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BOOK REVIEWS

SSE NEWS

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EDITORIAL

Farewell Missives

Stephen E. Braude

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This is a particularly rich issue of the *JSE*. And a hefty one. Its size is due primarily to two quite lengthy essays, one by Bryan Williams and one by Michael Sudduth. Of course, all of this issue's articles and reviews are worth reading; that's why we're publishing them. But these two huge essays merit a few extra comments.

Bryan Williams has given us something that I and various SSE members have hoped for over the years, a detailed review of a specific line of anomalistic research—the kind of article that would be useful to both veterans and newcomers to edge science. I've often tried to recruit such an opus from SSE colleagues at our conferences, hoping my considerable charm would dazzle them into accepting the opportunity. I've even been assured on several occasions that the solicited reviews would be forthcoming. But only Bryan, so far, has delivered the goods, a splendid essay surveying research on PK with random number generators. Not surprisingly, this review took Bryan a long time to write, and I want to thank him, not only for the result, but for his tenacity.

Michael Sudduth's essay is a forensic tour de force (as befits an admirer of the TV detective *Columbo*)—an unprecedently detailed critique of the James Leininger case of ostensible reincarnation. That case is both complicated and messy, and it illustrates a general problem with CORT investigations that I've dubbed the *Problem of Investigative Intricacy*.

All CORT cases are messy. Investigators must interview the subject, family members, and (when possible) crucial figures in the life of the previous personality. In fact, it typically requires considerable detective and interpretive work merely to identify the previous personality from the often vague or ambiguous behaviors and statements of the subject. And then, investigators must still obtain testimony from the remaining living persons (if any) who knew the apparent previous personality, simply to establish that the subject's apparent recollections about the previous personality are reliable. Moreover, the interview process can be muddied by the fallibility of memory, and by conscious or subconscious motives either to please (or frustrate) the interlocutor or simply to confirm a deep wish for the case to be a genuine instance of reincarnation. And of course, many cases also require the services of translators whose own biases, inadequacies, and needs might influence the direction or accuracy of the testimony obtained. So a great deal of work is required to obtain clear and reliable early-bird testimony (i.e., gathered before the apparent previous personality was identified), establish a strong link between that testimony (or the subject's behavior) and a previous personality, and to ensure the accuracy of the information obtained from interviews. In short, CORT investigations require a great deal of work simply to establish the reliable and relevant facts of the case.

However, other bodies of evidence don't require such a complex process either to identify the deceased or to establish a clear link between the living and the deceased. For example, in the case of the medium Mrs. Piper, we have many interactions between the medium and sitters who knew the deceased well. So when Mrs. Piper channeled a message having intimate relevance to the sitter (e.g., an affectionate and private nickname), we know immediately who the deceased seems to be. And arguably, transplant cases provide even clearer connections to the apparent deceased.

Sudduth's examination of the Leininger case is a breathtaking illustration of how difficult it is to properly evaluate evidence suggesting reincarnation. I imagine few of us have the fortitude or investigative skill for such a task. Indeed, I don't believe any ostensible survival case has ever been subjected to such a thorough examination. I should also note that Sudduth and Jim Tucker will have more to say about the Leininger case in the Spring issue.

....

Before signing off permanently from my bully pulpit here at the *JSE* and passing the baton to James Houran, I would like to say a few words about Carlos Alvarado, whose recent and untimely death comes as a great loss to the *JSE* specifically and psi research generally. Alfonso Taboas has paid a splendid general tribute to Carlos in this issue, and I'd like to add that Carlos had a considerable influence on the *JSE* during my tenure as Editor-in-Chief. It was Carlos who pushed initially to establish an Historical Perspectives section to the *Journal*, and that innovation seems clearly to have been a success. In fact, many of the historical papers we've published are also our most frequently downloaded. Moreover, Carlos was an effective and pro-active Associate Editor as well as a frequent contributor to the *Journal*. He was also quick to inform me about new books deserving to be reviewed in our pages. Without question, he made my job easier, and I mourn the loss of a great colleague, good friend, and wonderful person.

Now that I'm preparing to enter that purgatory reserved for retiring Editors, I must also acknowledge the other members of my editorial team who've contributed so much to maintaining the scientific and scholarly integrity of the *JSE*. Because the *JSE* considers more areas of anomalistics than any one editor can hope to master, I've often had to rely on Associate Editors whose areas of specialization and competence differ from mine. Fortunately, my AEs have been remarkably supportive and helpful, and I'll no doubt miss our frequent exchanges. During my 13 years as Editor-in-Chief, my team has undergone inevitable changes in personnel, too many to list here. At any rate, I want to remind readers that our AEs are vital actors behind the scenes. I'd like, therefore, to acknowledge the following intrepid souls who've (at one time or another during my tenure) served as Associate Editors: Carlos Alvarado, Imants Barušs, Daryl Bem, Robert Bobrow, Etzel Cardeña, Courtney Brown, Jeremy Drake, Renaud Evrard, Hartmut Grote, Bernard Haisch, Michael Ibison, John Ives, Julia Mossbridge, Roger Nelson, Dean Radin, Mark Rodeghier, Daniel Sheehan, Paul Smith, James Spottiswoode, Michael Sudduth, and Harald Walach.

And finally, I must once again express my appreciation for Managing Editor Kathleen Erickson, whose mastery of the mechanics of publishing is awe-inspiring, and who (as I've noted previously) manages the seemingly impossible task of preserving the illusion of my editorial competence. Kathleen has been a delight to work with, and I know that my Associate Editors and contributors to the *Journal* agree wholeheartedly.

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RESEARCH ARTICLE

Archaeological Artifacts from the Stone Age Solve the Mystery of the First Native Americans

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Abstract—Many scholars have sought to uncover the mysteries relating to the origins of the earliest Native Americans. Among many hypotheses, the Beringian Standstill Hypothesis and the Kelp Highway Hypothesis claim that the first Native Americans stayed in Beringia during the Last Glacial Maximum, before migrating to the New World by boat as the climate warmed. Unfortunately, neither hypothesis was devised with tangible supporting evidence, such as archaeological artifacts; as such, both remain controversial. To address this issue, this paper compares and analyzes the genetic data related to the first Native Americans and the data of six archaeological artifacts from the Stone Age. The results show that both hypotheses are correct, but that the first Native Americans stayed in Southern Korea rather than in Beringia.

INTRODUCTION

The mystery of the origin and diffusion of the first Native Americans (FNAs) continues to vex archaeologists, geneticists, and anthropologists. MtDNA—genetic material inherited from the maternal line—is an important tool for disentangling the origin and the numerous waves of migration of Native Americans (Nesheva, 2014). All FNA mtDNAs can be traced back to five haplogroups: A, B, C, D, and X (Mizuno et al., 2014). Our study focuses on the appearance of the sub-haplogroup X, which is genetically related to Europeans (Derenko et al., 2001), and the discovery of Australo-Melanesian DNA in several contemporary Native American groups, including those of the Aleutian Islands and the Surui people of Amazonian Brazil (Balter, 2015). This focus area was chosen to clarify why and how the genetic code of Native Americans contains both European and Australo-Melanesian DNA. While investigating this complex issue, Tamm et al. analyzed 623 complete mitochondrial DNA samples (mtDNAs) from the Americas and Asia, including 20 new ones from the Americas and seven from Asia (Tamm et al., 2007). Based on their results, the geneticists claimed that FNAs had occupied northeast Asia before becoming genetically isolated in Beringia during the Last Glacial Maximum (LGM) approximately 25,000 to 15,000 years ago. This argument is known as the Beringian Standstill Hypothesis (BSH) or Beringian Incubation Model (Tamm et al., 2007). This hypothesis is strengthened by accumulated evidence from various fields, including genetics (particularly the analysis of nuclear DNA, including Y chromosomes), physical anthropology, linguistics, and paleontology (Hoffecker et al., 2014).

Nevertheless, the BSH argument remains in question since no archaeological artifacts to confirm its hypothesis have been found. It is essential to find archaeological evidence to determine the true origins of FNAs, and to eliminate conflicting theories. The Kelp Highway Hypothesis (KHH), for instance, claims that FNAs moved not by land, but by sea, following the coastline from northeastern Asia and Beringia to as far south as Monte Verde in Chile, approximately 16,000 to 13,000 years ago (Braje et al., 2017; Wade, 2017). While a number of archaeologists have tried to find archaeological artifacts to support the BSH and KHH, rising sea levels since the LGM have, unfortunately, prevented them from discovering anything (Hoffecker et al., 2014; Braje et al., 2017; Wade, 2017). Consequently, other questions arise: What archaeological artifacts are archaeologists looking for to prove their claim? How accurate are their ideas and methods in their search of archaeological artifacts? In the search for explanations, existing literature was referenced to collect genetic data of skeletons related to the origins of FNAs. The six archaeological artifacts found near the skeletons are representative of the Stone Age and were selected to be used as study materials in this research. The sites where the artifacts and genetic data were found were visited. Consequently, all the archaeological artifacts and genetic data were compared, connected, and mapped. The results were nearly consistent with the places where Native American mtDNA appeared and the six archaeological artifacts were found. Additionally, it shows that FNAs of various races inhabited Southern Korea and Southern Japan, instead of Beringia, during the LGM (Potter et al., 2018; Alves et al., 2016). During this period, genetic mixing occurred (Turner, 1990; Jin et al., 2009; Achilli et al., 2013), and as the climate warmed and glaciers retreated, parts of these populations migrated to the New World as KHH claims (Braje et al., 2017; Wade, 2017). This means that studying both existing genetic data and the six archaeological artifacts found in proximity is critical to solving FNA-related mysteries.

MATERIALS AND METHODS

Selected Background of Six Archaeological Materials

Early studies on Native American mtDNA variation have demonstrated that all Native American mtDNA belong to haplogroups A, B, C, D, and X. Among these haplogroups, A, B, C, and D appear in Southern Siberia and adjacent areas of Mongolia (Starikovskaya, 2005; Derenko et al., 2000), while traces of haplogroup X appear in Europe, Western and Central Asia, Altai in Siberia, and North Africa (Derekno et al., 2001; Starikovskaya, 2005; Reidla et al., 2003).

MA-1 and Anzick-1 are used as genetic evidence to determine FNA mtDNA distribution. Geneticists mapped the genome (MA-1) of a four-year-old boy who died in Mal'ta, central Siberia, dated to approximately 24,000 years ago (Raghavan et al., 2014). The Y chromosome of MA-1 has frequently been found in hunter-gatherers of the Paleolithic and European Mesolithic periods (Bramanti et al., 2009), is basal to present-day western Eurasians, and is genetically closely related to contemporary Native Americans (Raghavan et al., 2014; Lell et al., 2002). In Montana, a construction worker discovered more than 100 stone and bone tools, and a young child's skull from about 12,700 years ago (Rasmussen et al., 2014). The child, called Anzick-1, is genetically similar to MA-1 (Jin et al., 2009) and European Mesolithic genomes. However, this boy has no clear affinities with Chinese, Korean, Japanese, or other East Asian populations (Raghavan et al., 2014). In addition, a *Science* and *Nature* genetic research team detected a trace of DNA in modern Native Americans related to indigenous groups from Australia and Melanesia (Balter, 2015), raising questions about why and how these traces exist.

In 2011, geneticist Lee and his team analyzed Neolithic human bones discovered on Gadeok Island, near Busan, South Korea. Estimated to be approximately 7,000 years old, the remains had been damaged by oxidization of acidic soil, resulting in the researchers analyzing just 17 of the 48 bones (Shin, 2014). Nonetheless, they discovered that some of the remains had Asian DNA, and some had H-type European mitochondrial DNA (Shin, 2014). Moreover, as with the DNA of MA-1 and Haplogroup X, this H-type maternal DNA was not detected in current residents of South Korea and Japan (Raghavan et al., 2014; Shin, 2014). Certainly, European skeletons of the Neolithic Period and Bronze Age have been discovered in South Korea, including those under dolmen No. 13 in Hwangseok-ri, Jecheon, Chungcheongbuk-do, in 1962 (Kim, 2010), and a dolmen in Auraji, Jeongseon-gun, Gangwon-do in 2005 (Lee, 2008).

At this point, various questions arise: Why are MA-1, Anzick-1, and Gadeok Island Man's mtDNA not currently found in Koreans, Japanese, or other East Asians (Raghavan et al., 2014; Kim, 2010)? Why are the mtDNA in the three regions associated with Europeans? To address these questions, this study investigated and compared archaeological artifacts, such as the Venus figurines found around MA-1; Clovis points found around Anzick-1; and artificial cranial deformations, dolmens, and comb-pattern pottery found around Gadeok Island. During the investigation of these archaeological artifacts, each object's shape was scrutinized against existing literature. In addition, the area where the artifacts were found and where mtDNA haplogroups X and H appeared were mapped and comparatively analyzed. Archaeological data were collected from November 2012 to January 2021, through direct observation of original artifacts. Known sites of findings related to FNA genes, in Europe, Turkey, Egypt, Northeast Asia, North and South America, the Pacific Islands, and Indonesia, were visited during this process. Museums, archaeological sites, and antique shops served as the primary locations of archaeological data collection. Archaeological data that could not be obtained by direct visits, was obtained through literature and data from more than five million Google images. Finally, the collected genetic and archaeological information was synthesized to create a new hypothesis: Both BSH and KHH are correct, but FNAs stayed in South Korea and southern Japan instead of Beringia. Existing paleontological and new archaeological data were analyzed to reaffirm this hypothesis.

Archaeological Artifacts Found in the Americas

Acheulean hand axes (Wynn & Gowlett, 2018), one of the oldest relics of the Paleolithic period, have primarily been found in Africa, Europe (Figure 1I), Western Asia, and the Indian Subcontinent, with a few scattered examples discovered in East Asia (Hou et al., 2000). In 1978, Greg L. Bowen, an American soldier, discovered a hand ax (Figure 1J) comparable to the Acheulean technology in the Hantang River near the Chongokni site, South Korea. This site has recently been dated to 350,000–300,000 years ago (Bae, 2010).

Pressure flaking, referred to as *nulerddeki* in South Korea (Seong, 2017), is a retouching technique used by prehistoric knappers to shape stone artifacts; the technique involves using the narrow end of a tool to exert pressure close to the edge of the worked piece (Mourre et al., 2010). Leaf-shaped projectile points not only exhibit morphological similarities to the leaf-shaped aspect but also use the same pressure flaking. It has also been found in Europe (Figure 1I, 1O), South Korea (Figure 1M), Japan (Figure 1L), Kamchatka and western North America (Figure 1K, 1N), and South America (Erlandsona & Braje, 2011).

Neolithic European skeletons (Shin, 2014) and leaf-shaped projectile points (Figure 1M) discovered in South Korea were commonly found in or around dolmens. In this scenario, should FNAs like Anzick-1 have been connected to South Korea, they would have introduced Neolithic cultural items and procedures, such as leaf-shaped projectiles, dolmens, and artificial skull deformations. To confirm the hypothesis, I visited Montana, from August 30 to September 3, 2019, where a dolmen (Figure 1E) with one large table stone supported by several small stones is located on the Dunn Peak along the Dunn Canyon Road, Route 69. Its shape (Figure 1E) is similar to those found in Gochang (Figure 1G) and Hwasun (Figure 1F) in South Korea. It is important to note that the quarries (Figure 1P), leaf-shaped projectiles (Figure 1K, 1N),

Figure 1. Leaf-shaped projectile points and dolmen in Europe, Korea, and North and South America.

- **A** Poulnabrone dolmen (4200 BC to 2900 BC) located in the Burren, County Clare, Ireland.
- **B** Dolmen on display at the Liaoning Provincial Museum in Manchuria.
- **C** As a result of a field survey in 2008, it was found that there is a total of 1,550 dolmens in Gochang, Jeollabuk-do, Korea. The dolmens in the picture are similar to the Poulnabrone dolmen.
- **D** This dolmen is located in the San Agustín Archaeological Park, Colombia, South America. It is similar to the Maison des Fées dolmen in Brittany, France.
- **E** This dolmen was found around Dunn Peak, in Montana, USA.
- **F** This dolmen is located in Hwasun, Jeollanam-do, Korea.
- **G** The dolmens in Gochang, Jeollabuk-do, Korea.
- **H** Dolmen in Fukuoka, Japan. Picture displayed at the Liaoning Provincial Museum.
- **I** European hand ax and leaf-shaped projectile points found in Europe. Photo taken at the Seokjang-ri Museum in Korea.
- **J** This hand ax was first discovered in 1978 in Jeongok-ri, Gyeonggi-do, Korea.
- **K** This Clovis point is at the Blacktail Ranch Museum, Montana, USA.
- **L** This point was excavated at the site of Shirataki, Hokkaido, Japan. Exhibition at Seokjangni museum.
- **M** Korea point displayed at Seokjangni museum.
- **N** A representative Clovis point displayed in the same place as K.
- **O** Europe's representative leaf-shaped projectile points. Photo taken at the Seokjang-ri Museum in Korea.
- **P** Quarry found around Helena, Montana.

and stone arrowheads (Figure 5H) found around dolmens there are consistent with findings in Korea. Dolmens have also been found in Great Britain and Ireland (Figure 1A); France, southern Scandinavia, the Netherlands, Denmark, Germany, the Iberian Peninsula, Italy, Malta, Bulgaria, Tunisia, Algeria, Israel, Syria, Jordan, Turkey, Iran, the Russian Caucasus, and Manchuria (Figure 1B); South Korea (Figure 1C, 1F, 1G); Southern Japan (Figure 1H); and Colombia (Figure 1D).

In South Korea, artificial cranial deformation (ACD) customs can be found in the *Records of the Three Kingdoms* (三國志魏書東夷傳), which were written by Jinsu, of Chinese heritage (陳壽, 233–297 BCE): "People who had lived near Gadeok Island had the custom of artificially transforming the skull by wrapping wood, stones, or cloth around their heads at infancy." A Korean skull with ACD (Figure 2B) was excavated in 1976 at the Yean-ri site in Gimhae, approximately 30 km from Gadeok Island (Jung & Woo, 2017). These customs were also performed among North America's Chinookan tribes of the Northwest, the Choctaw of the Southeast (Meigs, 1866), and in Latin America (Meigs, 1866; Schijman, 2005). This practice is evidenced by the jade sculptures representing ACD that have been found in other areas, including Panama, Costa Rica, Nicaragua, as well as coastal and mountainous regions of Mexico (Childress, 2011). Well-preserved ACDs have been found in Ica, Nazca, Paracas in Peru (Figure 2A), and in Tiwanaku in Bolivia (Figure 2C). Meanwhile, Friedrich Ratzel reported in 1896 that ACD had been discovered in Tahiti, Samoa, Hawaii, and the Paumotu Islands (Ratzel, 1896). Skulls exhibiting ACD have also been found at the Houtaomuga archaeologic site in Manchuria and are estimated to be 12,000–5,000 years old (Zhang et al., 2019). Similar customs have been identified in Indigenous Australian remains, such as the Nacurrie skeleton, which is estimated to be approximately 13,000 years old (Zhang et al., 2019). Meanwhile, in Japan, evidence of ACD has only been seen at Hirota, Kyushu (Heritage of Japan, n.d.). ACDs have also been found in Europe (France, England, Hungary, and Germany), Russia (Omsk, the Pyatigorsk Museum, and the Museum of Kislovodsk), Ukraine (Crimea), and Uzbekistan (the Afrasiab Museum of Samarkand).

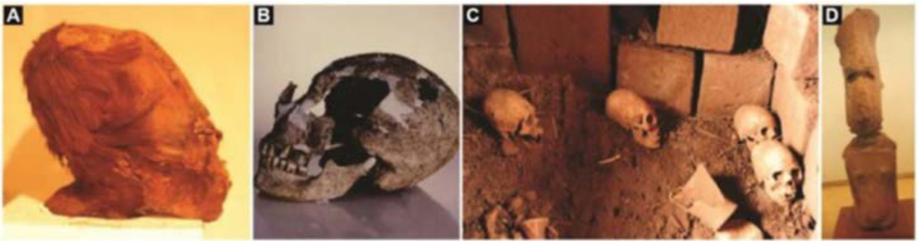

Figure 2. Cases of ACDs.
A Red-haired ACD in Paracas History Museum, Peru.
B This ACD was discovered at the site of Yean-ri, in Gimhae, Korea.
C ACDs found in a private museum located on the left side of the entrance to PumaPunku historic site, located in downtown Tiwanaku, Bolivia.
D Stone ACD displayed in the Father Sebastian Englert Anthropological Museum, Easter Island.

Venus Figurines, Neolithic Stone Statues, and Comb-Pattern Pottery

The 31 Venus figurines (Lipnina, 2012) (Figure 3A1) excavated around Mal'ta, where MA-1 was found, provide evidence of broad cultural connections and contacts between Europe and Central Asia during the Upper Paleolithic Period (Jung, 2001). Of the 31 Venus figurines, only 20 (64.5%) have two hands, where both hands are around the navel, and are in exactly the same positions (Lipnina, 2012). It is noteworthy that the positions of the hands of these figurines are the same as is seen on the T-shaped pillars (Figure 3B2) and Neolithic stone statues (NSSs) (Figure 3B1, 3B3–3B6) of Göbekli Tepe, Turkey, which is estimated to be about 11,500 years old. This supports the hypothesis that the Paleolithic Venus figurines could have gradually evolved into NSSs. In support of this hypothesis, a composite stone statue (Figure 3B1) of a bird and a human (Lipnina, 2012) was found in both regions. Additionally, the Y chromosome found in MA-1 relates to findings near Göbekli Tepe, Turkey (Raghavan et al., 2014; Lell et al., 2002).

While Venus figurines are female (Figure 3A1, 3A2), and most are in a standing position, most NSSs are male and are in upright, seated (Figure 3A4, 3A6), or kneeling positions (Figure 3E2, 3E3, 3F3). The positions of both hands of NSSs are mostly akin to those of Venus figurines, but in some statues, one hand is placed on the chest and the other around the belly button. Unlike other regions, statues found in the Anatolia region of Turkey (Figure 3A3–3A6) were crafted in varying shapes and positions, similar to the NSSs mentioned above, but illustrating how Venus figurines morph into NSSs. The criterion that differentiates NSSs from ordinary statues is that neither hand originates from the chest. However, in the female statues (Figure 3A6) of Çatal Höyük, Turkey, which dates to approximately 5500–6000 bc, the positions of both hands deviated from the chest. The custom of crafting NSSs persisted through the Bronze and Iron Ages, and persists in some regions today, including South Korea (Figure 3D3). Although the names of NSSs vary by region, they share a common meaning. All of them are references to ancestors, grandfathers, and ancestors of the gods (Forostyuk, 2004; Smith, 1915). NSSs were primarily made of stone, but wood or animal bones (Figure 3E6) were also used in some areas. Unfortunately, due to archaeologists' indifference and disinclination to conducting detailed research, most NSSs around the world are left unexamined.

NSSs have also been found in France and Ireland (Figure 3F4); Spain, Italy, Serbia, Bulgaria, Greece, and Turkey (Figure 3A3–3A6, 3B1–3B6); Ukraine (Figure 3C4); Azerbaijan, Uzbekistan, Kazakhstan, Kyrgyzstan, and Tajikistan (Figure 3C2); Altai and Russia (Figure 3C3, 3C5); western Mongolia (Figure 3C1); Manchuria (Figure 3C6); South Korea (Figure 3D1–3D3); southwestern Arabia, Iran, and Indonesia (Figure 3E5, 3E6); the South Pacific (Figure 3E3, 3E4); the Philippines, Mexico, and Colombia (Figure 3D4–3D6); Peru (Figure 3F1); Bolivia (Figure 3F2, 3F3); and Easter Island (Figure 3 E1, 3E2).

A 2013 genetic study suggested that East Asian haplogroup C3* chromosomes had been introduced into Ecuador no more than 6,000 years ago, perhaps via coastal or transpacific routes (Roewer et al., 2013). Venus figurines and comb-pattern pottery serve as archaeological evidence to support this claim. The locations of the hands of Venus figurines found in Ecuador's Valdivian Cultural Site (3500–1800 BC) coincide with those of the Mal'ta Venus figurines. The oldest pottery demonstrates many striking similarities in terms of decoration, vessel shape, and periodization to the comb-pattern pottery crafted in ancient Korea from 6,500 to 5,500 years ago (Figure 4B, 4C) (Estrada et al., 1962; Shin, 2019). In addition to South Korea (Figure 4B, 4C), comb-pattern pottery has been found in Finland, Sweden, northern Germany, the

A. Venus figurines **(A1–A2)** and NSSs during Anatolian Civilization **(A3–A6)** in Turkey.

B. NSSs found around Göbekli Tepe in southeast Turkey **(B1–B6)**

C. NSSs found in Central Asia **(C1–C5)**, Manchuria **(C6)**

D. NSSs of Korea **(D1–D3)** and Colombia's San Agustín **(D4–D6)**

E. NSSs of Easter Island **(E1–E2)**, the Pacific Ocean **(E3–E5)**, and Indonesia **(E5–E6)**

F. NSSs of Bolivia (F2-F3), Europe (F4), Manchuria (F5), and Olmec (F6)

Figure 3. Venus figurines and NSSs from around the world. Venus figurines (**A1–A2)** and NSSs from the Anatolian Civilization (**A3–A6)** in Turkey. NSSs found around Göbekli Tepe in southeast Turkey (**B1–B6)**. NSSs found in Central Asia (**C1–C5)**, Manchuria (**C6).** NSSs of Korea (**D1–D3)** and Colombia's San Agustín (**D4–D6).** NSSs of Easter Island (**E1–E2)**, the Pacific Ocean (**E3, E4)**, and Indonesia (**E5, E6)**. NSSs of Peru (**F1**), Bolivia (**F2, F3)**, Europe (**F4)**, Manchuria (**F5)**, and the Olmecs (**F6)**.

A1 and **A2** are imitation Venus figurines displayed at the Seokjangri Museum in Korea. **A3–A6** are NSSs found in Anatolia, Turkey, showing the process of changing from the Venus figurines.

B1–B6 are NSSs displayed at the Sanliurfa Museum, around Göbekli Tepe, in Turkey, showing the first NSSs.

C1 is an NSS of Western Mongolia and is locally called Balbal or Kurgan stelae. **C2** is an NSS found in Tajikistan. **C3** is an NSS of a Turkic warrior in the Altai, Russia, region, similar to **C2**. **C4** is a Scythian anthropomorphic NSS found in Berdyansk, Ukraine. **C5**, called Stone Turkic Man (618–907 CE), was discovered at Altai, Xinjiang Uygur Autonomous Region. **C6** is an NSS of the Manchurian Xinglongwa culture (8200 to 7200 years ago).

D1 and **D5**: The two teeth protrude from the lips, and **D2** and **D4** are characterized by close similarities. **D1** and **D2** were photographed in Korea, and **D4** and **D5** were shot in San Agustín, Colombia.

E1 is an Easter Island NSS and has a particularly big nose. **E3** is an NSS from the Open-Air Tiki Temple park in Tahiti; it is very similar to Easter Island's **E2**. **E4** is a Melanesian NSS exhibited at the Museum of Anthropology at The University of British Columbia, in Vancouver, Canada. **E5** and **E6** were found at antique shops in Bali and Sulawesi, Indonesia.

F1 is displayed at the Peru Lima Larco Museum. **F2** is called Kontiki after the Inca god Viracocha. **F3** is in front of the Tiwanaku Cathedral, in Bolivia, and looks so much like the **D3** in Korea. **F4** is on Boa Island, in Ireland, and resembles **C3**. **F5** is an NSS of the Manchurian Hongshan culture (6500 to 5000 years ago). **F6** is an NSS representing the Olmec civilization and resembles Africans and faces.

Vistula River (Poland), Estonia, the Upper Oka-Volga River (Russia), Ob River (Siberia), Lake Baikal, the Mongolian Plateau, Manchuria (Figure 4A), and southern Japan (Jung, 2001; Marek, 1997).

RESULTS

In addition to genetic evidence of H and X mitochondrial haplogroups, remnants of the Stone Age—namely, stone tools (i.e., Clovis points and microblade technology), Venus figurines, ACDs, dolmens, comb-pattern pottery, and NSSs—have been found in Europe, South Korea, and the Americas. However, south of the Great Wall of China, and in Honshu (Japan), northern Siberia, Beringia, and

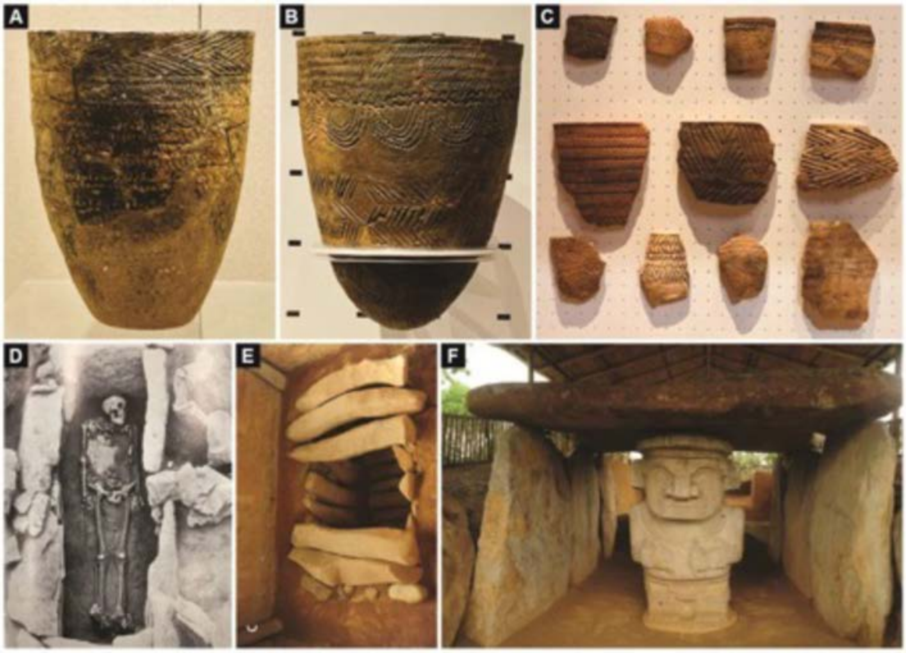

Figure 4. The comb-pattern pottery and the stone tombs.

- **A** The comb-pattern pottery on display at Shenyang Xinle Ruin Museum, in Manchuria.
- **B** The comb-pattern pottery on display at the National Museum of Korea.
- **C** The comb-pattern pottery remnants found around the skeleton at Gadeok Island, Busan, Korea.
- **D** Stone tomb at the site of Yean-ri, where an ACD skull was found.
- **E** Alto de las Piedras, from the stone tomb in San Agustín, Colombia.
- **F** Another NSS that "protects" the tomb of Alto de las Piedras in San Agustín.

the regions formerly covered by the Cordilleran Glacier (i.e., much of present-day Canada), no examples of these five types of archaeological artifacts, except stone tools, have been found.

The correlations between the findings of these artifacts in particular areas suggest that MA-1—an ancestor of the FNAs, who began migrating from Mal'ta about 24,000 years ago (Raghavan et al., 2014)—moved to South Korea and Southern Japan via Manchuria, the Lower Amur/Sea of Okhotsk region, to avoid the cold of LGM (Starikovskaya et al., 2005). While staying there, genetic variations occurred due to intermixing with people from Mal'ta, Australia, and Melanesia (Turner, 1990; Jin et al., 2009; Achilli et al., 2013). This explains the genetic variety

(A, B, C, D, X) of Native Americans (Mizuno et al., 2014). Archaeological evidence related to the verification of the above facts is found in ACD and NSSs. DNA tests of the red-headed ACD (Figure 2A) found in Paracas, Peru, indicate that this ACD originated in Europe and the Middle East (Holloway, 2016). Meanwhile, NSSs with facial features from Europe (Figure 3E1, 3F1), Mongolia (Figure 3D4), and Africa (Figure 3F6) are found in Latin America. Faces are distinguished through criteria relating to the shape of the eyes, the size of the nose, and the overall shape of the object. The above archaeological data are consistent with the complex and diverse genetic makeup of Native Americans.

The six archaeological artifacts presented by the study support the BSH and KHH, indicating that FNAs were largely divided into two groups that migrated to the New World. The first group migrated from Southern Korea to the New World by boat due to the population increase revealed by female lineage diversification when the climate started to grow warmer around 16,000–12,000 years ago (Potter et al., 2018). This group's representative archaeological artifacts are leaf-shaped projectile points, dolmens, and ACDs. The second group's representative archaeological artifacts are comb-pattern pottery and NSSs found only in Latin America. Additionally, only Native Americans in Latin America had full metallurgy with smelting and various metals purposely alloyed (Anawalt, 1992). This indicates that the second group had more advanced skills than the first group and migrated from Southern Korea to the New World around 12,000–6,000 years ago.

DISCUSSION

The concordance of genetic and archaeological data related to Native Americans proves that the perspective in the research method is accurate. The basis for the above argument is fourfold.

First, MA-1, Anzick-1, and Gadeok Island Man are both genetically and archaeologically related to each other (Raghavan et al., 2014; Shin, 2014). This explains why we should study the stone age archaeological and genetic data found inside and outside the Americas together. However, existing research tends to continue using existing research methods rather than finding new ones. As a result, many archaeologists are not interested in ACDs and dolmens (Figure 1E) found in the

Americas, and are attempting to find the archaeological artifacts they want underwater (Hoffecker et al., 2014; Braje et al., 2017; Wade, 2017).

Second, most scholars have not considered Korea's importance in North-East Asia. As an example, scientists who support KHH believe that there is no clear evidence that humans were in North-East Asia around 50,000 years ago (Potter et al., 2018). However, there are many archaeological sites in South Korea dating from the Paleolithic (350,000–12,000 years ago) to the Neolithic (12,000–6000 years ago) periods (Jeju National Museum, 2011; Kim, 2012). It is particularly noteworthy that more than 30,000 dolmens, or about two-fifths of the world's total (Nesterkina et al., 2017), are in Korea. Moreover, dolmens and ACDs are found in southern Japan, but rarely in Honshu, Japan (Heritage of Japan, n.d.). This is consistent with the hypothesis of this study that South Korea and southern Japan were refuges in the LGM period (Potter et al., 2018; Alves et al., 2016).

Third, China in East Asia during the Stone Age should be studied by clearly dividing it into north and south of the Great Wall. This is because most of the six archaeological artifacts mentioned are found in Manchuria, north of the Great Wall. This result aids in determining the route of FNAs, who began migrating from Mal'ta around 24,000 years ago to migrate to Korea via Manchuria, and then to the New World. It is important to note that the regions using the Ural Altai language, such as Manchuria and Korea (Kim et al., 2015), the regions where haplogroup X and the six archaeological artifacts were found, and the Silk Road route, all coincide.

Fourth is integrated thinking on archaeological artifacts. The San Agustín ruins in Colombia, dating back to around 3,300 BC (Velandia, 2005), are located about 900 km off the coast of Ecuador. Archaeologists are still looking for answers to determine by whom these ruins were made, and where they came from. To answer this question, this article proposes a novel study that integrates evidence with Ecuador's Valdivian culture. This suggestion is made because comb-pattern pottery and ACD found in Ecuadorian Valdivian culture, as well as dolmens, NSSs, and stone tombs found in San Agustín, are found on Gadeok Island as well. As a specific example, when comparing the NSSs of San Agustín (Figure 3D4) and Jeju Island, South Korea (Figure 3D2), the eyes, hat shapes, and Asian-like facial features are very similar. Similarly, a Korean stone statue (Figure 3D1), characterized by rounded eyes and two exposed fangs, is also found in San Agustín (Figure 3D5). In the case of the NSSs protecting the tomb of Alto de las Piedras in San Agustín (4F), the belt on the statue's waist and the position of the hands are very similar to the stone statues guarding the ancient Korean tomb. Additionally, the stone tombs of Manchuria, South Korea (Figure 4D), and San Agustín (Figure 4E) have similar shapes. All of this is consistent with the East Asian haplogroup C3* chromosome, which was introduced to Ecuador no more than 6,000 years ago (Roewer et al., 2013). Haplogroup C3* is virtually absent from North and Central America, but occurs in Kamchatka (38%), Mongolia (36%–38%), Korea (10%), and Japan (3%) (Roewer et al., 2013). Finally, 7,000 years ago, Koreans were made up of a variety of races, unlike present-day Koreans, and over time, they were replaced by Mongolian populations from Central Asia (Derenko et al., 2001).

The rationale for this hypothesis is as follows: a) European and Asian skulls discovered in Gadeok Island (Shin, 2014); b) Anthropologists claim that the territory near where MA-1 was discovered, from at least Neolithic times, was populated by mixed tribes with Caucasoid and Mongoloid anthropological features (Derenko et al., 2001); c) The phenomenon in the two regions not only coincides, but also raises the possibility that the Europeans found in Gadeokdo are Caucasoids from Mal'ta. Additionally, this hypothesis explains why the mtDNA from MA-1, Anzick-1, and Gadeok Island Man is currently not found in East Asians (Raghavan et al., 2014; Shin, 2014).

Nevertheless, no Korean historians or archaeologists have officially studied these skulls, and there appears to be a tendency to deny or ignore this (Kim, 2010). This is probably due to the political and religious beliefs held by Koreans about having been a "single race" for 5,000 years.

Why and How Did FNAs Move to the New World?

Monte Verde (Chile) provides some of the most promising evidence of the early inhabitation of the Americas before the widespread FNAs' Clovis culture (Dillehay et al., 2008). Radiocarbon dating of charcoal remains, charred animal bone fragments, and several lithic artifacts found on site in 1982 demonstrate that this area's average age is 14,800 years, about 1,000 years earlier than the Clovis culture (Dillehay et al., 2008; Taylor et al., 1999). However, the above archaeological artifacts have limitations for ascertaining who their protagonists were and how and from where they came. To address these questions, this paper focused on whale hunting petroglyphs found in the Atacama Desert, in Chile (Ballester, 2018), near the Pegtymel River, in Chukotka, Russia (Bland, 2010), and at Bangu-Dae, in Ulsan, South Korea (Shin et al., 2013) (Figure 5 A, 5C). These three sites depict a scene of a caught whale being dragged by a ship. The shape of the ships in the petroglyphs of these regions appear to be surprisingly similar. Additionally, petroglyphs associated with whales were found at the Beach State Historic Park, in Alaska (Heizer, 1947), and at the Ozette site, near Seattle (Figure 5E). The distribution of the above petroglyphs matches areas where leaf-shaped projectile points and shell middens were found (Schijman, 2005; Shin et al., 2013). In the end, observing the above petroglyphs and their connection with the discovered areas reveals who the protagonists of Monte Verde were and why and on what route they came.

Their migration is closely related to the food chain and the ocean currents traveling at up to 6 knots (11.11 km) per hour, as the latter determines the direction of plankton, thereby also determining the direction of whales (Kim, 2010). This would explain why FNAs may have migrated to the New World by boat rather than on foot. What should be noted here is that many archaeological artifacts presented in this paper are found around regions where ocean currents flow. For example, NSSs and ACDs are found on Easter Island (Figure 2D and Figure 3E1, 3E2), the South Pacific (Figure 3E3, 3E4), Indonesia (Figure 3E5, 3E6), the Philippines, Southern Japan, and South Korea (Figure 3D1–3D3) where the Equatorial Countercurrent and Kuroshio Current flow. This is consistent with the fact that the population of Sundaland, including Indonesia, was genetically related to indigenous Australians, Papua New Guineans, and Northeast Asians—including Koreans—during the LGM (Turner, 1990). Meantime, a ship from about 5,700 BC was excavated in Bibong-ri, Gyeongsang-do, Korea (Figure 5C) (Park et al., 2010). This ship had a length of 3.1 m, maximum width of 60 cm, thickness of approximately 2–5 cm, and depth of about 20 cm (Park et al., 2010). It is likely that human voyages during the Stone Age

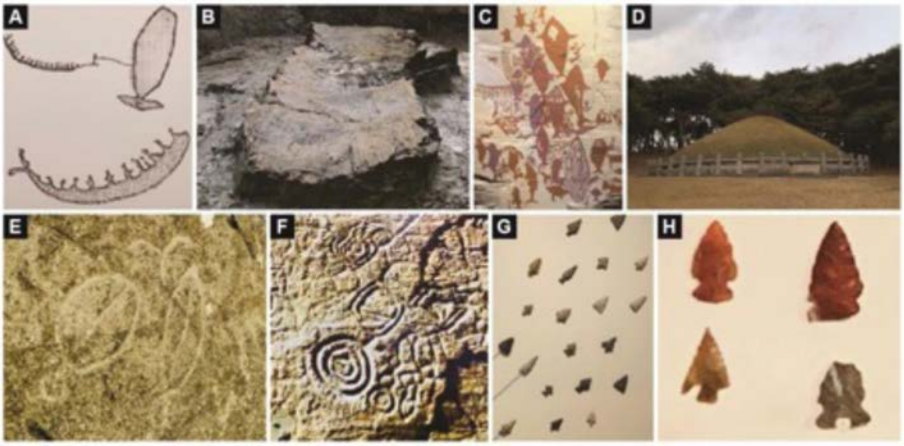

Figure 5. Relics related to the movement of humanity in the Stone Age.

- **A** Whale catch ship inscribed on rock engravings on the Ulsan Bangu-Dae archaeological site, South Korea.
- **B** A ship of about 8,000 years ago, excavated from Bibong-ri, Gyeongsang Province, Korea.
- **C** Rock engravings on the Ulsan Bangu-Dae archaeological site.
- **D** This tomb was filmed in Gyeongju, Korea, and has a similar overall shape as the 5,200-year-old Irish tomb Newgrange and the Enon Mound in Ohio, USA.
- **E** Whale petroglyphs at the Ozette site, near Seattle, USA.
- **F** Petroglyphs of the spiral symbol, Cheonjeon-ri, Ulju, near Ulsan, Korea. The spiral symbol is also found at Newgrange in Ireland and at the Sun Dagger in New Mexico, USA.
- **G** Stone arrowheads and Korean points from Korea during the Stone Age.
- **H** This stone arrowhead photo was taken at the Blacktail Ranch Museum, Montana, USA. The surprising fact is that the stone arrowheads found in Ireland are similar to those found in Korea and the United States.

were far more advanced than we think today. As a specific example, an archaeologist and chemist who studied obsidian discovered that 6,000 years ago, people sailed from Indonesia to the South Pacific, 3,500 km away (*Science* News Staff, 1996). Neolithic boat petroglyphs have been found near the site of Wadi el Baramiya in eastern Egypt, depicting 70 sailors (Rohl, 1999). Another example of archaeological evidence of advanced seafaring is African bottle gourds (*Lagenaria siceraria*) used by FNAs. One of the first plants grown by humans, African bottle gourds (Wade, 2014) would have been carried by boat when FNAs moved to the New World from Southern Korea. The basis for this claim is the result of an ancient DNA analysis of archaeological bottle gourd specimens that found the African bottle gourd was used in East Asia at least 11,000 years ago and in the Americas some 10,000 years ago (Kistler et al., 2014; Erickson et al., 2005). In addition, no archaeological evidence of African bottle gourds was found in Siberia, Alaska, or Canada (Wade, 2014), similar to the results of this study.

Meanwhile, this study was conducted by only one individual, who did not receive financial support from any organization, a limitation being that the researcher solely focused on similar shapes of each archaeological artifact. However, Europe's Neolithic tombs, Newgrange in Ireland, and three spiral symbols and arrowheads found around the upper tombs have also been observed as having similar shapes to items in South Korea (Figure 5D, 5F, 5G) and the Americas (Figure 5H). Furthermore, the mtDNA haplogroups H and X and the following archaeological sites are consistent: Stonehenge (England), Le Grand Menhir Brisée (France), Hypogeum (Malta), the Egyptian and Sumerian civilizations, Göbekli Tepe (Turkey), the Liao River civilization (Manchuria), the Olmec civilization (Mexico), San Agustín (Colombia), and the Tiwanaku civilization (Bolivia). Finally, we should all rethink why Stanford University's Ian Hodder argues that all previous theories of the Neolithic period were wrong (Symmes, 2010).

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RESEARCH ARTICLE

Effects of Background Context for Objects in Photographic Targets on Remote Viewing Performance

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Abstract—Photographic images of physical objects are common targets in remote viewing projects today. This exploratory experiment investigated whether the background within which the object is positioned may impact the accuracy of remote viewing. Twelve experienced remote viewers each completed 30 open-response, triple-blind remote viewing trials, requiring them to utilize extrasensory perception to describe the photographic image they would receive via email a few days later. Investigators created a photographic target pool of complex objects set within one of three background conditions: 1) White: devoid of information; 2) Normal: a setting in which the object would typically be found; 3) Unusual: a setting in which the object would not typically be found. Participants completed a total of 360 in-depth transcripts consisting of 8,460 written descriptors and 1,472 sketches. Two methods were used to analyze the transcripts for accuracy, the traditional sum of ranks matching procedure and an exploratory method involving the scoring of each item and sketch by both the participant and an independent judge. These two methods revealed significant but opposite differences for photographic targets of objects set within white backgrounds compared to the other two backgrounds. Better scores for targets with a white background were found for the traditional matching procedure, but worse scores were found for this background when each item and sketch were rated individually. In

addition, the individual items and sketches were found to describe the target object more frequently than the background when normal or unusual backgrounds were present. Results suggest that object background can affect the outcome of remote viewing sessions, although the effect may depend on the scoring method applied.
Keywords: Remote viewing, target material, extrasensory perception, anomalous cognition, photographic target material

BACKGROUND

Within the parapsychological literature, an emphasis has been placed on certain target material characteristics that might lead to greater success in free-response experiments involving nonlocal, psi-based perception. These characteristics include: familiarity with one's natural environment (Pratt et al., 1940; Sinclair, 1930; Warcollier, 1948); movement—dynamic vs. static (Honorton & Schechter, 1987; Honorton et al., 1990; Krippner & Zeichner, 1974; Morris, 1977; Warcollier, 1948); surprising, interesting, and meaningful content but not disturbing (Delanoy, 1989; Nash & Nash, 1961; Tart, 1980; Warcollier, 1938; Watt, 1989); use of objects or elements in which the foreground is distinctive from the background (Warcollier, 1948; Watt, 1989); and the portrayal of a potentially realistic scene or object vs. one that is abstract or presented in an unrealistic or unrecognizable fashion (Delanoy, 1989; Krippner & Zeichner, 1974). May (2011) noted that targets with thermodynamic properties, those involving the release of a large amount of energy in a short period of time, such as nuclear tests and rocket launchings, "never seemed to fail" (p. 65). Some researchers found that emotion-triggering images such as those containing sexual content sometimes produced stronger effects under certain conditions (Honorton, 1985; Bem, 2011).

A re-occurring theme across all the above studies was that individual participants often differed in their emotional responses to certain targets. Warcollier (1938, 1948) found that those who tended to exhibit stronger emotional reactions than others in their lives seemed to be impacted by the emotionality of targets more than others as well. Delanoy (1989) found what one is normally attracted to in regular perception tends to be the same thing that one is attracted to with psi perception, so for example a person who enjoys cityscapes over natural landscapes may have better success at scenes of cities over nature scenes. Additionally, many researchers have suggested that psi perception may parallel regular perception in terms of a participant's ease or difficulty in perceiving different types of targets or aspects of them. Warcollier (1948) argued that certain principles emerging from the Gestalt schools of psychology, such as figure and ground (the ways in which one distinguishes a figure from the background) and closure (the tendency to fill in missing information from an incomplete object) were applicable to emerging data from his own telepathy experiments. He stated, "We can look to the psychology of perception for other principles that reveal themselves in paranormal behavior" (p. 26). Schmeidler (1977) wrote "find what a person perceives best and perceives less accurately, whether in vision, auditory, etc., then test the hypothesis in remote viewing that he will be most accurate and least accurate—in parallel ways" (p. 1). Other researchers who theorized that psi perception may mirror regular perception included Pratt et al. (1940), Mitchell (1981), Watt (1989), May and Lantz (1991), May et al., (1994a, 1994b), Swann and Puthoff (1987), Targ et al. (1995), and Thorpe (2013).

Types of objects used in successful free-response telepathy, clairvoyant, dream ESP, GESP, Ganzfeld, and remote viewing experiments and applied projects spanning the past 100 years have included: simple drawings (Carrington, 1941; Sinclair, 1930; Warcollier, 1948); video clips (Krippner & Zeichner, 1974; Storm et al., 2010); picture compilations or collages including a mixture of photos and cartoon-like drawings (Honorton, 1985; Krippner et al., 2018); photographs of real locations (Katz et al., 2019a, 2019b; Müller et al., 2019); actual locations (May et al., 1990; Schwartz, 1977, 2019; Targ & Puthoff, 1977, 2005); and real objects (Mitchell, 1988; Targ & Puthoff, 1974, 1977; Targ et al., 1995).

Rationale for Current Project

In recent times, remote viewing projects outside of academic research settings have moved toward the use of photographs of objects as targets, encouraged by the ease of acquisition of photographic images through free or inexpensive online photo-sharing sites. This provides a much broader range of potential target objects. In these images the object is sometimes shown on a white or plain-colored background devoid of information, and sometimes it is set within a real background that can both provide contextual information about a target, but that could potentially distract from the main focus.

Despite lack of formal testing of the importance of image format, these varied images have often been used, perhaps forming the entire target pool or mixed with more traditional photographic images of actual locations, in applied and experimental remote viewing projects. Most of these projects have not been reported in the research literature, although the present researchers have participated in them as remote viewers and judges. These have been carried out by parapsychological researchers attempting exploratory studies, and by applied RV or ARV project managers (Katz et al., 2018a, 2018b; Rosenblatt, 2000; Rosenblatt et al., 2015; Williams & Siegel, 2014) for purposes of using psi for wagering in stock market predictions or sporting events or horse races.

Objects within Normal Settings vs. Unusual Settings

Additionally, one of the present researchers (the first author) noticed that many of her remote viewing students, who were located at various distances and meeting via teleseminar conferencing, had an easier time recognizing larger gestalts, even naming the target, when real objects were set in normal locations vs. unusual ones. A *normal background* would be a boat in the water, or a piano located in a living room. *Unusual* might be a boat or a piano positioned in a desert landscape. This seemed in alignment with results from earlier studies that Delanoy (1989) included in her literature review of target characteristics.

A Theoretical Model for a Conceptual Replication

We conducted a search of the more recent cognitive attention literature involving types of photographs that are more easily perceived than others. Of greatest relevance was a series of experiments conducted by Barenholtz (2013). The experimenters sought to understand factors involved in visual recognition of objects as they are related to environmental settings. While most visual research had focused on the inherent properties of objects, Barenholtz wanted to understand the relationship of visual context to object recognition through testing reaction time. The study involved comparing the time it would take for a participant to recognize an object when it was set within one type of setting or background, compared to another. To measure this, she devised a system where presented images would first be pixelated to such an extent they could not be identified. Then, the number of pixels was increased over time until the participant could identify the object.

Recognition reaction times were compared for objects in three settings: a background devoid of information (without context), a familiar background in which the object would normally be found, and an unusual background. Barenholtz found that object identification was faster when the object was in a familiar context. Objects in an unusual context were identified more slowly, and those presented with no background were identified even more slowly. While the present study does not attempt to replicate reaction time to changes in pixilation, and therefore adopts a different design from Barenholtz's, it does base its hypothesis on her findings regarding object–context familiarity, seeking to determine the extent to which these translate to nonlocal perception.

Objective

The purpose of this exploratory project was to perform a comparative analysis of remote viewers' performances when tasked with describing an object placed in one of three different types of background. We also wanted to know whether remote viewers are more likely to describe the main target object compared to the setting of the object within the photographic image.

Hypothesis 1. *Background/Setting within Photographs of Objects will make a difference to remote viewing success.* Based on the theory that extrasensory perception mirrors physical sensory perception, it was hypothesized that experienced remote viewers would have most success viewing objects set within their natural or expected background. It was predicted that performance would decline for these experienced viewers when working with photographs of objects placed within a plain background devoid of information. This is based on the idea that most of the participants are trained in and/or utilize methodologies that encourage them to use imagery that has them visualizing they are moving around a location and positioning their awareness at different vantage points, while allowing sensory data to come in on a full body level through asking probing questions such as "what do I hear, feel, taste, smell, etc." We predicted that viewers would have the highest rate of incorrect information when the target object was placed in an unusual, confusing, or illogical background. This prediction was based on our theory that viewers tend to have greater instances of psi missing or distortion of information when they cannot reconcile confusing elements during a session, and on Barenholtz's findings that participants in her research had the most difficulty identifying objects placed in an unusual, confusing, or illogical background.

Hypothesis 2. *Some participants will perform better than others.* This is based on findings from past studies that certain remote viewers did consistently better in ongoing projects then others (Utts, 2018) and that "select subjects" did better than non-select (Storm & Tressoldi, 2020). Further, a recent study found that had results been assessed individually, rather than collectively for the group of viewers, results (in terms of both hit/miss rate and amount of earnings from wagers made on predictions for sporting events) could have been significantly better (Katz et al., 2019a).

Hypothesis 3. *Object Categorization*. Given that all of our participants had substantially more experience with location-based targets than object-based targets, we hypothesized that more of their correct impressions would pertain to the background than to the main object. This was only relevant to two of the conditions, those with normal or abnormal backgrounds. Even though remote viewers were advised that all the targets contained objects as focal points, they were given permission to describe the entire photograph.

METHODS

Participants

Five men and seven women participated as remote viewers. Six had more than 10 years of remote viewing experience, four had 5 to 10 years, and two had 2 to 4 years. Three of the remote viewers had completed more than 1,000 sessions. Five had completed between 500 and 1,000 sessions. Three had completed between 200 and 500 sessions. Two had completed between 20 and 99 sessions. Nine indicated they had been trained in Controlled Remote Viewing methodology (Smith, 2014) or a derivative. One had training in Extended Remote Viewing (Morehouse, 1998). The remaining two had unspecified training and approaches. Ten of twelve indicated that they did not have a lot of experience describing objects within photos, but they felt confident they could do so.

Traditionally in remote viewing research or operation projects, participants have been assigned viewer numbers (Smith, 2005). However, researchers felt numbers can have a dehumanizing effect. In order to give viewers a sense of empowerment and create a fun, positive environment, viewers were told to choose a God or Goddess name of their choice from any mythology or tradition. These names were used as participant identifiers to maintain confidentiality.

Separation of Roles

Researchers' roles were defined to ensure appropriate blindness at all phases of the project. Descriptions can be found in Table 1.

Materials

Materials included the photographic targets, score sheets for Phase I judging, and survey forms and photo sets for Phase II judging.

Targets. *Targets were selected specifically for this project by the first author.* 30 photographs were created and utilized, each containing a prominent single object clearly and immediately identifiable, even by cursory observation, as the center of interest. We referred to these center-of-interest objects as "main objects." Photographs were mostly collected from royalty-free Internet sites.

Object themes. The main purpose of the project was to compare remote viewing performance for target objects set within three different background conditions. Therefore, it was necessary to ensure that the photos to be compared would be similar in other ways. It would have been ideal to be able to use the exact same object across each different background condition, but this would have then meant we had the same ten objects occurring across three backgrounds. It was felt that this might produce cognitive noise narrowing the "free response" into more of a forced-choice task, if the viewers started to suspect that the

TABLE 1
Researchers' Roles and Responsibilities

Researcher	Responsibilities
Researcher #1 (Katz)	Initial project design, Initial recruitment of remote viewers and judges; designing the participation agreement and training materials; overseeing viewer and judge trainings; creation of the target pool; sending target descriptors to an individual we will refer to as "the Randomizer"; creation of the photo sets for Phase II judging, overseeing data entry into master spreadsheets after receiving all score sheets from Researcher #2.
Researcher #2 (Bulgatz)	Oversaw the entire experimental phase of the project which included communicating with all potential subjects along with Phase I and Phase II judges, the randomizer, the score sheet creator, etc.; preparation and delivery of the score sheets; and serving as a liaison between these participants and Researcher #1.
Researcher #3 (Lane)	Served as a statistician for the overall project (in addition to and after having served as an independent judge for Phase I). He calculated all statistics and supplied the statistical portions of this paper and served as a scientific advisor.
Randomizer	This was the individual assigned to generate the random assigning of targets. He sent the list to Researcher #2, communicated on a biweekly basis with Researcher #2 by sending the photo feedback per trial according to schedule, etc.
Key Holder	The Director of the Rhine Research Center, John Kruth, accepted our request to hold the key in the event something should happen to the Randomizer. He was the only other person besides the Randomizer who ever had access to the target list of randomized numbers connected to their photo names, prior to the photo feedback being released. He had no other connection with the project, nor was he aware of the identity of any of the remote viewers.

same object was going to continue to reappear. Therefore, as the next best option, ten object *themes* were created, with the idea these would either include the same kind of object (but using a different photograph of that object type), or an object that was very similar in its construction, size, material makeup, purpose, emotional impact, etc., to the others in the theme.

For example, one theme was guns. A photo of a rifle in a hand in a nondescript room was chosen for the regular background; a machine gun underwater was chosen for the unusual background; and a photo of a colorful squirt gun against a white background was used for the third condition. For the musical instrument themes, a grand piano in a studio was used for the regular background; an upright piano located at a shoreline was used for the unusual background; and an accordion set against a white background was used for the third condition.

In constructing the target pool, attempts were made to balance gestalts of water, land, sky, movement, desert, indoors, outdoors, natural, manmade, human, animal, etc., so these did not appear too often. Many of the object selections were informed by findings in the parapsychological literature discussed above. For example, one theme included objects possessing thermodynamic qualities as defined by May (2011). In alignment with Bem's (2011) target pool we included a theme of guns (related to violence) and bras (related to sexual arousal). At the same time, we wanted to incorporate themes of objects that are often used as targets within informal associative remote viewing projects, such as food, animals, and transportation devices. Table 2 contains the object themes and types of objects used.

TABLE 2
Category Object Name and Type of Object Used

Subject Type	White Background	Regular Background	Abnormal Background
Guns	Squirt Gun	Rifle in Hand	Machine Gun under Water
Bras	Spikey Bra	Pink Bra on Woman	Bra on Tree in Forest
Instruments	Accordion	Piano in Studio	Piano on Beach
Food	Chocolate Cake	Italian Food on Table	Sushi Plate on Satellite in Outer Space
Cigarettes	Marijuana	Cigarette in Human Mouth	Cigarette in Fish Mouth on Land
Bikes	Green Quad	Tandem Bike with Kids on Pavement	Bike in Water
Buildings	Castle	House on Land	House in Sky
Explosions / Fire	Exploding Balloons	Nuclear Explosion in Desert	Fire on Burning Man
Animals / Birds	Parrot	Polar Bears on Ice	Penguins in Desert

Backgrounds and Settings

To test the extent to which differing backgrounds affected remote viewer performance to distinguish and parse prominent center-of-interest objects ("main objects") against an array of backgrounds, three visual background conditions were selected.

White background: The target object is shown on a white background, devoid of information or context

Normal background: The target object is shown within a setting in which the object could typically be found (e.g., piano in a living room)

Unusual background: The target object is shown in an unusual setting, where the object would not typically be found (e.g., piano on a beach).

Inevitably, both normal and unusual backgrounds might contain additional secondary objects within them. We selected backgrounds such that these secondary objects would be much less attention-attracting than the main objects, so as to interfere as little as possible in the process.

Blinding Protocols

All remote viewers and those in contact with them were blind to the nature and content of the assigned targets for each trial. They were told the definition of objects was very broad and could include anything of an inorganic or organic nature, with no parameters in terms of size or subject matter. They were told this in advance so they could adjust their chosen methodology to the task. All participants and both Researchers #1 and #2 were blind to the *order* of the assigned targets. During data collection, Researcher #1, who had constructed the target pool, intentionally did not communicate with the viewers. All communications were conducted between the viewers and Researcher #2, who was blind to the nature of the target pool and to all targets.

Randomization of Target Material

After careful selection of the 30 targets, Researcher #1 recruited a volunteer (referred to as the "Randomizer") with statistical experience who was willing to both randomize the targets and send the photo feedback to Researcher #2, according to a predetermined schedule. The randomizer was provided the link to an online computer program designed for the purpose of randomizing remote viewing targets, created by P. J. Gaenir (2013).

The Randomizer was instructed to examine the list of target themes to ensure that the same object theme did not repeat in succession. In this respect the randomization was only partial. The randomizer was not given any directions as to how to perform the separation. The rationale behind this request was that remote viewers often experience a sense of confusion when they begin to get impressions which remind them of a target they recently worked with. They cannot discern if it is related to the present target, or if they are just still focused on, or remembering, a past target. This then becomes a psychological dilemma and often they may choose to not report the aspects that are too close to the target they just had. This is not an intuitive problem, but an analytic one. Although it would decrease the overall effectiveness of randomizing, it was felt there was far less to lose from this decision than there would be to have too many repeating elements in a consecutive order, which was likely to happen given that out of ten target types, each would be repeating three times.

Once the list of target numbers was generated by this Randomizer, the list of target numbers only (*without* the attached photos) was given to Researcher #2.

Target numbers construction. Two-digit numbers were added on to the randomized numbers, according to the order they were assigned to viewers. So 01 was added to the end of the first random number for the very first trial and 30 was added to the very last target number that was assigned to viewers. These created the target numbers the viewers would see and work with.

Rationale for Participants Completing All Targets at the Same Time and in the Same Order

After much consideration, it was decided that all participants would receive the same target numbers at the same time, thus also receiving the same feedback photo at the same time. It was felt this would create greater cohesiveness, while decreasing the possibility of displacement occurring to other feedback photos, which has been observed to be a problem in our other projects when different viewers are receiving different targets during the same time period and then submitting their results to the same researchers. "Displacement" or "displaced psi" is thought to occur when one attempts to utilize one's psi-based perceptions to describe a target but then inadvertently describes something else, such as another photo in the judging set or another target that will assigned for a later trial. (Carrington, 1940/1941; Milton, 1986a, 1986b). This decision was also related to feasibility issues and a desire to maintain double-blinding protocols as our highest priority. We had 30 defined targets total, which had taken quite a bit of time to assemble. The only way to ensure that Researcher #2, who needed to have ongoing contact with the remove viewers, remained blind to all targets and even to the nature of the target pool, was to have her only see a single feedback photo per each trial, once all remote viewing transcripts and score sheets were submitted. Once she and the viewers saw a photographic target, it was not reused.

While decisions such as this have been criticized by some parapsychologists concerned with a *stacking effect*, according to Brier (1976), who first discussed the stacking effect in relation to forced-choice task type experiments involving multiple trials in one setting (such as when a deck of 52 cards is being "guessed" at), there are sometimes valid reasons to assign all participants the same target types, in the same order, such as when doing otherwise would render a project unfeasible, and this should not disqualify a design as being flawed (Thouless & Brier, 1970).

Timing and Procedures

Viewer location. All remote viewers completed their sessions by themselves, unmonitored, from the privacy of their homes.

Viewer instructions. Viewers were given the following instructions: *"Describe the main object that appears within the parameters of the photo that you will be shown at feedback time that is connected to the same target number you will be provided."* Based on an understanding of the participants' various mental imagery methods and approaches typically utilized in remote viewing, further instructions included: *"You are free to 'go' to the object in its location, or you can describe the photo itself. Not* *all photos may have a place to 'go' to. If you 'go' to a location, try to remain within the parameters of the photo because only your impressions that can be judged via looking at the photo will be scorable."*

JUDGING PROTOCOLS AND ANALYSIS—PHASE I RATING AND PHASE II RANKING OF SESSIONS

Two different modes of judging were performed at two different points in time. (The rationale for this is presented below in the Discussion section.) These were broken down into Phase I and Phase II, which used different approaches and were conducted at different time periods. Viewers were aware of Phase I but not of Phase II. Phase I included viewers as self-judges and then independent judges. Phase II only utilized independent judging.

Phase 1 Judging

The schedule for Phase I judging is displayed in Table 3.

Independent Phase I judges. Six independent, volunteer judges were recruited either by personal invitation, referrals, or from postings on remote viewing forums. Criteria for judging were prior experience with rating sessions, a background in remote viewing, or knowledge in research methodology. Each judge was assigned to two viewers throughout the duration of the trial and was responsible for rating all of their transcripts (a total of 60, 30 per viewer).

The rationale behind this was to increase the likelihood of judging consistency across all target types and categories. Since researchers have witnessed on multiple occasions in the past that some judges are more generous in their scoring while others are much more conservative, we were aware that this could play out in decreasing some individual viewers' overall performance ratings, while enhancing others. However, since it was the target categories and photos themselves we were ultimately testing as part of our main hypothesis, we went for this approach.

Phase I judging approach. This approach involved comparing every single descriptive item and sketch to the feedback image, designating each as "correct" or "incorrect." Judges used a rating sheet created by Alexis Poquiz. It is a simplified modification of his earlier system which

TABLE 3
Schedule of Remote Viewing Sessions and Phase I Judging (Biweekly)

Steps	Activity	Timing
1	Viewer was sent tasking email notifying them of the target number and due date.	2 to 3 days before due date, which is either Tuesday or Friday, by 9:00 a.m. PDT.
2	Viewer performs RV session, produces a transcript on white paper. Fills in data score sheet and emails transcript and data sheet to Researcher #2.	No later than 9:00 a.m. PDT on either Tuesday or Friday.
3	Researcher #2 contacts the randomizer to ask for the correct feedback photo to be sent. Randomizer promptly responds.	As soon as every viewer (12) has turned in their transcripts.
4	Researcher #2 sends the feedback photo instructing viewer to rate all perceptions and sketches using the same data score sheet for self-judging.	As soon as every viewer (12) has turned in their transcripts.
5	Viewers self-judge and indicate on the score sheet whether perceptions pertain to object or background. Return the score sheet. They then immediately turn in to Researcher #2.	Viewers need to send in prior to the start of the next trial, so either by Monday or Thursday.
6	Researcher #2 hides/locks viewers self-scores protected by a password and forwards the score sheet to independent judges.	Sent to independent judges once received from viewer.
7	Phase I independent judges make use of the viewer's RV perceptions already inputted into score sheet (so they don't have to input themselves) but are blind to the viewers' scores. They return the score sheet to Researcher #2.	Returned to Researcher #2 prior to commencement of next trial.

This completes the trial, and the process starts all over again.

the present authors have referred to as the Poquiz Method of Scoring (Katz & Bulgatz, 2013; Katz & Knowles, 2021), which utilizes an Excel sheet that allows for various fields to be hidden and locked and for individual scores from columns to be automatically calculated. The sheet allowed for the scoring of individuals' words and sketches.

The Poquiz Scoring system (Poquiz, 2012a, 2012b) is based on a systematic interpretation of the traditional SRI 7-point scale (Targ et al., 1995). Poquiz's interpretation uses numerically defined levels based on percentage values of correct and incorrect matches and the percentage value of unknown matches. The core concept of this approach involves systematically listing out every single descriptor and sketch from the viewer's session into a spreadsheet. Each descriptor and sketch is then rated by a judge as "Yes," "No," or "Unknown." The unknown response means the judge cannot evaluate the perception, but that does not necessarily mean it is correct or incorrect. For example, if the photo is of an ocean and the viewer says they feel a breeze, or smell fish, or hear birds. If these are not in the photo some judges may feel they cannot evaluate such impressions, but the impressions are not necessarily wrong.

This Phase I judging was completed by the viewers (self-judging), followed by independent judging by others who evaluated the same items. Judging was completed for one trial before the next trial could begin. This approach provides no statistical means to determine whether scores differed from chance. However, it was possible to compare the proportion of "yes" ratings (hit rate) across the three background conditions.

Phase II Judging

Judges for Phase II analysis method (sum of ranks matching). Twelve judges with remote viewing experience were recruited. These included newer remote viewing students and those who had either served as viewers, judges, or managers in multiple projects over the course of many years. One Phase II judge was assigned to one viewer, so that the same judge rated all 30 transcripts for that viewer. Most completed these during a one-week period following the completion of all viewing trials.

Phase II judging approach. Using standard matching procedures, the 12 judges compared the viewers' data sheets and sketches to a randomly selected set of four photos, rank ordering the matches from 1 through 4 (best match to worst match) using the sum of ranks method described by Solfvin et al. (1978).

Judging sets. The sets of four photos were created by Researcher #1 (Katz). Three decoy photos were chosen from the same royalty-free websites per the same rules as the original targets and combined with the target image into a set. While other experiments have often used the targets for one trial as decoys in another, researchers wanted to avoid the possibility of judges logically dismissing certain photo choices because they had seen them appear elsewhere. This was found to be a problem with early remote viewing experiments (Utts, 2018). For this reason, it was decided that photos in the judging sets would not repeat. Once all 30 sets of targets and decoys were created, these were randomized by an online survey program which automatically shuffled the sets into different positions for each judge.

Phase II judging sets followed the criteria for creating sets that have been outlined in the work of May et al. (1990) as far as the importance of photos to be orthogonally different, yet similar as far as their level of interest, emotionality, and in entropy. To ensure the photos within each set of four were different from each other, the following rules were established for creation of the sets: All photos in a set ideally should have different backgrounds in terms of all major and minor gestalts (water, land, structure, air). All objects should differ as much as possible in shape, color, size, texture, luminosity, patterns, number, concept, and function. To ensure that judges' choices were based on the quality of the remote viewing session rather than on clues that could skew results, each photo set contained different objects set within the *same background type*. For example, targets with white backgrounds were paired with three decoy photos of objects against white backgrounds.

RESULTS

Summary of Remote Viewing Sessions

A total of 360 remote viewing (RV) sessions were completed by the 12 viewers. Each viewer completed all 30 target images, ten in each of the three background conditions. Viewer transcripts provided a total of 8,460 descriptive items and 1,472 sketches for evaluation. The number of descriptive items and the number of sketches found within the remote viewing transcripts varied amongst the 12 viewers, with some providing many more than others. Table 4 summarizes these data.

TABLE 4

Number of Descriptive Items and Sketches by Viewer over 30 Trials

Values include Mean (SD) and minimum and maximum number.
Duration includes Mean (SD) and minimum and maximum time reported by the viewer.

Viewer ID	Number of Descriptive Items per Trial	Number of Sketches per Trial	Duration of Viewing (min)
Akeru	70.3 (16.0) 24 – 107	7.1 (2.8) 3 - 14	49.7 (24.6) 10 – 130
Athena	10.9 (3.6) 4 – 21	2.4 (1.1) 0 – 4	6.4 (3.7) 2 – 18
Bucephalus	28.6 (6.0) 19 - 40	1.9 (0.3) 1 – 2	11.4 (1.5) 8 – 15
Chicchan	23.4 (5.2) 16 – 43	4.2 (1.1) 3 - 7	7.0 (2.2) 4 – 11
Hashem	24.8 (5.7) 15 – 35	2.6 (1.2) 1 – 5	21.5 (15.9) 10 – 86
Isis	97.8 (23.5) 53 – 139	11.0 (3.) 6 – 16	23.5 (8.0) 12 – 40
Neptune	19.9 (4.2) 11 – 28	2.9 (1.1) 1 – 5	12.8 (3.6) 6 – 20
Nuadu	18.8 (5.7) 12 – 40	3.6 (1.1) 2 – 6	26.0 (5.7) 6 – 36
Parvati	6.1 (3.0) 1 – 12	7.8 (3.5) 2 – 15	19.5 (6.4) 8 – 30
Sulis	38.1 (5.5) 27 – 46	1.0 (0.0) 1 –1	23.7 (5.7) 10 – 35
Tawa	11.6 (2.6) 7 – 17	2.8 (0.8) 1 – 5	44.5 (11.3) 28 – 70
Zeus	10.0 (2.4) 5 – 15	1.9 (0.9) 1 – 4	9.4 (3.4) 5 – 23

Analysis of Sum of Ranks Scores (Phase II)

Hypothesis # 1—The primary hypothesis of the study was that the background in which an object is positioned would affect remote viewing success. This was confirmed. However, the prediction, based on visual perception research, that performance would be best in the normal background condition was not confirmed. Instead, for this form of judging involving a matching task activity, it was confirmed that trials containing objects shown on a white background were more successful than those with objects embedded within normal or abnormal conditions.

Repeated-measures analysis of variance of the target rank score for each transcript was conducted with a regression model including factors for background condition and target within condition (Proc Mixed, SAS ver. 9.4, SAS Systems, Cary, NC). Analysis revealed a significant difference among the three background conditions ($F(2,22) = 5.58$, $p = .01$). Pairwise comparisons of the three conditions revealed that the sum of ranks for the White background ($M = 21.2$, $SE = 0.97$) was significantly lower than for the Normal background ($M = 25.5$, $SE = 1.0$). This difference was 4.3 ($SE = 1.4$), with $p = .005$. (Note: For those unfamiliar with this type of analysis, lower indicates a stronger effect.) Similar differences were found between the White background and the Abnormal background ($M = 24.7$, $SE = 1.0$) with a difference of 3.5 ($SE = 1.4$) and $p = .02$. However, sum of ranks scores did not differ between the Normal and Abnormal background conditions ($p = .5$).

Hypothesis # 2—The hypothesis that some participants would perform better than others also turned out to be correct. The overall success of remote viewing was assessed for each individual viewer, both within each of the three background conditions ($N = 10$ sessions) and across all three conditions ($N = 30$ sessions) using Monte Carlo simulations programmed in SAS ver. 9.4 (SAS Systems, Cary, NC).

Simulations based on 10,000 trials revealed that a sum of ranks ≤19 exceeded the criterion for significance at $p \leq .05$ (one-tailed) for N = 10 targets and four possible ranks. For the 36 combinations of viewer and background condition, only five of the calculated sums of ranks scores were found to be significantly smaller than chance. The lowest four sums of ranks were all in the White background condition, and the fifth was in the Abnormal background condition. When all 30 trials were evaluated, the simulation estimate for 30 trials and four ranks was sum of ranks ≤65 for $p \leq .05$ (one-tailed), which only three of the 12 viewers achieved. These three participants achieved performance better than chance in both analyses, total and by condition.

Analysis of Phase I Judging—Item Hit Rate/Transcript Analysis

The secondary objective of the project was to explore an alternative procedure for evaluation of the RV transcripts, which assigned each descriptive item and sketch in the transcript a designation of "hit" or "miss" based on its relevance to the target object. Scores for each transcript were summarized for each target as the proportion of correct scores ("hit rate"), with separate hit rates calculated for descriptive items and for sketches. These two hit rates were averaged to yield a combined score that weighted them equally, despite the much higher number of descriptive items. Separate scores were created for the viewer (self-judging) and independent judge ratings that scored each transcript.

Separate repeated-measures analyses of variance were conducted for the viewer self-ratings and the independent judge ratings (SAS 9.4, SAS Institute, Cary, NC). Results for these analyses were mixed. For viewer self-rating scores, the effect of background condition was significant for the combined hit rates ($F(2,22) = 4.53$, $p = .02$). Pairwise comparisons of the conditions for viewer self-ratings indicated that hit rates for the Normal and Abnormal backgrounds were significantly higher than for the White background (see Table 3). This effect was opposite to that found in the analysis of sum of ranks scores.

However, for the independent judges' scores, the effect of condition was not significant ($F(2,22) = 1.76$, $p = .19$). Results are summarized in Table 5.

Hypothesis #3—object categorization. Our hypothesis that more of the viewers' correct impressions would pertain to the background than to the main object (based on the premise they are more experienced describing locations than objects) is rejected. Results indicated that viewers more often described the object than the background. We had hypothesized the opposite result would be true, based on our viewers' responses during a pre-experimental survey that they were more experienced with describing locations.

TABLE 5

Effects of Background Condition on Viewer and Judge Ratings (Hit Rate) of Descriptive Items and Sketches for N = 30 Targets in Three Background Conditions

Combined ratings equally weight items and sketch ratings for each viewer and trial.

	Condition Effect df = (2,22)	Normal Background M (SEM)	Abnormal Background M (SEM)	White Background M (SEM)
Viewers				
Combined Hit Rates	$F(2,22) = 4.53$, $p = .02$	0.61 (0.05)	0.62 (0.05)	0.53 (0.05)
Judges				
Combined Hit Rates	$F(2,22) = 1.76$, $p = .19$	0.43 (0.05)	0.46 (0.05)	0.41 (0.05)

Instructions for item scoring defined a hit as the relevant presence of the descriptor or sketch to any part of the target image. Using the data available for independent judges, each hit was assigned to one of four categories related to: the main object of the target; the background of the object; a non-essential or peripheral object in the image; or more than one of these.

Because trials yielded widely varying numbers of item and sketch hits, the number of hits assigned to each category was converted to a proportion of the total for that trial.

Table 6 summarizes the distribution of these data. Category proportions for each trial were averaged for each viewer, using all available trials. The viewer averages were then combined into an overall average for all viewers. A total of 207 trials were evaluated in this manner. The overall averages indicated that the majority of items were determined to be associated with the main object, with an average across viewers of 0.64. Much smaller proportions of items were associated with the image background (0.09), non-essential objects in the image (0.14), and more than one category (0.17). The results for individual viewers in Table 6 suggest large variations among viewers in the proportion of items specific to the target object.

TABLE 6
Average Proportions of Transcript Items (Descriptors and Sketches) Relevant to Four Categories within the Target Image by Viewer
N = number of targets included for each viewer.

Viewer	N	Main Object	Background	Non-Essential Object	Multiple Categories
Akeru	17	0.70	0.06	0.20	0.03
Athena	13	0.54	0.20	0.12	0.21
Bucephalus	18	0.45	0.03	0.18	0.30
Chicchan	16	0.66	0.22	0.24	0.18
Hashem	18	0.41	0.05	0.14	0.40
Isis	18	0.72	0.06	0.21	0.03
Neptune	18	0.53	0.13	0.08	0.27
Nuada	18	0.66	0.03	0.13	0.09
Parvati	17	0.98	0.07	0.05	0.09
Sulis	18	0.86	0.05	0.15	0.13
Tawa	18	0.48	0.04	0.10	0.02
Zeus	18	0.67	0.08	0.06	0.23

Comparisons of hit rates for viewers and independent judges. It was found that viewers acting as self-judges scored more of their own words and sketches higher than the independent judges did when rating the same information.

The difference between viewer and independent judge hit rate for each target was determined and summarized across targets (Table 7). Evaluation of the differences by *t* test revealed statistical significance for 5 of 12 viewers, all with higher hit rates for viewers. Comparing the average difference for all targets across the group of 12 viewers by paired *t* test revealed that viewers' hit rates (M = 0.58) were significantly higher than those for independent judges (M = 0.43) with $t(11) = 2.99$, $p = .01$.

The retest reliability of hit rates for viewers and judges, which is a measure of the consistency of scores between these two categories of raters, was evaluated by correlation/regression using the mean values for each viewer (N = 30 trials). The correlation between mean values of hit rates for viewers and judges was not significant ($r = 0.33$, $p = .30$). The linear regression of viewer mean scores on judges mean scores yielded $F(1,10) = 1.18$, $p = .30$, with $R^2 = 0.10$. Results show that mean hit rates for viewers and judges were unrelated and seemingly independent of one another.

TABLE 7

Comparisons of Target Hit Rates for Viewers and Independent Judges

Viewer	**Mean Hit Rate (N = 30 trials)**		**Test of Mean Difference t(29), (p)**
	Viewer Self-Rating	Independent Judge	
Akeru	0.73	0.25	11.5 (<.00000)
Athena	0.64	0.57	1.1 (.27)
Bucephalus	0.45	0.42	0.6 (.57)
Chicchan	0.36	0.40	–1.3 (.22)
Hashem	0.73	0.42	8.5 (<.00000)
Isis	0.56	0.15	9.9 (<.00000)
Neptune	0.46	0.47	–0.2 (.88)
Nuadu	0.57	0.47	1.7 (.11)
Parvati	0.59	0.61	–0.4 (.70)
Sulis	0.78	0.71	1.3 (.22)
Tawa	0.42	0.26	3.8 (<.001)
Zeus	0.75	0.48	5.3 (<.00001)

Relationship of Phase 1 hit rate and Phase 2 rank. The relationship between the viewer and independent judge hit rate score and matching rank were evaluated using repeated-measures analysis with Proc Mixed (SAS 9.4, SAS Institute, Cary, NC) with factors for trial and viewer. Statistically significant effects of rank were observed for viewer and independent judge hit rates. Results are summarized in Table 8. Estimated hit rates from the models indicate that hit rate was highest for targets given the best (lowest) rank and decreased monotonically as the rank increased. These results provide supportive evidence of a relationship between the two methods of judging for the trial transcript.

TABLE 8
Relationship between Phase 1 Hit Rate and Phase 2 Rank for Target Match

	F(3,33) (p)	**Estimated Hit Rate by Match Rank (M (sem))**			
		Rank 1	Rank 2	Rank 3	Rank 4
Viewer Self-Rating	6.20 (.002)	0.64 (0.04)	0.62 (0.05)	0.55 (.05)	0.50 (0.05)
Independent Judge	3.70 (.02)	0.48 (0.05)	0.46 (0.05)	0.42 (0.05)	0.37 (0.05)

DISCUSSION

Rationale for Including Two Different Types of Analysis & Self-Judging for Phase

When first designing the study, researchers wanted to avoid a method of analysis that would require the use of photo sets involving decoys, due to the extent of displacement they felt they had personally witnessed in participating in dozens of other projects utilizing matching tasks methods.

Displacement is thought to occur when a psi percipient who is attempting to obtain information about a target or subject matter instead accesses information that is spatially or temporally removed from the designated target. This effect has been observed by numerous researchers, starting as early as 1884 when Richet found a decline in results not due to lack of the existence of psi but because of "consistent confusions of one target for another" (Alvarado, 2008, p. 543). He was followed by others such as Bruck (1925) in relation to hypnotized subjects and Sinclair (1930) in relation to subjects performing telepathic and clairvoyant tasks who sometimes described a photo intended for a future date.

The actual term "displacement" or "displacement effect" was coined by Carrington in 1940, who ultimately concluded, "On the whole I think there is very little to be said about forced matching for this purpose, or any other. . . . it is liable to be completely wrecked from the phenomenon of displacement" (p. 74). J. B. Rhine in 1950 wrote that "Pratt and Foster of the Duke Laboratory have recently found that the subject's displacement and consistent missing combined may produce a highly complex pattern of significant effects" (Pratt et al., 1940). In 1953, Rhine's wife, Louisa, turned away from forced guessing experiments, opting for a qualitative focus, explaining "Over the years, subjects in tests have seldom made perfect scores, and such frustrating effects such as displacement and psi missing have frequently been encountered" (Rhine, 1962a, 1962b). Tart (1980) explored this topic further in a paper titled, "Are we interested in making Psi function strongly and reliably?" also arguing against the use of matching tasks. According to Milton (1986a, 1986b), Tart's paper reinvigorated interest in the topic of displacement and it also led to her writing an entire dissertation on the subject. The present authors also have observed such effects (Katz et al., 2017) and as a result were determined to find a way to mitigate it in the present study.

The present researchers sought to eliminate all factors that might lead to this phenomenon. However, during their proposal review, advisors felt that a standard matching task would be important in terms of statistical analysis. Therefore, we came up with a compromise to ensure that the viewer's attention would not be likely to move to the wrong photos in a set. We created two phases of judging, and only informed the remote viewers of the first phase. We instructed the viewers that immediately upon receiving their feedback, they would self-judge through rating every impression as correct or incorrect to form a hit-rate score.

This would require the viewers to spend a lot of time studying their feedback photo. While there is some recent evidence that suggests feedback may not be necessary (Müller et al., 2019), we theorized intensive engagement with the correct future feedback photo might strengthen target contact as is commonly asserted by those involved in applied projects (Katz & Knowles, 2021).

Viewers were advised that independent raters would also rate their responses and sketches in a similar manner, within 48 hours of when they completed their judging. In this way, all participants' attention was carefully directed and kept away from the Phase II Sum of Ranks matching tasks that did use sets of four photos, with only one being the correct target. To create even greater distance in time and space, Phase II judging was performed by a whole different team of judges than Phase I, and only occurred after the entire experimental phase and Phase I judging were completed. To this day, most of our viewers still are not aware that there was a second phase of judging that involved matching tasks and decoy photos.

Did We Manage To Avoid Displacement?

We did a sampling of the Phase II judging responses for those trials that resulted in misses, and polled the judges, looking to see if there were very close matches to the wrong photo in the sets. We did not find any examples of this. Since we can't know if there would have been displacement if only Phase II judging had been utilized, we can't say for sure that our approach of using two judging methods did in fact reduce displacement, but this approach does seem promising. Future projects could test this by having one group of viewers who are made aware of matching-task independent judging and another group blind to the procedure to see if there were differences.

Still, we (and some of our viewers) may have seen examples of temporal displacement to targets that appeared later (Crandall & Hite, 1983). One viewer felt this was happening quite a bit, and reviewing her session work we agreed with a few of her examples. One viewer in an early trial drew a picture of an accordion that was almost an identical match to the photo of an accordion against a white background. The odd thing was this picture did not appear in the sequence until several weeks later (Appendix Exhibit F). We cannot say of course if this was a displacement effect or simple coincidence but the sketch for trial 6 and feedback photo for trial 16 are strikingly close.

Differences Found in Judging Methods

We are somewhat perplexed at the opposite findings on the effects of background condition obtained by different judging methods (Phase I hit rates vs. Phase II Sum of Rank Matching Tasks). One possible explanation for this is that when it comes to matching tasks, there are possibly different perceptual and cognitive processes involved for judges. Both forms of analysis involve comparing impressions and sketches to a photo, but a rank ordering matching task involves repeating these comparisons multiple times per each photo in the set, and then having to make a number of decisions and choices. Given the complexities involved, it may be that it is simply easier for judges to make sense of data when there is no information in the background, hence the greater success with the white background conditions.

Regardless of the reason for the condition differences, these results suggest that if only a hit-rate type of approach is being used, it may be advisable to use photos of objects within their normal or abnormal backgrounds and to avoid the use of photos in white backgrounds. Conversely, if a project is going to use a matching sum of ranks form of analysis, it may then be best to use targets of objects against white backgrounds. A word of caution though: There are many designs within parapsychology experiments that use matching tasks but do not use rank ordering—meaning there will be only one score given to the best match, and no credit given to second-best match. We don't know if we would have obtained the same results using an alternative approach.

Differences Found between Self-Judging and Independent Judging

For our Phase I judging protocols (hit rates), 9 out of 12 viewers acting as self-judges (following submission of their transcripts to researcher 2) more frequently scored their own words and sketches correctly then did the independent judges when rating the same information. Also, three of the four viewers who generated the highest number of sketches and words produced the greatest differences between rater self-scores and independent judges scores. We cannot say if this is due to a desire to rate themselves more highly, or because they had a better ability to know what they meant both from their words and from their own sketches. Given that viewers were required to input their words into spreadsheets and submit these along with their transcripts prior to receiving feedback and self-scoring, we are confident it was not the case that they simply added in data after they received feedback.

Rater Reliability

For Phase I judging, the choice was made to have the same judge rate all 30 transcripts for the same two viewers so there would be consistency of judging background conditions, since this was the main focus of our exploration. We felt the judges did maintain intra-judging consistency, with little variability observed across trials. Although we provided training sessions for our judges that included verbal and written instructions, we did notice variability in judging styles *between* judges. We performed an informal test of inter-rater reliability for several of the viewers' sessions that had the greatest disparity between viewer's self-scores and independent rater scores, finding that our own scoring would have fallen somewhere in the middle of these scores. Therefore, we feel it was not appropriate to compare hit rates among viewers or to make definitive statements about their performance for this method of judging. One way around this issue would have been to assign the same judge to rate the transcripts of all 12 viewers, for the same trial. Then to maintain consistency across background conditions, the same judge would have to repeat the judging for all 12 viewers when the same object theme appeared two more times. Given the time-consuming nature of scoring all words and sketches, this may have been too formidable a task for some judges, although perhaps not so much so that we'd rule it out if we repeated the study.

Viewers described the main object. We originally hypothesized that given that the remote viewers are more experienced with describing locations, we expected they might be likely to describe locations rather than the main object. We hoped this was not the case, but just expected it to be, especially since over the years we have heard viewers express a dislike for photographs of objects rather than locations. They were instructed that the target pool consisted of objects within a variety of locations, and that their goal was to describe the main object, but that all correct information pertaining to every element (whether object or background) would be scored as correct. They proved us wrong, as results indicated they did most often describe the main object. However, since they knew that the photos did all involve a main object, we cannot say whether their own perception naturally and involuntarily went to these objects, or whether they directed themselves through voluntary attention (Ribot, 1903) to focus on the objects themselves.

This brings us to one of the main aims of our study: Did results correlate with Barenholtz' "normal" visual perception study findings? Again, for item/sketch hit rates it did, for matching sum of ranks tasks it did not. One may want to keep in mind that her design involved single participants attempting to recognize objects set within different backgrounds as pixels were increased as their response times were measured. There was no judging involved and hence no perceptual aspects of judges to potentially impact the process.

In Conclusion

Throughout our literature review two theoretical assumptions seemed to motivate investigations into target characteristics. The first was that a universalized set of characteristics for target material could, or should, be found, to enhance the non-local perception of all participants. The second was an acknowledgement that target characteristics may be more individualized, having a different effect on different viewers based on factors such as personal preferences, interests, experiences, and personality traits.

Given that only a few of our experienced remote viewers who have reputations for doing well at location-based targets achieved significant results for these object-oriented targets, future researchers should not expect that a remote viewer who performed well in one project with a completely different set of targets and protocols, will necessarily perform the same way with other materials and protocols.

Object categories and future research. Finally, we'd just like to say a bit more about the object categories. As noted above, choices were made to include some objects as targets based on past parapsychological literature demonstrating that arousing images (such as those of a sexual or violent nature or having more numinosity or entropy) may produce a stronger effect. These include bras, cigarettes, and guns and exploding items. Since comparing types of objects was not part of our original plan, we did not do a formal analysis of which targets produced the best results overall. However, we had a couple of volunteers go through all transcripts and visually choose which images stood out as the most striking matches to the target photos. In our Appendix we present a few of these. Exhibits A and C show examples of sketches related to our smoking targets. Exhibit B shows sketches for the nuclear explosion target, and Exhibit D shows examples of sketches produced in relation to the guns. One viewer referred to the older rifle as a "collector gun." Exhibit E shows one of our food targets, a cake, which some viewers we know anecdotally have shared they tend to miss. That viewer correctly identified it as a "cake," although misidentified it as a wedding cake instead of a birthday cake. What these examples suggest is that the target pool that was specifically created for this project may hold merit for other projects, whether simply to use as targets or to be compared to each other to see which produce higher hit rates based on the object content alone. These can be made available to other serious researchers upon request.

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APPENDIX

EXHIBIT A

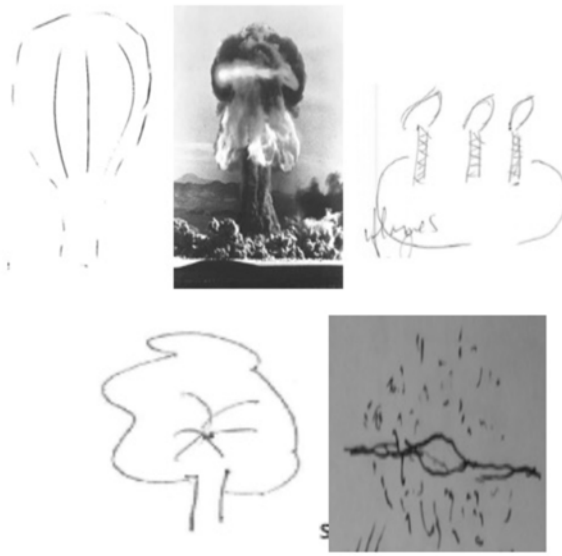
Target 1F13U006

Colors: Blues, rust, yellowish-gold, wheat, doody, white, striped, bright."

"Shiny, sticky, wet, moist, lifeform"; "reminds me of a whale, spongey, like a gold medal."

"Smoke smell, billowing".

EXHIBIT B



"Flames, hot, fiery, colorful, vibrant, ballistic, military, woozy, vertigo, mortar, funnels"

EXHIBIT C

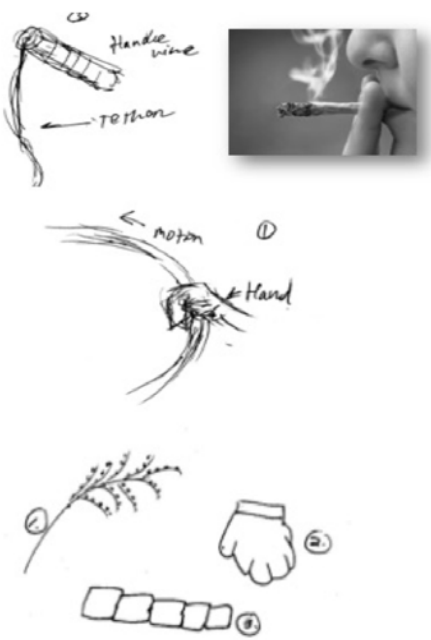


EXHIBIT D

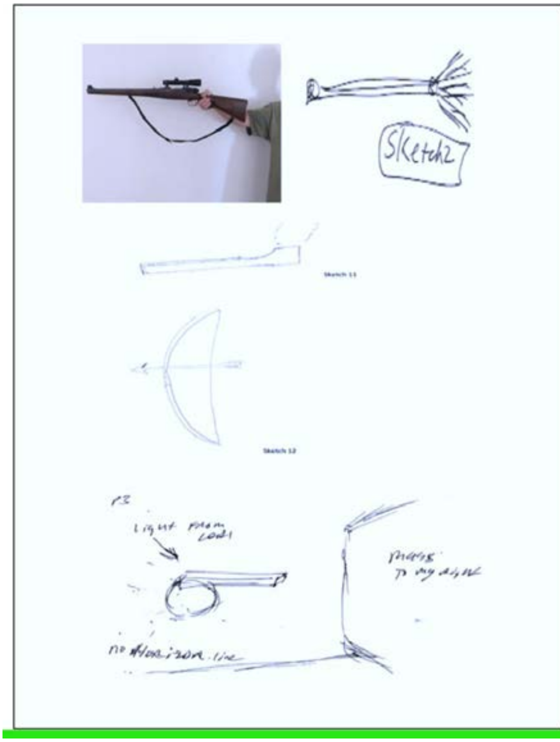
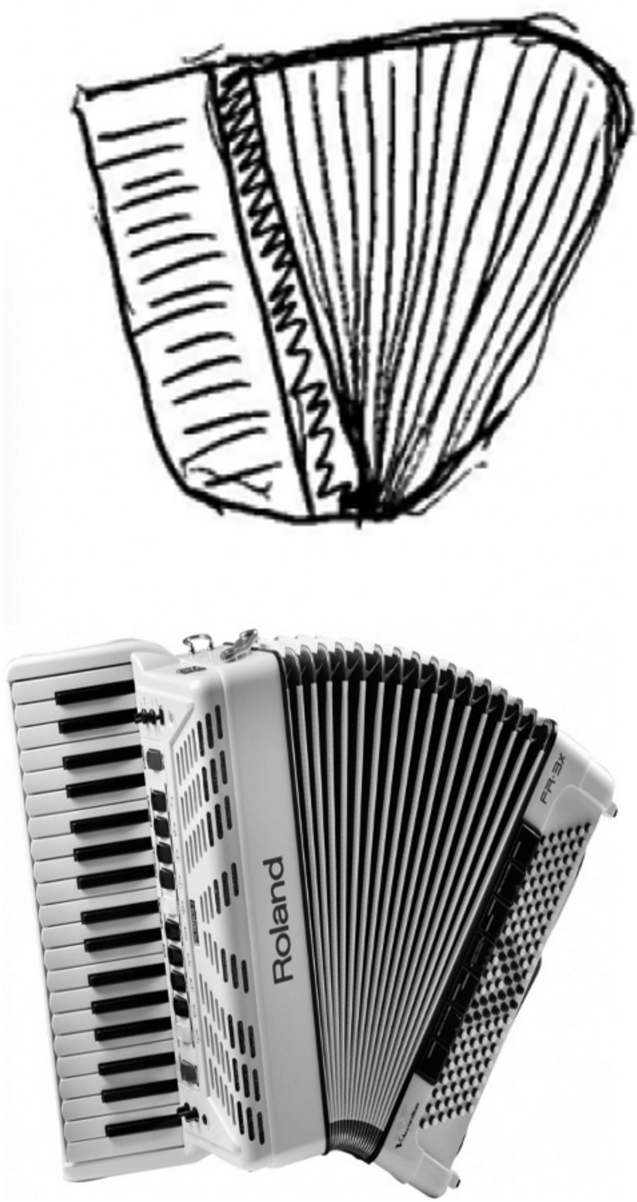


EXHIBIT E

"Wedding Cake." "Feels like there is writing on it." "Many layers," "Golden Brown"

EXHIBIT F



Example of potential out-of-time sequence displacement.

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RESEARCH ARTICLE

Nailing Jelly: The Replication Problem Seems to Be Unsurmountable—Two Failed Replications of the Matrix Experiment

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Abstract-—We have reported previously on positive effects found in the matrix experiment (Walach et al., 2020). This is a setup where a random event generator (REG) drives a display, which participants are instructed to “influence” at will, i.e., in a psychokinesis (PK) setup. The difference of this matrix experiment from standard micro-PK REG experiments was that instead of the deviation from randomness, a large array of 2025 correlations between the behavior of the participant and the behavior of the REG was tested. This previous experiment was significant, and we devised a consensus protocol, which was deposited before commencement, according to which we conducted two independent replications with the same experimental setup and equipment. In the first experiment 64 participants conducted the experiment in one location under the experimental guidance of KK (power = 0.88), in the second experiment 40 participants conducted the experiment in another location under the experimental guidance of HV (power = 0.69). The analysis used a non-parametric randomization test with 10,000 iterations. Neither of the two experiments was significant. While in the first experiment a very small, but non-significant effect was found, in the second experiment no effect was detectable. We discuss the findings in the context of the larger debate around replicability of parapsychological (PSI) research results and our theoretical model. The replication problem and this failed replication is likely part of the systematic nature of such effects. This makes it unlikely that experimental research alone will be successful in the long run in demonstrating PSI effects. Our conclusion is that the matrix experiment in and of itself is not a replicable paradigm in PSI research.

INTRODUCTION

Parapsychologists were probably among the first social science researchers to understand the necessity to publish all negative findings and to insist on replications. Replications come in various forms: Identical replications are rather rare and refer to the replication of the very same experiment, including all materials, methods, procedures, and statistical analyses except that either the same research group or other researchers run a set of different subjects. Mostly, however, replications also vary some kind of procedure, thereby making replications conceptual (Schmidt, 2016). We established previously an experimental paradigm which we hoped would lend itself to replication (Walach et al., 2020). We report here on two replication experiments that failed to replicate the previous result.

Parapsychology and Replication

The idea of replicating experiments is, of course, to exclude accidental findings, false positives, and small statistical fluctuations being identified as systematic effects. Physicists have long been taken as examples of doing natural science rigorously, and they demand at least 5 sigma or standard deviations of the standard normal curve and associated probabilities as confirmations from various experiments until they accept an effect as veridical (Abbot & LIGO Scientific Collaboration and Virgo Collaboration, 2016; Grote, 2018; Horton, 2015). They usually achieve this by accumulating data through multiple observations or through multi-lab experiments, as in the LIGO experiments that detected gravitation waves. Thereby, the very same experimental setup, including all analysis pipelines and data-collection procedures are standardized and logged. The idea behind this procedure is that experiments are detectors for stable and local signals that may be weak but can be eventually separated from noise. Local signals travel at, or slower than, the speed of light and appear regular, i.e., exhibit lawful behavior. The laws might either already be known and predict certain signals, as in the case of gravitation waves that were long predicted by the standard model of cosmology, but the signals might be very weak or very rare, like gravitation waves, or researchers might surmise that unknown laws underly hitherto undetected local signals. This is what some researchers in the parapsychology research community assume (Carr, 2015a; May et al., 1996; May et al., 2018; Radin, 2018).

In parapsychology we are looking for effects whose nature we do not understand, because there is no accepted theory in the first place. Some theories with testable consequences have been proposed, such as "decision augmentation theory" (DAT), which supposes that all PSI effects are precognitive effects, where anomalous cognition of future events is used to augment decision making (May et al., 1995, 2000, 1996). Apart from the fact that this model has problems explaining makro-PK, spuk, and poltergeist phenomena, it also has some empirical evidence against it (Dobyns & Nelson, 1998). Observational theories use some form of argument from a von-Neumann–Wigner-type of interpretation of quantum physics, in which human consciousness is central in collapsing the wave function (Houtkooper, 2002; Walker, 1975, 1979, 2011 [1974]). In this family of theories, it is the joint observational effect of those looking at the data that actually produce the effect. While this might be a theoretical option, it depends on the interpretation of the measurement process and the acceptance of a dualistic model, none of which are currently universally accepted

(Bierman, 2010). Finally, a model very similar to ours is the Conscious Induced Restoration of Time Symmetry (CIRTS) model (Bierman, 2010). This starts from the assumption that most physical theories, with the exception of thermodynamics and special relativity, are time symmetric, and that the brain sustaining consciousness might be a system that restores time symmetry through providing time-negative effects as in precognition. The CIRTS model assumes that there is a kind of signal or informational element in PSI-effects that are, however, bounded by physical theory. The model which we favor (see below) is also derived from physical theory and hence a potential scientific candidate for PSI-modeling, but more strongly than all other models assumes that signal coding is strictly prohibited.

Are PSI effects due to causal and local signals, i.e., obeying the special theory of relativity? Are the signals of a known type, i.e., belonging to the four types of exchange particles of the four basic known forces in the universe, for instance are we looking for photons as exchange particles of the electromagnetic force, or for other particles (Penrose, 2004), or are we looking for completely different, yet nevertheless local causes and signals of a physical nature? Or are we even looking for completely different types of signals that cannot be encompassed within the standard model of physics and hence would, if discovered and proven as stable and replicable, entail a widening of our worldview similar to that produced by the advent of quantum mechanics? Some physical concepts that use higher dimensional models of space and time than relativity theory and quantum theory would suggest this (Carr, 2015a, 2015b; Heim, 1989).

The parapsychological database is jagged so far. While we do have many extremely intriguing phenomena on a phenomenal level (Braude, 1986, 2017; Grosso, 2016), strong and well-documented cases, and highly significant meta-analyses summarizing research fields or experimental paradigms across researchers, variations, and time (Cardeña, 2018; May & Marwaha, 2018, 2019a, 2019b), critics are also correct in pointing out that it is not possible to name one single parapsychological experiment as foolproof and resistant to experimental replication (Alcock, 2003; Reber & Alcock, 2020). It is also true that we have a replication crisis in psychology in general, i.e., the inability to externally replicate experiments that were thought to be proven (Open

Science Collaboration, 2015; Schooler, 2011). This replication crisis affects all sciences, according to a survey where 90% of polled scientists say that there is a problem with replicability (Munafò et al., 2017). It is notorious in medicine despite the fact that medical interventions are widely used and believed in (Horton, 2015; Ioannidis, 2005). So why bother about the lack of replicability in parapsychology? Perhaps parapsychology is even more replicable than standard science, but only more controversial and hence less accepted (Radin, 2018; Schwartz et al., 2018)?

Assumptions Behind Replicability, Synchronicity, Generalized Quantum Theory, and Generalized Entanglement

Replicability, we observed above, makes the implicit assumption that we are dealing a) with local, causal signals that are b) regular, following some lawful rule and c) are therefore always available for experimental control and manipulation. In final consequence they would be amenable to human engineering, once the rules and the lawful behavior are fully discovered. We started our research from the assumption that the *lack* of replicability is part of the *systematic* nature of parapsychological effects. In other words, we assumed that the effects of parapsychology might be *lawful*, *but not* of a *local-causal* nature. This sounds like a contradiction, but it is not. The focus of science has so far been mainly on local-causal signals, because once they are discovered, they can be put to use: We have used electricity, once we discovered the nature of the electromagnetic force. We can use the gravitational force, e.g., by sending satellites into orbit. We even made use of the strong atomic force that keeps atoms together, when we started to engineer atomic fissure. We make use of the knowledge of the weak force in isotope calculations, Geiger counters, and the like. So, what would be a lawful, yet not causal-local event?

In the realm of psychology, Carl-Gustav Jung and Wolfgang Pauli, the physicist, discussed exactly such phenomena, i.e., lawful yet not causal relationships, under the umbrella term “synchronicity” (Atmanspacher & Primas, 2006; Atmanspacher et al., 1995a; Jung, 1952; Mansfield, 1995; Peat, 1992). These are material events in the material world that in their occurrence appear to be without cause, i.e., they happen “accidentally” or “randomly”, yet they have a correlation with the psychological state of a person who relates to those events. Someone calling on the phone, while a person is desperately in need of this contact would be a typical example. This phenomenon has also given rise to a series of experiments (Sheldrake & Smart, 2003) which, in our view, demonstrate that this phenomenon actually exists but is not of a causal nature (Schmidt et al., 2004a), exactly as Jung and Pauli would have postulated.

Jung and Pauli wanted synchronicity to be seen as a type of lawful relationship that is complementary to causal relationships. They codified this in their famous quaternity (Jung in a letter to Pauli on November 30, 1950, in Meier, 1992, p. 64; Meier, 2001), depicted in Figure 1. This also implies that synchronistic relationships that are due to psychic meaning-making or constellation of an archetype, as Jung called it, and are in a way part of a “deeper” structure of reality than local causes. For synchronistic, correlational relationships are part of this primordial level of “indestructible” energy. Similar to this ontic level of “indestructible energy”, that some physicists call the endo-physical level of unbroken unity (Atmanspacher et al., 1995b; Primas, 1994a, 1994b), there are also relationships that pertain to this level and might be put to use (Lucadou, 2019). They might be lawful, but they are not causal in nature. The causality principle only operates on the level of the space–time continuum, or on the level of exo-physics, where clear delineations and determinations can be made, because the original unity is broken into measured and measurable parts.

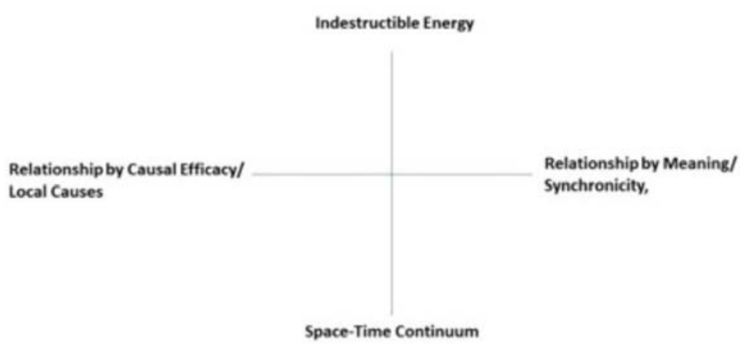

Figure 1. The quaternity Jung suggested to Pauli: Local causes are complementary to correlational relationships or synchronicity in a similar way as the space-time continuum is complementary to indestructible energy.

In that sense, attempts to uncover a purported causality in this realm is futile, simply because there are no local causes operative here, but only formal and final causes, to speak in Aristotelian terminology. Another way of putting this is that there are likely only correlations of a lawful but not causal nature. Indeed, there are examples in the physical world for such a type of relationship as well, namely quantum entanglement correlations (Atmanspacher et al., 2002; d'Espagnat, 1997; Schrödinger, 1935; Shimony, 1989; Stillfried, 2010). These are quite lawful, but not causal in the sense that the lawfulness of these correlations is not mediated by any exchange particles of force or energy. This is something that Einstein had already observed and this is the reason why he called them "spooky actions at a distance" (Einstein et al., 1935). They remained purely hypothetical for a long time, derivable from the formalism, but no one knew whether they are "real". This dispute was settled after John Bell derived his famous inequalities as a boundary condition for joint probabilities that are mutually exclusive, obeying locality conditions (Bell, 1987). This inequality gave rise to an operationalization and an experimental test which finally clarified the issue (Aspect et al., 1982a; Aspect et al, 1982b). There are indeed non-local correlations, i.e., lawful, yet not causally mediated regularities in nature.

Physical quantum correlations have been empirically documented as factual beyond any reasonable doubt (Handsteiner et al., 2017; Ma et al., 2012; Salart et al., 2008; Stefanov et al., 2002). Meanwhile, they are the basis for various new fields of research and application, from quantum computing to quantum encryption. And some think they are the basis for our neural operations as well (Hameroff & Penrose, 2014). Therefore, it might be reasonable to assume that such lawful, yet a-causal and non-local relationships could also play a role in the wider area of human affairs or in macroscopic nature. However, this would necessitate that either physical entanglement correlations that are normally only detectable under highly controlled and artificial conditions can also be preserved to some degree in the macroscopic environment; or that there is an equivalent to physical entanglement correlations that are exactly those meaningful correlations Jung spoke of, but not necessarily of a physical nature. Such correlations might be, for instance, systemic, i.e., pertaining to the general setup of a system of different physical constituents, and not only strictly physical in nature (Atmanspacher et al., 2002; Lucadou, 1995, 2015b). This is the path some of us have chosen, in assuming that there is a generalized form of entanglement that is operative in various types of systems, provided they have a certain structure (Filk & Römer, 2011; Walach & von Stillfried, 2011a, 2011b). We assume that parapsychological effects are due to such correlations, lawful, yet not causal, regular, yet not local (Lucadou, 2015b; Walach et al., 2014).

The No-Signal-Transfer (NT) Axiom and the Development of the Matrix Experiment

A corollary of this assumption is that if such correlations are mistaken for causal-local regularities and could be potentially used as such they will either change channel, i.e., show up in the control condition, or they will reverse signs, i.e., become significantly weaker, or are seen in different parameters. The reason for these observations is given by the fact that physical entanglement correlations must not be used as causal signals, and this can be formally proven (Lucadou et al., 2007). We therefore *assume* that this no-signal-transfer axiom (NT-axiom) also holds in the generalized case, although here it cannot be proven to be true, but is assumed to hold.

This NT-axiom states:

> *If a system is governed by non-local correlations but is treated as if the correlations were local causes, and if a signal is extracted from it, or could be extracted in principle, then those purported signals will break down in a second experiment, or when so used.*

This means that experiments on systems constituted by such non-local correlations that are repeated, constitute a violation of the NT-axiom and are likely to demonstrate a breakdown of such effects, either by a dwindling of the effect size or by demonstrating paradoxical effects, such as having the effect show up in the control group, or changing its sign. It also means: This is only applicable to *replications* of experiments. We will return to this problem in the Discussion.

Indeed, there is a series of empirical hints that testify to the ubiquity of this phenomenon in parapsychology. A recent example is a commissioned identical replication of a previously reported experiment in which mental effort of trained meditators was supposed to affect an interference pattern in a standard double-slit optical setup (Radin et al., 2012). The strictly preregistered and controlled study came out negative (Walleczek & von Stillfried, 2019), although an effect can be seen in a completely different channel, in the variance. The same was found in the multisite replication of the PEAR Lab's micro PK-experiment (Jahn & Dunne, 1987). Although a case can be made that the PEAR Lab's database was largely due to the effect of some gifted subjects, the consortium replication between Princeton, Freiburg, and Giessen was predefined as a large replication study of the PEAR Lab procedure in a protocol, in which Walter von Lucadou also predicted the negative result.

The replication was negative (Jahn et al., 2000), but secondary parameters that were not logged in the protocol, variance and non-linearity parameters, were clearly significant (Pallikari, 2001). Maier and colleagues had the same experience in a series of PK- and priming experiments (Dechamps & Maier, 2020; Maier et al., 2014; Maier & Dechamps, 2018; Maier et al., 2018). This has also been observed in other datasets from parapsychology (Bierman, 2000).

We started from the assumption that, if this lack of causal stability is to be expected there might be a workaround by testing for some kind of indirect parameter that would prohibit the coding of a causal signal. Standard experiments that use a control group are, by default, cause detectors and thereby allow the coding of a signal. For instance, they yield a result which in a strict replication experiment could be used to code a signal: Scores above or below the mean of the first experiment would be the signal to code a 1 or a 0. This, however, would violate the NT-axiom and hence would cause the effect to vanish or change track, if our model is correct. Therefore, we sought an experimental setup that would be as immune as possible to such potential violation.

One such setup was developed by Walter von Lucadou in what he called the "Matrix Experiment", meanwhile referred to as the "Correlation Matrix Method – CMM" (Lucadou, 1974, 1986, 1987a, 1987b, 1991, 2006, 2015a; Lucadou et al., 1987). The idea here is not to define a clear outcome parameter which would be prone to violating the NT-axiom, as it would allow for signal coding, but to use an array of variables in a correlation matrix. The correlation matrix reflects the correlation of the interaction of human intentions or human behavior with a physical system that is otherwise locally decoupled. In our case the physical system was a micro-psychokinesis (micro-PK) experiment. A computer displayed a fractal, a Julia-set, whose change—growth or shrinkage—was driven by a random event generator (REG). That means the behavior of the fractal could not have been influenced by ordinary means of interaction. However, human participants were instructed to do so by their intentionality and were asked to move the sampling process of the random event generator forward by pressing either one of two keys on the keyboard of the laptop computer that ran the experiment. These keystrokes represent the psychological or behavioral variables, while the behavior of the physical system represents the physical variables. These variables can be correlated across all experiments and all participants and yield a correlation matrix. If the correlation matrix contains a signature of the intentional effect or the entanglement effect of participants with the experiment or physical system, then we would expect more significant correlations than by statistical chance expectation or in a control experiment that is run without a participant present.

Indeed, von Lucadou's previous experiments were supportive of this idea and produced more significant correlations than expected by chance and more than seen in a control matrix. Thus, we set out to replicate this experimental setup with a larger, well-controlled experiment. We rebuilt the hardware and software—the random event generator and the control software—from scratch and enlarged the matrix into a matrix of 45 psychological and 45 physical variables (because there were 5 such variables per run and 9 runs made up an experiment), yielding a matrix of 2025 cells. We created a robust non-parametric system of statistical evaluation by simulating 10,000 such experiments and deriving the statistical significance from it. This first large replication in two labs yielded a significant but fragile effect, as significance broke down in reasonably improved methods of analysis (Walach et al., 2020).

We then convened an international consortium of experts to arrive at a consensus protocol. This protocol followed our original one quite closely with a few exceptions (see Methods section below) and formed the basis for future replications. One such replication was conducted by Karolina Kirmse as part of her master's thesis under the supervision of Peter Sedlmeier. Another replication was conducted by Hans Vogt and Harald Walach. Both replications came out negative. We report on these replications in this paper and will end with a few ideas about potential ways forward and why we think it will be a difficult challenge to experimentally prove anomalistic effects using experimental models (Rabeyron, 2020).

METHODS

We used a predefined protocol that was the result of a consensus meeting of experts. The studies reported here are in fact replications of the parent study (Walach et al., 2020). The protocol was defined and published beforehand on the Open Science Framework platform (https://osf.io/cx2tf/). Since it is described there in detail, we will only summarize the most important elements here. The experiment is a comparatively strict replication of the parent study, as the same equipment, the same material, and the same procedures were used with only a few exceptions that are described below. The criterion for a successful replication was a significant result as determined by a statistical randomization test (see below). Since we do not assume a stable, causal effect, a standard power analysis is not part of the protocol but can only be provided as a post-hoc analysis.

Material and Participants

We used the same equipment as in the parent experiment. KK was lent one of the four REGs that were used for the first experiment and received a copy of the software program that operated the experiment. This software program was custom written in C following the first code which was programmed in Basic. This program operated the experiment automatically, prompted the experimenter and the participants for inputs, and wrote the data into a file.

The first replication was conducted by KK in Dresden with a broad group of participants recruited mainly in public spaces, the second by HV in Witten with a group of students of psychology, gaining course credits through their participation. The experiment was advertised as an experiment in extraordinary facilities and was conducted face to face, one after the other. Before the experiment started, the experimenter switched on the computer and the equipment with a lead time of half an hour to allow for drift and warming up.

The experimenter greeted the participant and briefly explained the experiment and handed out a consent form, as well as a very short questionnaire. The questionnaire data were deliberately not used, as in the previous experiment, but had the function of involving the participant with the experiment. In Experiment 1, however, this questionnaire was extended and evaluated for exploratory purposes (see below). When the participants were ready, the experimenter started the program and left them alone. The participant could take as long as necessary. They had the instruction to "influence the movement of the fractal on the screen" in the indicated direction and knew that they had to press either of two shift keys on the computer keyboard to move the sampling process forward. Each time either one of these shift keys was pressed, the REG was sampled and the result was used to generate a movement of the fractal displayed on the screen. The sampling process was filtered by a Markov chain instead of the frequently used XOR-filter. This was done for two reasons. First, Markov-chain filtering makes a process smoother and look more natural. Most natural processes, like the weather, are Markov processes, i.e., they contain one or two lags of memory. Second. the Markov process preserves some of the physical properties of the REG. Perceptually, this resulted in the appearance of a very smooth movement of the fractal. What the participant did not know was that when both shift keys were pressed, the sampling process would go on until one of them or both keys were released. The sampling process was repeated 80 times, since each sub-run consisted of 80 such trials, and 3 sub-runs with three different instructions made up one experiment. Each run was associated with a specific instruction to either grow or shrink the movement of the fractal or keep it constant. These instructions were conveyed by red arrows on the screen, and each instruction was repeated 3 times at random. Thus, a full experiment consisted of 3*3 sub-runs with 80 trials or 720 data-points. In contrast to the parent experiment, each participant conducted only one experiment.

Outcome Variables

For creating the 45 x 45 correlation matrix, five behavioral psychological and five physical variables were generated.

The five behavioral psychological variables were generated by the behavior of the participants and defined as follows:

T1: Number of left key presses

T2: Number of right key presses

T3: Number of double key presses

DR: Mean time between key presses, i.e., speed

DV: Mean variance between key presses, i.e., constancy of behavior

The five physical variables were associated with the behavior of the random event generator (REG) and derived from the following values:

TR: Number of times the output of the Markov-chain parsing of the REG yielded "1" during one run, i.e., the physical behavior of the REG filtered by the Markov chai

DT: The number of steps the fractal display deviated from the experimental instruction in either direction or from the central position, i.e., this is the summarized number of steps the fractal deviated from the goal

KR: deviation of the actual physical output of the Markov chain from an ideal Markov chain, measured as the deviation of the theoretical autocorrelation function from the experimental autocorrelation function of the sub-run calculated over 10 steps

ZT: mean voltage output of the REG at channel 4 out of eight; this channel was defined a priori as the one where the voltage would be recorded, because it was the middle channel and hence least likely to be affected by currency changes due to physical switching processes; the other channels were measured but the data not checked and analyzed

ZV: the standard deviation of this voltage output at channel 4, i.e., of the variable ZT.

There was continuous voltage applied to the Zener diode which triggered a current. This randomly changing current was converted by analogue–digital converter. Each time a key press was enacted the converter was sampled. If the number of bits was smaller than the previous one the outcome was 0, if it was larger the outcome was 1, and if it was equal a new sampling was initiated.

The 5 variables were calculated for each run per participant. As each participant had nine runs, this yielded 45 behavioral-psychological and 45 physical variables. These variables were correlated across all participants, which together produced the 45*45 matrix with 2025 cells. These cells were filled by the Spearman rank correlations coefficients between the respective variables across participants. We counted the number of correlations significant at the predetermined level of $p < .1$ (one-sided, or .05 two-sided). This is arbitrary and followed previous practice and our protocol. We also report sensitivity analyses for correlations significant at a lower p-value than that. The idea behind the testing procedure is as follows: In each correlation matrix there is a number of correlations significant at a certain level by chance. For instance, in a matrix of 100 cells there would be 5 correlations expected to be significant at the level $p = .05$ or 10 at the level $p = .1$. Similarly, in a matrix of 2025 cells we would expect 202 to 203 correlations to be significant at a level $p = .1$. Therefore, we counted the number of correlations significant at the level $p \leq .1$ and tested (see below), whether this number of significant correlations found empirically was significantly different from a chance finding, using a randomization test, or different from the number of correlations found in a control experiment.

Control Experiments

After each participant had finished his or her experiment, the experimenter started a control experiment and then left the room. The control experiment consisted of the physical equipment running empty. This resulted in the generation and recording of the physical variables (TR, DT, KR, ZT, ZV as described above) without interference or interaction from a participant, sampling as many data points as during a real experiment. The generated array of physical variables was automatically written into a database, and the psychological variables of the previous experiment copied into the control database as corresponding psychological variables. Thus, each real experiment was matched by a control experiment with the same set of psychological variables, whereby all potential causal and non-causal effects were transferred into the control database and correlated with a new set of independent physical variables.

Special Features of the Two Experiments

Experiment 1, conducted by KK in Dresden, had, in addition, the following features: Instead of performing just one control experiment at the end of each session, a second control experiment was carried out at the beginning. In this way, the scope of the comparison was expanded. Furthermore, the questionnaire used in previous matrix experiments was modified by adding state variables identified as particularly psi-promoting (see Braud, 2002) and discussed by the matrix experiment consortium against the background of the Organizational Closure. These variables served as foundation of data to exchange the psychological variables (keystrokes) with the questionnaire data, using the "Phenomenology of Consciousness Inventory" (Pekala, 1995) in an additional analysis and to perform explorative analyses to examine which variables, determined as favoring psi effects, influence the number of significant correlations. The questionnaire was implemented in an online format on the computer where the experiment took place. In addition, this questionnaire was continued after the experiment had been performed in order to allow a comparison of the participants' states before and after the experiment. The experimenter was blinded to the responses during the experiment; only after the experiment was completed were the answers inspected.

Experiment 2, conducted by HV in Witten used two additional features: There was a switch implemented that allowed the system to choose between two types of REGs. One was the custom-made REG that was also used by KK, identical to the ones from the first experiment. The second was an off-the-shelf REG called TrueRNG which can be easily purchased and implemented via a USB-stick. The idea was to see whether our elaborate sampling process would really be better or whether we might be able to offer a simpler system for wider usage. A coin toss at the beginning of the experiment decided which REG would operate the experiment. The second feature referred to the implementation of an assessment of absorption (Glicksohn, 2001; Glicksohn et al., 1992; Watt & Tierney, 2013). It consists of measuring objective time the experiment takes and then asking participants to estimate the time they took to conduct the experiment. The difference can serve as a measure of absorption, as more deeply absorbed participants tend to underestimate the time (Sedlmeier et al., 2020).

Ethical clearances were given by the respective ethical boards.

Statistical Analysis and Data Preparation

Data analysis followed the predefined protocol and consisted in a randomization test as specified. Briefly, an analysis script was written in Matlab to reshuffle the data 10,000 times and to recalculate the correlation coefficients each time (see Appendix). For every permutation step, the number of significant correlations in the matrix was counted. The number of times out of those 10,000 permutations where an equal or larger number of correlations was found than in the empirical matrix, divided by 10,000, yields an estimate of the true probability that the empirical result or a more extreme one could have been found by chance.

The experiment might be challenged to be open to systematic causal coding, for instance if someone used a certain strategy such as hammering on the keyboard, or always alternating shift keys, there might be causal correlations between physical and psychological variables. Therefore, we defined a sensitivity analysis: We analyzed only those correlations that are found in the time-forward or upper part of the matrix. As the matrix unfolds 9 * 5 psychological variables in rows and 9 * 5 variables in columns the correlation of the first set of physical variables with the second set of psychological variables is a time-forward correlation of physical variables in the first run with psychological variables in the second run, which should preclude all causality, as causality normally does not run backwards in time.

In the second experiment, the data for the TrueRNG in Experiment 2 were found to not conform to expected behavior (Appendix Figure 1 and Appendix Figure 2). Closer inspection revealed that this was due to a newly acquired programming glitch when programming the switch between the REGs that led to a buffer overflow for the data coming from the TrueRNG. We normalized the data and after normalization they conformed well to chance expectation (Appendix Figure 3 and Appendix Figure 4). The programming mistake was corrected for subsequent usage.

RESULTS

Experiment 1—Dresden Experiment by KK

Sixty-four participants were recruited, 43 females (67%) and 21 males (33%). Due to the layout of the questionnaire, which used the original one by Walter von Lucadou, age was only available in categories. The category of 41 to 50 years was the modal one with 17 participants. Two participants were below 20 years, six were below 30, and 14 were between 31 and 40. Sixteen participants were between 51 and 60, seven were between 61 and 70 and one person was older between 71 and 80.

The results of the statistical analysis of Experiment 1 can be seen in Table 1. (Appendix Table 1 presents the data together with the results of the control matrices.)

TABLE 1
Result of Statistical Analysis (Permutation Test with 10,000 Iterations) of Experimental Matrix, Full 45*45 Matrix, Experiment 1.
Yellow: Significant Results; Red: Missing Significance

45x45	**sig_th**	**0,1**	**0,05**	**0,02**	**0,01**	**0,005**	**0,002**	**0,001**	**0,0005**	**0,0002**	**0,0001**
full	**zo**	245,00	141,00	69,00	37,00	13,00	10,00	6,00	4,00	1,00	1,00
full	**n_sim**	1069	594	401	551	2588	611	559	390	963	417
full	**p_sim**	0,107	0,059	0,040	0,055	0,259	0,061	0,056	0,039	0,096	0,042
part	**zo_part**	105,00	44,00	17,00	9,00	1,00	1,00	0,00	0,00	0,00	0,00
part	**n_part_sim**	2023	4854	4793	4057	8230	4612	4874	3042	1447	809
part	**p_part_sim**	0,202	0,485	0,479	0,406	0,823	0,461	0,487	0,304	0,145	0,081

sig_th: theoretical significance level at which the number of significant correlations is counted
zo: number of significant correlations empirically found at respective level
n_sim: number of simulated matrices out of 10,000 with significant correlations at or above the number found empirically
p_sim: actual significance level of observed number of correlations (n_sim/10,000)
zo_part: number of correlations in time-forward (upper) part of the matrix
n_part_sim: number of significant correlations found in 10,000 simulations at respective level in upper part of the matrix
p_part_sim: actual significance level of observed number of correlations (n_part_sim/10,000) in upper part of the matrix

The first line of Table 1 presents the significance level at which the numbers of significant correlations are counted, the second line gives the empirically found number of significant correlations at that level. The number of simulated matrices with significant correlations at or above the number found empirically out of 10,000 simulations follows in the next line, and the p-level is given by this number divided by 10,000. The red color indicates which one of those statistical tests did not reach formal significance, while the yellow color indicates significance. The lower part of Table 1 reports the same for the upper diagonal of the correlation matrix, which contains only time-forward correlations, i.e., the correlation of the physical variables in the first run with the psychological variables of the second run (abbreviated as "part" in Table 1). This contains the causally independent parts of the correlation matrix because they are time-forward.

Only for some of the levels of significance were there more significant correlations than expected by chance (remember that $p \leq .1$ was the predefined level), namely for correlations at the level of $p \leq .02$, $p \leq .0005$, and $p \leq .0001$. The number of significant correlations at $p \leq .05$ and $p \leq .01$ miss formal significance by a small margin. The number of significant correlations at the predefined level of $p \leq .1$ is not significant.

While in the original experiment (Walach et al., 2020) we found significant correlations beyond chance even in the time-forward upper part of the matrix, overall none could be determined in this case.

We also analyzed smaller matrices (27*45, 18*27) which correspond to the setup of previous experiments by Walter von Lucadou and can be considered as replications of the earlier experiments. None of them showed any consistent and clear-cut results (Appendix Table 2 and Appendix Table 3).

Experiment 2—Witten Experiment by HV

The experiment conducted by HV in Witten recruited 40 participants, all of them students at the university and most of them psychology students who received course credits. Thus, all of them were between 18 and 30 years old. In that experiment we also measured time and had participants estimate the time of the experiment. On average, participants estimated the experiment as 0.4 minutes shorter than it actually was, which is a sign of modest absorption or closure. Twelve participants reckoned that the experiment took longer than it actually took, and thus were likely not very involved. The result of the statistical analysis is given in Table 2. Graphical representations of the experimental matrices of Experiments 1 (KK) and 2 (HV), as well as one of the control experiments (by KK) are presented in Figures 2, 3, and 4.

As can be seen, at none of the evaluated levels of significance do we find more significant correlations than expected by chance, neither in the full matrix, nor in the partial one. The same is true for the smaller matrices (27*45, 18*27 matrix; data not shown). Because there was no systematic effect in the first place, further analyses as to the efficacy of the two different REGs or the importance of organizational closure, measured as absorption were no longer useful. The control matrices did not show a significant effect either.

Taken together, none of the two experiments corroborates our original findings and the replication must be considered failed.

TABLE 2

Result of Statistical Analysis (Permutation Test with 10,000 Iterations) of Experimental Matrix, Full 45*45 Matrix, Experiment 2.

Yellow: Significant Results; Red: Missing Significance

45x45	sig_th	0,1	0,05	0,02	0,01	0,005	0,002	0,001	0,0005	0,0002	0,0001
full	zo	160,00	89,00	35,00	18,00	7,00	2,00	0,00	0,00	0,00	0,00
full	n_sim	9437	7347	6865	5981	7219	7206	8287	6162	3395	1966
full	p_sim	0,945	0,735	0,687	0,598	0,722	0,721	0,829	0,616	0,340	0,197
part	zo_part	79,00	46,00	19,00	12,00	4,00	0,00	0,00	0,00	0,00	0,00
part	n_part_sim	7329	4308	3963	2234	4702	7848	5587	3556	1700	927
part	p_part_sim	0,733	0,431	0,396	0,223	0,470	0,785	0,559	0,356	0,170	0,093

sig_th: theoretical significance level at which the number of significant correlations is counted

zo: number of significant correlations empirically found at respective level

n_sim: number of simulated matrices out of 10,000 with significant correlations at or above the number found empirically

p_sim: actual significance level of observed number of correlations (n_sim/10,000)

zo_part: number of correlations in time-forward (upper) part of the matrix

n_part_sim: number of significant correlations found in 10,000 simulations at respective level

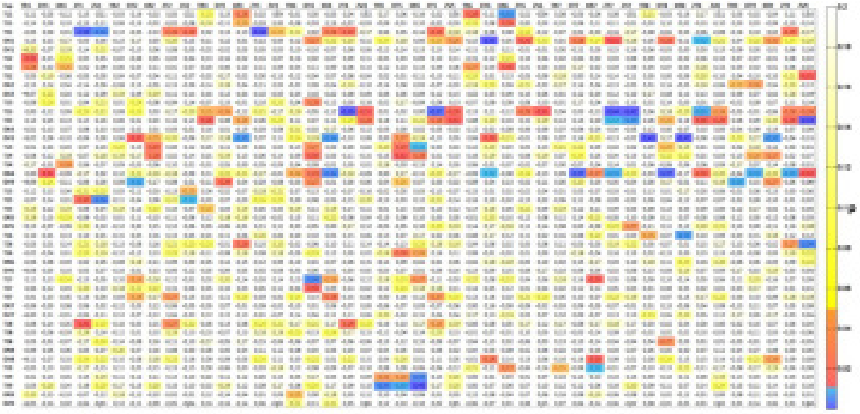

Figure 2. Experimental Matrix of Experiment 1 (KK).

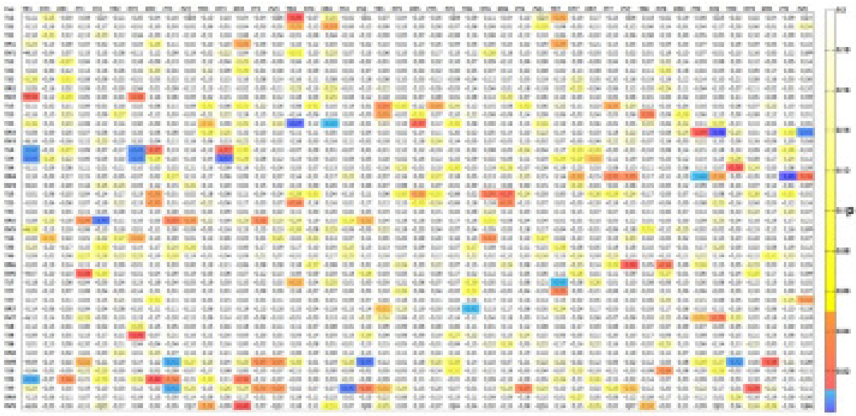

Figure 3. Experimental Matrix of Experiment 2 (HV).

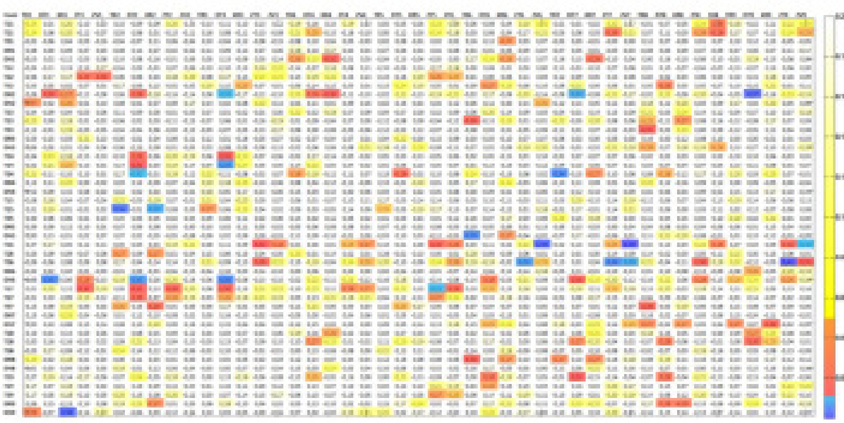

Figure 4. Control Matrix of Experiment 1 (KK).

DISCUSSION

Our hope that we might be able to replicate our earlier positive finding and those reported by Walter von Lucadou (Lucadou, 1986, 1987b, 1991, 2000; Lucadou et al., 1987; Walach et al., 2020) did not bear out. The two experiments, reported here, were part of a concerted effort to find a replicable experimental model that would circumvent the NT axiom. This prohibits signal coding for anomalous experiments which are supposed to operate on the basis of generalized entanglement correlations. Circumventing the NT axiom was not possible. By the same token, our negative results also preclude an anomalous signal. For had such an anomalous signal been there, we would have been able to see it, as it would have driven one of our variables (TR) that measures the deviation of the REG from randomness, and thus produced a series of significant correlations. This result has to be seen against potential weaknesses and against other results, partially positive and partially negative.

A major weakness of our experiments is that they are comparatively small. So, one could argue that they did not have the necessary power. While our predecessor experiment had 503 participants, these new experiments only had 104 participants together. Using the effect size of our predecessor experiment, approximately $r = .38$, our smaller experiment had a power of 69% and the larger one a power of 88% to detect the effect. Precisely because we assume that the effects are of a non-classical, non-signal–like nature a classical power discussion is beside the point, we contend. A classical power analysis assumes that there is a stable effect that can be detected, given enough resources. We do not think that this is the case. This is the reason why in our consortium protocol power analysis is not part of the protocol, but only definition of recruitment procedures and a preclusion of optional stopping. As we argue below, power is not the decisive issue, as there are various instances of strongly powered and well-prepared replications that were unsuccessful.

This lack of success in replication is not a problem of personal factors, as these experiments were conducted by two independent groups following the same protocol and using the same equipment. Rather, it feeds into a stream of similar results: Walleczek and von Stillfried (2019) were unable to replicate the Radin double-slit experiment, a careful replication in which Radin himself was involved, conducted the experiment according to a predefined protocol, and analyzed the data according to previous standards. Rabeyron was unable to replicate Bem's retro-priming results (Rabeyron, 2020). Maier and colleagues were unable to replicate earlier results and found higher-level regularities, i.e., an effect that moves in a kind of sinusoidal wave from positivity to negativity and potentially back again (Dechamps & Maier, 2020; Maier & Dechamps, 2018; Maier et al., 2018). The matrix experiment was repeated in a different form by Grote, who could not find clear-cut effects either (Grote, 2015, 2017). A newly designed experiment by Grote which replicated the general setup of the matrix experiment with new equipment and 200 participants was unsuccessful. This demonstrates that power does not seem to be the issue. However, an analysis of correlations of the same physical data of this experiment with different psychological variables, in that case questionnaire data obtained from each participant before the experiment, was marginally significant ($p = 0.064$) (Grote, 2021). Jolij and Bierman conducted two replications of Bem's retropriming paradigm, but found no effect (Jolij & Bierman, 2019). However, when they analyzed the questionnaire data that were also taken together with the psi data in a matrix analytical approach, they found a significant result ($p < 0.03$) in one experiment, the smaller one with 61 participants, and a borderline significant effect in the second study ($p = 0.06$) with 222 participants. This is again a clear hint that the decisive question is not about power.

These results have to be seen together with experiments by Ana Borges in Edinburgh who has conducted three experiments herself with clearly positive results and one commissioned by another experimenter with negative results (Ana Borges, personal communication and unpublished Ph.D. thesis, The University of Edinburgh, Department of Psychology).

The results of the experiments of Ana Borges can only be really discussed once they are fully published. Meanwhile one might suppose that in those experiments we are dealing with an experimenter effect, as the study conducted by a second experimenter who was indifferent to the results was clearly negative, while the studies conducted by Borges herself, who is enthusiastic about this work, were positive. We had such a setup implicitly in our experiments: KK tended toward hoping to find positive results, while HV was pretty indifferent toward the results of the experiment. Was the negative effect of HVs experiment a negative experimenter effect? As the CIRTS theory would suggest, all experiments might be in principle tests of experimenter PK (or the lack of it) (Bierman, 2008).

A strength and weakness of our experiment at the same time was the statistical analysis. The Monte-Carlo simulation of potential different matrices produces an empirical distribution against which statistical inferences can be made without any parametric assumptions and is thus a straightforward, non-parametric analysis. It is comparatively stable: The p-values change maximally by 10^{-3} and the values of significant simulated matrices by around 30, if the 10,000 iterations are repeated 30 times, i.e., instead of 1,069 significant matrices which translates into $p = 0.106$ we would have 1,099 significant matrices which translates into $p = 0.109$. It also corrects for potential causal biases, as these are destroyed in the permutations.

But such an analysis also destroys the intricate network between potential causal and non-local correlations, making the analysis conservative. The type of analysis chosen and defined in the protocol actually uses only the experimental matrix. One could also use difference scores between the experimental and the control matrix and other metrics for the statistical analysis. We have done that for exploratory purposes. But this does not change the result.

An optimal analysis might be able to use some difference metric between the control and the experimental matrix. One might argue that the effect is embedded within the whole experiment and not only within the experimental matrix. Thus, some difference measure between the two matrices might be better able to capture the effect. This is for a subsequent analysis of the data to decide.

In our view, the results seem to suggest a decline effect as observed by Maier and colleagues: Our own first experimental results were the stimulus for further work. They were very positive. The experiments of Ana Borges were immediate successor experiments timewise and were also positive. KK's experiment was next and had a small, nearly significant effect. HVs experiment was the last in this series and had a zero effect. This supports a decline effect and contradicts our expectation that the matrix method might help to mitigate such a decline. A decline effect is a prediction of our model (Lucadou, 2015b; Lucadou et al., 2007; Walach et al., 2014): The NT axiom states that whenever effects due to generalized entanglement correlations are mistaken as causal effects and could be used for signal transmission. the effects go away (decline), or change channel, i.e., become visible in another parameter not tested, or change sign, i.e., become obvious in the control group.

Obviously, the NT axiom (Lucadou et al., 2007) cannot be circumvented as we had hoped. It may take longer before a decline comes into effect. But eventually there is no experimental system that generates its own comparison standard through a control group that can elude it. For no matter how complex the system or how many degrees of freedom, eventually there will always be an option to code a signal. In our case it would have been the number of significant correlations.

We had similar experiences with other experimental models. A careful pilot study of a DMILS replication, in which we tried to replicate the originally successful DMILS studies of Schlitz and Braud (Braud & Schlitz, 1983; Braud & Schlitz, 1991; Schlitz & Braud, 1997), yielded a strong positive effect of r = .35, which was, however, not tested statistically as per protocol (Schmidt et al., 2001). A large replication with sufficient participants for detecting a much smaller effect failed utterly (Schmidt, 2002; Schmidt et al., 2002). We replicated the Grinberg-Zylberbaum study in which he had claimed that a visual stimulation of one subject had introduced transferred evoked potentials in the EEG of a spatially distant, but connected participant (Grinberg-Zylberbaum et al., 1994). In our study we could not find transferred potentials as such, but significant deviations from chance expectations (Wackermann et al., 2003). Harald Walach commissioned two large-scale replications in the same lab, which were clearly positive, but never published (Claudio Naranjo, personal communication; he had conducted the studies but was prohibited from publishing the data by Wackermann, the former head of the lab). We thought we had a replicable, if complicated paradigm and conducted another replication which was meant to be completely foolproof against fraud and artifacts, as it was between subjects separated by about 800 kms. But we could not find the effect in its original signature. We found an effect in the alpha frequency band which was significant in three studies. However, the relevance of this effect remains unclear as it only showed up after averaging thousands of trials. Instead, we saw an unexpected anticipatory or precognition effect (Hinterberger et al., 2008, 2007). The reverse priming study by Daryl Bem (Bem, 2011) did not prove to be as replicable as hoped either (Jolij & Bierman, 2019; Rabeyron, 2014; Ritchie et al., 2012).

It seems we have enough controversial data and failed replications. It is important to note at this point: Failed replications and positive results in meta-analyses do not contradict each other. It is possible that in a long series of experiments some very careful negative replications, although they might be important, either do not (Schmidt et al., 2004b), or only partially (Bösch et al., 2006) influence the summary result of the meta-analysis, because many other positive results are published or because effects that have been negative in the hands of one research group recover in other labs (Bierman, 2001). This is to be expected under the NT axiom, since it only applies to strict replications. As soon as parameters are changed, and they usually are when other groups replicate an experiment, it is, technically speaking, a new experiment, even though it might use the same experimental model and will be analyzed under the same umbrella by meta-analysts. Thus, one way out of the conundrum would be to conduct replications as conceptual replications, changing important elements in an experimental paradigm so as to prevent it from being a direct replication which could be used for signal coding.

Another thought might be worth considering: If our hypothesis is correct and generalized entanglement correlations exist and are the basis for most, if not all PSI phenomena, then we need to consider the fact that in real life they are normally always embedded in a series of local-causal correlations which also support and frame them, like water is supported in a sponge (Lucadou, 2019). In the experimental situation we are trying to separate the two out, squeezing the sponge, as it were, and then are surprised to find the structure and the water gone.

Thus, the current situation is an impasse: The directly replicable paradigm that critics demand seems to be impossible. The fact that so many studies have been conducted by different groups and in slightly varying designs allows meta-analysts to draw positive conclusions. Hence, both skeptics and proponents of PSI are right and wrong at the same time. The “Dodo bird verdict” which has beset psychotherapy research is valid here as well: All have won and all must have prizes (Luborsky et al., 2002; Rosenzweig, 1936). It has been pointed out that this constitutes a paradox: If PSI is real, as a lot of the data suggest, then by the same token it cannot be proven experimentally, because the experimental paradigm presupposes the possibility of partitioning reality into independent segments, which is exactly what PSI negates (Rabeyron, 2020).

What this series of replications together with other evidence shows, is in our view that a causal, signal-theoretical interpretation of PSI is unlikely. It rather strengthens, even though indirectly, an analysis and theoretical model that assumes these effects to be instances of generalized entanglement correlations, or similar processes. If so, critics will remark: Why is it that entanglement correlations could be empirically proven in the physical case, but not in such a generalized case as in parapsychology? The answer to this question is straightforward: In the physical case we have a very strong formalism that allows the derivation of expectation values or empirical bounds that are theoretically defined, such as Bell's inequalities. This defined frame is not given in the generalized case because the model is not strong enough and does not contain enough quantitative terms that would allow such a derivation. In the physical case, only combinations of for example polarization angles are measured, and whether they are correlated or not is *not* determined by an experimental control group of different or incompatible angles, but by the violation of Bell's inequalities, i.e., by the theoretical distribution of two joint probabilities. This is structurally completely different from determining the control standard by a control experiment. As long as we do not have an equally strong theoretical framework, we will not be able to provide a straightforward proof of the facticity of generalized entanglement correlations.

Proponents of remote viewing experiments often lament about the inadequacy of experimenting with people who have no special gift for PSI, as is the rule in experiments like ours (May et al., 2018). They liken it to trying to judge musical prowess in an average group of people, some of whom might be musically gifted while the majority won't be, diluting the end result. Experimenting with gifted people might help avoid this pitfall. However, it was estimated that this will be maximally one or two in one hundred (May et al., 2018). While this argument is certainly convincing in part, it conflates two distinct points: Working with gifted people is certainly a good idea. But this does not preclude failures, as the failed replication by Walleczek and von Stillfried (2019) showed. The remote viewing experiment is not an experiment in the sense the term is used here. And this might be the reason why remote viewing experiments cannot violate the NT axiom and hence can produce quite stable results (Targ, 2019; Targ & Katra, 2000).

In remote viewing there is no control standard that is *produced by the experiment*. The control is the expectation of no special information transferred, which is a generic null-expectation. Therefore, it can be replicated at will. The NT axiom would only come into effect in the counterfactual situation, which by definition never exists, if the same person were to target the very same target twice. But the same remote viewer will not normally do this, and once a target is described there is no point in having this repeated. Also, in experimental setups that are similar, targets and participants are normally changed, thus implicitly avoiding the NT axiom. Therefore, some free-response remote viewing or telepathy studies might be able to eschew the NT axiom, but all studies that produce their own control standard in a control group and are replicated as an exact replication will have the same problems as we experienced. Unfortunately, remote viewing and Ganzfeld telepathy studies belong to a category where a lot of expert knowledge, material, and facilities are necessary and hence do not lend themselves to the type of classroom experiment that is set up quickly and easily to demonstrate telepathy.

Thus, we might have to live with the fact that a definitive experimental paradigm is very difficult, if not impossible, to have. As long as a paradigm incorporates enough changes, for instance by way of conceptual replication, or changing variables, or outcome measures each time it is conducted, it may eschew the NT axiom. But by the same token it will also be less convincing to skeptics, who will keep demanding a strict replication. Thus, skeptics will likely have an easy life: They won't be bullied into acceptance by a foolproof experimental paradigm of PSI, because it simply may not exist. So, is experimenting, then, unnecessary and a waste of time and resources? Probably not, because it might teach us about higher order parameters, such as the recovery time it takes until an effect bounces back, or about the amount of change necessary to make an experiment conceptually a new one (Dechamps & Maier, 2020; Maier et al., 2018). Or it might help decide between theoretical options (Bierman, 2010). Or it might yield a higher class of models that not only predict when an effect might appear, but also when it will go away. Experimenting might thus also produce the parameters necessary to build a fuller model that contains enough richness to derive a formally more stringent theory.

But we should probably give up the hope that the intellectual fight about whether anomalous cognition effects or PSI is real, can be won with the brute force of rational argument and experimental evidence alone. This is very rarely the final arbiter anyway, even for very mundane questions, where social movements, intellectual fashions, generic worldviews, political considerations are often much more important (Latour, 1999). Perhaps a mixed approach will be best: devising clever experiments, avoiding the pitfalls of the NT axiom by changing procedures in replications, not forgetting qualitative real-world studies, observations of natural occurrence of PSI and analytical arguments combating the prevailing naturalistic stance that is more of a dogma than an intellectual necessity (van Fraassen, 2016; Williams & Robinson, 2016). All this together might help opening up the community for the possibility of PSI. Producing a final proof is likely a vain expectation, as our results show.

Our conclusion is: The matrix experiment is likely not a replicable experiment. The NT axiom that prohibits signal transfer in systems that are built on correlations might be operative even in this sophisticated experimental design. This makes likely that such effects are not of a local-causal nature. In addition, artefacts might be operative in this highly complex study. There might be other regularities involved which we do not understand as yet, but we can preclude signals with a high likelihood, else we would have seen their effect. Future studies should determine if conceptual replications of the matrix experiment changing important elements and parameters can avoid the NT axiom. In addition, further research efforts could advance the experimental setup of the matrix experiment/CMM, transferring it to other psi areas.

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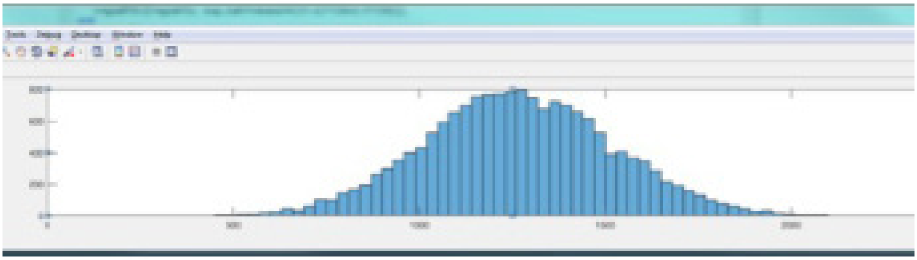
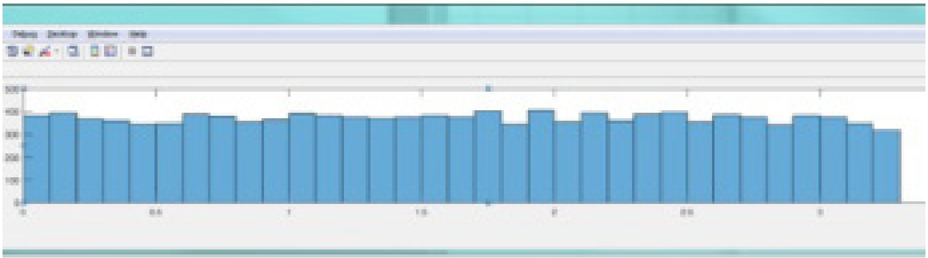
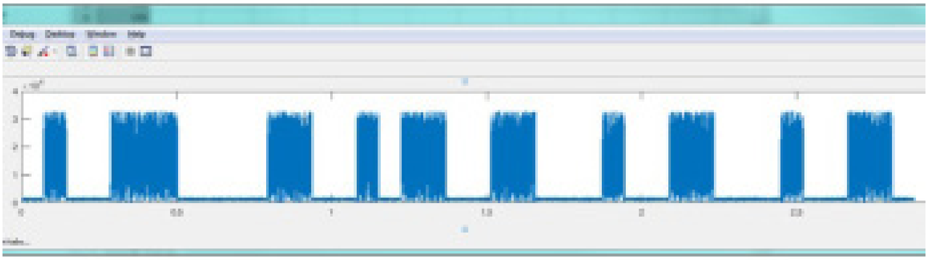
APPENDIX



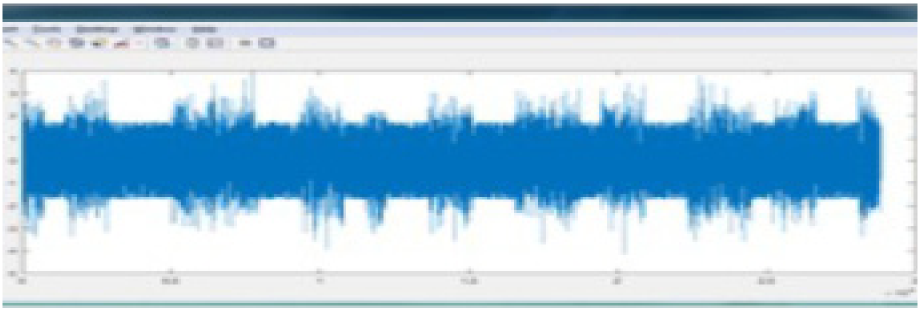
Appendix Figure 1. Distribution of sampling of True RNG.



Appendix Figure 2. Distribution of sampling of our traditional RNG.



Appendix Figure 3. REG-output of all REGs before normalization.



Appendix Figure 4. REG-output of all REGs after normalization.

APPENDIX TABLE 1

Number of Significant Matrix Elements in the 45 x 45 Experimental Matrix Compared to the Control Matrices C1 and C2 and to Chance Expectation Depending on Significance Level. Experiment 1 by KK, Original Analysis.

45 x 45 matrix	p-value (two-sided)	Number of significant correlations CE	Number of significant correlations C1	Number of significant correlations C2	Theoretically expected number of significant correlations	Difference between CE and C1	Difference between CE and C2
overall	0.1	246	226	241	203	20	5
part		115	83	121	99	32	-6
overall	0.05	141	111	134	101	30	7
part		52	32	70	50	21	-18
overall	0.01	39	22	19	20	16	19
part		11	1	7	10	10	4
overall	0.005	16	12	10	10	5	6
part		3	0	4	5	3	-1
overall	0.001	8	2	1	2	6	7
part		1	0	0	1	1	1
overall	0.0005	4	2	0	1	2	4
part		0	0	0	0	0	0
overall	0.0001	4	2	0	0	2	0
part		0	0	0	0	0	0

Because the data of this analysis were based on KK's own analytic strategy which is slightly different from that of TH who evaluated the data for this experiment statistically, some numbers deviate from Table 1.

APPENDIX TABLE 2

Statistical Analysis of Experiment 1 – 27*45 Matrix; Randomization Test with 10,000 Iterations

	27x45										
	sig_th	**0.1**	**0.05**	**0.02**	**0.01**	**0.005**	**0.002**	**0.001**	**0.0005**	**0.0002**	**0.0001**
full	**zo**	163.00	96.00	45.00	21.00	8.00	7.00	3.00	2.00	0.00	0.00
full	**n_sim**	697	439	511	1118	2482	585	990	676	1716	937
full	**p_sim**	0.0697	0.0439	0.0511	0.1118	0.2482	0.0585	0.099	0.0676	0.1716	0.0937
part	**zo_part**	83.00	36.00	13.00	8.00	1.00	1.00	0.00	0.00	0.00	0.00
part	**n_part_sim**	405	1817	2831	1903	5948	2727	3172	1879	838	446
part	**p_part_sim**	0.0405	0.1817	0.2831	0.1903	0.5948	0.2727	0.3172	0.1879	0.0838	0.0446

sig_th: theoretical significance level at which the number of significant correlations is counted
zo: number of significant correlations empirically found at respective level
n_sim: number of simulated matrices out of 10,000 with significant correlations at or above the number found empirically
p_sim: actual significance level of observed number of correlations (n_sim/10,000)
zo_part: number of correlations in time-forward (upper) part of the matrix
n_part_sim: number of significant correlations found in 10,000 simulations at respective level in upper part of the matrix
p_part_sim: actual significance level of observed number of correlations (n_part_sim/10,000) in upper part of the matrix

APPENDIX TABLE 3

Statistical Analysis of Experiment 1 – 18*27 Matrix; Randomization Test with 10,000 Iterations

18x27

	sig_th	0.1	0.05	0.02	0.01	0.005	0.002	0.001	0.0005	0.0002	0.0001
full	zo	63.00	33.00	18.00	6.00	3.00	3.00	1.00	1.00	0.00	0.00
full	n_sim	1667	1876	1015	2631	2381	722	1071	490	788	427
full	p_sim	0.1667	0.1876	0.1015	0.2631	0.2381	0.0722	0.1071	0.049	0.0788	0.0427
part	zo_part	30.00	11.00	4.00	1.00	0.00	0.00	0.00	0.00	0.00	0.00
part	n_part_sim	1606	3888	3778	4961	5211	2732	1600	870	376	215
part	p_part_sim	0.1606	0.3888	0.3778	0.4961	0.5211	0.2732	0.16	0.087	0.0376	0.0215

sig_th: theoretical significance level at which the number of significant correlations is counted
zo: number of significant correlations empirically found at respective level
n_sim: number of simulated matrices out of 10,000 with significant correlations at or above the number found empirically p_sim: actual significance level of observed number of correlations (n_sim/10,000)
o_part: number of correlations in time-forward (upper) part of the matrix
n_part_sim: number of significant correlations found in 10,000 simulations at respective level in upper part of the matrix
p_part_sim: actual significance level of observed number of correlations (n_part_sim/10,000) in upper part of the matrix

APPENDIX TABLE 4

Number of Significant Matrix Elements in the 45 x 9 Varied Experimental Matrix with Psychological Variables Obtained by Questionnaire Compared to the Control Matrices C1 and C2 and to Chance Expectation Depending on Significance Level; Experiment 1 by KK, Original Analysis

45 x 9 matrix

	p-value (two-sided)	Number of significant correlations CE	Number of significant correlations C1	Number of significant correlations C2	Theoretically expected number of significant correlations	Difference between CE and C1	Difference between CE and C2
overall	0.1	40	62	40	41	-22	0
part		36	54	38		-18	-2
overall	0.05**	24	26	23	20	-2	1
part		21	24	22		-3	-1
overall	0.01	7	6	7	4	1	0
part		7	4	7		3	0
overall	0.005	2	3	5	2	-1	-3
part		2	1	5		1	-3
overall	0.001	1	1	2	0	0	-1
part		1	0	2		1	-1
overall	0.0005	0	0	0	0	0	0
part		0	0	0		0	0
overall	0.0001	0	0	0	0	0	0
part		0	0	0		0	0

Note. The number of correlations were calculated between 45 physical variables (TR, DT, KR, ZT, ZV x 9 runs) and 9 psychological variables (joy, love, anger, grief, fear, arousal, inner dialogue, direction of attention, absorption), reflecting the states of consciousness of the participants measured with the Phenomenology of Consciousness Inventory (PCI) (Pekala, 1995).

Program Code for the Permutation Test in Matlab:

```
for n = 1:10000
    % random permutations
    EPh2 = EPh(:,randperm(size(EPh,2)));
    CPh2 = CPh(:,randperm(size(CPh,2)));
    EPs2 = EPs(:,randperm(size(EPs,2)));
    CPs2 = CPs(:,randperm(size(CPs,2)));
    % calculation of correlation matrix
    [E_rho, E_p] = corr( EPh2',EPs2', 'type', 'Spearman', 'rows', 'all', 'tail','both');
    [C_rho, C_p] = corr( CPh2',EPs2', 'type', 'Spearman', 'rows', 'all', 'tail','both');
nc= size(E_p,1)*size(E_p,2);
ti=0;
sig_th = [.1, .05,.02,.01, .005, .002, .001, .0005, .0002, .0001];
n_soll = sig_th.*nc;
for p_th = sig_th
    ti=ti+1;
    psig=(E_p<p_th);
    no_exp(ti) = sum(sum(psig));
    psig=(C_p<p_th);
    no_cont(ti) = sum(sum(psig));
    zo(ti)=(no_exp(ti)-no_cont(ti))/sqrt(2*no_cont(ti)*(1-no_cont(ti)/nc));
    yo(ti)=(no_exp(ti)-no_cont(ti))/sqrt((no_exp(ti)*(1-no_exp(ti)/nc))+(no_
cont(ti)*(1-no_cont(ti)/nc)));
    do(ti)=no_exp(ti)-no_cont(ti);
    eo(ti)=no_exp(ti);
end
...
end
```

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REVIEW

Minding the Matter of Psychokinesis: A Review of Proof- and Process-Oriented Experimental Findings Related to Mental Influence on Random Number Generators

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Abstract—Many experiments have been conducted over the past eight decades to explore whether the ostensible psychic ability of psychokinesis (PK, or "mind over matter") might be a genuine human potential, and the most extensive of these have involved attempts to mentally influence the output of electronic, binary-bit random number generators (RNGs). Research of this type can generally be divided into two lines: *proof-oriented* (concerned with the accumulation and statistical evaluation of data from controlled experiments designed specifically to test for the presence of PK effects on the microscopic scale) and *process-oriented* (concerned with conducting exploratory experiments designed to systematically vary certain test conditions in order to search for and identify any physical, biological, and psychological factors which might have a role in improving or moderating PK effects). To help orient novice investigators and cross-disciplinary researchers who may be considering work along these lines (as well as to offer some initial guiding insight on possible directions for future research), this paper provides a general review of some of the notable proof- and process-oriented findings that have been obtained to date in experimental microscopic PK research using RNGs. The review generally indicates that although a considerable amount of proof-oriented data for micro-PK has accumulated over the years, the relatively sparse amount of process-oriented data available at present leaves many open questions regarding the underlying factors involved, providing ample opportunity for novice investigators and cross-disciplinary researchers to make valuable research contributions in the future.

Keywords: anomalous perturbation, microscopic psychokinesis (micro-PK), mind–matter interaction, random number generator (RNG)

INTRODUCTION

Could humans be capable of affecting tangible objects and other forms of matter present in the physical world simply through mental means alone? Although such a question might initially seem totally illogical (or even quite ridiculous) to some scientists, it can be argued that the matter of whether *psychokinesis* (*PK*, often colloquially referred to as "mind over matter") exists as a genuine form of human ability is one which could be worth pondering even momentarily, considering that it would touch upon a broader fundamental issue relating to the nature of human consciousness and its potential boundaries. In that context, some of the relevant questions for open consideration would be: Is there an aspect of consciousness that might extend into the physical world? And if so, to what degree? Where exactly do mind and matter intersect with each other?

Phenomena hinting at a possible influence of mind upon matter have reportedly been witnessed and described in many cultures throughout the course of human history (Auerbach, 1996; Heath, 2011). Some of the earliest known accounts relate to apparent displays of *lithobolia* (or "stone-throwing") reported in the 17th and 18th centuries, in which numerous stones or pebbles were said to have spontaneously propelled themselves through the air in the presence of human witnesses (Davidson & Duffin, 2012; Puhle, 1999). Reports were particularly profuse during the height of the Spiritualism movement in the late 19th century, when various raps, knocks, table tippings and levitations, materializations, and other seemingly anomalous physical events were described in the context of séance sittings held in the presence of reputed physical mediums (Braude, 1991, Ch. 2; Carrington, 1920; Crookes, 1889; Gregory, 1985; Nisbet, 1973; Pilkington, 2006; Weaver, 2015). While their numbers seemingly began to decline from the 20th century onward, there remained to be a fair amount of spontaneous PK reports that surfaced from time to time in various situational contexts (Alvarado, 2006; Biondi, 2009; Rhine, 1963; Wright, 1998), most often in cases of ostensible poltergeist and haunt phenomena (Auerbach, 1996; Carrington & Fodor, 1951; Cornell, 2002; Flammarion, 1924; Gauld & Cornell, 1979; Huesmann & Schriever, 1989; Puhle, 1999; Rogo, 1986; Roll, 1977, 1983). Mild success at intentionally producing overt PK phenomena was also said to have been occasionally achieved in the context of sitter groups partly modeled after the earlier séance sittings (Batcheldor, 1966; Brookes-Smith, 1973; Brookes-Smith & Hunt, 1970; Owen & Sparrow, 1976; Pilkington, 2006; Ullman, 2001).

Although such reports of spontaneous PK-related phenomena often seemed quite intriguing, their evidential value is somewhat limited by the fact that they were often based primarily on anecdotal observations made under conditions that were not always ideally controlled. On this basis, questions could be raised about whether the phenomena did reflect genuinely anomalous physical effects, or whether they were simply spurious demonstrations artificially produced through trickery, misperception, or unrecognized ordinary physical factors. As a means of addressing this issue, attempts at producing PK phenomena in the laboratory became one focus of the development of experimental parapsychology in the 20th century.

Early laboratory experiments on PK were partly inspired by gambling and gaming scenarios, involving attempts to mentally affect rolling six-sided dice (Pratt, 1960; Rhine, 1970). Starting with the efforts initiated by J. B. Rhine and his colleagues at Duke University in the early 1930s, there were nearly 150 experimental PK tests with dice conducted over a 52-year period spanning from 1935 to 1987, involving 2,569 volunteer participants attempting to willfully affect nearly 2.6 million dice rolls. Collective evaluation of these experiments via meta-analysis reveals a small but highly significant overall effect (Stouffer's $Z = 18.2$, $p < 10^{-70}$), with an odds ratio greater than a billion to one. In stark comparison, the overall result for 31 control tests—amounting to 153,288 dice rolls—was entirely consistent with chance (Stouffer's $Z = 0.18$, $p = .429$), with an odds ratio of only about two to one (Radin & Ferrari, 1991). These findings seemed to be in line with a PK effect occurring on a small (possibly microscopic) scale.

To date, the most extensive number of experiments on PK have similarly entailed examinations of PK on the microscopic scale, involving attempts to mentally influence the output of electronic, binary-bit *random number generators* (*RNGs*, also sometimes synonymously referred to as *random event generators*, or *REGs*).[1] As with other experimental areas related to the study of ostensible psychic (or *psi*) phenomena, micro-PK research with RNGs can generally be divided into two lines: The first line involves *proof-oriented* research, which is mainly concerned with the progressive accumulation and statistical evaluation of RNG data from controlled experiments specifically designed with the intent to detect the presence of micro-PK effects on RNG output. In short, it involves research geared toward the effort to build a database of replicable evidence (or "proof") for seriously considering the existence of PK.

The second line relates to *process-oriented* research, which mainly focuses on conducting and evaluating exploratory experiments that are purposely designed to systematically vary certain test conditions in order to search for (and eventually identify, through consistent replications) any physical, biological, or psychological factors which might have a role in enhancing or modulating PK effects. In short, it involves research that is geared toward learning more about the processes which might underlie PK functioning, or (in simpler terms) learning more about "how PK might work."

After eight decades of research, one might wonder: What have we learned so far from these experiments, and in what directions should RNG-PK research perhaps be focused in the future? To help address these questions, a general review is provided here of some of the notable proof-oriented and process-oriented findings which have emerged from RNG-based micro-PK experiments to date. For the sake of keeping this review to a manageable length, the intent here is not to give an exhaustive treatment of all the available findings along these lines, but rather to focus primarily on findings which have emerged from certain subsets of related studies on a given proof-oriented or process-oriented topic. The aim is to try and begin gaining some preliminary insight on the following:

—What kinds of evidence for PK have amassed from proof-oriented RNG testing?

—Are they any kinds of physical, biological, and psychological factors which might be conducive to successful micro-PK performance?

It is hoped that this review will be particularly helpful in orienting novice investigators and cross-disciplinary researchers who may be considering research along any of these lines, as well as in offering initial guidance for pursuing further replications and exploratory research related to findings which seem potentially fruitful.

PROOF-ORIENTED RESEARCH: WHAT EVIDENCE IS THERE FOR PK FROM RNG TESTS?

Early PK Research by H. Schmidt

The RNG revolution in parapsychology largely began in the 1970s when Helmut Schmidt, a physicist at Boeing Laboratories in Seattle, Washington, had designed and introduced a compact, electronic RNG device that could be used for applications in psi testing (H. Schmidt, 1970b, 1970c). In its simplest form, the device based its randomness on natural radioactive particle decay from a sample of the isotope Strontium-90, which was monitored through a Geiger-Müeller tube. The tube was interfaced to a high-frequency electronic switch that rapidly oscillated between two equally possible binary state outcomes ("1" and "0"), with each having a 50% probability of occurrence. The decay emission of a particle happened at intervals that were inherently unpredictable, and whenever an emitted particle was detected by the tube the switch was stopped and the binary state outcome (indicated by the resulting momentary position of the switch) was registered on an electromechanical counter and recorded on a strip of paper tape. After a delay lasting about one-tenth of a second, the switch was reset and it continued its rapid oscillating behavior until the next emitted particle was detected. The device continually repeated this process for a pre-defined number of trials, thereby facilitating the generation of a random binary sequence.[2]

Schmidt initially utilized an RNG of this type (with the Geiger-Müeller tube interfaced to a four-choice switch) to conduct preliminary studies of clairvoyance and precognition (H. Schmidt, 1971b, 1990), but he soon came to realize that the significant results he obtained in those studies could potentially be explained not only by psychic perception of the outcome, but also by exerting a psychokinetic influence on the RNG output. This prompted him to explore the PK hypothesis in a pilot study comprising two individual test series with the binary-bit RNG device (H. Schmidt, 1970a).

During each of the test sessions in the preliminary series, a participant was seated before a visual feedback display consisting of a circular row of small lamps. The lamps were lit sequentially, one at a time, moving in either the clockwise or counterclockwise direction, with the direction being randomly determined at any given moment by the output of the RNG (to which the display was externally interfaced). The participant was tasked with trying to willfully make the lamps consistently light up in one particular direction for as long as possible, which could be hypothetically achieved by mentally influencing the RNG output to produce more "1" bits than would be expected by chance. Rather surprisingly, the overall result ("Prelim" in Figure 1) was suggestive of an influence in the direction opposite to that of the participants' willful intentions, with a success rate of 49.5% being obtained (13,695 successes out of 27,648 total bits; $z = -1.55$, $p = .06$).

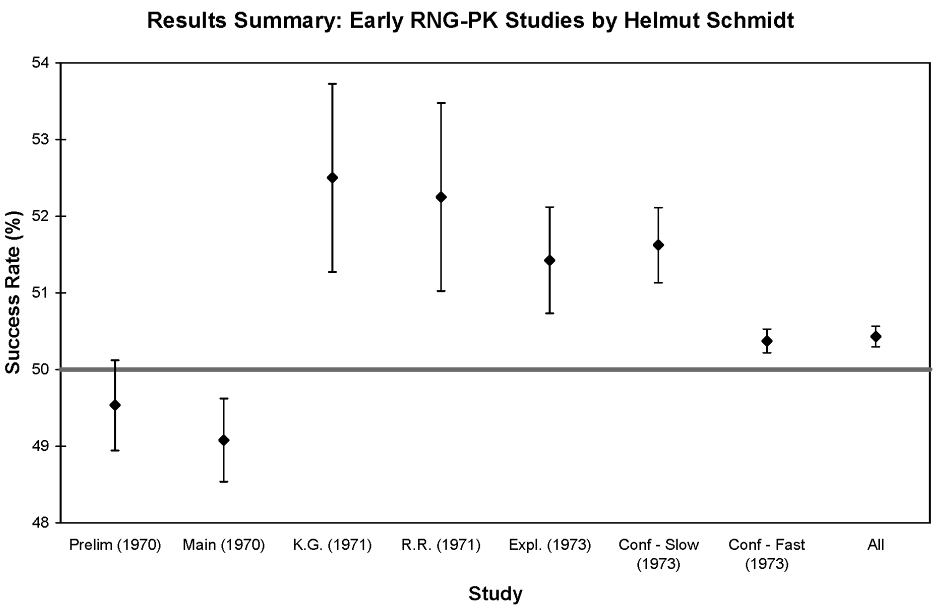

Figure 1. Graphical summary of the results obtained in the early RNG-based PK experiments conducted by H. Schmidt (1970a, 1971a, 1973), expressed in terms of success rate (black diamonds) and associated 95% confidence intervals (thin vertical bars). The thick grey horizontal line at 50% represents the mean chance expected success rate. See text for study details.

On the basis of this preliminary finding, H. Schmidt (1970a) predicted that the overall result in the second (main) series would also exhibit a negative deviation, and to try and encourage this tendency toward PK-missing he asked several participants who had exhibited a negative-scoring tendency in the preliminary series to again take part in the main series, and he further asked some of them to personally approach their second testing sessions with feelings of pessimism and discouragement. The result was indeed strongly in the predicted direction ("Main" in Figure 1), with a 49.1% success rate being obtained (16,082 successes out of 32,768 bits; $z = -3.34$, $p = .0004$).

To exclude the possibility that the significant results in these two series were due to a bias caused by a malfunction in the RNG, Schmidt also performed various control sessions in which the RNG was allowed to run unobserved for prolonged periods on separate days. The overall results, involving four million binary numbers, were found to be consistent with chance.

Notable results were also obtained in a second PK study that H. Schmidt (1971a) conducted with two selected participants. The first participant was "an aggressively outgoing American girl" (p. 758) known as K. G., who exhibited a tendency to influence the RNG output in a positive direction in an early informal test series conducted with her. This tendency persisted in a formal confirmatory test, with a 52.5% success rate being obtained (3,360 successes out of 6,400 bits; $z = 4.00$, $p = 3.17 \times 10^{-5}$; "K.G." in Figure 1). The second participant was a South American psi researcher known as R. R., who exhibited a negative-going tendency in his informal test series. This tendency was also maintained in his formal confirmatory test, which resulted in a success rate of 47.8% (3,056 successes out of 6,400 bits; $z = -3.60$, $p = .0002$; "R.R." in Figure 1).

To see if the PK effects observed in these previous test series could possibly be enhanced with a greater number of bits, H. Schmidt (1973) conducted a third study with a newly constructed RNG that could generate binary bits at a higher rate of speed (up to 1,000 bits per second) by sampling electronic noise. Feedback on the RNG's output could be provided to participants either through auditory means (by relaying the output sequence as a series of clicking noises played into their ears via headphones) or visual means (by representing the output sequence on a running marker chart, where each individual bit outcome was registered by a deflection of the marking pen in one particular direction), and the PK task involved the participant trying to willfully favor one particular side of the feedback relay over the other (e.g., by trying to make more of the clicks play in the right ear rather than the left, or trying to make the pen deflect more often to the right side of the chart as opposed to the left).

Two test series were conducted using each of the two feedback types: The first (exploratory) series, conducted with four selected participants (one of whom was Schmidt himself), was in line with an intended deviation in the positive direction, with a 51.4% success rate (10,285 successes out of 20,000 bits; $z = 4.03$, $p = 2.79 \times 10^{-5}$; "Expl." in Figure 1), with both types of feedback being effective to a roughly equal degree.

To see if PK performance might be affected in any way by varying the rate of bit generation, the second (confirmatory) series examined success rates at both high (300 bits/second) and low (30 bits/second) speeds. To make the two-bit generation rates appear overtly similar to each other from the participant's viewpoint, the individual test trials in each instance were set to span the same lengths of time. Both bit rates were found to produce positive results, with the slow bit rate resulting in a higher success rate (51.6%; $z = 6.49$, $p = 4.31 \times 10^{-11}$; "Conf. – Slow" in Figure 1) than the fast bit rate (50.3%; $z = 4.71$, $p = 1.24 \times 10^{-6}$; "Conf. – Fast" in Figure 1). The two types of feedback were again found to be equally effective in this series, as well. (Further discussion of the effect of varying bit rates will be made in a later subsection of this paper.)

When combined ("All" in Figure 1), the results across all of these early PK studies by H. Schmidt are highly significant (Stouffer's $Z = 4.06$, $p = 2.46 \times 10^{-5}$), with an average success rate of about 50.3% and an associated odds ratio of about 40,650 to one. (For a broader readable overview of these early PK studies, see H. Schmidt, 1974a.)

PEAR "Benchmark" RNG Program

One of the most extensive and well-known efforts to experimentally study PK using RNGs was conducted by the staff of the Princeton Engineering Anomalies Research (PEAR) Laboratory, which was active in the School of Engineering and Applied Science at Princeton University from 1979 to 2007 (Dobyns, 2015; Dunne & Jahn, 1995; Jahn, 1982; Jahn & Dunne, 1987, 2005, 2011; Nelson et al., 1986). The effort consisted of an experimental program that was run over the course of a 12-year period (Jahn et al., 1997), involving 91 volunteer participants who each made multiple attempts to mentally affect the output of PEAR's custom-designed "benchmark" RNG, a benchtop microelectronic device which, as succinctly described by Dobyns (2015), based its random binary bit generation on

> . . . the trickle current in a diode (a solid state rectifier) that was being forced to carry current in the "wrong" direction. Since such currents depend on the ability of electrons to cross an energetically forbidden region by quantum tunneling, this current is every bit as much a quantum-mechanical random phenomenon as a radioactive decay. (Dobyns, 2015, p. 220)

The device was typically programmed to generate a sequence of trial values of 200 random binary bits each, with each individual trial value reflecting the total number of "1" bits which resulted from that trial; the theoretically-expected mean value was 100, with a standard deviation of 7.071. An extensive series of calibrations performed with the RNG, amounting to just over 5.8 million trials total, had generally indicated that the device's bit output sufficiently conformed to these expected values over the long term (Jahn et al., 1997; Nelson et al., 1989).

In a typical experimental PK test run, a participant was seated across from the benchmark RNG (with no physical contact being made between the two) and asked to try and mentally influence its trial bit outcomes across three separate experimental conditions:

HI – the participant's influence was aimed at having the RNG generate more trial bit outcomes with high values (i.e., generating more "1"-bit sums that are above 100), or increasing the overall trial mean of the test run;

LO – being the opposite of "HI," the aim was at having the RNG generate more trial bit outcomes with low values (i.e., generating more "1"-bit sums below 100), or decreasing the overall trial mean of the run;

BL – the participant's influence is simply aimed at having the RNG generate a nominal baseline, with trial bit values that equal (or come in close range to) the theoretically-expected mean value. Equal amounts of data were gathered across the three conditions, with test runs ranging from 1,000 to 5,000 trials per condition.

As a convenient form of visual assessment, the statistical results of the test run were often graphically plotted as a cumulative measure of the degree of deviation away from the mean-expected value (often short-handed to "cumulative deviation"); a generic illustrative example is shown in Figure 2, representing the graphical display of a sequence of 3,600 trials generated by an RNG running unobserved, with no attempt at mental influence presumably being applied. Mean chance expectation (MCE) is represented in the graph by the level horizontal line at zero, while increases along the Y-axis represent a deviating tendency toward higher trial outcome values (above MCE), and decreases represent a tendency toward lower trial values (below MCE).

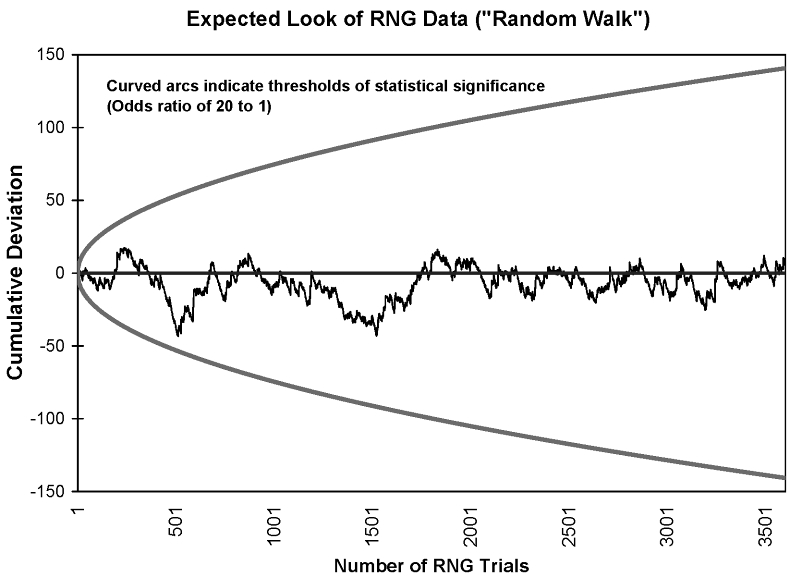

Figure 2. Illustrative example of a cumulative deviation plot, displaying the ideal look of a nominally random sequence being generated by an RNG while producing an output of 3,600 individual trials. The dark horizontal line at zero represents mean chance expectation (MCE), and the bold grey arcs represent the thresholds of statistical significance at $p = .05$ (above MCE) and $p = .95$ (below MCE) as trials accumulate.

The bold-curved arcs indicate the threshold of statistical significance at both the $p = .05$ (positive) and $p = .95$ (negative) levels as the trial data gradually accumulate over time. The actual trial output sequence of the RNG is represented by the jagged line, which in this example exhibits a fairly ideal random walk (sometimes colloquially referred to as a "drunkard's walk") that maintains a level degree of random fluctuation around MCE.[3]

A graphical summary of all the benchmark RNG data collected by PEAR over the course of its 12-year program (amounting to nearly 2.5 million trials total) can be seen in Figure 3. The graph shows the data divided up into the respective cumulative deviation plots for each of the three mental influence conditions (HI, LO, & BL), and it can be seen that for each condition a notable deviation from MCE was observed in conjunction with the influential aim of the participants. Of particular

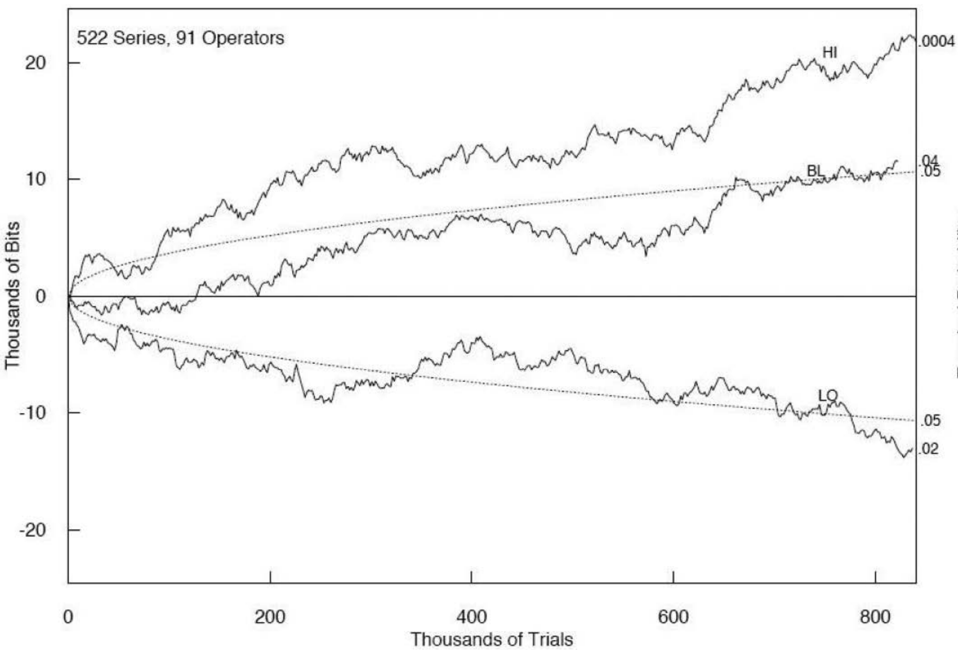

Figure 3. Graphical summary of the experimental data collected by PEAR over the course of its 12-year "benchmark" RNG program (Jahn et al., 1997), with the cumulative deviation plots for each of the three test conditions (HI, LO, & BL) being displayed. Adapted from figure 2 of Jahn et al. (1997).

note, the observed deviations for the HI ($Z = 3.37$, $p = .0004$) and the LO ($Z = -2.02$, $p = .022$) conditions were each independently significant, and the formal test of the PK hypothesis—predicted in the form of a notable overall difference between the HI and LO conditions—yielded a highly significant effect ($Z_{diff} = 3.81$, $p = 6.99 \times 10^{-5}$) with an odds ratio of about 14,380 to one.

Mind/Machine Interaction Consortium Replication Study

Partly as a means of further following up on the PEAR work on a broader scale, a "Mind/Machine Interaction Consortium" was formed in 1996 among three laboratories—the Institut für Grenzgebiete der Psychologie und Psychohygiene (IGPP) in Freiburg, Germany; the Justus-Liebig Universität's Center for Psychobiology and Behavioral Medicine in Giessen, Germany; and PEAR—with the intent of attempting to directly replicate the experimental methodology and main findings of the PEAR benchmark program as closely as possible, using second-generation RNG technology (Jahn et al., 2000). The three-year effort called for each laboratory to conduct 250 experimental PK sessions in which 227 volunteer participants were asked to try and mentally influence the same particular type of compact RNG (designed to utilize thermal noise in resistors as its source of randomness), with equal proportions (250,000 trials) of data being collected for each of the three influence conditions (HI, LO, & BL). The measure pre-specified in advance to serve as the formal test for replication was the significant overall difference between the HI and LO conditions that had been observed in the PEAR results. Automated calibration sessions which followed the experimental ones, amounting to just over 3.1 million trials total, generally indicated that the RNGs used by the three laboratories had remained sufficiently random as expected over the course of the replication.

The main experimental results of the Consortium replication (Figure 4), when graphically summarized and compared against the original PEAR benchmark findings in the manner depicted by Radin (2006, p. 156), seemed to indicate two things:

1. The replication z-scores for the HI, LO, and BL conditions (dark circles in Figure 4) seemed to generally reflect the same directional patterns as those observed for these three conditions in the PEAR benchmark program (shaded bars). This seemed to suggest that there

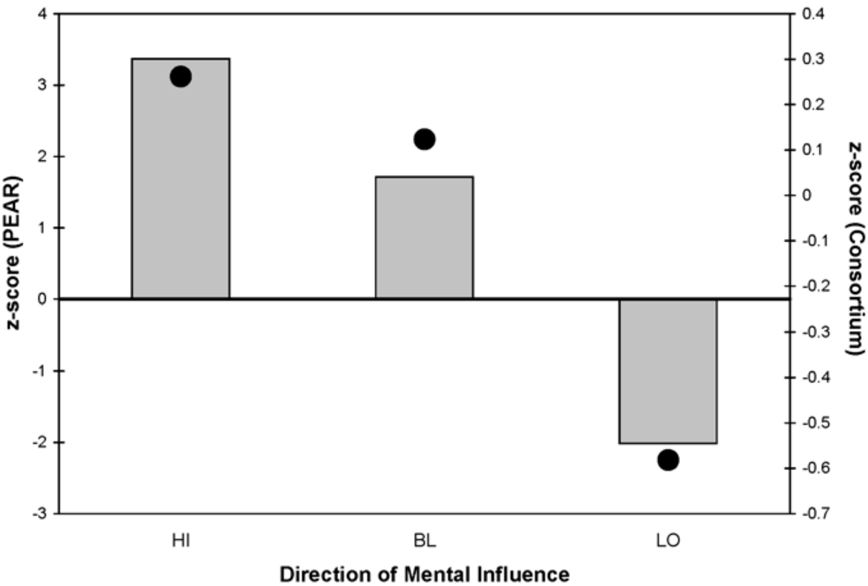

Figure 4. Graphical summary and relative comparison of the combined experimental RNG results from the "Mind/Machine Interaction Consortium" replication study (Jahn et al., 2000; dark circles) with the results obtained in the 12-year PEAR "benchmark" RNG program (Jahn et al., 1997; shaded bars). Adapted from Radin (2006, figure 9-4).

was a consistent tendency for the experimental RNG data in each case to become shifted in a manner in line with the influential aims of the participants.

2. None of the replication *z*-scores managed to attain the minimal standard score for statistical significance (i.e., $z > 1.65$), and when examined in relative comparison with each other, the replication scores are notably smaller than the PEAR benchmark *z*-scores by about one order of magnitude.

Combined together, the data from all three laboratories resulted in a nonsignificant HI–LO difference ($Z_{diff} = 0.59$, $p = .278$) that was one order of magnitude smaller than the previously observed PEAR difference. But while this result did not offer clear evidence of an anomaly, several secondary analyses planned in advance to look for certain structural patterns in the replication data that had been previously found in the PEAR benchmark data (such as serial position effects, discussed in a later subsection) did collectively produce a result suggestive of a lesser anomaly in the data ($p = .022$).

RNG Meta-Analyses: 1985–2006

In addition to those conducted by H. Schmidt, PEAR, and the Consortium, many other RNG-based PK experiments have been conducted by parapsychologists since the 1960s. When collectively taken into account as a whole, do the data from these experiments exhibit evidence of a notable deviation from expected randomness over time? To find out, five meta-analyses of the accumulating RNG-PK database have been progressively conducted and reported over the past several decades. The first was conducted and reported by Radin et al. (1986), who surveyed 381 experimental PK tests conducted by 38 different researchers over a 15-year period spanning from 1969 (when the pioneering work by H. Schmidt was first reported) to 1984. Using a simple result counting method, they found that 71 of these experiments had reportedly been successful at the $p < .05$ level, whereas only 19.05 would have been expected by chance. Such an outcome has an associated odds ratio greater than a billion to one (binomial $p < 5.4 \times 10^{-43}$).

The second analysis was conducted and reported by Radin and Nelson (1989), who examined 597 experimental and 237 control sessions conducted by 68 different researchers over a broader period of 28 years, from 1959 to 1987. When expressed in terms of success rate,[4] the combined, quality-weighted result for the experimental sessions is quite small (50.016%, "RN 1989" in Figure 5), reflecting only a narrow fractional shift above the expected mean value. But despite its small size, the result is highly significant, reflecting a six-sigma deviation from chance expectation with an associated odds ratio of about 30 billion to one ($Z = 6.53$, $p = 3.30 \times 10^{-11}$). In contrast, the result for the control sessions was entirely consistent with chance. (For a broader readable summary, see Ch. 8 of Radin, 1997.)

Following up on their initial finding fourteen years later, Radin and Nelson (2003) expanded their database for the third analysis to 515 experiments conducted by 91 researchers over a period of 41 years, from 1959 to 2000.[5] The success rate associated with the weighted overall experimental result was again quite small in magnitude (50.005%, "RN 2003" in Figure 5), but still highly significant, with odds of about 14,300 to one ($Z = 3.81$, $p = 6.95 \times 10^{-5}$).

The fourth analysis, conducted by Radin (2006), had further

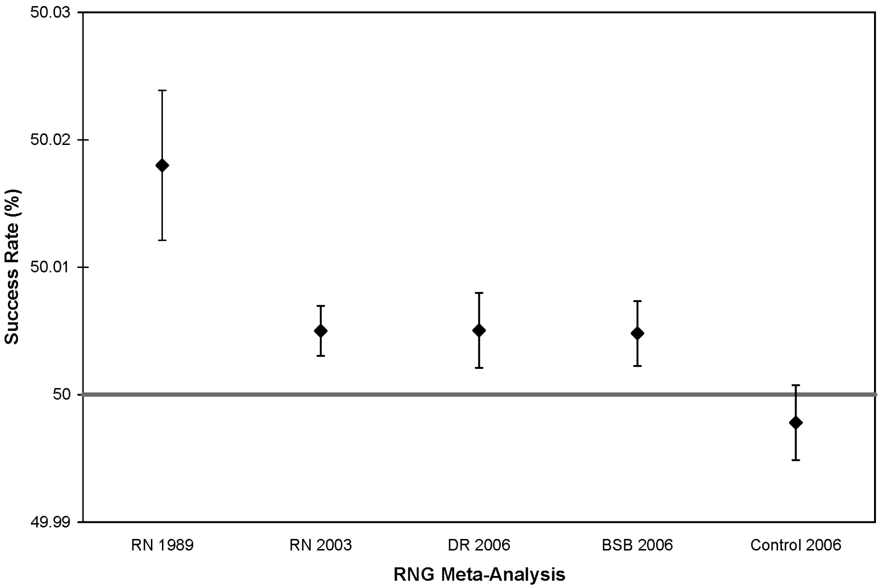

Figure 5. Graphical summary of the success rates (black diamonds) and associated 95% confidence intervals (thin vertical bars) from four RNG-PK meta-analyses reported by Radin & Nelson in 1989 ("RN 1989") and 2003 ("RN 2003"), Radin (2006; "DR 2006"), and Bösch et al. (2006a, "BSB 2006"), along with the control dataset analyzed by Bösch et al. (2006a, "Control 2006") for comparison. The thick grey horizontal line at 50% represents the mean chance expected success rate. See text for study details.

updated the database to 490 experiments covering the 47-year period from 1959 to 2006,[6] and yielded a comparable success rate (50.006%,[7] "DR2006" in Figure 5). This weighted result also was highly significant, with odds of about 39,000 to one ($Z = 4.05$, $p = 2.56 \times 10^{-5}$).

The fifth and most recent meta-analysis by Bösch et al. (2006a) had independently examined 380 experimental and 137 control sessions conducted by 59 researchers from the 1960s up to 2004. Conservatively, the combined success rate (50.004%, "BSB2006" in Figure 5) for the experimental sessions was comparable to those obtained in the previous two analyses, with an odds ratio of about 147 to one ($Z = 2.47$, $p = .0068$).

One contributing source to this more modest result appeared to be the three largest experiments contained in the test database, which exhibited significant directional trends of deviation that were opposite to prediction (i.e., they exhibited significant shifts from expected randomness in the negative direction, rather than in the often-predicted positive direction; more will be said about these experiments in a later subsection on RNG bit rate). When these three largest experiments were excluded, the combined experimental success rate increased to 50.029%, with odds of about 44,300 to one ($Z = 4.08$, $p = 2.25 \times 10^{-5}$). In marked contrast, the control success rate was very close to chance (49.998%, "Control 2006" in Figure 5), with odds of only 14 to one ($Z = -1.51$, $p = .13$).[8]

Retro-PK Studies

While most RNG tests for PK have involved participants attempting to mentally influence a sequence of random numbers being generated by the RNG in real-time (i.e., at the same present moment), a small proportion of them were also designed with the purpose of determining whether PK could possibly act *retroactively*—that is, to determine whether participants could possibly influence random number sequences that have already been generated and recorded some time *before* the PK test begins.

One of the earliest studies of *retroactive PK* (or *retro-PK*, for short) was conducted by H. Schmidt (1976), in which he initially programmed an RNG to automatically generate many individual sets of random number sequence outputs (each composed of 201 binary numbers) that were registered as audible clicks and pre-recorded on to audio cassette tape. The original tape was then duplicated and the original was kept secure by Schmidt, while the copy was given to psychic claimant Sean Lalsingh Harribance, who was asked to take it home and try to influence the clicks while listening to them being played back on a tape player. (When played back, the clicks were randomly presented at high- or low-volume, and Harribance's task was to try to increase the number of low-sounding clicks.) Subsequent counting of the clicks recorded on the original tape (done only *after* Harribance had fully listened to the copy, with the reasoning for this being based on the observational approach, described below) did indeed indicate a modest increase in the number of low clicks (55.5% success rate, $z = 2.23$, $p = .013$), in line with the intended goal.

In three other exploratory tests conducted in a laboratory setting, the pre-recorded random number sequences were coupled to the left/right movement of an instrument needle, and Harribance was tasked with trying to make the needle swing more often to the right. Though again in line with the intended goal, the result was only suggestive at best (53.6%, $z = 1.44$, $p = .075$).

When combined, these initial exploratory tests with Harribance resulted in a significant overall success rate (54.6%, $Z = 2.60$, $p = .0046$), with an odds ratio of about 210 to one. These (along with a few other pilot tests with promising results) had motivated H. Schmidt (1976) to pursue a series of three formal retro-PK experiments of varying complexity, each of which produced significant results with approximate odds ratios ranging from 45 to 2,000 to one (i.e., p-values ranging from .021 to .0005).

In addition to this early study by H. Schmidt, 23 other retro-PK experiments using various kinds of targets were conducted and reported by nine different researchers over an 18-year period spanning 1975 to 1993. Upon being combined, Bierman (1998) initially found that these 26 experiments collectively amount to a highly significant outcome ($Z = 5.31$, $p = 5.49 \times 10^{-8}$) with an associated odds ratio of about 18 million to one. If Bierman's analysis is further updated to the present and is limited solely to experiments using data pre-recorded from RNGs (see table in the Appendix), one finds that there have been a total of 42 experiments reported from 1975 to 2021, which continue to exhibit a highly significant overall result exceeding six sigma ($Z = 6.82$, $p = 4.57 \times 10^{-12}$) with nearly a trillion to one odds ratio.

If these significant results can be taken as being reflective of a retro-PK effect, then one may be led to wonder: How might the mind be able to influence random data that has already been generated and recorded prior to the PK test? One possibility is based on the assumption that PK may somehow be capable of operating in a retrocausal fashion (H. Schmidt, 1993b)—that is, it may somehow be capable of working in a manner *opposite* to the conventional temporal direction of cause and effect, such that the directional flow would be reversed to where an effect seems to precede (rather than follow) its cause. Another way of looking at this is that PK would have to somehow act *backwards* in time in order to influence the data at the moment it was being generated in the past, prior to the PK test.

An alternate possibility, which circumvents this assumption of retrocausality, has its basis in observational theory (Houtkooper, 2002a, 2006b; Millar, 1978; H. Schmidt, 1975, 1976; Walker, 1975, 1984, 2000), an approach based on certain interpretations of quantum physics which may be succinctly conceptualized in a single statement as: "[T]he act of observation by a motivated observer of an event with a quantum mechanically uncertain outcome influences that outcome" (Houtkooper, 2002a, p. 172). This alternate viewpoint begins with the recognition that an RNG's random binary number sequences are derived from sampling the quantum-based activity of subatomic particles, which is inherently probabilistic. Prior to the PK test, these sequences are automatically generated and recorded when no one is present to directly observe them, which presumably leaves their binary outcomes undetermined in a state of quantum superposition (where it remains equally likely that a particular binary outcome could result in a "1" or a "0"). It is not until the sequences are directly observed later on by the participant (upon being played back during the PK test) that their outcomes are actually determined (and presumably influenced in the process of being observed) by a motivated observer (i.e., the participant, who is aiming for a greater proportion of one particular binary outcome over the other). Thus, according to this observational approach, retro-PK might simply be viewed as being a kind of "delayed" PK effect (Houtkooper, 2006b; H. Schmidt, 1975, 1976, 1982, 1987a; Stapp, 1994; Walker, 1975, 1984).

This observational approach can help one to understand the rationale behind the procedure employed in H. Schmidt's (1976) earliest exploratory test, where the analysis of the original tape was conducted *after* Harribance listened to, and attempted to influence, the copy: Because the two tapes are exact duplicates of each other, they contain the same binary bit data stemming from the same source (the RNG), and from the perspective of quantum theory can perhaps be thought of as representing an entangled system. Initially, one would presume that, prior to being observed, the data contained on the two tapes would be in a state of quantum superposition. But upon being observed by Harribance (at the time he is listening to them), the binary bit outcomes for the data contained on the copy tape would presumably become determined (and influenced via PK). And presumably, since the data contained on the two tapes are entangled, the effect of Harribance observing and influencing the data on the copy tape should then also (nonlocally) affect the data contained on the original tape, such that its binary bit outcomes would become determined and influenced, as well. This would then facilitate an analysis of the data from the original tape as a means of evaluating the results of the PK test. Such a procedure would also happen to contain a convenient preventive measure against data tampering, since the original tape remains secured away by the experimenter during the test and is not observed or handled by anyone.

Field RNG Studies

The significant effects upon RNG output observed in laboratory PK experiments may lead some to raise the ecologically relevant question: To what extent might these effects apply to more naturalistic settings? As a preliminary means of finding out, the PEAR Laboratory began developing a more compact and portable electronic noise-based RNG system in the 1990s that could be deployed out in the field (Bradish et al., 1998), and these "field RNGs" were placed in close proximity to various kinds of social group venues and environments that initially included conferences, workshops, religious ceremonies, council meetings, and a geographic site where many witnesses reported seeing ostensibly anomalous atmospheric light phenomena. In most of these instances, the field RNG system was positioned unobtrusively in the background and programmed to continuously collect binary bit data in a silent fashion, with few or none of the group members being aware of its presence. An important implication of the latter is that unlike laboratory tests, these field studies of PK would not entail the conscious exertion of mental will—rather, any influencing effects upon the field RNGs would presumably be manifesting on the subconscious or unconscious level.

Upon being statistically combined, the field RNG data from these various group venues and environments had exhibited a significant departure from expected random behavior ($Z = 3.54$, $p = .0002$) that was very similar to the deviations from nominal randomness observed in laboratory PK experiments. In notable contrast, a matching set of field RNG data drawn from times when no social group activity was taking place at the venues and environments had resulted in an overall random outcome that was consistent with chance ($Z = 0.07$, $p = .461$) (Nelson et al., 1996).

A confirmatory study was subsequently conducted which utilized a conceptually similar set of group venues and environments that included conferences, workshops, rituals, music amd theater performances, and tours of geographic sites with historical or cultural significance—all of which were predicted to exhibit a significant departure from nominal randomness in conjunction with the social group activity (Nelson et al., 1998). The prediction was confirmed by the resulting field RNG data ($Z = 4.59$, $p = 2.20 \times 10^{-6}$), with a combined odds ratio of about 450,000 to one. Significant results were also obtained in field RNG studies with conceptually similar designs that were independently conducted by several other researchers around the same time (Bierman, 1996; Broughton, 1999; Radin, 1997, Ch. 10; Radin et al., 1996; Rowe, 1998; Schwartz et al., 1997). (For a broader overview, see Nelson and Radin, 2003.)

Efforts to expand this field RNG work to a much larger scale were undertaken by Nelson (2001) and his colleagues in 1998 with the founding of the Global Consciousness Project (GCP), an international collaborative experiment designed to generally explore the hypothesis that significant departures from nominal randomness may possibly occur across multiple RNGs during moments when the attention and emotions of many people around the world are collectively engaged in unison, often in response to notable news events and activities that tend to draw widespread notice (Nelson, 2015, 2019; Nelson & Bancel, 2011). At the heart of the experiment is an extensive, global-spanning network of RNGs, with each RNG node continually collecting 200-bit random binary samples every second of the day and sending its collected data over the Internet at regular intervals to a central server in Princeton, New Jersey, for archiving. Repeated testing of the experimental hypothesis is made in relation to the occurrence of an individually specified event or activity, with the data from the RNGs in the network being statistically combined and examined over the course of a defined period of time (typically a few hours or more) surrounding that event or activity. Some of the events and activities that have been examined include New Year's Eve celebrations, global meditations held on the International Day of Peace, U.S. Presidential elections, natural disasters (e.g., "Super Storm Sandy"), public ceremonies (e.g., the British Royal Wedding of Prince William and Catherine Middleton), and the devastating series of terrorist attacks on the World Trade Center and the Pentagon on September 11, 2001. Across a formal experimental database of 500 individual events and activities covering a roughly 17-year timespan (from August 1998 up until the end of 2015), a small but highly significant overall effect amounting to a seven-sigma deviation from chance expectation ($Z = 7.31$, $p = 1.33 \times 10^{-13}$) was found, with an associated odds ratio of about 4.1 trillion to one (Figure 6). (For a broader and readable overview, see Nelson, 2019.)

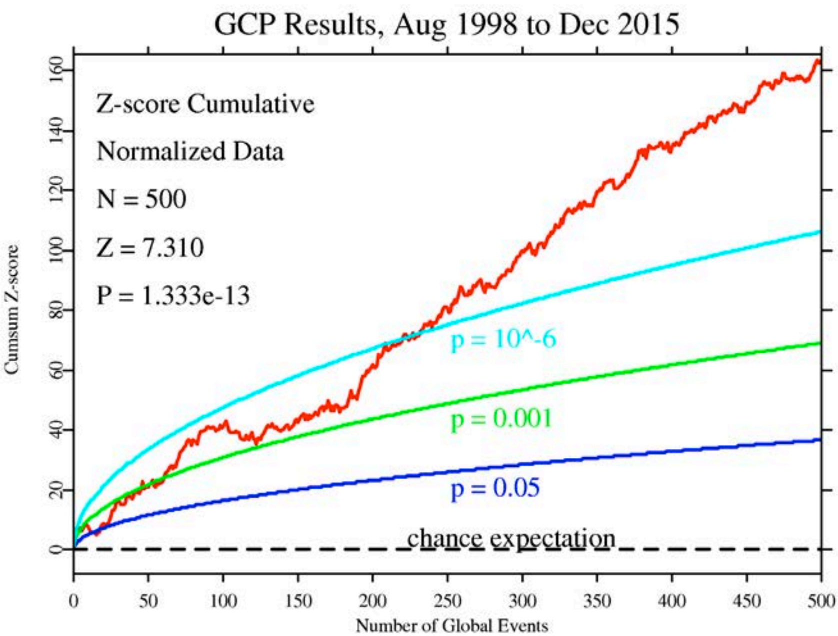

Figure 6. Graphical summary of the experimental results accumulated by the Global Consciousness Project with its worldwide network of RNGs over the course of its formal 500-event database from August 1998 to December 2015. Adapted from the "Results" page of the GCP website: https://www.global-mind.org/results.html

Other Notable Micro-PK Effects

Serial Position (Decline/Recover) Effects. Some participants in experimental tests for psychic ability have previously exhibited a notable parabolic (widened U-shaped) pattern in their test results, where their scoring performance initially starts off high at the beginning of the test series, then decreases (or declines) toward the middle, and finally begins to increase (or recover) again near the end of the series. This pattern of scoring is often referred to as the serial position effect and has been observed in tests for extrasensory perception (ESP) (Pratt, 1949; J. B. Rhine, 1969) as well as in the early dice tests for PK (Pratt, 1949, p. 15; Reeves & Rhine, 1943). It has also been observed in some PK tests using RNGs (Berger, 1988a; Houtkooper, 2002b), most notably in the benchmark experimental series conducted by the PEAR Laboratory (Dunne et al., 1994; Jahn & Dunne, 2011, pp. 178–179; Nelson et al., 2000) (Figure 7), as well as in the subsequent Mind/Machine Interaction Consortium replication (Jahn et al., 2000).

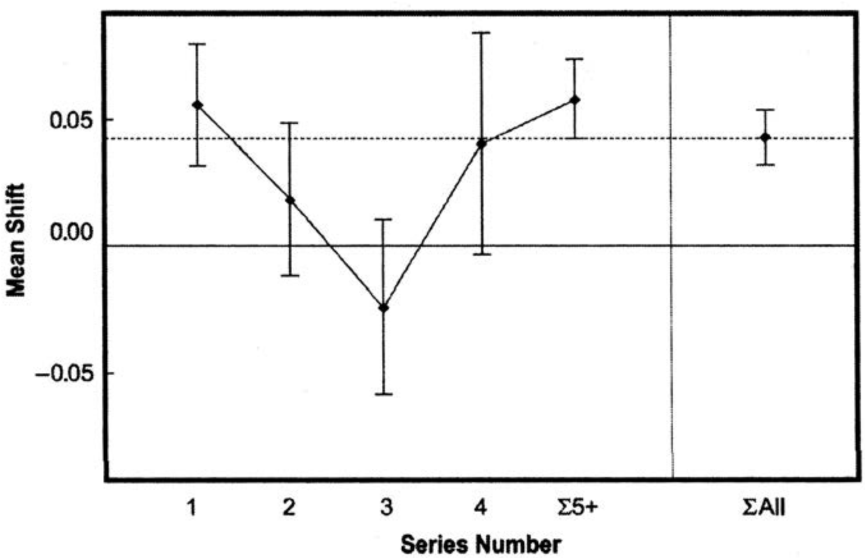

Figure 7. Graphical display of the serial position effect observed across the PEAR "benchmark" series of experiments, marked by a decline from Series 1 to 3, followed by an inversion and subsequent increase from Series 3 to 5. Adapted from Figure 2 of Dunne et al. (1994).

There is even a hint that this parabolic pattern may be observable on the broader level of an entire experimental database: In statistically evaluating the results of 264 RNG-based PK experiments conducted from 1959 to 1987, Bierman (2001, pp. 276–277) found a significant trend ($p < .03$) occurring across their z-scores with time, which indicated that the scores first tended to modestly decrease from 1959 up until the early 1970s, and then began to rebound and gradually increase from the late 1970s up to 1987.

Something particularly notable about this parabolic pattern is that it is very similar to the kinds of serial position effects that have been observed in mainstream psychological experiments (Thompson, 1994), most often in studies of learning and memory (Reed, 2004, pp. 102–103). This has led some researchers to suspect that similar kinds of factors may perhaps be involved; as Pratt (1949) observed in this regard: "When we find this fundamental characteristic of normal behavior [i.e., the parabolic pattern of the serial position effect] in ESP and PK data, we have ample reason for inferring that we are dealing here with established psychological principles showing themselves in only less familiar forms" (p. 14). If that is the case, then this may offer one indication that PK (and psychic ability, in general) may not be so inherently different from other relatively "normal" forms of human behavior such as sensory perception and cognition.

Statistical Balancing Effects. In her own experimental studies of PK using RNGs, Pallikari (2016) uncovered possible indications of another kind of pattern in the data that she labeled "statistical balancing" (Pallikari-Viras, 1997, 1998), which is based on the idea that there is ". . . a tendency of nature to remain close to a preferred, undistorted, state of randomness, so that it reverts back to that undistorted state when pushed away from it by the anomalous effect of conscious intention" (Pallikari-Viras, 1997, p. 115). The pattern was observable across two sets of RNG data, labeled "intention" and "no-intention," which Pallikari had collected consecutively, back-to-back, in the course of a PK test session. As its label implies, the "intention" data were collected during a short period in which the participant made an effort to mentally influence the RNG output, with the intent of producing an excess of one binary outcome over the other. The "no-intention" data were then collected immediately afterward, during a period of equally short length in which the RNG was allowed to run on its own without the participant making any (conscious) effort to intentionally influence its output.

Upon being analyzed, the two datasets were found to exhibit a notably contrasting parallel: They both exhibited shifts away from nominal randomness, but *in opposite directions*—for instance, if the "intention" data were found to exhibit a shift in the positive direction, then the "no-intention" data would subsequently be found to exhibit a shift in the negative direction. These two opposed shifts appeared to roughly balance each other in such a way that when the two datasets were statistically averaged, their deviations from randomness ended up roughly canceling each other, yielding an overall outcome consistent with chance. Pallikari has observed such a balancing effect across the resulting *z*-scores for the "intention" and "no-intention" datasets in two of her five PK studies (Pallikari-Viras, 1998), and has found some suggestive indications that it can even extend to other statistical parameters of the underlying binomial distribution, such as the standard deviation, skew, and kurtosis (Pallikari-Viras, 1997); as well as to broader meta-analytical findings (Pallikari, 2016). Similar kinds of results which may also be suggestive of a balancing effect have been observed and reported in at least a handful of other studies (Bierman & van Gelderen, 1994; Houtkooper, 2002b; Jahn & Dunne, 1987, pp. 116–119; Radin, 1993b), suggesting that further efforts toward exploring such an effect should perhaps be made, in order to determine how common it might be.

Assuming that it does reflect a genuine effect, one possible implication of this opposing pattern across the two datasets is that when PK (or some other influencing factor) induces an RNG's output to shift away from its nominal random behavior during the "intention" period, a secondary rebound shift may subsequently occur in the "no-intention" period to counteract the PK shift and bring the RNG output back toward nominal randomness; such an implication would appear to be in line with a physical conservation principle, like those governing energy and mass. If that is the case, then this would begin to suggest that PK may not be so inconsistent with some of the known principles of physics.

Physical/Psychological Matrix Correlations. As a premise of his Model of Pragmatic Information, von Lucadou (1988a, 1995) proposed that psi phenomena can possibly be conceptualized not as direct causal interactions between mind and matter (as they might often be thought of in terms of classical mechanics), but as non-causal correlations akin to those exhibited by systems of entangled particles according to the predictions of quantum theory. From this conceptual perspective, micro-PK might be conceptualized not as a direct, mental "force"-like form of influence manifesting upon matter, but more like a subtle, non-causal (or nonlocal) correlation existing between the properties of the physical target and the mind of a human influencer. Based on such a concept, one might predict that rather than being directly reflected by cumulative deviations from expected randomness occurring in the RNG output (presumably being causally produced from the direct exertion of a "force"-like mental influence), the effects of micro-PK might be more indirectly detectable as a relatively high degree of correlation existing between a range of psychological variables associated with the human influencer, and a range of physical variables associated with the operation of the RNG. This led to the development of an analytical approach involving correlational matrix analysis (von Lucadou, 1987a, 1987b, 1988b, 1994; von Lucadou et al., 1987). Generally, in the most recent studies utilizing this approach (von Lucadou, 2006; Walach, 2014; Walach et al., 2020), the PK test proceeds as follows:

Participants watch a monitor screen displaying an animated fractal pattern that dynamically changes based on the output of an RNG, and they are asked to try and mentally influence the change of this pattern in a certain progressive direction (either making the pattern grow or shrink), mediated partly through a freely-determined volitional act on their part; namely, pressing the shift key on either side of a computer keyboard at times of their choosing. (In actuality, this latter aspect is only an illusory motivational cover: Although the participants are told that this volitional act is part of the influencing process, the button press itself merely serves to progressively activate the RNG sampling process for the next test trial, and has no other direct link to, or effect upon, the behavior of the RNG.) After participants have completed all of the test trials, the controlling computer automatically re-runs the full PK test again in a simulation mode (with no participant attempting to influence the pattern) to act as a matching control.

The analytical approach is then applied to the resulting data by constructing, for each test run and its subsequent matching control run, a correlation matrix which pairs the various physical variables collected in the course of that respective run with various psychological variables collected from the participants. The physical variables could include: the mean voltage output of the RNG, the standard deviation of that voltage, the number of times a “1” bit was generated by the RNG during a test run, and the number of times the changing fractal pattern had progressively deviated away from the intended goal direction. Psychological variables could include: the number of times the participant pressed the left shift key, the number of times the right shift key was pressed, the amount of time it took for the participant to complete the run, and various measures of personality characteristics. Based on the prediction, one would expect to find the test matrix to contain a higher number of significant correlations than would be expected by chance, and that there would be a significant difference between the test matrix and the control matrix in the number of significant correlations they contain. This is what has been found in only some of the small number of studies of this type that have been conducted so far: Supportive results (with z-scores > 2.00) have been obtained in three of the five experiments conducted early on by von Lucadou (2006), and the most recent semi-independent attempt at replication (Walach et al., 2020) yielded a significant difference ($p = .017$) between the test and control matrices. Other recent attempts at conceptual replication using a small number of physical and psychological variables (Grote, 2017, 2021) have found either only weakly suggestive ($p = .095$) or chance results for the test matrix data, however, and additional replication data are needed for further clarification.

Why Are Micro-PK Effects So Small?

One commonality which seems to emerge from all of the experimental results examined so far is that although they tend to be quite significant, the PK effects observed in RNG studies also tend to be quite small in magnitude, even on the relative level of statistical outcomes. For instance, the two meta-analyses conducted by Radin and Nelson (1989, 2003) tend to indicate that the mean z-score for RNG-based PK experiments is only about 0.61 to 0.73, and Helfrich (2007) pointed out that, although it is not given precisely, the mean z-score for the data reported in the Bösch et al. (2006a) meta-analysis can be estimated to be about 0.6. In addition, the mean *z*-score obtained across the entire formal experimental database for the Global Consciousness Project is only about 0.33 (Nelson, 2015). Such scores suggest that the PK effects themselves tend to reflect only narrow fractions of a meanshift on average, and this may immediately lead some to wonder: Why do these effects tend to be so small, and not more relatively apparent?

While a clear and definitive answer has not yet been reached on this matter, one consideration recently put forth by Varvoglis and Bancel (2016) is that rather being an ability which is widely distributed across the entire human population, PK might instead be a more narrowly distributed ability which may be most frequently found among certain individuals who happen to be exceptionally adept at it (whether innately, or through developed practice). This suggestion was inspired by a closer re-examination of the PEAR benchmark RNG database, in which Varvoglis and Bancel (2016) had noticed that among the 91 individuals who participated in that experimental series, two of them particularly stood out from among the rest in terms of their exceptional PK scores: Whereas these two particular individuals had each achieved *z*-scores of 5.60 and 3.42, the other 89 participants in the database had collectively produced a *z*-score of only 0.82. A similar observation was made in a review of the PEAR database by Dobyns (2015), who noted that: “An analysis of the distribution of effects among operators [i.e., participants] in the REG [experiments] finds that the best-fit explanation is a ‘talented subpopulation’ model in which 15 percent of operators are responsible for the anomaly” (p. 233).

One can also find similar distinctions elsewhere in the experimental PK literature, as well: In their meta-analysis, Bösch et al. (2006a) found that studies with individuals who were especially selected for participation (based on their success in prior PK tests, or on their claims of possessing psychic ability) had produced a significantly higher ($p < 1.00 \times 10^{-10}$) overall *z*-score (6.87) than studies with unselected participants ($z = 1.84$). In evaluating the earlier PK studies with dice, Radin and Ferrari (1991) also found that participants selected on the basis of prior testing tended to produce higher effect sizes than unselected participants.

A potential caveat with these findings is that the total number of

PK test trials collected so far with selected participants is much smaller than that collected with unselected participants, which leaves open the possibility that the exceptional scores obtained by the selected participants could be somewhat inflated. However, if additional data continue to indicate that scores obtained with selected participants remain considerably higher, then it may suggest that one way to possibly increase the size of PK effects on RNGs would be to focus study more toward these kinds of participants. Such a focus along these lines may also go some way toward addressing some skeptical arguments that micro-PK effects are too small to be worthy of much serious attention (e.g., Wilson & Shadish, 2006).

Addressing the Skeptics

Despite the overall significance of their results, some professional skeptics have argued that there are a number of other potential issues with RNG experiments which do not allow them to be considered as evidence for micro-PK.

Several of these criticisms were leveled at H. Schmidt's early research: For instance, Hansel (1981) generally argued that H. Schmidt's (1970a) significant results could have been produced through fraud, in three possible ways: First, in being isolated in the monitoring room during the test session, the experimenter could have tampered either with the RNG device or with the subsequently printed data record of its output. Second, in being left alone in the testing room with the visual feedback display, the participant could have tampered with the functioning of the test system by creating electrical shorts in the feed wires connecting the display to the RNG in the adjacent monitoring room. And third, any other person with knowledge of electronics could have similarly affected the test system's functioning by simply tapping into the same wire feed. However, with regard to such claims, H. Schmidt (1987b) pointed out:

> [Participant] fraud would have required, apart from specific electronics knowledge, much undisturbed time for opening the bottom plate of the testing machine and feeding in electric pulses in order to fool the internal counters as well as the external recorder. . . . I was personally present in all tests, with the exception of a

> small part of the sessions with one [participant], and . . . the scores in these few sessions were not higher than the other scores. In none of my subsequently reported experiments was there any less stringent [participant] supervision. (H. Schmidt, 1987b, p. 609)

Hansel (1981), Hyman (1981), and Alcock (1987, 1988) argued that there might have been randomization issues with Schmidt's work, in that he may not have conducted sufficient checks to ensure that his RNGs were producing adequately random output and were free of biased outcomes over the course of both short- and long-term sampling runs. However, H. Schmidt (1970a, 1973) did report several long-term randomness checks that were run outside of the experimental context (with no one being present), all of which were statistically nonsignificant and thus sufficiently in line with expected randomness. He further noted that none of these checks revealed any indications of biased output (H. Schmidt, 1987b), and Akers (1987), who utilized RNGs designed by Schmidt in his own experiments, had also stated:

> I do not see any crippling design defects, such as generator bias, that provide an easy explanation for his results. Schmidt's control runs, though not strictly counterbalanced with experimental runs, were extensive, and they were conducted throughout the experiment. These control runs, *even when cut into small segments*, did not exhibit any evidence of short-term bias. (Akers, 1987, p. 567, emphasis in original)

Hansel (1980), Hyman (1981), and Alcock (1987, 1988) further claimed that rather than maintaining some degree of consistency by focusing on one particular type of RNG and/or test design, Schmidt would frequently skip from one type of RNG and/or test design to another, which kept him from ". . . focusing on a given research question, or refining his measurements, or examining the effects of various parameters in that particular situation, or working with one type of generator over a period of time so that he and others can come to appreciate its idiosyncrasies" (Alcock, 1987, p. 560). In response, H. Schmidt (1987b) noted that although their circuitry components tended to change over time (following advances in electronics technology), all of his RNGs commonly utilized the same source of randomness:

radioactive decay (or later, electronic noise; see H. Schmidt, 1973). H. Schmidt (1987b) rationalized his changes in test design by further pointing out:

> Even in a research effort that is very well focused, there will be side roads. We will want to explore, for example, other random generators and other forms of feedback to be reasonably sure that we don't overlook other, possibly more efficient, approaches to psi testing. On the other hand, we have to be selective because each study takes much time and effort. It therefore often seems more reasonable to postpone the study of some details until we have pursued the main questions that should contribute most to our understanding of the overall picture. (H. Schmidt, 1987b, p. 609)

And of the three critics, Hyman (1981) seemed to be the only one to partly recognize that finding similar PK effects occurring across multiple types of RNGs would go toward ". . . the desirable property of achieving generality among devices" (p. 37), helping to address the potential criticism that successful PK results obtained with one type of RNG might simply be due to bias-prone operational defects (or "bugs") in that particular type of RNG.

Alcock (1987, 1988) and Akers (1987) alluded to one RNG-PK experiment conducted by Schmidt that was particularly well-designed to guard against fraud and error, in that certain crucial parts of the procedure (namely, the random assignment of the target directions that participants should aim for, and evaluation of the resulting data) were supervised by independent observers (Schmidt et al., 1986). Although the overall result was significant ($z = 2.71$, $p = .0032$), Alcock and Akers both took a cautious "let's wait and see" stance, urging that further replications using the same type of design were necessary. It turns out that this particular experiment was the first in a series of five (Schmidt & Braud, 1993; Schmidt et al., 1986; Schmidt et al., 1994; Schmidt & Schlitz, 1989; Schmidt & Stapp, 1993) that Schmidt conducted with independent observers. Three of these five experiments had overall outcomes at or exceeding the conventional level of statistical significance (i.e., $z \geq 1.64$, $p \leq .05$), and when evaluated altogether their results remained highly significant ($Z = 3.67$, $p = .00012$), with an associated odds ratio of about 8,200 to one (H. Schmidt, 1993a). This seemed to indicate that positive

PK results were still achievable in Schmidt's experiments even when the conditions were tightly monitored and controlled.

Criticism was also leveled by Alcock (1988) at the PEAR research: Although he acknowledged the greater sophistication of the PEAR benchmark RNGs, Alcock (1988) still expressed concern about their randomness, asking: "[D]oes the machine when unaffected by the attempted influence of the [participant] produce output consistent with theoretical expectation" (p. 43)? To find out, the PEAR staff conducted extensive calibrations of their benchmark RNGs over the course of their 12-year program (Jahn et al., 1997), which, as noted previously, amounted to just over 5.8 million trials. The outcomes of these trials were found to conform well with the expected parameters of the underlying binomial distribution and were sufficiently in line with chance expectation overall ($Z = -0.83$, $p = .409$). As an illustrative case in point, an examination of one subset of the calibration data collected by the PEAR staff, amounting to 50,000 calibration trials, reveals a flat, level random walk (Figure 8), with no consistent directional shift away from expectation being exhibited overall (Nelson et al., 1989).

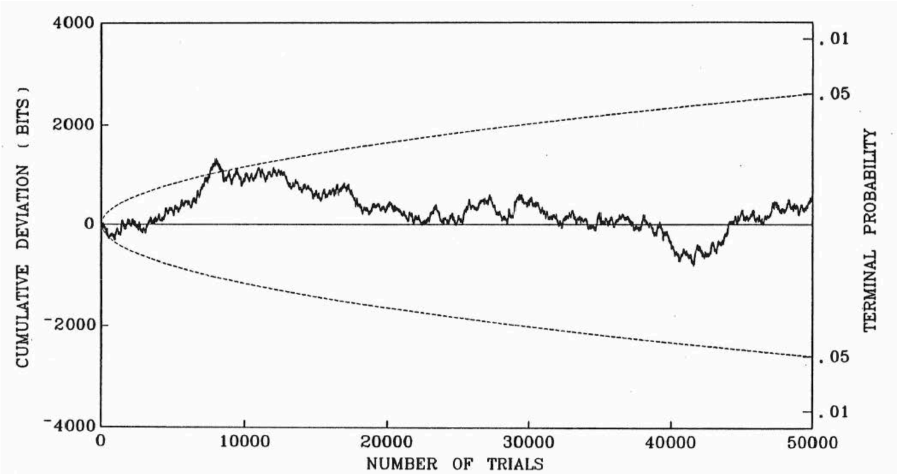

Figure 8. Cumulative deviation plot displaying an example subset of the extensive series of benchmark RNG calibration data collected by PEAR during its 12-year experimental program. Compare the random walk exhibited by these calibration data with the illustrative example shown in Figure 2. Adapted from figure 7 of Nelson et al. (1989).

Other criticisms of the PEAR research were later raised by Jeffers (1999, 2006), partly in relation to three micro-PK experiments which he independently conducted in collaboration with others (Freedman et al., 2003; Ibison & Jeffers, 1998; Jeffers & Sloan, 1992), and which were apparently motivated by the PEAR benchmark RNG program. Two of these experiments (Ibison & Jeffers, 1998; Jeffers & Sloan, 1992) had produced results consistent with chance, and have been touted in some skeptical circles as evidence which casts serious doubt on the results of the PEAR program. However, it is important to note that these two experiments cannot really be considered close replications of the PEAR benchmark RNG experiment, for they differed markedly from the latter in the type of PK target that was utilized: Rather than having participants try and mentally influence a benchmark-type RNG in these two experiments, Jeffers and his colleagues instead had them attempt to influence a more complex target: namely, the diffraction pattern produced by photons passing through a single- or double-slit screen (a well-recognized experimental setup often used to demonstrate the concept of wave–particle duality in physics). Although PEAR did conduct a few PK experiments early on using a similar kind of diffraction setup (Jahn, 1982, pp. 141–143; Nelson, Dunne, & Jahn, 1982), they were not as extensive or as successful as the benchmark RNG program.

The third experiment (Freedman et al., 2003) came much closer to replicating the PEAR benchmark RNG test, in that an RNG similar in design to the benchmark-type was actually utilized as a PK target. But even that experiment might only be considered a *conceptual* replication at best, because as Dobyns (2003) pointed out, there were still several other differences of note: For instance, Freedman et al.'s participant pool comprised neurological patients and hospital staff, whereas the PEAR participant pool was made up entirely of healthy, unselected volunteers. Also, the only subset of Freedman et al.'s data that could be considered the closest in comparison with that of the PEAR database was the subset collected with the hospital staff (being the individuals who were healthy), which had a nonsignificant overall outcome. However, the number of PK test trials that Freedman et al. collected with these participants was much smaller (94,000) compared to that of the PEAR database (over 1.6 million), which, considering the small size of micro-PK effects on average, might not have afforded enough statistical power to reliably detect such effects with these participants. (More will be said about this third experiment in a later subsection on neuropsychological factors.)

In discussing these three experiments in relation to the PEAR research, Jeffers (2006) has stated that:

> One characteristic of the methodology in experiments in which I have been involved is that for every experiment conducted in which a human has consciously tried to bias the outcome, another experiment has been conducted immediately following the first when the human participant is instructed to ignore the apparatus [i.e., the slit set-up in the case of his first two experiments, or the RNG in the case of his third experiment with the neurological patients]. Our criterion for significance is thus derived by comparing the two sets of experiments. This is not the methodology of the PEAR group, which chooses to only occasionally run a calibration test of the degree of randomness of their apparatus. (Jeffers, 2006, pp. 55–56)

Although Jeffers (2006) has claimed that this method of control is "scientifically more sound" than the one utilized by PEAR (p. 56), Dobyns (2003) has pointed out several statistical and procedural ways in which it can potentially be vulnerable to artifacts and loss of statistical power. In addition, it can be argued that Jeffers' method may be susceptible to a potential confound; to see how, it is imperative to briefly draw attention to a few relevant points: The first is that knowledge regarding the duration and reach of PK (and psi in general) remains appreciably small at this point in time, and for that reason there is no reliable way known at present in which to strictly gauge or control the manifestation of PK within a given temporal period (in other words, there is currently no known reliable way, as of yet, to precisely begin or stop PK from occurring at a given time). The other point is that it seems that participants in micro-PK experiments do not necessarily have to direct their attention towards an RNG in order to be able to affect its output; perhaps one of the clearest lines of evidence suggesting this is that of the experimental findings obtained with field RNGs, where significant deviations were observed in the RNG data despite there being no overt signs of intention or attention being directed toward the RNGs by the involved groups.

Arguably, both of these points could potentially factor into a test scenario akin to the one used by Jeffers (where collection of the control data immediately follows the collection of the experimental PK test data in a consecutive fashion) by considering the possibility that residual PK effects from the experimental period might sometimes unintentionally carry (or "bleed") over into the subsequent control period. Such a possibility has previously been given some consideration by the PEAR staff in relation to their field RNG experiments:

> When it is feasible to take [matching control] data in a given environment before and after the designated experimental segments, some of the surround time periods themselves may be subject to the same influences as the active segments. (Indeed, even in laboratory experiments there is evidence that traditional "control" data may not be immune to anomalous effects of consciousness). (Nelson et al., 1998, p. 452)

If this is plausible to some degree, then it may suggest that control data collected immediately following the experimental PK test could potentially be confounded as to whether the data would be totally free from the lingering effects of PK from the preceding experimental period, thus raising the question of whether they would constitute "pure" control data. On such a basis, one might argue that it would be preferable to temporally space apart the experimental and control periods in order to minimize any potential PK "contamination" of the latter.

Jeffers (1999, 2006) has further claimed that a problem exists with the baseline data collected by PEAR as part of the benchmark RNG program (the "BL" trace in Figure 3). He specifically points out that rather than maintaining a level random walk around expectation, the cumulative deviation plot of the BL data exhibits a modestly increasing shift away from expectation over the course of the program, which eventually exceeds the $p = .05$ threshold of statistical significance to a slight degree (with a terminal p-value of .04). Based on this observation, Jeffers (1999) argued ". . . that the behavior of the REG when examined over a 12-year baseline shows evidence of a slow but imperceptible drift indicating that it is not a random device as claimed [by PEAR]. Doubt must then be cast on the claimed terminal probabilities for the cumulative deviation plots with expressed operator intention [i.e., the HI & LO data plots in Figure 3]" (pp. 328–329). He continued to maintain this argument several years later (Jeffers, 2006, p. 56).

However, it is important to carefully recognize that Jeffers' argument seems to hinge upon an assumption that the term *baseline* is synonymous with the term *control* in this case (an assumption that also seems to have been made by Alcock [1988, p. 39] in his own evaluation of the PEAR research). An implicit hint of this assumption is indicated in certain statements Jeffers has made in reference to the baseline data, where he framed them in the context of data being collected in situations in which participants ". . . are *instructed not to interact* with the [RNG] device" (Jeffers, 1999, p. 329, emphasis added) and ". . . in which *no effort is made* to bias the equipment" (Jeffers, 2006, p. 56, emphasis added).

Such a characterization of the baseline data as control data turns out to be inaccurate in this case, as a careful reading of PEAR's main experimental report on the benchmark RNG program (Jahn et al., 1997), as well as various other PEAR publications (e.g., Jahn & Dunne, 1986, 2005; Nelson et al., 1986) would indicate that *the baseline condition in fact represents one of the experimental conditions*, where participants *are indeed instructed to intentionally interact with the RNG*, but only with the intent of trying to maintain as close to a steady baseline as possible. For this reason, the PEAR staff (Jahn et al., 1999) has cautioned that, in the case of their PK experiments with RNGs and other random systems,

> . . . baselines never should be regarded as control or calibration data. They comprise a *third* condition of operator [i.e., participant] intention (albeit probably subconscious), and display many of the same structural features as the high- and low-intention data, including mean shifts. . . . Rather the proper comparison standards for the active [i.e., experimental] data, for the baseline data, and for qualification of the equipment, are the calibration data, taken with no operators (or anyone else) in the experimental room. (p. 330, italics in original)

As mentioned with regard to Alcock's criticisms, the calibration data were found to conform well to chance as expected (Figure 8).

Moreover, with regard to the baseline data, the PEAR staff (Jahn et al., 1997, 1999) pointed out that since there is no pre-specified prediction of an intentional shift in a certain direction (positive or negative) being made for those particular data, they should be properly evaluated using two-tailed statistics, leading the marginally significant terminal *p*-value of .04 for the overall outcome of the BL data (Figure 3) to be reduced to a nonsignificant .09. Thus, even if one were to take the baseline data as being control data, they still would not achieve a sufficient level of statistical significance for them to be viewed as being questionably non-random.

Among the more general claims that critics tend to maintain about experiments relating to micro-PK (and psi in general) is that there is a considerable (negative) correlation between experimental quality and effect size, such that PK effects start to decrease as more safeguards are added to the experiments. Meta-analytical assessments do not provide a clear indication of this, however, as only one RNG meta-analysis (Bösch et al., 2006a) has found a very small ($r = 0.15$, $p = .004$) correlation in line with this claim so far,[9] while at least three others (Radin, 2006, p. 158; Radin & Nelson, 1989, 2003) have uncovered no significant correlation. In addition, the odd standing meta-analysis (Bösch et al., 2006a) had found that the average quality of the experiments actually tended to be fairly high, with more than 40% of them being rated highly for implementing the safeguards being assessed (pp. 507–508). Thus, there may be some initial suggestion that this critical claim may not be a serious one, although further meta-analytical assessments would be necessary for better clarification.

Another general claim that some critics have maintained more recently is that rather than being reflective of a micro-PK effect, the small but significantly positive results obtained in RNG-PK meta-analyses may instead reflect the effect of selective reporting (also known as the “file-drawer” effect), where it is claimed that researchers have a tendency to report only those experimental findings that are in line with the test hypothesis (especially if they are significantly so), while actively suppressing the public release of any other findings which are close to chance and/or tend to refute the test hypothesis (by supposedly keeping those findings locked away in the drawer of a file cabinet); such a bias-driven practice would have the effect of artificially skewing the combined data more toward a significantly positive outcome. This has been proposed as a way to possibly account for the range of widely varying effect sizes that have been found within the experimental micro-PK database; selective reporting would tend to leave wide gaps between the outcomes of one experiment to the next, effectively making them non-uniform (or unbalanced) in distribution (Bösch et al., 2006a, 2006b; Pallikari, 2015; Schub, 2006).

To address this claim, some analyses have demonstrated that the results remain significantly positive ($p = .00033$) even after being adjusted using statistical "trim and fill" algorithms designed to take the effects of selective reporting into account (Radin, 2006, p. 157), and an informal survey made by Radin et al. (2006a) of researchers in the parapsychological community who were known to have conducted RNG-PK experiments in the past had led to an estimate of about only 59 experiments going unreported, which falls well short of Bösch et al.'s (2006a) file-drawer estimate of 1,544 experiments that would be necessary to nullify the significance of their meta-analysis. Radin et al. (2006a) also note that some of these unreported experiments were said to have had significant results, which, if true, would stand in direct contradiction to the claim inherent in selective reporting that researchers actively seek to suppress negative or null findings.

In addition, Helfrich (2007) and Varvoglis and Bancel (2015) pointed toward there being an excessive amount of highly significant experiments (with z-scores ≥ 2.60) contained within both tails of the PK-RNG z-score distribution. As Varvoglis and Bancel (2015) have argued, reasonably accounting for these experiments by selective reporting can be quite challenging, as the existence of an extremely large file drawer would be necessary.

Radin et al. (2006a, 2006b) have also pointed out that it is possible that the wide range of RNG-PK effect sizes may be due to other factors besides selective reporting. They noted that some critics (e.g., Bösch et al., 2006a; Schub, 2006) have made the assumption that in these experiments, PK ". . . 'operates' by uniformly influencing each generated random bit regardless of the number of bits used per sample, the rate at which bits are produced, or the psychological conditions of the task" (Radin et al., 2006b, p. 362), and they argued that:

> The problem with this assumption is that there is no valid reason to expect that [PK] should behave this way. Indeed, we are unaware of any sort of human performance that is unaffected by such parametric changes. For example, a factory worker who can identify defective parts with 100% accuracy as they roll by at one or two per second will do no better than chance if the conveyor belt is suddenly accelerated to 1,000 parts per second. And yet the bit rate in the various [RNG-PK] experiments range over not just three orders of magnitude, but six orders, and they involve significant physical differences in the underlying mechanisms for generating random bits.
>
> Then there are the psychological considerations. Let's say we conduct two RNG experiments. The first involves 1,000 highly experienced meditators, each of whom is selected based on his or her performance on previous, similar RNG tasks. Each participant is asked by a cordial, enthusiastic investigator to engage in a daily intention-focusing practice for six months in preparation for the experiment. When the experiment occurs, the participant is asked to intentionally influence the generation of a single truly random bit. The outcome of that one random decision will be provided as feedback either as a moment of silence (counted as a miss) or as a breathtaking fireworks display (a hit). If the fireworks display is observed, then the participant will also win something especially meaningful, like a scholarship or other accolade. In the second RNG study, a bored student investigator indifferently recruits an apathetic college sophomore, who is asked to press a button and mentally influence 1,000 random bits generated in one second, with no feedback of the results, and with no consequences regardless of the outcome.
>
> The physical context of these two studies may be identical, employing the same RNG and the same statistics to evaluate the resulting datasets, each of which consists of 1,000 random bits. But it is clear that the psychological contexts differ radically. If we presumed that the only important factor in this type of experiment was the number of bits generated, then the two studies should provide about the same outcome. But if [PK] effects are moderated by variables such as the amount of time or effort one can apply in focusing mental intention towards the random events, or one's skill, or motivation, or how the bits were generated, then the first study might well result in an effect size orders of magnitude larger than the second. (Radin et al. 2006c, pp. 362–363)

Radin et al. (2006a, 2006b) further argued that a focus solely upon the physical aspects, while being appropriate for experiments devoted to pure physics, is not appropriate for experiments in PK, where psychological and other subjective factors can add a greater amount of variability that may ultimately affect the outcome.

As one means of addressing the issue of selective reporting in an empirical fashion, Bösch et al. (2006b) suggested that a psi study registry be established and broadly utilized, which would allow researchers to make the specific design and analytical details of any planned experiments (as well as meta-analyses) more widely available *prior to* proceeding with the collection of any data. Their suggestion has since become a realization under the auspices of the University of Edinburgh's Koestler Parapsychology Unit (Watt & Kennedy, 2015, 2017), and should be considered for use in the development of future studies.

PROCESS-ORIENTED RESEARCH: WHAT FACTORS MIGHT BE CONDUCIVE TO PK?

If it can be assumed that the experimental results examined so far do provide some evidence for a micro-PK effect in totality, then the next question that might naturally come to mind is: Are there any physical, biological, psychological, or technical design factors that might affect micro-PK performance?

Technical Design Factors

Pseudo-random RNGs. While some types of RNGs can be considered "truly" random, in the sense that they have been designed to produce output based on purely physical processes which are inherently unpredictable, such as radioactivity (Aguayo et al., 1996; H. Schmidt, 1970b, 1970c, 1977; Vincent, 1970) and electronic noise (Nelson et al., 1989), there are also some types of RNGs that can be considered "pseudo"-random, in which they have been designed to base their outputs on processes which are inherently nonphysical and more deterministic, such as mathematical algorithms (for a general overview of these latter types of RNGs and their possible limitations, see Radin, 1985). One might be led to wonder: If human mental intention can potentially influence the output of truly random RNGs, then could it also potentially influence the output of pseudo-random RNGs, as well?

One of the earliest efforts to explore this question was made by Tremmel and Honorton (1980), who examined the performance of more than 40 unselected participants on two PK tests using electronic noise RNGs, as well as on a third test using an algorithm-based RNG. While the combined data from the tests with the noise-based RNGs resulted in significantly above chance scoring ($z = 2.47$, $p = .018$), the test data with the algorithm-based RNG were found to be nonsignificant.

In one of his experiments, H. Schmidt (1981) used binary number sequences pseudo-randomly derived from an algorithm to control the directional rotation (clockwise or counterclockwise) of his lamp-lit circular visual feedback display. Significant results were obtained with both unselected volunteers ($z = 2.19$, $p = .014$) and selected participants ($z = 3.42$, $p = .0003$). One should note, however, that the test sequences used in this experiment were not pseudo-random in the purest sense, in that the starting seed numbers (which were entered into the algorithm to generate the sequences) were initially obtained beforehand in a truly random fashion through radioactive sampling.

Lowry (1981) designed and participated in two computer-controlled experiments in which he attempted mental influence upon a series of random digits (ranging from 1 to 4) being generated from an algorithm activated 0.2 seconds after the press of a key at the start of each test trial. The first experiment resulted in a success rate that was significantly below MCE (24.1%, where 25% was expected; $z = -2.61$, $p = .009$, two-tailed), although a cluster of positive scores was found in the first half of the test runs. Efforts to reproduce this positive cluster became the focus of the second experiment, which resulted in a success rate significantly above MCE (27.1%, $z = 4.23$, $p = .000023$, two-tailed).

In his effort to replicate Schmidt's work, Radin (1982a; Radin & Utts, 1989) conducted a series of 27 experiments in which he and other selected or unselected participants each attempted to influence an RNG controlling the lamp-lit circular visual feedback display. The RNG could be set up so that its output was produced either in a truly random fashion (based on radioactive sampling) or in a pseudo-random fashion (using seed numbers fed into an algorithm). The combined data across the entire experimental series resulted in a significant outcome ($Z = 2.94$, $p = .002$), and while not being significant on their own, the experiments in which pseudo-random number sequences were used had modestly contributed to the positive outcome ($Z = 1.12$).

Radin (1982b) further carried out a series of four experiments using computer-based tests relying on pseudo-random number sequences derived from the 32-bit UNIX *Rand* algorithm function. Overall, five of the 18 statistical tests performed on the resulting data from these experiments were found to be significant at the $p < .05$ level (exact binomial $p < .005$).

Initially, most of these findings seem to stand in contrast with those of PEAR (Dobyns, 2015, pp. 225–227; Jahn et al., 1997; Nelson et al., 2000), which, in an extensive series of just over 400 experiments, found no significant indications that algorithm-generated pseudo-random number sequences (seeded by stops of a computer clock) could be influenced by mental intention. However, closer examination has seemingly revealed possible hints of modestly deviating HI and LO trends present in the PEAR pseudo-random data (with one acting in line with, and one acting opposite to, intention), which may be counteracting each other in such a way that happens to lead to an overall null result (Jahn & Dunne, 2011, Ch. 6).

Thus, while a small number of experiments seem to initially suggest that pseudo-RNGs can possibly be influenced by PK, some degree of ambiguity still surrounds the issue and further clarifying data are needed.

RNG Bit Rate. Does increasing the number of bits being sampled from the RNG per second affect micro-PK performance in any way? As mentioned previously, initial exploration of this was made early on by H. Schmidt (1973), when he compared the scores obtained in PK tests where the RNG was set to run at two different bit generation speeds: a relatively slow speed (30 bits per second), and a relatively fast speed (300 bits per second). Though quite small, a significant difference ($z_{diff} = 4.75$, $p = 1.02 \times 10^{-6}$) was indeed found between the slow (51.6%) and fast (50.3%) success rates, on average.

H. Schmidt (1974b) further explored the question in a more indirect fashion through a formal study in which he compared the scores obtained in PK tests involving a relatively simple RNG (where a single bit was generated for each test trial based on radioactive sampling) with those obtained in tests involving a relatively complex RNG (where the bit for each trial was instead determined in a more complex fashion by taking a majority count—either of "1"s or "0"s—of a sequence of 100 binary numbers rapidly generated by sampling electronic noise). While a pilot experiment did initially indicate a significant difference (z_{diff} = 2.20, p = .028, two-tailed) between the simple (58%) and complex (51.2%) success rates, it was not found again in a subsequent and larger confirmatory experiment, although the average success rate with the simple RNG remained slightly higher (55.3%) than that with the complex RNG (53.8%).

Exploration of this question was also independently pursued by the staff of PEAR (Dobyns et al., 2004; Jahn et al., 1997) several years later, when they began looking at how the PK effect sizes changed as the bit rate on the benchmark RNG was set at three different sampling speeds: 20 bits per second, 200 bits per second (the standard rate used in most PEAR experiments), and 2,000 bits per second. The particular aim here was to explore a "bitwise influence" model of PK, which posits a possible PK operation in which mental intention is assumed to influence each individual bit being generated by the RNG, shifting its underlying probability more toward the likelihood of an intended (or desired) outcome. If PK happens to operate by such a mechanism, then one might predict that as the number of generated bits is increased, PK effect sizes should also start to increase, as well (in other words, higher bit rates should lead to bigger PK effects, according to this model).

Initially, the resulting data seemed to indicate that the PK effect sizes were indeed gradually increasing in a semi-linear fashion as the bit rates got higher; in other words, the PK effects did seem to be getting slightly bigger as the number of sampled bits went up, just as the bitwise influence model would predict. However, further study by Ibison (1998) also seemed to indicate that when the rate was set extremely fast (using a high-speed electronic noise source that generated 2,000,000 bits per second), a marked change unexpectedly occurred: The PK effect inverted itself from positive to negative, to a highly significant degree (Z = –3.56, p = .00037). On the surface, such a finding would seem to contradict the bitwise influence model.

This latter finding spurred a second, replication-oriented phase of this experiment (dubbed "MegaREG"; Dobyns et al., 2004) by PEAR. PK test trials generated at a rate of 2 million bits per second were randomly interspersed with trials of 200 bits per second, with both participants and experimenters being kept unaware of which trials were which (so as to control for any differences in performance or analysis that might result from having distinct knowledge of the two conditions); the remaining features and procedures of the PK test were otherwise kept the same as those used in the benchmark RNG experiment. A highly significant result was found for the 2 million-bit rate trials that was again flipped in sign ($T = -4.03$, 105 *df*, $p = .000057$, two-tailed),[10] again contradicting the prediction of the bitwise influence model.

However, the initial rise in PK effect size that was observed as the bit rate increased from 20 bits/sec to 2,000 bits/sec, followed by the sudden fall from positive to negative at 2 million bits/sec, would seem to hint at the possibility that perhaps somewhere between 2,000 bits/sec and 2 million bits/sec, there might be a particular "optimum" bit rate at which the PK effect size attains its highest value (i.e., it "peaks"), and beyond which the effect size inverts direction and starts to decrease. Could there be such a hypothetical "optimum" bit rate? Exploration of this question was pursued by K. Alexander (2019) as he sought to independently replicate and expand upon the MegaREG experiment. In his study, participants were asked to try and influence the output of a custom-made, four-unit RNG device that could be set to run at any one of 10 differing bit rates ranging from 200 bits/sec up to 16 million bits/sec. Although there was an indication that the PK effect sizes were gradually increasing again as the bit rates got higher (as suggested by another positive semi-linear trend, which was in line with the bitwise model prediction and inconsistent with the inversion observed in the original MegaREG experiment), the resulting data remained somewhat ambiguous because the effect sizes also seemed to be alternately switching from positive to negative (and vice-versa) in a "zigzagging" fashion as the bit rates got higher (i.e., some bit rates were correlated with a positive effect size, while others were correlated with a negative effect size). Thus, it still remains unclear as to whether PK performance might be affected by bit rate, and additional clarifying data are needed.

Physical Factors

Spatial Distance. Several of the known physical forces, such as gravity and the electrostatic force, have been found to obey a limitation that depends on spatial distance, which can be mathematically modeled and described by the inverse-square function: The strength of the force decreases as the inverse square of the distance from its source (Young, 2012, Sect. 0.4). If one were to assume, simply for the sake of conceptualization, that it might represent a unique kind of force (namely, a mental "force" with some degree of direct physical analog), then the question arises: Might PK have any kind of spatial limit of its own?

One aspect of the PEAR program (dubbed "Remote REG") had initially explored this question over a six-year period by having participants attempt to influence the output of a benchmark RNG from various distances away, with the spatial separation between the participant and the RNG ranging from less than one mile to more than 5,000 miles (Dunne & Jahn, 1992). Nearly 1.5 million bits were generated in total, and like with the standard (local) benchmark RNG experiment the overall result (again expressed as the difference between the HI and LO conditions) was small but significant ($Z_{diff} = 2.23$, $p = .013$), suggesting that the participants' attempted PK influence was effective regardless of how far away they were from the RNG. The results suggested that the influence tended to be most effective in the HI condition, which was highly significant ($z = 3.19$, $p = .0007$).

The spatial question was further explored in two experiments conducted by Tressoldi et al. (2014) just over two decades later. In the pilot experiment, small groups of three to seven participants were asked to try and collectively influence the output of a remotely activated RNG located 190 kilometers (approx. 118.1 miles) away over the course of a relatively short influence period (ranging from 1 to 3.3 minutes). Success was defined by the RNG's output achieving or exceeding a defined cutoff threshold set at the conventional level of statistical significance (i.e., $Z \geq 1.64$, $p \leq .05$) in either direction (positive or negative). Out of the 50 pilot sessions, 39 of them (78%) were found to have reached or exceeded this threshold. In contrast, only about half (48%) of the matching 50 control sessions had reached or exceeded the threshold, with the difference favoring the PK hypothesis by a Bayesian odds factor of 52.4.

The larger confirmatory experiment comprised 102 test sessions in which participants attempted to individually influence the remote RNG's output over the course of a shorter influence period (one minute).

Spatial distance between the participant and the RNG ranged from 4 to 1,512 kilometers (approx. 2.49 to 939.5 miles). Eighty-four (82.3%) of the test sessions were found to have reached or exceeded the threshold, whereas only 14 (13.7%) of the control sessions did so—a difference again favoring the PK hypothesis by a Bayesian odds factor of 7.3×10^{11}.

Tressoldi et al. (2020) later conducted a third experiment to explore the spatial question in which participants individually tried to influence a custom-built RNG device located between 15 and 4,000 kilometers (approx. 9.3 to 2,485 miles) away, over the course of a 15-minute Skype-based test session (again with a one-minute influence period for each of the 15 test trials that comprised the session). Compared to the matching control data, the experimental data were found to exhibit an approximately 50% higher amount of trials which met or exceeded the success threshold (again set to $Z \geq 1.64$, $p \leq .05$).

An examination of the spatial question was also made by Bancel (2015) in a field RNG context using the data gathered by the Global Consciousness Project, in order to see whether the RNG network data associated with major world events (i.e., those which tend to have a widespread impact upon human populations spread around the world) might be notably distinct from the data associated with relatively minor world events (i.e., those with an impact which may be more limited to relatively smaller populations located in certain regions of the world). And indeed, a significant difference ($Z_{diff} = 2.20$, $p = .014$) was indicated: The presumed group PK effects associated with minor world events seemed to exhibit a significant decline ($Z = -2.56$) with increasing distance from the area where the minor events were taking place, suggesting that the effects tended to be limited to the RNG network nodes positioned in the geographic areas immediately surrounding those events. This contrasted with the effects associated with major world events, which did not exhibit any such decline ($Z = 0.76$), suggesting that these effects tended to be distributed more globally across the various nodes of the RNG network.

These findings would seem to initially suggest that micro-PK does not really exhibit much of a spatial dependence, although additional replications would be valuable in further confirming this. It is also important to note that the PK influence periods used in the experiments by Tressoldi et al. (2014, 2020) were relatively short in length, and given the small effect sizes that tend to be associated with RNG-based micro-PK, longer periods of sampling may be a crucial factor to consider in any future replications.

Geomagnetic Correlates. Considerable effort has been made over the years to determine whether fluctuations in the Earth's magnetic field, which can often be influenced by the solar wind (Campbell, 1997; Lyon, 2000), might have an effect upon human health and behavior (Close, 2012; Palmer et al., 2006; Zenchenko & Breus, 2021). This may lead one to wonder: Might psi phenomena possibly exhibit any correlation with geomagnetic activity? A number of attempts have been made to empirically explore this question with regard to ESP, and while there seems to be a hint of a possible inverse (or negative) correlation in some cases, the study designs and results have generally been mixed and the answer remains far from clear (Ryan & Spottiswoode, 2015).

Fewer studies of this type have been done so far with regard to micro-PK: One of the earliest attempts was conducted by the PEAR staff in the late 1980s (Nelson & Dunne, 1987) by statistically comparing their extensive benchmark RNG database with the average antipodal (*aa*) index (a daily measure of geomagnetic activity derived from data collected from two monitoring stations, with each positioned in one of the Earth's two hemispheres; Campbell, 1997, p. 167) for the northern hemisphere. A slight negative correlation was indicated ($r = -0.028$) only for the RNG data in the "LO" condition, although it was not significant overall. No other indications of a correlation were found for the rest of the RNG database.

Three attempts were made in the 1990s: In the first, Gissurarson (1992b) compared the scoring data from eleven PK experiments using an RNG-based computer game design with the *K*-index values (a three-hour average measure of horizontal geomagnetic field variation based on data collected from 11 or more monitoring stations positioned around the world; Campbell, 1997, p. 161) obtained from stations locally positioned closest to the laboratories where the experiments were conducted. No significant overall correlation was found, although there was a weakly suggestive hint ($p = .078$) that "[h]igh scores were preceded by high geomagnetic activity, and low scores were preceded by low geomagnetic activity" (p. 163).

In the second, Bierman and van Gelderen (1994) compared the planetary *A* index *(PAI)* values with the data from a computer-based PK experiment in which 16 participants tried (on two separate occasions) to influence an RNG programmed to generate bits at a fast and a slow rate. Only a slightly negative but nonsignificant correlation ($r = -0.19$) was found overall.

In the third, Broughton and Higgins (1994) each tested themselves in a separate series of runs on a computer-based PK test, and their scores on the initial runs for each test in the series were combined and compared against the *Ap*-index values (an averaged planetary measure derived from the *K*-index that reflects "the equivalent daily amplitude"; Campbell, 1997, p. 163). A slightly negative correlation (Spearman's *rho* = –0.17) resulted, which was also nonsignificant overall.

Most recently, Caswell et al. (2014) examined the *Kp* index values (the averaged planetary measure of the *K*-index values taken across 11 monitoring stations; Campbell, 1997, p. 161) in relation to PK test data collected from 26 participants who tried to influence a computer-controlled RNG. Significant Pearson ($r = 0.54$, $p < .01$) and Spearman (*rho* = 0.41, $p < .05$) correlations resulted when the data at the time of the test were compared with data taken three hours before and after the test, although some limitations are important to note with regard to these results: First, the participants in this study took part in only one test session, with the PK influence period being only about five to eight minutes in length, which (again considering the small effect sizes involved in RNG-based PK tests) is likely to be too short of a sampling period to reliably detect any micro-PK effect. Second, the *Kp* values were compared against RNG-PK test data averages of only one minute in length, and in using such short temporal samples, it is not clear how deviations produced by simple chance-expected fluctuations in the random background noise of the RNG were taken into account. In addition to making it unclear whether they might be due to PK (as there is currently no unambiguous way to distinguish between small PK-related deviations and fluctuations of pure noise at this narrow level of examination), such short-lived fluctuations in the RNG data could have potentially inflated the resulting correlations.

Although some of these findings seem to hint at a possible negative correlation between RNG-PK and geomagnetic activity, the relative lack of statistical significance across studies tend to make this far from clear; part of this issue may be related to the relatively small RNG sample sizes used. In addition, Hubbard and May (1987) have pointed out that since a geomagnetic index

> . . . is determined from a weighted average of data reported from [multiple] worldwide measurement stations, the GMF activity may be relatively quiet in one area while the index is reflecting a magnetic disturbance detected at several other sites. Geomagnetic indices are therefore poor indicators of the local magnetic conditions that may possibly influence psi performance. (Hubbard & May, 1987, p. 80)

To further explore any possible correlation with geomagnetic activity, they suggest that psi test data should instead be examined in relation to direct local measurements of the ambient geomagnetic field taken in the immediate area where the psi test is being conducted.

Some exploratory findings by Stevens (2005) would also seem to hint at the initial possibility that ". . . the REG target system used in many micro-PK studies might be a sensitive detector of geomagnetic fluctuations, possibly through a process of stochastic resonance with intrinsic and/or external noise" (p. 143), which might account for some of the varying findings observed in PK studies using RNGs, though this remains a tentative consideration at present.[11]

Some findings by Radin (1993b, 1996) would also seem to hint at the initial possibility that PK (and perhaps psi functioning in general) could be correlated with more than one physical variable in a rather complex way, a consideration that may make the search for potential correlates on the individual level a bit more challenging.

Biological Factors

Gender. The question of whether there might be any distinct cognitive (and other behavioral) differences between females and males has been the subject of much study and debate (Miller & Halpern, 2014). This may lead some to wonder: Do females and males show any differences in psi performance? Many of the study findings have presented a mixed and unclear picture with regard to ESP (Schmeidler, 1994, p. 152), but what about for micro-PK? To date, addressing the latter question has been quite limited:

In his examination of the data from 11 PK experiments using an RNG-based computer game design, Gissurarson (1992b) found a significant directional difference ($F_{1,619}$ = 5.25, p = .02, two-tailed) between the mean scores of male and female participants, with males tending to score slightly above chance, and females tending to score slightly below chance.

A more extensive examination was later conducted by Dunne (1998), using the PEAR benchmark RNG database. The combined results indicated that the 41 female participants contributing to the database had tended to achieve significantly positive scoring (z = 3.339, p = .0004) on the "HI" condition, whereas the 50 male participants achieved a more modest level of scoring (z = 1.228), leading to a higher overall HI–LO difference (Z_{diff} = 3.382, p = .0004) for females as compared to males (Z_{diff} = 1.875, p = .03). Part of this difference might be attributable to the larger data contributions made by females, however, with the female average database being nearly twice as large as the male average database. Some of these differential effects associated with females also seem to extend to other parts of the broader PEAR program database, as well (see Ch. 13 of Jahn & Dunne, 2011, for a concise overview).

Mental Factors

Co-Op Efforts.Might micro-PK effects start to linearly increase (or "get stronger") if two participants were to simultaneously focus their mental intention efforts on the same RNG? This question was initially explored by Dunne (1991) in a subset of 42 experiments within the PEAR benchmark database, in which pairs of selected participants attempted to influence the same RNG in a semi-cooperative manner by aiming their influence efforts in the same direction. Overall, this experimental subset resulted in a modestly significant HI–LO difference (Z_{diff} = 1.883, p = .03), with the size of the "co-op" effects being slightly (about 3.7 times) larger than those observed in efforts made by individual participants alone. Despite this, there were some indications that the efforts of paired participants did not simply increase in a linear fashion; as Dunne (1991) pointed out:

> . . . some of our most successful individual operators [i.e., participants] produce null effects when working together, while others

> with minimal or even negative individual results are able to produce strong positive yields as a pair. (p. 4)

This observation seems somewhat consistent with those made in earlier "co-op" PK studies conducted using dice (Feather & Rhine, 1969, 1971; Humphrey, 1947), which were also often mixed in outcome such that ". . . when the two [participants] tried for the same target face they either succeeded very well or missed very badly" (Feather & Rhine, 1971, p. 91).

In addition (and further extending the data relating to gender), efforts made by pairs of participants of the same gender were nonsignificant and tended to be in the direction opposite of the one they were cooperatively aiming for. Yet in a curious fashion, efforts made by participant pairs who were of opposite gender tended to achieve results that were in the intended direction, and which were significant overall ($z = 3.317$, $p < .0005$).

The reasons for these mixed outcomes among "co-op" pairs remains unknown, and with so few studies having been done so far, further clarifying data are needed.

Gaming/Competition. On the opposite end of the spectrum to "co-op" efforts are instances in which participants attempt to aim their mental influence efforts in opposing directions, in a semi-competitive fashion. As one means of trying to motivate participants and (in some cases) encourage this kind of playful competition, some RNG-based PK tests were designed to have a visually engaging electronic game-type interface; this largely began in the 1970s and 1980s, with the advent and wider availability of the personal computer (PC).

Two early tests of this type were used by Weiner (1978, 1979): The first one was a PC game designed to conceptually simulate the conditions of a competitive horse race, with the "horses" being represented by four columns of numbers that would separately increase at random over the course of a 50-trial "race." Each of the columns would start at zero and begin counting upward as the race was under way, with the output of an RNG randomly determining the moments when each one would be incremented to a higher value (so that at the end of the race, each column could result in a different value). The playful engagement aspect of the test involved asking participants to choose one of the four "horses" (columns) to lay a wager on (with varying levels of monetary risk), and then having them try to mentally influence their horse's placing in the race (presumably through PK on the RNG) such that it would achieve the highest numerical value in the end (and thereby win the race). None of the main analyses performed on the resulting RNG data were significant.

The second one was a PC test designed to explore a possible relation between PK and motor skill by having participants attempt to influence the unsteady motion of a bar on a computer screen, such that it would remain centered on the screen for as long as possible and not move off the edges of the screen. In one part of the test, the participant attempted this manually by using a control dial to help keep the bar steady. In another part, the participant attempted this using PK, with an RNG randomly determining whether the behavior of the bar would become increasingly unstable or not during each test trial. Although male participants had attained higher PK scores and motor skill results than females, the difference was found to be only suggestive ($F_{1,24} = 3.94$, $p = .056$).

Broughton (1979) asked participants to interact with an electronic RNG-based PK test device that was designed to mimic the familiar carnival tests of physical strength, in which a person uses a mallet to hit a lever with as much strength as they can muster in an attempt to send a weight high enough along a wire to hit a bell at the very top (only with this device, participants attempted this with their minds instead of their muscles, making it a test of "psychic strength," in a sense). Participants were tested individually or in a small, playfully competitive group setting among friends, but no significant deviations from chance were observed overall.

Honorton and his colleagues at the Psychophysical Research Laboratories (PRL) developed three types of RNG-based PK games, two of which utilized the custom-designed PsiLab II hardware RNG and associated software running on the popular Apple II computer of the 1980s (Berger & Honorton, 1985; Berger et al., 1986; PRL, 1985). One of them, called "Volition," produced a running display of the RNG output in a manner roughly equivalent to that of the cumulative deviation plot utilized by PEAR (Figure 3), showing the bit output for each test trial as it randomly fluctuates in direction. The other, called "Psi Invaders," was a modified adaptation of the popular 1980s video arcade game "Space

Invaders," in which players attempt to use a horizontally-sliding laser gun turret to shoot and destroy descending rows of invading enemy spaceships while they avoid being hit and destroyed themselves by the enemy's own array of bombarding laser fire (this gets increasingly more difficult as the game progresses, as the rows of invading spaceships gradually begin to move and descend at a faster pace). The PK aspect of the game was integrated into the operation of the laser gun affixed to the player's turret, which was said to be "old and frequently misfires." Whether or not the gun will fire when the player presses the gamepad button is randomly determined by the RNG output, and the player is asked to use PK in attempt to make it fire more often.

The third game, called "Psi Ball," was conceptually similar to Weiner's (1979) motor skill PC test, in that players moved a lever in attempt to keep a ball on the computer screen away from the "wall" at the edges of the screen. The RNG output determined whether the ball would become increasingly more sensitive to small movements of the lever (thereby increasing the difficulty of the game), and players would try to minimize this by using PK. These three games could be set to run in two different modes: "Feedback," in which an overt change in some aspect of the game's dynamics (e.g., its pace, its direction, or its screen color) could give players real-time visual feedback on the RNG's output (and thus, their PK performance); and "Silent," in which none of the game dynamics were overtly affected by the RNG output, and feedback was given to the player only after the game was over (or, in some cases, not given at all).

Experiments with these three PK games were conducted over the course of the 1980s by the PRL staff (Berger, 1988a, 1988b; Honorton et al., 1983; Schechter, Barker, & Varvoglis, 1983, 1984; Schechter, Honorton, Barker, & Varvoglis, 1984; Varvoglis, 1989) and by other researchers (Gissurarson, 1986; Palmer & Perlstrom, 1987) with varied test outcomes ranging from null (e.g., Schechter et al., 1983, 1984; Varvoglis, 1989) to suggestive (e.g., Honorton et al., 1983) to significant (e.g., Berger, 1988b; Gissurarson, 1986). One notable finding which seemed to emerge from several of them was a tendency for some participants to produce positive PK scoring while the games were running in "Silent" mode (Berger, 1988b; Berger et al., 1986; Palmer & Perlstrom, 1987; Varvoglis, 1989), offering initial suggestion that perhaps some participants may be able to score well in the absence of real-time feedback (although in some cases, the “Silent” RNG data were viewed only by the experimenters, and not the participants, which may lend some consideration toward an experimenter psi effect, an alternate possibility that will be raised in a later section).

Another type of PK game test, utilized by Broughton and Perlstrom (1985, 1986, 1992), was a modified version of a commercially available computer-based dice game in which players took turns rolling a pair of electronic dice (randomly controlled by an RNG) on a screen and aiming for high scores while trying to avoid rolling doubles (which decreased the score). The test was first used in a short series of three experiments in which Perlstrom acted as the sole participant playing against the computer. The combined RNG data from all three experimental series resulted in a significant departure from chance ($Z = 4.841$, $p < .0001$), whereas data from a matching control series were consistent with chance (Broughton & Perlstrom, 1985).

The test was again used in another series of three experiments later conducted with volunteer participants who took part in a (sham) competitive test scenario, in which they were led to believe that they would be playing the game against an opponent on another computer connected via a modem link-up (when in actuality, there was no other human opponent and they were just playing against an imaginary opponent being simulated by the computer). Since many of the participants were college students recruited from Duke University, they were further told that their opponent was another student from local collegiate sports rival University of North Carolina, Chapel Hill, so as to further encourage a competitive school spirit in them. Although the combined RNG data from all three experiments were at chance, the overall results did seem to be consistently indicative of there being an inverse correlation (amounting to a combined $Z = 4.1$, $p = .00004$, two-tailed) between the level of anxiety reportedly felt by the participants and their subsequent scores on the competitive PK test (Broughton & Perlstrom, 1992, p. 302).

One other type of PK game test, utilized in experiments by Hansen (1990) and Roe et al. (2004), followed Weiner’s (1978) earlier PC-based test in simulating races with animals, this time using digitally animated horses and greyhounds, respectively, moving across a computer screen.

As in Weiner's test, RNG output was used to randomly determine the moments when a given race animal would take its next incremental step forward, and participants attempted to influence this via PK for their designated race animal. Hansen's (1990) experiment compared both a competitive condition (where participants separately attempted to influence their own individual horses) and a cooperative condition (where participants attempted to influence the same horse, only without being aware that they were doing so, making it subtly distinct from the mutual "co-op" experiments discussed previously), with the prediction that the latter condition would produce higher PK scores. The data resulted in the opposite outcome ($Z_{diff} = -2.054$), however, for reasons that were unclear. In addition to comparing it against an ESP condition, the experiment by Roe et al. (2004) examined whether participants' performance in a PK test condition was affected by either a state of high arousal or a state of passive calmness. Overall results in the PK task were at chance.

Thus, while it seems so far that RNG-based games have generally not led to improved PK performance, and that competitive conditions have been mixed in outcome, the number of experiments again remains relatively small and certain preliminary correlations have emerged from these types of studies that may be worth further exploration using PK tests that can be integrated into the more widely accessible and advanced computer and video game console technology that is presently available.

Psychic Signatures. Although human bodies all tend to be biologically structured in a similar way on the large scale, there is one bodily structure existing on the fine scale which happens to be unique enough across individuals that it can often be used as subtle personal identifier (or "signature"): the whorls of the human fingerprint. Could there be any kind of unique "signature" to a person's psi performance that might allow it to be individually attributable to that particular person?

Some preliminary consideration of this question had arisen in relation to certain scoring patterns that were unexpectedly seen in the data collected over the course of a test series in some of the early PK studies with dice (McConnell, 1989). Appearing from time to time in the presence of a certain experimenter, these patterns seemed similar enough in their general structure across the tests that they raised the issue of a possible experimenter psi effect (to be discussed later).

Other suggestive scoring patterns that seemed to be associated with certain test participants also were observed in the PEAR benchmark data (Babu, 1987; Nelson et al., 1986, pp. 266–268). Of this, it was noted that:

> Some operators [i.e., participants] achieve PK results in only one direction, some in neither, some in both, and some show inverted results. The [HI] and [LO] achievement patterns for a given operator are typically asymmetrical, and are often found to be dependent on the conditions under which the operator is performing the experiment, such as the pulse [i.e., bit] counting rate, whether each trial in the [test] run is initiated manually or automatically, or whether the operator chooses or is randomly assigned the direction of effort. (Nelson et al., 1986, p. 266)

Focused exploration of the question was initially undertaken in a pilot experiment by Berger (1988a), in which eight participants each completed two test series on Psychophysical Research Laboratories' "Volition" PK game. The resulting RNG data produced by each participant in their first series were then statistically compared with the data from their second series in order to see if the two sets of data might be notably similar in any way with regard to their scoring patterns (as would be inferred from the presence of a significantly positive correlation between them). Two of the eight comparisons resulted in a significant correlation, a suggestive outcome (binomial $p = .057$). As a further test, the data from each participant's first series were blindly compared against a randomized set of data from 21 other test series: One of the series contained in the set was the participant's actual RNG data from their second series, while the other 20 series were simulated RNG data produced by the computer to act as "decoys." A Pearson correlation coefficient was calculated for each statistical comparison, and the resulting 21 correlation values were then ranked from highest to lowest, with a 1 in 21 (or .048) probability of the participant's actual RNG data being ranked the highest. Overall, the participant's data was found to have been ranked highest in three out of the eight cases, resulting in a significant binomial probability of .005.

Further exploration was made in two studies conducted by Radin (1989, 1993a). In the first study, an artificial neural network was used to see if it could possibly recognize and identify individual participants on the basis of any subtle but unique scoring patterns that might be present in their PK test data. To do this, data collected from 32 participants who had taken part in the PEAR benchmark RNG program were divided into two halves: The first half (consisting of RNG data individually produced by each of the PEAR participants in a PK test) was used to "train" the network by running the data through the network hundreds or thousands of times, first in a feed-forward fashion, followed by a back propagation of the data (i.e., running them back through the network in reverse); this method of computation allowed the network to initially "learn" to associate any patterns that might be present within the individual data with a particular participant based on the multiple unique interconnectionist (or "linked") information processing pathways that it forms between its nodes (roughly resembling the cognitive processing pathways formed between neurons in the brain) and then gradually refines with each forward and back pass of the data.

Radin then used the second half (consisting of RNG data individually produced by the same PEAR participants in a separate PK test) to test the network and see if it could apply the knowledge it had "learned" about the patterns to new sets of data. Compared to when two types of control data—random (consisting of simulated RNG test data randomly generated by a pseudo-RNG) and scrambled (consisting of the actual RNG test data being randomly matched to different participants)—were used in the task, the number of times that the network was able to correctly identify participants was significantly higher ($p < .05$). Significant results were also achieved in the second study using data from additional PEAR participants.

Though limited in number, these results would seem to offer preliminary suggestion that the idea of there possibly being a "psychic signature" which may be unique to some participants is worth studying further.

Emotional Expression. Some people who claim to have spontaneously experienced PK in their lives have said that their experience seemed to occur at a moment when they felt emotionally stirred; in one such instance, a woman said she felt ". . . a surge of blood running through [her] veins" (L. E. Rhine, 1963, p. 99) just before a book seemed to mysteriously fall from a nearby bookshelf. Just before one PK experience occurred, another individual stated that she ". . . was in a highly emotional state; I'd been in a disagreement with a colleague and didn't feel I'd got my point across. So [I] . . . was very annoyed and frustrated." And before another experience, the same individual said that she felt "sadness, extreme sadness" (Heath, 2000, p. 60). Physical disturbances seemingly occurring in conjunction with periods of emotional turmoil have sometimes been reported in ostensible poltergeist cases, as well (e.g., Auerbach, 1996, Ch. 8; Carrington & Fodor, 1951, p. 19; Huesmann & Schriever, 1989; Rogo, 1986; Roll, 1977, 1983). Could this hint at a possible correlation between PK and emotion?

Four experiments were designed by Lumsden-Cook (2005a, 2005b) to further explore this possibility. In the first two, RNG data were collected while artificial mood induction techniques were used in attempt to elicit emotional states in two groups of participants: The first group comprised animal welfare activists, who were asked to recall a particularly infuriating moment in their life and share their reactions to reported incidents of animal cruelty (as a means of inducing feelings of anger). The second group comprised participants who attempted to elicit different mood states (e.g., anger, elation) through a combination of self-referential stimuli (statements worded to give them a sense of feeling a particular mood in themselves), recalling personal moments when they experienced a particular mood, and acting out certain emotional expressions. Although the results with both groups were mixed and not overly significant, some suggestive hints were found of non-random shifts in the RNG output that appeared to be correlated with subjective shifts between emotional states, particularly the release and dissipation of anger.

This suggestive correlation was explored further in the third and fourth experiments, using personal recall and voluntary mood induction combined with exposure to audio-visual stimuli (music, film clips) to elicit emotional states (e.g., sadness, happiness, anger) in two other groups comprising volunteer participants. Although the results with these third and fourth groups were nonsignificant overall, significant deviations from expected randomness ($p < .03$) were again observed in the RNG output during moments when participants tried to release their emotion and shift their mood back toward a calmer, more neutral state.

Three other studies, conducted in a field RNG context, also have obtained results seemingly in line with a possible correlation between PK and emotion: In the first, Blasband (2000) set up an RNG to continuously run in the background while he was in session with clients of his private psychotherapy practice. Several of the clients exhibited strong outbursts of anger and anxiety during the sessions, and these were found to correlate with significant non-random deviations in the RNG output ($z = 3.55$ for anger, and $z = -4.47$ for anxious crying; $p < .01$).

In the second study, Nelson (2008) examined a certain subset of events contained within the formal experimental database of the Global Consciousness Project and found that events which could be rated as having a relatively high degree of emotional impact had a tendency to be correlated with significantly stronger deviations from randomness in the GCP's global-spanning RNG network than events which could be rated as having a relatively medium or low emotional impact ($p \leq .004$).

In the most recent study, Shimizu and Ishikawa (2012) collected field RNG data while participants viewed three short films, each of which were intended to elicit a specific type of emotion (surprise, sadness, or laughter). When combined, the data from all three films revealed a significant deviation from expected randomness ($z = 2.08$, $p = .038$, two-tailed).

Although their results do seem to suggest a possible PK–emotion correlation, it is also important to note that the number of RNG samples collected in most of these studies (e.g., Blasband, 2000; Lumsden-Cook, 2005a, 2005b; Shimizu & Ishikawa, 2012) was relatively small, leaving open the possibility that the observed deviations in their output could still be due to inflated chance fluctuations. Additional replications will thus be necessary in order to determine whether similar findings can be obtained with larger samples.

Volitional Strategies. When participants attempt to mentally influence the output of an RNG, by what means do they try to do it? Are there any particular types of strategies that participants tend to use in their efforts to get the RNG output to conform with their willful intent, and which of these strategies (if any) tend to be successful?

One of the initial efforts to explore this was informally made early on in the PEAR benchmark program, and it was generally observed that

> . . . individual strategies varied widely. Most simply attended to the [PK] task in a quiet, straightforward manner. A few used meditation or visualization techniques or attempted to identify with the device or process in some transpersonal style; others employed more assertive or competitive strategies. Some concentrated intently on the process; others were more passive, maintaining only diffuse attention to the machine while diverting their immediate focus to some other activity, such as glancing through a magazine, listening to music, or even eating lunch. One of the more intriguing comments in the experimental logbook was from an operator [i.e., participant] who reported that "Vanilla yogurt really works." Again, little overall pattern of correlation of such strategies with achievement was found. Rather, the effectiveness of any particular operational style appeared to be operator-specific and transitory; what seemed to help one operator did not appeal to another, and what seemed to help a given operator on one occasion might fail on the next. If there was any commonality apparent in this diversity of correlations, it was that the most effective operators tended to speak of devices in frankly anthropomorphic terms, and to associate successful performances with the establishment of some form of bond or resonance with the device, akin to that one might feel for one's car, tools, musical instruments, or sports equipment. (Jahn & Dunne, 2011, pp. 70–71)

Exploration of the PK volitional strategies employed by participants has also been made in a few qualitative studies (Gissurarson, 1997; Heath, 2000), which have identified a wide range that includes:

— focused awareness or intent
— passive volition or "effortless effort" (mentioned further in the next subsection)
— relaxation
— entering a mildly altered state of consciousness (e.g., meditative states; a feeling of dissociation from one's sense of self identity; letting one's attention become entirely absorbed in the task)
— verbally coaxing the RNG or one's self to produce the desired effect
— mentally visualizing images that reflect one's own concept of the

PK process, or that would be helpful toward achieving the goal (discussed further in a following subsection)
— having a playful attitude
— eliciting emotional states
— concentrating or staring hard at the PK target
— developing a sense of resonance with the RNG and/or the test computer
— temporarily suspending all disbelief and analytical thinking
— inviting or channeling a form of transcendent "energy"
— frequently releasing (or "letting go" of) one's effort or attention (discussed in the next subsection)

As with the observation initially made in the PEAR program, the strategy that seemed to be most commonly mentioned in these studies was developing a sense of "resonance." An experimental effort to explore this was made by Houtkooper (2004), who asked participants taking part in the Giessen portion of the Mind/Machine Interaction Consortium replication to keep a record of the subjective strategies they used in their attempts to influence the RNG. In line with the test prediction, participants' use of the resonance strategy was found to be significantly associated with (positive) PK scoring ($t_{577} = 1.889$, $p = .03$), although a closer examination of the data later revealed some suggestive hints of an underlying experimenter effect, in that the

> . . . strong interaction found between the experimenter and the performance with different volitional strategies means that the optimum strategy depends on the person[ality] of the experimenter and, supposedly, the messages the experimenter is conveying, consciously or subconsciously, about the volitional strategies and possible [*sic*] the whole setting of the experiment (Houtkooper, 2006a, p. 82).

Passivity and Release of Effort. Some studies have also explored the passive volition or "effortless effort" strategy (Braud, 1978; Debes & Morris, 1982; Honorton & Barksdale, 1972; Roe et al., 2004), in which participants remain relatively relaxed and do not overtly seem to strive much toward intentionally influencing the RNG output. The results of these studies have generally been mixed in terms of significance and direction (some have produced positive scoring, while others have produced negative scoring) but the number of them remains very small, indicating the need for more clarifying data.

A somewhat related strategy is release of effort, in which PK effects seem to manifest in the moments after the participants have stopped focusing or exerting their mental intention upon the PK target (and may have also turned their attention away from it). This strategy was explored by Palmer and Kramer (1984, 1987) in two experiments that were designed to have three PK test periods: In the first study, participants concentrated on the RNG and attempted to exert their influence upon it during the first and the third periods, while they were asked to stop concentrating and rest during the second period. The release of effort effect was predicted to occur during the beginning of the latter period, and in line with this, the associated test data were found to be significant ($z = 2.30$, $p < .05$). The result did not replicate in the second study, however, being nonsignificant and in the direction opposite to prediction ($z = -0.30$). One potentially relevant difference in this second study is that the PK influence instructions given to the participants could be seen as having conveyed a less effortful suggestion, in that they instead ". . . were instructed to try and merge their consciousness with the computer while focusing on the target number" (Palmer & Kramer, 1987, p. 129). Some hints of an underlying experimenter effect were also indicated in the data for both studies as well, adding another possible confounding factor. However, the results of another (non-binary RNG-related) PK experiment (Stanford & Fox, 1975), in which participants attempted to influence the electrical resistance of a photocell tightly enclosed in a box to shield it from external light, did seem to be in line with a release of effort effect, suggesting that further empirical testing for such an effect should still be pursued in the future.

Visualization Practice. In some spontaneous experiences of ESP, the psychic impressions received by the experient have been reported to subjectively manifest in the form of mental images (Irwin, 1994, Sect 2.2). One might figure that mental imagery could also possibly be involved to some degree in the subjective experience of intention-based PK, since, as noted by Gissurarson (1992a), ". . . it would seem the assumption behind the importance of imagery for PK is that imagery and visualization play a large role in wishing and willing" (p. 330).

To explore that possibility, some studies have explored whether the practice of mentally visualizing volition-related images might be an effective strategy for producing PK. Such images may be of two forms: goal-oriented (images which directly or symbolically reflect the intended goal or desired outcome that is being aimed for), or process-oriented (images which symbolically reflect a hypothetical process or mechanism by which one could personally conceptualize and envision how PK works, such as by imagining it to be some kind of mental "force" which "pushes" the target in the intended or desired direction).

In reviewing the various experimental studies of this type up to the early 1990s, Gissurarson (1992a) generally found that:

> Of the 11 studies reviewed . . . eight can be viewed as having explored the role of some form of goal-oriented imagery in the production of PK . . . seven of the eight studies yielded PK scores in the expected direction for goal-oriented imagery, and the scores were significantly above chance in three studies in which immediate feedback of performance was provided. . . . Hence, it appears that goal-oriented imagery may be important in the generation of PK. (Gissurarson, 1992a, p. 330)

He did note, however, that at least two of the studies did not produce results to indicate that prolonged visualization practice was correlated with an increase in PK scoring.

Has there been any additional evidence gathered since that time to suggest that imagery tends to be associated with PK? At least three experiments have been conducted to further explore this possibility:

Two experiments, conducted by Taylor (1996), were conducted in a practical sporting context, in which two groups of athletes (gymnasts and bore shooters) were trained to engage in a self-paced guided visualization practice that was intended to help improve personal performance in their respective sport. The athletes in each group were divided into two subgroups, experimental and control, with the experimental subgroup undergoing an imagery training period that was twice as long as that of the control subgroup. To see if their imagery practice might be associated with PK performance, each group participated in a computerized PK test that was custom-designed to represent an animated simulation of their respective sporting activity (pommel horse vaulting for the gymnasts, and target shooting for the bore shooters), with the actions and outcomes being determined by the output of a (pseudo-random) RNG. Although they did not attain statistical significance, the results of both experiments were in line with the PK-imagery correlation, in that the experimental subgroups produced higher PK scores than control subgroups, with one of the experiments (involving the gymnasts) being suggestive ($t_{20} = 1.621$, $p = .06$). In addition, self-rated changes in imagery ability over the course of the training period were found to be slightly correlated with changes in PK scoring. But as Taylor (1996) importantly noted, the statistical power of this experiment was quite low (owing to the focus of its participant sample being only upon athletes who were committed to the practice of their particular sport), and other possible factors apart from imagery ability such as an experimenter effect and motivational factors might alternately account for the results.

Two experiments conducted by Collesso et al. (2021) were aimed at exploring whether participants' practice of two brief exercises (meditation and guided visualization) might correlate with enhancement of their PK performance. In the second of the two experiments, the participants were asked (prior to engaging in an RNG-PK test modeled after the one used in the PEAR benchmark program) to practice a three- to five-minute guided visualization exercise that was specifically intended to ". . . stimulate imagination and further induce relaxation as well as positive emotions, including promoting feelings of love" (p. 321). Analysis subsequently revealed a significant deviation from mean expectation in the RNG output ($t_{29} = 2.66$, $p = .01$, two-tailed). Moreover, this result was found to be significantly different ($t_{58} = -2.69$, $p = .009$, two-tailed) from the participants' performance in the first experiment (which was designed to act as a control condition, involving the same RNG-PK test with none of the exercises being practiced beforehand). However, due to its being paired with another exercise (meditation), it is unclear how much of this observed effect might have been due specifically to the guided visualization.

Thus, while the results of these two additional experiments do initially seem to be in line with the possible visual imagery-PK correlation, they do not offer unambiguous support for it, and additional clarifying data are still needed at present.

Meditation. As pointed out by Gissurarson (1992a):

> The term "meditation" has been used as a broad label covering different breathing techniques, mental relaxation, and concentration exercises for quieting the flow of thoughts by contemplating a given sound, word, image, thought, or even nothing at all. (Gissurarson, 1992a, p. 310)

A number of experiments conducted since the 1970s have produced results suggesting that meditative practice may be correlated with successful ESP performance (for reviews, see e.g., Honorton, 1977, pp. 437–442; S. Schmidt, 2008), and a smaller amount seemed to offer preliminary evidence suggesting the same for micro-PK (see, e.g., Braud, 1990; Gissurarson, 1992a, pp. 306–311; S. Schmidt, 2008). Does the latter continue to hold for some of the latest RNG-based experiments?

Schmidt and Dalton (1999) conducted one of the most recent studies touching upon this topic, in which they performed three retro-PK experiments involving three separate participant groups who were asked to try and influence pre-recorded sets of RNG data. One of the groups comprised individuals who practiced various forms of meditation. Each participant was asked to listen attentively to auditory tones being played at randomly determined times (derived from the RNG data), with the aim of trying to shorten the time interval between tone soundings. Overall scoring was slightly positive but nonsignificant ($z = 0.66$).

In one of the five experiments reported by Nelson and Schwartz (2006), RNG data were collected over a period of approximately one minute while Nelson engaged in various meditative techniques. A significant positive correlation was found between Nelson's subjective assessment of the depth of his meditative state (coupled with a sense of time distortion) and measures of the absolute deviation from randomness in the RNG output ($r = .458$, $p < .0001$).

Thalbourne (2006) initially conducted five experiments designed to explore possible deviations in RNG output occurring in conjunction with the practice of eliciting *Kundalini* (the "Eastern name given to a particular syndrome of psychophysiological changes taking place in the body, often as a result of long practice of meditation or yoga"; Thalbourne, 2008, p. 155). Acting as his own participant, Thalbourne attempted to elicit sensations of *Kundalini* "energy" through various techniques including mental absorption in music, mental attendance to a theological poem through repeated recitation, and focus on somatosensory "noise" (said to be sometimes associated with *Kundalini*). With the exception of one experiment where the results of both test runs had negative scores, there was a suggestive tendency for *Kundalini*-sensation runs to produce positive scores and non-*Kundalini* runs to produce negative scores. Thalbourne (2008) later attempted to replicate these findings in two additional experiments, but the results were opposite to prediction, with non-*Kundalini* runs resulting in positive scores, and *Kundalini* runs being near chance. The possibility that some of these results may have been due to a visual feedback artifact (associated with the display lights on the face of the RNG device) has been examined and found to be unlikely (Thalbourne & Storm, 2015).

In one of their two experiments, Collesso et al. (2021) asked participants to engage in two brief exercises, one of which was a two- to three-minute meditation exercise intended to induce bodily relaxation and the release of tension, before attempting to influence the output of an RNG. Results indicated a significant deviation from mean expectation ($t_{29} = 2.66$, $p = .01$, two-tailed), and when compared with the results of the initial (control) experiment where these exercises were not used, a significance difference was observed ($t_{58} = -2.69$, $p = .009$, two-tailed). As mentioned, however, due to its being paired with another exercise (guided visualization), it is unclear how much of the observed effect might have been due specifically to the meditation.

Other recent experiments examining the possible effects of meditation have been conducted in a field RNG context: Radin et al. (1996) had set up an RNG to continuously run in the background over the course of an all-day workshop devoted to the practice of Holotropic Breathwork, an approach which is intended to induce altered states of consciousness using a combination of spiritual insights and techniques (including the breathing techniques of *pranayama* meditation). The resulting data exhibited a significant deviation from expected randomness ($z = 2.96$, $p = .002$), whereas control data collected after the workshop was over were consistent with chance ($z = -0.979$).

Mason et al. (2007) set up two field RNGs to run continuously during a series of Transcendental Meditation sessions held over the course of a 96-hour period at the Maharishi University of Management (MUM) in Iowa. In addition, 149 hours of data were separately collected from the RNGs before and after the series, when they were left to run continuously in a university office as a control setting. Data from each of the two experiments conducted during the MUM series were found to exhibit strong deviations from randomness in the negative direction ($Z = -4.726$, $p = .000001$; and $Z = -3.872$, $p = .00005$), while control data taken before the series were consistent with chance. In addition, an unexpected result was found with the control data taken after the MUM series, in that these data also exhibited a very strong and persistently negative deviation ($Z = -7.28$, $p = 1.70 \times 10^{-13}$). Several possible ways to account for the latter were considered, including the possibility that it might represent a kind of "lag" or "carry over" PK effect based on the consideration that ". . . previous studies with this type of meditation had reported a carryover or lag effect on the experimental measurements even after the experimental period of meditation had ended" (p. 307). However, it is also important to note that such isolated instances of strong and persistent deviations in a certain direction are also sometimes observed in RNGs which are malfunctioning, leading them to produce a consistent artificial bias in their output. Although no clear evidence for malfunctioning was found, it would seem that this possibility cannot be ruled out with complete certainty, thus leaving a potential confound in the interpretation of this finding. However, it does provide an instructive example of what researchers should be watchful for, and be cautious of, when trying to interpret extremely deviant results.

Similar in aim to the studies by Thalbourne (2006, 2008), Ivtzan (2008) explored a possible micro-PK correlation with *Kundalini* practice by collecting data from two field RNGs during a series of 60-minute sessions held over the course of five weeks with ten practitioners of a form of meditation intended to elicit *Kundalini*. One RNG was located in the meditation room, while the other was located 2 kilometers (1.24 miles) away. Half of the practitioners were made aware of the presence of the first RNG in the room during the sessions, while the other half was kept unaware of its presence (by hiding the RNG in a bag). RNG data collected in the presence of the "aware" group exhibited a significant positive deviation ($z = 2.48$, $p = .006$), while data collected in the presence of the "unaware" group were nonsignificant. Data from the second (distant) RNG were found to be nonsignificant, as well.

In line with previous findings (Braud, 1990; Gissurarson, 1992a, pp. 306–311; S. Schmidt, 2008), the majority of the latest experiments continued to give results suggestive of a correlation between meditation and deviations from randomness in RNG output.

Healing Intention. Apart from physical targets like RNGs, a number of PK experiments have also been focused on the question of whether a participant can mentally influence small-scale biological targets such as cell cultures, electrodermal activity, shifts in human attention, plant growth, and animal locomotion (Braud, 2003; Delanoy, 2001). Meta-analyses of these experiments do tend to show a small but significant overall effect (Roe et al., 2015; Schlitz & Braud, 1997; S. Schmidt, 2012; Schmidt et al., 2004), which may provide a possible basis for conceptualizing ostensible psychic healing. One question which arises in the latter context is: Do these biological PK and ostensible healing effects involve a similar (if not the same) kind of ostensible influence process as PK effects upon physical targets? One reason for considering this possibility from a conceptual viewpoint comes from observations made of some of the subjective factors that seem relevant to the healing experience, a few of which seem suggestive of willful influence in that a healer consciously or subconsciously "must affirm the idea or intention to help people" and "direct one's attention in a positive direction" (Levin, 2011, p. 22). Another comes from an observation made by Nelson (1999):

> Living bodies, with homeostatic, immune, and nervous systems that epitomize the realm of applied nonlinear dynamics, are intrinsically susceptible to influence from small inputs and are able to identify and amplify the most subtle of inchoate patterns and information. Biological systems utilize random processes and uncertainty to maintain the highest level of sensitivity to subtle changes in the environment. They are reactive on the finest scale to information that reduces entropic disorder and provides an increment of structure and predictability, yielding a stable internal milieu and

> successful interaction with the environment. In this context, we see that healing a wound or recovering from an illness is dependent upon the generation or addition of appropriate information to help restore order and structure. (Nelson, 1999, pp. 20–21)

A relatively small number of studies, mostly utilizing a field RNG approach, have taken preliminary steps toward examining this question empirically (Nelson & Radin, 2003).

Schwartz et al. (1997) collected RNG data over the course of a large three-day meeting devoted to the traditional Chinese practice of Qigong and the study of its possible beneficial effects for human health, based partly on the claim that "Qi accumulates over the entire meeting" (p. 57). The resulting meeting data were found to exhibit a significant increase in the mean number of trials that were above chance expectation ($t_4 = 3.56$, $p < .023$), whereas such increases were not found in control data collected over an equal number of days before and after the meeting.

Crawford et al. (2003) conducted two experiments in which they statistically compared two sets of RNG data that were collected in two different settings over the course of 61 days: The experimental setting was the treatment room of a healer's office where non-contact bioenergy treatments were being given to patients on a regular basis, and the control setting was a local university library. The combined results indicated that there were significantly more instances over the course of the 61 days in which the experimental RNG data exhibited notable deviations from expected randomness (at or exceeding the $p < .05$ level) as compared to the control data ($X^2 = 16.3$, 1 df, $p < .0004$).

Radin et al. (2004) conducted a three-day experiment in which they continuously collected data from three separate types of RNGs before, during, and after a small group of healers attempted to affect the growth of cell culture targets and "condition" the space of the treatment room using the traditional Japanese spiritual healing practice of *Johrei*. Upon combining the data together, a highly significant deviation from expected randomness was observed across all three RNGs on the morning of the third day of the experiment ($Z = 4.80$, $p = .00009$).

Two more RNG experiments were independently conducted with *Johrei* practitioners by members of the PEAR staff (Jahn et al., 2006).

The first was a "Yantra" experiment in which the audio (rhythmic drumbeats) and visual (a mandala design that dynamically changes colors on a computer screen) aspects of the test environment are coupled to the output of an RNG (Dobyns et al., 2007). It differed from the standard "benchmark" PK test in that no real-time feedback was given to the participant on their performance during the test, and it consisted of only the "HI" and "LO" conditions (i.e., the "BL" condition was omitted). The three practitioners took part in the experiment both while engaging in *Johrei* practices and also while not; subsequent comparison of these two conditions indicated notable differences in their outcomes: Whereas the "HI–LO" difference from the non-*Johrei* condition was found to be at chance ($Z_{diff} = 0.387$, $p = .70$), the "HI–LO" difference associated with the *Johrei* condition was significantly in the direction opposite to intention ($Z_{diff} = -2.756$, $p = .006$).

Lumsden-Cook et al. (2006a) conducted an exploratory study in which South African native Zulu healers were asked to try and influence the output of an RNG using the same ritual healing practices that they perform on human patients. A significant positive shift away from mean expectation was observed across the combined data from all four healers ($Z = 2.531$, $p = .011$, two-tailed). This outcome prompted a second, follow-up study (Lumsden et al., 2006b) with a larger test sample of 20 healers who were again individually asked to try and influence an RNG in a healing context. This again resulted in an overall significant outcome ($X^2 = 113.02$, 80 *df*, $p = .009$) that differed from the first in that it was non-directional (i.e., both extreme positive and negative deviations from expectation among the test trials were collectively taken into account, regardless of their polarity or sign).

In a recent preliminary experiment, Moga (2015) collected RNG data during a series of 18 sessions conducted with five Healing Touch practitioners. Data from seven of these sessions were found to exhibit deviations from randomness exceeding the $p = .05$ level, and these occurred in sessions with three of the five practitioners.

While the findings of several of these studies do seem to indicate possible influence effects upon the RNGs, the number of them remains relatively small at this point. In addition, some of them (Lumsden-Cook et al., 2006a, 2006b; Moga, 2015) collected a relatively small amount of RNG test samples (e.g., the duration of the test periods employed by

Lumsden-Cook et al. were only about five minutes in length) and/or seemed to base their conclusions on the outcome of only a relatively small number of sessions. Considering the relatively small effect sizes associated with micro-PK, it can be difficult (with such small samples) to reliably establish the presence of a micro-PK effect upon the RNG output that is clearly distinguishable from expected chance fluctuations. Thus, additional data will be necessary in order to further verify the significant deviations observed in these studies.

Attitudes Toward and Personal Experiences of PK. One finding that has appeared fairly consistently across a number of experiments on ESP is that people who hold some degree of personal belief in this type of phenomenon also tend to score well in tests of it (Lawrence, 1993; Schmeidler & McConnell, 1958; Storm & Tressoldi, 2017). Is there a similar kind of correlation between belief and PK performance?

As part of their effort to explore this question (among others), Gissurarson and Morris (1991) conducted five experiments in which participants' responses on several personality questionnaire items were compared with their subsequent scores on a PK test. One of the items inquired about their level of belief in PK, and although one of the experiments did result in a significant positive correlation (Spearman's *rho* = 0.46, $z = 2.90$, $p < .005$, two-tailed), it was not persistently observed across the other four. A broader meta-analysis by Gissurarson (1990–1991) of 450 experimental test sessions conducted between 1946 and 1991 further indicated only a very modest and suggestive correlation at best (Spearman's *rho* = 0.19, $z = 1.38$, $p = .08$, two-tailed). Thus, the answer remains somewhat unclear and further clarifying data are needed.

One particularly notable finding to emerge from these two studies was an indication that participants who reported previously having a spontaneous PK experience had also exhibited a tendency to score well on the PK test. Combined results across the five experiments by Gissurarson and Morris (1991) revealed a significant positive correlation (Spearman's *rho* = 0.27, $z = 3.03$, $p = .001$) that remained fairly consistent in the meta-analysis by Gissurarson (1990–1991) that followed (Spearman's *rho* = 0.22, $z = 3.15$, $p = .0008$).

Though the amount of data is currently limited, these findings would seem to offer some initial hints for further examining the possible relevance of attitudes and personal experiences to PK performance.

Neuropsychological Factors

Cerebral Lateralization. Although there are some caveats, it has long been thought that the two hemispheres of the human brain generally tend to be specialized for certain cognitive capacities and modes of thinking: The left hemisphere has often been seen as the "seat of language" and as mostly being characterized by sequential, analytical, and rational thought; while the right hemisphere has been seen as the main area involved in controlling visuospatial skills and as mostly being characterized by non-verbal, non-analytical, and intuitive thought (Kolb & Whishaw, 2006, pp. 542–550; Springer & Deutsch, 1998). One might wonder: If psi is a genuine human capacity, then might it be associated with a particular hemisphere? While the current empirical evidence is not overly clear on this matter, there would seem to be some modest indication that ESP may be associated more with the right hemisphere (C. H. Alexander, 2002; Broughton, 1983; Williams, 2015, Ch. 8). What about for PK?

Initial hints of a similar association would seem to come from subjective accounts given by individuals who have reportedly experienced PK; for instance, one individual stated:

> I think the common thread . . . is that the cognitive left brain analyzing part of the mind is out of the picture for a while, either through my getting very emotional, or deliberately occupying that part of my mind with something else. So I think getting that cognitive, verbal part that said, "oh, you can't do it," just disabling that, or putting it out of the way for a while seems to help. (Heath, 2000, p. 59)

The relatively few attempts that have been made so far to address the matter using an empirical approach would seem to offer further hints, as well: Prior to engaging in a RNG-PK test, Andrew (1975) had two groups of participants listen to a brief relaxation audio tape, followed by a 23-minute long tape which emphasized a particular mode of thought: One group heard a "left thinking" tape that presented material oriented toward a verbal, analytical, and rational mode (such as vocabulary and grammatical lessons, mathematical and logical problems, and readings in philosophy, law, and physics), while the other group heard a "right thinking" tape containing material oriented toward a spatial and non-analytical mode (such as music and sounds which were meant to subjectively evoke imagery and convey a sense of depth). Results of the subsequent PK test indicated that the "right thinking" group tended to score positively (binomial $p = .02$), while the "left thinking" group tended to score negatively ($p = .011$), leading to an overall significant difference between them (Mann-Whitney $U = 8.5$, $p < .002$, two-tailed). Two follow-up experiments by Braud et al. (1976) produced somewhat similar results, and when the data from all three experiments were combined, there was a tendency for the "right thinking" group to produce significantly positive scores (binomial $p = .025$), and the "left thinking" group to either produce negative scores or null results, with a significant difference again being indicated ($p < .01$).

To basically assess the degrees to which they may have engaged in "left thinking" versus "right thinking," Krieger (1977) asked each of her participants to complete the "similarities" test from the Wechsler Adult Intelligence Scale (a test which assesses logical, abstract thinking by asking participants to analyze the relational features between two objects; Cohen et al., 1996, p. 306), as well as the Street visual Gestalt test (which assesses visuospatial thinking by asking participants to mentally recombine scattered visual elements together in order to perceive a whole object). A ratio was then calculated from the scores of these two tests to provide a numerical indicator of the relative engagement level of these two modes of thought. The participants were then tested for PK under two conditions: In the intention condition, they were presented with a target lamp that flashed on or off based on the output of an RNG, and they attempted to keep the light on for as long as possible. In the non-intention condition, the lamp was placed in a separate room and the participant was left unaware of its presence there. Statistical comparison of the participants' ratios with the scores they achieved in the two PK test conditions revealed a fairly even degree of correlation between them (intention: $r = -0.44$, $p < .02$; non-intention: $r = -0.43$, $p < .02$). In particular, a significant negative correlation was found between their intention PK scores and their performance on the "right thinking" task ($r = -0.53$, $p < .01$), and a significant positive correlation was found between their non-intentional PK scores and their "left thinking" task performance ($r = 0.37$, $p < .05$), suggesting that intentional and non-intentional (or unconscious) PK were associated with the right and left hemispheres, respectively.

Certain studies using electroencephalographic (EEG) and neuroimaging technology have produced a few other findings relevant to the question of whether micro-PK might be associated with a particular brain hemisphere; these will be described in the subsections to follow.

EEG Activity. To explore whether PK performance might be correlated with a particular type of brain wave activity, some experiments have sought to actively monitor participants' EEGs while they are engaged in an RNG-PK test. Several of these were designed to have a neurofeedback-type approach, in which the RNG was coupled to an EEG monitor and would become activated whenever a specifically defined brain wave frequency or voltage threshold was reached.

Two experiments of this type by Schmidt and Terry (1977) involved monitoring brain wave activity from the participants' occipital lobes, and the RNG was activated only whenever the EEG monitor registered activity in the alpha frequency range (8–12 Hz) during one half of the PK test sessions, and in the beta frequency range (13–30 Hz) during the other half. Upon being activated, the RNG began generating binary output that was converted to a random sequence of high- and low-pitched tones, and these were presented to the participants with the intended PK goal of producing more low tones. An overall success rate of 50.7% ($z = 3.12$, $p < .001$) was achieved, with the scoring being distributed across both the alpha and beta sessions.

A series of experiments by Heseltine (1977) utilized a similar PK testing setup in which an RNG was coupled to an EEG device that monitored fluctuations in voltage across successive brain wave cycles. Each time the zero point (going from negative to positive voltage, or vice versa) was crossed in the course of a wave cycle, the device initiated a test trial by triggering a digital flip-flop in the custom-made RNG, momentarily halting it and thereby causing a binary bit outcome to be randomly determined for that trial. If the outcome matched the binary number selected as the target by the controlling computer, a success was registered for that trial. The computer continually monitored the trial outcomes over the course of a three-second period, and using a majority-vote process (based on how many successes were registered over that period), it determined whether auditory feedback (in the form of a high- or low-pitched tone) would be presented to the participant;

the more successes that were registered, the longer the tone would persist. Participants were asked to listen for the tone but to try and influence it (by extending its length for as long as possible) only in a casually passive manner while letting their mind wander freely.

Results of the first experimental series indicated that participants tended to be successful at the PK test when the low tone served as the feedback target (50.8% success rate, $z = 2.48$, $p = .014$), while test trials with the high tone were at chance. The low tone was thus kept as the target in the second series, which again resulted in significantly positive results (50.4% success rate, $z = 3.53$, $p = .0004$). Spectral analysis further indicated that scoring tended to be highest when the participant's EEG was primarily within the alpha frequency range ($z = 3.60$, $p \leq .0005$, two-tailed) and, to a lesser extent, in the beta range ($z = 2.10$, $p \leq .04$, two-tailed).

Three more experiments in this series by Heseltine and Mayer-Oakes (1978) explored possible brain hemisphere correlations by collecting EEG data over the left frontal and parietal regions of the participants' brains in Series 3 and 5 and comparing RNG-PK performance with the EEG data that were collected over the participants' right frontal and parietal regions in Series 2 and 4. The combined data across Series 2 and 4 resulted in significantly positive scoring (50.3% success rate, $Z = 3.04$, $p = .002$), whereas the combined data for Series 3 and 5 resulted in significant negative scoring (49.8% success rate, $Z = -2.29$, $p = .025$), suggesting that the right and left hemispheres were associated with positive and negative PK scoring, respectively. Spectral analysis indicated that the highest scoring was again associated with the alpha frequency range ($z = 2.31$, $p \leq .025$) for the right hemisphere, whereas no clear correlations with frequency range were found for the left hemisphere.

Though they are limited in number, these experimental findings would seem to suggest that micro-PK tends to be most often correlated with alpha wave activity, a finding paralleled in experiments on ESP (for reviews, see, e.g., C. H. Alexander, 2002; Krippner & Friedman, 2010; Williams, 2015). Further attempts at verification of this with a broader participant pool would be useful in better determining whether this may indeed be a robust finding and whether it might be generalizable to a wider population.

PK Correlates of Bodily Motor Volition. In pondering the processes that might underlie the initiation of bodily movement, Eccles (1977) suggested that perhaps PK might play a minor functional role in the interaction between mind and brain ". . . when you will a movement and you actually trigger the brain to carry out the movement; using thought to bring about action" (p. 256). His basic premise was that, in order to convey its intent to move and thereby get the brain side of the process started, the mind might subtly exert a willful influence upon the electrochemical functions of one or more neurons contained in the areas of the brain involved in motor control. A potential target that Eccles specifically suggested for this influence is the functioning of the synaptic bouton, a bulbous cellular structure found at the ends of the neuron's branching arms (called teleodendria) which contains bundles of neurotransmitters that the neuron releases into the narrow synaptic gap to establish communication (through chemical receptor signaling) with adjacent neighboring neurons. Electrical excitation pulses received via the teleodendria help facilitate activation of each bouton and the subsequent release of its neurotransmitters, with an estimated "all-or-nothing" occurrence probability of about 50%. To initiate the movement process, perhaps the mind might willfully influence this probability to a degree higher than 50% (Beck & Eccles, 1992; Eccles, 1986).

Might Eccles' PK-related suggestion be plausible in any way? In examining this from a theoretical standpoint, Helfrich (2007) pointed out that this probabilistic electrochemical functioning of neural synapses can be viewed as being analogous to the generation of binary bits from an RNG, and through statistical reasoning he argued that the small effect sizes commonly found in RNG-PK studies might actually be of the right size to affect this probabilistic functioning.

Certain findings also seem to offer some preliminary support from an empirical standpoint, as well: In two biofeedback-type experiments designed to test Eccles' (1977) hypothesis in a basic manner, Honorton and Tremmel (1979) asked participants to attempt to mentally influence their own brain wave activity such that it would begin exhibiting rhythms in the alpha frequency range. A tone would be presented to the participants as auditory feedback by an EEG monitor whenever it detected the presence of alpha rhythms, and the aim of the participants was to keep this tone on for as long as possible. Data from an RNG running hidden in the background were found to exhibit significant deviations from chance in both experiments (Expt. 1: $X^2 = 145.7$, 100 *df*, $p = .002$, two-tailed; Expt. 2: $X^2 = 159.2$, 120 *df*, $p = .005$) while the participants were successful at this EEG influencing task. In contrast, no significant deviations were observed when participants were unsuccessful or were simply resting and not focused on the task.

Although they differed from the aforementioned one in several respects, a series of conceptually similar multi-factorial experiments by Varvoglis and McCarthy (1986) had found significant deviations from chance in the output of an RNG not only when participants were focused on directly influencing the RNG and were receiving real-time feedback on their performance ($z = 2.46$, $p = .007$), but also when the RNG was hidden and participants were instead focused on trying to influence their brain waves to produce alpha rhythms and were receiving feedback on their performance from an EEG monitor ($z = 2.59$, $p = .005$). However, necessary adjustments made to the EEG feedback procedure to improve control conditions in these experiments had made it unlikely that this latter result could be fully attributed to the participants' brain wave influence efforts.

Giroldini (1991) made a separate attempt to test Eccles' hypothesis by asking participants to try to influence the output of a custom-designed random generator that was intended to digitally simulate the electrical pulse activity of a simple network of neurons. The experimental data were found to exhibit a small positive shift on the order of nearly 1% (0.764%) that was highly significant ($p < .00001$) and in line with the participants' intention, whereas the non-intention control data were close to zero (–0.06%) and nonsignificant overall. Compared to control data, EEG data collected from the participants' frontal lobes during the experimental period were found to have higher mean amplitudes in both the alpha and beta range ($p < .001$). Significantly increased brain wave activity in the alpha and beta bands also was found by Giroldini and Pederzoli (2021) in a conceptually similar PK experiment conducted three decades later using another custom-designed random generator, and this EEG activity was also found to extend into the gamma (30+ Hz) frequency range.

Frontal Lobe Correlates. In their independent effort to conceptually replicate the PEAR benchmark experiment, Freedman et al. (2003) conducted an experiment in which two groups of participants—one consisting of healthy staff members of a local hospital, and the other consisting of neurological patients—were asked to attempt influence upon the output of an RNG similar in design to the benchmark-type RNG, with visual feedback on their performance being given by the horizontal back-and-forth motion of an arrow on a computer screen (with the head of the arrow pointing in the target direction being aimed for). The test paralleled the standard test procedure utilized by PEAR in having three experimental conditions: "Right" (attempting to move the arrow toward the right of the screen; corresponding to the "HI" condition of the PEAR procedure), "Left" (attempting to move the arrow toward the left; corresponding to "LO") and "Baseline" (not attempting to influence the arrow at all). The resulting test data indicated that while most of the staff members and patients only produced outcomes consistent with chance, there was one patient (a male who had suffered damage to his left frontal lobe) who produced a significant result ($t_{5998} = -3.169$, $p = .0015$) in the "Right" influence condition. The result was replicated when the patient was tested again in a follow-up experiment ($t_{37998} = -2.53$, $p = .0115$).

The same directional influence was observed when the patient was tested again in a third experiment (Freedman et al., 2018), which also resulted in a second neurological patient (a female who suffered considerable tissue loss in her frontal lobes due to dementia) who was also moderately successful at the same PK test ($t_{35998} = -2.16$, $p = .03$) being identified. These findings led the researchers to ". . . suggest that the frontal lobes, and in particular the left . . . middle frontal region . . . may act as a biological filter to inhibit mind–matter interactions" (p. 83; see also Freedman, 2010).

Although their results are intriguing, the number of test trials collected with these successful patients is still relatively small (especially compared to the PEAR benchmark database), and additional replications with larger samples would be useful for further evaluation.

MINDING THE EXPERIMENTER PSI EFFECT

Arguably, the general review of micro-PK that has been provided here would not be complete without at least a brief consideration of the experimenter effect, as it is a potentially confounding factor which is difficult to avoid in experimental PK (as well as other psi) tests, and which may be one of the factors possibly contributing to the challenging situation of replicating findings across parapsychological experiments (as it may offer one way to possibly account for why many of the individual findings tend to be so mixed and inconsistent with others at times).

Although it is often assumed that the ultimate source of any successful performance observed in experimental psi tests is likely to be the participants (since they are the ones being purposely tasked with the effort of trying to mentally influence an RNG or correctly perceive a hidden target using ESP), the possibility has also been recognized that some successful outcomes might alternately be traced to the experimenter(s) running the tests. As White (1976) described, this would be a scenario in which the participant in a test of ESP

> . . . may respond to the wishes and needs of the experimenter over and beyond what he [or she] is told by the experimenter and under conditions which rule out of the possibility of sense-mediation. In such cases, the only way the [participant] could obtain the relevant information would be from the experimenter by means of telepathy (i.e., a psi-mediated experimenter effect), or by clairvoyance of the testing situation (i.e., a psi-mediated experimental effect). (White, 1976, p. 334)

In the case of a PK test, a psi-mediated experimenter effect scenario would entail the experimenter (consciously or unconsciously) using his or her own PK ability to successfully affect the RNG output, instead of the participant; while a psi-mediated experimental effect scenario would presumably entail a process akin to the one basically proposed in Decision Augmentation Theory (DAT; May et al., 1995a, 1995b), where instead of mentally influencing the RNG, the experimenter may use precognition to predict and decisively select out ideal periods during the test session when the RNG output is exhibiting brief, non-random fluctuations purely by chance, which then cumulatively amount over multiple sessions to a significant result (for possible counterarguments to this, see, e.g., Dobyns, 2000; Dobyns & Nelson, 1998). As White (1976) further stated, in such scenarios ". . . it is difficult to see how the experimental situation can be separated from the experimenter, for in a sense it can be viewed as a trap which the experimenter has devised with the intention of catching a particular finding which will fulfill his [or her] hopes and expectations" (p. 334).

While a good amount of anecdotal and empirical evidence for it has accumulated over the years (for reviews, see, e.g., Kennedy & Taddonio, 1976; Palmer, 1997; Palmer & Millar, 2015; Parker & Millar, 2014; White, 1976, 1977), the extent to which the experimenter psi effect may pose a serious confounding issue for the interpretation of experimental results continues to be a topic of debate. Apart from those briefly cited in previous subsections, one illustrative example relates to the debate surrounding the interpretation of the significant outcome for the formal 500-event database collected by the Global Consciousness Project (Nelson, 2015, 2019; Nelson & Bancel, 2011): Although one possible interpretation of this outcome might be that it reflects a micro-PK-related shift away from expected randomness occurring across the RNG network nodes in conjunction with the mass attention and emotion being focused on certain events and activities in the world, there are also two other alternate interpretations that have been proposed: Perhaps the experimenter(s) responsible for defining the time period of examination surrounding each event or activity are (unconsciously) using precognition to predict and select out which particular events and/or time periods will exhibit short-lived non-random fluctuations in the RNG network data purely by chance (Bancel, 2017a, 2017b; May & Spottiswoode, 2011a, 2011b), or (in the case where the examination period is defined after the event happens) the experimenter(s) are using a form of retro-PK to intentionally influence the RNG network data after they have been recorded and archived (H. Schmidt, 2009). A number of points have been raised to possibly counter these alternate interpretations (Bancel, 2011; Nelson, 2011, 2017, 2020; Nelson & Bancel, 2009), though the issue continues to remain open and unresolved.

At the very least, in light of the persistence of this issue, it is important for novice investigators and researchers to be mindful of the relevance of the experimenter effect to the procedural design, results, and interpretation of any experiments they may look to undertake in the future. Parker and Millar (2014) and Bierman and Jolij (2020) have recently offered some starting points for consideration in this regard.

CONCLUSION

From the review given here, it should be clear that, although a range of proof-oriented empirical evidence for microscopic psychokinetic effects upon RNG output has accumulated over the past eight decades, there is still very little that is known at present about the processes that may possibly underlie them. This might be partly attributable to the following:

— In relative contrast to that which has been done in relation to ESP, a considerably lesser amount of process-oriented research has been conducted in relation to PK. As a result, few attempts have been made to independently replicate and expand upon the preliminary process-oriented studies that have been conducted so far.

— Considering the very small effect sizes that tend to be associated with micro-PK effects on RNGs, it is possible that several of these studies did not have sufficient statistical power with which to detect such subtle effects and thereby perform adequate tests for correlation. Although it tends to be a prime focus for purely proof-oriented studies of psi (Tressoldi, 2012), this issue concerning statistical power also tends to be important for process-oriented studies as well, because in order to be able to study the possible correlates of psi, one must first have a reasonable degree of confidence that psi is present to begin with.

This present state of affairs suggests that there is still much opportunity for novice investigators and cross-disciplinary researchers to make valuable contributions in this particular domain of study, and it is hoped that the experimental findings reviewed here can serve as a starting guide in paving the way forward for any future research efforts they may make. It should also be pointed out that valuable insight can potentially come not only from further collection and analysis of quantitative data relating to the purely physical side of PK (e.g., RNG output and numeric-scale instrumental readings), but also from collecting and analyzing more qualitative data relating to its subjective side as well, for although some preliminary effort has been made over the years (e.g., Gissurarson, 1997; Heath, 2000), Irwin (1994) has previously observed that: “The phenomenology of . . . PK experiences clearly has not been adequately documented” (p. 32).

With further progress along these lines, we may gradually come closer to gaining better knowledge about the relation between mind and matter.

NOTES

1 Since it seems that RNG tends to be the more frequently used acronym in parapsychology, it will be conveniently adopted for use throughout the rest of this paper.

2 RNGs with a similar conceptual design are also described by Vincent (1970), H. Schmidt (1977), and Aguayo et al. (1996).

3 It should again be emphasized that the RNG trial output example graph shown in Figure 2 only reflects an *ideal* illustrative example of how the RNG output should be expected to look when roughly conforming to purely random behavior (i.e., random fluctuations around MCE). RNG output does not always conform this closely to the ideal, as there can be some degree of variability in the random noise inherent in an RNG’s output which can lead the data to slightly “drift” away from MCE (whether upward or downward) purely by chance. Although such “drifts” tend to be relatively short-lived, they can at times be persistent enough to approach (and even exceed) the significance threshold for a brief time, again purely by chance. Over the long run, however, the output of suitably random RNGs tends to stay somewhere below this threshold. The graph shown in Figure 2 is merely meant to serve here as an ideal illustrative example for purposes of comparison with the PK graphs presented elsewhere in this paper.

4 Based on the proportion index (π), an effect size estimate developed by Rosenthal and Rubin (1989, 1992–1993) which is conceptually intuitive in that it “. . . shows the proportion correct, or hit rate, on a scale on which .50 [i.e., 50%] is always the null value” (Rosenthal & Rubin, 1989, p. 332); see also Bösch et al. (2006a, p. 501).

5 The reduction in the number of experiments (as compared to 1989) was largely due to 258 experiments from the PEAR program being statistically condensed down to a single data point.

[6] The reduction in experiment number was again due to the data from the PEAR experimental series and the Mind/Machine Interaction Consortium study being condensed down to smaller individual data points.

[7] This success rate is based on a converted value of Radin's (2006, p. 323) reported weighted effect size to the proportion index (Note 4), using the conversion $\pi = 0.5[\text{effect size}] + 0.5$ (Bösch et al., 2006a, p. 499).

[8] It should be mentioned that Kugel (2011) had reportedly found a number of possible issues with the Bösch et al. (2006a) meta-analysis, which could potentially confound interpretation of its results. These issues include: the inclusion of data from more than 30 ESP tests in what was meant to be a purely PK database; using arbitrarily defined criteria for determining which studies would be included or excluded from the meta-analysis; and suggestive instances of faulty data coding. The results of this meta-analysis should perhaps be approached and interpreted with some degree of caution for this reason.

[9] Readers might initially notice a potential inconsistency here, in that Bösch et al. (2006a) had reported this correlation as being *positive* in the text of their paper (which seems to have stemmed from the way they coded the variables in their assessment). However, they state for clarification that it is ". . . indicating that lower quality studies produced larger effect sizes" (pp. 507–508), which would imply a *negative* correlation.

[10] Readers may notice that the statistical result in this case is reported as a *T*-score rather than a *Z*-score, which was done because the instability of the high-speed noise source tended to cause the trial output of the RNG to deviate somewhat from its expected binomial distribution. But as Dobyns et al. (2004) point out, since the associated degrees of freedom ". . . are in the range 104 to 105 . . . the *T*-distribution differs negligibly from the standard normal distribution" (p. 373), allowing the two types of scores to be considered relatively comparable in this case.

[11] This is because Hansen (2006) has previously pointed out a potential statistical issue with Stevens' (2005) study that still may require addressing.

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APPENDIX TABLE
Retroactive Psychokinesis Experiments Using Pre-Recorded RNG Targets

Study	Description	Pub Vol #	*z*-score	*p*-value
1	Bierman & Houtkooper (1975) – High-Speed RNG	EJP 1(1)	0.89	.186
2	H. Schmidt (1976) – Pre-Recorded Targets 1	JASPR 70	3.14	.0008
3	H. Schmidt (1976) – Pre-Recorded Targets 2	JASPR 70	4.22	.00001
4	H. Schmidt (1976) – Pre-Recorded Targets 3-Pilot*	JASPR 70	2.41	.008
5	H. Schmidt (1976) – Pre-Recorded Targets 3-Confirmatory*	JASPR 70	2.03	.021
6	Millar & Mackenzie (1977) – Intentional & Unintentional PK	RIP 1976	0.00	.50
7	Houtkooper (1977) – Direct & Random Retro-PK-Pilot	EJP 1(4)	2.28**	.011
8	Houtkooper (1977) – Direct & Random Retro-PK-Confirmation	EJP 1(4)	–0.55**	.709
9	Broughton et al. (1978) – PK Experimenter Psi Effect	RIP 1977	0.00	.50
10	Davis & Morrison (1978) – PK Multiple Feedback Test*	RIP 1977	0.00	.50
11	H. Schmidt (1978) – Take-Home PK Test Pre-Recorded Targets 1*	RIP 1977	3.34	.0004
12	H. Schmidt (1978) – Take-Home PK Test Pre-Recorded Targets 2*	RIP 1977	–1.13	.871
13	Terry & Schmidt (1978) – Conscious & Subconscious PK	RIP 1977	–3.07	.998
14	H. Schmidt (1979) – PK Test Stroboscopic Light*	RIP 1978	2.90	.002
15	Morrison & Davis (1979) – PK Immediate, Delayed, Multiple Feedback–Delay 1*	RIP 1978	0.20	.421
16	Morrison & Davis (1979) – PK Immediate, Delayed, Multiple Feedback–Delay 4*	RIP 1978	0.19	.425
17	Houtkooper et al. (1980) – OT Identity & Feedback-Pilot Expt.	EJP 3(3)	3.23	.0006
18	Houtkooper et al. (1980) – OT Identity & Feedback-2nd Expt. Observe	EJP 3(3)	0.37	.356
19	Houtkooper et al. (1980) – OT Identity & Feedback-2nd Expt. Hidden	EJP 3(3)	–2.45	.993
20	Braud (1981)–Long Distance Time-Displaced PK Selected Participants*	RIP 1980	0.00	.50
21	H. Schmidt (1981) – PK Pre-Recorded & Pre-Inspected Seed-Pilot*	JP 45	2.12	.017
22	H. Schmidt (1981)–PK Pre-Recorded & Pre-Inspected Seed-Unselected*	JP 45	2.19	.014
23	H. Schmidt (1981)–PK Pre-Recorded & Pre-Inspected Seed-Selected*	JP 45	3.42	.0003

24	Talbert & Debes (1982)–Time-Displaced PK Effects Varying Feedback*	RIP 1981	0.00	.50
25	H. Schmidt (1985) – Addition Effect PK Pre-Recorded Targets	JP 49	2.59	.005
26	Bierman (1985) – PK Test for Babies-Amsterdam	EJP 5	–2.08**	.981
27	Bierman (1985) – PK Test for Babies-Bristol	EJP 5	1.00**	.159
28	Schmidt et al. (1986) – Channeling PK Independent Observers	JP 50	2.71	.003
29	Schmidt & Schlitz (1989) – Large-Scale Pilot PK Pre-Recorded Events	RIP 1988	1.66	.048
30	Schmidt & Braud (1993) – New PK Tests Independent Observer	JP 57	1.98	.024
31	Schmidt et al. (1994) – Channeling PK Independent Observers 2	RIP 1991	0.62	.268
32	Michels (1993) – PK Tests With and Without Skeptics	Skepter (4)	1.64	.05
33	Schmidt & Stapp (1993) – Pre-Observation PK Effects	JP 57	1.23	.109
34	H. Schmidt (1997) – RNGs & Living Systems Targets-RNG	JASPR 91	1.68	.046
35	Schmidt & Dalton (1999) – Repeated Efforts Pre-Recorded 1-Multi-PK	JP 63	0.06	.476
36	Schmidt & Dalton (1999) – Repeated Efforts Pre-Recorded 2-Meditation	JP 63	0.66	.255
37	Schmidt & Dalton (1999) – Repeated Efforts Pre-Recorded 3-Click-PK	JP 63	–1.99	.977
38	H. Schmidt (2000) – PK Tests Pre-Sleep State – Pilot	JP 64	2.45	.007
39	H. Schmidt (2000) – PK Tests Pre-Sleep State - Confirmatory	JP 64	2.24	.013
40	H. Schmidt (2000)–PK Tests Pre-Sleep State-Added Day Test	JP 64	1.01	.156
41	Dobyns (2006)–PEAR Retrocausal REG	AIP Proc 863	1.28	.100
42	Watkins & Walker (1996-9/2021) – RetroPsychoKinesis Project Summary	Fourmilab.nl	–0.27	.394
All			**6.82**	**10^{-12}**

Note: Following the approach taken by Bierman (1998), experiments that obtained null results and did not provide any other details or descriptive statistics in their reports with which to calculate a *z*-score estimate were arbitrarily assigned a *z*-score of zero.
AIP Proc–*American Institute of Physics Proceedings*; EJP–*European Journal of Parapsychology*; JASPR–*Journal of the American Society for Psychical Research*; JP–*Journal of Parapsychology*; RIP–*Research in Parapsychology*
* Studies missing from initial Bierman (1998) analysis
** Scores corrected from Bierman (1998) analysis; original sources consulted

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ESSAY

The James Leininger Case Re-examined

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Beginning at age two in the spring of 2000, James Leininger began exhibiting behaviors and making claims that were later believed to resemble the life and death of World War II fighter pilot James Huston, Jr. Over several years, James Leininger provided information about World War II, especially fighter planes, as well as specific details that seemed to match facts about the life of Huston. In 2009 Bruce and Andrea Leininger published *Soul Survivor*, a book in which they narrated the chronology of events that eventually convinced them that their son was the reincarnation of Huston. In 2021 Bruce Leininger published a prize-winning essay focused solely on documenting the alleged evidence in support of this belief.[1]

On the face of it, the James Leininger case exhibits many of the strengths of an ideal case of the reincarnation type. First, the case involves a young child in Western culture who made veridical claims that seem to match important facts about the life and death of a formerly living person. Second, some of the important claims attributed to the child were apparently documented before anyone had identified the previous personality. Third, the child also exhibited behavioral patterns resembling the previous personality. Finally, prominent reincarnation researcher Jim Tucker investigated the case and concluded that it's a "spectacular example of the phenomenon of young children who seem to remember previous lives."[2] Not surprisingly, many survivalists have regarded this case as one of the best-documented cases of the reincarnation type.

By contrast, skeptics have argued that no reincarnation hypothesis is needed to explain the experiences that form the foundation of the James Leininger narrative, nor is any reincarnation hypothesis needed to explain the behaviors and knowledge James subsequently exhibited over the course of several years and which constitute the scaffolding of the story (e.g., Fischer & Yellin, 2016; Shermer, 2018). Rather, quite ordinary experiences, unsurprising coincidences, and common psychological tendencies adequately account for the basic facts of the story. For example, skeptics have argued that James could have easily seen or heard things about aviation and World War II which his parents didn't notice, but which influenced his behavior and knowledge. And promptings by adults who suggested a past-life interpretation of his experiences could easily have contributed to the reincarnation narrative, which was reinforced by a combination of malobservation, confabulation, and a variety of dubious inferences on the part of James's parents.

Researchers who think the Leininger case provides evidence for reincarnation regard the skeptical rejoinders as ill-informed, purely conjectural, and unable to account for the quantity and specificity of the veridical claims James made or the salient behavior he exhibited. Of course, Skeptics regard favorable assessments of the case as resting on unwarranted levels of credulity concerning what the facts are and shortsightedness concerning how otherwise sincere and well-meaning people can be self-deceived and blind to ordinary explanations of seemingly unusual or improbable events.

Over the past two years, I've investigated various aspects of this case. My inquiries have included a broad range of interviews, considerable historical research, and a careful examination of the Leiningers' telling and retelling of their story over the years.[3] This has allowed an important but previously neglected fact-checking of the presumed facts of the case. Equally important, I've uncovered many previously unacknowledged facts that bear on the plausibility of ordinary sources of information shaping James Leininger's experience, behavior, and claims. In this paper I present my findings and discuss their implications for the assessment of the case as alleged evidence for reincarnation.

Jim Tucker was the first researcher to conduct a detailed investigation of the James Leininger case in order to assess its evidential value. His work has been the most thorough to date and the most cited in the literature. For these reasons, I will discuss the James Leininger case in the context of Tucker's earlier investigation and favorable analysis. I will also focus on James's behaviors and claims that were allegedly documented before anyone had identified the previous personality of James Huston, Jr. These "early-bird" items are ostensibly the case's strongest features, so much so that Tucker wrote an entire paper focused on these features of the case.

I will argue two main points. *First, the early-bird items in the James Leininger case are not evidence for reincarnation. Second, a skeptical attitude toward this case as a whole is all things considered more reasonable than endorsing it as even modest evidence for reincarnation.* I'm not arguing that the reincarnation hypothesis is false. I'm not arguing that there is no evidence for reincarnation. And I'm not arguing that there are no good cases of ostensible reincarnation. I'm arguing that the James Leininger case cannot reasonably be regarded as such a case.

Two kinds of general concerns support my skeptical assessment.

First, neither the Leiningers nor Jim Tucker has presented a narrative with a sufficiently *robust* chronology of events. Such a chronology must include contextual details that bear on the adjudication of various proposed explanations of the subject's behaviors and claims. The Leiningers and Tucker have failed to do this. As a result, their narratives exclude significant facts that increase the plausibility of ordinary sources of information shaping James Leininger's experience, behavior, and claims. These include the content of a Blue Angels aviation video James repeatedly watched and his exposure to World War II items on display at the Cavanaugh Flight Museum where he spent considerable time on at least two occasions. For reasons I'll explore, these facts significantly increase the plausibility of non-reincarnation explanations. Consequently, their exclusion creates a problem analogous to dark data in statistical reasoning—what you don't know matters. In the present case, the missing data create a deceptive appearance of plausibility for the reincarnation interpretation of the case.

Second, the plausibility of the reincarnation interpretation of this case depends essentially on there being a *credible* chronology of events. This in turn depends on the reliability of the Leiningers informing us what James said and when he said it, as well as their ability to give us a reasonable assurance that James was not exposed to sources of information that would plausibly have influenced his claims and behavior. I argue that the Leiningers do not satisfy a minimal threshold of reliability in these crucial respects since significant inconsistencies and factual errors vitiate their narrative, especially with respect to crucial features of the case. Moreover, their testimony over the years exhibits a variety of fact fudging and redactions of what James allegedly said and when he said it, typically after the Leiningers discovered facts about Huston's death that were inconsistent with James's claims as originally interpreted or reported.

In Section 1 I present a brief outline of Tucker's investigation and description of the case, together with his assessment of it, specifically with reference to early-bird items. Sections 2 and 3 address the first skeptical concern as a problem in Tucker's investigation and analysis. Sections 4 and 5 address the Leiningers' credibility problems. In Sections 6 and 7 I show the several ways the conclusions reached in previous sections undermine Tucker's favorable analysis of this case.

1. TUCKER'S ACCOUNT OF THE JAMES LEININGER CASE

Tucker first learned of James Leininger in 2002 as part of a planned ABC television program *Strange Mysteries* featuring the James Leininger story and for which Tucker was interviewed but which never aired. After a series of phone conversations and email exchanges with the Leiningers in 2004, Tucker eventually interviewed them over two days in 2010, after the publication of their book *Soul Survivor* (hereafter, SS).

He published a lengthy discussion of the case in his book *Return to Life* (2013), and in 2016 he published a case report article which focused on James's early-bird testimony and behavior.[4] Although the 2002 program never aired, the interviews for that program provided some documentation of James's behavior and claims his parents attributed to him before James's father Bruce allegedly identified James Huston, Jr. as the previous personality. Understandably, Tucker thinks the early-bird evidence is particularly important, so I'll focus on Tucker's 2016 paper below, supplementing it at points with his discussion in the *Return to Life* chapter.

As Tucker explains, the events began in February 2000 when James was 22 months old. James's father took him to the Cavanaugh Flight Museum in Addison, Texas. James was particularly "fascinated by the planes and in particular by the World War II exhibit" (2016, p. 201). He spent nearly three hours at the museum and left with some toy airplanes and an aviation flight demonstration video of the Blue Angels purchased from the museum gift shop and which James watched "repeatedly for weeks" (2016, p. 201). Bruce took James to the museum again on Memorial Day weekend.

Within two months of his first museum visit, James began repeating the phrase "airplane crash on fire" and slamming his toy airplanes into furniture. He also began having nightmares and would shout out "Airplane crash on fire! Little man can't get out!" And he would thrash about and kick his legs in the air.

Tucker says James provided further details about his nightmares in the months that followed. And here Tucker provides an account of increasingly more specific claims. I quote at length for narrative context.

> [James] indicated that [his dreams] were memories of events from the past. He said his plane had crashed on fire and that it had been shot by the Japanese. Two weeks after those statements, James said his plane was a Corsair ... and he talked about flying a Corsair several times . . .
>
> On August 27, 2000, when James was 28 months old, he told his parents he had flown his plane off a boat. When his parents asked him the name of the boat, he said, "Natoma." After that conversation, his father searched online for the word and eventually discovered a description of the *USS Natoma Bay*, an escort carrier stationed in the Pacific during World War II. He printed out the information he found, and the footer of the printout includes the date he did . . .
>
> James's parents asked him a number of times for the name of the little man in his dreams. He always responded with only "me" or "James." A few weeks after James gave the word Natoma, his parents asked him if he could remember anyone else who was with the little man. James responded with the name Jack Larsen.
>
> One day when James was just over two and a half, his father was

> looking through a book he was planning to give his own father for Christmas, one called *The Battle for Iwo Jima 1945*. His father reports that James pointed to a picture showing an aerial view of the base of the island, where Mt. Suribachi, a dormant volcano, sits, and said, "That's where my plane was shot down." . . .
>
> With the ongoing nightmares, James parents eventually contacted Carol Bowman, who had written a book about children's past-life memories. They began a lengthy correspondence. Following Bowman's advice, James's mother started acknowledging to him that the events he was describing had indeed happened to him before, while emphasizing that they were in the past and he was now safe. The nightmares then grew less violent and less frequent. (Tucker, 2016, p. 201)

Tucker provides further behavioral details—for example, when James began to draw, he drew planes in battle scenes with airplanes dropping bombs, and he wrote James 3 on the drawings, which he continued to do even after turning four years old.

As Tucker explains, when James began giving his parents more detailed information about his experiences and apparent recollections, his father Bruce began researching James's claims to see if they corresponded to anything factual. Bruce discovered that there was a Natoma Bay naval carrier that was stationed in the Pacific and took part in the Iwo Jima operations (allegedly verified on August 27, 2000). He later discovered that a man named Jack Larsen served on the carrier during the Iwo Jima operations.

Several months after the filming of the 2002 ABC program, Bruce Leininger attended a Natoma Bay reunion and acquired information that allowed him to verify several other facts. He learned that the Jack Larsen from Natoma Bay had survived the war. Though he was not at the reunion, he was still alive and James's father soon visited him. He also learned that only one pilot from the ship was lost during the Battle of Iwo Jima, a 21-year-old from Pennsylvania named James M. Huston, Jr. (Tucker, 2016, p. 201). Tucker (2016, pp. 201–202) further explains how, after the Natoma Bay reunion, Bruce Leininger discovered further details concerning Huston's death. Huston did not die on Iwo Jima itself, but at nearby Chichi Jima, but he was the only Natoma Bay pilot who died during the

Battle of Iwo Jima. And Jack Larsen flew with Huston on the fatal Chichi Jima mission. Tucker says, "As Huston was the only pilot from *Natoma Bay* killed during the Iwo Jima operation, and as his details closely matched James's statements, his parents concluded that he was the man whose life James had recalled" (2016, p. 203). In the months that followed, James made further claims about Huston and WW2 which the Leiningers confirmed.

Tucker's 2016 case report focuses on statements and behavioral patterns attributed to James in the 2002 unaired ABC program, filmed before the previous personality of James Huston had allegedly been identified. Tucker provides a chart that compares the accuracy of these statements and behaviors with Huston. The chart (Table 1) is a reproduction of the one Tucker provided in his case report. Tucker's table shows the item/source documented in the 2002 program. The verifications and comments are mostly from sources consulted after the filming of the 2002 program.

Tucker tersely considers four possible explanations of the facts of the case: Fantasy, knowledge acquired through normal means, psi-mediated transfer, and reincarnation. After dismissing the first three, he concludes:

> The documentation in James's case provides evidence that he had a connection with a life from the past. On the face of it, the most obvious explanation for this connection is that he experienced a life as James Huston, Jr. before having his current one. The facts in this case indicate that this explanation warrants serious consideration. (Tucker, 2016, p. 206)

In *Return to Life*, Tucker provides an expanded case discussion. For present purposes, the most important part concerns three other items Tucker says were documented before the Leiningers had identified Huston as the previous personality. The first is James signing his name James 3 on drawings he made between the ages of three and four. The second is James giving his parents the name Jack Larsen. The third is James giving the name Natoma as the name of the boat he flew his plane off of. Tucker mentions all three items in his 2016 report, but he does not put them in the category of early-bird claims there since

TABLE 1

Statements and Behaviors by James Leininger Reported in ABC News Interview, as Compared with James Huston's Death[a]

Item	Source of Information	Verification of Match with Huston	Comments
"Mama, before I was born, I was a pilot, and my airplane got shot in the engine, and it crashed in the water, and that's how I died."	James's mother	Memoir of veteran who saw Huston's plane get hit. James's father also talked to three eye witnesses. One later told a television interviewer, "I saw the hit. I would say he was hit head on, yeah, right on the middle of the engine."	
Died in the Battle of Iwo Jima	Narration states that James may have been one of the pilots who died in the Battle of Iwo Jima	History of Composite Squadron Eighty One (VC-81) and aircraft action report	Huston was the one pilot from USS *Natoma Bay* killed during the Iwo Jima operation. His plane was shot down as he took part in a strike against transport vessels in a harbor on nearby Chichi-Jima
Nightmares of plane crashing on fire and sinking and being unable to get out	James's father	Aircraft action report states no wreckage of the plane was left afloat	
Flew a Corsair	James's mother	Pictures of Huston with Corsairs and squadron VF-301; interviews by James's father with veteran and son of veteran who served with Huston in the squadron	James seemed to say that he was flying a Corsair when he crashed. This was incorrect for Huston, who was flying an FM-2 when he died, but Huston had flown Corsairs earlier when he made test flights with VF-301
Plane flew off a boat	James's mother	Numerous military records document that James Huston was a pilot on USS *Natoma Bay*	
His plane was shot down by the Japanese	James's mother	History of Composite Squadron Eighty-One (VC-81) and aircraft action report	
Corsairs got flat tires when they landed	James	Air Force historian interviewed in the segment stated Corsairs bounced when they landed, leading to flat tires	

[a] Interview conducted when James was four years old, before Huston was identified.

he's classifying the early-bird claims solely based on whether they were mentioned in the 2002 ABC program, and none of these items were mentioned in the program.

Tucker includes these items in his expanded list of early-bird claims because there is alleged documentation for each. He saw the drawings James made when he was three (and later four) and signed James 3. An image of one of these is included in his 2016 case report. Based on a printout of Bruce Leininger's Internet search activity on October 16, 2000—he searched the name Jack Larsen—Tucker places James's giving the name Larsen around that date. Tucker's justification for including "Natoma" as an early-bird claim is a date/time-stamped computer printout of the entry on the Natoma Bay from the online version of *the Dictionary of American Naval Fighting Ships*. Bruce claims he printed this document the night James gave the name Natoma. Tucker includes an image of the document in his 2016 report.

Evidential Force of the Case

Tucker doesn't explain why the presumed facts he presents are evidence for reincarnation, much less how strong the evidence is supposed to be. He seems to link explanatory value and evidential value, as if the former converts to the latter. Even if that is plausible, it needs to be argued, especially since there is nothing *obvious* about the explanatory value of a hypothesis with poorly defined parameters and which is contrasted with a truncated set of competing explanations. To the extent that Tucker offers support for his evidential and explanatory claims, his reasoning is mostly impressionistic. Such reasoning exploits the psychological tendency of people to make certain kinds of inferences; it does not show that the inferences are cogent. Such reasoning also conceals, rather than critically engages, a variety of contentious assumptions. These are fairly widespread conceptual problems in the literature on survival which I've discussed at length elsewhere (Sudduth, 2016).

For present purposes, I'll set aside the above conceptual problems and assume that reincarnation can explain the facts in the James Leininger case. I will also assume that the facts Tucker has outlined are, *all other things being equal*, evidence for reincarnation—that is, the presumed facts in this case provide *prima facie* evidential support for the reincarnation hypothesis. The crucial question is whether all other things *are* equal, or whether there are considerations that defeat whatever evidential force we might initially be inclined to give the facts in this case.

In general, the evidential force of facts depends on two conditions:

(N) There are no additional facts that neutralize the evidential force of the presumed facts.

and

(D) There are no significant grounds for doubting the presumed facts.

If either (N) or (D) does *not* obtain, then the *prima facie* evidential force of facts in support of a particular belief, claim, or hypothesis is overridden or defeated. To be clear, if only one of the two conditions is not satisfied, then what is otherwise evidence for a claim cannot reasonably be taken as evidence for that claim. Below I'll argue that neither (N) nor (D) is true in the James Leininger case, specifically with respect to the case's presumed early-bird items. Therefore, we should not regard these features of the case as evidence for reincarnation. Moreover, given the nature of the problems which infect the early-bird items, as well as how these items relate to the rest of the case, we should also be skeptical of the case as a whole.

2. A PRELIMINARY INVESTIGATIVE CONCERN

Before delving into my findings, there is a preliminary investigative concern which should be addressed, especially since it bears on some of what I'll subsequently discuss. The time gap between the original events and Tucker's subsequent investigation of the events is problematic.

Although Tucker had "sporadic correspondence with James's parents for several years" prior to his in-person interview with them, he wasn't able to conduct an in-person interview until 2010. This was after the Leiningers had published their book and ten years after the initial events and about eight years after the Leiningers claimed to have located the previous personality (Tucker, 2016, p. 203). This is a considerable time gap. The wider the time gap, the greater the challenge to meticulously reconstruct a robust and credible chronology—that is, a factually rich chronology based on reliable testimony and documentation. This is the only way to provide reasonable assurance that conditions (N) and (D) above have been met.

Tucker is aware of the potential problems here. For example, he acknowledges that a delay can create obstacles to getting the appropriate verification of alleged facts (Tucker, 2013, p. 79). He also concedes, "I might have been able to corroborate parts of [the Leininger case] more if I could have studied it sooner" (Tucker, 2013, p. 87). Of course, Tucker is convinced that the Leiningers' story holds up "very, very well," despite the liability he acknowledges.

I don't share Tucker's optimism. The specific issues I'll be addressing later in the paper will show why I don't think Tucker's optimism is warranted. But we can note here a few reasons why we should be suspicious of Tucker's optimism even before considering my findings.

First, contemporaneous notes or other forms of detailed documentation help with accurately reconstructing past events, but the Leiningers lost or discarded the notebook in which they kept notes of the unusual events they were watching unfold over several years (Tucker, 2016, p. 203). Tucker says this happened shortly after the Leiningers published their book, but he doesn't regard it as "fatal" to the case since "verification is available for much of the story" (Tucker, 2013, p. 79). These include the content of the unaired 2002 ABC program, primary source documents concerning the circumstances of Huston's death, and documents the Leiningers have presented but which are not independent of their testimonial claims about the genesis and context of the documents. For reasons to be explored later, these are meager verifications at best.

Second, Tucker admits that James's behavior from ages two to five play an important role in this case—for example, James's waking up from nightmares in a panic, his practice of crashing toy airplanes into his parents' coffee table, his mimicking aviator behavior, and his later drawings of warbirds in combat. Tucker thinks this behavior is consistent with post-traumatic play. Since there is allegedly no trauma in James's past in his present life, Tucker leverages this to support the conjecture that the trauma must have been in a past life.

But Tucker never directly observed James's relevant behavior. He had to rely on videos and what James's parents and relatives had to say about what they observed. While some of the reported facts may be accurate—for example, James crashed planes into furniture and broke off the propellors of his planes—what's crucial is the *interpretation* of the behavior. But the witnesses were not trained in child psychology or psychiatry and so can't be trusted with a nuanced interpretation of what they observed. It's reasonable to ask how well-justified a post-traumatic play conjecture can be in the absence of first-hand observations of and/or interviews with the subject close to the time he displayed the behavior in question.[5]

Finally, the chronology of events covers several years, roughly from February 2000 to September 2006, at which time James's parents took him to Futami Harbor, the crash site of James Huston. While the foundational events and claims of the case occurred between spring 2000 and early fall 2000, much of the scaffolding of the case comes from a broad range of incidents from fall 2000 to the summer 2002, prior to the Leiningers identifying Huston as the presumed previous personality. And from fall 2002 to spring 2004, there's another list of claims attributed to James concurrent with Bruce Leininger's further historical research into details about the Natoma Bay and the death of Huston.

The lengthy timeline invites many ordinary sources to shape James's experience, behavior, and claims. As I'll show below, there's a proliferation and aggregation of *occasions of exposure*—that is, experiences James had involving salient information. If these exposures are not highly plausible sources of James's claims and behavior, they at least muddy the water, for they would make it exceedingly difficult to distinguish between sources which *supplied* information and sources which *confirmed* what James knew independently of such sources.

Tucker's lecture and interview presentations of the case tend to obscure this crucial point. He tends to provide an itemized list of James's claims and their alleged verifications abstracted from the complex and protracted chronology to which they are tied. And even when he provides the sequence of events, considerable ambiguity vitiates the presentation. Presumably Tucker wishes to present the cumulative weight of all the facts and not take them in isolation from each other. A noble goal. But it's equally important to have a clear chronology that acknowledges and includes salient contextual details—for example, the ordinary sources available to James at various stages in the narrative. Without such a robust chronology, Tucker runs the risk of analyzing a narrative which is predisposed to favor an extraordinary explanation of what are in fact ordinary phenomena. Without a robust chronology of events, we're flirting with confirmation bias.

Some of the concerns above could've been mitigated had Tucker interviewed the Leiningers in 2004 or earlier. Regardless of the reasons for the delay, it impeded a professional investigation of the veracity of the Leiningers' claims. Between 2004 and 2010, the "facts" had ample time to evolve, or what is just as probable, certain potentially inconvenient facts had ample time to be consigned to an epistemically inaccessible past. While the Leiningers appear interested in presenting evidence that their son is the reincarnation of James Huston, Jr., it's at least problematic that they didn't permit at least one qualified investigator to document and underwrite their claims in a timely manner.

3. SIGNIFICANT FACTS EXCLUDED FROM TUCKER'S ANALYSIS

Above I said that a necessary condition of the facts Tucker presents being evidence for the reincarnation hypothesis is:

(N) There are no additional facts that neutralize the evidential force of the presumed facts.

All inductive reasoning, including explanatory reasoning, is subject to a total evidence requirement. It's relatively easy for facts to offer evidential support for any hypothesis or theory. Every instance of the fallacy of stacking the deck—only considering the evidence that favors one's preferred theory—demonstrates this truism. And it's just as easy for any positive evidential status to diminish with the acquisition of new facts. For this reason, we have to consider as many salient facts as possible, especially facts that (greatly) lower the plausibility of a hypothesis.[6]

In this section, I'll consider a large number of such facts which significantly lower the evidential force of Tucker's early-bird items. These are facts Tucker has excluded from his analysis, and in some cases not even acknowledged. They are facts that reduce the plausibility of the reincarnation hypothesis Tucker proposes, and they do so by raising the plausibility of non-reincarnation explanations of the early-bird claims. The facts concern James's wider experience and exposure to ordinary sources of information that plausibly shaped his claims and behavior. Among these known exposures are videos James watched and at least two lengthy visits to the Cavanaugh Flight Museum in Addison, Texas.

The exposures discussed below are significant for two reasons. First, they have explanatory relevance. James was exposed to information which appears as the content of his claims and which illustrates the behavior he displayed. So, he was exposed to potential ordinary sources of information that influenced his claims and behavior. More specifically, his exposures satisfy Ian Stevenson's criterion of explanatory relevance. In discussing the explanatory force of appeals to ordinary sources of information, Stevenson says, "It is one thing to speculate on possible sources of information and quite another to show a specific matching between a subject's statements and a definite source of information providing the ingredients of those statements" (Stevenson, 1974, p. 340). Second, the exposures were temporally prior to and concurrent with James's claims and behaviors. Not a few claims and behaviors, but nearly everything between spring 2000 to spring 2002. These are items the Leiningers and Tucker invest with considerable evidential significance, and which constitute the foundation of the James Leininger narrative.

The Blue Angels Video

Although not mentioned in the 2004 ABC *Primetime* television segment, when Bruce took James to the Cavanaugh Flight Museum in February 2000, he purchased at least one toy airplane and a Blue Angels video for James from the museum gift shop. James repeatedly watched the video for months (SS, pp. 21–22, 24, 57, 118). Although Tucker makes reference to this potentially significant contextual detail, he never watched the video. His explanation is straightforward: He couldn't locate it (Tucker, 2013, p. 67; 2016, p. 201). Yet, he doesn't consider this problematic. Referring to James's first visit to the Cavanaugh Flight Museum, Tucker writes:

> When they left after three hours, James had some toy planes, as well as a video called *It's a Kind of Magic* about the Blue Angels, the Navy's flight exhibition team. James loved the video, and he watched it repeatedly for weeks. The trip and the video started (or uncovered) his love for planes. This passion may have led to some of the knowledge of planes and flying that James often surprised his parents by voicing. The video, however, was clearly not the source of James's information about World War II, since the Blue Angels group was founded in 1946 after the war ended. (Tucker, 2016, p. 201)

If Tucker couldn't find the video, that's not surprising. Bruce Leininger gave the name of the video as *It's a Kind of Magic* (SS, p. 24; cf. Tucker, 2016, p. 201). But this is incorrect. There is no Blue Angels video by that name. The video (Figure 1) is actually called *Blue Angels: Around the World at the Speed of Sound* (1994, A&E Home Video), narrated by Dennis Quaid and featuring the Queen song "It's a Kind of Magic." To confirm this, I relied on photos of the VHS tape James watched which Bruce Leininger sent to researcher Leslie Kean.[7]

One might suppose that being mistaken about the title of the Blue Angels video is an insignificant factual error, except that it apparently prevented at least one qualified researcher from locating it and examining its content. And if the content is salient, the error is greatly consequential.

Tucker concedes that the Blue Angels video might be the source of some of James's knowledge of planes and flying. Of course, had Tucker seen the video, he could offer more than a conjecture here. He'd be able to say which claims attributed to James could plausibly have been derived from the video, or how the video might have otherwise shaped James's experiences and the evolving narrative of his experience. Instead of illuminating the case in this way, he dismisses the importance of the video. He reasons that since the Blue Angels were formed in 1946, after WW2, the video *clearly* was not the source of James's knowledge about World War II.

As it happens, discussion of WW2, often with archival footage, was common in the Blue Angels videos produced in the 1990s. That's because there are several connections between WW2 and the formation of the Blue Angels, including the motivation for forming the Blue Angels, the technology that was used, and the pilots who flew the

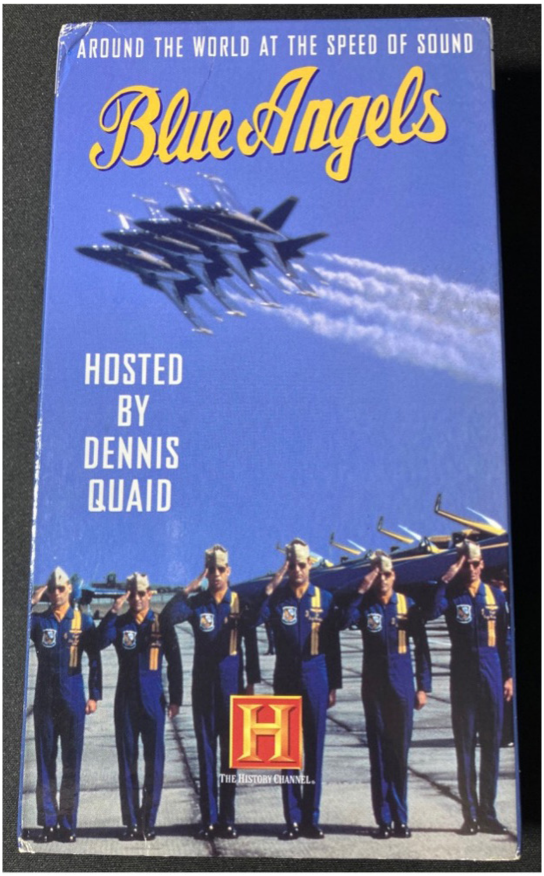

Figure 1. Cover to the 1994 Blue Angels VHS Tape. (Photo credit: Michael Sudduth)

planes. And the Blue Angels video James watched was no exception. It contains a 10-minute segment on WW2 with archival footage.[8]

More importantly, Tucker's reasoning is a red herring. Many of the evidentially salient features of this case aren't about James's knowledge of WW2 or his apparent knowledge of James Huston. Between May and August 2000, James made many claims about planes and about the content of his nightmares. These form the foundation of the case, but they are not explicitly about WW2 or James Huston. And this is true of the claims Tucker thinks make the case a strong one. Of the seven early-bird items that are the focus of Tucker's 2016 case report, at the most three are about WW2 and broadly related to Huston's role in the war, namely his death taking place during the Battle of Iwo Jima, being shot down by the Japanese, and his claiming to have flown a Corsair.

Moreover, as Tucker himself acknowledges, the case isn't merely about veridical claims concerning Huston's role in the war. It's also about *behavior* suggestive of Huston's experience as a fighter pilot and the circumstances and manner of his death. "This case," Tucker says, "involves a boy who displayed behaviors that suggested he was recalling a traumatic crash" (Tucker, 2016, p. 203; cf. 2016, p. 201). Item #3 on Tucker's list (in Table 1) concerns James's recurring nightmare experience of flying a plane that was shot, caught on fire, and crashed in the water. The behaviors also include James crashing planes into a family coffee table while saying "Airplane crash on fire," his recurring nightmares of a plane crash, and his panicked mimicking of the movements of someone seemingly trapped and trying to extricate himself from a compartment.

If Tucker never watched the video, he's poorly positioned to rule out the salience of the video as something that might have contributed to James's behavior. And since James's waking state behavior consists of his acting out or conveying what he was experiencing in his dreams, the question to ask is whether the content of the video might have contributed to James's nightmares. Without seeing the video, Tucker is in no position to rule it out as a plausible source of the experiential and behavioral features of the case.[9]

But the Blue Angels video is relevant for another reason. The Leiningers emphasize many of James's claims and behavior related to general aviation as evidence of his having lived a previous life. For example, by the time James was four years old, he had expressed a desire to fly an "F-18 Hornet" and be a "slot pilot" (SS, p. 127), was able to identify the tailhook as a distinctive feature of naval planes (SS, p. 130), could mimic settling into a cockpit, including adjusting the headgear (SS, p. 126–27), and bringing himself to attention and saluting (SS, p. 120). The Leiningers regard these items as atypical for a child his age and suggestive of an old soul inhabiting James's body, a soul intimately familiar with aviation.

So, what is the content of the Blue Angels video? And how does it plausibly illuminate aspects of this case?

Contrary to what Tucker and the Leiningers would lead us to believe, even a cursory viewing of the Blue Angels video shows that its content is highly relevant to James's nightmares, his behavioral patterns, and the kinds of claims for which his parents—on whom Tucker heavily relies to convey salient facts—could find no normal explanation.

> —*All* of the claims and behaviors the Leiningers attribute to James (above) as something ostensibly remarkable and for which they could identify no normal source are either mentioned or visually represented in the video, some nearly a dozen times—for example, the naval salute, carrier landings, fighter planes with tail hooks, and aviation terminology such as "slot pilot." (Atkeison, 2020, 0:06:25–0:06:30, 0:07:18–0:07:20, 0:08:18–0:08:20, 0:14:38–0:15:27, 0:23:28–29, 0:24:50–0:24:54, 0:40:34–0:40:38.)[10]
>
> —There is a 10-minute segment on WW2 that discusses the connection between WW2 and the Blue Angels, including a specific reference to the war in the Pacific and the importance of naval carriers to that aspect of the war. This segment includes archival footage of fighter planes landing on carriers (e.g., AT-6 Texan) with tailhooks visible, images and archival footage of Grumman F6F Hellcats (a prominent fighter plane in the latter part of the war in the Pacific), and it emphasizes how some of the first Blue Angel pilots were war heroes of WW2 (Atkeison, 2020, 0:14:40–0:23:00).
>
> — A pilot named Larsen appears in this video (name on screen), the pilot of the Blue Angels equipment transport plane, a Lockheed C-130 named after the children's television show *Fat Albert* (Atkeison, 2020, 0:52:42–0:54:00). Recall that James allegedly gave the name Larsen as the name of a pilot who appeared in his dreams.
>
> —Proximate to footage of fighter planes taking off and landing on naval carriers, there is archival footage of an American fighter plane shooting down an Iraqi MiG, the back end of which explodes after being hit with a rocket. An aerial shot of fighter planes dropping bombs on land targets follows this (Atkeison, 2020, 0:51:36–0:51:46).

James's early behaviors, his use of aviation language, and his demonstration of knowledge of naval fighter planes—all of which impressed and bewildered his parents—clearly mirror information verbally and visually conveyed on the Blue Angels video.

What's crucial here is not that the Leiningers regard these items as evidentially strong—this obviously undermines *their* credibility, something I'll discuss below. But there's a downstream consequence on Tucker's own account and analysis of this case. In addition to his not acknowledging the content of this video and its bearing on the case, he assumes that the Leiningers are reliable judges of the ordinary sources of information James was exposed to (Tucker, 2013, p. 83). Yet they failed to inform Tucker of just how much of their child's Huston profile is found on a video he was regularly watching for months, so much that his father had to replace the video with new copies. Tucker's analysis of the case depends on the Leiningers having a degree of reliability in at least one crucial respect that they do not have.

What's most significant, though, is that some of the content of the video is clearly relevant to aspects of the case Tucker thinks are especially salient to the reincarnation interpretation.

> —Contrary to what Tucker suggests, there is material on WW2 on the video, including archival footage of WW2 planes landing on carriers. That this material establishes a connection between WW2 and the Blue Angels is not insignificant given James's early idolizing of the Blue Angels.
>
> —Tucker was unaware that the video shows vivid imagery of a plane on fire after being shot during combat, nor was he aware that the video shows fighter planes dropping bombs on land targets. That this segment is proximate to the material on WW2 is also significant as it ties together two crucial threads: WW2 and a fighter plane on fire after being shot in combat.

I'll return below to the Leiningers' repeated assurances that James was not exposed to any images of planes on fire or being shot down. Clearly, he was. James repeatedly watched a video in which planes take off from and land on aircraft carriers. There are multiple references to WW2, the war in the Pacific, and the combat-ready nature of the Blue Angels, including archival footage of WW2 fighter planes. The video also exposed James to vivid images of a fighter plane being shot in combat and exploding on fire in the air, followed by scenes of land targets being bombed.

As I'll develop in the next subsection, the video wasn't James's only exposure to such vivid imagery, but even if were, it would still be a significant fact in the causal vector of the experiences that form the genesis of the narrative. It's unsurprising that a toddler who repeatedly watches a video with such images would develop nightmares in which he's flying a plane, gets shot, and his plane catches on fire and crashes in the water.[11] Nor is it surprising that this fear would motivate James's repeated claims about his family members dying in planes taking off from the Lafayette Regional Airport (SS, pp. 37, 48–50, 76). What is surprising is that his parents were apparently unaware of the content of this video or were aware of it but didn't think it was significant.[12]

Now let's consider how the video bears on Tucker's list of early-bird claims.

Of the seven items Tucker lists in Table 1, the Blue Angels video is a plausible source for at least three of those items. The Blue Angels video contains several scenes of planes flying off a boat (item #5 in Table 1). And items #1 and #3 convey the content of James's recurring dream. Surely, it's at least plausible that the video's graphic depiction of a fighter plane exploding in the sky after being shot during combat and bombs being dropped on land targets contributed to James's dreams. Perhaps other factors also played a role—for example, family stress from the recent move from Texas to Louisiana, James's hearing news reports about any of the several commercial plane crashes between February and May 2000, seeing his father off multiple times at the airport and indicating that his father's plane was going to crash, aviation amusement park rides proximate to the intensifying of his dreams, and his recently turning two, the age at which toddlers begin having nightmares. What would've been genuinely surprising is if James had no nightmares under these circumstances in this period of his toddler life.

From the beginning, skeptics proposed the *possibility* that James was exposed to something in his ordinary experience which triggered his nightmares or informed their content. But we don't need to posit a mere hypothetical possibility. We know the kinds of salient imagery he was exposed to while watching the Blue Angels video, and the exposure was long-term and repeated. We know his general life circumstances. We're not at a loss to offer plausible conjectures as to what ordinary events and experiences might've triggered James's nightmares.

But James also claimed that his plane was shot down by the Japanese, whom he identified by the symbol of the "big red sun," that he flew a Corsair, that Corsairs tended to get flat tires, and that he died in the Battle of Iwo Jima. What about these four early-bird claims?

This requires that we look at another important source of information in James's experience, partially concurrent with his exposure to the Blue Angels video, namely his trips to the Cavanaugh Flight Museum.

The Cavanaugh Flight Museum

According Bruce and Andrea Leininger, Bruce took James to the Cavanaugh Flight Museum in Addison, Texas, in February 2000 and again on Memorial Day weekend the same year. Despite the Leiningers mentioning this in their book, researchers discussing this case have routinely ignored or marginalized this aspect of the case. Tucker, for example, mostly ignores it. This is particularly astonishing since James's nightmares began no later than May 1, 2000, a couple of months after his first visit to the museum (SS, p. 3). And about two weeks after the second visit, around mid-June, James first began to verbalize the content of his nightmares (SS, pp. 28, 33–34).

The Leiningers say that Bruce and James spent nearly three hours at the museum on their first visit. During this time, James was mesmerized by the WW2 planes—including the "F-104 Thunderchief . . . Mustangs and Spitfires and Wildcats" (SS, pp. 23–24). We're also told that "James spent a lot of time browsing among the toy airplanes" in the museum's gift shop where the ticket office is located (SS, p. 24). We also know that James and Bruce were separated at least once because Bruce had moved on to a hangar which displayed more recent planes only to discover that James was not with him and had returned to the hangar with the WW2 planes (SS, p. 24). After Bruce purchased a toy airplane for James—we're not told the model of aircraft—and a Blue Angels video, he took James to watch planes take off at the Addison Airport (SS, p. 25).

There isn't much detail about the second visit at the end of May. It seemed to have been similar to the first visit, except that on this occasion Bruce and James had a conversation with Charles R. Bond, Jr., a war veteran who flew a P-40 Warhawk with the Flying Tigers in WW2 (SS, p. 25). It's unclear how long this conversation lasted or the extent of its content. We're only told that Bond identified himself, indicated that he had flown a plane like one of the planes in the hangar, and gave James an Angel pin. Unlike the first trip, Bruce took pictures of James at the museum.

Shortly after the Leininger case received public attention in connection with the airing of the 2004 *Primetime* episode, skeptics claimed that James most likely saw things at the Cavanaugh Flight Museum which influenced him. Since the museum has WW2 planes and other WW2 memorabilia on display, the general skeptical concern is reasonable, especially since the *Primetime* episode neglected to mention James's visits to the museum. Some skeptics have offered more specific arguments. For example, Michael Shermer (2018, p. 105) has argued that James's ability to identify the Corsair plane by name stems from seeing a Corsair on display at the museum. Similarly, Fischer and Mitchell-Yellin (2016, p. 127) propose that James's experience of interacting with a Corsair plane during his museum visit may plausibly explain his subsequently dreaming he was flying one. In response, Tucker (2013, p. 69) has claimed that there was no Corsair plane on display at the museum when James visited in 2000, and so the museum could not be the source of James's knowing the name of the plane or his ability to identify it.[13]

Tucker is correct about there being no actual Corsair plane on display at the Cavanaugh museum when James visited, but this is a weak response to the skeptical objection. Seeing an actual Corsair plane is not the only way James could plausibly have learned the word *Corsair* and associated the word with the distinctive looking inverted gull wing WW2 plane. A toddler who spends three hours at a natural history museum doesn't need to see a life-sized reconstruction or reproduction of a Tyrannosaurus Rex to learn the name of this particular dinosaur and how to identify it. But also, Tucker's response doesn't as much as even address the general skeptical objection, which is that it's plausible that the lengthy museum visits influenced James *in some salient manner*—James's ability to identify the Corsair plane is only one of many items of knowledge he exhibited.

But Tucker's response is otherwise problematic. The Corsair plane Tucker tells us was not at the Cavanaugh Flight Museum in 2000 when James visited was actually located in James's hometown of Lafayette, Louisiana, housed at the Lafayette Regional Airport, where James often journeyed to see his father off on business trips. It was also rolled out at various times for public events like the Sertoma Cajun Air Festival which James attended. Tucker attempts to eliminate the possibility that James saw a particular plane at a location 600 miles away from his home by proving that the plane wasn't at the location when James visited. This is not an effective dialectical strategy if that very plane was actually located 3 miles from James's front door at a location he frequented. I'll discuss this further below as it merits separate treatment, but clearly Tucker's rejoinder to the skeptical objection gets no mileage.

Let's not lose sight of the central investigative flaw here. Although Tucker contacted the Cavanaugh to discredit one particular skeptical objection, that's where his inquiry ended. He didn't inquire as to whether the museum had photos or artwork of Corsair planes on display, or model/toy Corsairs available in the gift shop. He didn't ask whether there were aviation videos on display for visitors to view, nor whether such videos had archival WW2 footage. Tucker didn't do any Internet sleuthing to acquire information about the layout and contents of the museum in 2000, nor did he acquire a museum guidebook from the time period to help with this. As a result, the general skeptical objection remains intact.

Recall Ian Stevenson's well-advertised criticism of skeptical objections: "It is one thing to speculate on possible sources of information and quite another to show *a specific matching between a subject's statements and a definite source of information providing the ingredients of those statements*" (Stevenson, 1974, p. 340, emphasis mine). I did precisely the latter with the content of the Blue Angels video. We can do the same with the Cavanaugh Flight Museum. We can do more than speculate about what James *could* have learned there because of what *could* have been on exhibit there. If we know what was on display at the museum when James visited, as well as the length of James's visits and the parts of the museum where he spent considerable time, we can make *plausible* inferences about information he acquired during his two known visits there.

As it happens, the contents of the museum when James visited are knowable. I consulted the 1999 *Cavanaugh Flight Museum Guidebook* and the Cavanaugh Flight Museum website for the years 1999, 2000, and 2001.[14] I also corresponded with Christy Bonds (assistant in the Cavanaugh Flight Museum gift shop 2000–2001 and 2003–2007, and manager of the gift shop 2007–2011) and Cassidy Rees (Gift Shop Associate at the Cavanaugh 2019–present). Bonds and Rees provided answers to about a dozen questions concerning the museum and its gift shop. Based on these sources, I compiled the following list of some of the relevant items that were on display when James visited the museum in February and May 2000.

—Large WW2 Japanese battle flag with the Japanese "red dot" symbol.

—Artwork and photography of warbirds in the museum gallery, including large artwork depicting the Corsair in combat over islands.

—B-25 and Spitfire drop tanks.[15]

—P-51 Mustang.[16]

—FM-2 Wildcat with a drop tank, the type of plane Huston crashed in.

—A room called the Canteen where visitors could watch aviation videos focused on WW2 and Vietnam.

—A variety of model and toy Corsair planes in the museum gift shop.

Let's consider the significance of these items, together with images of some of the salient items and exhibits. They correspond nicely to the six WW2 related claims attributed to James in the months following his museum visits in 2000.

i. The month following his first visit to the museum—March 14, specifically—James was able to identify a *drop tank*, the exterior fuel tank fighter planes often had to extend their mileage range. There was a bin full of toy airplanes and boats outside Hobby Lobby and James's mother had pointed to part of a plane and said it had a bomb on it. James said, "That's not a bomb, Mommy. That's a dwop tank" (SS, p. 17; cf. Tucker, 2013, p. 81). See Figures 2, 3, and 4 taken from the Cavanaugh webpage in 2000.

Figure 2. FM-2 Wildcat with Drop Tank (below wing). Photo credit: Cavanaugh Flight Museum

Figure 3. B-25 Drop Tank. Photo credit: Cavanaugh Flight Museum

Figure 4. Spitfire Drop Tank. Photo credit: Cavanaugh Flight Museum

ii. In May 2000, James's nightmares began, and by June he was able to describe some of the content of the dream. What did he describe? At first, he described his plane crashing and there being a big fire (SS, pp. 33–34). Then that he was trapped and unable to escape (SS, pp. 55, 63–64). Eventually he said his plane was shot down (SS, p. 65). See Figures 5 and the Blue Angels video. Figure 5 is a 2021 photograph of image that was in the museum gallery in 2000.

iii. In August 2000, he spoke of flying a *Corsair* off a boat (SS, p. 79). See Figures 5 and 12—These large framed paintings were in the gallery in 2000, though the photographs of the artwork were taken in 2021. Figure 5 is a 2021 photograph of an image that was in the gallery in 2000. Figures 6, 7, and 8 are 2021 photographs of the toy section and illustrate the kinds of toys and model planes, including Corsairs, which would have been on display and available for purchase in 2000.

iv. In August 2000 he spoke of being shot down by the *Japanese* (SS, p. 79). When asked how he knew it was the Japanese who shot him down, he told his parents it because of "the big red sun," a clear reference to the red dot symbol the Japanese used (SS, pp. 65, 69–70). See Figures 9 and 10, both of which are 2021 photographs of the same flag that was at the museum in 2000.

v. Andrea Leininger, in her early correspondence with Carol Bowman, said James was able to identify the *P-51 Mustang* (SS, p. 116). This plane was on display at the Cavanaugh museum, which even the Leiningers mention in passing (SS, p. 24). See Figure 11, which is from the Cavanaugh website in 2000.

vi. In November 2000, while Bruce Leininger was looking through a book on Iwo Jima (Wright, 1999, p. 3), James pointed to an aerial view of Mount Suribachi on the south end of Iwo Jima with its beach areas and said that's where his plane was shot down (SS, p. 104). See Figure 12. The image was at the museum in 2000, though the photograph was taken in 2021.

As the Leiningers sum it up: "There were the tantalizing clues: the big red sun, the Japanese involvement, the fact that James thought he himself was the guy trapped in the burning plane" (SS, p. 72). These might be tantalizing if James had not been exposed to such imagery on multiple occasions beforehand. But this is simply not the case.

Figure 5. Corsair in "Dogfight Over the Russells" (by Walsh). Photo credit: Cavanaugh Flight Museum

Figure 6. Model WW2 airplanes in gift shop, including Japanese Zero and Corsair. Photo credit: Cavanaugh Flight Museum

If we focus solely on James's early experiences and claims, it's clear the museum is a significant, plausible source for *all* of them. Most of the relevant items were in areas of the museum where we know James spent considerable time—the gift shop and gallery areas at the entrance and adjacent to Hangar 1. Here James would've seen the Japanese battle flag prominently displayed (Figures 9 and 10), a variety of toy and model planes, including the Corsair and Japanese Zero (Figures 6, 7, and 8), and lots of pictures and artwork depicting WW2, including carriers and Corsairs in battle scenes (Figures 5 and 12).[17] In the hangars, James would have seen the planes the Leiningers mentioned, and what they did not mention, namely the FM-2 Wildcat with a drop tank attached and B-25 and Spitfire drop tanks in the artifact areas of the museum (Figures 2, 3, 4).

The Canteen (Figure 13) is of particular interest since it was a room with tables set up with televisions displaying aviation videos, including WW2 videos. The Canteen is located at the entrance to Hangar 2. Today the room is a conference-style room used to display pictures celebrating the Cavanaugh Flight Museum, its history and planes.

Given the walk path that leads from Hangar 1 to Hangar 2 (see Figure 14), and the proximity of the Canteen to Hangar 2 (Figure 14A and Figure 14B), it's highly likely James went into the Canteen. We know the kind of videos that were on display or available for viewing. It's plausible he saw portions of the aviation videos visitors could view there, including videos with WW2 scenes. And we can't plausibly rule out that he saw a video that showed and mentioned something as specific as the Corsair. Videos of that sort were available at the time and commonly sold and shown at aviation museums, and the Corsair had been a prominent plane on display at the Cavanaugh. It's also plausible, given the amount of time James spent in high-traffic areas of the museum, that he heard visitors, employees, and perhaps his own father, use words he associated with the items he saw. He did not need to have any ability to read to absorb and retain information conveyed visually and audibly.

We can't *prove* James saw all of the items above and heard salient information about all of them. But a potent skeptical rejoinder doesn't need to prove this. The skeptical argument here is an inference from what we know. We know the general layout of the museum, and we

Figure 7. Toy airplanes in museum gift shop. Photo credit: Cavanaugh Flight Museum

Figure 8. Toy warbird planes in museum gift shop. Photo credit: Cavanaugh Flight Museum

Figure 9. Japanese Battle Flag (side view). Photo credit: Cavanaugh Flight Museum

Figure 10. Japanese Battle Flag (front view). Photo credit: Cavanaugh Flight Museum

Figure 11. P-51 Mustang at Cavanaugh Flight Museum. Photo credit: Cavanaugh Flight Museum

Figure 12. VF-17 Jolly Rogers flying Corsairs in Pacific Theater Battle (by Nicolas Trudgian). Photo credit: Cavanaugh Flight Museum

Figure 13. The Canteen. Photo credit: Cavanaugh Flight Museum

know what exhibits were on display and where they were located. We also know what kind of products were on display in the gift shop. And we know where James spent most of his time in the museum. So, it's probable that James saw most of the items if not all of the salient items and exhibits—for example, the specified hangar planes, toy/model Corsairs in the gift shop, and the Japanese battle flag located in between the gift shop and the gallery. He made at least two trips to the museum in 2000, each for a few hours. It's highly probable that he saw the items multiple times and for long periods of time, and in the company of other people.

Three things follow.

First, it's highly plausible that James's museum visits were the source of his ability to identify the Corsair plane, know a handful of warbird features, including their flying off boats, and to associate the Japanese with the symbol of the red sun. He would also be able to contextualize these facts in a wartime narrative, which included Corsairs in battle with the Japanese over islands.

Second, we have a very plausible, if not probable, ordinary

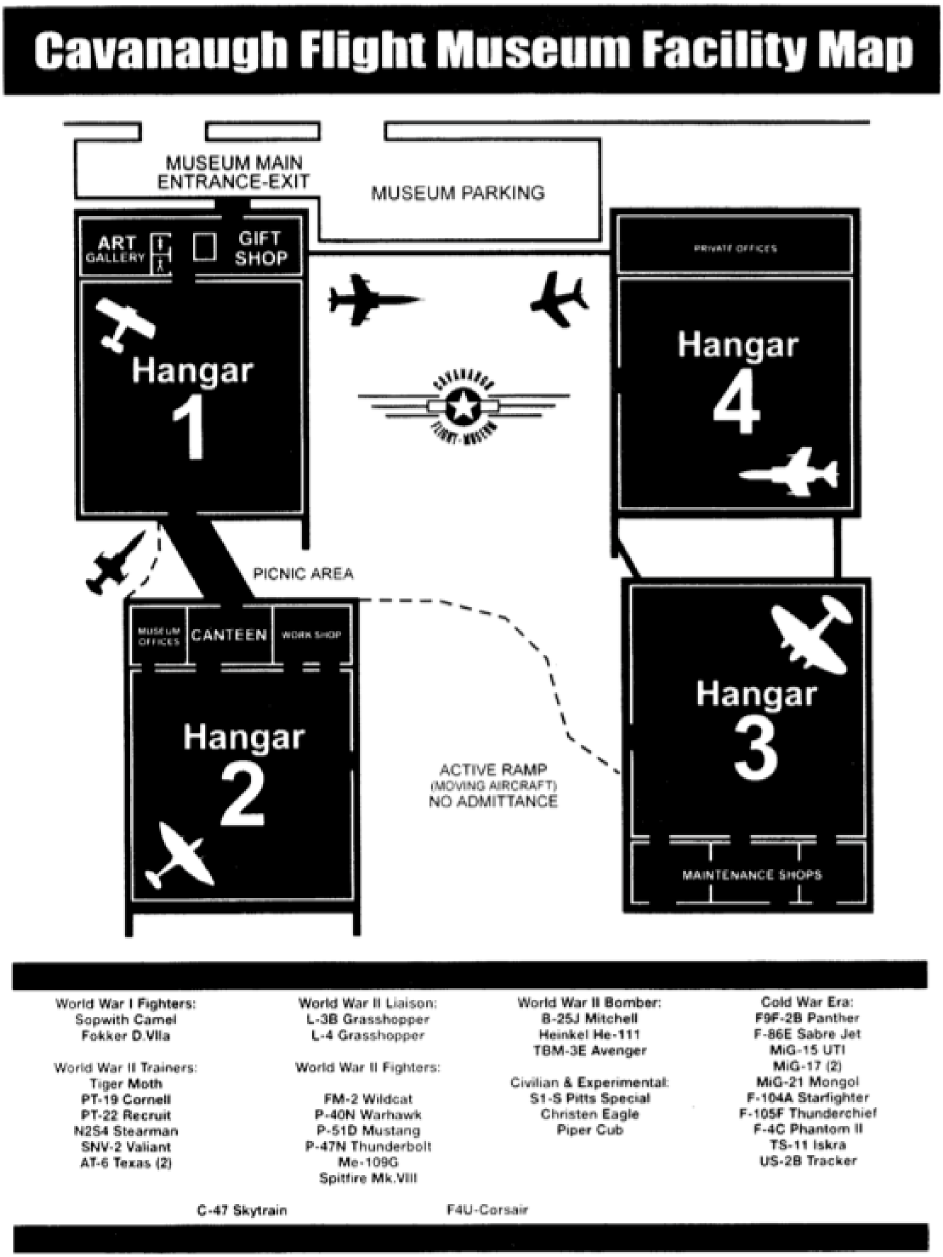

Figure 14. Cavanaugh Flight Museum Facility Map (Cavanaugh Flight Museum, 1999, inside front cover). Photo credit: Cavanaugh Flight Museum

Figure 14A. Hangar 1 map. Photo credit: Cavanaugh Flight Museum

Figure 14B. Hangar 2 map. Photo credit: Cavanaugh Flight Museum

explanation for items #4 and #6 in Tucker's list of early-bird items (Table 1). And Tucker has not plausibly ruled out the museum, together with the Blue Angels video, as a source for items #1, #2, #5, and #7.[18]

Finally, the Blue Angels video and Cavanaugh Flight Museum together provide a plausible, if not probable, source for nearly every item of knowledge the Leiningers attribute to James between March and September 2000 in their 2009 official account, as well as a plausible account of factors that contributed to the occurrence and content of his nightmares.

Based on the above, the skeptic is right to conclude that Tucker has not plausibly ruled out ordinary sources for the seven early-bird items he discusses, much less for the additional items the Leiningers attribute to James in their narrative as occurring between February and September 2000. Moreover, the skeptic is right to take the stronger position that ordinary sources do sufficiently explain the data in question, such that no reincarnation hypothesis need be invoked to account for the presumed facts in these early, significant stages of the Leininger chronology.[19]

Other Potential Sources 2000–2003

What ought to equally concern us are the sources James was exposed to but about which we have little or no knowledge. If the content of the Blue Angels video and the Cavanaugh museum flew under the radar of James's parents and Jim Tucker all these years, it's reasonable to think that more subtle sources of information might also have gone unnoticed. As the phenomenon of dark data in statistical reasoning reminds us, what we don't know matters.

For example, what was the content of the conversation with Charles Bond on the second visit to the Cavanaugh? What other comments or conversations did James pick up on in the context of immersing himself in WW2 planes and memorabilia for hours at the museum? Even the Leiningers acknowledge that at least one family member wondered whether James might have picked up something from the news that might explain his obsession with a crashing plane (SS, p. 57). Given that the national and international news had reported a dozen commercial plane crashes between February and July 2000, including the France Air Concorde crash that killed 105 onboard and Air Philippines in which 131 were killed, this concern is too easily dismissed in the Leiningers' own narrative.[20]

We should also consider that James's claims range over a period of several years, during which time we know he was exposed to an increasing number of sources related to aviation in general and WW2 aviation and warbirds in particular. Here are just some of the potentially significant sources.

> —Vintage Wings and Things, owned by David Jeansonne, was located in the Leiningers' hometown, in Carencro, LA, a suburb of Lafayette. This company had the same Corsair plane (N448AG) that in fall 2002 ended up in the Cavanaugh Flight Museum to replace their earlier Corsair that crashed in 1999. Jeansonne's Corsair was housed at the Lafayette Regional Airport until July 2000, and he would on various occasions roll the Corsair and his other vintage planes out for public display in Lafayette—for example, at the Sertoma Cajun Air Festival.[21] The Leiningers report that James went to this airport on multiple occasions while the plane was there (SS, pp. 37, 48), and we know James attended the Sertoma Cajun Air Festival (SS, p. 188).

> —James attended the Sertoma Cajun Air Show Festival, October 27–28, 2001 (SS, p. 188) held at Lafayette Regional Airport. At this festival, we're told James met with a few Blue Angels pilots. We don't know the content of these conversations or the extent of other conversations he had on this occasion or others, but these conversations would be potential sources of information he later exhibited, including his knowledge about WW2 and warbirds. The festival was a place where WW2 buffs would gather, chat about vintage planes, and display warbirds, so the scope of potential sources of information from conversations he overheard or participated in is quite large. The event also included flight simulators especially designed as a children's attraction.[22]

The Leiningers do not indicate how many times James went to the Sertoma Cajun Air Festival between 2000 and 2006, just as they don't indicate whether James went to the Cavanaugh Flight Museum more than twice. But there is evidence that 2001 was not the only time James attended the festival (Figure 15). John Fallis—owner and restorer of vintage planes in Lafayette—informed me that he met James at the festival when James was between six- and eight-years-old (so, between 2004 and 2006), and the two had a lengthy conversation about warbirds. Fallis had his rebuilt P-40 warbird on display at the time, and James seemed to know a lot about this plane, though he told Fallis the Corsair was his favorite plane.[23] Fallis said he remembers being impressed with James's technical knowledge of planes, which included how they were equipped with weapons. When Fallis asked James where he learned so much about vintage planes, James said, "Just reading." This was one of several occasions Fallis and James Leininger hung out and discussed vintage planes.

Although Fallis's conversations with James took place several years after the initial events in the Leininger chronology, quite possibly a year or so after the 2004 *Primetime* program, it's instructive in at least three ways. First, it shows that James attended the Sertoma Air Show more than once between 2000 and 2006, not surprising given his interest in warbirds. Second, it illustrates the kinds of conversations James was accustomed to having with veterans and warbird experts when he met them at public events, including this particular local festival. Finally, Fallis said that James's knowledge of vintage planes, at least by the

Figure 15. Poster—Blue Angels at the Sertoma Cajun Air Festival, October 27–28, 2001. Photo credit: Leon Labbe

time he met him, may very well have been informed by James reading *Walk Around* (Squadron/Signal Publications), a 1990s book series which featured technical details of warbirds, and which were sold at the Cavanaugh Flight Museum.[24]

—James viewed various WW2 related films over the years, some focused on specific fighter planes, including the 2002 A&E documentary *Battle Stations: Corsair Pacific Warrior*. This particular film—to be discussed in Section 4—first aired in 2002 and covered details on the landing issues with Corsairs, archival footage of carrier landings and air combat with heavy flack, how Corsairs were used as napalm bombers, and specific lines James later repeated. It was preceded by an episode on the war in the Pacific. The Discovery channel had an ongoing series on warbirds beginning in the 1990s and which featured an episode on the Corsair. Additionally, many Blue Angels videos produced in the 1990s and which were available for viewing and purchase at aviation museums contained WW2 information with archival footage.

—Other museum visits: In spring 2002 James visited the Lonestar Flight Museum for the filming of the 2002 ABC program. James attended another aviation museum in 2004 which had WW2 artifacts (Tucker, 2013, p. 82); this appears to have been the Nimitz Museum in Texas (SS, photos, p. 14). In summer 2002, the Leiningers vacationed in Hawaii (SS, p. 174) and stayed near Pearl Harbor. We don't know whether James visited any war museums or memorials during this trip. But given the extent to which his parents accommodated his interests in WW2, it would be surprising if he had no exposure to WW2 memorials and exhibits, which raises the reasonable question as to what he would've been exposed to in such situations.

When we look at these sources, we can't rule them out as plausible sources of information which shaped the Leininger narrative. One of the difficulties in this case is that we can't confidently distinguish between sources that confirm things James knew and sources that supplied James with information. Why? Because we have a broad range of known and unknown sources of information, claims evolving over several years, and—to be shown below—a deeply flawed chronology of events with significant gaps that make the narrative vulnerable to significant psychological confabulation. There is also a significant time gap in the Leiningers' chronology, roughly between early 2001 and spring 2002. The Leiningers say very little about what happened during this period, much less what James was exposed to which could have shaped his perspective. The Leiningers give every indication of supporting James's interest in airplanes and WW2, so it's unreasonable to think they changed their behavior and suddenly isolated him from salient sources like the ones outlined above. Regrettably, Bruce Leininger has repeatedly declined to answer my queries about the extent and dates of James's visits to various aviation museums and demonstrations.

If a few discovered sources, which Tucker and other researchers previously failed to acknowledge, change the face of the case in the ways I've shown, the possibility of further dark data, perhaps altogether inaccessible to us at this point, is a real concern. The likelihood of such untapped sources should not be ignored in the assessment of this case. What we don't know matters.

Finally, it's important to consider how various environmental factors the Leiningers mention in passing potentially illuminate James's wider psychological landscape. The family move in late February/early March of 2000 was stressful, and even Andrea Leininger's mother briefly considered that it played some role in James's nightmares (SS, p. 35). James also took trips to the Lafayette Regional Airport, usually to see his dad off on business trips every other week; this was also the occasion for James having outbursts about his father's plane crashing and bursting into flames (SS, pp. 36–38, 47–50, 76). James's nightmares intensified and he began verbalizing their content a week after visiting the Cajun Heartland Fair where he experienced aviation rides (SS, pp. 32–33). Tucker also doesn't pay much attention to any of these issues when considering psychological explanations. Even if he has dismissed them in his own mind, not addressing them gives the impression that they have been overlooked or dismissed without good reason.

Earlier I said that the presumed facts of the Leininger case are evidence for reincarnation only if:

(N) There are no additional facts that neutralize the evidential force of the presumed facts.

In this section I have argued that (N) is *not* the case. Tucker has excluded or not acknowledged significant facts that severally and jointly raise the plausibility that the factual scaffolding of the Leininger case has ordinary sources in James's wider experience. These facts provide straightforward, ordinary explanations for nearly every item in Tucker's early-bird table, and there is no item in that table that Tucker can claim to have plausibly ruled out as derived from the sources outlined above. I've also assumed Ian Stevenson's criterion of explanatory salience when dealing with potential ordinary sources of information.

Moreover, since these more obvious sources of information flew under the radar of the Leiningers and Tucker, we have reason to suppose that other sources shaped the evolution of the case in subsequent months and years, especially since James's subsequent experiences involve exposure to an increasing number of sources. Consequently, the presumed facts of the case cannot have the kind of evidential force Tucker attributes to them. Once we adjust the narrative of facts to include the material in this section, we have compelling evidence—not for reincarnation—but for how facts could give the misleading appearance of being evidence for reincarnation by excluding facts that point directly to ordinary explanations of seemingly extraordinary events.

4. THE RELIABILITY OF THE LEININGERS' TESTIMONY

The initial impressiveness of the Leininger case depends as much on *including* a whole range of presumed facts as it does on *excluding* facts concerning James's exposure to ordinary sources of information.

Earlier I said, the facts in this case are evidence for reincarnation only if:

(D) There are no significant grounds for doubting the presumed facts.

The presumed facts in the Leininger case include what James said, when he said it, descriptions of his behavior, and a variety of contextual details, including his parents's judgments about whether ordinary sources of information could have influenced his claims and behavior. There are also presumed facts concerning documents the Leiningers have presented, not simply facts about what the documents state but facts about what the Leiningers say about how and when they acquired the documents. All these facts are baked into Tucker's account and analysis of the case. But nearly all these facts are dependent on the testimony of Bruce and Andrea Leininger—two witnesses. Therefore, the presumed facts of this case have only as much credibility as the Leiningers' testimony has. And since Tucker's account and analysis is essentially tied to the testimony of the Leiningers, his favorable assessment of the case depends on the Leiningers being reliable witnesses, including reliable in their judgments about the plausibility of ordinary experiences supplying James with information.[25]

It's a basic principle of testimony that what people report about their experience is, all other things being equal, probably as they report them. So, it's entirely reasonable to accept what people tell us about their experience, unless we have overriding reasons to think their claims are false or their testimony unreliable—for example, due to dishonesty, asserting falsehoods, significant inconsistency, confabulation, or malobservation. In other words, we should trust people's claims about their experiences until we have reason not to do so. Testimony is *prima facie* reasonable, until other considerations defeat or override it.

In the case of the Leiningers, we have several different overriding reasons not to trust their testimony concerning the narrative of events and their interpretation of these events, including their repeated assurance that there is no possible way James could have known what he knew through any ordinary source. They are not reliable in these crucial ways and therefore not credible.

Poor Judges of Plausible Ordinary Sources

One area where the Leiningers lack reliability is in their judgment about the kinds of things James was exposed to at different points in the chronology of events and which plausibly shaped his experiences, claims, and behavior.

In Section 3 I gave six examples of James's aviation claims and behavior which the Leiningers thought were unusual, but each item is found on the Blue Angels video he regularly watched. The same video also contained imagery of a fighter plane being shot in combat and bursting into flames, as well as images of bombs dropping on land targets. Yet, the Leiningers are confident that they have ruled out this kind of exposure. Referring to the disturbing images James was experiencing in his dreams, Andrea said, "I kept thinking where is he getting this? What's he watching on television? But I was a stay-at-home mom, so I know there wasn't anything that he was exposed to."[26]

In Section 3 I also examined six WW2-specific claims the Leiningers attribute to James in the early months of the case and which they regard as inexplicable. I showed that all six claims involve information James would've been exposed to at the Cavanaugh Flight Museum. Yet, with reference to these claims, in the 2004 ABC *Primetime* segment, Andrea Leininger said, "I knew what he watched on television. I knew what stories I read to him. I'm a protective, first-time southern mother. There's no other place he could have been getting this information."[27]

Consider also how baffled the Leiningers were regarding some of James's crucial claims. When James identified a drop tank in March 2000, Andrea asked Bruce, "How would he know that?" adding that "he can't even say 'drop tank,'" and Bruce could only shake his head (SS, p. 17). Later when James said the Japanese shot down his plane, Andrea asked, "How did he know about the Japanese?" and Bruce replied, "I don't know. How the hell did he know about a drop tank?" (SS, p. 68). The next day James explains that he knew the Japanese shot him down because he could identify "the big red sun," a reference no less puzzling to James's family (SS, p. 70). They express similar amazement over James knowing the word *Corsair* and giving it as the name of his plane (SS, p. 79).

More recently, Bruce Leininger has said:

> He knew the plane [Corsair]. How could James know the name of a World War II fighter aircraft, much less with certainty that it was the aircraft in the dream? And how the hell did James know they were launched from aircraft carriers? Nothing that he had ever seen or read or heard could have influenced him to have this memory. (Kean, 2017, p. 21)

Given everything that was available to James at the Cavanaugh and on the Blue Angels video, the claims the Leiningers discuss above are not the least bit surprising. What *is* surprising is that the Leiningers found them surprising. Either they were unaware of what James was exposed to or they egregiously misjudged the relevance of such exposures. Yet, they're adamant—*Nothing . . . could have influenced him*. Their confidence is excessive and their language grandiose. We should be deeply suspicious of people who claim to know with certainty what they can't possibly know.

It's not as if the Leiningers haven't on occasion addressed the possibility that James's visits to the Cavanaugh influenced him. After acknowledging his visits to the Cavanaugh museum with his father, Andrea Leininger wrote:

> The flight museum has refurbished planes in it from WWI, WWII, plus the Korean War, Vietnam, and then more modern military aircraft. There was no Corsair on exhibit at the time, and there were no videos of burning or crashing planes. Just large hangers with the aircraft from each era sitting out on display. It was about 4 months after the trip to the museum that James had his first nightmare. When the whole night terror thing started, we briefly considered the trip to the museum as a cause, but since he had been just a very young toddler, so much time had passed (four months is a long time in toddler years!) and none of what he was telling us could be traced back to that museum visit, we eliminated it as a possibility.[28]

Andrea Leininger provides two reasons for eliminating the museum as a possible influence on James's experience and source of the information he later conveyed. *None of what he was telling us could be traced back to that museum visit,* and *so much time had passed between the visits and the beginning of the nightmares*. The first premise is demonstrably false—nearly everything James said in the first several months can be easily traced to the Cavanaugh museum and/or the Blue Angels video. The second premise is irrelevant. There are many ways dreams, including nightmares, can incorporate material from the waking-state, and they can do so over varying periods of time. Moreover, in James's case, his initial nightmares came around the time toddlers first begin having nightmares, namely around age two. There's nothing unlikely about his first museum visit shaping his initial nightmares, especially since he was repeatedly viewing a video of a plane bursting into flames more proximate to the emergence of his nightmares.[29]

But what's most striking is what Andrea Leininger does *not* say. She says nothing about the Corsair planes in the gift shop, the Japanese battle flag with the big red sun, the drop tanks on display (on planes and in the artifacts hangar), the battle scene images in the gallery, or the Canteen where aviation and WW2 videos were viewable. She says nothing about the artwork which shows Corsairs flying over islands and engaged in battle scenes in the Pacific theater. The Cavanaugh was clearly not *just* large hangars with aircrafts on display. She assumes without justification that there were no videos of burning or crashing planes. Like Tucker, she incorrectly assumes that the only way James could have picked up the word *Corsair* was if there was an actual Corsair at the museum. And she is silent about the Blue Angels video purchased at the museum and which we know contained images of a plane on fire after being shot in combat.

The Leiningers have provided further examples of claims they say have "dumbfounded" them. One particularly important example is an incident Bruce has regularly cited since 2009, though it was not included in *Soul Survivor*. On Bruce's birthday when James was five-years old — October 7, 2003—James reportedly said to his father, "Every day is like a carrier landing. If you walk away, you're okay."[30] This particular anecdote has gained prominence in the retelling of their story over the years. Most recently, Bruce presented this incident as important evidence in his 2021 prize-winning essay for the Bigelow Institute of Consciousness Studies. Bruce wrote: "7 October 2003—Out for a walk on my birthday with James I was talking about the tough day I'd had. James said 'Dad, every day is like a carrier landing. If you walk away from it you are OK!' Again, this was not a 5-year-old child speaking; it was an older soul" (Leininger, 2021, p. 16).

In the 2021 documentary *The Great Beyond Revealed*, Bruce said the following about this incident:

> He knew things or was able to convey information that just dumbfounded us. One night I'd come home from a consulting day that was rather harsh. And we went for a walk around the block . . . He looked at me, and I was kind of commiserating with a three-year old, and he said "Well, Dad, every day is like a carrier landing, if you can walk away from it, you're okay." Huh? You have a three-year grandson or child, is that what they tell you? (Honeywell et al., 2020, 0:18:13–0:18:56)

As the Leiningers exhibit elsewhere, they have a very loose connection with facts in the retelling of their story over the years. Despite how impressive this incident was in Bruce Leininger's mind, he can't decide whether it took place when James was three or five years old—the version of the story in the 2021 documentary is inconsistent with the version in Bruce's 2021 BICS paper.[31] The more serious problem, though, is that James's philosophical insight has a fairly obvious ordinary source which didn't show up on Bruce Leininger's radar. James's statement is found in a 2002 A&E documentary on the Corsair plane. In *Battle Stations: Corsair Pacific Warrior*, WW2 Corsair pilot Colonel Archie Donahue said, "Each day in life is like a carrier landing. If you can walk away from it, you're in good shape."[32] The A&E documentary focused on the role of the Corsair in the war in the Pacific, and Donahue made his statement while discussing the difficulties of carrier landings.

We know James saw the Corsair documentary. It's the "History Channel program about Corsairs" the Leiningers admit James watched on a video tape (SS, p. 268). The Leiningers don't give the title of the video, but since they describe a scene uniquely characteristic of the program, there's no doubt they're referring to the *Battle Stations* Corsair episode. Although they're not clear about when James watched the tape of the program, we can establish a timeframe. The documentary first aired on December 26, 2002 on the History Channel network.[33] Since James made the philosophical statement to his father on October 7, 2003, he had nine months to acquire a tape of the program and watch it repeatedly and absorb its content, even if he didn't see it when it originally aired.[34] Yet, Bruce Leininger doesn't as much as even acknowledge that the statement he invests with such evidential value appears on the video.

Other examples could be provided, but these are more than sufficient to raise a tough and serious question: If the Leiningers have failed to notice or acknowledge just how many of James's claims and behaviors came from just two video sources and the Cavanaugh Flight Museum, are they in any position to give a reasonable assurance that *any* claim they have attributed to James doesn't derive from something he saw or heard somewhere? I think the answer here must be *no.*

Distorting Facts Concerning the Death of James Huston, Jr.

Another vital tier of the Leiningers' story is their attempt to show that the facts concerning Huston's death match what their son had to say about Huston's crash—that is, what James said about the crash he was experiencing in his dreams and had apparent waking state recollections of. But their attempt to verify James's claims involves a distortion of the facts surrounding Huston's death.[35]

In their effort to get at the facts surrounding Huston's death, the Leiningers rely on primary documents (composed in 1945 shortly after Huston's death), secondary source documents in the form of veteran recollections and narratives composed decades after the war, and interviews Bruce Leininger conducted with several WW2 veterans who participated in the mission in which Huston was killed. However, Bruce Leininger's handling of these sources produces a highly misleading narrative of Huston's death.

The U.S.S. Natoma Bay Aircraft Action Report (hereafter, AAR) for March 3, 1945, provides a very detailed description of the circumstances of Huston's crash. The Leiningers refer and allude to this document (SS, pp. 175–76, cf. 239, 242), and they reproduce a diagram of the aircraft strike on Chichi Jima contained within the document (SS, p. 12, photos section). But they never quote directly from the AAR. On the face of it, this is odd. The AAR provides a detailed description of Huston's crash and was composed proximate to the actual event. And it's the source for material in other documents on which the Leiningers rely. Since the Leiningers spend at least a third of their book trying to reconstruct the circumstances of Huston's death, it's surprising that the content of this document wasn't included in their book, except when parts of the AAR's account appear in other documents.

Return briefly to the Leiningers' narrative of Huston's death based on the claims they attribute to their son. Huston's plane was supposedly hit in the engine and immediately burst into flames. Huston was trapped inside the burning plane and trying to extricate himself from the cockpit. Then it crashed in the water at Iwo Jima. Little James reportedly said, Huston was still alive both before impacting the water and after, struggling to extricate himself from the plane. According to Andrea Leininger, "After his plane was hit in the engine, it crashed nose first into the water. From what my little James told me after his nightmares, he was alive in the plane when it went into the water, and was kicking to try and break out the canopy to escape the sinking plane."[36] The book omits this detail and focuses instead on the plane bursting into flames after being hit in the engine and James apparently being trapped in a burning plane quickly descending toward the water, but where it's at least unclear is whether his struggle to extricate himself also took place after the plane hit the water.[37]

The Leiningers claim that the after-action reports are "muddled and confused" (SS, p. 239), but they never show this. Their criticism looks like a subtle concession to the lack of confirmation primary source documents provide for some of the claims they attribute to James. But their dissatisfaction with the after-action reports is their stated reason for seeking out alternative sources of information about Huston's crash, sources that as it happens turn out to be more amiable to confirming James's airplane-crash narrative.

Ultimately, Bruce Leininger relies on the testimony of four veterans from a different carrier who were part of the Chichi Jima mission in which Huston died. The men were members of the VC-83 squadron on the *U.S.S Sargent Bay* and who flew the TBM Avenger bombers in the strike on Chichi Jima. Bruce Leininger interviewed John Richardson, Bob Skelton, Ralph Clarbour, and Jack Durham. Durham said the plane "took a direct hit on the nose" and he could see "pieces falling into the bay" (SS, p. 241). Richardson allegedly said Huston's plane was "hit in the engine by what seemed to be a fairly large shell" and "there was an instantaneous flash of flames that engulfed the plane" (SS, p. 244). Skelton is reported as saying he saw Huston's hit and the plane "blew up" (SS, p. 249). Clarbour said he saw Huston's plane hit "right in the engine" and there was an "instantaneous flash of fire" before the plane dove and crashed into the harbor (SS, p. 250).

There's something odd about this from the vantage point of the Leiningers' own approach to documentation. In trying to establish the details surrounding Huston's death, the Leiningers indicate that they "couldn't rely on anything that had the informal taint of old memories," and so they marginalize the unofficial log Natoma Bay veterans compiled in the 1980s—the so-called *Blue Book*—and privilege the official government documents typed in 1945 "right after the battle and the debriefing, when everyone's memory was fresh" (SS, p. 166). So, the Leiningers felt they could not rely on the recollections of the Natoma Bay veterans in the *Blue Book* composed three to four decades after the war, but they could rely on the recollections of a few elderly veterans from a different squadron on a different carrier six decades after the war. The testimony of the Avenger pilots does confirm certain details of James Leininger's crash narrative. But the Leiningers give a decent reason for privileging primary source documents over veteran recollections. In the light of the Leiningers' reluctance to trust the "informal taint of old memories," it's hard to see why they would think they *could* rely on the memories of Avenger pilots nearly sixty years after the war, except that their testimony confirmed aspects of James's airplane-crash narrative which the primary source documents could not confirm.

The main problem, though, isn't the Leiningers' apparent methodological inconsistency. It's that the AAR contradicts important aspects of their narrative, and the Leiningers have conveniently omitted these details. Whether deliberate or not, ignoring the AAR allows the Leiningers to bury important disconfirmations of claims they attribute to their son.

The AAR indicates that on March 3, 1945, eight pilots from the Natoma Bay (flying FM-2 Wildcat fighter planes), together with eight TBM Avenger bombers from the *U.S.S Sargent Bay*, attacked the harbor at Chichi Jima, about 160 miles north of Iwo Jima. The FM-2 pilots were the escort for the TBM bombers, which had a three-person crew in each bomber: pilot, turret gunner, and radioman/bombardier/ventral gunner.

Here is the relevant summary of the circumstances of Huston's death provided in Section XII of the document. I quote the material in full.

> All aircraft recovered to the west toward the entrance of the harbor. Heavy anti-aircraft took them under fire from each side of the harbor. It was thought to be 3 inch batteries. Lt (jg) J.M. HUSTON was apparently hit by this fire as he approached the harbor entrance. *None of the other pilots saw a hit and his airplane was not on fire*, but it suddenly nosed over into a 45 degree glide crashing into the water, exploding and burning. At the time the plane nosed over, it was at about 1500 feet altitude and was estimated to have crashed while

> making about 175 knots. There was no wreckage left afloat and only a greenish yellow spot on the water marked the crash. There was no evidence of a survivor and *it is believed that it would have been impossible to survive the crash and resulting explosion*. (Position of crash is indicated on diagram). The position of the crash is in enemy territory but possibilities of compromise of classified material is considered improbable. He may have been hit and killed by light fire as *no one reported observing any damage to the plane*; most of the bursts in the vicinity where he crashed were from 3 inch (or equivalent) guns. (Italics are mine).[38]

The AAR does not confirm details the Leiningers wish to confirm. More importantly, it actually disconfirms crucial parts of their narrative. It thereby also raises considerable doubt about the reliability of the testimony Bruce Leininger attributed to the four TBM Avenger pilots he interviewed nearly six decades after the incident.

First, the report states that witnesses saw Huston's plane suddenly nose over and go into a 45-degree glide and crash into the ocean. It exploded and burned on impact. Who saw this? The Leiningers would have us believe that only the Avenger bombers Bruce Leininger interviewed witnessed Huston's death (SS, p. 242). But this is poorly reasoned and patently false. Bruce only interviewed a small fraction of the over thirty men who participated in the mission; he can't rule out that individuals he didn't interview were the source of the information in the AAR, including members of Huston's VC-81 squadron. Eight FM-2 Wildcat pilots flew off the Natoma Bay on the mission in which Huston died. Seven returned. Of those seven, Bruce Leininger apparently interviewed only two of them—Jack Larsen and Bob Greenwalt. Larsen didn't see what happened to Huston, and the Leiningers say nothing about what Greenwalt saw or didn't see, which means it's doubtful he could confirm the details Bruce Leininger was looking to confirm.

It's not difficult to figure out the source of the information in the AAR. Since the Natoma Bay AAR would've been filled out by the Intel officer for the VC-81 aboard the Natoma Bay, either someone in the VC-81 *saw* what is reported in the AAR or one or more members of the VC-81 *heard* radio communications from the TBM Avenger pilots of the VC-83 describing what happened to Huston.[39] But if the latter, it certainly wasn't the Avenger pilots Bruce Leininger interviewed, unless they changed their story after 1945, because the AAR contradicts what they had say about Huston's crash. The report is explicit: *the plane was not on fire* and *no one reported observing any damage to the plane.* If the veterans Bruce interviewed were the source of the information in the AAR, that report should indicate that Huston's plane was damaged by a direct hit to the engine and propeller and the plane burst into flames. But the AAR says the opposite.[40]

Second, although no one witnessed the plane being *hit*—not the same as no witnesses to what happened—the obvious inference from the circumstances of the mission is that Huston was hit by anti-aircraft fire. But the report leaves open whether some part of the plane was disabled by anti-aircraft fire or whether it was Huston himself who was hit by artillery that penetrated the cockpit and killed him instantly. If the latter, it would have been impossible for Huston to have been kicking and otherwise trying to extricate himself from the plane, even before his plane hit the water. If the former and he was still alive, he had at the most between 10 and 20 seconds to try to release the canopy before the plane hit the water. While the report is consistent with Huston being alive and struggling to bail out, it's also consistent with anti-aircraft fire instantly killing him. It also doesn't indicate that either of these two scenarios is more likely than the other. Thus, the AAR cannot be invoked to confirm this part of the Leininger narrative.[41]

Third, the report further conveys the belief that it would have been impossible for anyone to have survived the crash and resulting explosion. Sensible enough given Huston's speed on impact—it would have been like slamming into a brick wall at 200 miles per hour. Not surprisingly, no wreckage was observed, only a greenish yellow spot on the water where the plane hit. The discoloration on the water surface was likely sea dye marker used to help rescuers locate the crew of downed planes. The dye would've been in the life raft. In the FM-2 Huston was flying, the raft would've been stored in a compartment behind the pilot's head and accessible from a hatch outside the plane. The flotation vest Huston would've been wearing likely had the dye as well. The discoloration on the surface of the water, appearing so quickly, implies that the impact violently and suddenly breeched the cockpit. The crash was catastrophic. Even if Huston was alive before the plane crashed—and that's not more probable than not—the impact killed him. He did not die by drowning. So, the AAR disconfirms a crucial component of the Leininger narrative.

The AAR confirms at the most *four* of the claims attributed to James concerning Huston's death: Huston flew off a boat named Natoma Bay, he was shot by the Japanese, his plane crashed in the water, and he had some association with a person named Jack Larsen. It disconfirms *three* fairly specific claims: the plane was on fire (prior to hitting the water), Huston was alive after the plane hit the water and struggling to get out of a sinking plane, and the plane crashed at Iwo Jima. And *two* claims are at best neither confirmed nor disconfirmed: The plane was hit in the engine/propeller, and Huston was alive after the hit and struggling to get out. So, of the nine claims attributed to James about the circumstances of Huston's death, only four are confirmed, and only one of these—the Jack Larsen claim—is idiosyncratically true of Huston.[42]

The Leiningers buried the details of the AAR. Instead, they quote from other primary and secondary documents (for example, the VC-81 War Diary) which *exclude* the claims that contradict their narrative—the plane was not on fire, no damage was observed, it was possible that the plane crashed because anti-aircraft fire killed Huston in flight, and it was not possible for Huston to have survived the crash. They also focus on the testimony of four pilots from a different carrier who participated in the mission and whose account—nearly sixty years after the fact—*happens* to fit James's description of events. This from Bruce Leininger who "felt he couldn't rely on anything that had the informal taint of old memories" (SS, p. 166).

We should ask ourselves, what's more probable? That the primary source documents, composed within days of the incident, are incorrect about crucial details, including those that would be essential to any decision to attempt a rescue of Huston, or four elderly veterans misremembered some of the details of one sortie they flew on one particular day in a war nearly six decades in the past? Of course, even if we had an independent reason to trust the reliability of the Avenger pilots so long after the event, we would still have conflicting testimony. And when the best sources don't agree, we must acknowledge we don't know what happened. And if we don't know what happened, there is no known fact to serve as a confirmation of James's claims.[43]

5. NARRATIVE REDACTION

Another set of closely allied problems that impugn the credibility of the Leiningers concerns narrative redaction. The Leiningers have presented various iterations of their story since 2002. Over the years, they have changed important details concerning what James said and when he said it, either omitting or adjusting the content of his claims or the dates on which he allegedly made the claim. Consequently, there isn't *one* James Leininger story. There are many versions of the story. Since they are inconsistent with each other, they can't all be true. The Leiningers' propensity toward narrative redaction further impugns their reliability—in the telling of any story, you need to consistently get the important stuff right.

Of course, not all inconsistencies are equally egregious. So, let me be clear at the outset that the inconsistencies I'll canvass below are not innocuous or benign. Far from it. First, they suggest that the Leiningers are not sure about what James said and when he said it, especially with respect to some of the most important claims they attribute to him. At best we have no way to adjudicate these multiple and incompatible iterations of their story. Second, most of the redactions—for instance, as they appear in their 2009 book *Soul Survivor*—were made after the Leiningers had done considerable research and had a robust knowledge of James Huston. It's concerning that the Leiningers made changes to their story in the light of facts they later discovered. And it's suspicious that the changes were fortuitous—they resulted in making James's claims better fit the historical record concerning Huston's life and the circumstances of his death. Such conceptual gerrymandering easily produces a deceptive appearance of impressiveness.

Three Forms of Narrative Redaction

The official Leininger narrative involves three kinds of redactions: (a) chronological redactions—changes to the dates when James allegedly said certain things, (b) claim omissions—the official account in *Soul Survivor* excluding claims the Leiningers have elsewhere attributed to James, and (c) claim adjustments—the official 2009 account in *Soul Survivor* altering what James reportedly said or the Leiningers adjusting their interpretation of claims subsequent to the publication of their book.

A significant illustration of b-type redaction includes Andrea Leininger's claim in July 2005 (above) that James told her Huston didn't die from the crash but died by drowning after he failed to extricate himself from his sinking plane. It's antecedently unlikely that Huston survived the crash, and the AAR sensibly claims it was believed to have been impossible. So, if we trust the AAR, that testimony counts against the veracity of James's claims here. And if we doubt the accuracy of the AAR document, why appeal to it at all to support other claims? But the Leiningers' official version of the story omits this highly detailed version of James's description of the crash. In this way, a clearly disconfirmed claim is omitted from the official narrative.

There are two very significant redactions of the c-type, one concerns what James said about the Corsair and the other to what he said about Iwo Jima.

In their book, the Leiningers reproduce a conversation they allegedly had with James surrounding the content of his nightmares. Since he had been describing a "little man" being in a plane crash, they asked him what kind of plane the little man flew. James said a Corsair, and he indicated that he flew it off a boat, which a few sentences later he identifies as the Natoma (SS, p. 78–80). The context strongly suggests that James is the little man and he was telling his parents that the plane he crashed and died in was a Corsair, and that he had flown this plane off a boat called Natoma. The Leiningers interpreted James to mean exactly this for at least two years (SS, pp. 113, 144, 152, 224). Tucker also acknowledges this as the suggested narrative in the 2002 ABC program.[44] But subsequent to the publication of their book, the Leiningers have said that James never explicitly *said* his Corsair took off from the Natoma or that he was shot down while flying a Corsair (B. Leininger, 2009).

This is quite misleading. While it may be true that James never explicitly said he flew a Corsair off the Natoma Bay and died while flying one, what matters is what James *meant*.[45] But whether we are talking about children or adults, we rely on a person's explicit words and what they imply given contextual details. Which is exactly what the Leiningers rightly did until it became inconvenient to their evolving narrative. We may never know what James said or meant, but we can't ignore the context in which he made the claims attributed to him. In the portion of dialogue between James and his parents reproduced in their book, it's contextually clear that the Leiningers were asking James about the plane he was flying in his nightmares and in which he crashed. His answers to their questions are naturally interpreted to mean that he was flying a Corsair when he crashed and that he flew the Corsair off the boat he called the Natoma. And Bruce and Andrea Leininger thought this is what he meant for at least two to two-and-half years.[46]

As far as I can see, the only relevant fact that guided the Leiningers' reinterpretation of James's claims is their later learning that Huston did not crash in a Corsair but that he did test fly a Corsair before joining the Natoma Bay (SS, p. 226, 269–270). The more general claim—Huston flew a Corsair—can be made to fit the facts concerning Huston's life. The more specific claims—flew a Corsair off the Natoma and crashed and died flying a Corsair—do not fit the facts. But, given the absence of any independent reason to prefer interpreting James as intending one thing as opposed to the other, it looks as if subsequently discovered facts motivated the reinterpretation of the earlier claims with the result that it created a better fit with the facts. The facts thereby confirm rather than disconfirm the reincarnation narrative the Leiningers wish to perpetuate. In a game of darts, this would be similar to painting the target around the spot where the dart has landed so that the dart is located in the bullseye, then claiming to have hit the bullseye. No one can deny that the dart fits perfectly in that space, but how it ended up in that spot matters.

A more important redaction concerns James's important Iwo Jima claim. Prior to the publication of their book and as early as 2002, the Leiningers state that in late November 2000, James pointed to an ariel view of Mount Suribachi at Iwo Jima and said, "Daddy, that's where my plane was shot down."[47] The image James pointed to was in a book on the Battle of Iwo Jima (Wright, 1999, p. 3), which Bruce had recently purchased for his father. However, in *Soul Survivor*, we get a different version of this story. The Leiningers alter the claim they attribute to James. James now allegedly said, "Daddy, that's *when* my plane was shot down" (SS, p. 104). Bruce Leininger and Jim Tucker interpret this to mean his plane was shot down during 'Iwo Jima operation', not at the island of Iwo Jima (Tucker, 2016, p. 204, Table 1). Tucker attributes the change to Bruce correcting a decade-long incorrect memory about what James said (Tucker, 2013, p. 75).

While the redacted statement attributed to James is vague, at least the historical record doesn't clearly refute it. But that advantage comes at a cost. Misremembering for nearly ten years what James allegedly said on a vital point in the story doesn't induce confidence in the reliability of one's testimony, especially since the Leiningers regard the Iwo Jima claim as one of the more significant claims they attribute to James (SS, p. 104; cf. B. Leininger, 2021, p. 10). In early versions of the story, James even clarifies his comment at Bruce's request and says, "My plane was shot down *there*, Daddy."[48] Also, Bruce initially discounted Huston as the pilot in his son's narrative because the facts he discovered in September 2002 didn't fit James's claims. For example, Bruce reasoned, "Huston wasn't even killed at Iwo Jima. He was killed on a mission a couple of hundred miles away, at a place called Chichi-Jima" (SS, p. 141)—a strange observation if James never said that the *location* of his crash was Iwo Jima. Most importantly, though, the change to what James said about Iwo Jima comes only after Bruce Leininger learned that Huston's plane crashed at Chichi Jima, not at Iwo Jima, but that he nonetheless crashed and died during the Battle of Iwo Jima. Conveniently, the redaction results in attributing to James a claim the facts don't obviously disconfirm.

Additional evidence for chronological redaction comes from comparing Carol Bowman's version of the Leininger story and the Leiningers' version of their story.

According to *Soul Survivor*, Bowman didn't get involved in the case until winter 2001, at which time Andrea wrote her and they had correspondence about James's nightmares (SS, pp. xii, 116–117). After March 2001, they didn't have further correspondence or discussions until March 2002 (SS, pp. xiii, 117, 121–125). At that time, Bowman reached out to Andrea regarding the prospects for doing the 2002 ABC TV show. In her "Forward" to the Leiningers' book, Bowman says that when she reconnected with Andrea Leininger in spring 2002 for the first time in about a year, James had provided *new details* since their previous interactions. James had "remembered the type of plane he flew [Corsair], the name of the aircraft carrier [Natoma], and the name of one of his pilot friends [Jack Larsen]" (SS, p. xiv). Elsewhere Bowman adds the Iwo Jima claim as occurring after their 2001 correspondence (Bowman, p. 2010, p. 55).[49]

The problem here should be transparent. If—as the Leiningers say—Carol Bowman had her initial correspondence with Andrea Leininger in winter 2001, then one of two things must be true: either the items Bowman lists as new details were not new or those items must have happened between winter 2001 and spring 2002, not between August and November 2000 as the Leiningers claim in their book. Since Bowman lists the new revelations from James as occurring between 2001 and 2002 in her "Foreword" to the Leiningers book, the inconsistency is internal to *Soul Survivor*. Something isn't right about the Leiningers' story at this point.[50]

The Leiningers' inconsistency concerning when Bowman got involved in the case further obfuscates the matter. In their book, the Leiningers say winter 2001, and Bowman agrees.[51] But in *the Great Beyond Revealed* documentary, Bruce Leininger indicates that Bowman first got involved in August 2000, at least a month before James gave the name Jack Larsen.[52] It's unclear why Bruce Leininger would present a narrative in which Bowman enters the picture during summer 2000, given that he had written a book in which he says she first appeared nine months after that date.

The 2003 Chronology of Events

Further evidence of large-scale redaction in the Leininger narrative comes from a detailed, four-page document I acquired from Lucinda DeWitt, the daughter of Natoma Bay Association historian John DeWitt. John DeWitt was a key player in Bruce Leininger's attempt to acquire historical documentation to verify his son's claims, as he supplied Bruce with primary source documentation about the Natoma Bay and James Huston (SS, pp. viii, 137, 142, 166–169, 175–176). The document is titled "A Chronology of Events James M. Leininger and James M. Huston, Jr." I'll hereafter refer to this document as the 2003 Chronology. Lucinda DeWitt informed me that Bruce sent her and/or her father this document in fall 2003, seven years before the Leiningers published *Soul Survivor*. It's written in the first person, with Bruce Leininger as the ostensible narrator providing a detailed chronology of events from February 2000 to September 2003.[53]

There are serious and troubling discrepancies between the official timeline published in 2009 and the 2003 Chronology. I list some of the more significant ones here.

Claim: James identified himself (James) as the little man in the plane and said he was flying a Corsair, and that he flew off a boat.
Official 2009 Chronology: August 27, 2000
2003 Chronology: Late September–October 2000

Claim: He flew off a boat named Natoma.
Official 2009 Chronology: August 27, 2000
2003 Chronology: Late October–November 2000

Claim: Jack Larsen flew with me.
Official 2009 Chronology: October 5, 2000
2003 Chronology: Late October–November 2000

Contextual Detail: Initial Bowman and A. Leininger correspondence
Official 2009 Chronology: January–February 2001
2003 Chronology: July–August 2000

There are a few things worth noting here.

First, there are two consistent features of the above discrepancies. They all involve important facts of the case, and they include the three most important facts of the case: Natoma Bay, Jack Larsen, and Carol Bowman's involvement. And the official chronology places each of these facts at a later date than the 2003 Chronology, as little as about a month and as great as six to nine months. The later the date when James makes the claims attributed to him, the more opportunity there is for ordinary sources to shape his claims. Not only because there are more opportunities for exposure to sources, but his verbal skills would have been more developed and so also his capacity for internalizing information, and eventually he would have had reading skills.

Second, consider the Bowman factor. According to the 2003 Chronology Carol Bowman got involved six to nine months before the Leiningers' book says she got involved.[54] Not only are the two accounts incompatible, the difference is significant in the wider context. Bowman's advice to the Leiningers was to tell James his experiences were of events he had experienced before. This would strongly suggest and easily instill a past-life narrative. As the Leiningers explain, "Carol advised Andrea to tell James that what he was experiencing were things that had happened to him before, that it was now over, and that he was safe" (SS, p. 117). As Bowman explains it, "I told the Leiningers that James was remembering a past life death, and I reiterated the techniques in my books: acknowledge what James was going through as a literal experience and assure him that he is was now safe, that the scary experience is over" (SS, p. xiii).

Regardless of *when* (on a calendar date) Bowman got involved, the crucial issue is whether she gave her advice to the Leiningers before or after James began making apparent past-life claims. I've already shown how this problem arises in the official narrative when paired with Bowman's account of events. It's even more explicit in the 2003 Chronology. Here James's explicit and implicit claims about a past life occurred *after* Bowman instructed James's parents to present the reincarnation narrative to him as fact, whereas the book's chronology brings her into the case after James allegedly made ostensible past-life claims.

But even if Bowman entered the picture in winter 2001, the explicit reincarnation claims attributed to James came later. Tucker says "By the time he [James] was three, he had told them [his parents] that before he was born, he was a pilot who flew from a boat. His plane got shot in the engine by the Japanese, he crashed in the water, and that's how he died" (Tucker, 2013, p. 64). And Bruce Leininger estimates the date when James said this to be September 1, 2001, which means James would have been three-and-a-half years old.[55] So, even if we accept the chronology in *Soul Survivor*, James's explicit reincarnation claims come many months after the Leiningers had been inculcating this narrative in James.

James's past-life claim is an utterly unsurprising claim for a three-year-old to make given his immersion in WW2 imagery and the suggestion of a past life having been explicitly given to him repeatedly for at least half a year. The official narrative conceals this by introducing Bowman only after James had spoken of the Corsair, Jack Larsen, Natoma, the Japanese shooting his plane down, and his plane crashing at Iwo Jima. This clearly masks how Bowman and the Leininger family (wittingly or unwittingly) might've guided the construction of the reincarnation narrative James subsequently verbalized.

There are reasonable allowances we should make for error and narrative discrepancy. But the quantity and quality of the Leiningers' timeline inconsistencies far exceeds what is rationally permissible. The 2003 chronology was composed in 2003, years before the book, when—in the words of the Leiningers—"everyone's memory was fresh" and they wouldn't have had to rely on "the informal taint of old memories" (SS, p. 166), and long before the Leiningers had thrown away or misplaced their contemporaneous notes. Moreover, a sequence of unintentional mistakes still undermines the reliability of testimony, especially if the mistakes are numerous and involve important facts. Finally, Bruce was ostensibly writing the 2003 summary for John DeWitt, a historian and acquaintance at the time. The four-page chronology of events gives every indication of having been meticulously prepared by someone who appreciates historical accuracy and who has given due diligence to accuracy and detail.

The "Natoma" Veridical Claim

In *Soul Survivor*, the Leiningers say James gave them the word *Natoma* on August 27, 2000, when they inquired about the name of the boat his plane flew off of. Bruce Leininger claims he conducted an Internet search that same night and located information on a WW2 escort carrier named the Natoma Bay (SS, p. 79–80). The information was from *the Dictionary of American Naval Fighting Ships*. Bruce allegedly printed out the information and retained the document which has the website URL and date-stamp of 08/27/2000 in the footer.[56]

Next to the Bowman factor, the most controversial discrepancy between the official 2009 narrative and the 2003 Chronology concerns the date and circumstances of James giving his parents the word *Natoma*. The discrepancy here raises a serious concern about the veracity of this part of the Leiningers' story.

In the 2003 Chronology, the Natoma claim allegedly took place in late October/Early November 2000, *not August 27, 2000.*

> Late October—November 2000
> More details
> Key Items:
> 1) After several attempts and numerous tries to understand

> what he was saying we finally got a word "NATOMA" from him. It resulted in finding that there was a Natoma Bay and that it had been a WWII Escort Carrier that had served in the Pacific.
>
> 2) James gave us the name Jack Larson and said Jack flew with him.[57]

This is no mere discrepancy; it's a glaring and disturbing one.

First, if the Leininger's official story and Jim Tucker's retelling of it is correct, then the 2003 Chronology contains a surprising kind of error. Bruce Leininger regards the Natoma claim as one of three pillars of the case. And unlike many of the other things he attributes to James, he didn't have to rely on his memory to accurately locate this event in his chronology. If the official account is correct, Bruce had the Natoma Bay document on hand with a timestamp that memorialized the date—August 27, 2000. He didn't have to rely on his memory.

Second, the 2003 Chronology doesn't make any mention of the Internet printout. Of course, that chronology also specifically places James saying *Natoma* in late October/early November of 2000, followed by the opaque note that James's dropping the name *Natoma* "resulted in finding that there was a Natoma Bay and that it had been a WWII Escort Carrier that had served in the Pacific." But no mention of the website printout, which is surprising since Bruce was ostensibly trying to document evidence for the Natoma Bay Association historian John DeWitt.

Third, in his 2016 case report Tucker doesn't list *Natoma* as an early-bird claim, despite the timestamped printout. That's because the early-bird claims in his report are based on what was documented in the 2002 unaired ABC *Strange Mysteries* segment, and the Natoma claim and its alleged supporting document were not included in the program. Tucker claims producer Shalini Sharma explained to him that this bit of evidence might have been excluded because other producers judged it was too weak as evidence. This suggests someone associated with the production at least knew about the document. In which case it's odd that when the production crew assisted the Leiningers with trying to locate the mysterious Jack/John Larsen/Larson (Tucker, 2013, p. 74, cf. SS, p. 138) they ignored the crew of the Natoma Bay and instead looked elsewhere and located a naval pilot named John M. Larson who had no connection with the Natoma Bay.[58] This is especially bizarre since the Natoma Bay veteran Leo Pyatt allegedly told Bruce in December 2000 that a Jack Larsen served on the Natoma Bay, and in January 2001 Bruce printed out a list of the crew of the Natoma Bay killed in action in WW2 from the Battle Monuments Commission website (SS, pp. 140, 150). The list included the name of pilot James Huston, Jr., the only crew member killed while the Natoma Bay was supporting operations at Iwo Jima. These facts were known nearly a year-and-a-half before the filming of the *Strange Mysteries* program.

Recall also that Andrea Leininger did not present the Natoma Bay discovery to Carol Bowman in their initial correspondence. Bowman herself places the Natoma Bay discovery as one of the new developments in the case which she learned about when she re-established contact with Andrea Leininger in spring 2002 (SS, p. xiv; Bowman, 2010, p. 55). If James dropped the word *Natoma* after the initial Bowman correspondence and the initial correspondence took place in winter 2001, then James would've had to have given the name *Natoma* after Winter 2001 but before spring 2002 when Bowman reconnected with Andrea. In that case, James could not have first provided the name *Natoma* on August 27, 2000.

Finally, in their book and in various interviews, the Leiningers describe James as giving the word *Natoma* with ease. Given his age, this is unlikely. Quite sensibly, in the 2003 Chronology he only gives his parents the word "after several attempts and numerous tries to understand what he was saying."[59] This is more realistic, but it doesn't engender confidence that the Leiningers even correctly heard what James said. Did he say *Natoma* or *Sertoma* (the name of the Lafayette Air Festival)? Did conflate *Sertoma* and *the Natchez* (a boat he rode while in New Orleans, SS, p. 58) or some other word? Did he utter nonsense? Who can say? And that's the point. When the Leiningers tell us that James said the name of his boat was Natoma, they are giving us their best *guess* at what they *think* he said. Seeing as the Leiningers get so many important details wrong, there's no good reason to trust that they got this detail right. And the degree of inconsistency surrounding the Natoma claim is more than enough to justify a thorough-going skepticism about this feature of the case. We don't know what word James uttered and we don't know when he uttered it.

6. TUCKER'S ANALYSIS RE-EXAMINED

We're now in a position to carefully consider how the problems raised in previous sections undermine Jim Tucker's analysis of the case. In Tucker's 2016 report, he presents seven early-bird items:

1) "Mama, before I was born, (a) I was a pilot, and (b) my airplane got shot in the engine, and (c) it crashed in the water, and (d) that's how I died."[60]
2) Died in the Battle of Iwo Jima
3) Nightmares of (a) plane crashing on fire and sinking and (b) being unable to get out.
4) Flew a Corsair
5) Plane flew off a boat
6) His plane was shot down by the Japanese
7) Corsairs got flat tires when they landed

Each of these, Tucker claims, was documented before Huston was identified. For each of these items, Tucker provides the source of information—the Leiningers—and the alleged verification of the match with Huston, together with additional qualifying comments for items (2) and (4). Tucker claims that reincarnation provides the best explanation of these items and that they are evidence for reincarnation.

In the light of what I've shown in the previous sections of the paper, we should be highly skeptical of Tucker's analysis and conclusions.

One serious problem is that Tucker's reasoning assumes that for each of the above items, he is reporting a claim indisputably made by James prior to the identification of James Huston *and* that there are facts sufficient to confirm that the claims match Huston's death or some feature of Huston's life. This is simply not true. We can apply what we've seen in previous sections to show this.

Disconfirmed Claims: Items (1), (2), and (3)

The facts disconfirm some of the items in Tucker's list, and so they cannot plausibly be said to match Huston's life. These are items (1), (2), and (3).

Yes, Huston was a pilot, his plane was shot down, and it crashed in the water. These highly general claims are (unsurprisingly) correct. But (1) and (3) include more specific claims—(1d), (3a), and (3b). The

Leiningers attributed to James the claim that he (as James Huston) died by drowning after his plane crashed in the water. That's suggested in the wording of (3b), and it's how the Leiningers interpret *how* in "that's how I died" in (1d). But the historical description of Huston's crash and accompanying commentary in the Natoma Bay aircraft action report make it highly improbable that Huston survived the impact. Moreover, if, as the Leiningers explain, James actually intended to state that his plane was on fire before it hit the water, the aircraft action report also contradicts this. So, of the six specific claims contained in (1) and (3), significant evidence disconfirms three of them. Since the veracity of James's narrative crucially depends on the details of Huston's death as James allegedly described them, the disconfirmations here are significant.[61]

(2) is clearly false. Huston did not die *in* the Battle of Iwo Jima, and he's not listed among the fatalities in that battle. Of course, James never said or implied that he died *in* the Battle of Iwo Jima. Prior to the publication of their book, the Leiningers said James pointed to an arial view of Iwo Jima and said that's *where* his plane crashed. That claim is also demonstrably false. We know Huston died 240 km (=149 miles) north of Iwo Jima, in Futami Harbor at the island of Chichi Jima, an altogether different island in the Bonin island grouping. James's claim is false. Full stop.[62]

Tucker bases his attribution to James on an interpretation of Bruce Leininger's correction of a decade-old false memory, a redaction the Leiningers made while writing their book. They changed their account of what James said to "That's *when* my plane was shot down" (SS, p. 104). They even altered what James said after his father asked him to clarify what he meant—from "My airplane got shot down *there*, Daddy" to "That's *when* my plane got shot and crashed" (SS, p. 104, cf. Tucker, 2016, p. 201). Like the Leiningers, Tucker interprets James's redacted claim to mean the time period in which his plane crashed.[63] Apart from the dubious nature of the redacted claim, Tucker's interpretation is otherwise unjustified. Moreover, if he purports to be analyzing the early-bird claims as documented in the 2002 ABC program, he should state the claim as it was attributed to little James before the Leiningers had identified Huston. The actual early-bird claim is demonstrably false, and Tucker's parsing of what James said is without warrant.

Ambiguities and Redactions Masking Further Falsehoods: (4) and (7)

The Leiningers' book makes it clear that, for a few years, they had interpreted James's claims about flying a Corsair to be a reference to the plane he was flying when the Japanese shot him down and he died. Moreover, the reproduction of the conversation in which James Leininger allegedly said he flew a Corsair (SS, p. 78–79) contextually implies he flew a Corsair off the Natoma Bay (false) and died flying a Corsair (false). For reasons discussed earlier, this was a plausible interpretation of James's claims given the context. The Leiningers later rejected it only because they discovered that Huston was not shot down while flying a Corsair and did not die in one, but he at least test flew one. Since they had decided on Huston as the previous personality, they retrofitted James's claims to this more mature narrative. But regardless of the reasons for the redaction, Tucker should state the claims as they were attributed to James prior to the identification of the previous personality. James indicated the Japanese shot him down when he was flying a Corsair and that he died flying a Corsair, both of which are false.

Item (7) does not concern Huston, except indirectly since Huston test flew the Corsair while he was with the VC-301 squadron in April 1944 prior to joining the VC-81 and serving on the Natoma Bay. The problem with this claim is that it is ambiguous. It's either a claim about something that was true of the Corsair and other WW2 planes or something that was true of Corsairs in contrast to other WW2 planes. If the former, the claim is true, at least when suitably qualified (see below). If the latter, the claim is false. Hence, the ambiguity straddles the line separating a claim that is easily knowable through ordinary means or easily disconfirmed.

Flat tires and collapsing landing gear were a common problem for WW2 fighter planes that had to make carrier landings. It wasn't uniquely true of Corsairs.[64] This point needs emphasizing since there seems to be considerable confusion about it among people who discuss this case. Tucker cites an unnamed Air Force historian he didn't personally interview in support of the claim "Corsairs bounced when they landed, leading to flat tires" (2016, p. 204). The cited interview clip does not imply that flat tires were unique or especially true of Corsairs, and there’s no evidence for that. While a deck bounce *can* cause flat tires, statistical data would be needed to show that it led to more flat tires with Corsairs than other warbirds, especially since this problem appears to have been linked to inexperienced pilots flying the Corsair.[65] Most importantly, though, the Corsair bounce problem was resolved when the oleo struts were redesigned in early 1944. The F4U-1 Corsair Huston tested with the VF-301 in April 1944 already had the redesigned oleo struts. Huston had probably heard about the bounce problem in earlier Corsair models, but he would have tested the redesigned Corsair after the bounce problem had been fixed.[66]

James’s apparent knowledge about Corsairs getting flat tires is standard information on Corsair and other WW2 videos, especially ones that discuss naval fighter planes. For example, it’s mentioned in the Corsair *Battle Stations* video discussed earlier. These videos also mention how the Corsair was redesigned during the war, the kind of detail a toddler is likely to miss. James gives no indication of being aware of the characteristics of the specific model of Corsair he would have test flown had he been Huston. His claims about the Corsair are what we’d expect from a toddler who had viewed videos or heard conversations about carrier landings, but who lacked a nuanced understanding. His claims are not what we would expect from someone who experienced flying a Corsair or who had a more conceptually developed understanding of WW2 warbirds.

Dubious Confirmation: (1b)

Tucker’s list also includes at least one item with a dubious confirmation. The only evidence for (1b)—Huston’s plane being hit in the engine—is the alleged testimony of the Avenger pilots Bruce Leininger interviewed. But, as argued earlier, their testimony is inconsistent with the narrative provided in Natoma Bay aircraft action report and which the Leiningers excluded from their book. We simply don’t know that Huston’s plane was hit in the engine. This isn’t even probable given all the evidence. Maybe it’s true. We just don’t have overriding reason to suppose that it is. So, the facts do not all things considered confirm this early-bird item.

Plausible Alternative Explanations for Hits and Misses

So, what does this leave us with?

The only claims attributed to James that are *indisputably* true of Huston are four: (1a)—he was a pilot, (1c)—his plane crashed in the water, (5)—he flew his plane off a boat, and (6)—His plane was shot down by the Japanese. But these claims are highly general and true of thousands of other pilots killed in WW2. So, it's unsurprising that they should fortuitously turn out to be true of Huston. The disconfirmed claims are the more specific ones. That James gets all these wrong, or that we're in no position to say whether they are correct or not, is quite telling. This outcome is exactly what we would expect from a fiction narrative. Whether we select a random dream we have or the narrative of a Stephen King novel, we're bound to find something that's true, especially very general truths, but the specifics are where we can expect to find falsehoods or claims so disconnected from reality that we can neither confirm nor disconfirm them. James's narrative of the plane crash in his dreams is utterly banal, in both its matches and misses. It's exactly what we would expect from a flight of imagination, not the real flight of a real WW2 pilot. The only thing that is extraordinary is that anyone would think otherwise.

This is especially the case when we consider that his dream imagery has very plausible ordinary explanations in known exposures that the Leiningers and Tucker have not acknowledged. The Blue Angels video and his visit to the Cavanaugh Flight Museum were more than sufficient to supply him with the idea of being a pilot and the imagery of flying a Corsair fighter plane off a boat, being shot in combat with the Japanese, his plane catching on fire, and the plane crashing in the water. And the imagery of being trapped in a sinking plane is hardly one that exceeds the imaginative capacities of a two-year old. Even the timing of the origin of the nightmares is what we would expect given mainstream developmental psychology.

Moreover, even the overt reincarnation narrative in (1) has an obvious explanation. Based on the evidence presented earlier, James said (1) when he was around three and a half. As such, it has a quite pedestrian explanation: His parents gave him the narrative framework.

The Leiningers followed Carol Bowman's advice (beginning in February 2001) and began telling James that his nightmare experiences were of something that had happened to him *before* (SS, p. 117). We have no evidence that James affirmed that he had lived a past life prior to his parents suggesting this to him. And that's true even if we rely on the official 2009 chronology. Tucker's analysis ignores the temporal priority of Bowman to the reincarnation narrative that subsequently seemed to be part of James's self-understanding.

Reassessment of Tucker's Table of Early-Bird Claims

In Table 2 below, I present a breakdown of twelve distinct and specific early-bird claims Tucker attributes to James in the seven items examined above, together with Tucker's regarding them as a match with historical facts.

TABLE 2
Tucker's List of Early-Bird Claims/Verifications 2002

Items	Match with Huston
1) I was a pilot.	Yes
2) I flew a plane off a boat.	Yes
3) The Japanese shot my plane down.	Yes
4) My planed crashed and sank in the water.	Yes
5) My airplane got shot in the engine.	Yes
6) I died because 4) and 5).	Yes
7) My plane crashed on fire.	Yes
8) I could not get out of my sinking plane.	Yes
9) I died in the Battle of Iwo Jima.	Yes
10) I flew a Corsair.	Yes
11) Corsairs got flat tires when they landed.	Yes
+	
12) James's metanarrative claim "*before I was born*, I was a pilot, and my airplane got shot in the engine, and it crashed in the water, and that's how I died."	

Tucker would have us believe that James made the twelve claims above before Huston had been identified and that (1)–(11) match the life of Huston, and that (12) indicates James's awareness of having lived a past life. He says this is evidence for reincarnation. But the latter is not true, and it's not true because the matches to Huston's life are illusory. The appearance of genuine matches in Table 2 (and Tucker's original seven items) depends on the following logically dubious maneuvers:

—treating disconfirmed claims as confirmed—(7), (8), and (9)

—replacing disconfirmed early-bird claims with non-disconfirmed redacted claims—(9) and (10)

—relying on unwarranted verifications—(5)

—relying on ambiguity to force-fit matches with Huston—(6), (9), (11), and possibly (7).

—Ignoring the temporal priority of parental influence via Bowman shaping the reincarnation narrative attributed to James's self-understanding—(12).

Only four of the twelve claims—(1), (2), (3), and (4)—are indisputably known to be claims James made and which clearly match the life of Huston. By themselves, these four claims are hardly surprising and wouldn't merit any serious attention. But when they are placed in the context of other more specific claims presumed to be matches, the result is a more robust narrative. In that narrative, the four claims that would otherwise be unsurprising matches now seem like elements in a narrative that resists ordinary explanation.

Tucker has created the appearance of something impressive. There's no doubt that his narrative will prove psychologically impactful to some people, but in point of logic the reasoning is dubious. First, everything in the list is very plausibly explained by a combination of chance coincidence and exposures to the content of video(s), WW2 exhibits and memorabilia, and Bruce Leininger's book on Iwo Jima. Nothing more is needed to explain any of this. Second, the more specific claims are actually unwarranted, either because they are disconfirmed or because they are not sufficiently confirmed. Third, in speaking of James's verbalizations of the content of his nightmares prior to August 27, 2000, Tucker says "James indicated that they were memories of events from the past" (2016, p. 201). He thereby implies that James's reincarnation self-understanding contained in (12) occurred before August 27, 2000. This is both unwarranted and false, even by Tucker's own admission elsewhere (Tucker, 2013, p. 64). And after mentioning the events of December 2000, he says, "With the ongoing nightmares, James's parents eventually contacted Carol Bowman" (Tucker, 2016, p. 201). He thereby suggests that James made several of his important past-life claims before Bowman entered the picture, but this is highly dubious for reasons discussed earlier. *Consequently, Tucker's big-picture narrative deceptively gives the reincarnation hypothesis an appearance of plausibility which it simply does not have.*

If we strip the fictional scaffolding away from Tucker's list of early-bird claims, we get a very different table of claims and match-with-Huston outcomes (Table 3).

In Table 2, eleven claims allegedly match Huston, and (12) provides the reincarnation framework for the 11 claims. However, in Table 3 *only four* of the eleven match Huston. Moreover, the matches are all very general claims, whereas the specific claims do not match Huston, and in at least one case we don't know if the claim is veridical or not. And once we acknowledge that Bowman conveyed a reincarnation narrative to the Leiningers and they passed it on to James before he made any past-life claims, the ostensible reincarnation claim in (12) is plausibly interpreted as something instilled in James. This is a considerably less impressive outcome, even if we have no way to assess how evidentially strong the results are in Table 2.

For reasons adduced earlier, I think we have good reasons to prefer Table 3 over Table 2. But even if we had no overriding reason to prefer Table 3 over Table 2, it's at least clear that we have no non question-begging reason to prefer Table 2 over Table 3. So, *either* there's good reason to prefer Table 3 over Table 2 or we have no overriding reason to prefer either table. Since Tucker's case for reincarnation depends essentially on the robust narrative in Table 2 and we at least have no reason to prefer that narrative to the alternative robust narrative in Table 3, Tucker's case for reincarnation fails.

TABLE 3
Alternative List of Early-Bird Claims

Item		Match with Huston
1)	I was a pilot.	Yes
2)	I flew a plan off a boat.	Yes
3)	The Japanese shot my plane down.	Yes
4)	My plane crashed and sank in the water.	Yes
5)	My airplane got shot in the engine.	?
6*)	I died by drowning (after my plane crashed).	No
7*)	My plane was on fire before crashing into the water.	No
8*)	I was trapped in my sinking plane and tried to escape but couldn't.	No
9*)	My plane crashed at Iwo Jima.	No
10*)	I was flying a Corsair when I crashed and died.	No
11*)	Corsairs had a unique problem of getting flat tires when they landed.	No or?
+		
12)	James metanarrative claim "*before I was born*, I was a pilot, and my airplane got shot in the engine, and it crashed in the water, and that's how I died."	

His case for reincarnation also fails because there is a most plausible ordinary explanation for the veridical claims in Table 2 and all the claims in Table 3. We have a straightforward explanation of the content and occasion for James's nightmares, as well as why James gets correct what he gets correct, and why he gets incorrect what he gets incorrect.

From this vantage point, we can see why Tucker's dismissal of alternative explanations in (2016) fails. He dismisses an explanation of this case in terms of "knowledge acquired through ordinary means" since allegedly it would not have been possible (or at least would be highly improbable) for James to learn about the Natoma Bay or James Huston through ordinary means given the assumed lack of general availability of such information. It's not clear what claims Tucker has in view here (other than James allegedly naming the Natoma Bay and Jack Larsen), but clearly what Tucker is trying to do is dismiss ordinary sources as an explanation because that hypothesis would seem unable to account for everything. But no hypothesis can account for everything, and Tucker has nowhere shown, but only asserted, that reincarnation can explain such recalcitrant data. It's also hard to see how Tucker's argument works here without assuming the credibility of the Leiningers in ways that I've shown are implausible.

More importantly, though, neither *Natoma Bay* nor *Jack Larsen*, nor many of the other claims Tucker classifies as items of knowledge about James Huston count as early-bird claims (Tucker, 2016). So, Tucker has surely not ruled out ordinary explanations of the early-bird claims. It's understandable, of course, why someone unacquainted with the content of the ordinary sources James was exposed to might regard the early-bird claims as immune to such an explanation. But as I've shown, this is an illusion created by dark data and fact fudging.

7. NATOMA BAY, JACK LARSEN, AND JAMES 3

I have focused on the seven early-bird items Tucker lists (2016), but there are three other items Tucker discusses (2016). And in his chapter on James Leininger in *Return to Life* he regards them as additional early-bird claims. These are James allegedly giving his parents the names Natoma and Jack Larsen, and his signing his name *James 3* on drawings of airplanes and battle scenes and his saying he was the third James.

We can easily dispense with "James 3." James started signing his name *James 3* months after he turned three-years-old. That he continued signing his name *James 3* after he turned four is hardly surprising given the attention this behavior had elicited. Moreover, James may very well have believed that he was the third James. After all, he had already internalized the reincarnation narrative his parents had suggested under the influence of Carol Bowman. Given these contextual factors, there's nothing psychologically peculiar here. There is nothing in need of an extraordinary explanation.

So, this leaves us with *Natoma* and *Jack Larsen*.

On the face of it, these two claims, even if they do not have any obvious ordinary explanation, would not rescue the early-bird Leininger narrative from its impoverished evidential status. We often find ourselves in an epistemic situation where a set of facts seems to resist ordinary explanation. This is typically because of what we don't know, so-called dark data. But upon further scrutiny and as our knowledge expands, we acquire ordinary explanations. Our set of otherwise inexplicable facts accordingly shrinks. This pattern often continues as we acquire more information, and the domain of otherwise inexplicable facts shrinks even more. What does conscientiousness demand in such situations? It demands that we revise our degree of credence. As the set of otherwise explicable facts shrinks—say, from 100% to 80% to 60% to 40% to 20%—we ought to be less confident that the remaining items will remain inexplicable. And we certainly ought not to appeal to the dwindling space of seemingly inexplicable facts as a lens through which we view the totality of facts to reassert their extraordinary nature.

This is precisely the situation in the Leininger case. Prior to the publication of this paper, neither the Leiningers nor Tucker acknowledged just how much of this case, especially the alleged early-bird claims and behavior of James, were easily and naturally explained by ordinary experiences and pedestrian sources of information. To date, the reincarnation interpretation of the presumed facts in this case has depended largely on an illusion of plausibility created by dark data, fact fudging, and dubious inferences. This needs to be emphasized at the outset to underscore a straightforward implication regarding the status of the two remaining early-bird items. One cannot plausibly leverage the seeming inexplicability of two facts to reinstate the reincarnation interpretation of the rest of the facts.

But we can otherwise dispense with these remaining features of the Leininger case.

In Section 5 I provided several reasons for skepticism about the Natoma attribution—skepticism with respect to whether James said *Natoma*, when he said it, and the narrative context in which he said it. We are simply not justified in accepting this claim given the level of obfuscation surrounding it. And we are certainly not justified in regarding this item as an early-bird claim.

Tucker's justification for treating the Natoma attribution as an early-bird claim is implausible. He says, "we have definite documentation—from the *Strange Mysteries* interview and from printed records—of statements and behaviors from James Leininger, items that were recorded before James Huston was identified" (Tucker, 2013, p. 77). The printed records Tucker has in mind include the Natoma Bay Internet printout Bruce provided Tucker and which is timestamped 08/27/2000. As mentioned earlier in the paper, Tucker has reproduced this document for readers, with the website URL and date appearing in the footer (Tucker, 2016, p. 202, Fig. 1).[67]

But the timestamped document is not a record of anything *James said* before Huston was identified. It's simply the entry on the Natoma Bay from the online version of *the Dictionary of American Naval Fighting Ships*. Even if Bruce Leininger printed this document on the date in question, it's not *definite documentation* in the form of a *printed record of a statement from James Leininger*, which was *recorded before James Huston was identified*. In spring/summer 2002, the 2002 ABC program documented claims the Leiningers attributed to James at that time, months before the Leiningers had decided Huston was the previous personality. But the Natoma Bay document is no record of anything James said, much less a record of something he said prior to Huston being identified.

Tucker's justification for giving the Natoma attribution early-bird status is an inference from the Natoma Bay document (allegedly printed in 2000) and the testimony of the Leiningers in 2010 about what James allegedly said prior to Huston being identified, as well as the Leiningers' story about the circumstances that led to the printout. That's clearly a dubious strategy to underwrite an early-bird claim. Tucker says that, while other features of the James Leininger case are dependent on the Leiningers' memories, this particular piece of evidence isn't (Tucker, 2013, p. 69). That's false. Without the contextual narrative for the printout, it's useless as evidence for what James allegedly said before Huston was identified. But the contextual narrative depends on the reliability of the Leiningers' testimony. But their testimony is unreliable. And the opaque narrative surrounding the Natoma Bay document illustrates this. Since there are serious discrepancies in the historical record concerning when James uttered the word *Natoma*, the circumstances in which he said this, how the Natoma Bay printout came about, and whether James even said *Natoma*, the problem for Tucker is that this particular piece of evidence is married to egregious obfuscation and inconsistency.[68] There is no justification for including it as an early-bird item.

The "Jack Larsen" attribution is no less problematic, though for different reasons.

According to the official 2009 chronology (SS, p. 89–90), the Leiningers elicited the following responses from James in a conversation surrounding one of his nightmares.

> "Can you remember anyone else in the dream?" asked Andrea. "Any friends?"
> James concentrated for a moment; then his face lit up and he said, "Jack!" . . .
> "Do you remember Jack's last name?" asked Andrea.
> And then James said, very clearly, "Larsen. It was Jack Larsen." . . .
> "Was Jack James's friend?"
> And James replied, "He was a pilot, too."

While it seems that James is here giving his parents some very specific information, the information is less specific than it first appears. Although we have a name, we don't know who the person is who is named, nor is there much of a narrative context to dial this in. We only know he is a pilot who seemed to have appeared in James's dreams and that James seems to say he is a friend. But it's not clear whether Larsen is supposed to be the previous personality or someone the previous personality knew.[69] Even Bruce and Andrea Leininger couldn't agree on the identity of Larsen and his place in the narrative for nearly two years (SS, pp. 91–92, 95–96, 140–142; Tucker, 2013, p. 71). What James said was obviously not clear at the time, but this is problematic if we intend to justify claims about a match between James's Jack Larsen and a historical person by that name.[70]

It is true, of course, that there was a pilot named Jack Larsen on the Natoma Bay and he flew with Huston on the day Huston died. So, one can plausibly argue that there is a *similarity* between the facts and what James said. But are they *identical*? That's the real question. Similarity does not prove sameness.

In the context in which James Leininger gave the name *Jack Larsen*, this is considerable vagueness, but vagueness and ambiguity are fertile grounds for cultivating a deceptive appearance of an identity between two different things, people, or events. This is because vagueness creates very broad parameters for the kind of facts that can subsequently be viewed as a match. We don't even know whether *Jack* is supposed to be a middle or first name, or a nickname—that is, assuming little James didn't make a mistake about the name altogether (cf. SS, p. 92). And even if we assume, as Tucker does, that James meant Jack Larsen was his friend (and not the previous personality), this doesn't sufficiently constrain the parameters for what would count as a match. Since the parameters of "friend" are not well-defined, especially since it's a two-year-old using this word, the space of potential matches remains quite large. We're not told when they were friends, the circumstances of this, for how long, or anything specific about the friendship.

Here we see a logical sleight of hand that has arisen elsewhere in the Leininger case: the logical mistake of treating sufficiently similar events as if they're identical, what is referred to in probability theory as *the law of near enough*.[71] Descriptions are often very similar to particular events or people without being genuine matches, and the vaguer or more ambiguous a description is, the more unrelated facts it will resemble in some way or other. But if we expand the space of potential matches in this way, it will be unsurprising that we find something that seems to be a match, however unrelated it might be. If Larsen is either the previous personality or another pilot who knew the previous personality, and either alive or dead, the parameters are considerably large. In that space the Jack Larsen of the Natoma Bay fits, as do many other Jack Larsens (and men with similar names) in WW2.

One can, of course, mask the dependence on the law of near enough by adding assumptions that tighten the parameters for a match. Assume that Jack Larsen is someone other than the previous personality but who knew the previous personality. Assume Larsen was a naval pilot, as Bruce Leininger does (SS, p. 96). Assume that James's Jack Larsen was present when the previous personality crashed his plane. Assume that the crash happened during the Battle of Iwo Jima (instead of at Iwo Jima). Assume Larsen is still alive. Assume there is consistency and significant continuity between James's nightmares over several months, and together with his waking state comments that he provided a coherent narrative of events set in WW2. Assume that when James said the name of the little man in his dreams was *James*, he was intending to give the name of the previous personality, rather than intending to refer to himself.

The problem is that these assumptions are mostly unwarranted, and some of them never even occurred to the Leiningers until *after* they learned about the Jack Larsen and James Huston of the Natoma Bay beginning in late 2000 and early 2001. One can always combine the law of near enough and selection procedures to dial-in a potential match. The challenge is to avoid selection bias, which we don't if we use post hoc assumptions to constrain the space of possible matches. The raw claims attributed to James Leininger can with an equal show of plausibility be connected in many different ways. But Tucker has not provided any rules for doing this that dial-in the Jack Larsen of the Natoma Bay in a non-question–begging or otherwise logically suspect manner.

Tucker says, "It seems obvious in retrospect that James was saying he was a pilot named James who knew another pilot named Jack Larsen" (Tucker, 2013, p. 70). Perhaps, but this lends no warrant to the contention that James actually meant this. If the original claims are vague enough, any subsequent fact can be made to fit them. The resultant narrative will seem obvious *in retrospect*. It may also seem intuitive that we can reasonably tweak a narrative on the basis of facts we subsequently discover, but this can be a serious liability. We might incur the disadvantage of knowing only those facts we were unlucky enough to stumble upon first, facts that misled us into thinking a particular narrative was obvious when it was actually false. So, of course, given that we know Huston and Larsen were members of the VC-81 and served together on the Natoma Bay during the Battle of Iwo Jima, it looks obvious in retrospect that James was referring to himself as James Huston and the Jack Larsen in his dreams was a friend of Huston's. But had Bruce Leininger happened upon a different set of facts or adopted a different set of assumptions, a different conclusion about the identity of the previous personality would have seemed just as obvious in retrospect.

What I will call "counterfactual matches" are instructive here.

If claims are vague enough, we can envision many counterfactual scenarios in which other facts would seem like an obvious fit to a different conjecture. Tucker thinks it's obvious that Larsen refers to a friend of the previous personality, but if Bruce Leininger had discovered a Corsair pilot named Jack Larsen who flew off a carrier in the Pacific and was shot down on the island of Iwo Jima, it would have been obvious in retrospect that Jack Larsen was the previous personality. Had this Jack Larsen test flown a Corsair in 1944, but later died after being shot down near Okinawa, it would have been obvious in retrospect that James was referring to himself as Jack Larsen.

Two other points are worth noting. While it may seem like the different elements involved in James's early-bird claims serve to delimit the range of possible matches, this is an illusion. It's quite the opposite. The law of combinations tells us that "the number of combinations of interacting elements increases exponentially with the number of elements."[72] In the Leininger case, this is particularly egregious because not only is the identity of Jack Larsen ambiguous, so are the other elements being used to dial-in the identity of a previous personality. The Leininger narrative relies on the law of near enough with respect to several interacting elements of the story. What James says about Iwo Jima and the Corsair are, if not false, at least vague. And the reasoning above makes use of this combination of vague claims to significantly increase the number of scenarios that would count as a match.

Nor does inserting *Natoma Bay* into the equation make any significant difference to how the laws of probability work here. Even if that attribution were credible, there would still be an incredible amount of vagueness in the interacting elements of the case to permit hundreds of counterfactual matches. If Larsen had been a member of the *U.S.S Sargent Bay* and flew one of the TBM Avengers on the Chichi Jima run with Huston, then this would count as a match to James's claims. If Larsen had been a member of the VF-301 and test flew Corsairs with Huston off the *U.S.S Gambier Bay*, then this would have counted as a match. If Larsen had not served on the Natoma Bay but had otherwise participated in the Battle of Iwo Jima with Huston—perhaps they shared the sky on a particular mission or knocked back some skunked brewskies over a plate of two-day old meatloaf—that would count as a match.

Now, if we grasp the conceptual point about counterfactual matches, we should see the salience of our ignorance for assessing actual matches with actual facts. What we don't know matters. What unknown actual facts are out there which, had the Leiningers discovered them first, would have made a different narrative look equally extraordinary? If we only focus on the facts that we have been unlucky enough to stumble upon first and which seem to fit a particular narrative (or are made to fit it), we are likely to ignore the salience of the *possibility* of other facts which, had we stumbled upon them instead, would have made a different narrative look equally extraordinary.[73]

So, while Jack Larsen and Natoma (and other features of the case) can be made to fit a narrative where James Huston is the previous personality, the method of generating this fit seems poorly insulated against the charge of ignoring how different laws of improbability combine to give the deceptive appearance of something extraordinary.

8. CONCLUSION

In this paper I have provided a detailed examination of the James Leininger case during its crucial formative phase, roughly spring 2000 to fall 2002. I've focused on features of the case Tucker classifies as early-bird items. These items, allegedly documented before the Leiningers identified Huston as the previous personality, are ostensibly the case's strongest features. Tucker claims reincarnation provides the best explanation of these items and that these items are evidence for reincarnation. I have argued that Tucker is incorrect.

As explained early in the paper, the evidential force of facts depends on two conditions.

(N) There are no additional facts that neutralize the evidential force of the presumed facts.

and

(D) There are no significant grounds for doubting the presumed facts.

If either (N) or (D) does not obtain, the *prima facie* evidential force of facts in support of a particular hypothesis or theory is defeated.

I have argued that neither (N) nor (D) is true with reference to the presumed early-bird items Tucker identifies in (2013) and (2016). Otherwise stated, Tucker has not presented a robust, credible chronology of events. We have many significant, additional facts Tucker has not acknowledged in his investigation and analysis. These facts neutralize the evidential force of the early-bird items—for example, by raising the probability and explanatory efficacy of non-reincarnation explanations. And the multi-faceted unreliability of the Leiningers gives us considerable reason to doubt the presumed facts of the case which Tucker adduces in favor of the reincarnation hypothesis.

What does this say about the merits of the James Leininger case as a whole?

Although I have focused on Tucker's early-bird items, my discussion has considered a wider range of alleged facts that encompass the two-year formative phase of the case, as well as some items after fall 2002. I have at various points argued that my skeptical assessment of the early-bird items applies equally with respect to this wider range of presumed facts of the case. But the arguments I've presented would seem to raise significant doubt about the case as a whole. First, I've argued that significant doubt vitiates the foundational narrative of the case between 2000 and 2002. A weak foundation does not support a solid superstructure. Second, the early-bird items are the case's strongest features—if the stronger features don't survive critical scrutiny, *a fortiori* the weaker features won't. Finally, this paper raises significant doubt about particular features of the case—for example, the reliability of the Leiningers—that are essential to the larger story. So, the big-picture Leininger narrative is no less subject to doubt than the small-picture narrative. Neither warrants belief in the reincarnation hypothesis

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Between 2019 and 2021, I interviewed or corresponded with more than two dozen people as part of my investigation and research into the James Leininger case. I wish specifically to acknowledge the assistance of the following individuals: Stewart Bailey, Christy Bonds, Carol Bowman, Lucinda DeWitt, David Hand, John Fallis, Leslie Kean, Shalini

Sharma, and Suzanne Stratford. Special thanks to the Cavanaugh Flight Museum for granting me permission to use the museum images in this paper. And special thanks to Cassidy Rees at the Cavanaugh for acquiring museum permissions on my behalf, as well as for providing information and photos of the museum. Much appreciation to Steve Baughman, Stephen Braude, and Keith Augustine for discussing this case with me during the course of my research and investigation. And Jim Tucker—although I have offered a critique of Tucker's investigation and analysis of this case, I'm grateful for the time he has taken since 2019 to discuss this case with me. Finally, Bruce Leininger—I had hoped he would've been willing to discuss his story with me beyond our initial email exchange and address the concerns developed in this paper. To date this has not happened. Nonetheless, I appreciate the answers he gave to my initial query.

NOTES

[1] See B. Leininger, 2021. The Bigelow Institute for Consciousness Studies Awarded Leininger an Honorable Mention prize and $20,000 for his essay in the 2021 competition for essays presenting the "best evidence" for life after death.

[2] The quote is taken from Tucker's blurb in the front material of the Leiningers' 2009 book *Soul Survivor* (U.S. edition).

[3] My research is based on variety of primary and secondary source documents (referenced in the paper), as well as correspondence and interviews I conducted with over two dozen people, including the individuals mentioned in the Acknowledgments at the end of this paper.

[4] In this paper, I will provisionally accept the early-bird status of claims attributed to James in the 2002 ABC program. However, genuine early-bird claims must be documented prior to anyone *attempting* to verify the subject's claims or behavior (Stevenson, 1974, pp. 4, 71; Braude, 2003, p. 182). Once someone tries to verify a subject's claims, information acquired about a potential previous personality may influence what a subject subsequently says or influence the recollections of others and what claims they subsequently attribute to the subject. Bruce Leininger began attempting to verify James's claims as early as August 2000. From the very beginning, James's claims and

his father's ongoing discoveries were inextricably entangled. Tucker says the 2002 program documents what James claimed before Huston was *identified*. This is a considerably looser standard since it allows an evolving narrative of past-life claims to be shaped by information acquired from attempts at verification, even if no decision has been made or conclusion reached about the identity of the previous personality.

5 Tucker cites psychiatrist Terr (1981) on childhood trauma to support his repetitive play conjecture. But this is problematic. First, post-traumatic play is symptomatic of trauma typically experienced in childhood. Second, Terr discusses 11 characteristics of post-traumatic play; Tucker narrowly focuses on a couple of these. Third, in Terr (2003) co-occurring symptoms are discussed as parts of a robust diagnosis of past trauma, but Tucker's suggested diagnosis doesn't consider any of this. Most importantly, both Terr and the *DSM-V* presuppose that the clinician has observed the subject engaged in play and has a robust account of the subject's developmental history and other salient biographical facts, including a traceable (hence known or knowable) incident in the subject's past that establishes a plausible cause for present observational behavior. Tucker is relying on behavior he never directly observed to infer that a child experienced a trauma as an adult in a past life. This seems like a misapplication of the clinical literature. One might just as plausibly conclude that James's behavior was either not post-traumatic play or he suffered a trauma in this life that did not show up on Tucker's radar. This disjunction of possibilities is considerably more probable than a conclusion arrived at by misapplying clinical work on childhood trauma.

6 Assume hypothesis h has some degree of probability N given facts f: $\Pr(h \mid f) = N$. The evidential value of f for h is diminished just if there is some other fact(s) f^*, such that the conjunction of f and f^* results in h having a probability less than the probability of h given f alone. The probability that Ian was at Mr. Prescott's book launch party in Tuscan on Friday night may be high given that Ian said he was planning to attend the party and three attendees claim to have seen Ian there, but many additional facts would greatly lower this probability. Perhaps none of the surveillance video cameras at Mr. Prescott's house capture images of Ian at the party, though they do capture images

of the other attendees. Maybe surveillance cameras capture images of Ian at a business conference in San Francisco on the evening of Mr. Prescott's party. Or perhaps we learn that Ian's wife says Ian was home all night and Ian's twin brother, also a friend of Mr. Prescott, was in town for the weekend.

7 In correspondence (12/13/19) Leslie Kean sent me images of the front and back cover of Bruce Leininger's copy of the Blue Angels VHS tape. He had previously sent these images to Kean. Although I have the images of Bruce's copy of the video, Figure 1 is a photograph of *my* copy of the same 1994 VHS tape with its original cover.

8 In correspondence (8/17/21), Tucker said he meant the video could not be the source of James's alleged specific knowledge about Huston's *role* in WW2. But the issue is not whether James acquired information about Huston's role in the war from the video, but whether the video is a plausible source for James's experiences, behavior, and claims in the first several months of the story long before he made any claim that was specifically or idiosyncratically true of Huston. It's not required that the video be the source of everything James said.

9 In correspondence (08/17/21), Tucker said that almost anything taken in isolation could be the source of James's nightmares. But the issue is not what, taken in isolation, *could* have influenced James's experience, but rather what provides a *plausible* explanation for it in the immediate context. To determine this, we should consider what James was *actually* exposed to and its causal relation to the content and affect in his dreams. A child who has nightmares about a fighter plane that is shot down is transparently less surprising if he's been repeatedly watching a video displaying such imagery than if he had not been exposed to such imagery.

10 While the VHS tape is widely available, for ease of reference the cited timestamps are from a version of the video on YouTube.

11 We might also ask whether a two-year old is likely to distinguish between an Iraqi MiG being shot down and a Blue Angels plane being shot down when these flash on the screen in proximity to each other in fast moving clips. We cannot easily dismiss the possibility that James perceived the imagery as showing a Blue Angels plane being shot down. Thanks to Steve Baughman for pointing this out to me.

12 The Leiningers do acknowledge (SS, p. 57) that at one point in June

2000 James's mother temporarily took the Blue Angels video away from James—she told him it broke—in the effort to divert his attention away from airplanes. But they give no indication of being aware of the specific content of the video or how it might plausibly have been affecting him.

[13] Tucker indicates that the Cavanaugh Flight Museum confirmed this when he checked with museum staff (2013, p. 69). As Tucker also indicates, the Corsair previously on display at the museum crashed at an airshow in Wisconsin on July 29, 1999. What Tucker does not say is that the pilot flying the Corsair when it crashed was Laird "Lad" Doctor, the director of the Cavanaugh Flight Museum. The crash left Doctor a quadriplegic. This detail is relevant to the Leininger story because the Corsair's fiery crash at the Oshkosh airshow continued to be publicly acknowledged and discussed at the museum as late as March 2000, shortly after James first visited. See AviationChannel.net (2017) and "10/22/99 Statement on Lad Doctor" (Cavanaugh Flight Museum 2000a).

[14] I accessed the museum website using the Wayback Machine at Archive.com. See especially Cavanaugh Flight Museum (2000a, 2000b, 2000c).

[15] See Cavanaugh Flight Museum (2000b) for a listing of the museum's WW2 artifacts in 2000 with links to images.

[16] See Cavanaugh Flight Museum (2000c) for a listing of the museum's WW2 fighter planes in 2000 with links to images.

[17] In correspondence (08/09/21), Christy Bonds (former Cavanaugh Gift Shop Manager) said, "we had several versions at different price points for Corsair toys and models." She also said the extensive collection of photos in the museum's gallery included images of Corsairs, as Figures 5 and 12 in the text illustrate.

[18] There is only one possible exception: James allegedly giving the word *Natoma* on August 27, 2000. But Tucker doesn't include this as an early-bird item in 2016. For reasons to be discussed in subsequent sections, the Natoma attribution is dubious.

[19] If skepticism means withholding assent to reincarnation as the best explanation—that is, neither affirming nor denying that reincarnation is the best explanation—it will suffice to show that *Tucker has not plausibly ruled out ordinary sources*, in which case we don't have good

enough reason to affirm that reincarnation is the best explanation. A stronger form of skepticism involves denying that reincarnation is the best explanation on the grounds that ordinary sources provide the better explanation.

[20] 1001crash.com.

[21] John Fallis, a long-time resident of Lafayette and friend of the late David Jeansonne, confirmed these facts in correspondence (9/3/2021) and during a phone interview with me (9/4/2021). The location of the Corsair in Lafayette until July 2000 and its subsequent arrival at the Cavanaugh in 2002 is documented at the Warbird Registry (2019). In correspondence (8/31/21, 9/1/21) Terry Sherman of Sherman Aircraft Sales confirmed the details of the purchase of Jeansonne's Corsair in April 2000 on behalf of Paul Morgan in the UK and the Corsair being flown from Lafayette Regional Airport to Jacksonville, Florida, in July 2000.

[22] Several sources indicate that there were flight simulators at the event. "In addition to our featured acts and static displays, the air show will also have a wide range of activities for all ages! There will be several aircraft simulators, antique cars, helicopter rides, photo opportunities in various aircraft and much more!" Sertoma Cajun Air Festival (2001a). The September 2001 Press Release for the event stated: "A tremendous military and civilian static display as well as several simulators will round out the exhibitions" (Sertoma Cajun Air Festival, 2001b). A post-event news article in the local paper stated: "Members of the audience were allowed to sit in a flight simulator complete with video footage showing the view from the cockpit. They also met pilots for pictures and autographs" (Bier, 2001).

[23] Phone interview with Fallis (9/4/2021). Fallis said he doesn't recall James ever saying anything about Huston or Wildcats or anything about memories of a past life. But James was known as the kid at school who drew pictures of airplanes and knew all about vintage planes.

[24] The first in the series was *F-16 Fighting Falcon* (January 1993). Issue number 4 was *F4F Wildcat* (July 1995), number 7 was *P-51D Mustang* (January 1996), and number 8 was *P-40 Warhawk* (October 1996).

[25] This is not only implicit in Tucker's discussion in (2013) and (2016), at times it's explicit—for example, when Tucker says the Leiningers

knew of no (ordinary) way James could've learned a particular fact (Tucker, 2013, p. 83).

[26] Stratford (2012, 00:02:13–00:02:20). Stratford's interview was conducted in 2005.

[27] Unexplained Phenomena (2017, 00:12:17–00:12:27).

[28] A. Leininger (2009).

[29] The first trip to the museum was mid-February 2000 and the nightmares began sometime between mid-April and May 1, 2000 (cf. B. Leininger, 2021, p. 8). So, the time gap between the first museum visit and his nightmares was between two and three months, not four as Andrea Leininger says. But Andrea Leininger's reasoning is otherwise odd given that she accepts that James's nightmares incorporate his waking state experiences as Huston from March 3, 1945. That's a temporal gap of 55 years, considerably longer than a few months. If a time gap of a few months prevents a past event from influencing one's dreams, an event 55 years in the past shouldn't be able to influence a dream. These kinds of logical inconsistencies vitiate the Leiningers' storytelling.

[30] Hachette Book Group, 2009, 0:03:10–0:03:30.

[31] In correspondence (7/9/21) Bruce confirmed that this incident took place on Bruce's birthday in 2003, though he didn't acknowledge the Corsair video as the source of the statement he attributes to James. See below in text.

[32] Freeman, 2017, 0:31:22–0:31:29.

[33] TVDB, 2021, S01, E22.

[34] The video also provides details about the Corsair with archival WW2 footage. For example, this aviation video and others like it, discuss the difficulty of carrier landings—one of James's claims about Corsairs. Since Bruce Leininger is inconsistent about when James made certain claims, for all we know some of James's claims about Corsairs came after he saw this program, or perhaps from another video or television programs like it. There were many such programs on television during the late 1990s and early 2000s.

[35] Primary source documents composed during WW2 include *U.S.S. Natoma Bay Airport Action Report* (1945), *U.S.S. Natoma Bay CVE62 War Diary* (1945), as well as *the U.S.S Sargent Bay CVE-83 Action Report* (1945) and *U.S.S Sargent Bay, March 1945 War Diary* (1945). Bruce Leini-

nger credits Natoma Bay Association historian John DeWitt with providing him with copies of primary source documents and many other materials (SS, pp. viii, 166. 175–76). The Leiningers also describe *The Blue Book* as a makeshift, unofficial log the Natoma Bay crew compiled in the 1980s based on veteran recollections. The so-called *Blue Book*, also known as the LOGBOOK, is available at the U.S.S. Natoma Bay CVE-62 Historical Archive Website (2008–2014). Members of the Natoma Bay Association authorized the creation of the LOGBOOK in 1974. While it's true that the Blue Book contains veteran recollections of the war, much of its content is based on primary source documentation, as is illustrated in the content of portions of the LOGBOOK that cover the histories of the VC-81, VC-63, and VC-9 squadrons. See Wall and Sassano (n.d.).

[36] A. Leininger (2005).

[37] Jim Tucker confirmed in correspondence (08/06/21) that he understood the Leininger narrative to include these additional claims about Huston surviving the crash impact and dying by drowning. He seems to have had Andrea Leininger's version in mind in his description of this early-bird item.

[38] *U.S.S. Natoma Bay Aircraft Action Report*, p. 4.

[39] Thanks to Stewart Bailey for explaining this.

[40] In discussions with Stewart Bailey (correspondence 12/19/2019) about the details of Huston's crash, Bailey said that a 3-inch incendiary round hitting Huston's plane could have caused a brief flash, even a large fire burst. But the plane need not have caught fire, much less remained on fire as it glided into the bay. Hence, it's possible that the Avenger pilots were correct about seeing a flash but simply misinterpreted this as Huston's plane being on fire. However, the other claims by Durham and Skelton, inasmuch as they imply serious damage to the plane, cannot be reconciled with the AAR's denial of this.

[41] Since the report provides details on the altitude of Huston's plane at time of nosing over (1,500 feet), his speed (175 knots), the angle of dive (45 degree), and the distance across Futami harbor (1.15 miles), we can calculate how long he had before the plane hit the water. Stewart Bailey calculated that Huston had as little as 5.08 seconds but certainly not more than 20 seconds. We don't know at what precise point in the run Huston was hit (except that it was apparently as he

approached the harbor entrance), so this contributes to a generous 15-second window. Futami Harbor is 1.15 miles across, which would take 20 seconds to cross at Huston's speed, so that permits fixing a generous upper limit to the time he had. What's most likely is that he had 5 to 10 seconds from the time his plane was hit to its crashing in the harbor. That's barely enough time to bail under optimal conditions, let alone trying to kick open a jammed canopy as the Leininger narrative purports. Huston would have had to (i) realize he'd been hit sufficiently bad enough to bail out, (ii) unbuckle, (iii) reach for the pair of canopy release rings and pull, (iv) realize the canopy didn't pop open, and (v) attempt to push it open and/or reposition himself to kick it open, while the plane was in the 45-degree gliding descent at 175 knots. Perhaps this is doable if he had as much as 10 seconds, but not if he had less time (Bailey correspondence 1/9/2020).

42 Even this analysis is overly generous. Of the four confirmed claims, the first two make third probable. Also, if we take the alternative version of the claims attributed to James so that he said he flew a Corsair off the Natoma and died when his Corsair was shot down, the four confirmations reduce to two. So, of nine claims, the evidence only confirms two of them, disconfirms five of them, and two are neither confirmed nor disconfirmed. This is weak. Especially when we consider that of the two confirmed claims one is highly general and would likely be true even if the more specific claim about Jack Larsen was a chance hit.

43 There are various questions we have to consider in assessing the reliability of the testimony of war veterans. I discussed these issues with Stewart Bailey (correspondence 12/12/2019). It's not simply the amount of time that has passed between the events and later recollections, but the original experiences may have involved skewed perceptions—for example, due to youth, inexperience, and being in life-threatening circumstances. Bailey offered several examples of veterans who were sure they remembered incidents correctly, but which objective evidence subsequently demonstrated were incorrect. So, some degree of skepticism is warranted in accepting the testimony of the four Avenger pilots Bruce Leininger interviewed. That two of the Avenger veterans—Richardson and Skelton—were in poor health and the Leiningers' testimony is independently unreliable are further

reasons to be skeptical of the alleged testimony of the Avenger pilots. Allegedly Jack Durham (from the U.S.S Sargent Bay) wrote down the details of March 3, 1945, in an informal memoir (SS, pp. 240–241). However, even if Bruce Leininger has accurately reported the details of Durham's memoir, it would contradict the AAR. Bruce also claims that the war diary for the *U.S.S. Sargent Bay* supports Durham's account since the diary claims that Huston's plane was hit in the engine and the front exploded in a ball of flames (SS, p. 242). However, neither the war diary nor the aircraft action report for the *U.S.S. Sargent Bay* says what Bruce Leininger attributes to it here.

44 Although Tucker lists the early-bird claim as "Flew a Corsair," he indicates that "James seemed to be saying he was flying a Corsair when he crashed" (2016, p. 204). See also Judith Kroeger's article in the *Daily Courier* (Kroeger 2004), as well as Suzanne Stratford's 2005 news segment (Stratford, 2012, 0:01:36–0:02:20).

45 James never explicitly said a lot of things which the Leiningers are nonetheless quite comfortable attributing to him when it supports their reincarnation narrative—for example, the full name of the carrier *Natoma Bay*, that their son claimed to be *James Huston, Jr.*, that he died *during the Battle of Iwo Jima*, and that *Jack Larsen was James Huston's friend*.

46 That James implied he crashed while flying a Corsair is even clearer in the 2003 Chronology to be discussed in subsequent sections. Under the time block of September–October 2000, we find the following "key items obtained from James's dreams: 1) Plane was on fire, crashed in the water, and the man was trapped. 2) Plane took off from a boat on the water. 3) Plane was shot down by Japanese. 4) When asked if he knew who it was he said 'James' we thought he was talking about himself having the nightmares. 5) When asked if he knew what type of air plane. He said it was a Corsair" (2003 Chronology, 1). James didn't say *I flew a Corsair*. He gave the name *Corsair* in response to his parents asking him what type of plane he was flying in the dream in which the Japanese shot him down and he died.

47 The book was Wright (1999, p. 3).

48 Bruce provides this more contextually sensitive account in the 2004 *Primetime* program (Unexplained Phenomena, 2017, 0:08:26–0:08:50).

49 In Bowman (2010, p. 55) she says that, in their initial/early 2001

correspondence, the Leiningers mentioned James's nightmares, his "uncanny knowledge about flying World War II airplanes," and his naming the *Corsair*.

50 One might suppose that the claims the Leiningers say James made in fall 2000 actually *occurred* at that time, but they simply didn't *disclose* any of these extraordinary claims to Bowman until spring 2002. In that case, the claims attributed to James wouldn't have been "new details" or the "latest revelations" from him (SS, p. xiv). And the Leiningers would've had ample opportunity to correct the misleading impression, both in their conversations with Bowman in 2002 and in reading Bowman's "Foreword" to their book. But the scenario envisioned here is otherwise implausible. In her early correspondence with Bowman, Andrea Leininger gave every indication of wanting to inform Bowman of James's display of extraordinary knowledge, which Andrea took to be evidence that her son was "experiencing a past life" (SS, p. 116). If James had made the extraordinary claims the Leiningers say he made in late Summer and Fall 2000, it's implausible that Andrea would not have shared any of these gems with Bowman and instead just offer as evidence that he was obsessed with World War II airplanes and could identify them.

51 In her lectures, Bowman has said Andrea Leininger first contacted her in 2001. In personal correspondence (11/21/2021), Bowman told me that Andrea first contacted her in February 2001.

52 Honeywell, et al. (2020, 0:07:42–0:08:45). In correspondence (7/9/2021), I asked Bruce Leininger if I could see the original early correspondence between Andrea and Carol Bowman. He said he'd look for it but "it was 21 years ago." Taking him at his word implies the correspondence began in summer 2000. He has not responded to my follow-up queries. See Sudduth (2021).

53 Lucinda DeWitt claims her copy of the document is dated October 17, 2003, a month after the last events recorded in the chronology. Given that the Leiningers threw away their original notes, this may be the earliest extant written documentation of their story. In several emails I sent to Bruce Leininger between summer and early fall 2021, I asked him about any chronologies he might have composed in 2003 and whether any were sent to John DeWitt around this time. To date, Bruce has not responded, not even to deny that he is the au-

thor of the document. See Sudduth (2021) to read my last two emails to Bruce Leininger.

[54] "July–August 2000 Event: Issue of 'Past lives' surfaces. Andrea contacted Ms. Carol Bowman a specialist in past lives counseling. During this period Andrea spoke with her mother and her Mom who provided a resource she found on the internet a Ms. Carol Bowman. Carol had a practice on this phenomena. I was, at best, very skeptical. Andrea began a dialog with Carol. Based upon the guidance received Andrea began to talk to James about his nightmares in order to get him to stop the nightmares by using the techniques that Carol provided" (2003 Chronology, p. 1).

[55] See B. Leininger (2021, p. 12). If Bowman first gave advice to the Leiningers in February 2001, James's past-life claim would've been more than six months after the Leiningers had been telling him that his experiences were of events that had happened to him before. Curiously, in Bruce Leininger's prize-winning BICS essay (B. Leininger, 2021), Bruce says nothing about Carol Bowman's involvement in the case. He has expunged her from the entire narrative.

[56] Tucker (2016) doesn't include this item in his list of early-bird claims because it wasn't included in the 2002 *Strange Mysteries* program, but he does discuss it and includes a reproduction of the printout with the URL and date-stamp in the footer. Tucker (2013) provides a defense of this item as an early-bird claim. I'll discuss Tucker's handling of the Natoma claim in Section 7.

[57] 2003 Chronology, p. 2.

[58] This certainly gives the impression that the document wasn't on anyone's radar during the three-month production of the 2002 ABC program. This is also supported by the testimony I received in a phone interview with a member of the production crew who said they didn't recall the Leiningers showing them the document in question during the interview process or at any point thereafter during production, though the crew member vaguely recalled "Natoma Bay" subsequently being part of the Leiningers' official story.

[59] A reporter who interviewed the Leiningers in 2005 told me that, during her interview, Andrea Leininger mimicked James's stuttering and stammering to illustrate the difficulty James had articulating the word the Leiningers decided sounded to them like *Natoma*.

[60] I have added lowercase letters for items (1) and (3) since these are conjunctions of multiple claims, and it will be important to consider the claims individually as well as jointly.

[61] The testimony of the Avenger pilots about Huston's plane apparently being on fire doesn't sufficiently alleviate the difficulties here. First, it's possible to read their account as an interpretation of a brief burst of fire created by the shell impact to the plane, but not that the plane was on fire prior to hitting the water, as the Leiningers suggest. Second, if we cannot adjudicate the inconsistent testimonies, this aspect of the Leininger narrative would be neither confirmed nor disconfirmed. For reasons mentioned earlier, I favor the testimony of the primary source documents.

[62] Although in (2016) Tucker attributes to James the early-bird claim "died in the Battle of Iwo Jima," elsewhere he more accurately states the claim as "died at Iwo Jima" (2013, p. 78). The accompanying note he provides in (2016) indicates that Huston was killed *during* Iwo Jima operations on *nearby* Chichi Jima (2016, p. 204). This is his attempt to show that James's claim is somehow a partial match. But this is implausible. Iwo Jima and Chichi Jima are both islands in the Ogasawara archipelago in the Pacific Ocean, but this hardly makes them either the same location or even sufficiently similar to count as a match. If James had claimed to have died in a car crash in San Diego, California, and we later learned that the presumed previous personality died in a car crash in Santa Monica, California (about a hundred-and-fifty miles north of San Diego), James's claim would still be false, even though it would be true that Santa Monica is near San Diego and both are costal locations in California. Or suppose James had pointed to Gettysburg on a map of Pennsylvania and said *that's where/when I was shot and died*. Then we learned the presumed previous personality died of a gunshot wound at Mt. Pleasant, Pennsylvania (about 150 miles west of Gettysburg). James's claim would still be false, even if the death took place during the Battle of Gettysburg. It would also be false to say he died *in* the Battle of Gettysburg. If a falsehood can count as a (partial) match, it's unclear what couldn't count as a match.

[63] Assuming that Bruce Leininger's correction to the claim he attributes to James is accurate, he and Jim Tucker each ignore the quite plausible counterpoint that little James confused *with* and *when*,

hardly a surprising kind of mistake for a two-year-old.

[64] Stewart Bailey confirmed this in correspondence and referenced two of the most well-known Corsair experts—Barrett Tillman and Nick Veronico—who make no claim about flat tires as something unique to the Corsair in their WW2 aviation books. Nor is it mentioned in standard Corsair manuals, though other problems are discussed. I confirmed this in correspondence with Barrett Tillman (9/11/2021).

[65] Dial (1965, p. 9).

[66] Ibid.

[67] The URL in the footer of the Natoma Bay printout is http://metalab.unc.edu/hyperwar/USN/ships/dats/CVE/cve62.html. This URL turns up a 404 error at ibiblio. A search in the Wayback Machine at https://web.archive.org shows that sometime, no later than fall 2000, the document originally hosted at metalab was moved to https://www.ibiblio.org/hyperwar/USN/ships/dafs/CVE/cve62.html. Tucker checked the metalab URL in 2011 and discovered it wasn't active, but he found the same Hyperwar Natoma Bay document at ibiblio (correspondence 8/17/21).

[68] In personal correspondence (8/30/21), Tucker told me that he does not recall how the copy of the Natoma Bay printout was made.

[69] It's worth noting that in the 2003 Chronology Bruce says James gave the name Jack Larsen and said he flew with him. That's not the same as what the 2009 account says.

[70] Tucker (2013, p. 71) claims that Bruce Leininger's rejection of reincarnation influenced his early interpretation of *Jack Larsen* and led him to think James was dreaming about a pilot named Jack Larsen, not himself. This strikes me as a red herring. James's giving the name *James* as the name of "the little man" in the dream is naturally interpreted as James indicating that he—James Leininger—is the little man in the dream, and this is quite independent of whether one accepts or rejects reincarnation. Similarly, the ambiguity of the identity of *Jack Larsen* is baked into the claims James made. This also is independent of whether one accepts or rejects reincarnation.

[71] Hand (2014, p. 164).

[72] Hand (2014, p. 88).

[73] Thanks to David Hand for discussing with me the application of the improbability principle to aspects of the Leininger case.

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OBITUARY

Remembering Carlos S. Alvarado

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I have remote but clear memories about the first time that I met Carlos S. Alvarado. It was in 1972. He was 17 and I 19 years old. Let me explain how we met at so young an age. In 1972 I was a member of the Society for Psychical Research (SPR). By that time, I was intellectually precocious and already had about 200 books and many journals on psychical research and parapsychology. However, I felt completely alone: Nobody in Puerto Rico seemed to know the academic literature on those subjects. Therefore, I wrote to the secretary of the SPR asking if there was another member in Puerto Rico. At last! Yes! A person by the name of Carlos S. Alvarado. My first reaction was of total elation. However, on second thought, I was nervous and hesitant. I expected to meet a much older person who, seeing my young age, would dismiss me.

So I wrote Carlos a long letter where I explained that I was a young person but with a very serious approach to parapsychology. Rapidly he replied to me (remember, by mail; there was no email then). He really surprised me when he told me that he was nearly two years younger than I was! Not only that, our mothers had been friends when they were younger!

So, in that way, a friendship that extended for 49 years began. In this obituary, I want to concentrate my comments on those early years that were important in Carlos' decision to study parapsychology and to dedicate his intellectual life to the subject.

In the 1970s Carlos became my best friend. Every Saturday I visited him at his home and played chess (I never won) and we talked about the latest books or journals that he or I had acquired. In a letter that I wrote to him in 2012 I mentioned that I was remembering all the good times together. He wrote me back: "I too have good memories of our friendship and interactions. Do you remember when we played chess? . . . Many other memories have come to my mind as I write this."

A curious aspect of our interest in parapsychology was that both of us were especially interested in the study of mediums (both physical and mental) and in spontaneous cases. Of all the mediums that we discussed, the one that seized our attention was Eusapia Palladino. Therefore, we began to collect a lot of information about Eusapia. We shared a very positive outlook on her physical phenomena. In addition, we were curious and impressed by the work of Dr. Ian Stevenson. We both took a special interest in his work with reincarnation cases and telepathic impressions.

Approximately in 1973 we decided to publish a journal on psychical research with the name *Explorando lo Paranormal* (Exploring the Paranormal). The journal was important for us, as it permitted us to elaborate on our thinking about the subject and receive feedback from other professionals interested in parapsychology.

One of our favorite authors at that time was D. Scott Rogo. That was precisely because Rogo's approach was consistent with our interests. Rogo wrote extensively about mediumship, poltergeists, and spontaneous cases. In addition, we were truly impressed by his historical knowledge of the field. We both enjoyed reading his book: *Parapsychology: A Century of Inquiry* (1975). Therefore, when Rogo was killed in 1990 we both were deeply saddened by his untimely death at the age of 40.

With Carlos I shared very special moments. We wanted to document or see some paranormal phenomena. Therefore, we both investigated some spontaneous apparitional cases, a poltergeist case, visited and slept at a haunted house, and, with a group of people interested in UFOs, we slept in desolate mountains where it was said that there were UFO landings. Despite all our efforts, we never encountered a single paranormal event that convinced us.

It is important to mention that Carlos' father was a well-known public figure in Puerto Rico, especially in certain political parties. His father was truly interested in the education of his two sons. In fact, Carlos' brother (Alberto) studied architecture and eventually worked with his father. However, Carlos was more inclined to the social sciences. Therefore, he completed his BA in psychology at the University of Puerto Rico. He was fortunate enough that by that time there was an elective course on parapsychology, so he took it and he discovered that he was more knowledgeable about the field than the professor was. After finishing his BA in psychology, Carlos told me that he would not study clinical psychology, because he was not interested in psychotherapy. His real interest was in parapsychology. Therefore, he communicated his decision to his father and mother. His father was concerned with his decision. He believed that parapsychology was not a prestigious field and he had doubts about the financial future of his son. Nevertheless, he respected Carlos' decision, and always supported him.

By that time (1978), John F. Kennedy University was offering a Master's Degree in parapsychology. Many times we evaluated the possibility that Carlos would move his residency to the USA and his prospects of dealing with a profession that was uncertain in terms of financial and professional prestige. Eventually he decided to study at John F. Kennedy University. I remember that when I saw him leaving Puerto Rico I began to cry in my car. I recognized that our intimate and constant interactions would never be the same. Long-distance relationships are a challenge.

While he was in the USA we communicated by telephone and by letter. Eventually Carlos returned to Puerto Rico. He wanted to reside in Puerto Rico, but soon he discovered that the door to the field of parapsychology was closed there. He went to different research centers and universities to offer courses on the subject. Only one university (Carlos Albizu University) offered him the opportunity to give a single introductory course on the subject. Therefore, he became frustrated with the situation in Puerto Rico and eventually understood that he had no chance to progress and prosper on the Island.

I also remember when Carlos and Dr. Nancy Zingrone met in the USA. I believe they began to know each other in 1983. They shared so many interests and developed a unique and extraordinary bonding that lasted until his death. When Carlos talked about Nancy with me, he never expressed any complaints. On the contrary, he admired her intellectually and I can say that she was the love of his life. There is no doubt about that.

I enjoyed writing some articles with Carlos (Martínez-Taboas & Alvarado, 1981; Alvarado & Martínez-Taboas, 1983, 2002), and in Puerto Rico we participated in some joint conferences, mostly about spontaneous cases and mediumship. It is no wonder that his only book was on Charles Richet, who was well-known for his interest in mediums and in Eusapia Palladino.

As I write this obituary, I remember Carlos's face. He was usually playful, full of joking comments, and very respectful toward his intellectual opponents. I remember his incisive critical skills, his prodigious memory, and his passion toward the subjects he wrote about. We will remember him for his numerous writings about the history of psychical research and parapsychology. Carlos was one of the most systematic and respected historians in the field of psychical research, in my opinion as prominent as Dr. Alan Gauld. In addition, he will be remembered for his kindness toward all those fortunate persons who knew him. He will be sorely missed.

I close my comments with an anecdote. In the 1970s, Carlos had a stuffed serpent in his room that we called "the boa." We had a lot of fun with it; in fact, the last time that we talked by telephone we joked about it. In my house, I have a wooden serpent made in Indonesia that I also call "the boa." Eight days after Carlos died, the wooden figure of the boa, which is in a secure place, inexplicably fell to the floor. I just thought, is that Carlos communicating with me? I will never be sure. Nevertheless, what is beyond doubt is that I have had the privilege of knowing an excellent man and scholar.

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ESSAY REVIEW

THE Most Important Book about Climate Change

Unsettled: What Climate Science Tells Us, What It Doesn't, and Why It Matters by Steven E. Koonin. BenBella Books, 2021. 320 pp. (hardcover). ISBN 978-1950665792.

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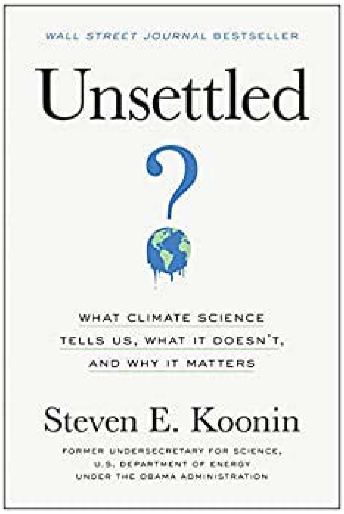
If you want to know the facts about global warming and climate change, if you want to reach your own informed opinion about what should be done about it, this is the book to read. All relevant facts are presented clearly, very much including the historical records together with scrupulous citing of primary sources, among which the official published reports, international and national, feature prominently.

The author's credentials for this work could hardly be more impressive. Steven Koonin has been a successful physicist, including pioneering work on the use of high-performance computers in simulation and modeling. He gained some insights from the administrative role of Vice President and Provost at Caltech; another highly relevant experience as Chief Scientist for the oil company BP, focusing on renewable energy possibilities; and he was an Undersecretary for Science in the Department of Energy in the Obama Administration, focusing on energy technologies and climate science. He understands the viewpoints of scientists, of government, and of industry; in other words, he knows whereof he speaks.

Throughout the book, Koonin takes deliberate, explicit care to write dispassionately and factually. At the outset, he acknowledges that the



path to convincing any wide audience calls for arousing emotion, so that scientists are inevitably in a dilemma: whether simply to describe the facts and hope for appropriate attention to them, or whether to find emotionally impressive ways of spinning and hyping the facts. Koonin takes the first course quite effectively. For example, "the science doesn't support what's portrayed in most popular discussions" might also have been expressed as, say, "We are being massively misled about so-called climate science, by media, pundits, and official climate scientists." Thus the book's careful, factually based wording may easily seem incongruous as it describes unwarranted and unscrupulous mis-stating of facts, spinning, and hyping, which seem to be an integral part of official "consensual" "climate science," to which the book refers as "The Science" to contrast with actual evidence-based science.

Still, it emerges inescapably that essentially everything that the media, the pundits, the official experts, and politicians everywhere disseminate is simply untrue. Koonin (2014) had summarized his discussion already seven years ago in a 2000-word piece in the *Wall Street Journal* without bringing any change to the pervasive, continual, public misleading by official sources, the media, NGOs, activist groups, self-interested "climate scientists," and of course politicians. The official assessment reports are not transparent, and sometimes even wrong about what the science actually says (p. 100): Summaries in the reports, and government and media and press releases, continue to disseminate assertions that do not accurately reflect the data in the reports themselves (p. 4). Koonin points out that all this misleading "serve[s] the interests of diverse players including environmental activists, the media, politicians, scientists, and scientific institutions" (p. 12).

Unsurprisingly, therefore, predictions made by official experts have quite frequently turned out to be wrong (p. 9).

Koonin's thesis in a nutshell: Yes, human activities influence climate. Yes, the globe has warmed recently. However, to the present time the effects of human influences have been negligible by comparison to natural forces. All the hysterically hyped prophecies, the concept of an "existential crisis," are based on invalid models. Many informed scientists agree with Koonin's thesis but will not do so publicly (p. 5).

The computer models of "The Science" are demonstrably inadequate (Chapter 4, "Many muddled models"). They are unable to replicate over any appreciable length of historical time the actual facts about temperature, carbon dioxide, or anything else. Koonin enables us to understand why, by explaining the host of variables involved, our ignorance about the many relevant natural forces and variables, and the fact that there is considerable variation in weather and climate between different regions of the world. Furthermore, modeling *changes* calls for having an accurate initial baseline, and we do not have sufficiently detailed local and regional data to establish such a starting point. So current models involve "tuning" of parameters to make a better fit with actual data; which in other contexts might be described as inserting fudge factors to hide inadequacies. Results from the various models differ dramatically both from one another and from observations. But you wouldn't know that unless you read deep into the United National Intergovernmental Panel on Climate Change (IPCC) reports (p. 86).

The globe warmed from 1910 to 1940 at about the same rate as in the late 20th century; but the computer models calculate a rate only half of what was actually observed (pp. 88–89). Models in 2014 were no better than in 1979 at calculating climate sensitivity; and "the models are even worse at describing changes in regional climate than they are at describing changes in global quantities" (p. 145).

The very concept is a fairy tale, of some average global temperature or of a global warming that would impact all parts of the world in anything like a similar fashion; how very differently warming has taken place since 1986—there was even *cooling* in some regions—is illustrated in figure 1.5 (p. 37).

There can be no substitute for reading the whole book. Here are just some of the noteworthy points, all of them described in the book in detail and with scrupulous citing of sources:

"[T]he science is unsettled enough that *any* unusual weather can be 'attributed' to human influences" (p. 178).

"[T]he increasing concentration of carbon dioxide has been a significant factor in [agricultural crop] yield improvements" (p. 173).

"[H]eat waves in the US are now *no more common* [italics in original] than they were in 1900 . . . the warmest temperatures in the US have not risen in the past 50 years. When I tell people this, most are incredulous. . . . and some get downright hostile (p. 1)."

"Observations extending back over a century indicate that most types of extreme weather events don't show *any* significant change—and some such events have actually become less common or severe" (p. 97).

"[T]he science says that most extreme weather events show no long-term trends that can be attributed to human influences on the climate" (p. 99); "temperature extremes in the contiguous US have become less common and somewhat milder since the late nineteenth century," but official reports claim the very opposite, that temperature records in the US are becoming more frequent (p. 107).

"The coldest temperatures . . . [are] rising more rapidly than the warmest temperatures—the climate is getting milder as the globe is getting warmer" (p. 36).

As to hurricanes, "the assessment reports . . . present summary 'spin' inconsistent with their own findings" (p. 112). An apparent increase in the number of annual tornadoes is most likely owing to steadily increasing ability to observe weak tornadoes that would have failed to be recorded in the many decades before weather radar came into wide use (p. 122 ff.); "if anything, US tornadoes have become more benign as the globe has warmed over the past seventy-five years" (p. 126). Humans have had no detectable impact on hurricanes over the past century.

Human influences currently amount to only 1% of the energy that flows through the climate system (p. 58); "Water vapor . . . accounts for more than 90 percent of the atmosphere's ability to intercept heat" (pp. 50–51).

> Overall and on average, there has been no increase in flooding in the U.S. in the past century; but some regions have seen more flooding while others have seen less (fig. 7.7, pp. 137–138). "There is not very much changing very quickly with precipitation, either globally or in the US" (p. 147).
>
> So too with droughts, for the globe as a whole: "*low confidence* in a global-scale trend in drought or dryness since the middle of the 20th century" (p. 138); indeed, "high confidence for droughts during the last millennium of greater magnitude and longer duration than . . . since the beginning of the 20th century in many regions" (p. 140).
>
> The oceans are the climate's long-term memory holding more than 90 % of the climate's heat (p. 38).
>
> "[W]e don't know how much of the rise in global sea levels is due to human-caused warming and how much is a product of long-term natural cycles" (p. 165). Over the past 400,000 years, sea levels have varied over a range of more than 150 meters (fig. 8.1). The summaries in official reports are as misleading about sea-level changes (p. 157 ff.) as they are about extreme weather events.
>
> Greenland's ice sheet isn't shrinking any more rapidly today than it was eighty years ago.
>
> The net economic impact of human-induced climate change will be minimal through at least the end of this century. "The consensus on the minimum overall economic impact of rising temperatures is well known to experts [to which] a prominent environmental policy maker [responded]: 'Yes, it's unfortunate that the impact numbers are so small'" (p. 180).

National policies can also be counterproductive as to controlling emissions by going electric: The US imposed tariffs that increased the cost of solar panels, and the EU imposed substantial import duties on energy-efficient lightbulbs (p. 234).

That official statements, and the summaries of official reports, misrepresent the detailed data in those reports "might be due to incompetence, but I suspect otherwise" (p. 109). The UN framework convention had hijacked the very term "climate change" by *defining* it as "a change of climate which is attributed directly or indirectly to human activity" (p. 35).

Many other devices have also been deployed to prevent questioning of that. For instance, figure 1.1, showing the familiar "hockey-stick" of rapid increase in temperature from about the 1960s, illustrates how misleading an impression a graph can deliver by choosing the scale used: the graph shows "temperature anomaly," *deviation* from average, in tenths of degree; yet the *total global warming from 1850 to 2010* is barely 1 °C, and there were two periods of actual *cooling* (~1880–1910 and ~1940s–1970s).

An article (Ghebreyesus, 2020) by the Director-General of the World Health Organization attributes to climate change "deaths [that in reality are] due to ambient and household air pollution . . . Such brazen misinformation by the WHO's leadership is particularly upsetting for its potential to diminish confidence in the organization's vital public health mission" (p. 171).

Chapter 9 discusses in detail three "Apocalypses that ain't," namely (p. 167 ff.):

> That "'climate-related deaths' [are] a menace" [is] an assertion "based on speculation, strained assumptions, and incorrect use of data."
>
> That there is impending "a future agricultural 'disaster' . . . is belied by the evidence and requires acrobatic distortion to even detect."
>
> "[P]urportedly 'enormous' economic costs . . . turn out, even based on the data presented, to be minimal, if not too small to measure".

Chapter 11 gives Koonin's ideas for "fixing the broken science." He recalls that since a workshop in 2014, he had been "tracking the misrepresentation of climate science by the media and politicians . . . and . . . was freshly irked by the misleading presentation of hurricane data in the 2014 National Climate assessment . . . described in Chapter 6."

Koonin's chief proposition is the need for a "Red Team exercise" whereby "a group of scientists would be charged with rigorously questioning one of the assessment reports, trying to identify and evaluate its weak spots . . . a qualified adversarial group would be asked 'What's wrong with this argument?'" And, of course, the 'Blue Team' (presumably the report's authors) would have the opportunity to rebut the Red Team's findings. "Red Team exercises are commonly used to inform high-consequence decisions such as testing national intelligence findings or validating complex engineering projects . . .; they're also common in cybersecurity" (pp. 197–198).

Much of Chapter 11 describes many evasions and political distractions that illustrate the mainstream's unwillingness to engage in meaningful substantive discourse; the book cites a number of typical invalid mainstream polemical devices:

> Referring to a dissenting scientist as "denier" or "alarmist."
>
> Claiming a 97% consensus among scientists.
>
> Confusing weather with climate.
>
> Omitting numbers.
>
> Quoting alarming quantities without context.

Finally, in typically even-handed fashion, Koonin recommends two blogs "that seriously and consistently cover recent climate science": real climate.org for the mainstream, judithcurry.com for the "non-consensus point of view."

Chapter 12 explains why the concept of a carbon-free environment is a chimera, unattainable in practice; and data are cited to illustrate that much of the political discourse about restraining carbon emissions is simply silly in light of the actual facts. The following Chapter 13 points out further that changes in the mode of energy generation entail or require changes in many other things as well: sources and distribution of fuel, consumer behavior, shifts in costs and prices of *everything*. Carbon-generating modes of energy generation would have to be phased out *slowly* as the more desired forms are introduced: There can be no significant change in short order: "energy supply facilities such as power plants or refineries require large up-front investments and last for decades (over which those investments are gradually paid off)" (p.

228). Changes also impinge on the interests of "many different players: industry, consumers, governments, and NGOs. For example, there are often yearslong disputes over the routing of pipelines and the siting of power plants" (p. 229).

Chapter 14 discusses alternatives to the attempted control of carbon emissions: geoengineering or adaptation. "I soon discovered that any mention of geoengineering to governments or NGOs was met with tight-lipped silence, if not actual hostility. The focus was on reducing emissions, and any distraction from that goal, especially one that could allow the world to continue to use fossil fuels, was not to be contemplated" (pp. 238–239).

Geoengineering could include changing the reflectivity of the Earth to cut down on sunshine and capturing carbon dioxide and disposing of it. Adaptation, which Koonin seems to regard as the most feasible approach, has the advantages (pp. 245–246) that

> it can be effective whether climate change is natural or human-influenced: "Humans have been successfully adapting to changes in climate for millennia . . . without the foggiest notion of what . . . might be causing them. . . Societies have thrived in environments ranging from the Arctic to the tropics.
>
> it would be realistic, proportional to need.
>
> it would be local, and "naturally tailored to the different needs and priorities of different populations and locations. This also makes it more politically feasible."

It's clear that media, politicians, and often the assessment reports themselves in their executive summaries, misrepresent blatantly what the science says about climate and catastrophe. Chapter 10, "Who broke 'The Science' and why," discusses "The perfect storm of interests that leads to a fervent belief in a consensus that isn't" (p. 183).

In just a few places, Koonin does allow himself some righteous wrath:

> "I have no problem with activism, and the efforts of NGOs have made the world better in countless ways. But distorting science to further a cause is inexcusable, particularly with the complicity of

> those scientists who serve on their advisory boards" (p. 194).
>
> "I've been dismayed . . . by the willingness of some climate scientists—abetted by the media and politicians—to misrepresent what the science says, and then by the many other scientists who are silently complicit in those misrepresentations" (p. 249).
>
> "I think we should begin by restoring integrity to the way science informs society's decisions on climate and energy" (p. 255).
>
> "As a scientist, I'm disappointed that so many individuals and organizations in the scientific community are demonstrably misrepresenting the science in an effort to persuade rather than inform" (p. 196).

LESSONS BEYOND CLIMATE CHANGE

Koonin has begun to learn what the fate is of even the most judicious and well-informed critic of an official consensus: questioning of his credentials, personal attacks, criticism by former friends and colleagues (pp. 14–16).

I say only "begun" to learn because nowhere does the book show awareness that it is far from uncommon for official sources to publicly disseminate mis-information on important matters (Bauer 2012, 2021); this is not restricted to global warming and climate science.

Active researchers, specialist experts in particular fields, tend to be fully occupied with exploring and seeking to expand what is known and understood in their particular field. In the normal course of events, scientists assume that they can rely on what the consensus is in other fields of science. Individual scientists, particularly the most original ones, occasionally come up against the fact that the system is not working how it should and as they thought it did: Few active researchers have not experienced incompetent or unfair peer review, without drawing the extrapolated conclusion that the system of peer review is in and of itself no guarantee of objective reliability.

An awareness of the inadequacies, weaknesses, and dysfunctions throughout the scientific community as a whole is common not among active researchers in The Sciences but among outsiders who take an interest in scientific activity and its relationship to the wider society: historians, philosophers, sociologists, political scientists, what nowadays has matured into the discipline of Science & Technology Studies (STS); as well as "anomalists," people who take an interest in topics that mainstream science ignores. Every student of any science, and all active researchers, should be required to read Bernard Barber's (1956) discussion of "Resistance by scientists to scientific discovery".

Unsettled is a marvelously substantive, fact-respecting work—rather to my surprise, as I had been anything but a Koonin fan three decades ago when he had been prominent among the physicists who had pooh-poohed the report by Fleischmann and Pons of finding nuclear processes at ordinary temperatures in electrochemical cells ("cold fusion"). Physicists everywhere were unable to reproduce the reported findings—ignoring that non-experts doing a few months of electrolysis might not be properly replicating the work of long-experienced, expert, electrochemical specialists. There remain scientists all over the world who have achieved results analogous to or comparable with the Fleischmann-Pons claims (Goodstein, 1994); the phenomenon is apparently highly sensitive to the state of the solid electrode, and the need to get it occupied to by deuterium to greater than or equal to ~85% of the number of palladium atoms in the solid lattice of the electrode; and the overvoltage applied is also critical—theory suggests that can mimic something like 10^{27} atmospheres of pressure (Bauer, 1990). Koonin, like all of us, can be spot-on right on some matters but mistaken on others.

In calling for a "Red Team" exercise on climate-change calculations and projections, Koonin acknowledges that "Of course, both the UN's IPCC and the US government claim that their respective assessment reports are authoritative because they are already subject to rigorous peer-review before publication" (p. 198). Exactly. As I have been arguing for some time, proponents of a mainstream consensus simply refuse to engage with critics, which is why a Science Court is needed, with the authority to *force* substantive, public engagement on issues of public importance (Bauer, 2017, 2021).

The publishers are to be congratulated for the brave publication of a book that demolishes the conventional wisdom about global warming and climate-change dogma; but there is one highly annoying feature: All figures are black-and-white, making it in some cases impossible to decipher differences between different shades of gray among half-a-dozen areas or lines.

This book should be read by every journalist, pundit, and environmental activist, and by all politicians, policymakers, as well as their staffs.

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BOOK REVIEW

The Believer: Alien Encounters, Hard Science, and the Passion of John Mack by Ralph Blumenthal. High Road Books, an Imprint of the University of New Mexico Press, 2021. 330 pp. $29.95 (hardcover). ISBN 9780826362315.

Reviewed by Thomas E. Bullard
Bloomington, Indiana

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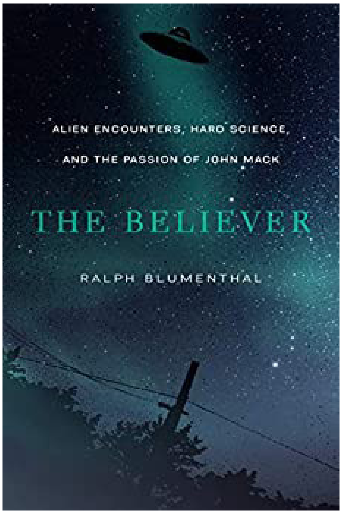
I remember the golden era of UFO abduction research. In 1966 John G. Fuller published *The Interrupted Journey* and suddenly everyone seemed to know the strange story of Barney and Betty Hill taken from their car by aliens and subjected to medical examinations aboard a flying saucer. The whole town talked about it, so did the whole country. Only ufologists held back, having learned the hard way that the more sensational a UFO story sounded, the more likely it stood on evidential legs too slender to support it. UFOs enjoyed newsworthiness and unaccustomed respect this year; it was no time to puncture the swelling goodwill by falling for a fake, fantasy, or honest mistake (Bullard, 2018).

Still, this case was different. The Hills were highly respected in their community, Fuller was a *Saturday Review* columnist, and Dr. Benjamin Simon, the psychiatrist who recovered memories of the Hills' period of missing time by means of hypnosis, was a pillar of his profession. Incredible as the story sounded, it unfolded with credible verisimilitude. It told us more about the aliens and their spaceship than any previous trustworthy report. It even made sense according to the prevailing conception that aliens arrived as explorers like our astronauts, their mission to observe, collect soil samples, and doubtless to study the numerous large mammals having the technology to ride around in four-wheeled machines.

At first a curious one-off, the Hills' encounter soon acquired companions. A search of past files uncovered several accounts with comparable features, while a slow trickle of new cases picked up speed in 1973 when the alien kidnap story of Charles Hickson and Calvin Parker and in 1975 when Travis Walton's five-day absence aboard a UFO made national headlines. In-depth investigations by Ted Bloecher, Jerome Clark, Ann Druffel, Raymond Fowler, Leo Sprinkle, David and Walter Webb added American cases to the literature, with further examples coming from Canada, Europe, Australia, and South America.

Abductions had leaped to the forefront of ufological interest by 1980 thanks to Budd Hopkins, an established New York artist whose own sighting first piqued his interest in UFOs. While apprenticed to Ted Bloecher as an investigator of close encounters, he soon discovered that missing time followed by acute anxiety often marked the witness as an abductee. Hopkins teamed with a clinical hypnotist to follow up this clue and discovered an "invisible epidemic" of cases, which he documented in his 1981 book *Missing Time*. A little later an Indianapolis woman contacted him with an abduction story that took a sensational new turn. She related a succession of abductions as aliens removed ova from her, implanted a fertilized egg, stole the fetus during her pregnancy, and finally introduced her to a frail little girl with wispy hair and large eyes, her hybrid alien-human child. Hopkins told this thrilling story in *Intruders* (1987) and replaced the old narrative of aliens as explorers with the dark alternative that they had come here to exploit humans for purposes of their own.

Hopkins acted as a tireless advocate for the abduction phenomenon. He was a spellbinder in speaking and writing who transformed the incredible into the plausible, a non-scientist who nevertheless recognized the importance of physical evidence like distinctive cut marks on the bodies of abductees and small hard bodies implanted by alien examiners. He exercised a magnetic attraction on people who would become his informants, supporters, followers, colleagues, and patrons. When famous horror-fiction novelist Whitley Strieber suspected he had experienced abductions, he turned to Hopkins. Temple University history professor David Jacobs, initially skeptical but soon converted, became a close and avid co-worker. Hopkins won the support of wealthy donors for conferences and research, enabling increasingly sophisticated meetings that drew academics and professionals willing



to consider there might be something to this abduction business after all.

The year 1987 saw publication of Hopkins' *Intruders* and Strieber's *Communion*, a book long on the best-seller list. Together they pulled abduction out of the shadows of the UFO subculture and into the center of popular awareness. The public took interest and cases emerged by the hundreds, even to the point that local support groups for abductees organized around the country. Artists and actors flocked to Hopkins, academics and professionals to Jacobs, truck drivers, housewives, and farmers to a ufologist in southern Indiana, the abductees reflecting the demographics surrounding the investigator and representing a cross-section of society. Standard psychological tests affirmed that most abductees were normal, free of mental pathology, likely to share some characteristics of people known to have suffered a traumatic experience. Abduction stories maintained a similarity in plot and details that belied the variety expected of imaginary narratives, down to obscure elements present before the story became familiar and seemingly unavailable from mass media influences. The surge of clues that this phenomenon was real, widespread, and unearthly swept up many proponents in expectations of a climactic and world-changing epiphany.

As the subject rose in popularity, skeptics amplified their attacks on its credibility, the alleged evidence, and the hypnotic techniques used to recover the memories. The phenomenon needed a stamp of approval from official science to validate its reality, a recognition that ufologists had long craved for anything about UFOs. The next best thing would be for someone famous to step forward as an abductee and Strieber gave cause for celebration, though skeptics doubted that the writer could separate his imagined from his alien horrors. "Why couldn't Carl Sagan get abducted?" became an insider joke. With no sign that Sagan had any secrets to spill, an alternative tactic hoped the available evidence, suitably presented, might win over high-level scientists to investigate the phenomenon. And where would better suit such a presentation than the Boston area's neighboring high bastions of academia, MIT, and Harvard?

This dream began to materialize early in the 1990s as David Pritchard, a distinguished research physicist at MIT, became intrigued with abductions and settled on a large-scale conference for exchange of ideas among active researchers. Pritchard sought to balance his expertise in physical science with an expert to represent the psychological aspects of the phenomenon and engaged John Mack, a professor of psychiatry at the Harvard Medical School, who joined the venture as co-chairman. The Abduction Study Conference Held at MIT came to fruition June 13-17, 1992, a professionally organized conclave that met in a campus auditorium filled with abductees, ufologists, psychologists, sociologists, medical professionals, religion scholars, folklorists, select members of the press, and a skeptic or two swimming against the tide (Pritchard, 1994).

I well remember John Mack—tall, striking, slightly stooped, prone to gestures as he spoke with a clear voice and informal eloquence. His quiet authority owed little to position or reputation, much to instant respect freely given as his agile, far-ranging mind convinced us that he saw more and understood more of this phenomenon than anyone else in the room. He did not so much command attention as win it from a willing audience, the cavernous auditorium hushed and still as he told how a friend introduced him to Budd Hopkins two years before, how he thought anyone who believed in abduction by aliens must be crazy. He came away so impressed with Hopkins, the experiencers, and their accounts that he undertook his own investigations and they convinced him that something real had happened, something with momentous meaning for the world. Most of us arrived at the conference in agreement with those words. Many of us departed with certainty because we had John Mack's word for it.

He would continue his exploration of the meaning of UFOs, writing two books on his abduction research, discussing the subject with the Dalai Lama, visiting Zimbabwe to interview school children who shared an encounter, and establishing the Program for Extraordinary Experience Research (PEER). His efforts came to a sudden and shocking end in 2004 when a drunken driver in London killed him. Though I met him face to face only a few times, his death came as a heavy blow. I felt that a rare and beautiful soul had departed, that the world had dimmed a degree and faded a shade with his passing.

So who was John Mack and why did he leave such a deep impression? I knew of his involvement with UFOs and that he won a Pulitzer Prize, but I did not appreciate the breadth of his remarkable career until I read Ralph Blumenthal's *The Believer*. Blumenthal holds credentials of his own with the UFO story. Along with Leslie Kean and Helene Cooper, he co-authored *New York Times* articles in December 2017 that revealed the Pentagon's Advanced Aerospace Threat Identification Program and familiarized the public with the U.S.S. *Nimitz* sightings from 2004—and in the process, inaugurated a renaissance of attention to UFOs (Cooper 2017). He took an interest in the Harvard psychiatrist whose espousal of the discredited subject of UFO abductions led to a passion in two senses of the word, one a driving quest to understand the meaning of this mystery, the other an academic trial for heresy. With the cooperation of family, friends, and colleagues, Blumenthal gained access to Mack's papers, notes, and journals for a thorough overview of the life of the man, a record of his thoughts, and insights into the motivations that drew his ever-questing mind toward heterodox ideas.

Blumenthal outlines Mack's career as a dizzying whirlwind of activity: A stellar medical student who rose quickly to prominence in his chosen field of psychiatry, Mack stood out even in the Harvard faculty where outstanding was the norm. He published a stream of papers and books as befits a Harvard professor, ranging across subjects like nightmares, adolescent suicide, Holocaust survival, self-esteem in childhood, and the impact of nuclear fear on children. His psychiatric study of Lawrence of Arabia resulted in *A Prince of Our Disorder*, the biography that earned him a Pulitzer Prize in 1977. In Mack's successful practice treating patients for anxieties and disorders, his coworkers praised him for his innate empathy and humanity.

Along with his work as a clinician, teacher, and scholar, Mack was also an organizer and activist. He established the Center for Psychological Studies in the Nuclear Age (later the Center for Psychology and Social Change, still later the John E. Mack Institute), PEER, and the Group for Research and Aid to Abductees (GRAA), among others.

Harvard's medical facilities were of the finest, but not so those of the neighboring Cambridge City Hospital until Mack spearheaded a drive to create psychiatric and mental health services that not just met basic needs of city residents but won prizes for excellence. He entered informal diplomacy for peacemaking between Israel and the PLO that culminated in a meeting with Yasir Arafat and he acted in an international physicians' group to protest nuclear weapons, delivering impassioned defenses of physicians entering an arena usually reserved for politicians. He was arrested along with Daniel Ellsberg, who released the Pentagon Papers, and Carl Sagan in peace protests outside a Nevada nuclear test site. For him there was no hard line between advocacy and the academic.

Budd Hopkins marveled that Mack succeeded in everything he attempted and accomplished so much with seeming effortlessness and self-confidence. Others close to Mack saw less the superman and more a vulnerable human wounded by the death of his mother when he was eight months old and driven for the rest of his life on a quest to recover that loss, to reestablish the primal connection that fate had denied him. This need led him to a "succession of passionate enthusiasms"—health services, world peace, UFO abductions, and finally life after death, a sequence of causes he believed in with emotion and intelligence but, in the case of UFOs, with impractical rashness.

It comes as no surprise that a psychiatrist would explore his own motivations, and Mack found sources for his affinity with T. E. Lawrence in certain parallels of their biographies. Lawrence needed to act heroically, so did Mack, manifested in his strivings to help the emotionally damaged and save the world. In his life he acted out the mythic journey of the hero as described by Joseph Campbell—leaving behind the familiar to journey into the unknown, undergoing an initiation of transformative trials and ordeals, becoming a new person with powers and wisdom that he brings home to benefit his people. The experiences reported by UFO abductees shared many aspects of the initiation of the hero or the shaman, similarities already important to Mack. Strange as they were, abduction stories beckoned him at once with their familiarity and significance, cosmic significance. The stars aligned—and Mack set off on a new hero's journey.

Though UFO abductions entered Mack's purview late in his career and occupied him only for the last fourteen years of his life, this episode was fraught with such exotic and dramatic qualities that Blumenthal understandably centered his book around it. Here was Mack at his most characteristic, the passionate pursuer heading off on a quest for meaning, this time as a modern Don Quixote tilting at consensus science. He was in many respects an irrepressible optimist who saw in the abduction phenomenon an opportunity to punch through established dogma and reach new understandings. Perhaps he also envisioned this crusade as the climax of his life's work, but his euphoria blinded him to the practical consequences which would in fact provide a climactic moment in his biographical story, only not a triumphal one.

After their meeting in early 1990 Mack absorbed the abduction work of Hopkins and began his own sessions with experiencers. Fellow psychiatrists tried to dissuade him; Sagan reasoned with him against the reality of these accounts. Thomas Kuhn, a fellow Harvard professor who had literally written the book on scientific revolutions, warned him that scientific authorities would resist any challenge to their established paradigms with a fierce resolve, and advised him to proceed with constant self-criticism and always triple-check his evidence (Kuhn, 1970). He plunged onward instead, working with a growing number of abductees as more haunted people learned of his interest and sought his help, buoyed by his excitement despite the emotional strain. A quick conviction that physical and clinical evidence converged to confirm a real phenomenon sustained him, while his ideas of what abductions were all about evolved into increasingly panoramic forms.

Mack had no temperament to keep silent about his strange subject. He went public to the Harvard community in 1991, presenting his work-in-progress to an informal faculty meeting where responses ranged from doubt to hostility, then to a packed audience where revelations of fetus-stealing aliens and tapes of abductees screaming in terror bombed out the heads and hearts of listeners and sent them reeling from the auditorium. The credibility of UFO abductions reached high tide with the 1992 MIT conference. Abductees received respectful attention as normal people recounting extraordinary yet genuine experiences. Mack defended their accounts as consistent, backed by physical evidence, and reported by children too young to be delusional or influenced by popular culture. Speaker after speaker tossed further bits of support onto the growing pile. Many participants carried home a sense that, against all odds, this evidence and its leading proponents carried the momentum to overwhelm all doubters and deniers.

Meanwhile back at Harvard, friends and colleagues worried that Mack had lost it, stepped over the edge, gone crazy. Yet few negative consequences had befallen him so far. The optimist in him counted the professional criticisms he had received as a rite of passage that freed him to continue and he imagined he faced no enemy but his own hesitancy. He did acknowledge the need to publish his work in the professional literature and submitted an article on the "abduction syndrome," rejected for its hundred-page length and inadequate treatment of methodology. The editor of another major journal replied that the subject matter made the article unpublishable and returned it unread. Mack was too impatient for revisions and expanded the article into a book which appeared in 1994, *Abduction: Human Encounters with Aliens*, heralded by publicity and TV appearances including the *Oprah Winfrey Show*.

Most of his utterances on abductions had occurred within the confines of Harvard, within the UFO community, or in private conferences (the proceedings of the MIT conference also appeared in 1994). Now with his research and thoughts exposed in a book from the popular press, all the world could read and comment—and many influential commentators were unkind. The *Wall Street Journal* had taken digs at Mack and his work even before the MIT conference, but now the knives came out in the *New Republic, New York Times, Psychology Today, Time*, and even the local *Harvard Crimson* with descriptions like "mystically detached and certain that he is right," "high priest," and "Pied Piper" of the abduction phenomenon, and an honorable man with a "halo of perfection" who should therefore be held to higher than average standards. These attacks also reflected badly on Harvard, until at last institutional authority rumbled into action.

Blumenthal delivers a gripping account of Mack before the faculty inquiry—how only matters like research protocols and informed consent of his abductee patients were supposed to be at issue and not his controversial views. How he anticipated a collegial exchange but faced a panel of accusers chaired by Arnold Relman, with whom he had clashed earlier over the propriety of physicians participating in antinuclear activism. Grilled on whether he acted as therapist or researcher, Mack insisted the two were often inseparable. When Relman pressed him on how he could treat abduction accounts as real, he argued that they felt clinically like something happened, and because some physical evidence supported them; but Relman insisted there was no evidence. In fact, UFO abductions could not be real because UFOs did not exist.

Though Relman denied the inquiry was an inquisition, friends saw that Mack was being set up and advised he needed lawyers at his side. The lawyers pushed back against apparent efforts to portray him as shoddy in research, careless of the well-being of abductees, and too committed to a belief in aliens to entertain alternative explanations. Abductees, co-workers, and academics defended his work through more than two dozen sessions of the year-long inquiry, but the panel's final recommendation ignored most supportive testimony, repeated the same litany of accusations, and concluded that Mack's actions did not meet Harvard standards.

After all this drama, higher administrators overturned the panel with an affirmation that Mack was free to explore his own interests and needed only to uphold the high standards of the university. This anticlimax cleared him of any wrongdoing or unethical practice, but it did not expunge the stain on his reputation or reassure less-famous colleagues that they could defy orthodoxy with impunity. In fact, a similar case unfolded in 2021 when one critic among many declared that Avi Loeb, chairman of the Harvard Department of Astronomy, "used to be" a good scientist until he proposed that an asteroid-like object named 'Oumuamua might have been an alien space probe passing through the solar system. He stated his case in the professional literature and wrote a popular book (Loeb, 2021), but the response is unmistakable: Publish 800-plus papers and you are a fine scientist, co-author a paper with Stephen Hawking and you are an outstanding scientist, but speculate just once on an alien spaceship and you are out of the club faster than a ten-year-old boy caught speaking a kind word to a girl. Were it not for Mack's bulldog lawyers, his outcome might have been dire indeed. May Loeb and others be spared.

His Harvard ordeal behind him and undeterred by his critics, Mack continued to investigate UFOs during trips to Australia and Brazil, met with some 200 experiencers during the 1990s, and continued to discuss abductions in public appearances. He undertook with his PEER colleagues the kind of research his critics wanted, like a study of the psychology of experiencers which found no evidence for fantasy-proneness or any other personality differences that might explain their experiences; Relman complained that the research was imprecise. A conference attended by psychologists, psychiatrists, hard scientists, a philosopher, and a theologian met with Relman's disappointment in the "limited range of attendees." A published and peer-reviewed article critiqued psychological explanations for abductions and concluded that they did not work; that, in fact, the "parsimonious" solution had to admit scientific reductionism failed to find a solution. Whatever abductions were, science did not know.

Here, in Mack's contention that science does not have the answer for mysteries circling in the darkness just beyond the light of the rationalist campfire, lies a thematic constant in Mack's life and thought, and an issue that runs under and through Blumenthal's entire narrative. If we seek the cause of friction between Mack and Relman, Mack and Harvard, Mack and the 20th century, we find it in Mack as rebel against a scientific-rationalistic worldview that drew narrow limitations then excluded experiences that did not fit as unreal and unworthy of inquiry. By the recurrence of this theme Blumenthal underscores it as the motive behind many of the paths, passions, and risks that Mack chose throughout his career.

Mack had the upbringing and training of a scientific materialist, but Blumenthal uncovered a penchant for unconventional subjects and unorthodox understandings before UFOs ever crossed his horizon. He sometimes used hypnosis in his therapeutic work, and in search of a better technique he learned breathwork from Stanislav Grof, a method of relaxation that could also induce altered states of consciousness. These experiences included primal memories with visions of incubators that would later tie in with abductions. His interests extended to astrology, yoga, LSD, Amazonian hallucinogens, non-Western worldviews, reincarnation, and life after death, his mind more eclectic than systematic as he assimilated diverse influences into the framework of his thinking. Sometimes a chance meeting provided him with clues to the ongoing puzzles that occupied him—one such meeting happened on his African trip to interview the Ariel School children when he met Credo Mutwa, a Zulu shaman who shared native lore and understanding of otherworld beings that influenced Mack's thoughts throughout his book, *Passport to the Cosmos.* Another was the meeting with Hopkins that he almost turned down but then credited with setting his life on a whole new course.

For all his immersion in the rational Western mindset, Mack wrestled with its restrictions throughout his life. Conventional religion held no appeal for him, yet he took a keen interest in religious experiences and entertained ideas of Source, Creator, or the Divine as an ultimate cosmic consciousness. He found a mindless, amoral, desolate universe to be inadequate and rejected it in favor of order, purpose, and benign agency in the cosmos, though what was wrong with the materialistic concept was clearer in his thinking than what the right alternative should be. His readiness to accept that elusive otherworlds surrounded us, usually experienced only in altered states of consciousness but as real as the material realm and liable to intrude with mental and physical consequences, prepared him to accept UFO abductions as one of the ways intelligences from the spiritual sphere interfaced with the everyday to tutor, warn, and guide us.

It hardly needs saying that these unconventional ideas ill-equipped him for life in a stronghold of materialistic science. Mack was a psychiatrist in a field oriented toward patient treatment and understanding human psychology in terms more humanistic than scientific. His approaches threw him out of step with modern psychological research that tested behaviors in a laboratory or profiled personalities according to standardized scales, but the two sides could coexist peacefully enough so long as one did not trespass into the other's territory. Mack risked overreach when he speculated on the nature of the cosmos; but when he dared suggest that aliens really abducted humans, he started a war against community, reality, and truth.

Like Carl Jung before him, Mack accepted the physical reality of UFOs (Jung, 1959). Also, like Jung, he cared little about their physical reality. It was there but of secondary importance; what mattered about abductions were their human meanings and transformational potentials. His lukewarm commitment to nuts-and-bolts UFOs caused a rift with Hopkins and Jacobs, who regarded abductions as compensation for the aliens' lost reproductive abilities or even as an *Invasion of the Body Snatchers* scenario where progressive hybridization prepared a takeover of the earth by a new species, human in biology and appearance but alien in mind. Abductions were physical events that endangered humanity (Hopkins, 1987; Jacobs, 1998).

Mack knew that abduction had undeniable negative effects. It often caused terror and resentment and left his experiencers disturbed, anxious, even traumatized. One woman did not want to have children for fear the aliens might take them. But he looked to the positive side—some abductees felt the experiences were beneficial, developed an affinity for the entities, gained psychic powers, knowledge, purpose, openness, wisdom. They sometimes described apocalyptic scenes, sometimes idyllic, heavenly visions. He interpreted these sights as warnings against ecological destruction of the earth and glimpses of a harmonious future, perhaps helpful guidance from alien sages or an intervention of spiritual powers crossing into the physical realm. Despite harmful side effects the experiences were overall beneficial, even necessary shocks to complacent materialism and an alarm to reform our consciousness before it was too late.

By 1999 when he published *Passport to the Cosmos: Human Transformation and Alien Encounters*, he had largely disencumbered himself of the physical aspects of abduction accounts. Experiencers still reported examinations and missing fetuses, but these occurrences, like other paranormal phenomena, might occur in other vibrational dimensions, or represent an evolution of consciousness to recognize planetary peril, a transformation away from materialism toward older, wiser modes of knowing, to rediscovery and reconnection with the living mind of the cosmos. He remained happy to speak of abductions as real events, but the reality he meant diverged from the foursquare universe of established science.

This idiosyncratic cosmology of Mack's caught him between two camps: On one side Hopkins and most ufologists insisted that abductions happened in physical reality though the aliens imposed mental controls on their victims. On the other side Relman and other skeptics drew a sharp dichotomy between real and unreal and exiled all UFOs to the latter, where they existed only as mental phenomena or errors. These antagonists over facts shared the same understanding of reality. Mack's reality combined physical effects with mental and emotional consequences in a broad and imaginative proposition with intriguing possibilities, but it remained hazy speculation without clear mechanism or principle, evidence, or demonstrability. His vision of a reality beyond established science developed year after year but his ability to explain his new science never caught up with his vision. Sometimes this divergent path led to comical turns, like a discussion of whether the abductee's body remained in bed or transported in some paraphysical state, that recaptured the tenor of a Medieval theological dispute.

If Mack was happy enough to stray from the consensus and pursue his own dreamy thoughts, he also isolated himself from the mainstream of academic discourse to become a lone voice. In all fairness, many criticisms levelled at him had merit. The physical evidence for abductions had already begun to unravel with the MIT conference, where David Pritchard admitted that a supposed alien implant consisted of human organic material and two pro-UFO physicians reported no evidence that any fetuses went missing. Budd Hopkins described a sensational abduction in Brooklyn observed by several independent witnesses, but the story and the witness testimony soon fell apart. A Roper Poll that suggested several million Americans might have experienced UFO abduction received sharp criticism for its design from multiple sources. Relman was right that Mack had none of the hard evidence that science demanded.

While hypnosis recovered the supposedly repressed memories of traumatic abductions, a broader controversy over recovered memories had shaken the country through the 1980s and early 1990s. Thousands of adults "remembered" sexual abuse by their parents or ritual abuse by cults, while young children implicated their daycare providers in similar ongoing acts. High-profile trials ensued and dozens of the accused went to prison. Supporting evidence was dubious or nonexistent and convictions rested on lurid memories revealed through hypnosis or high-pressure suggestion from overzealous and underprepared therapists. Noted psychologists like Elizabeth Loftus demonstrated that fictitious stories based on suggestion and solidified by reinforcement resulted in false memories that carried the same emotional weight and feel of genuine memories. This entire social panic amounted to delusions confabulated by believing therapists and patients (Loftus, 1994). Though Mack was probably more circumspect than ufologists in his dealings with abductees, his confidence that he guarded against leading or influencing his witnesses does not seem to have taken account of the research findings and excesses exposed during the "repressed memories" epidemic.

Consensus science serves as both the practical avatar and the gatekeeper of scientific truth. Scientists sometimes—now more than ever, so it seems—find themselves called upon to resist ignorance, pseudoscience, irrationality, and propaganda. The repressed memories epidemic made criminals out of elderly parents and traumatized victims out of people with minor depression or trouble sleeping. The harm was real while the alleged causes never happened outside the minds of subjects and facilitators. Here was a clear example where scientists had a professional obligation to defend the truth of evidence and investigation for the common good, and, more generally, to protect hard-won scientific knowledge against contamination by doubtful claims and half-baked theories. His scientific critics may have acted with brutality to tear the wings off his speculative butterfly, but had he lived to see the past few years, no one would experience more anguish at the corruption of truth than John Mack.

Readers of *The Believer* feel from beginning to end the author's respect and admiration for his subject. The man who left an impression on everyone who met him, heard him speak, or read his books works similar magic on his biographer. Ralph Blumenthal's selfless account lets Mack tell his story largely in his own words and the words of others close to him, chosen with a deft hand to highlight the varied themes, aspirations, tensions, conflicts, and character traits that rigged the sails and set the courses of this complex man. Fortunate for Mack's legacy, fortunate too for we readers, Blumenthal is a masterful writer who crafts a story rich in detail, rigorous in organization, and attentive to complicated ideas, yet tells it in prose like music, vibrant with the personalities of the characters, and paced as engagingly as a thriller. This is a book that conveys both understanding and delight in generous measures and shares with readers the sense that with John Mack we have entered the presence of a special light. I can only think he would approve.

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BOOK REVIEW

The Essential Guide to the Loch Ness Monster and Other Aquatic Cryptids by Ken Gerhard. Crypto Excursions, 2021. 253 pp., $19.99 (paperback). ISBN 978-0- 578-84746-7.

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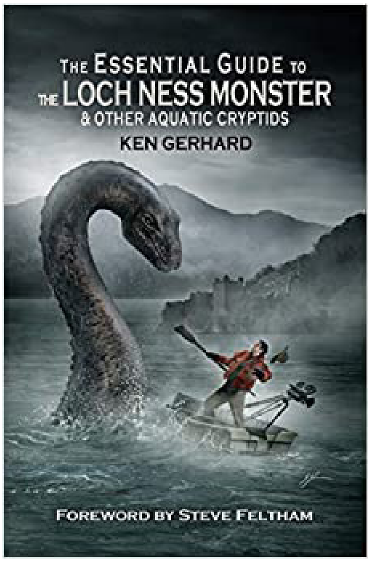
This is a readable overview of reports of unidentified aquatic creatures in the oceans and in freshwater, and it can be a useful introduction for people who have not previously read much about this subject. Cryptozoologists, however, will find nothing new here; the treatment is purely descriptive rather than analytical and critical.

A nice Foreword by Steve Feltham, in residence at Loch Ness for 30 years looking for Nessies, includes the important point that the number of actual sightings is a large multiple of the number of publicly known reports.

The first two chapters are about the Loch Ness Monster. Chapter 3 deals with sea serpents. Chapters 4 through6 are about the Canadian "Ogopogo" of Lake Okanogan, the American "Champ" of Lake Champlain, and less-well-known lake monsters of North America. Chapter 7 reports on lake monsters around the world. Chapter 8 surveys the typically mysterious carcasses periodically found on seashores. Chapter 9 mentions the surprises that the ocean depths occasionally reveal, notably the coelacanth, the giant squid, and the megamouth shark, as well as the little-known beaked whales, oarfish, and sturgeon of monstrous size.

The book's bibliography is excellent, listing 45 relevant books; I could think of no significant work that is missing. Furthermore, the coverage is very much up-to-date, including even the environmental DNA studies at Loch Ness in 2019.



On the other hand, there is no index. An increasing number of recent books on various topics fail to include an index, which I find quite inexcusable at a time when the software used for writing the work offers the possibility of very easy automatic, at least rudimentary, indexing.

My chief substantive criticisms are:

1. A regret at the overall lack of detailed analytical and critical discussion. The most severe lack here concerns the many cited reports of apparently very long "creatures" undulating vertically to give the appearance of humps. Those are most likely misinterpreted wave effects, viewed from the side at low elevation. Unless one has seen this phenomenon for oneself, it may be difficult to believe how easily this can be mis-"seen". In 1983 I filmed such humps breaking the surface periodically, with appropriate foam thrown up; and it was only after many viewings for more than a year, and more in-person observations of boat wakes at Loch Ness, that I was able to realize that these "humps" were small remaining sections of one side of a wake from a long-passed boat. Several bits of film shown on TV news programs and called "remarkable" and explainable only as animate have been of such wave effects, or wind effects that can mimic something moving fast across the surface. As Adrian Shine, after decades of Nessie hunting, has pointed out, any *periodic* phenomenon is most likely a wave effect.

2. This regret does not apply to Chapter 8, where Gerhard appropriately points out that all carcasses initially described as indicating some unknown species have so far turned out to be decomposed whales, basking sharks, or whale blubber. The exception is the remains of what really seems to have been part of a genuinely giant octopus.

3. There is a frustrating lack of photographs or sketches, or footnotes or endnotes with detailed citation of where such illustrations could be found.

4. There are a few factual errors about Nessies:

—The Surgeon's photo has *not* been shown to be a hoax (Shuker, 1995, p. 87) and should not be labeled (p. 47) "infamous". The little-known second "Surgeon's" photograph published by Constance Whyte (1957), and mentioned only in passing here, debunks the notion of a hoax.

—The Dinsdale film definitely shows an animal and not a boat;[1] and the so-called "recent new enhancements with modern software" are anything but new or with appropriate software.[2]

—The Jet Propulsion Laboratory did no re-touching (p. 44) of the underwater "flipper" photos, only computer-enhancements.[3]

—There is simply no possibility that Nessie "migrates out to sea on occasion" (p. 63), given the shallowness of the River Ness as it flows through the middle of the town of Inverness, and the several locks on the canal.

—The Mansi photo of Lake Champlain's "Champ" is *not* "widely considered to be the best photographic evidence of any aquatic cryptid" (p. 126).

But Gerhard is accurate about Nessie facts that others often get wrong, for example (p. 13) that St. Columba was reported to have encountered a water-monster at the *River* Ness, not the Loch; and that Operation Deepscan in 1978 covered not the whole of Loch Ness but only perhaps 60% of it (p. 52).

NOTES

1 The film has been made available on the internet by Dinsdale's son Angus: See *The Man Who filmed Nessie.* https://www.themanwhofilmednessie.com/tims-nessie-film.html

2 The book must be referring to Adrian Shine's use of a 3rd-generation video copied from a TV: See my "Comments on Shine's 'The Dinsdale

Loch Ness Film, An Image Analysis'," p. 2, in "To whom it may concern" on the cryptozoology internet discussion board. https://mega.nz/file/lGIWSDCI#oW8JA8obFfPDq1LuObT-GhtpU6CrDdljfcy_mv65yto

3 Letter from Charles W. Wyckoff to *Discover* magazine, 27 August 1984. https://mega.nz/file/lCJk2KCR#w8txlHb2KJVL75iPpH_G6fyHMJcI57HfzF8xMzBzD24

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BOOK REVIEW

Advances in Parapsychological Research 10 edited by Stanley Krippner, Adam J. Rock, Harris L. Friedman, and Nancy L. Zingrone. McFarland, 2021. 224 pp. $75 (paperback). ISBN (print): 978-0-7864-7792-0. ISBN (ebook): 978-1-4766-4118-8.

Reviewed by Damien Broderick

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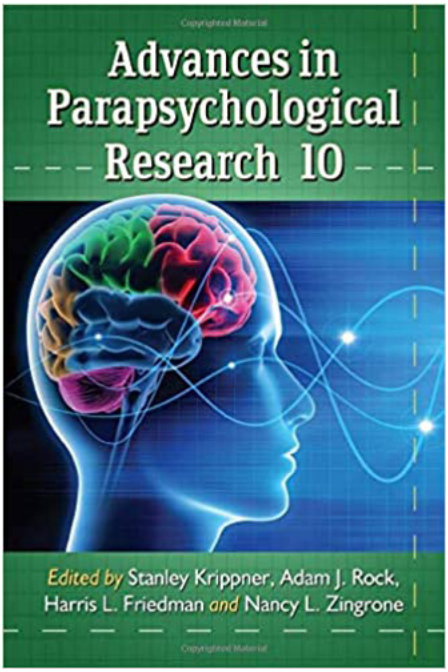
It's been a long, strange road, especially for those who died but came back for another circuit or remained alive and pursued mystic abilities for secret national intelligence service sponsors, or carefully tried to influence the past or forecast the future, or struggled with quantum theory because it looked eerily similar to magic in its weirdness even though mathematical experts denied any meaningful resemblance.

Since 1977, this tussling with the improbable has been tracked in a series of volumes edited by Dr. Stanley Krippner, a Fellow in five American Psychological Association divisions. They are devoted to essays condensing and confronting claims of paranormal phenomena—to use a term deplored by many who regard such anomalies as *normal*, less Sixth Sense than First but frustratingly skittish.

But the strange long road seems often to be covering the same rutted stretch, and its explorers grow weary. While the second volume appeared a year after the premier, pace slowed in the next seven volumes, and seemed all but exhausted by number 9, published sixteen years after its predecessor. The latest volume paused for eight more years. Granted, Dr. Krippner is now 88 years old, but still diligent and adventurous. So are his fellow editors and contributors, although sadly two of them (Professor William Roll, 1926–2012, and Professor Michael



Persinger, 1945–2018) are dead and incommunicado.

The most startling recurrent theme in this tenth volume is how much hard and conscientious empirical work in a number of varieties of psi leads to the conclusion that many psychic phenomena just are not as substantial as researchers have claimed for more than a century.

Consider the beefy concluding essay "Psychics and Police Investigations" (pp. 167–217), by Dutch co-founder of the Parapsychology Laboratory at the University of Utrecht. Technical scientist Sybo A. Schouten tracks spores of pragmatic police investigations and compares them to claims of psychic information. His final determination is unremitting, based on many detailed accounts of missing or murdered victims and significant details of the perpetrator: "If, when compared to psychics, experienced officers using the results of [conventional] research appears as good or better in predicting the thereabouts of missing persons, then there is little reason to believe that there is anything mysterious about psychic detection" (p. 215). Of course one might try to salvage the reputations of psychics by arguing that police, too, might unconsciously draw upon psi in their hunt for clues, bodies, and wrong-doers. Added to their specialized training, psychic hints might provide a welcome boost without the stigma of acknowledged psi. In the light of Schouten's detailed case histories, though, this seems desperate handwavium.

In a searching "Meta-Analysis of Autonomous Information by Mediums," a specific way to test the validity of mediumistic powers is applied by Dr. Adam J. Rock (who has emerged lately as a strong researcher in Australia, often collaborating with Dr. Lance Storm but in this case two others working in Australia, Dr. Natasha M. Loi, and Associate Professor Einer B. Thorsteinsson (both health psychologists), plus Italian Dr. Patrizio E. Tressoldi who discerns in the mind nonlocality and quantum effects. Here is their executive conclusion: Across 200 trials "the trend showed that mediums performed at chance level, which in turn fail to support the plausibility of both the survival of consciousness and living agent psi" (p. 63). They add: "We imagine that psi-skeptics will be encouraged by our negative findings. In contrast, psi-proponents may bemoan the small number of studies and, indeed, trials that contributed to the heterogeneity of our data base" (p. 65).

Such inevitable contestation was staged most revealingly in a 2018–2019 exchange between notable parapsychologist Professor Etzel Cardeña (of Sweden's Lund University) and two established and aged skeptics, cognitive scientist Arthur S. Reber (Visiting Professor at University of British Columbia) and James Alcock (a Professor of Psychology at Canada's York University). In the premier journal *American Psychologist* Cardeña published a defense of the demonstrable existence of psi, which was dismissed sarcastically by Alcock and Reber with no reference to more than a century of empirical evidence, on the absurd grounds that the very notion of psi was ontologically absurd, scientifically impossible, and hence self-refuting.

In an excellent treatment of this dialectic, Bryan J. Williams sets the argument straight while keeping his temper. Regular readers of *JSE* will recognize Williams' first pass at this commentary, "Reassessing the 'Impossible'," which appeared here in 2019, Volume 33, No. 4. His extended essay asks "Too Strange to be True?" He presents a case that psi is not only far from being *too strange* and hence *impossible*, but rather offers a path to "possible insights about the nature and reach of the human mind" (p. 41). Admittedly, summarized so briskly, even so cautious a riposte might be thought the equivalent of depending on messages from angels or the far side of the Moon. Luckily, Williams provides a thought-out deconstruction of the skeptics' own unsupported mockery which is "based solely upon the personal opinions of the authors themselves, as well as certain assumptions about psi which turn out to be unfounded. *And this turns out to be the ultimate flaw in their argument*" (his Italics). This assertion is best tested by reading Williams' detailed case (pp. 15–48).

An important addendum to this general critique provides its own quagmire, though, especially for those of us who have carelessly failed to do a Ph.D. in advanced physics. As a research student of the late parapsychologist Dr. William G. Roll, Bryan Williams is well placed to bring together the varied conjectural mechanisms of psi advanced by Roll, Dr. Cheryl H. Alexander (who worked in what is now the Rhine Research Center, on EEG biofeedback), the late Professor Michael Persinger, and Williams' own estimate of the situation. Their substantial composite essay bears the bold title "Parapsychology, Quantum Theory and Neuroscience" (pp. 70–166). As a confessed and often confused autodidact in the boiling theories of both quantum physics and advanced neuroscience, I can only point to this document and hope that others qualified to sport among particle accelerators and cortical scanners will render their informed verdict on the arguments advanced.

I admit that I do get a queasy feeling when people happily claim that only conscious observation of entangled particles can cause them to collapse from a mutual superposition to a sharp-edged single state. Or worse yet, that the entire spacetime universe is built out of primordial consciousness, and that perhaps the Sun is a mighty Mind communing with its own entangled spiritual spaghetti across the entire cosmos and also in your sleeping brain. Or something. Let us hope with all our might that this high-level word salad condenses into a future theory at once robust, difficult but not impossible to comprehend, and perhaps solving the puzzles of psi in time to appear in the next volume of *Advances in Parapsychological Research*, which might bubble up inside our many harmonizing minds (who knows?) with meme messages from the far and enlightened future.

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BOOK REVIEW

Rethinking Consciousness: Extraordinary Challenges for Contemporary Science edited by John H. Buchanan and Christopher M. Aanstoos. Process Century Press, 2020. 248 pp. $20.00 (paperback). ISBN-13: 9781940447438.

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This slender volume is the twentieth member of a series entitled "Toward Ecological Civilization," organized under the leadership of distinguished process philosopher and process theologian John B. Cobb, Jr. It grew directly from the 10th Whitehead International Conference, held in Claremont, California, June 2015, and more particularly from a single conference track (out of the more than 80 making up the program) devoted specifically to various kinds of "extraordinary experiences" (especially parapsychological and transpersonal experiences) that directly challenge the materialist/physicalist worldview which arose in the 17th century and still dominates the contemporary scientific, educational, and cultural mainstream. The central premises of the series as a whole, the 2015 conference, and the present volume are that postmodern civilization faces life-threatening crises rooted in that impoverished physicalist worldview, that we desperately need a more commodious, life-affirming, and ecologically sound alternative to it, and that Whitehead's metaphysical vision can help take us in the needed direction. Like Stanley Krippner in the book's brief Foreword, I am strongly sympathetic to these views.

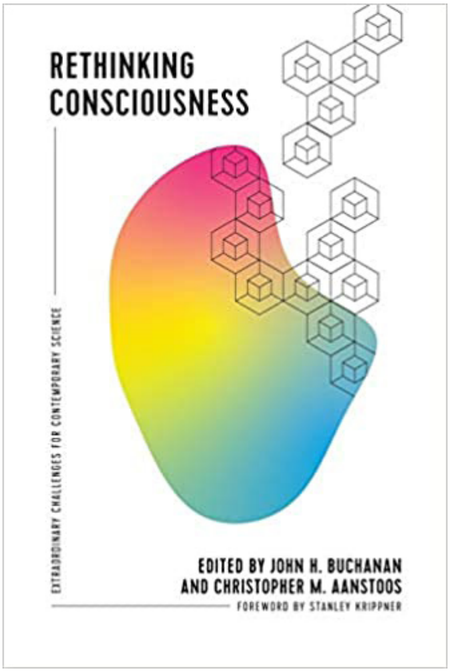
A helpful Introduction by the editors expands somewhat on this general background and then briefly sketches the chapter-by-chapter content of the book.

In Chapter 1—"Denigration of the Subject in Late Modern Thought"—John B. Cobb, Jr., himself incisively surveys the history and consequences of the progressive concentration of science on the material or "objective" side of Descartes's conceptual bifurcation of the natural world. Particularly in the wake of Darwin's work on evolution, Aristotle's final causes—purpose and teleology—largely disappear in favor of efficient or mechanical causation, while consciousness itself becomes epiphenomenal and ineffectual, or identified with physical processes in the brain. We, like all other animals, are really nothing but complicated machines. The computational theory of the mind (CTM) arises from the ashes of Watson's radical behaviorism and remains ascendant today in psychology and neuroscience. Psychiatry revolves increasingly around cells, molecules, and pills. We "objectify" everything. In the economic realm, Adam Smith's "invisible hand," which had been firmly anchored to a shared sense of community, becomes detached from any such ethical constraints with the result that other humans, animals, and the resources of the planet in general become increasingly subject to greedy and reckless exploitation for commercial gain.

The word "materialism," which initially denoted just the metaphysical position, acquires its other meaning of a lifestyle devoted to self-aggrandizing consumption. One cumulative result of these pernicious socioeconomic trends—global warming—now threatens our very existence, and this leaves aside additional ever-present threats such as possibilities of nuclear holocaust and further global pandemics. Yet for all its perils Cobb also sees this postmodern period as a time of great opportunity, in which we can potentially save ourselves and the planet by carrying forward a separate tradition, represented in the work of people such as William James and Alfred North Whitehead, which puts the conscious experience of effective human agents at the center of its scientific and philosophical concerns. A contest is under way between these radically divergent worldviews, and our collective fate may well be determined by which side prevails. The story of this contest remains to be told in much fuller detail, but Cobb's picture is surely more or less correct in outline.

Next comes a typically strong chapter by eminent process philosopher David Ray Griffin on "Parapsychology and Whiteheadian Panexperientialism," which briefly summarizes themes he has written about more extensively before (Griffin, 1997, 1998). The scientific controversies over parapsychology arise from apparent conflict with materialism in its two historically primary forms—an early version combining a mechanistic conception of nature with human souls and a God, and the more modern version dating from late nineteenth century which is mechanistic, materialistic, and atheistic throughout. Whitehead, however, building especially upon the late work of William James, elaborated a richer metaphysical vision which seems able to accommodate most parapsychological phenomena and yet also appears compatible with fundamental developments in modern physics such as relativity theory and quantum mechanics. Griffin provides an outline of relevant parts of that vision, emphasizing Whitehead's restoration of final causation and his introduction of the concept of non-sensate "prehension" between "occasions of experience" (the fundamental units of his metaphysical system), which allows for action at a distance and hence potentially accounts for both receptive and expressive forms of psi. He then surveys some of the main lines of evidence which have persuaded him of the reality of telepathy and clairvoyance, including both spontaneous cases (of which there are thousands), and laboratory experiments such as the Maimonides dream studies, remote viewing, and Ganzfeld research. Given the strength of this evidence, he wonders, why is parapsychology still so often viewed as "pseudoscience" and not more widely accepted by the scientific community?

After addressing and dismissing some of the usual claims of supposed a priori incompatibility between psi phenomena and established science, he focuses on the one form of psi—true precognition—that he believes really deserves such treatment. He is well aware of the substantial body of apparent evidence for precognition, but insists that this purported psi phenomenon, unlike its siblings, is *logically impossible*: Its ostensible causes do not yet exist and hence cannot cause anything. Moreover, even if they did exist they would be subject to the destructive argument against retrocausation by Braude (1991, pp. 256-277). Precognition also seems to imply determinism and the negation of free will, both of which Griffin rejects. Ostensible evidence



for precognition therefore *must* be—and in fact often easily *can* be—explained in alternative ways, of which he provides several examples. Finally, surmising further that precognition is our primary obstacle to scientific acceptability, he urges parapsychologists to strike it from the list of accredited psi phenomena. I believe we should decline to accept this recommendation, partly because some of the evidence seems too strong to dismiss in the suggested ways, and also because Griffin has not exhausted the possibilities of accommodating true precognition theoretically without sacrificing free will (see Kelly et al., 2015, Ch. 13, pp. 455, 465, Ch. 14, pp. 526-530; Rosenberg, 2021). Apart from this specific disagreement, however, I strongly support Griffin's more fundamental contention as to the continuing value of Whitehead for parapsychological and transpersonal theorizing.

The next chapter is by veteran parapsychologist John Palmer, who had previously engaged directly with Griffin in a very constructive dialogue concerning Whitehead and parapsychology (Griffin, 1993; Palmer, 1993; Griffin, 1994). Surprisingly, none of that material appears or is even referenced here; instead, Palmer now contributes a wide-ranging commentary on parapsychological research and theory that appears intended mainly for newcomers to the field. Considering first the existing experimental evidence for the reality of psi, he properly dismisses the possibility of individually perfect or conclusive experiments, and turns instead to meta-analyses of the Ganzfeld, Remote Viewing, RNG PK, and DMILS paradigms, all highly significant. He next advocates for process-oriented vs. proof-oriented research and for engaging critics in the effort to explain how apparent instances of psi occur. A notable recent trend is the turn toward “implicit psi” paradigms such as the Bem experiments, Decision Augmentation Theory (DAT), studies of experimenter psi, and the Global Consciousness Project (GCP), which may provide direct evidence in support of the transpersonal worldview if its chief architect Roger Nelson can be ruled out as the primary source of the observed effects. Turning next to postmortem survival, he first sketches the present stalemate between survival and living-agent psi interpretations of mediumistic communications and suggests that progress might result from shifting emphasis toward study of unusual skills such as those that sometimes appear in rebirth cases.

He next examines a pro-survival argument set forth in Chapter 6 of *Irreducible Mind* (Kelly et al., 2007), based on NDES occurring under extreme physiological conditions such as deep general anesthesia and/or cardiac arrest, and without dwelling excessively here upon the details I think Palmer underestimates the force of that argument in several ways: First, he gives too much credit to the various proposed “normal” physiological explanations. Second, he underestimates the strength of individual cases like those of Pam Reynolds and Eben Alexander; his expressed doubts about the severity of the latter’s impairment, for example, have been decisively undermined by a subsequent review of his 600+-page medical record by three physicians (Khanna et al., 2018). These cases come straight from the heartland of contemporary biomedical science, and it appears virtually certain that we will encounter more and even better ones as resuscitation medicine improves its ability to retrieve patients from the borderland of death. Finally, Palmer also misreads the specific purpose of our argument, which was aimed not at providing direct evidence for survival, but at showing that consciousness can operate intensely in the absence of physiological conditions believed by virtually all contemporary neuroscientists to be necessary for conscious experience of any sort. Such cases conflict with the conventional “production” interpretation of brain/mind correlations, and directly support the alternative “filter” or “transmission” or “permission” interpretation formulated by William James, F. W. H. Myers, Henri Bergson, and others, and this in turn removes the key *logical* obstacle to the possibility of postmortem survival—the “immovable object” of Gardner Murphy (1961; see also Kelly, 2021). In the final section of his chapter Palmer turns to existing theories of psi, classifying these under two main headings—psychological and physical—but chooses to discuss only theories of the latter type, briefly touching upon Dean Radin's double-slit work, the "Observational Theories" of psi, and some quantum theories of consciousness. The world described by Quantum Theory definitely seems more psi-friendly than the world described by classical physics, even if we cannot at present work out all the relevant details.

The following Chapter 4, by James Carpenter, is by far the longest in the book. Titled "Parapsychology Needs a Theory—and It Has One", it picks up on the other side of Palmer's theory classification by providing an overview of his "First Sight Theory" (FST), a leading example of a well-developed *psychological* theory of psi. Spontaneous phenomena can certainly be impressive, but they are also notoriously infrequent, fugitive, and unpredictable. Carpenter's strategy is to pursue order and understanding primarily in the context of experimental studies of psi, of which he provides a brief history culminating initially in crystallization of that tradition in the form of the carefully controlled card-guessing and dice-tumbling methods developed by J. B. Rhine and his co-workers at Duke. Several further generations of experimental work of various kinds have produced large amounts of additional evidence for psi, and yet the field remains academically marginal and grossly underfunded. What we most need, Carpenter believes, is a theory that can explain in particular the *amount* and *direction* of scoring in psi experiments. FST seeks to achieve this by integrating psi research with large amounts of modern mainstream work on the "cognitive unconscious," which includes topics such as subliminal perception, priming, "blindsight," and perception without awareness. This literature provides numerous examples of such processes, plus well-developed methods for studying them, and psi processes appear to work in analogous fashion. FST postulates that psi, far from being rare and fugitive, operates constantly in the background, entering moment-by-moment into the construction of all everyday experience, combining its effects with those of unconscious intentions, ordinary sensory, mnemonic, affective processes, and all the rest.

This generalization of the mainstream conception of a cognitive unconscious to include psi renders psi primary and in effect embeds the human individual in a far wider spatiotemporal environment. In his chapter and especially his book, Carpenter is able to show in remarkable depth and detail how thus situating the paranormal in the context of other preconscious receptive and expressive processes allows us to understand numerous existing experimental psi findings in a way that makes excellent psychological sense. He essentially normalizes the paranormal in a fashion that mainstream cognitive neuroscientists ought to admire. For further information see Stanford (2015), who provides a thoughtful analysis of similarities and differences between FST and his own earlier but closely related psychological theory of Psi-Mediated Instrumental Response (PMIR). Let me also comment here that both PMIR and FST share with mainstream cognitive neuroscience a picture of the human psyche as consisting essentially of an emergent everyday consciousness supported by massively parallel unconscious neural computation (formerly described as "unconscious cerebration"), and nothing else. I believe that picture to be too limited and will return to this subject in the concluding section of this review.

Chapter 5 by co-editor John Buchanan, titled "First Sight: A Whiteheadian Perspective," seeks simultaneously to ground FST in Whitehead's metaphysics and to construe evidence for psi as supporting it. In Part One, "Whitehead and Transpersonal Anomalies," he alludes to his own teenage encounters with powerful transpersonal experiences induced by psychedelics, briefly describes how Whitehead's metaphysics helped him come to terms with these, and goes on to provide another sketch of relevant aspects of Whitehead's system, making contact along the way with Stan Grof's description of psychedelics as nonspecific amplifiers of normally unconscious contents and processes. In Part Two, "First Sight and Process Vision," he bears down on some basic similarities between the two theories. Most fundamentally, Whitehead's concept of prehension provides at least in abstract form the theoretical opening for psi that FST itself assumes but does not attempt to explain. Both theories are also strongly experience-centered, and they provide strikingly similar accounts of the formation of individual units of conscious experience.

I agree that there are strong affinities between the two theories, but there are important differences, too. In particular, the role of prehension in Whitehead's process of experience-formation ("concrescence") is limited in ways alien to FST, in that access to past occasions is limited to the backward light cone, and access to future occasions is specifically denied, as noted above in relation to Griffin's chapter. I should perhaps add that Whitehead has always seemed to me to share in considerable degree the 2-level picture of the human psyche mentioned above (consisting just of everyday consciousness plus its unconscious supports), which for me makes it difficult to understand transpersonal and mystical experiences, as well as extreme forms of creative genius, in the context of his metaphysics. The chapter ends with an appeal for overcoming the longstanding mutual distancing between parapsychology and transpersonal psychology, plus a prediction that when the physicalist paradigm finally collapses, as it surely will, it will be supplanted by "something much like a Whiteheadian understanding of reality" (p. 111). With these sentiments I heartily agree.

Next comes Chapter 6, "Revision and Re-enchantment of Psychology," by Stanislav Grof. Stan Grof of course is one of the principal founders and theoreticians of transpersonal psychology, and this chapter—which he contributed following the conference—eloquently summarizes the main contours and conclusions of his lengthy career focused primarily on the healing, transformative, and evolutionary potential of non-ordinary states of consciousness that he terms "holotropic" or tending toward wholeness, whether occurring spontaneously or deliberately induced using technologies such as LSD and other psychedelics or more recently his own "holotropic breathwork." Viewing these subjects primarily through the lens of his training in various forms of psychoanalytic theory and practice, he begins by sketching the cross-cultural history of shamanic and mystical experiences suggesting the existence of a larger Self rooted in a conscious ultimate reality of some unfathomable sort.

He then explicitly rejects the prevailing physicalist metaphysics, rejects its associated production model of the brain-mind relation in favor of the filter/transmission model (p. 120), and outlines his own revised "cartography" of an expanded human psyche including perinatal, transpersonal, imaginal, archetypal, and cosmic components. He ends by testifying to the importance of spirituality in human life, distinguishing this carefully from religion (pp. 125–129), and by strongly advocating for intensified scientific study of transpersonal and mystical experiences. I find much to admire in this summary of Grof's life work and must acknowledge that I am sympathetic to many of his basic ideas, despite his relying almost exclusively upon clinical observations to justify them. It is noteworthy, moreover—and I will return to this in the concluding section of this review—that in presenting his own conceptual framework he makes no reference whatsoever to that of Whitehead.

Chapter 7, "Amplified Subject" by Leonard Gibson, echoes and expands upon Grof's views, specifically in the context of holotropic states of consciousness induced by psychedelics. After briefly reviewing some cultural history of the subject, he discusses the phenomenology and neuroscience of psychedelics at some length, embracing along the way the Huxley/Bergson picture of the brain as reducing Mind-at-Large to the relative trickle of information needed to cope with everyday needs, and celebrates the healing potential of psychedelic experiences. He then proceeds to his main theme, which is to provide rich descriptions of the manner in which psychedelic experience can provide insight into, and perhaps directly confirm, elements of Whitehead's metaphysics. He does this under four main headings: the unreality of "time"; the felt immanence of the past in the present; transience, or the perpetual perishing and formation of occasions of experience as analogous to death and rebirth; and "enjoyment of value," or discovery of previously unsuspected harmony among superficially conflicting elements of experience.

Gibson's reference point throughout seems that of a person focused primarily on the value of psychedelics in therapeutic and personal-growth contexts, and he clearly anticipates the impending renaissance in their FDA-approved use for such purposes (see Pollan, 2018). It is now abundantly clear that psychedelics have also opened a pathway to experimental study of the phenomenology and neuroscience of mystical experiences, and like Stan Grof I hope this kind of work, already under way in various places, will flourish in coming years.

Chapter 8 by Robert McDermott, titled "David Ray Griffin on Steiner and Whitehead," consists of a mostly appreciative commentary on a paper that Griffin had presented 30 years earlier (!) at a conference centered on the theoretical and applied work of Whitehead's contemporary Rudolf Steiner. Griffin's original presentation had meticulously analyzed the work of Steiner and Whitehead in terms of commonalities and contrasts between them, advantages of Whitehead, and advantages of Steiner. It happens that McDermott is to Steiner roughly as Griffin is to Whitehead, and in that capacity he now summarizes and comments upon Griffin's analysis. In brief, both thinkers saw reconciliation of science and spirituality as the most pressing task of modern civilization, and both sought to develop philosophies more inclusive than the prevailing physicalism, which they both rejected. Both developed philosophies centered on experience, affirmed freedom but rejected dualism, accepted universal interconnectedness and the reality of psi phenomena, and accepted the reality of evolution.

Both also advanced concepts of God as a final cause both influencing and influenced by the world process, and both belong to the general category of evolutionary panentheists. Whitehead was perhaps more focused on truth, and Steiner more on human transformation, but both were interested in both. Steiner devoted a much greater share of his own work to methods of personal growth and transformation, and to transformation of the culture at large, but from Griffin's point of view—which in these respects I share—Steiner was too strongly tied to "occultism," and altogether too confident in his ability to predict the future. These factors have probably contributed to Steiner's declining influence, and indeed this faltering trajectory seems to me ultimately to provide an important cautionary tale: Specifically, although it is certainly not too soon to begin thinking about how to bring an improved worldview effectively to bear on our current cultural crisis, in doing so we would clearly be well-advised to take care that we have genuine consensus on the theory side, and to remain anchored as firmly as possible in real science. I thank McDermott for bringing this practical applications issue into the foreground, and before moving on let me also express, on behalf of our entire field, gratitude for his overseeing production of William James's collected papers on psychical research (James, 1986).

Chapter 9 by co-editor Christopher Aanstoos, titled "A Phenomenology of the Ecological Self," approaches the cultural crisis framed by John Cobb in Chapter 1 from a very different direction. Rather than enlisting exceptional experiences in an effort to overthrow the prevailing physicalism on scientific grounds, he takes that overthrow as already accomplished, driven mainly by the impending ecocatastrophe itself, and seeks instead—treating Whitehead and sundry modern phenomenologists as having already created the needed alternative worldview—to explore how we as individual humans can adapt ourselves to that real natural order in more productive ways. Others may find this useful, but I must confess to a constitutional inability to extract much meaning from most phenomenological discourse, including this, and think the exercise premature in any case as explained more fully below. In sum, this chapter seems to me to do little to advance the central purposes of the book.

The final chapter, "All Tangled Up: Life in a Quantum World" by Larry Dossey, was submitted after the conference but has the feel of an after-dinner conference wrap-up talk—breezy, entertaining, and hopeful. He begins by briefly echoing John Cobb's assessment of how physicalism has underwritten the destructive modern plundering of our planet, but goes on to portray quantum holism, with nonlocality and entanglement as its key features, as forcing revision of that worldview in a direction more favorable to the well-being of our precious planet and all of its passengers. It is becoming more apparent that the world is quantum through and through, with effects originally identified at the subatomic level now regularly being found at larger and larger scales including our own.

Psi phenomena may be an example of this, as argued for example by Dean Radin in *Entangled Minds*, and related possibilities can perhaps be found in things such as swarming behaviors and group intelligence in various insects, fishes, birds, and mammals. There are even indications from within physics itself that consciousness is a fundamental constituent of reality and not a byproduct of physiological processes in mammalian brains. In sum, classical physics certainly captures much of reality at scales near our own, but it can no longer pretend to dictate the ultimate nature of things (Dossey, 2014). Note that Dossey, like Stan Grof, makes no reference whatsoever to Whitehead in articulating his own alternative to the prevailing physicalist metaphysics.

Having now described and commented upon its chapters individually, let me conclude with a more general evaluation of the book as a whole. As noted at the outset, I resonate strongly with its basic premises. Our world is definitely a mess, and much of the blame can surely be traced directly or indirectly to the physicalist worldview. We desperately need an expanded science-based vision of reality that is capable of accommodating a wider range of human experience and fostering improved individual and collective human behavior, and Whitehead's metaphysics seems definitely a significant step in the right direction. I also resonate strongly to the appeals of several chapter authors for better cooperation between transpersonal psychology and parapsychology, and for intensified scientific study of mystical experiences as windows into the deeper aspects of reality.

Beyond this, however, I found the book ultimately somewhat disappointing. It contains much of substance and interest, to be sure, but I see little real progress in the intended direction. To begin with, there are signs of haste in the book's production: In addition to superficial things such as numerous typos and the absence of an index, Whitehead's ideas themselves are nowhere presented in sufficient detail to allow newcomers to obtain a reasonably clear sense of the overall character of his system. Furthermore, the several chapters that do sketch his ideas overlap considerably, while others make no contact with Whitehead at all. In sum, the editors could have done much more, in my opinion, to strengthen the organization and integration of their raw material.

The book also reinforced my discomfiting prior sense that many Whitehead enthusiasts, especially those on the transpersonal psychology side, tend to see his metaphysical vison as a finished product—already perfected, complete, and static. But surely the last thing Whitehead himself would have desired is for *Process and Reality* to become some sort of sacred text or scripture. In Chapter 9 of *Irreducible Mind* (Kelly et al., 2007), my co-authors and I had introduced his metaphysics as representing a possible path forward, and David Ray Griffin thanks us here for doing so (pp. 46–47). But in our larger comparative effort that followed, Whitehead's system emerged as just one member of a sizeable family of promising conceptual frameworks or worldviews, ancient and modern, that tend strongly in broadly similar idealist/panentheist directions (Kelly et al., 2015; Kelly & Marshall, 2021). It remains to be seen which if any of these, or perhaps something else of similar type, will ultimately emerge as the best prospect.

One of the great strengths of Whitehead's system, for example, certainly lies in its apparent compatibility with developments in modern physics, but some of the other contenders can specifically lay claim to the same important property. These include, for example, Paul Marshall's "inverted" monadology (Kelly et al., 2015, Ch. 11), deriving from Leibniz who was another major influence on Whitehead, and the conceptual framework being developed by physicist and microelectronics pioneer Federico Faggin (Kelly & Marshall, 2021, Ch. 8). I have difficulties with other aspects of Whitehead's system as well, including not only its attitude toward the future and the possibility of true precognition (as discussed above in relation to Griffin's chapter), but also its privileging of occasions of experience vs. selves as its basic units.

The moral so far is that although it is certainly worthwhile to attempt grounding whatever psychological theories of psi we have in a deeper metaphysical context, as John Buchanan attempts to do for First Sight Theory using Whitehead in Chapter 5, it is by no means certain that Whitehead's system is the optimal choice for that purpose. I must also say here that although FST itself is fine as far as it goes, I doubt its own ultimate adequacy as a *psychological* theory. As indicated in my comments on Chapter 4, Carpenter's FST shares with Stanford's PMIR and current mainstream cognitive science (and to some degree with Whitehead as well), a picture of the human psyche as consisting basically of an emergent everyday consciousness plus unconscious supportive processes, and nothing else. I believe this picture is ultimately too confining, because it leaves out something essential—specifically, F. W. H. Myers's concept of the Subliminal Self, which later becomes "The More" of William James in *The Varieties of Religious Experience* and *A Pluralistic Universe*—a more comprehensive consciousness, equipped with "adits and operations" of its own, that expresses itself in shifting fashion as a function of varying conditions in the brain and body.

This is not the place to discuss these issues in depth, but only in this way, I believe, can we adequately accommodate phenomena such as psychological automatisms and secondary centers of consciousness, extreme forms of creativity, and mystical experiences. All of these involve characteristic properties of increased speed, complexity, precision, and vivacity of mental operations as the normal brain-based constraints on the operations of an underlying greater consciousness are reduced, resulting, in Myers's terms, in "the abeyance of the supraliminal"; Kelly et al. 2007, 2015). Note that this expanded conception of our human psychological organization is also much closer to the central ideas of transpersonal psychology, as articulated by Stan Grof in the present volume, and to the world's wisdom traditions in general.

In sum, it is certainly not surprising that the present volume leans so strongly on Whitehead's system, given its provenance, but that seems to me ultimately a limitation, because Whitehead's system itself still needs to be carefully and knowledgeably updated in light of more recent developments in physics, and if possible integrated more closely with other related currents in postmodern thought, as attempted for example by Eastman (2020). There are many striking convergences between Whitehead's views and the other conceptual frameworks canvassed in our own recent books, and this gives me real hope that a non-physicalist metaphysics capable of grounding an expanded psychological theory of the Myers/James type is within reach, but the present volume ultimately does little to move us further in that direction.

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BOOK REVIEW

After: A Doctor Explores What Near-Death Experiences Reveal about Life and Beyond by Bruce Greyson. St. Martin's Essentials, 2021. 261 pp.

Reviewed by John B. Alexander

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After portrays the epic journey of a true skeptic who chose to follow the data rather than reject seemingly fantastical accounts from his patients. It is an account of transformation while maintaining a thoughtful balance accommodating both spiritual and material worldviews. Spurred on by serendipitous events, uniquely Greyson has spent more than four decades chasing the previously inexplicable, and gossamer-like, but tantalizing glimpses of what was reported to be the continuation of consciousness beyond physical existence.

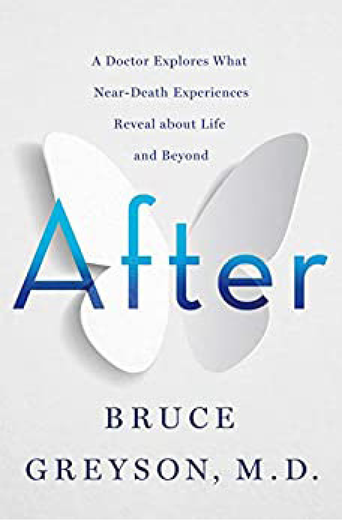
There have been a number of compelling books describing detailed accounts of those who pierced the veil and returned to report on it. In *Proof of Heaven*, Dr. Eben Alexander, a neurosurgeon with over two decades of experience, first related his extensive interactions in the nether realms while suffering from gram-negative meningitis; a condition from which he was not expected to survive. Dr. Mary Neal, incredibly was revived after more than 30 minutes submerged in the raging waters in the Los Rios region in southern Chile. Writing *To Heaven and Back*, similarly, she reported events that can only be described as miracles and extracorporeal encounters with heavenly deities. While discarnate, she was provided accurate information of events that tragically would come to pass, thus substantiating the validity of that experience.

Those books, and many others, provide insightful details into often dramatic experiences. What Greyson brings to the table is more than four decades of intensive research and examination of thousands of reported near-death experiences (NDEs), the breadth and depth of which are unparalleled.

For Greyson, when a young aspiring psychiatrist, the adventure began with a serendipitous encounter with a patient who had attempted suicide. Initially, he had seen the comatose patient in the emergency room. It was his follow-up discussion that shocked him. That patient, Holly, told him that she recognized Greyson from a conversation he had conducted with her roommate located some distance from the patient's room. Logically, he knew that it would have been impossible for the unconscious patient to have seen or overheard the discussion. Yet, here she was providing accurate details of that conversation.

Throughout the book Greyson artfully dissects the claims made by skeptics who, materialistically-oriented, reject the notion of NDEs out of hand. One of the most common explanations by such doubters is that the NDEs must be hallucinations. A number of studies have proven that not to be the case. Oft cited by experiencers is that they were out of their bodies (OBE) and able to observe their immediate surroundings. To that end, Greyson sites the study of OBE by Professor Jan Holden in which she found that 92 percent of the reports were totally accurate, 6 percent contained some error, and only 1 percent could be determined to be totally wrong. Given the known fallibilities of human memory, that is an outstanding track record. Anoxia, or lack of oxygen, is often cited as a precipitant of hallucinations. Amazingly, studies have shown that those reporting NDEs actually have an increase in oxygen levels during those events.

Among the critical issues addressed is that of the accuracy of reporting of NDEs by patients who are rightfully concerned about how their mental health will be perceived by medical staff. This points to cases in which interviews conducted over time often reveal substantial details that were previously withheld. When asked, patients have indicated that they were concerned about the reception even after previous discussions were accepted in a positive manner. Patients who have experienced NDEs become quite adept at discerning the amount of detail that their interviewers can entertain, and adjust their responses accordingly. It is reasonable to assume that there is a substantial amount of information that has not been relayed to researchers because of these concerns. Greyson notes that the reluctance to share information is fully justified



as many patients have been greeted with skepticism, as in some cases drugs are prescribed with the intent to suppress perceived hallucinations. Hopefully, the medical community is becoming more aware of such experiences are less likely to reject such reports.

In ***After***, the topics of religion are discussed in fair detail, especially those relating to experiences that are perceived to relate to Heaven and Hell from a Christian perspective. Since Christianity is only one of many religious orientations, questions that arise as to the experiences of those who come from predominantly Hindu, Buddhist, Muslim, or even atheistic backgrounds. Then too there is the question of interaction with discarnate entities or deities. Raised are the concepts of meeting the Christian Jesus, interactions with God, an omnipotent supreme being, or other notions of ineffable experiences beyond human ability to comprehend.

On a more understandable level, he describes reports of individuals who have met with deceased relatives which is reportedly a relatively common experience. Of even more interest, are the observations of interaction with deceased individuals whose death was not known to the patient at the time of the NDE yet later verified.

Readers will find there is substantial discussion regarding the perennial conundrum regarding the brain and the mind. It is noted that the Greek physician Hippocrates was involved in similar thought more than 2000 years ago. Those experiencing NDEs decidedly come down on the side of that the mind is separate from the brain and seems to act as a modulator of thoughts. Included is an interesting quote from Dr. Larry Dossey who stated, “We are conscious not because of the brain but in spite of it.”

Greyson covers many aspects of the NDE reports. Best known are the feelings of peace and tranquility that are often relayed by the patients. But there are other concerns as well. Some of those experiences are decidedly negative at the time but will usually provide some resolution. Then he addresses the other aspects of the process of reintegration into the lives of friends and family. Some of them neither want to accept the incredible stories that the experiencer relates, nor understand that the NDE may have had profound impact on that person resulting in significant changes in their personality. Given NDEs usually happen precipitously, when the experiencer attempts to relate ineffable circumstances, it can create considerable stress in their previously existing relationships. While mommy may have just had a conversation with God, her young children are likely to be more interested in dinner. The potential for conflict is obvious. It is also worth noting that the full impact of the NDE may take considerable processing over a period of weeks, months, and even years.

Of course, Greyson raises questions that remain unanswered and possibly are unanswerable. Why is there such variance in the reports? Why do only a few people have these experiences even though many other people under comparable circumstances do not report similar events? Why do some patients with severely diminished cognitive capability suddenly have short periods of lucidity shortly before death? Can continuation of consciousness beyond physical death be scientifically proven?

To that last question, Greyson in the final chapter lists a series of lessons to be learned from NDEs. Acknowledging there may be other explanations derived later, he states, " . . . .but until then, some form of continued consciousness after death seems to be the most plausible working model." Clearly, a most important aspect of NDEs is the *reduction in fear of death*, as reported by both those who have experienced such events, as well as those who learn about them.

For truth in advertising, readers should know that I have known Bruce Greyson personally for nearly 40 years and consider him both a friend and one of the best researchers in the field. We are both past presidents of IANDS and have come a long way in our understanding of the complexity and importance of NDEs to the patients experiencing them, to the medical community, and to the public in general.

The bottom line is that Bruce Greyson's ***After*** is one of the most important books on near-death experience studies that has ever been published. It is highly recommended reading for all members of the SSE.

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BOOK REVIEW

Smile of the Universe: Miracles in an Age of Disbelief by Michael Grosso. Anomalist Books, 2020. 228 pp. ISBN 978-1949501131.

Reviewed by Todd Hayen

https://doi.org/10.31275/20212275

Smile of the Universe (2020) by Michael Grosso is a delightful book filled with magic and intrigue for the inquisitive reader. Grosso starts our journey with citing many historical events that have been deemed "miraculous" not only by the Catholic church, which seems to have had the corner on miracles since the Church's beginnings, but also those that fall out of the realm of organized religion. What made this book delightful for me is that Grosso's own particular interest in miracles resonates with mine, and I am fairly certain will resonate with many other readers.

In the opening pages of Grosso's book he immediately makes it clear that he personally has experienced a miracle, several in fact. The reader then knows that this book is not just a documentation of dry historical events, but also a publication that presents phenomena the author himself has been personal witness to. We are immediately engaged in both the subjective and the objective: storytelling at its best. And what are miracles but human stories created around a set of unexplainable material events? A dream, what most believe to be purely subjective and containing no material substance at all, is not considered a miracle. But if that dream manifests in material form, then indeed it becomes miraculous. Not all miracles are first dreams of course, but all miracles do have a material construct, and what makes them a miracle is their stubbornness in ignoring the set, presently known, laws of nature.

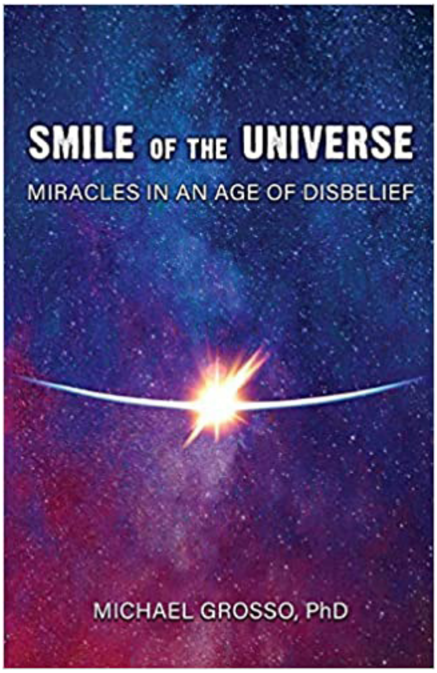
The opening chapters tell the stories of many miracles. Many of them were first documented by the Catholic church. As mentioned above, the Catholic church has always had a robust interest in miracles as they are a required criterion for canonized sainthood (Romolo & Grant, 2019). Grosso defines a miracle as the term is used in his book:

> A miracle, as I use the term, is an event that parapsychologists call *paranormal*, but of a particular type: *miracles are paranormal events that occur in the context of religious belief, symbols, and experience.* In a broader, looser sense, I will use the term to refer to any event that prima facie is physically unexplained, as with non-religious paranormal phenomena, a category much wider than the mainly religious. (p. xiv)

That said, many of the miracles cited are of the religious type.

It is beyond the scope of this review to comment on, or even list, the miracles that Grosso investigates. Although the individual stories are skillfully compressed to be certain that the reader gets enough to understand the full nature of the miracle, they all leave the reader wanting to learn more. Although I have become, over the years, familiar with most of what Grosso cites, I was quite excited to read of some miracles for the first time, and was left wondering why I had never run across these particular stories in my own research—one other reason this book was a delight! There definitely is always more out there to discover! Grosso also leaves some room, when it seems necessary, for skepticism. He presents the miracles "as they are" without any assumptions that would be easy to make if you are a believer, or a Catholic when referencing a Catholic/Christian event. Miracles are, after all, anomalies within the physical cause and effect universe; if they can be explained through a materialist lens, then they should be, as they then no longer fall under the definition of miracle. However, the removal of a miracle from the list of paranormal, metaphysical, occurrences does not always remove its meaning from the affected observers.

After the initial chapters of miracle presentations, Grosso asks some very important questions. One such question is whether a miracle, or what comprises a "beyond physical" event is in fact a "normal" element of reality. An element that has been all but eliminated through



the current worship of dogmatic scientism and materialism. Grosso suggests:

> The word *miracle* one might take as code word for powers latent in all human beings that manifest in special and mostly unpredictable conditions—a word that represents a temptation to push matters as far as possible to the edge of something altogether new. (p. 108)

In my own book *Ancient Egypt and Modern Psychology* (2017), I suggest that in the ancient culture of Egypt there may have been a time where people moved in and out of a "non material" reality without even being aware of it since it was commonplace and simply part of their day-to-day reality. In this metaphysical state they could do miraculous things that did manifest in their material reality. Grosso also suggests this as a current tenet of Tibetan Buddhism: ". . . Tibetans have no words for miracle or paranormal but see such things as natural byproducts of spiritual training, of achieving highly concentrated states of mind" (p. 13).

Grosso devotes one of his final chapters to a concept he attributes to William Blake, *living by miracle*. Blake said, "As to Myself, about whom you are so kindly interested, I live by Miracle."[1] Grosso ventures into a thorough investigation of this concept and how one would proceed to make it a tenet of their own life. Living by miracle could be described as living a life open to the day-to-day occurrences that are unexplainable by material science. But Grosso also suggests that living by miracle is living by the truths of one's own individual construction of reality through an individual imaginal world. He says:

> Words like Blake's have a ring of mystery, but what do they mean? Where can they take us? What would it feel like to live, move, and have our being from inside a miracle-conducive world? I'm of the opinion that we have to follow our own path to become decently evolved individuals. (p. 117)

Grosso continues with this theme for the remainder of his book. He questions how we could go about living a life where all experience is integrated into the lived life, not only the material reality but a non-material reality, a transcendent awareness. Grosso says:

> We may think of ourselves as part of a subliminal self that is greater, wiser, and stronger than our everyday selves. Just being aware of the deeper possible relationship with our unknown self has a power to liberate us. It is a momentous shift in perspective, realizing that part of you lies in a kind of normal latency and oblivion, awaiting the right circumstances to be awakened. (p. 144)

Here is where the message of Grosso's book becomes of paramount importance. He then journeys into different examples of spiritual practice from various cultures and within various historical contexts. He touches on Greek philosophy, Hebrew prophets, he tells the story of Joan of Arc and her visions and voices that led her to lead the French army into the Dauphin. He writes about the early diviners of ancient Greece, and the course of scientific mind through the subliminal mind. He presents the story of Ramanujan, the Indian mathematical genius whose unprecedented ability of connecting seemingly unrelated mathematical concepts came through a spiritual, metaphysical, conduit to the Dravidian goddess Namagiri. All of these are wonderful examples of an integration of the physical, material, perception of the physical world, and a subliminal world of meaning and purpose beyond the material that surfaces into consciousness through means other than the physical senses.

I will conclude my review of this excellent and inspiring book with a quote from the author in his last chapter: *Last Words: Beyond Science and Religion:*

> We have tracked and confronted various reports of miracles, tried to peel away cultural accretions, and zeroed in on the empirical core. Miracles, the most extreme forms of paranormal phenomena, stretch our imagination of the possible. Encased often but not all the time in religious settings, access can be convoluted and awkward. What I found reveals a dizzying picture of super-human abilities, a mirror image of what I believe is latent in us all, by virtue of our being grounded in the substratum of the One Mind—the working hypothesis of this book. (p. 181)

NOTE

[1] From a letter to Blake's friend George Cumberland, 1799.

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BOOK REVIEW

The Science of Spirit: Parapsychology, Enlightenment and Evolution by Luis Portela. Toplight Books, 2021. 1982 pp.

Reviewed by Robert Ginsberg

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When one sees an opening chapter entitled "From Science to Love," it begs for further reading. After all, for most people these are incongruent terms that represent two seemingly opposite sides in a debate, logic and systematic evaluation vs. emotion. There have been many books written about the convergence of science and spirituality, and one cannot help but notice how some of today's physicists are sounding more like spiritualists than scientists, but Dr. Portola uses this platform as wakeup call for humanity.

There is a brief discussion about some well-known enlightened beings in our history, including founders of today's organized religions, and our progression from animism to polytheism, and eventually monotheism, but Portola astutely points out the role that science played in explaining natural phenomena. Our ancestors could not have imagined these discoveries, and it was science that moved us past idol worship to spiritual independence. The author is well-versed in the primary disciplines of research that suggest survival and devotes part of the book to discussions of the nature of consciousness, neuroscience, thought transmission, psychokinesis, near-death experiences, reincarnation, mediumship, and instrumental TransCommunication, but the overall message is clear that science can show us the road but changing the world paradigm starts with individual transformation. In the discussion of the neurosciences and parapsychology it is suggested that perhaps an adjustment to the scientific method is indicated by putting more value on the anecdotal personal experiences that are so prevalent. In his discussion about a paradigm shift, Portola suggests that all people, including scientists and mediums, will understand more by first finding themselves through contemplation and reflection. It is only after we open to the subtle layers of consciousness that we can achieve the necessary balance needed to reach our objectives. The suggestion that scientists also need to contemplate and reflect upon the bigger picture seems contrary to the definition of science, but I suspect that Portola is correct.

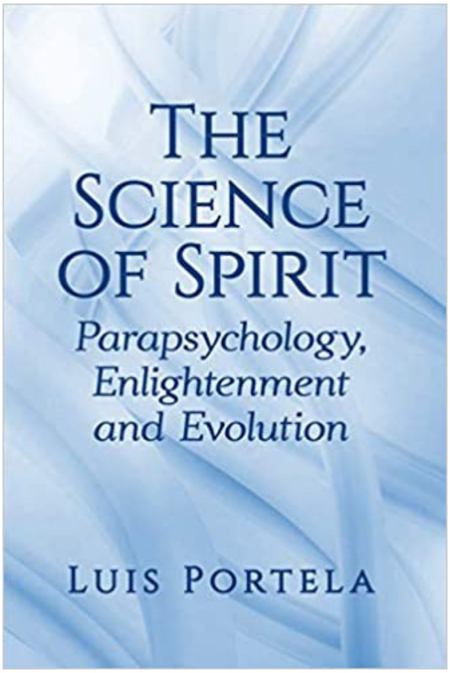
Portela feels that the path to enlightenment begins with us all taking responsibility for our thoughts and actions. He reasons that we are mistaken in blaming our environment for our life conditions, as it is we who choose our circumstances based upon our overall need for growth. Instead of blaming others for our misfortunates, we need to take responsibility for our thoughts, our actions, and for what befalls us. This can come about only after we begin thinking of ourselves as particles of energy and balancing our physical lives with the universality of the non-physical realm, and the fundamental principles on which it is based.

The concept that we bring on both the good fortune and hardship that affect our physical lives is very difficult for many to grasp. Many among those who have lost loved ones, developed critical illnesses, or suffer from poverty and hunger are hard-pressed to accept the fact that they brought this upon themselves. It is more comforting to believe in fate, genetics, the environment, or the actions of others. Although I am certainly open to the possibility, the suggestion that I chose my parents, my environment, and the losses in my life is admittedly a hard pill to swallow. However, Portola urges us to examine our compliance with immutable universal laws and adjust accordingly. Great pleasure or suffering depends upon our recognition and adherence to unifying principles. We may suffer or rejoice in our personal lives, but our collective thoughts and actions also determine our direction on a global level. Emotional, mental, or physical suffering is often a catalyst for exploration and self- discovery, and Portola urges us to use our free will to seek the path of perfection. We do this by focusing on spiritual evolution instead of physical survival.

The author believes that we all tune in to spiritual networks that can be negative or positive. Which network we gain access to depends upon the characteristics of our thoughts. We are attracted to networks that mirror our thoughts and, at the same time, repel the contrary signals. This is a universal concept that applies to both physical and non-physical environments.

Portola includes an interesting discussion on what is referred to as the Astral Body. He cites doctrines that defend the existence of a subtle body, with similar form to the physical body, that is made of an ethereal substance. Both the astral and physical bodies are animated by spirit, which provides the vibrations responsible for thought. The physical brain decodes and interprets the thoughts communicated by spirit to the astral body. The theory is that the astral body is molded by spirit and is the repository of memories, and this body can sometimes be seen by others and is reshaped into other forms once free from the physical body. The connection between the astral and physical bodies is often referred to as the "silver chord" which can stretch during an out-of-body experience and which breaks when the soul separates from its temporary housing at death. As Portola points out, the interesting part of this theory relates to people in a comatose state. Perhaps the cord partially breaks, disconnecting the brain but not the filaments to the heart. I find this theory intriguing, as it might explain why, when the brain apparatus is no longer functioning, the signals are still being received and possibly acknowledged.

Acknowledging that some in the scientific community look askance at past life regression, Portola incudes a chapter examining the practice. He points out that in regression therapy some patients describe scenes from both past and future lives, sometimes validated by evidence. This of course would be consistent with current research into the nature of time, specifically how remote viewers can see targets from the past, present, and future, as well as from psi research that shows both causal and retrocausal phenomena. Regarding receiving information about future events, it is suggested that chance does not exist. Our present conditions are purely the result of our past decisions and attitudes, and our future is something that we construct by our thoughts, actions, and free will. Everything we go through is caused by something that falls within our own responsibility. Try as I may, I struggle with the concept that there are no coincidences and that chance is out of the



equation. My own hypothesis is that some stuff just happens for no apparent reason, and the randomness is part of the design. This is seemingly a contradiction in terms, but it is not all that different from what Dr. Portela describes. How we react to the random stuff put before us, both good and bad, is how we progress.

The discussions about mediumship and intuition are interesting, especially the suggestion that intuition could be considered a form of mediumship. The author defines intuition as the "capacity of direct and immediate knowledge, without recourse to reasoning or the sensory organs," which essentially also defines mediumship if we specify that the information is coming from a discarnate source. Although people are encouraged to incorporate their intuitiveness in their daily lives, Portela wisely cautions against trusting intuition too much. Too much reliance on such information without some thoughtful analysis and contemplation can result in poor decisions and actions.

The author intersperses many pearls of wisdom throughout the book, all related. Some of my favorites include the observation that we have become used to living in lies as if they were truth, despite our hearts telling us otherwise. He urges us to become transparent in our thoughts and practice toward that end daily. He also believes that we reside in a non-judgmental universe, but we must be balanced and recognize that we alone are responsible for our actions. He urges a "vision beyond immediate interests, with a universal perspective, which offers a lucidity of appreciation of those who are not yet perfect and still in an evolutionary phase." He describes happiness as expansive as it invigorates life, and sadness as constrictive and focused inward, weakening the organism. He cautions that both have the power to destroy in excess. Discernment is described as the consciousness of being part of a whole, putting personal interests aside. To discern, one must reflect about what truly matters as one hears the voice of the whole. In discussing universal values, it is suggested that looking for someone else to help might be interfering with the lives of others. The wiser path would be to wait for people to come to you for assistance as they express their trust. The person seeking guidance will then listen, evaluate, and decide what is best for them.

Buying into the spiritual concepts expressed by Dr. Portela necessitates acceptance of some basic principles. Our thoughts and attitudes are governed by the immutable law of cause and effect, and we are always personally responsible for what happens to us. Everything has a reason and chance does not exist. We are on this Earth to learn, self-improve, and we have our own evolutionary trajectories.

The takeaway message from this book is that we can all be considered scientists during our physical lives. We absorb, examine, and contemplate a bigger picture as we seek meaning and purpose. Sometimes we change the direction of our lives as the result of personal experiences, some profound and some subtle, but they cause us to accept another level of reality as we move closer to a knowing state. We live in a world that often fosters the feeling of separateness, individuality, and materialism. Changing the paradigm to one of connectiveness and responsibility is no easy task, but one that Portela most definitely feels we are moving toward.

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ESSAY REVIEW

Unfathomed Dangers from Aluminum—Alzheimer's? Autism? Multiple Sclerosis?

Imagine You Are an Aluminum Atom: Discussions with Mr. Aluminum by Christopher Exley. Skyhorse, 2020. 151 pp., $22.99. ISBN 978-1510762534. Kindle, 14.99.

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That aluminum is fatally toxic is beyond doubt, demonstrated by mass human poisoning at Camelford in Cornwall in Britain in 1988 (Chapter 18) and by occasional fish kills (p. 49). Aluminum may also be a contributing factor in a number of human diseases, in particular those involving brain and nerves (p. 124)—autism, Alzheimer's disease (AD) (Chapter 14), Parkinson's disease, multiple sclerosis. Aluminum salts used in kidney dialysis may be responsible for dialysis-related encephalopathy (pp. 38, 79).

Aluminum adjuvants in vaccine can cause macrophagic myofasciitis (MMF: inflammation and associated microscopic muscle necrosis at the injection site) as well as such whole-body ailments as chronic fatigue syndrome and marked cognitive deficits (Rigolet et al. 2014); one middle-aged individual injected with five aluminum-adjuvanted vaccines within 4 weeks became work-disabled (pp. 72–73).

Studying the possible dangers associated with aluminum adjuvants in vaccines is complicated by the fact that aluminum acts as an antigen as well as an adjuvant—the immune system generates antibodies against aluminum itself, so that later exposures to aluminum might produce an antibody cascade capable of damaging any of the tissues in which aluminum had accumulated (pp. 73–76).

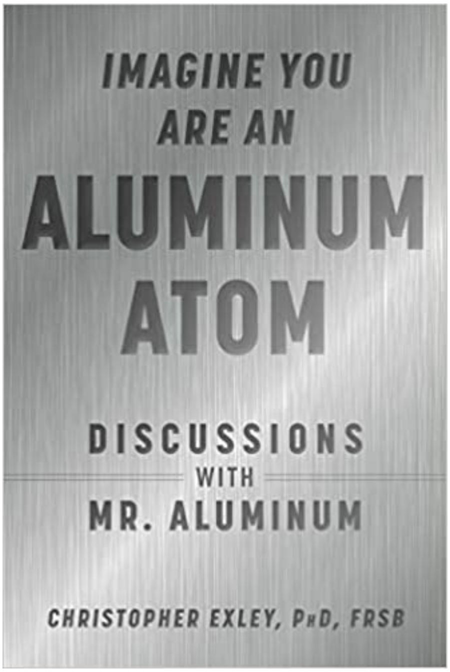
Christopher Exley has studied aluminum in relation to human health for some 35 years. This book summarizes his work and cites the pertinent primary publications (some 200) in appropriate peer-reviewed journals. The book also offers quite convincing evidence of the determined efforts by a variety of vested interests to disparage and suppress Exley's work and findings.

In my opinion, the published work summarized in this book makes a plausible case based on empirical evidence that aluminum may be a contributing causative factor in neurological and nerve diseases. Further, Exley suggests fully detailed mechanisms that are quite plausible for how that comes about. Beyond that, he points to a fundamental a priori reason why aluminum, among all the other elements and metals, might be so uniquely dangerous. It is the third most abundant element in the Earth's crust (after oxygen and silicon), yet there are no known biological uses of aluminum. By contrast, several other metals and non-metals are essential components of some biological systems, for example, iron in hemoglobin in blood (pp. 5–6, 11). Those two facts make it far from implausible that absorption of aluminum could be biologically harmful, by competing with or replacing other metals, or perhaps just because of its chemically oxidative properties.

But if all that is so, how has the Earth's biosphere flourished for billions of years without succumbing to the toxicity of the super-abundant aluminum?

Because naturally occurring aluminum is stored safely in ores not accessible to the animal kingdom. Only since 1889, with the invention of a process for extracting aluminum from its ores (p. 9)—the beginning of what Exley calls "The Aluminum Age"—have innumerable compounds of aluminum been manufactured and used in ways that expose human bodies to aluminum in a variety of chemical forms as well as to the metal itself. Because exposure of the biosphere to aluminum began so recently on the geological and evolutionary timescales, natural selection has not evolved biological mechanisms for protecting living systems against aluminum (p. 6 ff.).

Much of the evidence for the dangers from aluminum is inevitably somewhat circumstantial, since experimentation on human subjects is not possible and there are no suitable animal models. There are also significant technical problems (chapter 5). Even establishing how much aluminum is present in human bodies rests on assumptions about how the element is distributed through various organs and how it enters the body. It remains unknown whether aluminum in urine is a reliable measure of the body burden, or whether the amounts of aluminum in hair or in the blood or in skin tissue represent sound estimates of the body burden. A further complication is that exposure to aluminum can only be estimated, involving a range of assumptions, because it comes from such diverse sources as food, cosmetics, and drugs. One particularly dangerous potential source is the aluminum adjuvant present in a number of vaccines; and investigating that last aspect is hindered not only by the technical difficulties but also because that possibility is anathema to the pharmaceutical industry as well as to the aluminum industry.



That powerfully influential vested interests have attempted to hinder Exley's work is demonstrably, sadly, dismayingly true. That will of course seem quite normal to any other researchers who take minority stances on any scientific matter at all (Bauer, 2012). Thus Exley was attacked viciously (pp. 2–3; p. 132 ff.) by the legions of individuals and groups who label as "anti-vaxxer denialist" anyone who suggests that any vaccine might occasionally have damaging "side" effects—even though his article about aluminum in autistic brains had only a single by-the-way mention of a possible relationship: "Paediatric vaccines that include an aluminum adjuvant are an indirect measure of infant exposure to aluminum and their burgeoning use has been directly correlated with increasing prevalence of ASD."

The saddest illustration of the campaigns against Exley's research is the manner in which his own university tried to prevent support of his research by outside donors (p. 142 ff.), after changes in the university administration coincided with funding to the university from the Gates Foundation and from a British pharmaceutical company.

The strongest evidence for Exley's claims about harms from aluminum comes from brain tissues from deceased individuals who had suffered from AD or autism: Those brains contain unusually high amounts of aluminum (pp. 81–83, 84, 100); by contrast, normal brain tissues did not contain appreciable amounts of aluminum. There is corresponding circumstantial *negative* evidence as well: Control brain tissues from individual donors who did *not* die with a diagnosis of AD, Parkinson's, multiple sclerosis, or autism have only low amounts of aluminum (p. 94). Furthermore, the first reported case of familial AD came within two decades of the beginning of the Aluminum Age (p. 101); and at least one case report is of early onset AD in an individual occupationally exposed to aluminum (p. 28).

There are indications that genetic predisposition to Down's syndrome and familial AD (p. 100) may also predispose to absorption and accumulation of aluminum; perhaps those diseases would not manifest even in those genetic circumstances in the absence of aluminum (p. 27)?

Aluminum does seem to accumulate in the body over time even absent of neurological diseases (p. 25). Acidic soils facilitate uptake of aluminum by plants whose products enter the human diet via tea, coffee, soy products (p. 36). Aluminum salts are very cheap, and they are used in food processing to improve texture and color. Aluminum also gets into food via cans and other packaging material, and too much is present in many infant formulas (pp. 65–66).

There are no evidence-based guidelines, let alone regulations, as to aluminum in food. The European Food Standards Agency regards 1 mg of aluminum per kg of body weight per week as safely tolerable, but Exley regards this as ***in***tolerable, pointing out that it is based on only a few studies in animals (pp. 30–31). In Bavaria, pretzels were once toxically contaminated by aluminum owing to the use of sodium hydroxide in conjunction with aluminum baking trays.

Human skins are exposed to appreciable amounts of aluminum compounds in antiperspirants, sunscreens, and other cosmetics (pp. 14, 39). Smoking tobacco leads to ingestion of aluminum (p. 42). Aluminum-based antacids have actually been suggested to be risk factors for AD (p. 37).

But perhaps the most clearly and directly dangerous aluminum compounds are the adjuvants in some vaccines (p. 68); aluminum adjuvants have the advantages of being cheap, sufficiently but not too toxic, and unregulated (p. 85). As the vaccine is created, the amount of aluminum is continually increased until the vaccine yields the desired antibody response. That adjuvants can cause harm is suggested by Merck's Gardasil anti-HPV vaccine which produces 24,000 instances of injury for every 1 million injections (p. 85). But the dangers of aluminum adjuvants are not revealed in safety trials because the controls include adjuvant rather than being genuinely inactive placebos (pp. 69, 85).

Thus Exley gives the clear impression that he regards aluminum as a modern Satan: an omnipresent evil. "The chances of being overloaded with aluminum are actually quite high" (p. 89). The book ranks aluminum as a significant risk factor (p. 90) for anemia, asthma, autoimmune conditions, breast cancer (Chapter 15), chronic fatigue syndrome, epilepsy, Gulf War Illness, multiple sclerosis (Chapter 16), Parkinson's disease, problems with fertility and reproduction, vascular disease. "If I could remove all aluminum from my body, I would expect an overall increase in vitality" (p. 91); "We now know that Alzheimer's disease can be prevented [by eliminating aluminum]" (p. 103).

All this seems excessive, but elsewhere in the book Exley makes clear that he is offering his own convictions, acknowledging that the objective evidence is not yet conclusive; he insists only that the case is strong enough to warrant further research.

Unfortunately, despite that disclaimer, the book's mode of presentation makes it easy to accuse Exley of "protesting too much" in seeing dangers from aluminum everywhere. Skeptical readers may find too much speculation based on too little hard evidence; for example, "there are sufficient indications across scientific literature to suggest that its [aluminum's] effects [on human reproduction] are more widespread than currently acknowledged" (p. 61), raising the question whether human exposure to aluminum could be a contributing factor in the lower sperm count in men in the developed world (p. 62). Or, that "burgeoning childhood allergy" may be "linked to increasing everyday exposure to aluminum" (p. 70) since infants are exposed to aluminum through infant formulas, baby powder, and antacids. The colorings in children's sweets often contain aluminum compounds; perhaps it is the aluminum in the sweets rather than the sugar that may play a role in "abnormal behavior such as Attention Deficit Hyperactivity Disorder (ADHD)" (p. 71).

The documented information provided by this book should be of interest to everyone, but Exley does not make the best case for himself—far from an uncommon problem with researchers who take a path not trodden by others and whose work is perpetually hindered by powerful vested interests. The book's publisher could and should have improved the presentation greatly through competent copyediting. The book's ridiculous title hardly entices an average reader: *Aluminum as Cause of Alzheimer's?* (say) would surely have attracted more media attention and book sales. The absence of an index is inexcusable. The lack of a glossary is frustrating: that aluminum in hair could be measured "using either TH GFAAS or ICP MS" (p. 20) is not very illuminating if one has no idea what TH GFAAS or ICP MS are.

A good copyeditor would also have reduced the number of repetitive complaints about a "hostile and nonscientific background of ill-found criticism" (p. 77), or naming fairly prominent individuals as aluminum ambassadors "who have accepted the 40 pieces of silver that are always on offer" (p 97), or similar grouches (pp. 86, 98, 110, 125, and more). Not that these words and sentiments are unjustified; they are quite justified. Exley is quite typical of researchers who push minority views and cannot get proper satisfaction in the face of official misdeeds. not only rank suppression but failing to disclose conflicts of interest on the part of Journal editors (p. 129) who also simply ignore substantive critiques of articles they published (p. 141). However, these repetitive complaints just preach to the choir, hardly a strategy for encouraging initially unbiased readers to look carefully at the body of rather solid evidence for the central substantive claims in the book.

A competent copyeditor would also have eliminated or had clarified a few points where the text is unclear or even seems wrong: If "dwarf thistles are identical species to . . . tall thistles," how can the "former have a different genetic makeup"? (p. 112). Again, to claim that "world class computational chemistry" has "proven both the existence and pro-oxidant activity" of aluminum dioxide (p. 114) even though "it remains to be identified directly in any biological milieu" surely places too much confidence on empirically untested theory. "Is it only a coincidence that those countries in the world using the most sunscreen have the highest incidence of melanoma?" (p. 16)—perhaps not; but that would not necessarily indict the aluminum in sunscreen, the cause might well be the unusually high exposure to sun rays that brings high use of sunscreen.

The most striking claim in this book is that aluminum can be eliminated from the body by drinking silicon-rich mineral water (Chapter 9), and that AD (and perhaps autism and other aluminum-caused ailments) can be cured in this way (p. 103). This belief originated as anecdotal evidence published by Exley and his mentor in 1989: The toxicity of aluminum to fish appeared to be eliminated in silicon-rich acid waters (Birchall et al. 1989). Additional circumstantial evidence comes from a small trial in which drinking silicon-rich mineral water facilitated urinary elimination of aluminum in individuals diagnosed with AD; most remarkably, significant improvement of cognitive abilities in people with AD was reported in 20% (3 of the 15 participants) after they drank, for 12 weeks, 1.5 liters per day of silicon-rich mineral water (p. 55). The book also claims "positive reports" of benefits of silicon-rich mineral water "in relation to Alzheimer's disease, multiple sclerosis, autism, epilepsy, and vaccine injury" (pp. 58–59).

This reader's skepticism was aroused, however, by the book's caution that "there are no effective alternatives to natural silicon-rich mineral waters" and that silicon and silica supplements sold in health stores have not been shown to facilitate removal of aluminum from the body (pp. 57–58). But silicic acid is just an ordinary chemical substance. This book describes its origin as from leaching of the Earth's silicon-rich crust by rain (p. 52). I cannot understand why silicic acid could not then be synthesized from silicon materials by leaching with acidic water, thereby avoiding the difficulties Exley describes in obtaining mineral waters for his research (pp. 54–55) and his apparent current reliance on the surely expensive importing of proprietary mineral water from Malaysia. I could not readily believe that synthesis of silicic acid for research by Exley requires “sophisticated laboratory equipment” and that it is difficult to make it “biologically safe” (p. 58): How biologically safe is natural, silicon-rich mineral water commercially bottled in Malaysia?

This book’s claims about AD, autism, and other ailments in possible connection to aluminum should be considered by everyone. Further research is desperately needed.

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Contributions can be empirical research, critical or integrative reviews of the literature, position papers, policy perspectives, and comments and criticism. Studies can adopt diverse methods, including qualitative, ethnographic, historical, survey, philosophical, case study, quantitative, experimental, quasi-experimental, data mining, and data analytics approaches.

A. **Regular Articles** (~12K words max). Primary research or interesting and important theoretical papers that foster the diversity and debate inherent to the scientific process. This entails novel or innovative ideas that have some 'fragmentary' experimental or empirical support but which can be evaluated with logic and open-mindedness to present academia with provocative hypotheses that would otherwise be rejected by most conventional journals.

—All empirical results that have not been replicated should be called 'preliminary' with the findings treated as such. Peer-review and publication priority will be given to studies that are (a) pre-registered or (b) replications. Note that 'replication' can involve repeating the research procedure in a (nearly) identical separate study to be reported within the same paper (e.g., 'Study 2: Replication'). Or, large datasets can be divided randomly into 'Training' and 'Test (or Validation)' sets, i.e., the research findings presented are those results that replicated in the Test set.

—To promote stricter transparency and context for readers, all analyses where appropriate should provide effect size statistics in the form of direct percentages of either *association* (correlative analysis) or *mean percentage differences* (ANOVA, *t*-tests, etc.). In the case of correlative analysis, reported results shall report $R2$ to provide a covariance percentage estimate. Mean tests shall provide a 'percentage change' indicating the actual percentage change between groups (e.g., $M = 3.44$ Group 1 versus $M = 4.02$, in Group 2, on a five-point scale is calculated by the following: ABS [$M1 – M2/5$ (scale range)] = 11.6% shift or change in means). Standard effect statistics also are allowed, so long as the above percentage techniques are likewise reported. These statistics should be reported in results as 'percentage effect' and follow immediately after standard statistical analysis notation. For correlation, ($r = .43$, $p < .01$, percentage effect = 18%), for means tests ($M1 = 3.44$ versus $M2 = 4.02$, $t = 3.443$, $p < .01$, percentage effect = 11.6%).

B. Systematic, Narrative, and Scoping Reviews (~13K words max). All meta-analyses and systematic reviews should include a PRISMA flow diagram to clarify for readers how the exclusion/inclusion criteria were applied to create the literature set under consideration: See http://www.prisma-statement.org/

C. Brief Reports—Rapid Publications (~2K words max). These are usually pilot studies, direct or conceptual replication attempts of previous work, case studies, brief evaluations, reviews, or 'citizen scientist' efforts that are unique, first-time reports, with no more than two tables and/or figures and 10 references. This rapid publication option is especially appropriate for graduate-level student studies, pilot or preliminary research, or descriptions of important new methods or instrumentation. These reports are subject to blinded peer review in the same manner as research articles. Authors should follow all requirements for longer manuscripts when submitting Brief Reports, including that they have not been submitted or published elsewhere.

D. Book Reviews (~2K words max). Structured for readability and utility in which the content is suitably contextualized and includes links to general model-building or theory-formation in the respective domain(s). Please use the following headers, or otherwise incorporate these themes into the review: 'Author Disclosures; Content Overview; Pros, Cons, and the Book's Contributions to the Literature; Recommendation; and References' (if applicable). For an example, see: https://www.spr.ac.uk/book-review/poltergeist-night-side-physics-keith-linder

E. **Essays** (~8K words max). Important conceptual or philosophical commentaries, observations, or arguments to spark constructive discussion or debate relative to theory, methodology, or practice.

F. **Letters** (~1K words max). Must address substantive issues relative to recently published content in the Journal.

Submissions (A) to (C), and (E) as appropriate, must also include the following sections:

—Lay Summary (~150 words max). Placed at the beginning of the article before the scientific abstract, this is a short—3 to 5 sentences—bottom-line assessment of the value of the paper. Any technical terms used require a short parentheses description (which will educate). Include a closing conclusion sentence where it is clear that this is the researchers' interpretation of the results.

—Implications and Applications (~150 words max). Placed immediately after the Discussion section to succinctly summarize or suggest how the study's methods or findings can potentially inform the study of other issues, anomalies, fields of study, including interdisciplinary and multidisciplinary approaches.

—**Author Contributions** (Contributor Roles Taxonomy). Please include this subsection under the Acknowledgments section. Follow standard guidelines such as this one from Elsevier: https://www.elsevier.com/authors/policies-and-guidelines/credit-author-statement. Also, please include ORCID numbers for authors where possible (on the online submission page).

—**Data-sharing requirements**. Primary (raw) data (redacted for confidential or personally identifying information) must be uploaded to a freely accessible repository for independent verification or analysis by qualified researchers and the URLs shared in the paper and in a section called Data Availability under the Acknowledgments section. The Journal can provide such space.

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