



ESSAY

# The Dark Spirit of the Trickster Archetype in Parapsychology

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**HIGHLIGHTS**

All scientists, and notably those working in frontier areas, must be vigilant to protect against over-interpretations and mis-interpretations of research results even when these are not outrightly ambiguous.

**ABSTRACT**

In this paper, the phenomenology of the Trickster (its 'darker' side) is explored. The archetypal Trickster is shown to manifest as psychosociological aberrations and bizarre physical effects often associated with unique individuals during certain emotionally charged states. Though the Trickster and its many variants have mythological roots, the modern-day equivalent (free, for example, from anthropomorphization) can be seen as an activated psychological proneness to err in thinking when a liminal phase is entered into—that borderland between doubt and certainty. Mainstream academia considers the field of parapsychology to be controversial—it is marginalized because the phenomena it studies (the paranormal) is mostly illusive, usually weak even when proved to be statistically anomalous, and the psi process itself has not been theoretically explained. This state of affairs propagates uncertainty which can trigger 'tricksterish' (spurious) interpretations of parapsychological data and findings: Long-term experimenter psi and chronological decline effects are cases in point. Due caution and bias-free analysis of the data and findings may help ameliorate, perhaps even dissolve, the problem of the Trickster.

**KEYWORDS**

Archetypes, meta-analysis, paranormal, parapsychology, psi, synchronicity, Trickster.

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**INTRODUCTION**

To write this paper, I have to admit a certain dependence on a controversial book by George Hansen, *The Trickster and the Paranormal* (Hansen, 2001). Occasionally Hansen himself seems evidently prone to Trickster

phenomenology, but his general message is nonetheless sobering and should make parapsychologists think hard about their positions in the field, no matter how entrenched and safe they think they might be inside or outside academia. Likely as not, that 'entrenchment' and apparent safety might underlie a naïve system of belief set



up by the parapsychologist to help create a false sense of security.<sup>1</sup> Ultimately, parapsychologists can find themselves marginalized, which is one of the main issues Hansen deals with in his book. However, this paper aims not to explore those issues (at least, not entirely); of greater interest is finding out how the Trickster archetype relates to a range of effects, not necessarily paranormal.

The Trickster is only one of many archetypes. Jung (1952/1969, para. 841, 847) described archetypes as nodal points or structural components of the so-called collective unconscious (deep in the mind) that govern or influence our “patterns of behavior” (para. 841). In other words, archetypes play a pivotal role in how we think, feel, and act (see also Haule, 2011, for neuropsychological parallels and justification for the construct). These effects are most often psychological but may also be parapsychological, as observed by Jung (1952/1969) in his synchronicity theory. It may help to conceive of archetypes in a similar way instincts are understood. The archetype functions (metaphorically) like a blueprint that is not evident in the building *per se*, but must be presumed to exist. Since these archetypal effects often occur outside our conscious control, they can give the impression that something other is responsible.

While I will state that it is likely some of these Trickster effects may be examples of synchronicity—an acasual connecting principle (defined shortly)—other tricksterish effects are not paranormal *per se*, but may include spurious research findings in parapsychology, which puts an even bigger burden on the parapsychologist’s shoulders. As I hope to show, readily identifying signs of tricksterish elements in psi research can help us avoid the pitfalls of the Trickster archetype.

Hansen (2001) describes the Trickster as a personification of a “collection of abstract properties that tend to occur together,” such as “disruption, deception, ... psi phenomena, and marginality” (p. 427). Further details can be found in Jung (1959/1968, para. 456-488). The Trickster is a personality of “no fixed shape, form or image” (p. 427), but ‘he’ seems more than a personality, and a changeable, fickle one at that (cf. Hermes/Mercury, Pan, and Loki for Western mythological equivalents).<sup>2</sup> Outwitting the gods of the various pantheons for his own sake (sometimes just to make a point), or for the sake of humankind, he (or it) is best described as a process and is activated (i.e., the archetypal qualities that it embodies are constellated psychologically) when a liminal phase is entered into. This liminal phase or condition can lead the individual, or a social group, to acts of subversion or deconstruction of binary (i.e., oppositional) systems whereby “there is a reversal of the positions of privilege or a blurring or collapse of the line dividing the pair [of

opposites]” (p. 62). Such opposites include God/Human, Heaven/Earth, Male/Female, Life/Death, rational/irrational, normal/paranormal, and so on. For example, rational behavior may become irrational, and even the normal physical rules we rely on can be overturned.

The Trickster, then, is activated under circumstances where conventional, privileged structures are criticized, threatened, or indeed attacked and subverted.<sup>3</sup> These criticisms can be seen as ‘liminal’ or as the embodiment of ‘liminal phases’. Thereby, the Trickster challenges the rationalistic assumptions uncritically laid down and maintained by our institutions. These conventional structures are embodied in academia—specifically our universities and other public institutions, including our political and bureaucratic structures, corporate and business enterprises, and orthodox religions (and let us not forget the media, who are never impartial when it comes to controversy). Ironically, we also see situations in our institutions where the Trickster is successful in overthrowing convention and takes root in the form of, say, new policy, and consequences such as reduced status, relationship or career failure, etc., can threaten those who fail or refuse to adopt the new perspective.

The Trickster can take a more subtle guise. There is the Trickster of misinformation or misunderstanding (or even self-deception) about the nature of things, as is propounded by amateur and professional paranormal researchers who mistake one thing for another or are too easily swayed by half-truths or insufficiently developed theories and models (the same argument can be extended to conspiratorial ideation, which is not to undermine the truth-seeking objectives of its claimants). Yet for all that, the result can be an increased consciousness, *provided one is open to being wrong*. We also have the Trickster of serendipity (accidentally finding something of use while looking for something else), which can be mistaken for synchronicity.

Then there is the Trickster of the anti-hero, who always lands on his feet and succeeds in spite of his stumbling (e.g., Maxwell Smart in the TV show of the same name, and Inspector Clouseau of the *Pink Panther* films; see also Cambray, 2009). There are so many Trickster types, it is almost impossible to keep track of them all (e.g., ‘Q’ in the TV Sci-Fi series *Star Trek: The Next Generation*, whose role is to mislead for higher, but often self-serving purposes). Of course, all these types are idealized versions to better illustrate the concept, but we can sometimes spot tricksterish activity before our very eyes, and even get caught up in it. And just as it is possible to be a saint one day and a sinner the next, tricksterish manifestations come and go, and it is that elusive unpredictability that catches the individual unprepared.

What does the Trickster have to do with the paranormal? The simple answer is the paranormal too easily threatens conventional privileged structures. While delving into the study of psi and the supernatural, or having a passing interest in it, does not mean one need fear activation of the Trickster archetype in oneself, one may be a victim of the activated Trickster in someone or something else. For example, some Trickster individuals (e.g., James Randi), Trickster institutions (e.g., the Committee for Skeptical Inquiry), and Trickster publications (e.g., *Skeptical Inquirer*), feel compelled to debunk all manner of paranormal claims without serious investigation, or (what is worse) even bring to a close, by devious practices, parapsychological research programs in university labs or units, and cause trouble for the members of those labs or units (for examples, see Thalbourne, 1995). These, I feel, are the parapsychologist's real threats—not so much activation of the archetype from within.

Historically, the supernatural, and therefore the paranormal, emerged and evolved alongside the hegemonic tradition of the various privileged structures, and since the paranormal is anything but normal, it is immediate fodder for mainstream attack, persecution, and ostracization, as just described. Jung (1959/1968) was strongly aware of these negative tricksterish forces (para. 457, 469) and their sociological impact—hence, his hesitancy to introduce synchronicity formally to the world, as indicated in his Foreword to his *Synchronicity* essay (Jung, 1952/1969, para. 816), publication of which was delayed by 20 years because of the negative reception he feared for his theory.

Most people who have read even a little history are aware of 'dichotomous' or dualistic schemas (even prejudices) that are age-old, and part of the human condition. The gender divide and the Reformation are classic examples, the repercussions of which have echoed down the proverbial corridors of time and are still with us. Hansen (2001) gets more concrete when he takes other examples from the real world. For example, in the world of the paranormal, the Trickster is activated in the relationship between UFO sightings and psychic ability—a relationship noted earlier by journalists (e.g., Keel, 1978) and researchers (e.g., Basterfield, 2001; Basterfield & Thalbourne, 2002). This relationship is not coincidental, but destined nonetheless to activate the Trickster quality of marginalization. The likely consequences? Public ridicule by the media; loss of vocation, ostracization by friends, family, and loved ones; and even early death.

Patterns of events like these occur over and over again, and they seem to have beleaguered parapsychologists since the late 1800s. Hansen (2001) points out that the Trickster is also constellated as a result of attempts

to institutionalize parapsychology and psychical research because *organizing* the paranormal is antithetical to the Trickster. Hansen gives examples to show how success in parapsychology, in the form of establishing laboratories, research output, and journal circulation, is generally short-lived (other researchers have commented on this problem—(Schlitz, 2001; White, 1994). Even successful runs of psi experiments are destined to meet with deceptive Trickster phenomenology in the form of real and *apparent* experimenter effects, decline effects, and other biases that make psi elusive and 'mercurial' (i.e., tricksterish)! But even these so-called effects could be tricksterish nonsense. For the rest of this paper, I will focus on these phenomena and show how they may be physically real but can also be mere phantoms, in both cases generated by the Trickster archetype, especially its darker side. As I will seek to demonstrate in this paper, I believe the Trickster can be foiled, and though it cannot be torn asunder from the human psyche, it loses traction when we become more vigilant and aware of our biases.

### The Trickster in the Laboratory

There is one place where the Trickster is unwelcome—the laboratory. The Trickster constellates during specific moments when events and outcomes hang in the balance—these moments mark the critical phase of an experiment when probabilities of outcomes shift so that unlikely and unwelcome results (e.g., accidents) may be likely. Notably, these may be as rare as we expect them to be, thus evoking chance as a causal factor, but that does not necessarily lead to a satisfactory solution; especially at the stage when data and patterns in the data are to be interpreted. Jung (1959/1968) warns of the types of "accident", "gaffe", "slip", or "faux pas" that can thwart one's will and one's actions which are naïvely "chalked up as defects of the conscious personality" (para. 469). Jung wants it understood that the Trickster is like a personality in its own right working behind the conscious personality, and these 'defects of the conscious personality' themselves can open the door to trickster phenomenology.

There are well-known incidents where this problem has been demonstrated. Nobel Prize winning physicist Wolfgang Pauli is a case in point. As the story goes, Pauli had such a unique but destructive psyche that experimental physicist Otto Stern banned Pauli from his Hamburg laboratory. There were other occasions at the physics laboratory in the University of Göttingen, Germany (an expensive measuring device stopped working), and likewise at Princeton University, New Jersey (a particle accelerator sustained serious fire damage). Pauli seems to have had an affinity with fire and the damage it can cause.

His peers even coined the term ‘Pauli Effect’—jokingly, a second Pauli ‘Exclusion Principle,’<sup>4</sup> According to which, “a functioning device and Wolfgang Pauli may not occupy the same room” (Burri, 2005).

The Pauli Effect, which is markedly synchronistic in nature, may have been exacerbated by the fact that Pauli drank to excess, and he was noted for his irritability when giving seminars—his students dubbed him the ‘scourge of God’ and ‘the terrible Pauli’ (Peat, 2008). Synchronistically, the breakages in the laboratory are mere physical correlates (accidental parallels) of Pauli’s fractured mental state. Jung (1951/1969, para. 984) defines and describes synchronicity in the following ways:

1. The coincidence of a psychic state in the observer with a simultaneous, objective, external event that corresponds to the psychic state or content ..., where there is no evidence of causal connection between the psychic state and the external event, and where, considering the psychic relativity of space and time, such a connection is not even conceivable.
2. The coincidence of a psychic state with a corresponding (more or less simultaneous) external event taking place outside the observer’s field of perception, i.e., at a distance and only verifiable afterward.
3. The coincidence of a psychic state with a corresponding, not yet existent future event that is distant in time and can likewise only be verified afterward.

Jung adds that an archetype usually underpins synchronicity. C. A. Meier (2001) relates a synchronistic event involving inner and outer fire which took place outside the Café Odeon in Zurich where Pauli was seated, “pondering over his inferior function (feeling-color red)” (p. xviii):

There was a large, unoccupied car parked outside the café. [Pauli] could not take his eyes off it, and then it suddenly caught fire and burst into flames. (p. xviii)

This example gives further evidence of the psychological (inner) link of the experient to the physical (outer) event without the need to assume the link is causal, which is to say, Pauli did not *cause* the car to catch fire—that can be explained in any number of normal ways. In fact, relatively speaking, acausality *per se* is not the mys-

tery; the real mystery is how Pauli managed to get himself into these situations. Perhaps seeking an answer, it is worth noting that Pauli entered into therapy with Jungian analyst Marie-Louise von Franz whose toils with him have been made public (Gieser, 2005).

But what does the Trickster have to do with Pauli? Does it mean Pauli played tricks on people? No, of course not. At least, not out-rightly and certainly not consciously—when it comes to the slippery concept of the paranormal, we never really learn the source of the mischief (for want of a better term), even if we have our suspicions. Notwithstanding the fact that even scientific equipment can fail, and quite dramatically, and there is the possibility of incompetence on the part of Pauli’s peers who might have been looking for a scapegoat to cover their own ineptitude, the Pauli Effect might still be referred to by some as Pauli’s psi. But ‘psi’ could be a misnomer—as was the case with Ted Owens (the PK Man). Mishlove (2000) was never sure that chance could not account for some of the seemingly paranormal feats attributed to Owens. In the real world, though we may convincingly rule out causal connections, we still do not know how to rule out chance factors. For example, mechanical devices and equipment can also fail of their own accord, and since the phenomena are rare and surprising and seem to happen in the presence of certain people, our interest and attention should, by rights, switch to those persons rather than the phenomena (see Braude, 2007, pp. 148-149). As already noted, acausality *per se* then becomes less of a mystery compared to the greater mystery of how it is that Pauli managed to get himself into these situations. In the laboratory, we must note that Pauli was not welcomed as an experimenter or collaborator; quite the opposite—he was uninvited, a *persona non grata*—which speaks volumes.

Any experimenter can be a ‘Pauli-type’, though most would say they were just ‘having a bad day’ (such phenomena are referred to elsewhere as “experimental miscarriages”, and can include “various accidents such as equipment failure, procedural error, inappropriate randomizations, etc.”—Kennedy & Taddonio, 1976, p. 10). Of course, the Pauli-type *in reverse* is also possible, resulting in positive outcomes. Though nobody really knows if it is psi, whose it is, and whose is the strongest (the experimenter’s or the participant’s), psi from the experimenter has come to be known as the ‘psi experimenter effect,’ and many parapsychologists make hypotheses about it, they discuss it, they argue about it, and they even expect it (more on that later).

Given these few points, let us now consider the Trickster in slightly more positive aspect (some might say ‘Trickster in reverse’)—an arguably favorable effect.

In another synchronistic case, we have tricksterish phenomena at play in Jung's (1952/1969, para. 872-915) research into astrology. Jung conducted a series of studies in the 1940s as an attempt to test his theory of an underlying meaningfulness that would connect causally unrelated events. Jung was trying to establish empirical (statistical) evidence of a "psychic equation" between external events (astrological aspect) and inner experience (choice of marriage partner). Results suggested the presence of synchronicity in the form of three Lunar aspects of special interest to Jung (i.e., Moon/Sun, Moon/Moon, and Moon/Ascendant), each of which appeared in three respective subsets of data (para. 902), formed seemingly randomly "just as the post brought them in" (para. 873). The corrected statistics calculated on the pooled data were first regarded by Jung as "inconclusive" (1951/1969, para. 989), but not to "a degree which one could have described as probable" (1952/1969, para. 906), meaning the results were too improbable to be attributable to chance alone. Oddly, he still referred to "the chance nature of the result" (para. 909). The upshot of the whole effort is that Jung first found something, then he wasn't sure, then he was, then he wasn't!

The story, however, does not end there. Of particular relevance, synchronistically speaking, are three smaller experiments that Jung himself instigated. Three experimenter-subjects were instructed to select sub-samples randomly from the larger sample of natal charts used in the main analysis. The type of conjunction that appeared more often than any other in each sub-sample exactly described the psychological profile of each of the relevant subjects who selected that sub-sample:

1. The first subject was "in a state of intense emotional excitement." Mars is the "emotional" planet, and the sub-sample was dominated by Mars aspects (para. 897).
2. The second subject was a female patient "whose main problem was to realize and assert her personality in the face of her self-suppressive tendencies." Her sub-sample featured the ascendant-Moon conjunction, which has a passive (suppressed) influence in the chart (para. 898).
3. The third subject was a woman "with strong inner opposition whose union and reconciliation constituted her main problem" (para. 899). Sun-Moon conjunctions dominated her selection, symbolizing the union of opposites.

Statistically, the results were not considered significant, but the variances in the data, which indicated the featured conjunctions of each sub-sample, corresponded with the psychic state of the subject. Serendipitously, Jung had found a variant of what he was looking for—suggestive evidence of meaningful coincidence, not once, but three times.

Seemingly in denial, Jung later made this unwarranted remark to a friend: "what an excellent joke was made with the use of astrological statistics; people have even thought I wanted to prove something in favor of astrology. It is hardly worthwhile to deal with all this pack of nonsense" (J. Kirsch, 1972, as cited in Progoff, 1973, p. 137). In truth, Jung was seeking a proof of sorts for *meaningful coincidence* as an *astrological effect* (1952/1969, para. 867). As a side issue, he refers to the three above-listed effects as synchronistic demonstrations of the "secret, mutual connivance" (para. 905) and definitely claimed his co-workers' psyches were connected to the physical material (natal charts) by way of their apparently random selections that demarcated outcomes as nothing other than meaningful coincidence. While those 'selection biases' were not categorical proofs of astrology, it can be conjectured that the human psyche was involved in the synchronistic process.

A similar case of serendipity occurred in Christopher Moreman's (2003) fabrication of a so-called cross-correspondence effect.<sup>5</sup> Using pseudo-scripts (randomly selected portions of text from 15 randomly selected library books) and pseudo-mediums, he showed how very easy it is for investigators to concur that similar (meaningful) patterns can present across unrelated texts from different books. Parapsychologists and statisticians might wish to muse over the implications of Moreman's finding, but I also make the point that in the very same study, Moreman spotted a "surprising coincidence": One of his randomly sampled texts was by Rudyard Kipling, whose sister Alice Kipling Fleming (as 'Mrs. Holland') was a medium involved in the original cross-correspondences work. Moreman was prompted to note "how often strikingly meaningful coincidences actually occur" (p. 232). While Moreman shows how cross-correspondences can be fabricated and, therefore, can be meaningless, the irony and meaningfulness of the Kipling correspondence did not escape Moreman.

These trains of events are Trickster phenomenology at its most surprising, or even frustrating, if one wants to view it that way, as most people will see that these timely interferences get in the way of the so-called objectivity of science. Speaking of Jung's study, Progoff (1973) even noted that Jung thought there was some "trickster element at work" (p. 137). Slightly appropriate in this instance, it is interesting to note how Jung describes the Trickster

in a person's behavior whereby the person "manages to achieve through his [*sic*] stupidity what others fail to accomplish with their best efforts" (para. 456). But I am certainly not insinuating incompetence on Jung's part (only impatience, as he freely admits) or implying it is a characteristic of anyone working in fringe or mainstream science. And we have to take note of the second part of the quote—that our best intentions can still result in failure or misunderstanding. I will not elaborate on these aspects here—more importantly, the point I want to convey is that the Trickster plays out in a process that 'turns the tables' on us (as Jung and Moreman noted), and like all archetypes, it concludes with a shortlist of possible outcomes, all of which are predictable, and it is up to experimenters not to allow themselves to be deceived (the usual outcome) merely because they are swayed by what they perceive to be insurmountable evidence (for or against) that may later prove to be premature and/or not all that convincing upon closer inspection.

Jung (1959/1968) notes that the Trickster would be viewed with skepticism in a civilized society (for example, Parker & Millar, 2014, wish to avoid the anthropomorphic aspect the concept entails), but many of us would still describe our slip-ups as "fate playing tricks" or "things being bewitched" (para. 478) as if some outside entity was at work. And if we upscale, and propose that the Trickster might manifest in a more subtler guise, anything that happens worldwide to large groups of researchers over sustained periods of time might be naïvely viewed as good science when it was merely tricksterish science all along. That is a most important lesson to learn and highly relevant to the rest of this paper.

### Experimenter Psi and the Decline Effect

In this section, I will speak of two major problems that are, to varying degrees, the bane of the psi experimenter's existence—the experimenter psi effect and the decline effect. These problems are not unique to parapsychology, and they are not necessarily as bad as each other—experimenters can expect to come across one or both of these problems from time to time. Of course, experimenters will disagree with each other over which is worse. The point of this section is not to settle that score, but demonstrate the relevance of each problem to psi experimentation in the context of Trickster phenomenology, and show how subtle and tricksterish elements can be involved in the interpretation of experimental results.

The experimenter psi effect (a.k.a. 'E-psi') refers to "unintentional psi which affects experimental outcomes in ways that are directly related to the experimenter's needs, wishes, expectancies, or moods" (Kennedy & Tad-

donio, 1976, p. 1; for a discussion of E-psi, see Palmer & Millar, 2015, pp. 295-299).<sup>6</sup> This definition is limited, as we cannot rule out the experimenter's intentional (i.e., deliberate) psi, implied in the fact that some experimenters/investigators arguably try to "psychically influence their results" (see Palmer & Millar, 2015, p. 296). Also, those needs, etc., could be pro-psi or anti-psi. Either way, the experimenter is likely to have more motivation than participants, so the assumption of E-psi seems not only plausible but seemingly confirmed in some instances. However, this motivational bias does not mean E-Psi is always present; nor should we assume any outcome from E-Psi is necessarily in the direction favored. E-Psi is well documented but tends only to be discussed as a within-study effect (Kennedy & Taddonio, 1976). Sometimes related to E-Psi, is the so-called decline effect (DE), which is defined as "the tendency for high scores in a test of psi to decrease, either within a run, within a session, or over a longer period of time" (Thalbourne, 2003, p. 27). The DE is also well documented (Colborn, 2007; Rhine, 1969).

I must clarify my position on E-Psi and DEs. First, I am sure E-Psi will always be a problem as long as there are experimenters,<sup>7</sup> although I only see it as a problem *within* studies (if only because we cannot accurately determine participants' psi contributions let alone those of experimenters), but not so much a problem across studies (i.e., *between* studies). A solution to the problem of E-Psi is unlikely, but it is likely that E-Psi across studies (i.e., *between-studies* E-Psi)—as can be shown in the meta-analyses—is a pseudo-problem anyway.

Second, Thalbourne's definition of DE indicates there are two kinds of decline—'*within* study' and '*between* studies'.<sup>8</sup> In this paper, my main focus is on the *between-studies* DE, which is a steady decline in psi across studies conducted by a broad range of experimenters and laboratories. My reasons for this focus will become evident throughout this paper, but basically, *within-study* declines are mostly caused by boredom, lapses in interest, and loss of motivation (I do not dispute these reasons), whereas the *between-studies* DE requires more sophisticated explanations such as improvements in study quality over the years, or deliberate changes in experimental design (from simple and fun, to complex and tedious) because theoretically oriented experimenters want to understand the psi *process* rather than merely *prove* psi's existence. To varying degrees, Bierman (2001) disputes both explanations for DE, as I do. I argue that *if* there appears to be a *between-studies* DE, that decline is *more than likely* an artifact. However, mistakenly accepting the DE artifact as a genuine effect (this happens, and often) illustrates how Trickster phenomenology can hold unwarranted sway over a community. I will make the

same argument for E-Psi.

## Types of Experimenter

Millar (2012) argued that “parapsychology’s elite are themselves particularly endowed with psi ability: they attribute scoring to subjects, in line with the psychological tradition, while it actually comes from their own psi” (p. 1). The ‘elite’ are the experimenters, and ‘subjects’ are participants. Under the assumption that experimenter psi (E-Psi) exists (albeit to varying degrees), there are only two mutually exclusive ways it might manifest: Either exactly as Millar proposes—Hypothesis 1 (H1): Experimenters cause *all* the psi and participants do not have any psi—or Hypothesis 2 (H2): Experimenters can combine their psi with their participants’ psi (this is the more conventional view).<sup>9</sup> Whichever is the case, it is the custom for psi researchers to seek support for the psi hypothesis by finding a significant effect as psi-hitting, but E-Psi can sometimes manifest as psi-missing, and intentionally so (more on psi-missing later).

Having stated H1 and H2, I find I need to consider both in the light of the “distinct groups” conceived by Palmer and Millar (2015), who classify experimenters as “Virtuoso” (one who “regularly produces clearly significant results”), “So-So” (“occasional significance”), and “Psi Challenged”, a group who “(almost) never reports significance” (p. 298).<sup>10</sup> Parker and Millar (2014) argue that there are some star experimenters or ‘Virtuosos’ (e.g., Helmut Schmidt, Rupert Sheldrake) who do well on their own (i.e., they produce significant results when they self-test), which explains why they produce significant results in studies with participants (under the premise that participants have no psi), whereas less-gifted (‘Psi Challenged’) experimenters who do poorly on their own prove unable to replicate Schmidt’s and Sheldrake’s results (‘So-So’ experimenters are somewhere in between).

To argue that virtuoso solo experimenters must bring their psi to their experiments *because they are virtuosos* is not a valid assumption, as it is based on inductive logic (not straight-forward deduction). After all, I am not entitled to say there must be a black cat on my roof, and I need not check, because looking through my windows, I can see black cats on both my neighbors’ roofs. Granted, there may be *some* ideal scenarios as outlined, but perhaps ‘Virtuosos’ are merely good at encouraging psi in their participants. ‘Psi Challenged’ experimenters, however, may discourage psi in their participants. It is also possible that participants use psi to pick up the ‘vibe’ of their assigned experimenter to give him/her what they want. Despite the seemingly adequate measures to control such overt and covert psychological effects, some-

thing like this may have occurred in the landmark study by West and Fisk (1953), where West was notably psi-inhibitive, and Fisk was notably psi-conductive: West’s participants produced chance results, but Fisk’s produced psi-hitting (see also Wiseman & Schlitz, 1997). As Palmer noted: “each hypothesis can explain the results of the other” (Palmer & Millar, p. 297).

We must also consider intention, which manifests in the sheep-goat effect, as well as outcomes in systems of divination, such as *I Ching* (Storm & Rock, 2014) and Tarot (Blackmore, 1983). The unique divinatory outcomes (as feedback) are pertinent (meaningful) as far as participants are concerned (see Storm & Rock, 2014), but the best Millar would offer is that E-Psi is used to probe individual minds to determine their beliefs and/or mental states, and outcomes are E-psi-adjusted accordingly. But surely, these are virtuoso performances of a capacity far beyond Schmidt and Sheldrake. And because the effects are so obviously synchronistic (not garden-variety psi)—that is, outcomes are *meaningful*—the participant, as an individual, surely deserves all bragging rights if they produce a significant effect. It is perhaps no surprise that common sense prevailed, and Parker (not so much Millar) made a concession, admitting that participants must have their own agency: “if there are psychic experimenters then there have to also be psychic participants” (Parker & Millar, p. 45). So, as a matter of convenience, the problem of E-Psi invariably confines itself to the premise put forth in H2 above, and we have to abandon the idea that experimenters can maintain E-Psi consistently *and* do it with or without participants.

To add to the argument, Palmer and Millar (2015) argue that the “influence of the experimenter” could be found in a “large scale meta-analysis” (p. 298). While it is arguable that the Palmer-and-Millar classifications are transferable from solo-based to participant-based studies, we can agree that meta-analysis may tell us something more substantive about the limits of E-Psi. As it happened, Parker and Millar (2014) had earlier pointed out that the Ganzfeld meta-analytic results (in Storm, Tressoldi, and Di Risio, 2010) are “relatively independent of the experimenter” (p. 48; for other examples, see the next subsection). Those results indicate that at least *some* Ganzfeld psi (arguably most) *must be coming from participants*. I note, however, that the ‘E-Psi isolation problem’ is born of the nature of psi itself as a hypothesized effect that has virtually no limits—theorists can always conceptualize and re-conceptualize psi parameters to meet any theoretical contingency, so we cannot exclude H1 absolutely, but on parsimonious grounds, H2 is preferred.

## The Meta-Analyses: I. Experimenter Psi

Given H2, participants must be regarded as doing most of the psi work, and must account for most of the psi variance, but to a lesser extent E-Psi has variance of its own, which is to say, it too has positive or negative influence, but sometimes fails, and has no influence at all (even Millar would agree about E-Psi variance given his part in forming the above-listed classifications). I reach this conclusion about participant-psi and the limited role of E-Psi on empirical grounds: the meta-analyses I have co-authored (Storm, Sherwood, et al., 2017; Storm & Tressoldi, 2017; Storm et al., 2010, 2012) have found no significant investigator/laboratory differences in the following domains: free response,<sup>11</sup> ganzfeld,<sup>12</sup> forced choice,<sup>13</sup> dream ESP,<sup>14</sup> and sheep-goat effect (SGE). I find the same patterns in other researchers' meta-analyses dating back decades (NB: the following meta-analyses were conducted on databases comprised of many dozens, or scores, or even hundreds of studies):

- Ganzfeld: "... the significance of the overall effect is not dependent on one or two investigators" (Honorton, 1985, p. 61);
- Forced-Choice Precognition: "The effect is clearly not due to a few major contributors" (Honorton & Ferrari, 1989, p. 285);
- Dice Throwing: "the overall effect does not appear to be due to a few exceptional investigators" (Radin & Ferrari, 1991, p. 68);
- Sheep-Goat Effect: "these data provide strong evidence against the view that the results ... are attributable to a few 'lucky' major contributors" (Lawrence, 1993, p. 78);
- Free Response: "... no significant difference in effect size between studies with different principal authors" (Milton, 1997, p. 289);
- Extraversion/ESP Forced Choice: "... the impact of ESP/extraversion testing order is consistent across investigators and is not attributable to idiosyncratic research styles or other characteristics of a single prolific investigator" (Honorton, Ferrari, & Bem, 1998, pp. 264-266);
- Extraversion/ESP Free Response: "the ESP/extraversion relationship is consistent across investigators" (Honorton et al., 1998, p. 267);
- Direct Mental Interaction: "... no significant difference among the mean effect sizes obtained in these labs ["San Antonio, Las Vegas, Edinburgh, and Freiburg"]" (Schmidt, Schneider, Utts, & Walach, 2002, p. 12).

Ignoring my own studies for the moment, the parapsychological community had these facts about experimenters and laboratories before the end of the twentieth century—i.e., quite some time ago.<sup>15</sup> There appeared to be no convincing evidence of long-term E-Psi, yet it seemed that virtually no one noticed. We could argue that these experimenters may mostly be psi-believers, and that is why the meta-analyses produce significant overall effects, which also tend to cluster (hence, no experimenter/lab difference), even though there may be a few psi-non-believers conducting studies. In fact, one comparison (Storm et al., 2012) included the very successful psi-believing experimenter Rupert Sheldrake who was highly praised by Brian Millar (see Palmer & Millar, 2015), but we see that he did not excel (was not an outlier) when compared to other experimenters, and neither was the skeptical experimenter Richard Wiseman an outlier in the opposite (psi-missing) direction.

Most authors of the relevant meta-analytic studies seem rather casual (or quietly confident) and merely reported their findings in a perfunctory way, and only a couple downplayed the concern that had been generated by such critics as Akers (1987). For example, Honorton and Ferrari (1989) made the point that Akers was wrong in claiming that "successful parapsychological outcomes are achieved by only a few investigators" (p. 285). Lawrence (1993) also dismissed Akers's remarks. Overall, to maintain that between-studies E-Psi is a serious impediment to parapsychology is perhaps not the most productive attitude to hold—the meta-analysts should certainly agree. Yet many parapsychologists still appear to be overly concerned by E-Psi, and still express this concern (e.g., Hövelmann, 2015; Palmer & Millar, 2015; Parker & Millar, 2014). Irwin (2014), in his survey of members of the Parapsychological Association, stated:

The possibility that parapsychological effects on experimental psi data may stem from experimenters themselves has been recognized for nearly 30 years. Nevertheless, many contemporary parapsychologists continue to be mindful of our failure to resolve this dilemma and see it as one of the major problems facing parapsychology today. (p. 158)

Pertinent here is the earlier effort taken by Parker and Millar (2014) to identify the source of psi: They profiled some key figures in parapsychology based on psi-conducive style and manner that may have worked favorably on participants to elicit their psi, at least as much as E-Psi, if not more so. Other factors to be noted included laboratory ambiance, motivational factors such



as experimenter attitude, history effects over the duration of testing, prior experiences of success (and failure), and what is even more revealing, the “morphing” effect (an admitted “trickster effect” where the psi effect changes its guise across time and situation; p. 48). Despite this broad-based approach, they demonstrated that controls and manipulations have never been good enough to isolate E-Psi.

While some academic curiosity concerning *within-study* E-Psi may be warranted, parapsychologists should perhaps entertain less tricksterish notions and not be so dismayed by the pseudo-dilemma of *between-studies* E-Psi. Irwin (2014) noted that only one respondent “seemed prepared to accept experimenter-psi effects as intrinsic to the modus operandi of psi” (p. 159)—in the respondent’s own words: “*Experimenter effects are expected if consciousness has nonlocal properties*” (p. 159). We might take that to mean most other respondents are still bothered by E-psi nearly two decades into the 21st century. Of relevance, another respondent said there was “*Not enough consideration of the effects of ... the “Trickster archetype”*” (p. 155). Some degree of awareness might be all we need—just enough to curb our concern and limit our credulity. I must remind readers that the Trickster, as an activated archetype, is a psychological proneness to err in our thinking when a liminal phase is entered into—that borderland between doubt and certainty—that provides the trigger to err.

I suggest the Trickster archetype has been at play over many decades, giving us one big paranormal headache, but it has been found out. We cannot completely bury the phenomenon; it is reasonable to expect it to turn up in any given experiment. That is equally our problem; E-Psi will not go away entirely, but its global effect across studies looks more like a mere phantom than anything else. All that aside, the time spent looking for E-Psi was not wasted—parapsychologists found something worth knowing, and if they can adopt appropriate levels of *warranted* confidence and (not false) certainty commensurate with that finding, they are one step further towards relinquishing the doubt that triggers the Trickster that subsequently leads to the creation of spurious beliefs and findings.

Where the Trickster still rears his ugly head is in the insistence on E-Psi being a ubiquitous effect, occurring both *within* studies and *between* studies. As usual, the skeptics are our nemesis. Wiseman over-generalizes in dismissing E-Psi as the “get out of a null effect free” card (Wiseman, 2009, p. 37) when he should know that the string of meta-analyses cited above yield no evidence of between-studies E-Psi. At the very least, Wiseman should change his argument and tell parapsychologists to con-

fine their argument to within-studies E-Psi only, for Wiseman knows *from personal experience* that within-study E-Psi has been demonstrated (see Wiseman & Schlitz, 1997). Wiseman even managed to have his cake and eat it too, having gainfully used the so-called “Wiseman experimenter effect” (O’Neill, 2001, p. 5) to facilitate a non-significant result (Wiseman, 2001). Reference to later failed attempts to demonstrate E-Psi (as experimenter differences) only furthers my point (Parker & Millar, 2014).

Finally, Alcock (2003) says the psi-experimenter effect is really just “a lack of consistency, a lack of general replicability” (pp. 38-39), so we can take his comments as generally referring to within-study *and* between-studies E-Psi. Like Wiseman, Alcock is wrong on two counts: (a) long-term consistency and replicability *have* been demonstrated in the meta-analyses; and (b) only within-study E-Psi exists (see West & Fisk, 1953; Wiseman & Schlitz, 1997). This refinement on the limits of E-Psi is an issue that will need more focus in the future.

### The Decline Effect

The decline effect, generally speaking, is a complicated multi-factorial affair manifesting in many and varied guises, depending on how the psi effects and the decline effects themselves are defined and how they are tested (for a review, see Colborn, 2007). Nevertheless, the common understanding is that psi goes into decline over a given time period (Colborn, 2007; Kennedy, 2003). Rhine, for example, said:

We destroy the phenomena in the very act of trying to demonstrate them. Evidently, the tests themselves get in the way of the abilities they are designed to measure. (Rhine, 1947, p. 190)

However, Alcock (2003) and Wiseman (2009) both argue that the psi-decline is just another excuse for failure to replicate. Like E-Psi, I find no conclusive evidence for psi-decline in the long run (i.e., *between* studies), though I allow for the possibility in the short term (i.e., *within* studies, including the natural attrition of *recurrent spontaneous psychokinesis*; RSPK; see Roll et al., 2021, pp. 94-98). Colborn (2007, p. 2) refers to these two types of decline as “chronological” and “episodic”, respectively. As will be seen, the argument for declines is complicated by issues to do with study quality, but my meta-analyses tend not to show long-term declines—the distributions are generally flat or even *inclining* in one instance (i.e., forced choice).<sup>16</sup>

Contributors to Wolman’s (1977) classic handbook spent very little time discussing the decline effect—the

word 'decline' is not even in Wolman's Index—and furthermore, although it was somewhat of a talking point amongst Rhineans (Pratt et al., 1940/1966), that was only in the context of within-study declines. Some have made passing reference to the possibility of broad-ranging (but still limited) declines (e.g., Beloff, 1994; Pratt, 1978). Like the between-studies E-Psi, long-term (between-studies) declines seem to have come to our attention in the 1980s and 1990s via the meta-analytic literature but, like the between-studies E-Psi, these meta-analyses gave us early warning that the declines were probably aberrations.

While chronological declines can reasonably be expected (and explained) in single studies (e.g., for the psychological reasons already given), the problem with declines spanning many years or decades is that we need some kind of historical or methodological change to explain it. After all, it could be argued that when we find such declines, it possibly indicates *improvements* in study quality over the years (i.e., psi effects are artifacts of poor experimental design), and psi effects would diminish or vanish with the flaws. As will be seen shortly, there is no support for that hypothesis. The other possible reason was given above—shifts in experimental designs to complex and tedious tasks that demotivate participants and thus lower psi responses (for the Ganzfeld anyway; see Radin, 2006). Demotivation would not be the case in earlier studies, but nobody has tested this claim, and Bierman (2001) refutes its likelihood across a number of paradigms anyway. Bem, Palmer, and Broughton (2001) presented a similar argument for the Ganzfeld—deviations from the standard protocol tended to result in failure, but they did not stipulate what is inherent in the deviations that inhibit psi (e.g., what is so offensive about a musical target compared to a visual target?). Thus, we never did find convincing explanations for these alleged declines, yet continued to believe in them based on well-meaning articles that highlighted the effects (Bierman, 2001; Colborn, 2007; Kennedy, 2003).

## The Meta-Analyses: II. Declines and Inclines

As it happens, the earlier meta-analyses (forced choice [precognition only], free response, dice throwing) give us some evidence to dispute the between studies (chronological) DE in the form of an array of correlations that are easily interpreted. In those same meta-analyses from the 1980s and 1990s referred to above, we find that methodological quality *improves* significantly over time (using publication date as the time measure),<sup>17</sup> While effect sizes are *constant*<sup>18</sup> over time (Honorton & Ferrari, 1989; Milton, 1997; Radin & Ferrari, 1991).

Alternatively, in other earlier meta-analyses (extra-

version-ESP, sheep-goat effect), when methodological quality is *constant* over time, effect size is either *constant* over time (Honorton et al., 1998; Lawrence, 1993) or in *decline* (Honorton et al., 1998), although that one decline refers to extraversion/ESP forced-choice studies only (not extraversion/ESP free response), and the authors attribute the decline to an artifact due to administration of the extraversion measure *before* the ESP task in earlier studies. In these cases, we could have an artifact caused by poor quality, but there is little to go on.

Furthermore, in these and other meta-analyses, the relationship between study quality and effect size was also investigated (a negative relationship would also suggest that psi effects are artifacts). Overwhelmingly, the relationships were not significant (Honorton, 1985; Honorton & Ferrari, 1989; Lawrence, 1993; Milton, 1997; Radin & Ferrari, 1991;<sup>19</sup> Radin & Nelson, 1989; Schmidt et al., 2004<sup>20</sup>). Again, no evidence of artifacts.<sup>21</sup>

For the Ganzfeld domain, significant declines over time have been observed (see Bierman, 2001; Milton & Wiseman, 1999), but ostensible declines<sup>22</sup> existed long before Hyman and Honorton (1986) implemented guidelines that focussed on qualitatively improving the Ganzfeld methodology, so it cannot be assumed that study quality was single-handedly responsible for those declines. As Palmer (1986) pointed out, it should not be assumed that declines result from removal of flaws, just as it is presumptuous to claim that past Ganzfeld successes were due to the presence of flaws. Furthermore, Bierman (2001) does not present an overwhelming case for declines, as he also found a so-called rebound effect (i.e., U-shaped curve) in the Ganzfeld effect-size distribution, suggesting a "recovery from the decline effect" (p. 5), and he found a rebound effect in the RNG domain, but no proof either way for a decline in the Remote Viewing domain (see also Utts, 1996).

Taken together, the above meta-analyses do not provide conclusive evidence for chronological DEs, and the argument for quality-based DEs is simply not convincing. This is a subtle issue, and it is easy to overlook or just brush aside as frivolous, but we have to realize that it becomes more and more apparent, as we dig deeper, that a major ongoing (many decades long) state-of-play in parapsychology has been this phantom of chronological declines. Critically, parapsychologists had the evidence to hand *at the time* to dismiss the effect outright (just as they could in regard to E-Psi).

While the occasional commentator has not been convinced by the claims for chronological DEs (Utts, 1996; Walach et al., 2002), the effects seem to have been taken at face value. Thus, many parapsychologists and skeptics spoke of chronological declines in real terms. Bierman

(2001) states:

... it soon became clear that apart from sequential effects within a run of trials, it was quite difficult to exactly 'replicate' ... the results of original studies: a sequence of similar studies tended to show a decline of effect size from study to study. (p. 3)

It is a peculiar response from Bierman, given he found those rebound effects. Bierman even saw the need for a "radical change in world view" to explain the failure to replicate. Perhaps parapsychologists need not go so far just yet—not until they know what it is they are supposed to be explaining. Yet other parapsychologists have followed Bierman's lead and taken the effect seriously:

- Stokes (2007) declared: "... most investigators are not able to obtain reliable and replicable experimental evidence for psi ..." (p. 80). He puts the initial significant findings down to fraud (more on that shortly);
- In regard to the Ganzfeld studies, Radin (2006) rightly disputed the skeptical claim that the "decline [over time] is due to improving methodologies" (p. 122). However, he does attribute the decline to "changes in experimental goals" (p. 122)—a worthy rationalization possibly or partially befitting the facts, but not really called for at the time if the databases (i.e., the meta-analyses) had been examined more thoroughly. And evidence of declines in other disciplines does not get us off the hook (assuming there is a 'hook')—*two wrongs don't make a right*;
- Kennedy (2003) also spoke of "widespread declines in psi effects" (p. 58), concluding that "psi effects are unsustainable" (p. 69);
- Despite Colborn's (2007) own warning not to reify declines out of credulity, he still reviewed and considered the likelihood of a range of empirically related causal factors for declines (I am particularly focussing on chronological declines here). For theoretical reasons, that is not necessarily inappropriate, but it implies a problem that was poorly substantiated in the first place. Colborn does, however, list "counter-advocates" who also do not believe in long-term declines (e.g., Walach et al., 2002).

Have we reached that point where we should drop the whole idea of chronological DEs as we find more and more evidence justifying that move? For example, simi-

lar patterns to those described above emerged in later Ganzfeld and forced-choice meta-analyses, in an RNG meta-analysis (Bösch, Steinkamp, & Boller, 2006), and in a meta-analysis by Mossbridge, Tressoldi, and Utts (2012) on presentiment. (Note that *ES* = effect size in the list below):

- Ganzfeld: there is "a linear decline in [effect-size strength] over a 34-year period ... [but] a rebound effect is also indicated" (Storm et al., 2010, p. 478);
- Ganzfeld: "... the correlation between mean quality scores and *ES* values ... was extremely weak and not significant" (Storm et al., 2010, p. 475);
- Forced Choice: "We note that the correlation between year of study and *ES* is positive and significant for the database ... indicating an incline, meaning that *ES* values increased over the 24-year period" (Storm et al., 2012, p. 257);
- Forced Choice: "The correlation between mean Quality Scores and *ES* values ... was very weak, negative, and not significant, ..., suggesting that effect size was not an artifact of poor experimental design" (Storm et al., 2012, pp. 251-252);
- RNG: "... the quality of studies was high" (Bösch et al., 2006, p. 507); and "study quality was also positively correlated with year of publication" (p. 508);
- Presentiment: "the higher-quality studies produced a higher overall *ES* and level of significance than the lower-quality studies" (Mossbridge et al., 2012, p. 7), whereas the correlation "between quality score and *ES* was not significant" (p. 8).

In spite of these new findings, coupled with the earlier findings, explanations for DEs still persist, resurrected in the form of a slightly modified argument from fraud—"fraudulent experimenters" in the early stages of research are replaced by newer "nonfraudulent experimenters" at a later date (Stokes, 2015, p. 47). Hence the decline. In other words, Stokes was still arguing the case for fraud, as he did back in 2007. Yet he knew well enough that

Many meta-analyses have found no evidence for a positive relation between methodological flaws and the size of reported psi effects. (p. 44)

This problem is simply explained away as more fraud!

... people who report fraudulent results may also report more perfect methodology than was actually used (assuming that the experiment was even conducted). (p. 44)

Unsubstantiated statements like this prove beyond a doubt that the Trickster knows no bounds. Again, the mud-slinging only starts when doubt and suspicion arise: “My newfound skepticism regarding the existence of psi is not based on any new concrete evidence for fraud among parapsychologists [emphasis added], but rather on a general lowering of my respect for all scientists” (Stokes, 2015, p. 45). What are we to make of a science where empirical evidence can be trumped by opinion?

Contemporaneous contrary findings from other researchers may help steer a reasonable course for parapsychology (Baptista, Derakhshani, & Tressoldi, 2015), while more recent meta-analytic evidence gives even more justification to dismiss the DE as a *chronological* (not *episodic*) problem.<sup>23</sup>

- Ganzfeld update: “There was no statistical evidence for an effect-size decline in the ganzfeld domain ...” (Storm & Tressoldi, 2020, p. 213);
- Ganzfeld update: “The correlation between mean Quality and *ES* was weak and not significant ... so we claim that effect size is not likely to be an artifact of poor experimental design” (Storm & Tressoldi, 2020, p. 200);
- Remote Viewing: “Looking at the *ES*s ... we can affirm that there is no sign of decline in almost 40 years” (Baptista, Derakhshani, & Tressoldi, 2015, p. 203);
- Sheep-Goat Effect (SGE): “The relationship between the sheep-goat CR(*ES*) [critical ratio effect size] values and year of publication is positive and significant.... In other words, the SGE has increased across the span of about 20 years ...” (Storm & Tressoldi, 2017, pp. 93-94);
- Sheep-Goat Effect (ESP): “ESP effects [over the years] generally have not increased significantly [in a two-tailed test, but the ESP effect is marginally significant in a one-tailed test]” (Storm & Tressoldi, 2017, pp. 93-94);
- Sheep-Goat Effect (SGE & ESP): Storm and Tressoldi (2017) state, “in both cases [SGE and ESP] ... increases in effects have been independent of study quality” (p. 94);
- Dream-ESP: Storm, Sherwood, et al. (2017) report “no significant decline in *ES* related to quality, ..., but quality control in experiments has

improved over the 49-year period” (p. 130).

We actually see an *incline* effect for the SGE, while the ESP effect is stable (Storm & Tressoldi, 2017). Regarding the dream-ESP meta-analysis, Storm, Sherwood, et al. (2017) did find that the correlation between year of study and effect size was negative and significant, but they also found suggestive evidence that “illustrates the complete opposite of the sceptical hypothesis that improvements in quality necessarily mean *ES* must plummet” (p. 130). Although it was found that two dream-ESP databases (Maimonides Dream Laboratory [MDL] studies and post-MDL studies) were not significantly different from each other in terms of mean effect size, the difference did approach significance. Though not reported in the paper (Storm, Sherwood, et al.), I tested both databases separately for effect size declines over their respective periods, and neither DE was significant.<sup>24</sup>

### Ramifications of Experimenter Psi and Decline Effect

We now come to the ramifications of these findings in the context of the Trickster, but first, here is how the Trickster has manifested itself: On the evidence, the parapsychology community argued the case for the experimenter psi effect (E-Psi)—it argued a similar case for chronological DEs. This went on for decades. Most parapsychologists accepted the ‘evidence’ for both effects based on single studies, or small sets of studies, and then larger sets. The propositions seemed valid, reputations were at stake, time and money had been invested; reasonable explanations had to be proffered to the wider community to save face. The mainstream (largely skeptical) community took and ran with the parapsychologists’ arguments and, *in their opinion*, saw them as mere excuses used to explain the failure to reject the so-called Null hypothesis (i.e., the hypothesis of no effect). That gave cause for embarrassment, even ridicule (trickster outcomes). True to Trickster form, however, there is a twist—perhaps parapsychologists spoke too soon: E-Psi and DEs do seem to occur in a number of studies, but these apply mainly to short-term (within-study) effects, whereas no (or debatably too few) long-term (between-studies) E-Psi and DEs are demonstrated in the various meta-analyses comprising many studies that are considered representative of the respective experimental domains (i.e., free response, forced choice, etc.). Given the evidence, chronological manifestations of the effects appear to be unfounded, but episodic effects look genuine (the evidence persists).

It is important to note that my thesis is not that the Trickster lies behind E-psi and DE effects in the sense that it actually causes them. These two effects could occur,

but it would not be the Trickster that caused them if they did occur, but if they did not occur, it would not be the Trickster that prevented them. Instead, I have asked (and answered) the question, Was there evidence for these effects, and if there wasn't, and it was thought there was, what is behind that mistake?

If we were somewhat myopic, we could argue that the parapsychological community might have spared itself some embarrassment if it had waited a little longer, but how long is a 'little longer'? Science is an accumulative enterprise, and scientists make discoveries and present them in fits and starts; the point being science is an ongoing process of continual revelation. Truth be told, parapsychologists, singly or as a community, simply did not know better and can only work with the best data available at the time. So it is easy to argue that hindsight has 20/20 vision, whereas science does not. But that is to ignore the earlier clues. The literature should have helped ameliorate the negative 'exposure' caused by E-Psi and DEs, and perhaps both issues could or should have been downplayed given that there was not a lot to go on—indeed, some investigators did that.

Interestingly, Radin (2006) gives an appropriate metaphor that conveys more than is first realized. He refers to the Pygmalion effect, which he parallels with the experimenter expectancy effect (i.e., E-Psi). The Pygmalion effect owes its origins to the myth of a Greek sculptor Pygmalion, who fell in love with a statue he carved from ivory and subsequently named Galatea. Aphrodite, the Goddess of Love, rewarded his fervor by bringing the statue to life—as Radin says: “This myth reflects the concept of a self-fulfilling prophecy” (p. 285); a classic cautionary tale warning us to be careful what we wish for (although Pygmalion seems to have done well out of it for the couple lived happily ever after).

While Radin directly acknowledges the point that sufficiently motivated experimenters can use their own good manner and/or psi in the laboratory, the less direct point is that the myth also warns researchers not to make too much of anything they *think* they find because that gives it reality—parapsychologists have to be careful they do not talk an effect into existence; and this *caveat* applies to DEs, not just E-Psi. Indeed, Colborn (2007) makes the point about the decline effect: “we might call it into existence via reification” (p. 3). I would argue that parapsychologists have made too much of these long-term effects—they appealed to them too often (again, I emphasize I am not discounting *within* study declines). I give the final word to physicist and parapsychologist Roger Nelson, who said my findings are matched by his experiences exactly,

... in the lab, in meta-analysis, and in long-term programs of research. EE [i.e., E-Psi] explanations are too easy and need (as we often say) prospective research to really establish bona fides. (personal communication, August 21, 2017)

And the same can be said of decline effects.

### Overcoming the Trickster

So, how does modern parapsychology resolve the Trickster dilemma? Thus far, I believe I have rendered aid just in identifying the types and situations of which one should be wary, and thus hinted at ways of dealing with the Trickster. But parapsychologists must become more conscious of their aims, their behaviors and methodologies, the way they interpret their findings, and the social ambiance in which they are immersed. This is not to imply that a state of paranoia be adopted—Palmer (2016) warned that excessive policing of parapsychological research was enough to make one “paranoid” (p. 14)—but a healthy and vigilant attitude about the movements and changes within the social sphere is a must. An attitude like that can only help.

Will keeping a low profile also help? Most parapsychologists who happen to be academics find themselves braving the worst on occasion (see again, Thalbourne, 1995), and nothing is more inimical to the parapsychologist's ventures than exposure to judgmental, disapproving, conservative, and naïve authority figures. Those figures will invariably seek the aid of mass media to garner support from that subpopulation of the general public that laps up scandal. But persistence can win the day (for example, see Rupert Sheldrake's successful struggle with TED and YouTube described in Weiler, 2020).<sup>25</sup>

Parapsychology will continue to have its ups and downs, and parapsychologists simply have to ride them out. That 'ride' is part of the big picture view of science which incorporates the human aspect—that means more than how scientists deal with unpalatable glitches in their findings, for it goes deep into the psyche to the archetypal core of our being. As an archetype, the Trickster shows how our minds meld and interact with the world, social and otherwise, and call into existence the various anomalies, whether we intend them or not.

### CONCLUSION

Trickster phenomenology permeates deeply into the sciences. It is not a simple matter of dismissing the Trickster as mere superstition and calling out its anomalous effects as the result of delusional states or chance. Certainly, supernatural agency is not proposed as a cause though

some phenomena are so bizarre it is hard to resist. However, in the modern era, we have learned we must take responsibility for our psychological aberrations, but we still often fail to see that they are our own, and we thus dismiss them as attributable to 'other' (external) sources. Renaming the Trickster does not rid us of it. The Trickster (whatever it really is) not only manifests acausally in the laboratory (as Pauli may have discovered), and in how sets of data are collected and collated (as Jung may have realized), it has a unique way of clouding the issue and rendering effects that can confuse and lead to false understanding, as is the case in parapsychology—specifically in relation to E-Psi and chronological decline effects. We must be careful we do not over-reach, even when the evidence looks conclusive, but what is to be said when evidence is ignored or not seen for what it is? Avoiding these pitfalls successfully calls for constant vigilance based on a clearer understanding of the human factors in psi research. Researchers make a virtue of their conservatism (implying they take a cautious approach), but that does not entirely rid them of their preferences, prejudices, cherished hypotheses, and entrenched notions and beliefs. This paper suggests that interpretations and positions can be hard to shift despite the findings.

## ENDNOTES

- <sup>1</sup> The sense of security is false because of the unresolved prejudice faced by parapsychologists, especially in academia. The scene is changing for the better in some parts of the world (noticeably Europe, especially the UK), but I (writing in Australia) have personally experienced institutional career-threatening prejudice (twice, in fact), and I know a number of parapsychologists overseas who have similar stories to tell. This prejudice extends beyond centers of learning to the media and mainstream society.
- <sup>2</sup> Jung (1959/1968) equates the Trickster with the alchemical Mercurius, which is the equivalent of the Greek god Hermes (see Combs & Holland, 1996). The mythological parallels are too complex and interwoven to go into detail here.
- <sup>3</sup> One reviewer stated that Social Identity Theory, or theorists like Marx, Durkheim, and Weber, have explained how these kinds of conflict can occur, but they do not appeal to Trickster activation. What they do appeal to is some kind of causal factor which, at its root, I suggest must be archetypal (fundamental) in human nature. Also, these theories/theorists do not speak to psi (unless from a skeptical standpoint).
- <sup>4</sup> Pauli's Exclusion Principle (the first one for which he won the Nobel Prize) is so-called because matter is

explained as occupying its own space to the exclusion of other material objects, such that one cannot pass through the other. Light and radiation are given exclusive exception to this rule. This principle is fundamental to our understanding of the nature of matter, for example, in its formal presentation in the periodic table of elements.

- <sup>5</sup> In the original Cross-Correspondences work (see Saltmarsh, 1938/1975, for a review), members of the Society for Psychical Research found correspondences hidden within messages from different mediumistic communications. The messages in these communications were singly meaningless and only seemed to make sense once the contents were cross referenced. The findings were considered evidence for survival, though others argued that ESP would explain them.
- <sup>6</sup> Eisenbud (1963) is probably one of the earliest researchers to bring E-Psi to the attention of parapsychologists.
- <sup>7</sup> A survey of members of the Parapsychological Association (Irwin, 2014) reported that many parapsychologists regard experimenter psi as a major problem for parapsychologists, and many refer to the need to disentangle participant and experimenter effects.
- <sup>8</sup> Between-studies DEs might also be called "*chronological declines*" (CDs; Irwin, 1999, cited in Colborn, 2007, p. 2), but I will not use that term because CDs can be either across experimental paradigms (e.g., Ganzfeld), or over the lifetime of an individual, and I only want to focus on the paradigms. CDs contrast with "*episodic declines*" (EDs), which are "within an experimental run or within a session" (Colborn, 2007, p. 2)—I refer to these as within-study DEs.
- <sup>9</sup> As I am focusing on E-Psi, I will not be discussing the third (unlikely) option, that experimenters do not have E-Psi, and all effects come from participants only.
- <sup>10</sup> Palmer and Millar (2015) do not dwell too much on participant-psi. It appears they regard participants' psi as constant (and inconsequential?), and prevalent within and between studies. It also appears they take "significance" to mean psi-hitting since they state that parapsychologists are "rigorously" selected (even self-selected) "for success in their experiments" (p. 298), and success usually means significant psi-hitting.
- <sup>11</sup> Free response is a term that "describes any test of ESP in which the range of possible targets is relatively unlimited and is unknown to the percipient [perceiver/receiver]" (Thalbourne, 2003, p. 44).
- <sup>12</sup> The Ganzfeld is a "special type of environment (or the technique for producing it) consisting of homogenous, unpatterned sensory stimulation" to the eyes and ears of the participant, who is usually in "a state of bodily comfort" (Thalbourne, 2003, p. 45).

- <sup>13</sup> The forced-choice design is so named because the target guess is “one of a limited range of possibilities which are known to [the participant] in advance” (Thalbourne, 2003, p. 44).
- <sup>14</sup> Dream ESP involves ostensibly paranormal communication while in an altered state of consciousness, commonly known as dreaming. Pioneer research into the telepathic nature of dreams was first conducted at the Maimonides Dream Laboratory in New York during the 1960s and 1970s
- <sup>15</sup> The Schmidt et al. (2004) study was, in fact, started well before 2002 (an abstract was published that year and results presented at a PA Convention). They used a database of studies up to the year 2000, so I include Schmidt et al. as indicative of DMILS research around the turn of the century.
- <sup>16</sup> We can make the argument that E-Psi is responsible for these declines—experimenters (like participants) tire and lose motivation, focus, and interest, as they get older. We might also argue that aging ‘star’ experimenters eventually retire, and perhaps they are not replaced by younger ‘star’ experimenters. Then we could expect DEs. However, since I find no convincing evidence of long-term declines in the meta-analyses, all experimenters (assuming E-Psi) must, *on average* and *on occasion*, be putting in equal amounts of their own psi across the decades (a pretty tall order), or they put in none at all.
- <sup>17</sup> Note that Schmidt et al. (2004) did not look at quality over time. (Footnote 15 explains this study’s inclusion).
- <sup>18</sup> Schmidt et al. (2004) may loosely be considered an exception; they report two significant declines over time (DMILS and Remote Staring [RS]). However, date of study (year of publication) dropped out in a regression analysis (DMILS), and for RS, it was shown that *N* (sample size) correlated significantly with date—thus, the earlier smaller studies tended to “overestimate the true effect size” (p. 13); besides which, there were only 15 studies in the sample.
- <sup>19</sup> At first, Radin and Ferrari (1991) found that quality correlated negatively and significantly with effect size, suggesting that design flaws present in low quality studies were contributing to the success of earlier experiments, but further analysis of a homogeneous subset of data (i.e., outliers were removed) revealed no suggestive evidence of decline in psi effect due to poor quality.
- <sup>20</sup> For DMILS, I used the revised non-significant “overall study quality and effect size” correlation (Schmidt et al., 2004, p. 244). For Remote Staring, the effect-size/quality correlation was positive and non-significant.
- <sup>21</sup> In some studies, we cannot draw any strong conclusions because findings were limited—that is, patterns over time were not checked for quality (Schmidt et al., 2004) or were not checked for both quality *and* effect size (Honorton et al., 1998).
- <sup>22</sup> Surprisingly, Honorton (1985) did not test the correlation between effect size and year of study—if he did, he would have found a weak negative (albeit crucially non-significant) relationship for his 28-study database,  $r(26) = -0.20, p = .155$  (one-tailed).
- <sup>23</sup> Declines over time have not been reported in the more recent RNG meta-analyses (Bösch, Steinkamp, & Boller, 2006; Radin & Nelson, 2003).
- <sup>24</sup> MDL studies:  $r(12) = -0.02, p = .943$  (two-tailed); post-MDL studies:  $r(34) = -0.20, p = .238$  (two-tailed).
- <sup>25</sup> TED (‘Technology, Entertainment, Design’) is an organization that engineers public speaking events for anyone with something to say.

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