and Podmore, would have been placed at the beginning, since they present more general perspectives. The same may be said about the summary of the Paola Lombroso essay, consisting of biographical notes about the medium.

While the anthology could have been better-crafted, particularly for those with little background on the topic, it still has much to offer. Several of its chapters present useful information about Palladino that will inform readers about her phenomena and other aspects of her career. This includes the work of Lombroso, Venzano, and Morselli, among others. Similarly, readers will learn about the opinions of Carrington, Baggally, and Feilding, aspects of the New York séances, and about doubts and controversies such as those discussed by Podmore. All in all, within the above-mentioned limitations, this anthology can assist readers to obtain information about the legendary Eusapia Palladino.

Notes

[1] The main theoretical concept I am referring to is the idea of biophysical forces coming out of the body of mediums to produce physical phenomena (Alvarado 2006). Both Morselli (1908) and Carrington (1909) are examples of theoreticians of this sort of material. There is much about instruments and controls in Courtier (1908) and Feilding, Baggally, and Carrington (1909).

[2] The article (Venzano 1907) is not only in the August issue of the journal, as indicated in the anthology (p. 41), but also in the September issue. It is a two-part article.

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The Compelling, Confusing Evidence for an Afterlife

Surviving Death: A Journalist Investigates Evidence for an Afterlife by Leslie Kean. Crown Archetype/Random House, 2017. 416 pp. $17.70 (hardcover). ISBN 978-0553419610.

Leslie Kean's *Surviving Death* is a wonderfully readable, carefully constructed summary of the evidence for the existence of what is colloquially called an "afterlife." That is, she considers evidence for the hypothesis that individual human minds and personalities possess an existence going beyond their attachment to any particular body—so that, for instance, an individual with a certain name and certain traits may sometimes continue to perceive and act, even when the body typically associated with that individual is dead and gone.

Most of the book comprises moderately detailed descriptions of specific cases, involving specific people, which indicate the existence of some sort of "afterlife" for individual human minds, or potentially give some information regarding the nature of this afterlife. Kean considers a gamut of phenomena such as past-life memories, near-death experiences, mediumistic trances, poltergeists, and so forth. However, she also makes a significant effort to draw general conclusions, lessons, and hypotheses from the totality of these cases, while maintaining respect for the confusing and in many ways still mysterious nature of the phenomena under discussion.

Each of the topics considered in the book has been reviewed and analyzed in more depth elsewhere. What Kean does, however, is provide a clear, evocative, and rational survey of the many types of evidence that are directly relevant to the possibility and nature of an afterlife for individual human minds.

As the topics Kean covers in her book are so contentious, it's probably best for the reader's understanding if I lay my cards on the table in terms of my own views of the subject matter: I generally agree with Kean that some sort of "afterlife" probably exists. So from my view, as a reader, what interested me in her book was mainly the rundown of particular situations and phenomena, giving insight into what the heck this "afterlife" might be like and how it relates to what happens in this world.

I should also note that my belief in the probable existence of an

afterlife for individual humans is fairly recent. I was raised atheist and have never been religious. For a substantial part of my life, I considered belief in "life after death" as the most obvious of absurdities. It seemed completely obvious to me that foolish people maintained the idea that they would continue living after their bodies died, simply because they were afraid to confront the blunt fact of their impending nonexistence—or else because they were brainwashed by various religious belief systems. I have never been extraordinarily afraid of death, but I've always considered physical death something better avoided, and I've put a fair bit of effort into working toward the radical extension of the human healthspan (e.g., via applying Artificial Intelligence technology to understand the biology of aging and discover ways to extend human healthspan via gene therapy and other methods, perhaps even via uploading human minds into classical or quantum computers).

What shifted my probability estimate regarding the existence of an afterlife was, basically, reading a lot of the evidence—and then, after doing a lot of reading, talking to some of the people who had gathered some of the evidence I had read about; and talking to a few people whose direct experiences constituted some of this evidence. So, basically, what shifted my perspective was an encounter with the same body of evidence that Kean summarizes in her book—though I read many books and papers, not just one, and thus encountered the evidence in a much more voluminous and less well-organized fashion. Going through all this evidence carefully did not convince me of the detailed veracity of any of the traditional religious depictions of the afterlife. But it did convince me that *something* perplexing and afterlife-like is very likely going on.

One thing I have found, since shifting my view on the issue of afterlife, is that most people who hold a skeptical view on the topic have NOT really looked at the evidence very carefully. This is parallel to the situation with psi phenomena such as ESP, remote viewing, psychokinesis, and so forth. Most people who are skeptical that these phenomena exist, have not actually reviewed the data regarding the phenomena in any detail. These are complex and confusing matters, and there is certainly room for rational disagreement and argument among people who HAVE studied the data. But the arguments one has with people who have looked at the data carefully, are very different from the arguments one has with naïve "skeptics" (many of whom might be more accurately termed "negative believers," in the sense that some people's belief in the NON-existence of psi or afterlife is extremely fixed and strong, belying the open-mindedness traditionally associated with the term "skepticism").

Given the importance of the issue of afterlife to humanity generally,

and the often-confusing nature of discussions surrounding the topic (with religious believers and "skeptical" negative believers often expressing very strong positions with great emotion), I would recommend Leslie Kean's book to nearly everyone.

For readers who intuitively feel some sort of afterlife does exist, the book will provide a wealth of interesting particulars, enabling them to flesh out their understanding of what the afterlife may be like and how it may intersect and interact with this world.

For readers who are skeptical (in the genuine sense) of the idea of an afterlife, the wealth of perplexing real-world cases considered will, at least, intrigue and give pause.

For the reader who is unsure what to think about the existence of an afterlife, Kean's book seems likely to provide a nudge in the direction of "Hmm, either some sort of afterlife exists, at least in some cases, or else something else quite strange is going on."

In the rest of this Review, I'll run through a series of the specific afterlife-related phenomena that Kean discusses, highlighting some of the key points she makes. Of course this sort of summary lacks the emotionally and empirically compelling details one obtains from reading about each case in detail; but to get those, you'll have to read Kean's book, and/or dig deeper into the primary literature.

Having surveyed many of the various relevant phenomena, I will then briefly turn to the question of explanation. Kean contrasts two classes of explanations of the phenomena she surveys: "survival" and "super-psi." Similar to Stephen Braude in his beautifully rigorous and much more academic treatment of the same issues, *Immortal Remains* (Braude 2003), Kean comes down mostly on the side of survival. I tend to agree—but I think one has to be quite careful in thinking about what "survival" really means.

But before going any further in that direction, let's romp quickly through some of the fascinating and bizarre phenomena Kean reviews in *Surviving Death* . . . and some of the properties and lessons she abstracts from the various case studies she considers . . .

Reincarnation

No high-level summary is going to do justice to actually reading the stories Kean presents. For instance, she describes a young boy telling his mother, after identifying in a photo the man he felt he was reincarnated from, "Mom, you still don't get it, do you? I am not the same as the man in the picture on the outside, but on the inside I am still that man. You just can't see on the inside what I see" (p. 61). It is very compelling to read this sort of quote after reading a detailed, evocative rundown of the boy's parents' long

efforts to understand the strange memories, dreams, and statements their son kept making—and their difficulties coming to grips with the idea of reincarnation, which was against their religious worldview but which was an inevitable consequence of their experiences with their son.

In this case, the boy's parents tracked down the daughter of the man their son claimed to be reincarnated from, and asked her to verify various obscure statements he had made about his past life. The statements checked out.

Kean is an excellent writer, and so as she tells story after story of this nature, from America as well as from India where such events are more widely accepted, it is bound to have a real emotional impact on the reader. One empathizes with these children who are confused to be plagued with someone else's memories, and with parents who don't know why their kids are going through such things—but who are relieved to find there seems to be some deeper explanation, rather than their kids just being nuts.

From a scientific view, however, it is more important to look for the abstract patterns beyond the individual stories. This is not so straightforward given the diversity of the different cases involved; but, being abstraction-oriented, as I read I made a list of some of the more general properties of the reincarnation cases that Kean cites:

> • These children typically start talking about a past life very early . . . with the average age being thirty-five months. This happens not through hypnosis, but spontaneously, as the children begin recounting events they say they experienced in another life. Though they may talk about a past life many times and with great intensity, they tend to stop making such statements around the age of six, the same time when children typically lose memories of early childhood (p. 46).

> • Most of the children describe only one past life. Their memories usually focus on people and events from near the end of that life, and three-quarters of them relate how they died. They very rarely report being anyone famous. Instead, they recall a largely nondescript life of a person who typically lived fairly close by, almost always in the same country. The one part of the life that is often out of the ordinary is how the previous person died. Around 70 percent of the children describe a life that ended in an unnatural death, such as murder, suicide, accident, or combat. Though there are exceptions, the life also tends to be quite recent. The average interval between lives is four and a half years, while the median interval—meaning half are shorter and half are longer—is only seventeen months (p. 47).

> • Along with talking about a past life, many of the children show behaviors that seem connected to their statements. A lot of them display great emotion when they discuss events from that life. They do not dispassionately list a number of facts, but instead they cry that they miss people or beg to be taken to them (p. 47).

• Others show phobias related to how the previous person died. In cases involving an unnatural death, 35 percent of the children have an intense fear about the mode of that death (p. 48).

   • Along with statements and behaviors, many of the cases include physically tangible signs of a connection to a past life. Some of the children have birthmarks that match wounds, usually the fatal wounds, on the body of the previous person. They are often unusual in some way, in shape or size or by being puckered or raised rather than flat (p. 49).

These patterns emerge among literally thousands of cases studied by Kean and the other researchers she references (e.g., the curious reader may want to check out the books of Jim Tucker and Ian Stevenson; e.g., Tucker 2013). These are some of the facts that any general theory of such phenomena must account for.

Intermission Memories

Adding to the intrigue and perplexity, some children have clear memories of the "intermission" between the end of their previous life and the beginning of their current one. Jim Tucker, who studied these intermission memories, categorized them as "referring to three main phases: a 'transitional stage' just after death, a 'stable stage' for most of the time between the lives, and a 'return stage' involving events close to the time of birth" (p. 128).

As Kean notes,

> Interestingly, the intermission memories tend to arise in the stronger re-incarnation cases, where more statements were made about the past life that were verified and more specific names were remembered, than in the weaker cases. In other words, if a child has a keener memory of his previous life, he is more likely to remember the intermission stage. Also, when inter-mission memories are reported, the child's memory of the mode of death from the previous life is more likely to be verified. This supports the possible accuracy of the unusual between-lives memories, since these children have so many other verified memories. "Only an unusually strong memory, and not any other characteristic of the subject or previous personality" distin-guishes cases with intermission memories from those without them, report Tucker and Poonam Sharma, a medical student at the University of Virginia School of Medicine, in a 2004 paper. "Their reports of events from the inter-mission period seem to be part of a pattern of a stronger memory for items preceding their current lives." (p. 128)

The study of intermission memories becomes a complex pursuit unto itself. It is more difficult to study these scientifically, as compared with past life memories, because there is no consensus reality, accepted among human scientists, to compare intermission memories to. However, it is striking that

so many children report similar intermission memories, even in cases where the children's parents did not believe in reincarnation and the children had no apparent prior education regarding theories of reincarnation, intermission, etc. This could of course be explained via commonalities among human brain structure leading to common patterns of delusion among various children. But in the context of so many validated past-life memories and other associated paranormal phenomena, it seems unwise to gravitate too reflexively toward a wholly neural-reductionist explanation of intermission memories.

Near Death Experiences

A different sort of evidence regarding the possibility and possible nature of survival after death is provided by NDEs or "near death experiences"—in which a person, during an event where they almost die, has experiences that feel to them like encountering another world, or venturing slightly into an afterlife and then returning, etc.

There are broad commonalities among many NDEs, such as seeing white lights and a feeling of comfort and bliss and "coming home" (Bellig 2015). There are also aspects to NDEs that seem more culturally dependent; e.g., Kean notes

> Some talk about heaven and seeing God, but it's not clear to what extent these concepts were learned as children. Some report meeting deceased relatives or other discarnate personalities . . . (p. 129)

It seems clear that whatever is happening during an NDE, it's not just typical imagination; it's processed by the brain as a strange kind of perceived experience:

> A lengthy 2014 paper in *Frontiers in Human Neuroscience* by nine scientists from the University of Padova, Italy, reports on the use of electroencepha-lography (EEG) "to investigate the characteristics of NDE memories and their neural markers compared to memories of both real and imagined events." This team reached the same conclusion as the Belgian one. "It is notable that the EEG pattern of correlations for NDE memory recall differed from the pattern for memories of imagined events," they state. "Our findings suggest that at a phenomenological level, NDE memories cannot be considered equivalent to imagined memories, and at a neural level NDE memories are stored as episodic memories of events experienced in a peculiar state of consciousness." (p. 99)

Of course, the fact that the brain perceives NDEs as a kind of experience

doesn't really tell us anything about the origin
of NDE experience. The brain can play all
sorts of tricks. The "white light" commonly
seen during NDEs is associated with electrical
activity in the brain around the time of death
(Chawla et al. 2009); on the other hand,
this observation does not necessarily imply
a reductive explanation of the white light
phenomenon nor of NDEs in general (Mays
& Mays 2011).

And the various psi phenomena often
encountered in the course of NDEs are
harder to "explain away" via mainstream
neuroscience. Kean recounts the case of
Pam Reynolds, who had a vivid NDE during
surgery when she had no measurable brain activity.

As Pam reported:

> I then saw my uncle, who had passed away at the ripe old age of thirty-nine.
> He didn't use his mouth to communicate with me. He did it in another way
> that I remembered from my early childhood. He had the look. He would
> look at me and I would understand. And, it didn't take long until I under-
> stood that everyone communicated in this fashion. They had the look.
> They'd look at you and you understood. I also describe it as the knowing,
> because you just know. And all of these people had this ability to just kind
> of look and know. (p. 104)

> The sound, however, is an entirely different matter and that really interests
> me. As a musician, I've been taught from the cradle that if you put two tones
> that are too close together, what you get is discordance. But, in the place
> where I was, every being had their own tone and every tone was close to
> the next and yet, when these tones were put together, when everyone was
> sounding off, it was beautiful. It was harmonic. It was beyond anything that
> I could ever compose or direct here, or hope to. (p. 105)

> Having had this NDE, I no longer fear death. I fear separation. I thought at
> first that I wouldn't even fear separation, but there is no experience that
> makes the separation okay when you lose someone. But when my time
> comes, I will embrace death. In fact, I know people who are dying right now
> and I envy them their journey. It's a wonderful, wonderful place to go. But, I
> just don't like being left behind. I don't think any of us do. (p. 107)

This is an especially fascinating case because it combines paranormal
perception with clear visions and experiences of an afterlife. The paranormal
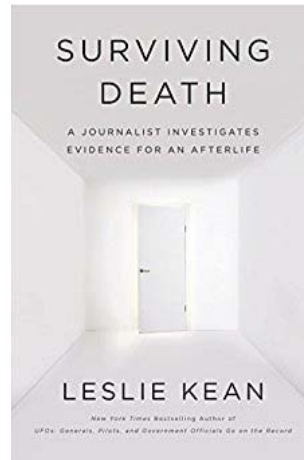
perceptions involved—of the very specific sights and sounds of the operating theater, perceived in spite of Pam being deeply anesthetized with no brain activity, with eyes closed and ears full of noisy machinery—could of course be the result of garden variety psi, performed by her neurons while in some sort of active state not measured by our current tools for detecting brain activity. But the coupling of these perceptions with her perceptions of deceased relatives and of music transcending the principles of Earthly sound certainly makes one wonder—especially when one combines this with other evidence about the possible nature of an afterlife, obtained for example from reports of reincarnation and from mediumship.

Kean's qualitative conclusion is fairly confident:

> Something actually happens during an NDE that we have yet to understand. Experiencers have no doubt that they crossed over into a wondrous afterlife realm to which they will someday return, and that death is merely a doorway into another world. (p. 99)

An added twist to the story is provided by the physical manifestations often observed by others at the time of a person's death; Peter Fenwick in his chapter cites numerous cases such as a hospice chaplain reporting

> Sometimes I've seen a light, which is in a corner, like candlelight, it's a golden light. It's not electric light and it's not one of the hospice lights. It just appears sometimes. It goes when they die. They take their last breath and everything settles down and the light goes out. (p. 141)

One case like this can be easily written off as a hallucination or delusion. A large number of cases, which is the reality, becomes a definite perplexity. Coupled with so many other psi-ish and afterlife-ish phenomena associated with NDEs, on the other hand, such occurrences start to seem almost unsurprising.

Mediumship

In the 1800s and before, it was relatively commonplace for people to go to a "medium" who would serve as a go-between to help them communicate with their dead relatives or friends. In modern culture, this sort of practice is frequently referred to as a joke, as if its fraudulent nature is obvious. And indeed, there were surely many frauds. But when one carefully reviews the many carefully recorded case studies of mediums apparently communicating with the deceased, this position becomes harder to maintain. Robert McLuhan makes this case thoroughly and eloquently in his book *Randi's Prize* (2010).

Often the medium, when "channeling" the voice and expressions and gestures of a dead person, displays an uncanny mimicry of the vocal and body-language mannerisms of the deceased. Recounting of information that was known only to the deceased and a handful of others—e.g., the location of long-hidden objects—is also common.

This sort of information transmission could plausibly be due to psi between the medium and the living relatives of the deceased. However, there are various different examples of mediumistic channeling that stretch this sort of explanation, e.g.,

> In some cases, correct information was apparently given about the contents of books in classical Greek; yet neither Mrs. Leonard, nor the sitter, nor the alleged communicator knew classical Greek, while the person who lent the books (Mrs. Salter), though she knew Greek, had not properly studied several of the volumes. Neither telepathy with the living, nor communication with the dead, nor yet clairvoyance, would seem to supply us with an adequate explanation here. (p. 210)

Mediums nearly always interpret their experiences in terms of survival.

> Many mediums, including Laura, say that "our loved ones on the other side" can hear our thoughts; that they are with us and can receive mental messages from us. (p. 162)

What happens in the medium's mind during a reading seems to be quite different from what happens in a psychic's mind when exercising psi perception. As one medium put it,

> With psychic information, I have to "squint" from the inside out like to focus on something in the distance. When I do mediumship, it's not squinting at all. It's just receiving. (p. 178)

And as psychologist Jeff Tarrant noted about medium Laura,

> One area of the brain is becoming active while she is receiving mediumship information; the other area is becoming activated when she is involved in a psychic reading. Coincidentally—or not—Laura reports that she sees psychic information in her left visual field, and she sees mediumship information on her right visual field. Actually, that is exactly what we see on these brain images. So this appears to be confirmation of what Laura reports from her own experience. (p. 187)

Mediums and Tip-of-the-Tongue Phenomena

One quirk of mediumistic séances that frequently strikes me when I read about them is the prevalence of what psychologists call "tip-of-the-tongue" phenomena. For example, from Kean's account of her own visit with a medium:

> She said that Budd mentioned my sister, saying that she lived in New York and had an "L" name . . . Without telling Laura, I was also thinking about my brother, and eventually I asked her if there was anyone else there. After finally getting Budd to pull back, she said a male on my father's side was there, and then recognized him as a brother. She said he had a "J" or "G" name (the only name my brother went by was Garry), and that his death was unexpected, which it certainly was. (p. 158)

This sort of memory pattern—where the beginning letter of a name comes right to memory, but the name as a whole can only be recalled with great effort—occurs in many cases besides mediumship, obviously. Psychologists call it the "tip-of-the-tongue" phenomenon:

> People experiencing the tip-of-the-tongue phenomenon can often recall one or more features of the target word, such as the first letter, its syllabic stress, and words similar in sound and/or meaning. Individuals report a feeling of being seized by the state, feeling something like mild anguish while searching for the word, and a sense of relief when the word is found. While many aspects of the tip-of-the-tongue state remain unclear, there are two major competing explanations for its occurrence, the direct-access view and the inferential view. The direct-access view posits that the state occurs when memory strength is not enough to recall an item, but is strong enough to trigger the state. The inferential view claims that TOTs aren't completely based on inaccessible, yet activated targets; rather they arise when the rememberer tries to piece together different clues about the word. Emotional-induced retrieval often causes more TOT experiences than an emotionally neutral retrieval, such as asking where a famous icon was assassinated rather than simply asking the capital city of a state. Emotional TOT experiences also have a longer retrieval time than non-emotional TOT experiences. The cause of this is unknown but possibilities include using a different retrieval strategy when having an emotional TOT experience rather than a non-emotional TOT experience, fluency at the time of retrieval, and strength of memory.
>
> . . .
>
> The transmission deficit model is based on a multi-component theory of memory representation that suggests that semantic and phonological information is stored in memory and retrieved separately. The transmission deficit model posits that TOTs occur when there is activa-

tion of the semantic component of the target word memory but this ac-
tivation does not pass on to the phonological level of the memory of the
target word. Thus, TOTs are caused by the deficit in transmission of activa-
tion from the semantic memory store to the phonological memory store.
(https://en.wikipedia.org/wiki/Tip_of_the_tongue)

What does the general psychology of tip-of-the-tongue phenomena tell
us about mediumistic channeling? Memory is in large part constructive;
and in the case of a mediumistic séance, it's not quite clear: Who is doing
the construction? Perhaps it's a combined effort between the mind of the
medium and the mind of the person being channeled? One gets the sense that
what mediums are doing is somehow a cross between receiving messages
and constructing systems. Their minds unconsciously must construct a sort
of image of the mind they are channeling, based on the fragmentary and
erratic signals they receive; and using this image, perhaps, they collaborate
in the construction of the memories of this other, now discarnate, person.
The type of persistent wholeness and identity possessed by the discarnate
entity, as distinct from the medium's mind, is far from clear.

One thing that seems very clear from the mediumistic and other evidence
is that the "leakage" of individual minds from the "afterlife" (whatever it
may be) into our present world, is very weak and slippery. When living
individuals receive signals from minds in the afterlife, these signals are
noisy and confused and it requires lots of interpolation and elaboration
and struggle to make sense of them. As I noted above, the pattern of
information transmission from "apparent disembodied minds" to mediums
is a bit similar to the pattern of recollection within a human mind, in cases
where emotional charge is high and semantic memory access is easier than
phonological memory access. The connections made between a living mind
and an afterlife mind seem to involve a combination of relatively abstract
emotional and semantic patterns, with occasional concrete mental contents
that generally either have high symbolic value or are manifested with great
effort. And these connections seem most vivid and insistent when there
is some major emotional content, key to the self-structure of the afterlife
mind, involved.

Poltergeists and Materializations

Kean saves for last the afterlife-related phenomena that put some of the
most obvious strain on the materialist point of view: ghosts, poltergeists,
and various more solid paranormal materializations.

We have all heard various ghost stories—haunted houses are a staple
of U.S. Halloween celebrations and amusement parks. As Kean recounts,

there are numerous cases where these phenomena have been observed by multiple people, including skeptical ones, and have carried out behaviors displaying the clear imprint of the mind of some deceased person. There is a fair resemblance between these ghosts and the minds of the deceased summoned during séances—except that for the apparitions, no mediums are required, the spirits just keep coming back again and again, generally to the place where their bodies died or some other place that had great significance to them. Similar forms of PK tend to occur here as in séance situations; and similar to reports delivered in séances and in cases of well-remembered reincarnation, a post-death mind that is highly active in this world seems often a consequence of a death that is in some way violent or especially troublesome.

Poltergeist investigators Alan Gauld and A.D. Cornell are quoted, characterizing the phenomena as "outbreaks of spontaneous paranormal physical phenomena centering upon the organism of some particular individual," and noting that there is often some intelligence involved, which "seems to organize and direct the various happenings."

> In some cases communication, such as responsiveness to questions by rapping in code, occurs; the phenomenon seems to exhibit a purpose; and sometimes it focuses on one particular object. (p. 270)

In isolation, these various poltergeist cases might seem like a diverse bunch of unexplained anomalies, mixed up with hallucinations and mental problems. In the context of reincarnation-related and near-death experiences and mediumship and so forth, one is led instead to wonder what might be the underlying dynamics that, after a body's death, cause some sort of transition to another incarnation to happen for some minds, temporary poltergeistly embodiment to happen for some others, etc.

But ghosts are not the strangest phenomena Kean recounts. What about the appearance of actual, solid human hands? According to Stephen Braude, who researched the 19th-century medium Daniel Douglas Home extensively, among the fifteen different "mind-boggling" types of phenomena that Home repeatedly generated were:

> Hands, supple, solid, mobile, and warm, of different sizes, shapes, and colors. Although the hands were animated and solid to the touch, they would often end at or near the wrist and eventually dissolve or melt. Sometimes the hands were said to be disfigured exactly as the hands of a deceased ostensible communicator (unknown to Home) had been. (p. 292)

And some mediums reported the materialization of entire bodies, not

just hands. Richet, who studied these phenomena in the early 20th century, was perplexed yet compelled by these shocking, bizarre occurences, and analyzed them in a measured and thoughtful way. After witnessing many partial and full materializations, he commented that it is just as difficult to understand the materialization of a living, mobile hand, or even a finger, as it is to understand "the materialization of an entire personality, which comes and goes, speaks, and moves the veil that covers him." In 1934, he wrote:

> I shall not waste time in stating the absurdities, almost the impossibilities, from a psycho-physiological point of view, of this phenomenon. A living being, or living matter, formed under our eyes, which has its proper warmth, apparently a circulation of blood, and a physiological respiration, which has also a kind of psychic personality having a will distinct from the will of the medium, in a word, a new human being! This is surely the climax of marvels. Nevertheless, it is a fact. (p. 307)

I am reminded of a passage in the recent book *Sivananda Buried Yoga* (Manmoyanand 2008) in which a yoga master living in a remote Indian cave materializes a bottle of Jack Daniels and some fries, simply to blow the mind of a skeptical visitor. Reading this in a book, it's easy to assume one is reading a fabrication. But after reading so many other strange stories from 100 to 200 years ago, it's hard to be so certain.

Struggling Toward Explanations

So how might we explain all this?

It is possible, of course, that all the evidence Kean surveys in *Surviving Death* is a bunch of hokum. All the people reporting these various odd experiences—and all the scientists studying them—could be either delusional or fraudulent, or some combination thereof.

From reading Kean's book alone, one can't really dismiss this sort of hypothesis—after all, the skeptical reader could easily wonder whether Kean just made all this stuff up, or naïvely believed a bunch of kooks and frauds who were lying to her. But if one digs deeper and reads more and more of the primary reports on these phenomena, and talks to more of the researchers behind these reports, the "delusion and/or fraud" explanation comes to seem more and more of a stretch. In the end we can't totally rule out the hypothesis that the Apollo moon landing was a fraud, or that a cabal of Jewish bankers or reptilian aliens are controlling all the events on Earth (so that the whole of the world economy and society is a kind of fraud and delusion) either. At some point one has to adopt, as a working hypothesis, the direction in which the abundance of evidence appears to be pointing.

So, supposing the phenomena Kean surveys are mostly real, then—what gives?

What I above referred to as the "super-psi" hypothesis, Kean refers to mainly as the LAP or Living-Agent Psi hypothesis. In this theory, once a human body dies, the associated mind is dead and gone, too. But living humans, via leveraging various paranormal powers, are sometimes able to dig up information about dead people in surprising ways. Perhaps by reaching back in time with trans-temporal telepathy, to extract information from a now-dead person's mind back when they were living; or perhaps by reaching into the minds of other living people who knew the now-dead person. In this theory, living human minds are also sometimes able to create weird phenomena—say, disturbances that look like poltergeists, or dissociated personalities inside their own minds that speak with the voices of dead people.

It becomes clear, after a bit of thought, that essentially any survival-ish phenomenon can be given SOME explanation in terms of living-agent super-psi, if one is willing to get ambitious enough about the level of psychic ability attributed to the living humans involved. However, there is a plausibility issue here. There is a tremendous amount of evidence about the nature of psi phenomena—e.g., Damien Broderick and I summarized some of it in our book *The Evidence for Psi* (Broderick & Goertzel 2014) and Ed May and Sonali Marwaha provided a masterful overview in their book *Extrasensory Perception* (May & Marwaha 2015). This evidence suggests that phenomena such as ESP, precognition, and psychokinesis do exist—but they are weak in most situations; and even when they are strong, there is no evidence that they are nearly as strong as would be needed to "explain away" all the observed survival-ish phenomena as consequences of super-psi.

In sum, the counterargument against the super-psi explanation of survival-ish phenomena is twofold. First, it becomes somewhat like the Ptolemaic epicycle theory of planetary orbits—i.e. it violates the Occam's Razor heuristic that militates toward simpler explanations. Any phenomenon can be explained somehow or other via sufficiently powerful psi, but the nature of the psi ability posited often needs to be customized quite exquisitely to fit the phenomenon one wants to explain. This reeks of statistical overfitting and feels uncompelling. And secondly, the degree of power and reliability of psi required to make these super-psi explanations work, seems out-of-sync with the known data on psi phenomena.

Lacking any solid theory of psi phenomena themselves, it is hard to rule out the super-psi hypothesis in a really definitive way. But I agree with Kean and Braude that it seems fairly implausible.

But is the theory of "survival" really a coherent alternative, given the evidence?

Some form of survival hypothesis does seem to have Occam's Razor on its side. As Kean notes,

> The similarities between descriptions of NDEs, intermission memories, and end-of-life experiences reinforce the possible reality of another realm or nonphysical dimension where consciousness dwells after death. I believe that these interconnections give weight to the survival hypothesis. (p. 133)

In other words, if one takes what people report from NDEs, intermission memories, and end-of-life experiences fairly literally, one concludes they are reporting that some sort of afterlife exists, in which individual minds exist and persist. This hypothesis also provides a direct sort of explanation for mediumistic channeling and many examples of poltergeists. It's of course possible that what people report in NDEs, end-of-life experiences, and so forth is largely constructed by their own minds, and that even if it's being triggered by some domain of being beyond our physical world, these experiences represent a highly distorted interpretation of this other domain. However, the fact that so many of these experiences appear to point directly at some form of survival of the individual mind is noteworthy and deserves to be taken seriously.

One thing that jumps out at me from the various cases surveyed by Kean (and the other reading I've done) is the lack of reports about the everyday goings-on in the afterlife. There are few if any cases where a mind sends a message from the afterlife in the vein of "Hey, things are really great up here! I just had an amazing 84-dimensional experience with these purple-eyed aliens who died on a planet somewhere near Andromeda, 4,000 years ago. And Saint Anselm dances an amazing cha-cha!" Or, "It's really great not to be tied to a physical body that needs to eat and sleep and shit all the time, and live in a purer way. Directly perceiving the truths of all mathematical theorems independent of what axiom system one uses to formalize them, is also pretty cool. But occasionally I sort of miss the taste of cheesecake . . . " *The Simpsons* TV show depicted Heaven as involving Jimi Hendrix and George Washington engaged in a pleasant game of air hockey.

It is not especially clear whether the afterlife mind accessed in a mediumistic session is an entity that was persistently growing, changing, and acting in some afterlife domain, independent of the mediumistic access—or whether it is some sort of entity that is created by the mediumistic session itself, out of some more abstract "individual mind stuff" that continues to exist independently of the body previously associated with that now-

afterlife mind. But the dearth of concrete narratives about the afterlife from these afterlife minds provides some weak evidence that, if these afterlife minds are engaged in actions and dynamics when no medium is channeling them, they don't necessarily much resemble the actions and dynamics we are engaged in here.

Of course, it COULD be that human minds are romping in some sort of afterlife that is vaguely similar to our everyday human life on Earth, and just don't have any way to communicate directly about this life to us, or are restricted from communicating such things to us by some sort of post-embodied security clearance. Some theorists have posited that the dead people mediums talk to are stuck in a kind of temporary limbo, and haven't yet completed their journey from this world to the next. But the evidence for these sorts of hypotheses is very scant.

I would also say that, if one looks at the evidence Kean summarizes, there isn't terribly much that points in the direction of the traditional Judeo–Christian narrative regarding the afterlife. Some people do report God and Heaven sightings in their NDEs, but these are quite various in specifics, and it's easy to see how these would occur as a result of cultural conditioning. Phenomena like "white light" are extremely common in NDEs but are quite generic and don't indicate any specific theological explanation.

Tibetan Buddhism and other Eastern wisdom traditions seem to hold up a little better in the light of the evidence, particularly in the area of reincarnation. Some of these traditions have very specific theories of reincarnation, involving a certain amount of time spent by each soul between one body and the next, and so forth. However, these also leave a lot of key questions unanswered, such as (among many, many other issues) the mechanism of creation of new souls (given the explosion of human population, which would imply there are not enough reincarnated souls to go around for all the new babies). And their explanations of phenomena such as mediums talking to the minds of people dead for decades or centuries (long after their souls should have landed in new bodies), become complex and start to feel Ptolemaically "overfit" to the phenomena they're trying to account for. Overall, my own sense is that these Eastern reincarnation-oriented narratives fit the observations significantly better than Judeo–Christian heaven/hell oriented narratives, but don't constitute anywhere near a final or adequate explanation. (Kean does not phrase her conclusions in exactly this way, but given the tenor of her discussion of reincarnation-related phenomena, my impression is that she probably roughly concurs.)

Kean explicitly leans in the direction that an afterlife exists, and individual human minds persist in it in some form; but that the properties of this afterlife may be quite different from anything we imagine. . . . She

cautions against narrow, traditionalist versions of survivalism, noting that "certain beliefs about what survival would look like, which cannot be proven, are built into what survivalists call their 'hypothesis'" (p. 20). She uses quantum mechanics as an analogy to illustrate how aspects of the broader universe can operate very, very differently from our expectations:

> Think of it this way: We know the quantum world—the infinitesimal components of matter imbued with life—is governed by different principles and realities from the ones we know in our everyday lives. How different and unimaginable to us might be a world where consciousness exists postdeath? (p. 21)

In the chapter by Pim Van Lommel, he does consider that the evidence regarding survival-ish phenomena presents a compelling refutation of the idea that the mind exists ONLY in the brain; in fact he phrases this a little more strongly than I would:

> I have come to the inevitable conclusion that most likely the brain has a facilitating or receiving and not a producing function in the experience of consciousness. So under special circumstances our enhanced consciousness would not be localized in our brain nor be limited to the brain. (p. 123)

However, if one takes a careful look at quantum mechanics, one realizes that events and entities at different points in time may be considered subtly interconnected, beyond our commonsensical notions of causality. How much more true might this be once one moves beyond the confines of our everyday "material" spacetime continuum? Could it be that the extended consciousness of a human individual exists in spaces and times other than those occupied by the brains and bodies with which that individual is associated—but that still these brains and bodies play an integral role in maintaining and constituting that consciousness? But perhaps this falls under the aegis of what Kean would call a "facilitating role"? We currently lack a clearly-defined language for talking about such things.

Summarizing the evocative and compelling but generally confusing nature of the evidence regarding survival, Alan Gauld in concluding his chapter presents a series of heartfelt queries:

> And even if one accepts that in the present state of our knowledge some sort of survival theory gives the readiest account of the observed phenomena, many issues remain undecided. In the vast majority even of favorable cases, the "surviving" personality that claims continuity with a formerly living, or previously incarnated, personality, is only able to demonstrate such apparent continuity on a very limited number of fronts, and may, indeed,

markedly fail to demonstrate it on others. This does not, of course, mean that behind the observed manifestations there does not lie the fullest possible continuity; but equally it means that the hypothesis of complete continuity is unproven, and all sorts of possibilities remain open. Is there partial or complete survival? Sentient survival, or (far worse than mere extinction) survival with just a lingering, dim consciousness? Is there long-term survival or survival during a brief period of progressive disintegration? Is there enjoyable survival, or survival such as one would wish to avoid? Survival as an individual, or survival with one's individuality for the most part dissolved in something larger? Is survival the rule, or is it just a freak? To these and many other questions I can at the moment see no very clear answers. (p. 220)

Challenges

An excellent book like *Surviving Death*, confronting a critical and confusing topic, inevitably presents the reader with many challenges.

Even for the reader with a basic intuition that some sort of afterlife exists and that "paranormal" phenomena sometimes occur—and even more so, for the more skeptical reader—some of the phenomena recounted in *Surviving Death*, especially toward the end of the book, are going to be a challenge to believe. Did whole people really materialize out of thin air? But there's a slippery slope with all these strange phenomena. Once you accept mediumistic PK—tables jumping around and all that—then poltergeists are just a small step. And once you accept "traditional" poltergeists, is it that much more outrageous to throw in the occasional materialization?

Making sense of all the complex, confusing evidence from the various weird phenomena recounted, provides a different sort of challenge—and one that nobody has really met successfully so far. It is not entirely obvious that these phenomena *can* be rationally and scientifically understood—there is no logical requirement that everything in our universe must be regular and predictable enough to be susceptible to the methods of science. However, there do seem to be many recurrent patterns in the way various afterlife-related paranormal phenomena happen; I have summarized some of these above. These phenomena are anomalous relative to our modern scientific worldview, and relative to most of what happens in our everyday lives (especially in modern society; arguably relatives of these phenomena played a larger role in peoples' lives in many pre-civilized societies); but they are not utterly random and unpredictable glitches in the universe. It seems it may well be possible to form a rational scientific theory of how and why and when and where such phenomena occur, and what causal factors underlie them. But at the moment this remains a (fascinating) challenge.

In a recent paper (Goertzel 2017) I have proposed one potential theoretical direction for explaining psi phenomena and perhaps survival-

type phenomena as well. In the approach suggested there, our familiar spacetime continuum is viewed as being embedded in a wider space called a "eurycosm," whose properties are in some ways more mind-like than traditionally physics-like. The dynamics of the eurycosm have aspects similar to what Sheldrake (2009) has called "morphic resonance," which results among other phenomena in a sort of "pattern completion," wherein the presence of some part of an entity causes the emergence of other parts of that entity. Pattern completion dynamics in the "near eurycosm" (the part of the eurycosm closely coupled to our physical world) provides a different sort of route to explaining paranormal phenomena—different from materialist physical explanations and also different from spiritually or religiously focused explanations. For instance, the appearance of more and more parts of a dead person's mind in the mind of a medium, or an apparent recipient of reincarnation, may be explained—at least on a conceptual level—via a dynamic wherein the pattern of the dead person's mind, once it starts to flow from the eurycosm into a particular region of our spacetime continuum, is driven on by pattern completion dynamics to flow more and more. I find this theoretical direction appealing; but admittedly at this point it is a fairly raw set of speculations. The challenge of explaining either psi or survival-type phenomena in a rigorous and convincing way is not yet met.

As with psi-related phenomena in general, but more so, in addition to the conceptual challenges there are also sociocultural challenges involved with talking about, or doing research on, afterlife-related issues. In these contexts I often feel myself trapped between, on the one hand, intensely religious or spiritual people who place a great deal of faith in cultural narratives regarding psi- and afterlife-related phenomena; and, on the other hand, devoted scientific materialists who consider it obvious that I have somehow fallen for a bunch of delusive and fraudulent nonsense. Writing a good book on survival-related phenomena requires artfully dodging both of these camps; and Leslie Kean has met this challenge admirably, via focusing on clear, dramatic but precise accounts of real-world observations (which is clearly the right path given the weakness of our current theoretical understanding).

In reading *Surviving Death* I frequently found myself thinking about the film *The Matrix*, and the broader "simulation hypothesis"—the possibility that perhaps our universe is in fact a computer simulation or something similar. What if we are just conscious players inside some advanced variant of *The Sims*? What if they are looking in, amused as heck at our wacky theorizing about Heavens and Hells and morphic resonance and all that, because they know that the "paranormal" experiences we're fussing about so much are mostly just bugs in the software of the simulation? What if they know that mediumistic appearances of the minds of deceased people are

simply a matter of software code that allows the simulation at one point in time to access the backup database of information about the simulation at previous times? And so forth.

The "universe as a computer simulation" idea in its most literal form is probably too current-tech-culture–centric to be true, but something along those lines seems far from impossible. Who knows what types of simulation-building or universe-building technologies post-Singularity humans might create, or post-Singularity aliens might have created in the past? The main conclusion one is driven to, when musing about the large category of thinkable explanations for the various phenomena Kean reviews, is that we really have no idea what kind of world we live in. The regularities we have observed as a culture, and codified in the formalized patterns we call the "laws of nature," are both beautiful and useful to us in our context; but they are obviously not complete. Theological and philosophical ideas may well say something useful about aspects of the universe that science does not currently touch; but their vague and error-prone nature is obvious. Extending our current understanding of the universe to convincingly encompass the various phenomena Kean reviews in her book, with their implication of the apparent reality of some form of afterlife, is fairly likely to lead us to an understanding as far beyond our current worldview, as our current worldview is beyond that of a Stone Age tribe.

The issue of surviving death is, of course, not only a scientifically and philosophically fascinating matter, but an intensely personal matter for each of us. In that vein, I find it interesting to ask myself whether my relatively recent "conversion" to believing some form of persistence of the individual self beyond the death of the body is likely, has made me somehow more comfortable with the idea of my body dying. I would say that it has, but only slightly. I still feel strongly motivated to help find a cure for human death, aging, and disease. Aging and death have a lot of suffering associated with them, and that is not good, according to my ethical system. It is a bit reassuring to feel that, even if my body dies, the abstract patterns of my mind and personality, and perhaps stray concrete aspects here and there, may reintersect this world at some future points in time, independently of my body. But to me, this is not so different from the reassurance I feel that if my children and grandchildren live and flourish and reproduce, my essence in some sense will live on. Being reincarnated, even if it occurs, is not the same as keeping on living; and me existing in some space outside our spacetime continuum is awesome if it's a reality, but still not the same as me existing here in this spacetime. I suppose the bottom line is that the more concrete aspects of my personality are attached to their own continued existence, and not just to the continued existence of the more abstract patterns to which they are attached.

But that's just me. Each reader who is convinced at a gut level by Kean's book that survival of the individual after death is at least pretty plausible, is going to react in their own way. And this is how, I think, we will finally get to the bottom of these confusing, complicated, and critical matters. As more and more science-minded people study the evidence for survival-related phenomena, via *Surviving Death* and other books and articles in the same vein, more people will be thinking about the matter in both its scientific and its more human aspects. And this is just what we will need in order to finally crack these puzzles. Leslie Kean has done us all a significant service by writing an entertaining, highly readable book that also has the capability of stimulating readers to think and to reflect hard on these topics that are critical in both a personal and a scientific sense.

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BOOK REVIEW

The Enigma of Rosalie: Harry Price's Paranormal Mystery Revisited
by Paul Adams. Hove, UK: White Crow Books, 2017. xix + 273 pp.
£12.99, US$17.95. ISBN 978-1786770134.

This unusual and absorbing book reads like a detective story, as it should,
for that is what it is—the search for a plausible solution to one of the most
controversial episodes in the history of psi research.

It began on December 8, 1937, with a telephone call to Harry Price
(1881–1948), the highest profile psychical researcher of his generation,
making him an offer he could not possibly refuse: to attend a meeting of
a private home circle at which the materialized spirit of a six-year-old girl
named Rosalie regularly appeared. No names were mentioned other than
hers, and Price had to agree not to reveal the whereabouts of the private
house somewhere in the London area where the sittings took place.

He duly attended the meeting, and the following day a number of his
colleagues noticed that he seemed to be unusually affected by the events of
the previous evening. "Shaken to the core," said one. "Deeply disturbed,
almost distraught," said another, while his longtime associate Kathleen
('Mollie') Goldney recalled that "he was more excited and shaken than I
had ever seen him." What can have had such influence on a man known
for his willingness to unmask fraudulent mediums, which in his experience
far outnumbered those such as Stella Cranshaw and the Schneider brothers
Willi and Rudi whom he considered to be genuine?

To his credit, by the end of the day Price had written a 5,000-word
report on what he had experienced. It had been an unusual séance, for Price
had no idea who his hosts, Mr. and Mrs. X, or their guest Mme. Z, really
were except that Mr. X was a prominent businessman and Mme. Z was
the French mother of the deceased six-year-old who, he was assured, often
dropped in at their meetings. The Xs' teenage daughter and a young man
Price assumed to be her boyfriend were also present.

Price was understandably somewhat befuddled by his evening's work,
which had begun with a thorough search of the whole house during which
he sealed all the doors and windows, leaving him satisfied that there was
nowhere for an accomplice to lurk. He was perplexed by the apparent absence
of a medium, or any of the usual rituals of the Spiritualist meetings he had
so often attended. He was impressed, however, by the arrival on the scene

of Rosalie, accompanied by much weeping and wailing from her bereaved mother, who allowed him to examine the phantom by touch, which he did. He also noted that Rosalie, by the light of the luminous plaque he was also permitted to use, "looked older than her alleged years." One way and another, he found himself wondering "if Rosalie was a genuine spirit entity or if the whole thing was an elaborate hoax." Only a second sitting in his well-equipped laboratory could settle the matter. It was one he particularly wanted to settle, as he faced an uncomfortable dilemma: Either the spirit world really existed, something he had always denied, or he had been hoaxed despite his long experience of revealing hoaxes inflicted on others.

Price's account of his meeting with Rosalie was included in his book *Fifty Years of Psychical Research* (1939), which was published barely a month after the outbreak of World War II. It was generally well-received, reviews featuring such phrases as "erudite, critical yet vastly entertaining," "comprehensive and well-documented," "stimulating and very interesting."

There were minority dissenting voices, however. One suggested that the Rosalie episode might be one of "definite and rather brazen fraud," another finding it "a complete invention and unworthy even of Price," while Price's former colleague Eric Dingwall wondered "what is the real object of telling these tales?"

Following Price's premature and unexpected death in 1948, his reputation as Britain's leading authority on ghostly matters took some severe battering, notably in the attempted debunking of his best-known case, that of Borley Rectory (Dingwall et al. 1956), and later in Hall's (1978) shamelessly biased and vituperative biography. Rosalie put in another appearance in a book by Dingwall and Hall (1958), described by Paul Adams as "a catalogue of missed opportunities which, if properly exploited, could have gone a long way towards solving the Rosalie case."

Instead, it was "a superficially impressive but ultimately flawed and prejudiced examination." Adams pointed out that there were several witnesses still alive who could have given support to Price's activities at the start of the case, but none was consulted. Fortunately for posterity, new researchers now entered the fray. One was David Cohen (1965), a factory worker from Manchester who headed a small group of like-minded enthusiasts in his area, and who decided to carry out his own search for
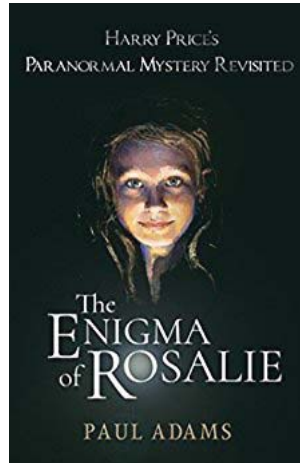
the solution to the Rosalie mystery. He was later joined, independently, by fellow Society for Psychical Research members Richard Medhurst and Mary Rose Barrington, who tramped the streets of much of London in search of a house that fitted Price's description of the X residence (Medhurst 1965).

It was Cohen who obtained the scoop of his career when he managed to contact Rosalie herself, or at least the woman who had been masquerading as her, and to obtain her lengthy written confession, which Adams prints in full as Appendix B. This, if true (and there were those in the SPR who suspected otherwise) is a plausible scenario that answers many questions, including: Who were Mr. and Mrs. X and Mme. Z? Why were they so keen for Price to attend a séance, but only once and only if unaccompanied? Why were they so unlike all members of Spiritualist groups that he had encountered? What were they really up to? Paul Adams tackles these and many other questions head-on, and his intriguing and fully referenced book makes lively, enjoyable, and often surprising reading.

Reviewers of mystery stories should not give away their endings, so this Review will leave future readers with a brief trailer, from the letter from Rosalie to David Cohen (emphasis added):

> It struck me as very amusing that Mr. Price should take so much trouble to seal the doors and windows when he was actually sealing Rosalie *inside* the room.

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BOOK REVIEW

Here's Nessie: A Monstrous Compendium from Loch Ness by Dr. Karl P. N. Shuker. CFZ Press, 2016. 274 pp. $19.99 (paperback). ISBN 978-190948845-8.

Not another book about Nessie! Even its author bemoans the "veritable ocean of Nessie-themed books" already crowding the shelves. But wait, this one is different!

Advertised up-front as a compendium, one might suspect that it could be just a rehash of the author's earlier publications, repackaged for improved marketing. Shuker does indeed draw on his extensive work on the Loch Ness creature, much of it unpublished except as recent installments of a blog, but he brings everything up to date in an original and attractive presentation.

Here's Nessie is also different in terms of the scope of its contents. The core chapters include a brief review of some of the most compelling observations, followed by an extensive discussion on the nature of the beast. A chapter on the "relic plesiosaur hypothesis" documents the ongoing efforts by its proponents to reconcile the long-necked creature described by witnesses with what they imagine a plesiosaur could have morphed into over 65 million years of evolution. Another chapter deals in a similarly meticulous fashion with the "long-neck seal" hypothesis. Other suggested identities also receive consideration.

Beyond the fundamental zoological enquiry, where Shuker's professional expertise clearly stands out, further chapters cover other Nessie–cryptozoological topics: early encounters with British saints; an account of the 1987 International Cryptozoology Society meeting in Edinburgh; striking hoaxes; and a couple of "Nessie-ssary" reviews (*Loch Ness Discovered*, Discovery Channel, 2005; Tony Harmsworth's *Loch Ness: Nessie & Me*).

At this point, the presentation gets more personal. Shuker presents an extensive color photo gallery of the many faces of Nessie, wherein he also appears, smiling, in the company of Nessie models and figurines. We now also learn about his wider interests: Nessie on stamps, Nessie in music—a chapter on Loch Ness Monster–themed tunes, many accessible on YouTube—motorcycling, and even some poetry. This is what I found most appealing about his book: its candid approach, offering an opportunity
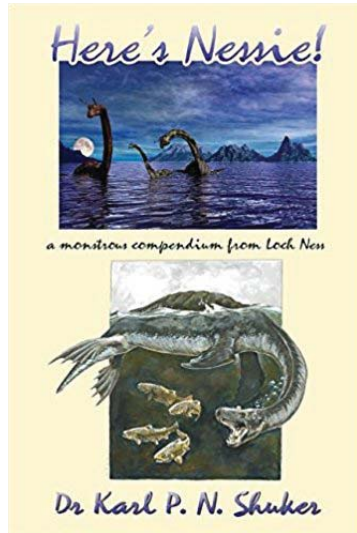
to get to know an original and passionate scientist, independent and unburdened by the fetters of academic proprieties, much in the tradition of Bernard Heuvelmans, the father of cryptozoology.

I was pleased to find an extensive bibliography, a list of current Nessie-themed websites, and an index of animal names. The book is a product of the Centre for Fortean Zoology (CFZ), "The World's Weirdest Publishing Company," as it describes itself in a short appendix. The style and presentation reflect the enthusiasm and earthiness of the CFZ as well as of its eccentric and indefatigable leader Jonathan Downes.

I would recommend *Here's Nessie* to newcomers as an introduction to the Queen of Cryptids; I am sure veteran and knowledgeable Nessie fans will also greatly enjoy it.

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BOOK REVIEW

The Rise and Fall of Modern Medicine by James Le Fanu. New York: Basic Books, 2012 (revised and updated from 1999 Abacus edition). xviii + 590 pp. $22 (paperback). ISBN 978-0-465-05895-2.

This is a phenomenally instructive book, a level-headed analysis, and recommended without reservation.

Le Fanu is an M.D. in general practice in London and a regular columnist for *The Telegraph*. (The book is written with British spelling, and a few remarks are specific to UK's National Health Service, but everything is nevertheless relevant internationally, globally.)

The instructive first part of the book describes "twelve definitive moments" in the development of modern—i.e. contemporary—medicine: 1941, penicillin; 1949, cortisone; 1950, streptomycin, also smoking and Sir Austin Bradford Hill (epidemiology); 1952, chlorpromazine and revolution in psychiatry; 1952, polio epidemic in Copenhagen and birth of intensive care; 1955, open-heart surgery (the last frontier); 1961, new hips for old; 1963, transplanting kidneys; 1964, triumph of prevention (of strokes); 1971, curing childhood cancer; 1978, the first test-tube baby; 1984, Heliobacter, cause of peptic ulcer.

Those episodes are described at length, followed by an analysis of this "Rise" of medicine. Those defining events came from serendipitous discovery of drugs, the development of clinical science, for example Bradford Hill's statistical epidemiology, and staggering technological innovation: heart–lung machines and laparoscopic surgery. But credit for all this goes not only to the brilliant and persistent pioneering physicians and researchers, Le Fanu credits also "the mysteries of biology": the unanticipated, unforeseeable fact that antibiotics can be effective against a range of bacterial pathogens, and the equally astonishing fact that cortisone is capable of treating or ameliorating a staggering range and variety of conditions.

The analysis is both deep and level-headed, as illustrated by a cautionary note in the story of prevention: two adverse effects of informing someone that their blood pressure needs to be lowered: first, it induces worry and the associated nocebo effect of adopting "a sick role"; second, some small proportion of people find side effects of the treatment unacceptable—for example headache or (in men) impotence (pp. 154–155). Furthermore,

the dramatic benefit of lowering *obviously high* blood pressure to prevent strokes came to be extended to "treating" "mild hypertension," where medicating 850 people prevents only one stroke per year (p. 155). Similarly with cholesterol. "And so the great—and very desirable—project of preventing strokes by treating hypertension has enormously expanded the scope of medicine from treating the sick to finding, in the majority who are well, 'illnesses' they do not necessarily have, and treating them at enormous cost" (p. 156).

A central, crucial point made several times is that vast ignorance characterizes medicine: "The causes of the common diseases of middle life are simply not known, and self-evidently without knowing their cause, they can be neither prevented nor cured" (p. 203). The Rise of modern medicine "owed more to a synergy between the creative forces of capitalism and chemistry than to the science of medicine and biology" (p. 245). The "golden age of drug discovery, 1940–75" is summarized on p. 246. Le Fanu acknowledges that some genuinely useful drugs were discovered more recently, but from the 1990s on most of the "blockbuster" drugs have been simply variants of discoveries from a couple of decades earlier. Two cited more recent discoveries are a vaccine against hepatitis B, and the triple-therapy cocktail for treating AIDS (p. 284)—but the latter is a hugely damaging mistake based on the erroneous view that HIV causes AIDS.[1] Many newer drugs are of doubtful efficacy, for example prescribed in Alzheimer's disease or multiple sclerosis (p. 285).

The marvels of technology that contributed to the "Rise" have become abused: too much unnecessary testing (pp. 289–291) with subsequent harm from misguided treatment, for example foetal monitoring (pp. 291–295); and prolonging quality-lacking, burdensome life by methods that may be responsible for about one third of the $62 billion spent in the USA on intensive care (pp. 296–299). So the optimism engendered by the Rise dissipated, and the Fall ensued, guided by two misguided ideologies: "The New Genetics" that looks to genomes as the cause of every ailment, and the "Social Theory" that assigns so much blame to environmental causes and lifestyle.

Genetics, Le Fanu argues, can hardly be a very significant factor in common human diseases since we have evolved as an extremely successful species. Truly genetic disorders are not very common. For such diseases as cancer, genetics contributes only as one of several factors, of which the most important one is ageing (p. 347).[2] Carriers of the gene implicated in retinitis pigmentosa may or may not develop the disease (p. 349). There is no simple path from genome to later development. The original idea that one gene codes for one protein was wrong. Genes interact with one

another, so-called "junk" DNA does have important functions, and complex signaling systems modify what genes do and turn them on and off at just the appropriate times (Ast 2005).

Social Theory indicts lifestyle including diet. However, the first test of cholesterol-lowering drugs (cholestyramine) found no difference in all-cause mortality between treated and control groups. Admittedly, there were fewer heart attacks among the treated—30 versus 38, group sizes both 1,900. So a reduction by 8/38, about 25%, enough for proponents of lowering cholesterol to cite it in support. But since all-cause mortality was *not* decreased, the treated people were simply dying of other causes, perhaps even from side effects of the treatment (p. 377).

Narrowing of arteries, atherosclerosis or arteriosclerosis, "when examined under the microscope, is strongly suggestive of an inflammatory process." Indeed, one study of heart attacks found chlamydia infection in a significant proportion of victims; and later studies concluded that the development of arteriosclerosis depends on the number of various infections encountered. Moreover, the changing incidence of heart disease over time, rising and then declining again like a drawn-out epidemic (p. 375) is consistent with an infectious cause. At any rate, cholesterol levels in the blood have turned out not to be the critical causative factor. Nevertheless, the interests vested in the cholesterol theory have been so powerful as to maintain hegemony of "the great cholesterol deception" (pp. 381–382).

Moreover, Le Fanu argues, it is very difficult to change the body's internal physiology by changing diet. Evolution has produced interacting systems that keep physiological variables within healthy limits by increasing and decreasing production of all sorts of substances. It is quite implausible that changing diets could change drastically the equilibrium levels of cholesterol in the blood (p. 382).

"The notion that cancer might simply be caused by the sorts of food we eat is strongly suggestive of quackery" since the incidence of cancer "is so strongly related to age," increasing ten-fold per decade of age (p. 383). The evidence offered for diet as a cause consists primarily of such comparisons as between rates of pancreatic cancer in Connecticut (60.2 per million) and in India (21 per million) (p. 386). What else than diet could account for this?

So "cancers common in the West, such as those of the breast, colon and pancreas, have been attributed to a 'high-fat' diet." But in the USA, there is no difference between Mormons and Seventh-Day Adventists in the incidence of these cancers, yet the former are meat-eaters whereas the latter are vegetarians (p. 387).

Le Fanu is similarly skeptical of claims of significant harm from the "minuscule" amounts of pesticides and the like in food. He cites the
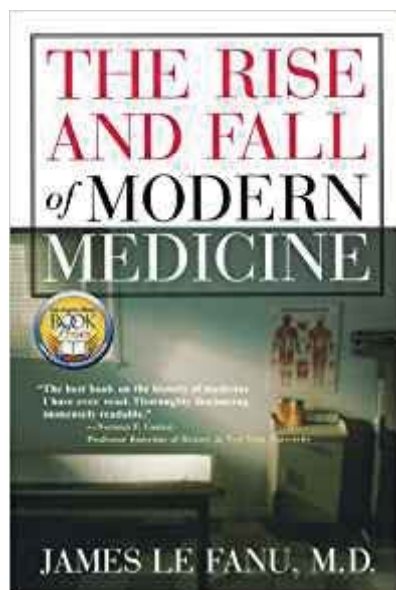
study by Ames (1990) which found that synthetic pesticides were no more carcinogenic in animal models than are the natural pesticides in all common fruits and vegetables; and 99.9% of all pesticides to which people are exposed are those natural ones (pp. 392–393). The concern that "feminising [sic] chemicals" are responsible for declining male fertility are similarly implausible in view of the presence of natural oestrogens in such foods as cabbage, carrots, coffee, corn, garlic, olive oil . . . (pp. 393–394). Aaron Wildavsky (1995) is cited: "Of all the subjects I have studied in over thirty years as a social scientist, environmental issues are the most extraordinary in that there is so little truth in them" (p. 395).

The data that the New Genetics and the Social Theory seek to explain are gathered by epidemiology, which cannot however discover an unknown biological factor as did the serendipitous observations that led to antibiotics and steroids. Epidemiology can only study observables, but the common chronic ailments arise from ageing or from unknown biological factors; so explanations that epidemiology seems to offer "are likely to be pseudo-explanations" (p. 398), misleading like the "cancer-causing genes" of the New Genetics or the dietary and environmental claims of the Social Theory. Unfortunately, it is easy to do epidemiology, hence the myriad studies faithfully reported in the media that find that coffee causes cancer, and then that it does not; and that fat causes heart disease, and then that it does not; and so on (p. 403). Contemporary medical epidemiology lacks the rigorous methodology that it needs (p. 399), yet policies and recommendations are based on less than rigorous epidemiological reports, for example that baked beans prevent cancer or that children might ingest carcinogenic chemicals if they chew plastic ducks (p. 404).

The causes are simply not known for most diseases: neurological (e.g., multiple sclerosis), rheumatological (e.g., rheumatoid arthritis), and of the gut (e.g., Crohn's disease). There are unknown biological factors somehow at work. Le Fanu believes that there may well be unrecognized *infectious* agents involved. Multiple sclerosis (MS), he suggests, has characteristics of

an infection: it is episodic; much more common in some geographic areas than others; became 10 times more prevalent in Britain over 50 years; and, a common feature of infectious disease, it has become less severe over time, bringing death after about 8 years in earlier times but after about 25 years nowadays. Admittedly, there is a genetic association, since the incidence of MS is quite high (one in 50) if a sibling has MS, and one in two if the sibling is an identical twin. Nevertheless, Le Fanu believes this represents *susceptibility*, not a direct genetic cause (p. 408). Most suggestive of all: MS was unknown in the Danish Faroe Islands before 1943, but 16 cases occurred (in a population of only 30,000) between 1943 and 1949, after the islands had been occupied by 7,000 British troops. So the cause of MS may be a widespread infection to which only a small proportion of people are susceptible.

Childhood leukemia, similarly, occurs in clusters in some geographic areas that seem to have this in common: they were previously isolated, small communities that experienced an influx of a large group of outsiders (p. 410).

Admittedly it is quite radical to suggest that MS or childhood leukemia could be owing to infections, but Le Fanu also gives suggestive evidence (see above) that heart disease reflects something infectious and not cholesterol levels; and he points out that:

➢ The Heliobacter that causes peptic ulcers was not discovered until 1984.
➢ Dandruff is caused by a fungal infection.
➢ Lyme disease and syphilis are both caused by spirochetes bacteria that are notably difficult to detect, especially in chronic infections that sometimes persist if treatment in the acute phase of infection has not killed all the bacteria.
➢ Some believe that rheumatoid arthritis may be induced by the proteus bacterium (p. 411).
➢ Prions exemplify the quite recent recognition of an entirely new genre of infectious agents.

Unfortunately, a retrovirus is also mentioned as a possible pathogen (p. 413), citing HIV, whose implication as the cause of AIDS turns out to be mistaken.[3] Earlier (p. 284), the book had been misleading in citing favorably for treating AIDS the mid-1990s triple-therapy cocktail, all of whose components are seriously toxic (Bauer 2007:130–131). It bears recalling that Luc Montagnier, credited as the co-discoverer of HIV, had shown that the cell-killing cause of AIDS was not HIV but rather a mycoplasma.[4] And

mycoplasmas would be an additional example of the often unrecognized or unsuspected infectious agents (Pease 2005).

The notion that modern medicine experienced a Fall after an initial rise "is admittedly difficult to accept," Le Fanu concedes. But he asserts this is actually a general phenomenon: "Every field of human activity has its Golden Age, which is followed by a decline in creativity and new ideas" (p. 418), citing geology; natural history culminating in Darwin and evolution by natural selection; theoretical physics peaking with relativity and quantum mechanics [and declining into string theory!]. That generalization is said to be consistent with a "Law of Acceleration" proposed by "the American historian Henry Adams," but no source is cited. However, the idea that human activities naturally experience a decline following a notably successful rise was explicitly discussed by Parkinson (1958), illustrated by the history of the British Navy.

This book is an essential addition to my bibliography[5] of works describing what has gone wrong with modern medicine. Every reader will surely learn something from it and be stimulated to further thought and enquiry.

Notes

[1] Henry H. Bauer, The Case against HIV. http://thecaseagainsthiv.net

[2] Most people likely "know" what the media hyped after discovery of the first gene (BRCA) that supposedly predisposes to breast cancer. The media did not subsequently disseminate with equal fervor the finding that about 70% of breast cancers are not associated with heredity, and the BRCA genes are held responsible for only one quarter of the other 30%: Tabitha M. Powledge, "Breast cancer genes: Beyond BRCA1 and BRCA2" (Genetic Literacy Project), 8 April 2014.
https://www.geneticliteracyproject.org/2014/04/08/breast-cancer-genes-beyond-brca1-and-brca2

[3] Rethinking AIDS, http://rethinkingaids.com
The Case against HIV, http://thecaseagainsthiv.net

[4] References 26 to 31 in The Case against HIV, http://thecaseagainsthiv.net

[5] What's Wrong with Present-Day Medicine, https://www.dropbox.com/s/2cxs7a7862kmism/What%27sWrongWithMedicine.pdf?dl=0

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BOOK REVIEW

Making Sense of Quantum Mechanics by Jean Bricmont. Springer, 2016. 331 pp. $69.99 (hardcover). ISBN 978-3319258874.

Quantum mechanics is both perhaps our most successful scientific theory and the least understood. The standard or Schrödinger equation of quantum mechanics fits the experimental data remarkably well. Within the traditional (Copenhagen) framework, this equation describes the evolution of a wave function (a grouping of potential states) until a measurement discontinuously triggers the wave function to "collapse" into the observation of an experiment. As is well known, this interpretation provides no mechanism or ontology to account for this instantaneous collapse. Currently, there is no consensus that favors an interpretation for this measurement problem.

Jean Bricmont's *Making Sense of Quantum Mechanics* is a welcome contribution toward helping us navigate through the complex and paradoxical nature of quantum mechanics, as well as the various attempts to explain it. While Bricmont offers a great deal of technical rigor, he focuses on the conceptual problems in a relatively straightforward and accessible way. To be clear, Bricmont does not eschew mathematics. However, the level of mathematics involved here is what would typically be required in a first or second year course for scientists and engineers: linear algebra, complex numbers, Fourier transforms, basic differential equations, and classical mechanics. And most of the formal proofs and analyses are relegated to the appendices. In addition, many technical aspects and references to more advanced literature are placed in the footnotes. This book is therefore organized in a way to serve a wide range of interested readers.

Bricmont begins by reviewing key aspects of the philosophical debate that emerged among the founders of quantum mechanics and their students. What evolved to be a primary thread of the Copenhagen interpretation, championed by Niels Bohr, Werner Heisenberg, Max Born, as well as others, gave the experimental observer a deus ex machina role in supplying definite properties to objects without explaining how this occurs. Bricmont provides to us quotes to demonstrate how Bohr, Heisenberg, and their followers argued that quantum mechanics did not deal with elementary particles per se but rather our conception of them. As a result, they argued, quantum physics ends up primarily dealing with what we can or cannot say about the subatomic realm. And these views have prevailed and persisted

throughout the history of quantum mechanics via later advocates such as Eugene Wigner and John Wheeler.

However, Albert Einstein and Erwin Schrödinger were opponents of arguments that put such emphasis on awareness, observation, or measurement at the expense of objective reality. Einstein believed that the statistical nature of quantum mechanics reflected the fact that the theory was incomplete and that a more complete description would eliminate the need to refer to an observer. Einstein eventually formalized his thinking (with his colleagues Podolsky and Rosen) into what is now referred to as the EPR argument, which held that the nonlocality inherent in quantum mechanics implied that the theory is incomplete; that is, some form of hidden variables was needed to make sense of the results and rule out action at a distance. However, John Bell later showed, in the context of the EPR framework, that assuming both locality and hidden variables leads to a contradiction. And Bell's argument was eventually verified by experiment. Bricmont not only takes us through the arguments of EPR and Bell, but he discusses the confusion on the part of many who misinterpreted Bell's results to rule out the role of hidden variables. As Bricmont notes, Bell argued that his work demonstrated the nonlocal nature of quantum mechanics, not that hidden variables of some sort were ruled out.

Bricmont also addresses the confusion on whether or not Bell's work vindicated Bohr. Despite Bell's demonstration of the nonlocal nature of quantum mechanics, he was firmly opposed to the role assigned to the observer by Bohr and Heisenberg. Here are two sample Bell quotes Bricmont uses to illustrate this:

> One wants to be able to take a realistic view of the world, to talk about the world as if it is really there, even when it is not being observed. I certainly believe in a world that was here before me, and will be here after me, and I believe that you are part of it! And I believe that most physicists take this point of view when they are being pushed into a corner by philosophers. (p. 13)

> But experiment is a tool. The aim remains: to understand the world. To restrict quantum mechanics exclusively about puddling laboratory operations is to betray the great enterprise. A serious formulation [of quantum mechanics] will not exclude the big world outside the laboratory. (p. 15)
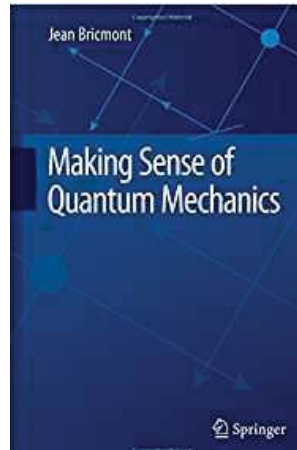
Bricmont's layout of this debate prepares the reader for his primary aim throughout the book: advocating for the de Broglie–Bohm interpretation of quantum mechanics (sometimes elsewhere referred to as Bohmian mechanics). The theory was introduced at approximately the same time as the Copenhagen interpretation by Louis de Broglie, but it was rejected by a large majority of physicists. After de Broglie abandoned the theory, David Bohm rediscovered and developed it. John Bell later became a strong advocate. Bricmont's presentation also relies on more recent work by Detlef Durr, Sheldon Goldstein, and Nino Zhangi, as well as by their collaborators.

As Bricmont explains, under the de Broglie–Bohm theory subatomic particles have well-defined positions and (highly non-classical) trajectories regardless of whether one measures them or not. (The particle positions are the hidden variables in this approach.) This is achieved through a guidance equation that acts on each particle. This guidance equation, which depends on the wave function, can be compared to the Hamiltonian in classical mechanics. Since this system is deterministic, the randomness can be traced to the uncertainty of the various particle positions or initial conditions. Thus the de Broglie–Bohm theory provides a relatively straightforward story about the way the subatomic world behaves without requiring the epistemological quandaries demanded by Bohr and Heisenberg.

However, you might say there is a catch. The guidance equation depends not only on the wave function, but the system configuration, that is, the positions of all the particles in the system. Further, this relationship is inherently nonlocal. Thus the wave function's influence on a particle may also depend on some other particle (or group of particles) at an arbitrary distance away. Also, within the de Broglie–Bohm framework, the subatomic system under investigation is entangled with every aspect of the process of measurement, which is after all another physical system influenced by the same laws. Therefore the particles under investigation, guided by the wave function, cannot be completely isolated from the measurement apparatus. The configuration of the system, which includes both particles under investigation and its environment, functions as a whole to determine the outcomes of observation. (Bricmont does not make explicitly clear that the relevant system in Bohmian mechanics is the universe, because of this entanglement between the system under investigation and the process of measurement.) Thus the wave function ends up inhabiting an extraordinarily large dimensional space of 3N, where N is the number of particles in the universe. I'll add more on this below.

Bricmont also discusses why the de Broglie–Bohm theory survives various "no hidden variable" arguments. One influential case was due to von Neumann; however, Bell has shown that he imposed some questionable

mathematical assumptions. Another hurdle includes work by Kocken and Specker, which was also thought to rule out hidden variable arguments. Kocken and Specker showed that measurements were essential aspects of the overall "context" of any quantum system under investigation. As a result, they and others argued that it was incorrect to posit that various properties of a quantum system had pre-assigned values that some process of measurement was supposed to detect. But the de Broglie–Bohm theory not only respects this "contextuality," it helps us to understand it. That is, the entanglement of the measuring process with the system under investigation leads the measuring process to influence the values of the various observables under investigation.

Bricmont also discusses in considerable depth and clarity the various alternatives available to the de Broglie–Bohm theory (as well as the Copenhagen interpretation). These include the Everett (many worlds) interpretation, spontaneous collapse theories, the decoherent histories approach, and QBism. Of course, Bricmont is not unbiased, but he does a decent job of providing arguments for both sides for each explanation. I'll focus here on the Everett interpretation, which posits that there is no "collapse" of the wave function at all; that is each possible outcome manifests. Hence the universe is continuously branching into a vast number of parallel realities. In addition to this ontological peculiarity (at least to some), Bricmont discusses the problem of reconciling the Born probabilities (the different probabilities associated with different outcomes or branches) with Everett's claim that all branches are actual (none are more real than the others). The author also dives into various permutations within the many worlds framework. These include functions describing mass densities, weighting factors applied to physical existence, and a "many-minds" scheme (where the splitting occurs inside a set of minds). He notes that all of these seem to depart from common sense realism and suggest we are radically deluded about existence itself.

Needless to say, the paradoxes of quantum mechanics have given rise to a bewildering array of theories, interpretations, and models that might lead us to abandon any hope of arriving at something resembling our experience. Bricmont endeavors to argue that the de Broglie–Bohm theory deserves our attention for providing an ontology most congruent with our world. This might raise the question: How is it that after a century of debate we are still no closer to a consensus theory? Bricmont addresses this too by exploring some of the history of thought behind quantum mechanics. He notes that the brilliant minds at the early Solvoy Congresses were grappling with unprecedented paradoxes. But Bricmont suggests how the power of authority within academia has arguably been used to lead physicists

and philosophers astray. He also explores how and why most physicists have managed to overlook the proposals of de Broglie and Bohm, as well as more recent efforts by Bell. A key question here is why de Broglie's proposal didn't receive more favorable attention at its original presentation. Apparently, an important factor was that de Broglie himself had doubts about his own theory because it posited a wave function existing in a space with an unusually high number of dimensions.

It is at this point that we might note some ways where Bricmont's admirable efforts perhaps fall a little short. The ontological status of the wave function's high-dimensional space remains an unresolved and baffling question, even among advocates of de Broglie and Bohm. Does our reality truly contain, as Albert (1996) argues, a mind-numbingly large number of dimensions? If so, how is such a reality linked with our familiar 3-dimensional space? On the other hand, perhaps the high-dimensional space of the wave function is merely a mathematical convenience, as Goldstein and Zanghí (2013) have argued. Goldstein and Zanghí maintain that the high-dimensional space of the wave function most likely demonstrates a nomological (lawlike) aspect of how subatomic particles behave in 3-dimensional space. Another important question is how can a wave function that requires a configuration space of 3N (again where N is the number of particles of the system) be reconciled with quantum field theory, where particles fluctuate in and out of existence. Ney (2013) suggests that we might deal with this through positing that the wave function inhabits an infinite dimensional space. It is perhaps regrettable that Bricmont stopped short of exploring a fascinating debate on this high-dimensional space that apparently led de Broglie to have misgivings about his own theory.

Of course, such questions lead us into Bohm's (1980, 1993) later work. Unlike de Broglie, Bohm embraced the reality of a space with perhaps infinite dimension. According to Bohm, this "space," which he termed "implicate reality," was an inherently nonlocal and holistic substratum of reality through which our familiar physical reality unfolds. And Bohm's implicate order was the foundation, not only for physical matter, but for conscious experience as well. It is perhaps unfair to criticize Bricmont for stopping short of exploring Bohm's later and more controversial work. This limit likely reflects a consensus opinion among physicists and philosophers of physics, which include advocates of the de Broglie–Bohm theory, that Bohm had gone beyond physics into mysticism. Few physicists are willing to seriously consider the possibility that consciousness may be in some sense fundamental. Thus Bohm's implicate order appears to breach a no-go zone within physics.

However, I believe that Bohm's implicate order deserves more attention.

A growing number of philosophers of mind are arguing that physicalist explanations cannot account for consciousness. David Chalmers has dubbed consciousness "the hard problem" and has persuasively argued that progress requires considering that consciousness may indeed be fundamental (Chalmers 1997). Given the persistence of both the "hard problem" and the measurement problem of quantum mechanics, it is hard to justify ignoring Bohm's implicate order while mainstream physics continues to make room for interpretations that in some ways are arguably even more radical.

Bohm's implicate order also departs from the more deterministic nature of the de Broglie–Bohm theory. That is, Bohm argued that the more fundamental space of the implicate order was composed of pure potentiality, which was likely the ultimate source of the Born probabilities in quantum mechanics. This underlying strata of potentialities, as the basis for both consciousness and matter, provides an interesting framework for explaining various anomalous behavior such as psi. Bohm himself explored the possibility that precognition and psychokinesis could be explained within his implicate order framework. It is perhaps the case that such efforts will win him few mainstream advocates anytime soon. Yet it is perhaps commendable that Bohm was unusually unconstrained in his thinking. Perhaps such radical proposals are needed in order for us to make advances on the stubborn problems of quantum mechanics and consciousness.

The possibility suggested by Bohm's implicate order that mind and matter may be subtly linked raises a rather important philosophical point that I skipped over above: whether scientific realism holds within the domain of quantum mechanics. Bricmont notes that arguments limiting our ability to truly probe the quantum realm have assumed various forms of idealism. Overall, I am sympathetic to Bricmont's argument that we ought to be able to discuss the underlying ontology of our world without getting snared within our own processes of observation and experience. But Bohm's implicate order, as well as the persistent mystery of consciousness, suggests that more open-mindedness about how we treat consciousness and matter is justified. At the least, scientific realism's demand that the physical world remains independent of human consciousness may end up requiring some caveats.

It's interesting that Bricmont begins his discussion on scientific realism by quoting Bertrand Russell: "I see nothing impossible in a universe devoid of experience. On the contrary, I think experience is a very restricted and cosmically trivial aspect of our tiny portion of the universe" (p. 73). We can note that this quote does not well represent all of Russell's thinking. He favored idealism at an early stage in his career. More importantly, Russell's (1927) thesis on the intrinsic aspect of matter has been recently gaining

currency among philosophers of mind. A key point for us is that this argument led Russell to a view that can be fruitfully compared with Bohm's implicate order. The heart of this argument is that while science provides us with a sophisticated mathematical understanding of our world, it is nevertheless silent on its intrinsic aspect. That is, science informs us about the quantified relationships between ultimates such as mass while telling us little about the ultimates themselves. Russell noted that our most basic experiences are perhaps the best candidate for something intrinsic. Thus he proposed that experience itself is intimately connected with this intrinsic aspect of reality. This is the foundation of Russell's neutral monism; however most contemporary philosophers have been using it to explore the possibility of panpsychism. In any case, those who consider together Russell's argument, Bohm's implicate order, as well as such persistent mysteries as the high-dimensional space of quantum mechanics and consciousness, can consider themselves on solid ground for refraining to follow the more conventional thinking that divides mind from matter.

In many respects, Bricmont succeeds and covers an impressive amount of ground. He provides a clear, in-depth, and wide ranging exploration on the problems of quantum mechanics and various proposed explanations (with emphasis on his preferred choice of course). The mathematics is constrained, but only a bit, with most of the heavy lifting relegated to technical appendices. And I find it refreshing that Bricmont has devoted so much space around philosophical debate and historical context. This book is a welcome contribution toward making sense of a highly abstract and puzzling subject. However, the possible links between consciousness and the subatomic realm will need to be explored elsewhere.

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