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Journal of Scientific Exploration
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# Whither UAP Data?[1]

GUEST EDITORIAL

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*[NB: this editorial uses the term UFO/UAP instead of either UFO or UAP, except when referring to early works where the original term UFO is referenced or in modern works where UAP is explicitly noted.]*

Recently, an intense public interest in unidentified anomalous phenomena (UAP) has prompted a number of scientists to get involved in the topic. However, they have found that concrete scientific data on the nature of UFO/UAP is lacking.

> Although many UAP reports remain unsolved or unidentified, AARO assesses that if more and better quality data were available, most of these cases also could be identified and resolved as ordinary objects or phenomena. Sensors and visual observations are imperfect; the vast majority of cases lack actionable data, or the data available is limited or of poor quality (United States Department of Defense, 2024b, p. 7).

How can this be? Several databases of unidentified flying object (UFO) and/or UAP cases are known to exist and have been used in analyses to interpret and understand the nature of the phenomena. Despite this, there seems to be reluctance among scientists to embrace the hundreds of thousands of reported UFO/UAP observations as evidence of non-human intelligence on Earth. Why? Indicative of the view of such databases is the mandate of the Galileo Project, which states:

> The goal of the Galileo Project is to bring the search for extraterrestrial technological signatures of Extraterrestrial Technological Civilizations (ETCs) from accidental or anecdotal observations and legends to the mainstream of transparent, validated, and systematic scientific research (Harvard University, n.d.-b, para. 1).

However, regarding UFO/UAP report databases:

> Moreover, the Galileo Project will not engage in retroactive attempts to analyze existing images or radar data, or speculate on prior UAP, observations or anecdotal reports, as these are not conducive to cross-validated, evidence-based scientific explanations (Harvard University, n.d.-a, para. 1).

In order to understand any kind of phenomenon, it is necessary to gather data so that analyses can be done. This holds true whether the subject is climate change, political preference, traffic congestion, or UFO/UAP. Preferably, the data in question would be

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acquired through dedicated instruments, sensors, random sampling, polls, or repeatable observations. However, this is not always the case.

Areas of study such as birdwatching, human relationships, and plane- or trainspotting all contribute to knowledge through gathering and reporting of subjective data, much of which may not be repeatable or recordable. When it comes to UFO/UAP, belief about the nature of such phenomena is often based on the many reports of anomalous objects over the past several decades. The amount of UFO/UAP data varies greatly depending on the source, but it is generally agreed that there are tens of thousands, if not hundreds of thousands, of UFO/UAP sightings on record. The National UFO Reporting Center, based in Washington State, has over 170,000 international UFO/UAP reports on file, the Canadian UFO Survey has about 24,000 Canadian UFO/UAP reports, and the United States Air Force's infamous Project Blue Book investigated and recorded 12,618 UFO reports from around the world between 1952 and 1969 (see: https://nuforc.org/about-us/ and https://www.canadianuforeport.com/survey/essay/2011survey.html).

Many believers argue that this large body of cases must be enough to prove there is a real phenomenon, if not proof of actual alien visitation. Could so many UFO witnesses be right? (ABC News, 2008). Scott Waring, editor ufosightingsdaily.com, states: "Something big is going on for so many UFO sightings world wide to be occurring" (Martin, 2020, para. 12).

Alas, UFO/UAP data is more problematic than most UFO zealots appreciate. Many studies have shown that eyewitness testimony is often flawed, incorrect, and inaccurate. A recent book that is essentially a meta-analysis examining the reliability of UFO/UAP witnesses addressed this in great detail:

> The acceptance of a true anomaly behind UFO sightings lies in the dogma that the testimony of witnesses is absolutely reliable, even if the stories told are abnormal by mainstream science standards. But this is far from certain; it is merely a presumption that matches the fantasies of the proponents. Single witnesses and shortage of material verification lie in the antipodes of how real-life works. Not only are there no error-free witnesses, but people's imagination and prejudices can play unforgettable games on them (Olmos & Heiden, 2023, p. 10).

The problem of reliability in UFO/UAP data was summed up by Hendry (1979), a researcher with the Center for UFO Studies, in his landmark work *The UFO Handbook*, over forty years ago, but largely forgotten. Hendry raised serious concerns about UFO data, "Do UFO statistics represent a valid pursuit for more knowledge about this elusive phenomenon, or do they merely reflect frustration that none of the individual reports are capable of standing on their own two feet?" (Hendry, 1979, p. 269). Hendry challenged UFO researchers who presented UFO case data as proof of an unexplained phenomenon and asked some hard questions:

1) Does the report collection reflect truly random sampling?
2) Have the individual cases been adequately validated?
3) Are apples and oranges being compared? Are NLs (Nocturnal Lights) necessarily the same kind of UFO as DDs (Daylight Discs)?
4) Are differing details among cases obscured through simplification for the purpose of comparisons?
5) Does the study imply the question: "Surely this mass of data proves UFOs exist?"
6) Do the correlations really show causality? (p. 269).

Each of these questions, while proposed decades ago, are still relevant to any examination of UFO/UAP reports today. Let us look at each one with consideration of the current state of UFO/UAP data collection.

## Does The Report Collection Reflect Truly Random Sampling?

For the first question, the randomness of a collection of UFO/UAP reports is highly dependent on the agency receiving report data. Is UFO/UAP reporting itself random? Can we be sure that UFO/UAP witnesses represent a true cross-section of the population, or is there some bias in favor of those who 'believe in UFOs' and therefore may report identified flying objects (IFOs) as UFO/UAP? Are there other biases involved? For example, military observations of UFO/UAP are not routinely made available to civilian researchers. Are these cases somehow different from civilian-reported cases?

In fact, in responding to questions from reporters following the release of the recent All-domain Anomaly Resolution Office (AARO) report, acting director Tim Phillips made this comment about UAP data:

> So we've had about 1,200 cases that we've looked at. We approximately receive anywhere between 90 and a 100-110 a month from the operating forces. And you'll see in our reporting, there's a real bias to the Department of Defense because they're out there flying. They tend to have the advanced sensors. And if you're clearing

> a range before you go hot, if you're looking for something, you might find it. We're starting to see an increase in civil aviation sightings, you know, from—through the FAA and through NASA. We're starting to get few or more cases in, and you'll see that reflected in our heat map on our website. You'll see, with the bright red, that tends to reflect where DOD is operating, where they have those detections (United States Department of Defense, 2024a, para. 40).

In other words, AARO UAP reporting is heavily biased towards military reports and military bases geographically. As for other sources of UFO/UAP data, Hendry (1974) stated explicitly that because of "a skewed geographical distribution for report collection ... UFO groups are not getting a portrayal of U.S. "hot areas" and "cold areas" (p. 244), as a function of uniformly distributed, random sampling.

The question of "hot spots" of UAP reports has often been raised by UFO/UAP fans and media, who select specific areas of interest, sometimes based on Zip Codes and other factors such as population density and overall report numbers (CBS News, 2017; James, 2023; University of Utah, 2024). A frequent belief is that UFO/UAP sightings tend to occur over nuclear facilities and military bases (Janos, 2019; Porritt et al., 2023).

One definitive result of statistical studies of UFO/UAP looking at geographical distribution is the fact that UFO/UAP sightings are related to population density. Essentially, the greater the population density, the higher the number of reports. This is logical in that since it is UFO/UAP sightings that are being studied and not UFO/UAP themselves. It makes sense that the more potential witnesses available, the more reports will be generated. Because of this, there is a tendency for more UFO/UAP sightings to be reported from metropolitan centers. However, this was not completely related to the population itself. In self-reported cases, witnesses often note a nearby town or city, which may not be the actual location of where the UFO/UAP was observed. When investigators enter UFO/UAP case data, similar errors might arise.

With so many biases and other factors involved, it is uncomfortable, to say the least, to suggest that UFO/UAP data are rigorous enough to confidently use in statistical studies.

## Have The Individual Cases Been Adequately Validated?

In a perfect world, each UFO/UAP case would be fully documented and thoroughly investigated by trained researchers with unlimited time and expenses, as well as through perfect cooperation with civilian and military authorities. In reality, though, this hardly is the situation. UFO/UAP investigations are often conducted by relatively untrained enthusiasts with little free time, sometimes working in isolation from official sources of useful information. Many UFO/UAP investigators do not have formal backgrounds in astronomy, meteorology, or aviation—fields that are useful in evaluating reports of unidentified flying objects. Thus, there is no way to ensure that all cases contributed are adequately validated.

The good news is that a small percentage of UFO/UAP enthusiasts do go on to receive additional instruction and guidance and become field investigators. Training for UFO/UAP investigation is offered for field investigators in MUFON, including interview techniques and the use of Geiger counters. However, varying biases among UFO/UAP investigators can reduce objectivity and skew results. Furthermore, the number of investigators varies by region, making onsite investigations impractical in some areas of North America.

## Are Apples and Oranges Being Compared? Are NLs (Nocturnal Lights) Necessarily the Same Kind of UFO as DDs (Daylight Discs)?

We do not know the answer to this question with certainty. However, since nocturnal objects constitute the vast majority of UFO/UAP cases, this may not be a serious problem. We can also ask if all nocturnal objects are themselves homogeneous. Is UFO/UAP data concurrently valid with itself? Hendry (1979) noted:

> ... A thin veil can often separate an identifiable object from a truly 'worthy' UFO. Beware of statistical exercises that boast of thousands of reports in the data; there aren't thousands of well-investigated reports. Also, beware of attempts to catalog every known UFO in a certain category (e.g., the Center for UFO Studies' Physical Trace [CE II] Catalogue) when cases are plugged into the statistics, whether they are anecdotal or well studied. Efforts to weight the probability of cases or even to divide them into certain or non-certain groups virtually never appear in these compilations (p. 268).

Hendry (1979) suggested caution in this regard: "The huge variety of UFO shapes and behaviors works against any casual grouping of UFOs. Collections of IFOs are a mixture of many unrelated sources with unrelated characteristics—is this also true for the UFOs?" (p. 268).

Even the All-domain Anomaly Resolution Office (2023) may have fallen victim to this issue when it released a cumulative set of "Typically-Reported UAP Characteristics" reported by military witnesses.

### Are Differing Details Among Cases Obscured Through Simplification For The Purpose of Comparisons?

This is true to a certain extent. A witness who chooses red as the primary color of a UFO/UAP with red and white lights may have made an error of judgment. Similarly, when the data is encoded, 'red and white' could be entered differently from 'white and red.' In some categories, this is a valid concern. In others, such as date and location, this is not a problem. However, when evaluations of cases are made, subjective interpretations will certainly cause some difficulties.

### Does the Study Imply the Question: "Surely This Mass of Data Proves UFOs Exist?"

As noted earlier, the answer is clearly "No." All that can be said is that people are reporting sightings of unusual objects, some of which have no simple explanation. Hendry (1979) warned:

> Beware of attempts to collect as many reports as possible of, say, EM [electromagnetic] interference cases or abductions accompanied by the question: "can all of these people be wrong [or liars or crazy]?" Remember, for every valid UFO judgment there were nine UFO impostors—IFOs—where the sincere witnesses were most certainly wrong. You seldom get reminded of this truism, however (p. 268).

### Do the Correlations Really Show Causality?

While it is tempting to use UFO/UAP case data to prove correlations and gain insight into the nature of alien spacecraft, the data are simply not homogenous enough for such conclusions. It is known that almost all UFO/UAP reports are either explainable or have insufficient information for meaningful analysis. Therefore, using data from a collection of UFO/UAP case reports means that what is being correlated are not characteristics of UFO/UAP but only witnesses' observations of objects.

In fact, Hendry (1979) advised that UFO/UAP report data that is missing some parameters should be suspect:

> In my own reports, I would never have dreamed of making an IFO/UFO judgment without important parameters like shape and duration. Instead of dumping these reports into the "insufficient information" pile where they belong (or better yet, seeking out the additional data), they saw fit to make commitments to them. To judge reports like these as "UFOs" and "IFOs" and to include them in the chi-square tests is sloppy investigative and statistical process. Nor should they have included "not stated" figures in the chi-square tests at all, since "not stated" is not a characteristic of the "knowns" and "unknowns," just of poor investigation (p. 267).

Some UFO/UAP organizations seem to focus on their record of unexplained and unsolved cases and classify them as "unknowns," but in many instances, these might be better classified as "insufficient information."

A good example illustrative of problems in interpreting UFO/UAP data is the series of statistical studies of UFOs by neuroscientist Michael Persinger, often cited for his scientific approach to ufology. Persinger published more than one hundred papers on the subject, many in peer-reviewed journals, to support his view that electromagnetic fields are correlated with UFO/UAP sightings. One of Persinger's papers purported to show a correlation between the variance of UFO report numbers and seismic events in southern Manitoba, in accordance with what was posited as the tectonic strain hypothesis (Persinger & Derr, 1985). This suggests relationships between small, undetectable seismic events deep underground and observations of luminous phenomena.

Persinger and Derr (1985) had shown "large correlations" in other areas that experienced seismic events within about 150 km of reports of UFOs, and had been challenged to test the hypothesis in a seismically-inactive area, specifically southern Manitoba, Canada. From 1974 to 1977, a significant number of UFO reports were recorded for a region centered on Carman, Manitoba (Persinger & Derr, 1985). But, Persinger found that through expanding the radius of influence to more than 1000 km and counting seismic events three years before and three years after the Carman UFO flap suggested a correlation. During that period, there were exactly two seismic events of significant magnitude, and those were nearly 500 km away and 870 km away from Carman, Manitoba, and were not even sensed by any Manitoba residents. Despite this, Persinger and Derr (1985) stated, "We concluded that the temporal distribution of seismic events with 1250 km of Winnipeg and the occurrence of UFORs [UFO reports] during the 34 mo. that constituted the Carman episode indicate the two phenomena are related" (p. 811). It was further pointed out by Persinger and Derr (1985) that the

data used to show a relationship between UFO reports and seismic events were almost all not UFOs at all.

In a report on Manitoba UFO sightings, data from a bar graph were used to compare the number of UFO reports in Manitoba with earthquake occurrences in nearby Northern U.S. states, supporting the hypothesis. However, the same report emphasizes the fact that most UFO sightings are easily explained as astronomical or aeronautical objects. Knowing this, we can then wonder what the statistical correlations might mean. It is suspected that inaccurate and/or inadequate data are being used to verify an unfounded physical mechanism. Indeed, if as some researchers believe, all UFOs are explainable without invoking this hypothesis, then the hypothesis is possibly an artefact of the statistical analyses (Rutkowski, 1986). It is absurd to think that proof of a geophysical phenomenon could be based on misidentifications of stars, planets, aircraft, and satellites, yet that was what was proposed. The statistics were sound, but the data were inappropriate.

Similarly, an organization that advertises on behalf of casinos and online gambling sites for several years has been sending news releases to media on topics that show the odds or likelihood of various events, including the sighting of UFO/UAP. A recent release titled "Here's How Likely You Are to Get Abducted by Aliens in Provinces Across Canada" stated,

> In a new report published by Casino.ca, the provinces in Canada where you're most likely to get abducted by aliens were revealed, and the results may surprise you. Using data provided by the National UFO Reporting Centre, the gambling site was able to rank and rate sighting hotspots across the country.
>
> "Each data point was categorized into its corresponding province/territory," Curiocity was told in an email. To make an accurate assessment, the sightings were divided by the province/territories population, to allow us to understand the sightings per capita.
>
> So, who came out on top? Who else but the Yukon! According to the company, the Northern destination ranked #1 overall, followed by the Northwest Territories, New Brunswick, and Nova Scotia (Stefanic, 2024, para. 1–6).

As there are currently a total of 24 UFO/UAP reports from Yukon in the NUFORC database, this works out to a per-capita rate of UFO/UAP observation of 0.06%. In comparison, NUFORC lists 2,539 UFO/UAP reports for Ontario, with only a 0.02% per-capita rate of UFO/UAP observation. Hence, the statistics bear out that Canada's least-populous territory has a higher rate of UFO/UAP observation than the largest province by population. Of course, this is a preposterous result. In 2023, there were only two UFO/UAP reports from Yukon filed with NUFORC, only one in 2022, and none at all in 2021. Yet, Ontario had 133 UFO/UAP reports in 2023, 216 in 2022, and 215 in 2021. Apart from the fact that "ordinary" UFO/UAP sightings by themselves have not been shown to be directly related to the abduction phenomenon (if it exists at all), claiming a trend based on only one or two data points per year is not supported by reality.

Unfortunately, even when good data are used, interpretations can be misleading. In 2022, a Canadian politician made public his inquiry into UFO/UAP observed "in or near Canadian nuclear facilities." He noted several "open-sourced incidents," such as:

> May 24, 2021, 9:34 pm
> Pickering, ON
> Over Pickering Nuclear Plant, 4 to 5 orange lights, very slow moving, one at a time came from the water/behind nuke plant. They would get to a certain point in the sky and then vanish and then another one would come from behind the other lights like they were trading places … (Maguire, 2022, p. 18).

There are several issues with this kind of UFO/UAP report. First, assuming the person reporting was legitimate, were the UFO/UAP seen actually over the nuclear facility, or were they simply in the line-of-sight direction? How was this determined? Was an onsite investigation made? Pickering is a city directly under a flight path from Toronto to Ottawa and within a heavily populated region of Ontario, Canada. It would be expected that many aircraft could have been in the skies that night. Also, May 24, 2021, was a national holiday, and there would have been some fireworks and paper lanterns sent aloft to celebrate that night, so the UFO/UAP might have had mundane explanations.

This means that using this particular report as a data point in a study of UFO/UAP correlated with nuclear facilities could skew any results or interpretations that UFO/UAP show an interest in nuclear facilities. With this in mind, it is prudent to be cautious about larger and more robust studies that are said to show correlations between UFO/UAP and military and/or nuclear facilities, such as those published by the Scientific Coalition for UAP Studies (e.g., Porritt et al., 2023).

Hendry (1979) noted that,

> Military bases have sentinels and twenty-four-

> hour logged records of daily events, unlike most of the rest of the world. Consequently, it can seem that they have more UFOs 'hovering nearby.' Similarly, police are out patrolling while the rest of us are asleep (p. 262).

In a paper about the misuse of statistical correlations, Vogelstein (2020) advised:

> Be modest about the role of statistical inference in scientific inference ... "Scientific inference is a far broader concept than statistical inference"... Because of the strong desire to inform and be informed, there is a relentless demand to state results with certainty ... Resist the urge to overreach in the generalizability of claims ... Accept that both scientific inference and statistical inference are hard, and understand that no knowledge will be efficiently advanced using simplistic, mechanical rules and procedures (p. 6).

Then, there is the issue of the quality of UFO/UAP reports themselves. In its Historical Record Report published in March 2024, the US government All-domain Anomaly Resolution Office (AARO) noted its concerns regarding the nature of UAP data:

> Previous and current investigations have been challenged by insufficient data and information for intelligence and scientific analysis to resolve anomalous incidents. Insufficient data and information [n.b.] was compounded by inconsistent reporting and lack of continuity among investigations and investigative practices. Capt Ruppelt, the first director of Project BLUE BOOK, noted that the inability to collect the UFO's altitude, size, and speed was a recurring and significant obstacle to resolving cases... Most UAP sightings have no data associated with them beyond an often vague narrative account, and when there is hard data, it is often incomplete or of poor quality. In terms of military reporting, the sensors on which UAP most frequently are captured are calibrated and optimized for combat (United States Department of Defense, 2024b, p. 38).

What kinds of UFO/UAP data then are useful in helping to better understand the phenomena? These vary depending on the institution, agency, or investigator and can change with time. CUFOS, for example, in its large UFOCAT database of cases, included the categories of the source of the report, date, time, location, state and county (or country), the numbers, ages, sexes, and names of the witnesses, the type and special features of the report, the number of objects seen, duration, size (estimated or angular), and latitude/longitude. Some UFO/UAP organizations, such as MUFON and NUFORC, have lengthy reporting forms asking for details such as date, time, location, shape of object seen, color, estimated size, estimated distance, and the number of witnesses. Other organizations have short forms and rely more on a textual description of what was experienced in witnesses' own words.

As mentioned earlier, eyewitness testimony is not always as good as most people believe. AARO recognized this and noted:

> Some literature suggests individual accounts can be unreliable as they are subject to a person's interpretation of sensory data through the filter of their experiences, beliefs, or state of mind during the event. A person who reports a case might be credible, in that they believe the elements of their account to be accurate. However, their reliability, which is their ability to accurately interpret events—as well as to recall and convey those events due to a range of factors—is altogether different from their inherent sincerity (United States Department of Defense, 2024b, p. 12).

Each detail of a particular report, however, has its own set of limitations and interpretations. The category of duration is interesting in that it represents the subjective length of time a witness believes a UFO/UAP experience lasted. Naturally, these times are greatly suspect because it is known that people tend to misjudge the flow of time. Yet some individuals appear to be good at estimating time, so this value does have some meaning. Although an estimate of "one hour" in a particular case may be in error by several minutes, it is unlikely that the correct value would be only one minute. Furthermore, there have been cases when a UFO/UAP was observed and clocked accurately so that we can be reasonably certain that UFO/UAP events can last considerable periods of time. A case of extremely short duration might not have enough content to be considered truly unexplained, but a long-duration case would likely be explainable as a star or planet.

Previous analyses have shown that long-duration sightings tend to occur in the early morning hours, from about midnight until 6:00 a.m. It is probable that the majority of observations at this time are those of astronomical objects moving slowly with the rotation of the Earth. Duration data by itself is not wholly useful

in analyzing UFO/UAP behavior. Duration events of a few seconds to about ten seconds are usually fireballs or bolides, while very long duration events of an hour or more are very probably astronomical objects. In between, there can be no way to distinguish conventional objects from UFO/UAP solely with duration data. Hendry (1979) cited a Canadian study by an Ontario UFO group which timed aircraft observations and found that the duration of such sightings varied between 15 seconds to more than eight minutes.

Even a witness' estimate of the duration of his or her UFO/UAP sighting, which can give insight into the nature of the object being observed, can be problematic. Hendry (1979) advised, "Duration is a powerful feature of identity when it refers to extremely short and long events, but is otherwise mostly a reflection of the witness's behavior during the event, coupled with the fluctuating behavior of the objects watched" (p. 249).

Hendry further expanded on this:

> I regard it as a mistake to expect "duration" to stand on its own feet. Consider all the conditions that affect duration that have to be checked out on a case-by-case basis:
>
> —Did the witness start watching the object from the "start?"
>
> —Did the witness leave the event without watching it to its completion? (This is disappointingly common.)
>
> —Did the witness have a wide-open view of the sky (horizon to horizon)? Was it partially restricted by trees, buildings, or clouds? Was his view severely restricted by, say, looking out a window?
>
> —Was the witness stationary, or did he try chasing the object in a car? (Not uncommon.) (p. 249).

And that is just one of the characteristics of a typical UFO/UAP report that can be entered as data into a database for analysis.

Another set of data usually collected for a UFO/UAP case is the shape of the object observed. The shape of a perceived object depends on many factors, such as the witness' own visual acuity, the angle of viewing, the distance of viewing, and the witness's own biases and descriptive abilities. Nevertheless, in combination with other case data such as duration, shape can be a good clue towards a UFO/UAP's possible explanation. However, witnesses' descriptions of the shapes of UFO/UAP vary greatly. A large percentage of reported shapes are simply "point sources"—that is, "starlike" objects or distant lights. In recent years, it has become common for a witness who has seen simply a light in the sky to label it an "orb."

It is important to determine if this was just a judgment on the part of the witness. "Orb" is commonly used by UFO/UAP fans to describe a simple light observed in the night sky, even at a great distance, believing the light to be a much larger object, or something that is spherical in nature, despite the human visual limitation and inability to determine an actual shape of a distant light. Therefore, "orb" should not automatically imply a spherical object.

The classic "flying saucer" or disc-shaped object is quite rare in UFO/UAP reports today, comprising only about five percent or less of all reported shapes. It is worthwhile noting that a disc-shaped object viewed on the edge will appear to be a cylinder or a cigar-shaped object, and when viewed from above or below, would appear as a circle or sphere. Waxing or waning shapes such as a football or egg—or indeed, a "tic tac"—could be simply discs viewed from other angles. Therefore, a witness's opinion on the shape of an observed UFO/UAP, unless observed at close range, should be suspect.

What about color? Does a witness' description of the color of a UFO/UAP constitute good data? Again, no. Apart from the obvious problem of some people being color blind, even those unaffected by that condition can misjudge the colors of the lights in the sky. A distant white light can appear to be yellow, orange, or reddish if seen through a dusty atmosphere, and if a light is sufficiently dazzling, determining a color might not be possible at all.

If so many recorded characteristics of UFO/UAP can be in doubt, is there value in databases of report data at all? Hendry (1979) was highly critical of any statistical studies of UFO data:

> There is hardly a statistical effort that has ever been applied to the UFO phenomenon that is not problematic in its construction or interpretation. Short of some seemingly impossible changes in the collection mechanisms that feed these efforts, it hardly seems likely that such efforts in the future will fare any better. That doesn't mean that they won't be undertaken, as the temptation to reduce large bodies of UFO data to statistical conclusions is very strong; "overinterpretation" is always the real menace (p. 268).

Given the often incomplete nature of UFO/UAP data, is there any way that databases of report data can be useful? First, it should be noted that simply having statistics on dates and times of UFO/UAP sightings is useful in determining reporting and temporal trends. Having exact dates and times can be used to compare with known

satellite passes, aircraft flights, astronomical phenomena and events, and occurrences of balloon launches, fireworks festivals, and celebratory paper lantern releases. These alone could lead to explanations for many UFO/UAP sightings. If there are multiple reports from a small geographical area on a certain date, known as flaps, the likelihood of a mundane explanation is increased.

Over time, the variance of the number of UFO/UAP reports, spanning decades, can show trends known as "waves," which may be noticeable over large regions of the world. For instance, the increase in UFO/UAP reports at the beginning of the COVID-19 pandemic might have been at least partially due to more people in isolation spending time looking into the night sky.

Despite the noted limitations of other variables, as long as statistical studies do not purport to support definitive explanations for UFO/UAP, they can show trends in how witnesses themselves are reporting UFO/UAP. As the term "flying saucer" fell out of use over time, reported shapes such as triangles become more common, probably due to the popularization of these alternative shapes in media.

Recently, following the "leaked" US Navy video of a UAP shaped like a "tic tac," this shape began being reported by other witnesses. In the 1800s, witnesses described unidentified aerial objects as "airships," as there were no other kinds of objects that flew in the sky. Later, unidentified objects were thought to be unusual "aeroplanes." Mysterious objects tend to be described in terms of objects popular during a given era. The biggest problem with UFO/UAP data, however, is the lack of consistency or standardization of the data itself. Almost every UFO/UAP organization created its own method for recording witnesses' observations in reporting forms. This was true both for government and civilian agencies. While paper reporting forms have only relatively recently transitioned to digital files, the majority of UFO/UAP reports are now self-reported on online websites.

The key attributes of observed UFO/UAP, therefore, vary greatly depending on the investigating agency or person, and this can cause problems when entering data into a spreadsheet for analysis. Indeed, the person entering the data may need to interpret the UFO/UAP characteristics, even when a coding key is employed. There is no "central repository" or collecting agency for UFO/UAP, with sightings reported to a variety of organizations, both civilian, and governmental. Many individuals, associations, clubs, and groups claim to investigate UFO/UAP reports and otherwise solicit reports from the general public. However, very few of them actually participate in any kind of information sharing or data gathering for scientific programs. Many are only interest groups, perhaps based in museums, planetariums, church basements, or members' homes, and do virtually nothing with the case reports they receive. Indeed, because there is no way to enforce standards in UFO/UAP report investigations, the quality of case investigations varies considerably. Some researchers do not maintain useable case files and do not retain quantitative criteria in their investigations (e.g., alien abduction or contactee groups).

The Canadian UFO Survey attempted to circumvent some of the concerns with UFO/UAP data files with two adjustments. The first was to accept report data from UFO groups, but only the lowest common denominators. Only basic UFO/UAP report data, such as report date, time of day, and witness location, were initially collected. If other specifics such as color, shape, and duration of the sighting were available, these would be included, but with an understanding of the limitations involved. The second adjustment was that in addition to entering the available data, the researcher entering the data would make an evaluation of the quality of the report based on the Hynek Scale of Strangeness and Reliability.

Hynek (1972), an astronomer, was a scientist who took the subject of UFOs very seriously and believed that reports could be analyzed for insight into the nature of the phenomenon. In his landmark work, *The UFO Experience: A Scientific Inquiry*, Hynek noted two factors that would be very useful in studying UFO reports:

> The Strangeness Rating is, to express it loosely, a measure of how 'odd-ball' a report is within its particular broad classification. More precisely, it can be taken as a measure of the number of information bits the report contains, each of which is difficult to explain in common-sense terms...
>
> Assessment of the Probability Rating of a report becomes a highly subjective matter. We start with the assessed credibility of the individuals concerned in the report, and we estimate to what degree, given the circumstances at this particular time, the reporters could have erred. Factors that must be considered here are internal consistency of the given report, consistency among several reports of the same incident, the manner in which the report was made, the conviction transmitted by the reporter to the interrogator, and finally, that subtle judgment of how it all 'hangs together' (p. 25).

Through these adjustments, UFO/UAP reports could be triaged so as to refine the data and reflect the

intrinsic quality of the cases in question. Finally, using these additional criteria, it would be possible to make a judgment as to whether a specific UFO/UAP report had a simple explanation (e.g., a Starlink train), a possible explanation, insufficient information for analyses, or no apparent explanation. Such a conclusion would not have any meaning in terms of evidence or alien visitation, of course.

Polls have shown that about 10% of North Americans believe they have seen UFO/UAP. This means that about 40 million Americans have had UFO/UAP experiences. If you have seen a UFO/UAP, you are in good company with many, many others. UFO/UAP witnesses range from farmhands to airline pilots and from teachers to police officers. Witnesses represent all age groups and racial origin. What is being observed? In most cases, only ordinary objects. However, this begs a question. If people are reporting things that can be explained, then the objects they observed were "really" there. Were the objects we cannot identify "really" there as well? If so, what were they? These are questions that only continued and rational research can answer, and only if researchers have the support and encouragement of both scientists and the public. If enough high-quality data on UFO/UAP reports are gathered, analyses of the data may be able to shed light on the true nature of the phenomenon. That does not even include the possibility of non-human involvement in the equation.

## ENDNOTES

[1] The title of this editorial is in reference to an excellent paper by ufologist Richard Hall: "Whither Ufology?" (https://www.nicap.org/papers/whither_ufology.pdf), prepared for January 19-21, 2002, at a "Think Tank" sponsored by the Fund for UFO Research. Hall envisioned a "formation of a nonpartisan National Public Fact-Finding Commission" composed of scientists, politicians, astronauts, journalists, and even former Presidents who would, as a body, evaluate UFO evidence from a scientific point of view. Hall may have, in turn, borrowed the title of his presentation from none other than Allan Hendry, whose chapter "Whither UFOCAT?" is in his 1979 book, *The UFO Handbook*.

## REFERENCES


ABC News. (2008, September 12). Could so many UFO witnesses be right? https://abcnews.go.com/Primetime/story?id=5790432

All-domain Anomaly Resolution Office. (2023, April 17). UAP reporting trends. https://www.aaro.mil/UAP-Cases/UAP-Reporting-Trends/

CBS News. (2017, April 26). Californians reported the most UFO sightings in recent years. https://www.cbsnews.com/news/californians-reported-the-most-ufo-sightings-since-2001/

Harvard University. (n.d.-a). Scope. Retrieved May 19, 2024, from https://projects.iq.harvard.edu/galileo/scope

Harvard University. (n.d.-b). The Galileo project: "Daring to look through new telescopes." Retrieved May 19, 2024, from https://projects.iq.harvard.edu/galileo

Hendry, A. (1979). *The UFO handbook: A guide to investigating, evaluating, and reporting UFO sightings*. Doubleday.

Hynek, J. A. (1972). *The UFO experience: A scientific inquiry*. Regnery.

James, H. (2023, October 13). What are the odds of seeing a UFO in each Canadian province? *Super Casino Sites*. https://www.supercasinosites.com/2023/10/03/what-are-the-odds-of-seeing-a-ufo-in-each-canadian-province/

Janos, A. (2019, June 23). Why have there been so many UFO sightings near nuclear facilities? *HISTORY*. https://www.history.com/news/ufos-near-nuclear-facilities-uss-roosevelt-rendlesham

Maguire, L. (2022, March 4). [Untitled]. https://www.larrymaguire.com/_files/ugd/ba2ab2_16ce66b178fa4cf2b33e3c34150a3b6a.pdf

Martin, S. (2020, October 22). UFO fleet seen over Walmart in California "aliens want you to see." *Express*. https://www.express.co.uk/news/weird/1350589/ufo-fleet-video-california-alien-news-aliens-conspiracy-theory-evg

Olmos, V.-J. B., & Heiden, R. W. (2023). *The reliability of UFO witness testimony*. Edizioni UPIAR.

Persinger, M. A., & Derr, J. S. (1985). Geophysical variables and behavior: XXXII. Evaluations of UFO reports in an area of infrequent seismicity: The Carman, Manitoba episode. *Perceptual and Motor Skills, 61*(3), 807–813. https://doi.org/10.2466/pms.1985.61.3.807

Porritt, I. M., Hancock, L. J., & Grosvenor, S. (2023). UAP pattern recognition study 1945-1975 US military atomic warfare complex. *Zenodo*. https://doi.org/10.5281/zenodo.7295958

Rutkowski, C. A. (1986). Geophysical variables and behavior: XXXIV. Further comments. *Perceptual and Motor Skills, 63*(1), 18. https://doi.org/10.2466/pms.1986.63.1.18

Stefanic, C. (2024, March 14). Here's how likely you are to get abducted by aliens in Canada. *Curiocity*. https://curiocity.com/aliens-canada-abductions-2024/

United States Department of Defense. (2024a, March 6). Media engagement with acting AARO Director Tim Phillips on the historical record report

gagement-with-acting-aaro-director-tim-phillips-on-the-historical-recor/

United States Department of Defense. (2024b). Report on the historical record of U.S. government involvement with unidentified anomalous phenomena (UAP). https://media.defense.gov/2024/Mar/08/2003409233/-1/-1/0/DOPSR-CLEARED-508-COMPLIANT-HRRV1-08-MAR-2024-FINAL.PDF

University of Utah. (2024, March 13). Scientists have identified the best region to spot UFOs. *SciTechDaily*. https://scitechdaily.com/scientists-have-identified-the-best-region-to-spot-ufos/

Vogelstein, J. T. (2020). P-Values in a post-truth world. *ArXiv* (Cornell University). https://doi.org/10.48550/arxiv.2007.03611

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RESEARCH ARTICLE

# The Composition of Ian Stevenson's *Twenty Cases Suggestive of Reincarnation*

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## HIGHLIGHTS

The answer to why some people rather than others develop specific maladies might be found in the concept of reincarnation as implied by Ian Stevenson's work, although the mainstream medical community dismisses this idea.

## ABSTRACT

Ian Stevenson was trained as a physician and, later, as a psychiatrist. He made significant contributions to biochemistry, psychosomatic medicine, and other areas before turning to parapsychology in mid-career. From the start of his involvement in parapsychology, Stevenson was interested in claims to remember previous lives. As his research with such claims progressed, he became convinced of reincarnation's potential to shed light on unresolved problems in medicine. This paper describes the background and traces the development of Stevenson's classic collection of case reports, *Twenty Cases Suggestive of Reincarnation*, whose first edition appeared in 1966. Stevenson expected his monograph to be recognized as making the important contribution he believed it did and thought that it would lead to public funding for further research on reincarnation. Sixty years on that has yet to happen, perhaps due to Stevenson's emphasis on the proof-oriented aspects of the cases he reported, to the neglect of other issues that might have connected more easily with mainstream interests, and more directly countered criticisms of his research methodology.

## KEYWORDS

Case studies, field research methodology, history of parapsychology, reincarnation

## INTRODUCTION

Some years ago, I published in this journal a historical review of Ian Stevenson's *Twenty Cases Suggestive of Reincarnation* (Matlock, 2011). That article was based exclusively on published sources, but I have since been given access to unpublished correspondence and other materials housed at the Division of Perceptual Studies (DOPS), a unit within the Department of Psychiatry and Neurobehavioral Sciences at the University of Virginia, and at the New York-based Parapsychology Foundation. These unpublished materials support a much more nuanced portrayal of Stevenson's early engagement with parapsychology and the lengthy process that led to the book's appearance in the *Proceedings* of the American Society for Psychical Research (ASPR) in 1966 (Stevenson, 1966).

*Twenty Cases* proved to be a commercial success for the ASPR and was republished in 1974 by the University

Press of Virginia (Stevenson, 1974a). A paperback edition introduced in 1980 is still in print, a highly unusual circumstance for a university house. Not only is *Twenty Cases* Stevenson's best-selling collection of case reports—and the only one of his several books that many people have read—it has become a classic in parapsychology and reincarnation studies, justifying this updated look at its composition.

The first section of this article is based largely on published accounts (Kelly, 2013; Stevenson, 1989, 2006; White, 2007). Unless otherwise noted, all information in later sections derives from the unpublished materials I obtained from DOPS and the Parapsychology Foundation.

A who's who of people figuring in the narrative is presented in the Appendix.

## STEVENSON'S EARLY LIFE AND CAREER, 1918–1955

Ian Stevenson was born in Montreal on October 31, 1918, near the end of the Great War on the continent of Europe and at the start of the global influenza pandemic that followed. He was the second child and second son of his parents; his brother Kerr was 21 months old when he was born. Their mother, Ruth, who was English, and father, John, who was Scottish, had moved to Canada when John accepted a post as a political correspondent for the *Toronto Star* newspaper. The family resided in Ottawa when Stevenson entered the world.

From birth, Stevenson suffered bouts of bronchitis, which in infancy led to bronchiectasis, the permanent inflammation of the bronchial tubes. Bronchiectasis may have a genetic origin and may be associated with diseases such as cystic fibrosis, but Stevenson's family had no history of chronic respiratory infections, nor was Stevenson afflicted with cystic fibrosis or other sometimes contributory ailments. The influenza pandemic in his first year and the harsh Canadian winters he endured thereafter likely aggravated his condition, which appears to have been congenital (Stevenson, 2006, p. 13). Although he learned to manage his bronchiectasis, it remained with him and may have been the source of what, late in life, he identified as the "leitmotif" of his career, the question of why one person rather than another developed a given malady (Stevenson, 1989).

In 1923, when Ian was about five, Ruth decided to take him and Kerr to Los Angeles, in the hopes that the milder climate would improve Ian's health. John remained in Ottawa, but visited them occasionally over the two years they lived in California. Their stay there appears to have achieved its principal aim: The exacerbations of Ian's bronchiectasis abated and perhaps ceased for a while. Neither Kerr (White, 2008) nor Ian (Stevenson, 1989, 2006) mentions them as a feature of this period.

While living in Los Angeles, Ruth became acquainted with Richard and Isabella Ingalese, whose occult philosophy was a variation on the Theosophy of H. P. Blavatsky popular in those years. After Ruth and the boys returned to Ottawa in 1925, Theosophy began to occupy a major place in her library. Ian read many of those volumes, but because he could see no way to evaluate their claims, he was unpersuaded by them.

Ian continued to be bothered by the bronchiectasis that kept him out of school for days at a time, but thanks to an unusually retentive memory, when his health was good, he jumped ahead of his peers academically. At the age of 13, in 1931, he was sent to an innovative "public" ("private" in American parlance) school called Bryanston in Dorsetshire, England. Bryanston employed the Dalton Plan, an educational system influenced by Montessori methods. It emphasized self-pacing and group learning rather than teacher-centered classroom instruction. At Bryanston, Ian developed a strong interest in history. According to Kerr, he memorized "almost every historical date of importance worldwide" (White, 2008, p. 13). At the back of many of his books, he made notes of dates, places, people, events, and other facts he wished to remember, as well as errors he encountered. In 1935, he started keeping a record of the books he read. He maintained this record until 2004, three years before his death, at which point it included 3535 entries.

In 1937, Ian enrolled at the University of St. Andrews in Scotland, intending to major in history, but he was there for only two years. World War II was in the offing when he returned home for the summer of 1939. His medical condition made him ineligible for military service and he switched to McGill University in Montreal for the fall term. At McGill, he studied physics, chemistry, and biology, along with history. After receiving his B.Sc. in 1940, he enrolled in McGill's medical school. He excelled at this new pursuit, completing the four-year program for an M.D.C.M.[1] degree in three years and graduating at the top of his class in 1943.

Stevenson did the first year of his postgraduate residency at Royal Victoria Hospital in Montreal, conducting research in biochemistry. Unfortunately, his bronchiectasis returned and intensified, and after several instances of pneumonia, he was advised to relocate to a warmer and dryer environment. He completed his residency and internship at St. Joseph's Hospital in Phoenix, Arizona, in 1945–46, and the following year, held fellowships in internal medicine at the Alton Ochsner Medical Foundation and in biochemistry at the Tulane University School of Medicine, both in New Orleans, Louisiana. The move to the southern United States brought about an immediate

improvement in his health, as had the earlier sojourn in Los Angeles.

As Denis Fellow in Biochemistry at Tulane, Stevenson undertook research on the oxidation of rat kidney slices in association with Emil L. Smith. Stevenson and Smith's findings ran counter to the views of German chemist Otto Warburg, who in 1931 had been awarded the Nobel Prize in Physiology or Medicine. Stevenson thought little about the significance of this until a German acquaintance told him that his paper could not have been published in Germany, owing to the esteem accorded Warburg there. This introduction to the politics of academic publishing left a great impression on Stevenson: "From this episode I may date my strong interest in all the obstacles that confront the conduct of original research and the communication of its results," he later said (Stevenson, 1989).

The research with rats had another, more profound effect on Stevenson, and brought about another change of direction. To use rat kidneys in their experiments, he and Smith had to sacrifice the rodents. He found this repugnant and realized he was not interested in reductionist science and its concern with body parts, but would rather devote his efforts to "something closer to whole human beings" (Stevenson, 1989). He applied for and received a Commonwealth Fund fellowship to study with Harold Wolff and Stewart Wolf at New York Hospital in New York City. Wolff and Wolf were establishing reputations in psychosomatic medicine, exploring the role of mental states in disease etiologies. Stevenson made an additional life-altering decision at this time. Before leaving for New York in the fall of 1947, he married pediatrician Octavia Reynolds, whom he had been courting in New Orleans.

The New York Hospital group was concerned with a variation of Stevenson's leitmotif question: Why, under stress, did one person develop asthma, another high blood pressure, and a third a peptic ulcer? Harvard physiologist W. B. Cannon had previously shown that fear and rage evoked changes in the body similar to those that came with physical exertion. He called this the "fight-or-flight response": The body reacted to perceived threats by preparing to do battle or to flee. Wolff and others at New York Hospital elaborated on Cannon's idea with conjectures about the symbolic meanings of physiological responses to stress. A woman who reacted to her predicament with a running nose was trying to wash her troubles away; a man whose bronchial tubes constricted during an asthma attack wanted to shield himself from an unpleasant truth. Some of Wolff's group sought to identify attitudes that would predictably induce certain symptoms.

Stevenson had little patience with this sort of effort. He could not believe that cardiac arrhythmias served any meaningful purpose for those afflicted by them. He noticed that physiological responses similar to those appearing under stress might occur when people were unusually happy and began to collect examples of physical symptoms associated with pleasurable emotional states. Beethoven and Goya, for instance, were ailing, but declined and died in response to news that made them ecstatically happy. His colleagues' reaction to these accounts was not what he had expected. Wolff, in particular, continued to insist that physiological symptoms had meanings and served purposes for the persons experiencing them. Stevenson waited to publish his findings on the physiological effects of positive emotional states until his two years at New York Hospital were over. He published many other articles in psychosomatic medicine over the next few years, more than 30 altogether between 1949 and 1954 (Kelly, 2013).

In 1949, Stevenson became a naturalized U.S. citizen.[2] That fall, he returned to New Orleans as Assistant Professor of Psychiatry and Medicine at the Louisiana State University School of Medicine. Three years later, he was promoted to Associate Professor of Psychiatry, having enrolled in the New Orleans Psychoanalytic Institute, then a branch of the Washington (D.C.) Psychoanalytic Institute. He left psychosomatic medicine not so much because of differences with colleagues at New York Hospital, but because it had failed to develop into the specialty he had anticipated. He considered going into internal medicine, but psychiatry, he thought, offered greater promise for studying the effects of mental states on the physical body.

Stevenson remained in psychoanalytic training until 1958, although he did not care much for it. He judged some of what he learned to be beneficial, but the atmosphere of the institute, tightly focused on the teachings of Freud and a few of his followers, ran counter to his eclectic inclinations. For Freudians, religion and art were expressions of infantile cravings. Adult psychological conflicts stemmed from failures to overcome the Oedipus (for men) or Electra (for women) complexes, sexual attractions to parents of the opposite sex. Stevenson considered Freud's ideas no less reductionist than the biochemistry he had left behind. Moreover, when put to the test, these ideas failed to find empirical support. In *Sex and Repression in a Savage Society*, anthropologist Bronislaw Malinowski (1927) had shown that the Oedipus complex did not obtain among the matrilineal peoples of the Trobriand Islands and so could not be universal.

Stevenson read Aldous Huxley's (1954) *The Doors of Perception* when it was published and shortly thereafter met Huxley, although exactly when and where I have not been able to determine. Stevenson was impressed

by Huxley's account of what happened when he ingested mescaline, the active substance in peyote, and by the potential of psychedelic drugs for psychiatry. Huxley held that a healthy brain acted on consciousness like a reducing valve, restricting conscious awareness to that which was useful in navigating the material world and furthering the survival of the species. Psychedelics like mescaline disabled the reducing valve by depriving the brain of glucose, letting through a wider range of perceptions and greater access to a Mind at Large. Over the next few years, Stevenson took or had administered to him a variety of drugs and anesthetics, mostly LSD and mescaline, as part of a hunt for narcotics that could assist in psychiatry. He was among the first academics to conduct research on psychedelic drugs. Between 1957 and 1961, he authored or co-authored ten journal papers on the topic (Kelly, 2013).

Reflecting on the drugs' effect on him, Stevenson observed that his body's sensory apparatus was defective: His eyesight was poor, his hearing imperfect, and his sense of smell dull. By contrast, Octavia was a gifted amateur artist, with acute perceptions that permitted her to take in aspects of the physical world to which he was oblivious. Mescaline vastly improved his appreciation of this outer world. The beauty of the colors he saw inwardly under its influence made him forever after more sensitive to color in both art and nature. LSD was different. It brought not beautiful colors, but memories of his early life. In one LSD session, he had a mystical experience, a sense of unity with all things. Following his second session, he passed three days "in perfect serenity" (Stevenson, 1989).

These experiences increased Stevenson's conviction that mind and body were independent entities. Consciousness certainly interacted with and was affected by the brain, but he could not understand how the brain could produce consciousness. He could not believe that his brain generated the images he saw while under the influence of the drugs, even though the changes to his neurochemistry engendered by the drugs made these images possible. During his drug trips, he apprehended nothing that did not originate in his mind. He had no verifiable extrasensory perceptions, as were sometimes reported with psychedelics. His drug experiences enhanced his interest in extrasensory abilities, but were not the genesis of that interest, he wrote (Stevenson, 1989).

## ENTRY INTO PARAPSYCHOLOGY, 1955–1958

From its inception in 1935, Stevenson's record of books read shows that he perused volumes on parapsychological phenomena along with literature, history, the healing arts, philosophy, and occasional works of occultism and Theosophy. Gradually, his reading on topics closer to parapsychology increased. In 1951, he took up Jan Ehrenwald's (1948) *Telepathy and Medical Psychology*. At the end of 1954, he read J. B. Rhine's (1953) *New World of the Mind*. The latter book is primarily a popular summary of experimental research conducted by the Parapsychology Laboratory at Duke University, but on page 242, Rhine mentions the American Society for Psychical Research (ASPR), which he notes had a Medical Section that included Ehrenwald and other psychiatrists. Stevenson had not gotten in touch with the ASPR during his stint at New York Hospital and seems to have been unaware of the Society's existence before the mention by Rhine, but he was quick to act on the information. He joined the ASPR in February 1955 and began to receive its quarterly journal.

In March 1956, Stevenson was asked if he wished to be considered for the tenured position of Professor of Neurology and Psychiatry and Chairman of the Department of Neurology and Psychiatry at the University of Virginia School of Medicine. He was receptive, although he did not want to leave New Orleans for another year, due to commitments there. He stated that he was concerned with integrating psychiatry with other areas of medicine, undergraduate teaching, research, and writing. In his job interview, he admitted to a burgeoning interest in parapsychology.[3] Stevenson's appointment at the University of Virginia came in March 1957, effective July 1. He was not yet 39 years old when he and Octavia moved to Charlottesville in the summer of 1957.

Stevenson read Morey Bernstein's *The Search for Bridey Murphy*, a bestselling account of an age regression to an apparent previous life, when it was published early in 1956. At some point, Stevenson got in touch with Bernstein, but this correspondence has not survived, and we cannot be certain of the date. That they were in contact is clear from a July 1956 letter Stevenson received from philosopher C. J. Ducasse of Brown University. From 1951, Ducasse had been a member of the ASPR's Board of Trustees and was book review editor of the Society's journal, the *Journal of the American Society for Psychical Research (JASPR)*. Ducasse had a longstanding interest in reincarnation as a possible way of surviving death (a topic he had explored in his 1953 *Nature, Mind, and Death*), and was then working on his *A Critical Examination of the Belief in a Life After Death* (Ducasse, 1961). Ducasse (1956) reviewed *The Search for Bridey Murphy* and later (Ducasse, 1960) responded to the skeptical backlash the case received. Ducasse also was in contact with Bernstein, who mentioned Stevenson to him.

In his July 1956 letter, Ducasse invited Stevenson to review hypnotherapist Milton V. Kline's edited collec-

tion of essays, *A Scientific Report on The Search for Bridey Murphy* (Kline, 1956), for *JASPR*. Stevenson's review duly appeared in the January 1957 number. It was his first publication in parapsychology and his first dealing with reincarnation. He noted that there had been considerable reaction to Bernstein's book from psychiatrists, such as the authors recruited by Kline, who argued that the hypnotic subject might have constructed Bridey's life story from things she had heard and read about Ireland. This position could not rightly be called scientific, because no alternatives were considered, Stevenson held. Although, for various reasons, Bridey's story did not provide strong evidence for reincarnation, the regressed subject had recalled sundry recondite details that could not be explained as knowledge picked up casually. The case, therefore, deserved more than a curt rejection, in line with preconceived assumptions. "In proclaiming science the authors have only succeeded in defending orthodoxy," Stevenson concluded (1957, p. 37).

At the University of Virginia, Stevenson continued to produce articles for medical journals on topics ranging from how children acquire behavior, to the role wishes play in dreams and psychoses, to the emergence of multiple personality disorder (Kelly, 2013). He completed his first book, *Medical History-Taking* (Stevenson, 1960c). But, increasingly, his attention was shifting to parapsychology. In 1957 and 1958, he wrote to several other persons affiliated with the ASPR, including Laura Dale, office manager and editor of the Society's publications, as well as psychiatrist Robert Laidlaw and social psychologist Gardner Murphy, both members of the Board of Trustees. Laidlaw, recently retired from the psychiatry department at Roosevelt Hospital, was in private practice in New York. Murphy, who had served as president of the American Psychological Association in 1944–45, had been closely involved with the ASPR throughout the 1940s when he was at Columbia University. His involvement lessened in 1952 when he moved to Topeka, Kansas, to become Director of Research at the Menninger Foundation, although he continued on as the ASPR's First Vice-President and, in 1962, succeeded to the presidency.

During the same period, Stevenson sought out Eileen Garrett, who had teamed with philanthropist Frances Payne Bolton to found the Parapsychology Foundation in New York City in 1951 (Alvarado et al., 2001). Bolton, who was independently wealthy, gave up much of her salary as a member of the U.S. House of Representatives from 1940 to 1969 to various causes, including parapsychology. With Bolten's financial backing, Garrett sponsored professional conferences and publications and oversaw the distribution of funds through research grants.

By 1958, Stevenson was sitting with mediums in Philadelphia and other cities. He aspired to undertake studies of age regression to previous lives, à la Bernstein. He had heard of a Louisville, Kentucky, housewife who had regressed to the life of a Confederate soldier who had fought at Shiloh and Nashville, and proposed to bring her and others to Charlottesville for observation and testing. Another investigation concerned a woman, to whom Stevenson assigned the appellation T.E., who, when regressed under hypnosis, identified herself as a man named Jensen Jacoby and spoke Swedish responsively, if only to a limited extent. Jensen could understand English and reply to it, but he responded more readily when addressed in Swedish. He spoke some English, but in a heavily accented and halting manner. The regressions in question had occurred in eight sessions between 1955 and 1956, but they had been tape-recorded and could be assessed by persons other than the two Swedish speakers who had conversed with Jensen. Besides having the tapes appraised by linguists, Stevenson arranged for new sessions and interviewed persons acquainted with the family to verify that T.E. had not had an opportunity to learn Swedish in her present life.

At the suggestion of Laidlaw and Murphy, Stevenson applied for a Parapsychology Foundation grant and received $1,500 for 1959, allowing him to give up some of his clinical hours for parapsychological research.

## CENSUS OF REINCARNATION CASES, 1956–1960

Stevenson and Ducasse shared an interest in reincarnation and traded opinions about books and authors from their first interchange in July 1956. In a September 1956 letter, Stevenson wrote that he had read a book by DeWitt Miller on reincarnation. It was mostly nonsense, he thought, but it had a chapter by a psychiatrist, Russell G. MacRobert (1956), who interpreted purported memories of past lives as spirit obsession. Stevenson thought this might be true in some instances, but probably not all. In reply, Ducasse informed him that MacRobert was a member of the ASPR's Medical Section. MacRobert took survival seriously and was interested in mediumship, but he assumed reincarnation was impossible because of its apparent conflict with mediumistic communication.

Ducasse sent Stevenson a manuscript in which he discussed means of retrieving past-life memories. Stevenson commented that he had doubts about the value of hypnosis because of the extraordinarily heightened suggestibility that is a feature of the hypnotic state. This might make a great many spurious recollections possible. Of course, it would not disqualify memories including information that could be independently verified or those, like the T.E.-Jensen Jacoby case, with responsive xeno-

glossy. Still, the most valuable cases seemed to Stevenson to be those of involuntary recoveries of memories in the waking state. He had come across seven accounts of the latter kind in the scholarly and popular literature. He found it interesting, "although not surprising in view of what we know about memory," that all seven of the spontaneous cases had child subjects. An example was the 1911 Italian case of Alexandrina Samona, in which a child appeared to be reborn to the same parents and was given the same name as her predecessor.

This is Stevenson's first reference to a "census" of past-life memory claims he was undertaking. Stevenson and Ducasse did not correspond again until the end of August 1957, by which time Stevenson was in Charlottesville. He had continued to collect reincarnation accounts and now had "about 75," 20 of which, if they had transpired as reported, he judged to be quite good evidentially, requiring either reincarnation or retrocognitive ESP as explanations.

The ASPR's Laura Dale referred Stevenson to Henrietta Weiss-Roos, who had been identified by a sensitive as a reincarnate, although she had no past-life memories. Dale also passed on to Stevenson a February 1958 letter from Hemendra Banerjee, director of the Seth Sohan Lal Memorial Institute of Parapsychology in Sri Ganganagar, Rajasthan, India. Banerjee proposed an international registry of reincarnation cases similar to Stevenson's census, but of yet-undocumented claims. Stevenson replied to him in March with encouragement and suggestions and began to hear from Banerjee about unreported Indian cases, which he added to his census.

When the ASPR announced an essay contest in honor of William James (Essay contest, 1958), Stevenson stepped up his search for published accounts of reincarnation. This activity consumed most of the hours he spent on parapsychology in 1959 under the Parapsychology Foundation grant. By the time he submitted his contest entry in August, he knew of 44 cases that seemed to require a parapsychological explanation. The cases came from 13 countries in Europe, North America, and Asia. In 28, there were no known connections between the past and present families, yet the subjects made six or more verified statements about the previous life. The verifications were possible because the previous lives recalled lay close to the present lives in space and time. Several cases were described in detail and appeared to be satisfactorily reported, in books or journals. Stevenson provided summaries of example cases, presented a statistical overview of his data in tabular form, and explained why spontaneous experiences furnished better evidence for reincarnation than events relived under hypnosis or communicated through mediums. He submitted his essay in July 1959, and on September 15, Ducasse notified him that he had won the prize. His paper, "The Evidence for Survival from Claimed Memories of Former Incarnations," appeared in *JASPR* in two parts in April and July 1960 (Stevenson, 1960a, 1960b).

## FIELD INVESTIGATIONS, 1961–1963

Aside from the Weiss-Roos case (Stevenson, 1960a, pp. 57–58),[4] Stevenson's contribution was based exclusively on published reports. Under the heading, "Proposal for Further Investigations" (Sevenson, 1960b, pp. 110–112), he discussed the promise of regressions for probing reincarnation and the possibility of past-life memories arising under the influence of psychedelics, but said nothing about field investigations. He had heard from Banerjee about Jasbir Singh, but this was an unusual case, involving a change of personality upon recovery from an apparently fatal illness. Stevenson wanted answers to a great many questions before he felt confident writing about it, as he ultimately did in *Twenty Cases*.

In July 1959, Banerjee began to urge Stevenson to come to India to pursue his own investigations. Stevenson replied that although he would like to do so, he had neither the time nor the means. As Banerjee continued to inform him about new cases, he started to think about obtaining funds, however. In a September letter to Ducasse acknowledging the essay prize, he told him he had decided to apply for a Parapsychology Foundation grant for the purpose, and asked if he would support this. Ducasse said that he would. If the Parapsychology Foundation was not interested, perhaps the Asia Foundation would be, but when applying to the latter, Stevenson should make the proposal about more than reincarnation cases, Ducasse advised.

At the start of October, Stevenson raised the possibility of a research trip to India with Eileen Garrett, emphasizing the growing number of cases that were coming to his attention. Banerjee had four cases awaiting investigation and he had heard about others from other correspondents. Garrett wrote back with a warning about Banerjee. Banerjee was known to the Parapsychology Foundation and J. B. Rhine as someone who picked up material from magazines and published it as his own. "Forgive my note of caution, but I think it would be useless of you to embark on this, and then to find yourself being used and not altogether scientifically," she warned, turning down his $2,500 request as premature. She did not see that the trip would accomplish anything that could not be achieved through correspondence.

Following a suggestion from Robert Laidlaw, Stevenson next approached the ASPR's Research Commit-

tee. Gardner Murphy, who had visited India in 1950, was encouraging, but felt that entire Board of Trustees, not just the Research Committee, should make the decision. Ducasse now was cooler toward the prospect. He was "not oversanguine about how fruitful of anything solid such a trip as you contemplate would be likely to be," but deferred to Murphy. Stevenson wrote to Garrett again, first in May, when he told her that since his *JASPR* paper had gone to press, he had learned of several additional cases that seemed to deserve investigation, then in September, after the second part of his paper had appeared, letting her know he had hopes that the ASPR would support him. Garrett responded with a handwritten note saying that she had reconsidered and would underwrite his trip to India.

Stevenson began to plan a research tour for the summer of 1961. Banerjee had given him preliminary details about three cases besides Jasper Singh: Sukla Gupta, Prakash Varshnay, and Swarnlata Mishra. These cases had similar features but also presented variations. Swarnlata Mishra was said to recall two previous lives, the penultimate one substantially better than the more recent. Stevenson requested information on where the children and their purported previous families lived, and how far apart they were, to construct his itinerary and budget. Banerjee arranged for Stevenson's affiliation with the University of Allahabad and, with the support of J. B. Rhine, came to the United States in April and May 1960. Bannerjee and Stevenson met in late April to refine the tour arrangements. At Banerjee's suggestion, Stevenson wrote an appeal for information about additional cases, to be submitted to Indian newspapers.

Stevenson decided to visit Ceylon (renamed Sri Lanka in 1972) following India. Since March, he had been in touch with a British expatriate living on the island, Francis Story. Story was a lay monk and Religious Director of Bauddha Dharmadutadhara Sangamaya in Sri Jayewardenepura Kotte. He was associated with the Buddhist Publication Society in Kandy, which had put out a book he had written describing reincarnation cases he had examined during eight years in Burma (Story, 1959). Stevenson considered doing his own research in Burma (now Myanmar) but gave up the prospect when his inquiries to that country went unanswered. Meanwhile, Story had learned about a promising Ceylonese case (the case of Gnanatilleka Baddewithana) that Stevenson wanted to examine. He planned to spend a week in Ceylon at the end of August.

Stevenson finalized a $4,533 grant proposal and submitted it to the Parapsychology Foundation in November 1960. He was requesting too much, he was advised, not by Garrett herself, but by a member of her staff. The Foundation was "deeply interested" in his investigations, he was told, but could provide a maximum of $2,500, $1,500 for the trip and $1,000 to support writing up the results. Perhaps the ASPR would cover the additional expenses. Stevenson queried Ducasse about the possibility, but Ducasse said he did not think Stevenson would find enough cases to justify the outlay, and declined to back an approach to the ASPR.

By this point, Stevenson was convinced that Ducasse was wrong: He had received a good response to his newspaper appeals and had preliminary information on several Indian cases. He had requested funds to stop in Europe for consultations on the way to India, but omitted these layovers and flew directly to Delhi, arriving there on July 17. After going through the roster of cases and deciding which to inspect more closely, he spent two weeks operating out of Delhi with Banerjee as assistant and interpreter. For another three weeks, he traveled around India, meeting fellow researchers, some of whom had been his correspondents on cases. One, P. Pal of Itachuna College in West Bengal, had made his own investigation of the case of Sukla Gupta, which he was shortly to publish in Banerjee's *Indian Journal of Parapsychology* (Pal, 1961–62) with an introduction by Stevenson (1961–62).

Most of the cases Stevenson included in his tour were located in the northern or central Indian states, with a single comparatively weak one in the southern part of the country. Altogether, he spent time on 17 cases in India and four in Ceylon. The past and present families were unrelated and unknown to one another in all except one case. In three cases, it was not possible to identify the previous incarnation, but in the others, Stevenson interviewed witnesses to both the present and previous lives. One of the Ceylonese cases represented the past life of an Indian boy who recalled having resided in Ceylon. Stevenson suspected deception in one case but saw no evidence of it in the others. In two cases, he was able to conduct interviews in English or French, without interpreters. Some case subjects had grown out of childhood and no longer remembered what they had said when younger; in these cases, Stevenson could obtain accounts of the memories from their elders only. In the case of Swarnlata Mishra, records had been made in writing before the previous incarnation of the penultimate life was identified.

Stevenson set out on his Asian tour with the assumption that the recitation of memories was the most salient aspect of the cases and was surprised to discover that not only did the children describe events about which they should have known nothing, but their behaviors matched the behaviors of the deceased people with whom they identified (Stevenson, 2006, p. 16). Two children (one Indian, the other Ceylonese) who claimed to remember living in England exhibited English mannerisms. Two girls

who recalled having been male were noticeably boyish in their interests and demeanors. Swarnlata Mishra performed songs and dances, which she said belonged to her intermediate life in West Bengal. Stevenson included these details in a report filed with the Parapsychology Foundation.

Stevenson made a presentation about his trip to the annual meeting of the Parapsychological Association (PA) in September. Murphy afterward told him that he had given a "most thoughtful and stimulating report." Banerjee heard "daily" that the address had been "very favourably received" and observed, "it appears that you have changed the course of the parapsychology movement." Stevenson reprised his PA talk at the ASPR in November 1961. I have not found a copy of the written text, and one may not exist. Probably, the talk echoed Stevenson's report to the Parapsychology Foundation and emphasized the subjects' behaviors as well as their memory claims. It almost certainly concluded with a call for follow-up investigations to learn more about cases of this type.

Follow-up investigations were made possible by Chester Carlson, whom Stevenson had met at the 1960 PA convention in New York City, ten months before he went to India and Ceylon. Carlson had become a multi-millionaire thanks to his invention of the dry-copying Xerox process in the 1930s. Before his second marriage, he had accepted that the mind was a product of the brain and that mental activities were strictly physical operations. He began to question this assumption when he married a second time. His new wife, Dorris, had a history of psychic experiences, which led him to look into the research being done by Rhine at Duke, then to make financial contributions to Rhine's Parapsychology Laboratory and to attend the parapsychology discipline's professional meetings (Stevenson, 1989b, 2006).

At the conclusion of the September 1960 PA convention, Stevenson and Carlson went to lunch. Carlson told Stevenson that his wife believed she had memories of a previous life in early 18th-century France. Upon his return to Charlottesville, Stevenson sent Carlson reprints of his journal papers in parapsychology and they entered into a regular correspondence. In the Spring of 1961, Carlson offered to help Stevenson financially. Since Stevenson already had Garrett's commitment for his trip to India, he asked only for a portable tape recorder. Carlson promptly sent a check. He believed Stevenson's work was important and wanted to provide any assistance desired.

Stevenson waited until after his PA presentation in September 1961 to broach the financial issue with Carlson again. He hoped to raise the subject at a luncheon after the meeting, but others were present, so he wrote it in a letter afterward. First, he explained that the tape recorder had proven less useful than anticipated. It had been impossible to have private conversations in India. Recognizing a group of voices on tape was difficult, besides which there was uncertainty about the spelling of names. He was accustomed to psychiatric interviewing and found that detailed written notes captured more of the essence of what was said and done; written notes were also easier to consult as required later. He sold the tape recorder (a small battery-operated model) in India, devoting the proceeds to unforeseen expenses of the trip.

Stevenson's fieldwork had shown the need for further investigations, which in the immediate term could be pursued by Banerjee, Pal, and Story, if he could cover their travel requirements. He had in mind $1,000 to distribute among the three of them, "not necessarily equally." His trip had given him the opportunity to observe these men in action. Pal and Story could be assigned tasks without supervision, but Banerjee needed guidance. Stevenson was confident he could provide this from Charlottesville, waiting for three or four years before returning to India himself. When he was satisfied they had done what they could, he and Banerjee would write up a report of 12 to 15 Indian cases. Stevenson and Story likewise would report on three or four Ceylonese cases. He would also like to send Story to Burma and Thailand, where he had contacts, in search of cases there.

Carlson was delighted to be asked for further assistance. His new check reached Charlottesville on September 21, and Stevenson immediately began communicating with Banerjee, Pal, and Story about things he wanted them to do with the money he could now provide. Story followed up on cases in Ceylon, then went to Burma and Thailand. Pal researched Swarnlata Mishra's purported Bengali life and tried to identify her songs and dances. Banerjee proved more difficult to manage. Stevenson wanted him to finish collecting data on cases for which he had already opened files, but Banerjee was more interested in identifying new cases, both in India and abroad. He did some work on the cases to which Stevenson gave priority, but was eager to go to Nepal, and talked Carlson into directly financing a trip to Turkey. When Stevenson heard about this, he discouraged Banerjee from going to Lebanon to look for cases there.

Stevenson was concerned that Banerjee was spreading himself too thin. He thought it best to study a few cases thoroughly and get them published; after that, research funds would flow more freely, he believed. In October 1962, he reminded Banerjee that he wanted as much detail as possible: "It seems to me that we have a sufficient number of cases, indeed more than enough, so it is quite clear that there is something important to be studied in all these cases and something strongly sugges-

tive of rebirth. What we must now do, I think, is gradually tighten our investigations in every way possible and possibly investigate fewer cases more intensively rather than a great number superficially." He would soon send the draft of a report of the four Indian cases (Jasbir Singh, Swarnlata Mishra, Sukla Gupta, and Prakash Varshnay) he considered sufficiently well investigated to be published.

At the same time as he was working with Banerjee, Pal, and Story to ready reports on his research in India and Ceylon, Stevenson began to investigate cases among the Tlingit Indians of southeastern Alaska. Early in 1961, Louisa Rhine had sent him a letter she had received about a Tlingit boy who had recognized, and claimed as his own, a gold watch that had belonged to the person whose life he seemed to recall. After an unsatisfactory period of correspondence, Stevenson realized he needed to investigate the case in person. With $500 provided by Carlson, he went to Alaska for a week. With the assistance of witnesses with whom he had corresponded, he was able to look into this and three other cases on this occasion.

In the summer of 1962, Stevenson made a follow-up visit to Alaska. He expected on this second trip to complete his study of the four cases on which he had begun work, but besides doing so, he learned about four additional cases. None of the Alaskan cases were as rich in statements and behaviors as were the Asian cases he had studied. Most involved returns among relatives, which reduced their evidential value. Nonetheless, they followed the patterns of the Asian cases and directed attention to features that were relatively uncommon in them.

The Tlingit cases gave Stevenson abundant examples of what he decided to call "announcing dreams" (pregnancy dreams in which deceased persons appeared) and birthmarks resembling scars on the bodies of deceased persons, both of which the Tlingit relied upon to ascertain the previous identity of a newborn child. The birthmarks seemed especially significant. Stevenson was acquainted with birthmarks purportedly related to reincarnation, principally in accounts from Burma (Fielding-Hall, 1898; Story, 1959). Altogether, he knew of 25 cases with birthmarks commemorating injuries or other scars on the bodies of deceased persons. He decided to add an Indian example, the case of Ravi Shankar Gupta, to the paper he and Banerjee were preparing. Ravi Shankar claimed to recall having been decapitated and had a linear birthmark across the front of his neck consistent with such a wound.

Stevenson had received intriguing reports of past-life memories in South America as well, so late in the summer of 1962, before his return trip to Alaska, he went to Brazil and Argentina. He returned with enough material to write about two Brazilian cases, both in the same family. These cases had features similar to the cases he was studying elsewhere, although again, there were differences. An unusual number of Brazilian subjects claimed memories of someone of the opposite sex and included gender-nonconforming behaviors. Paulo Lorenz was especially interesting because he recalled having been his deceased sister, who had killed herself, saying she wanted to be a boy. When he was not yet four years old, Paulo demonstrated how to thread and use his deceased sister's sewing machine.

Stevenson initially planned to write up his cases in a series of papers for *JASPR* and the *International Journal of Parapsychology*, the latter a publication of the Parapsychology Foundation, but was persuaded it would be better to combine them in a single book-length *Proceedings* for the ASPR. He had been due to go to Zurich on sabbatical in August 1962, but circumstances required him to put this off a year. He used the delay to further his reincarnation monograph. In December 1962, he sent Ducasse a draft of the Jensen Jacoby case coauthored with T.E.'s husband, six Indian cases coauthored with Banerjee, four Ceylonese cases coauthored with Story, seven of his Tlingit cases, and his two Brazilian cases. He still had to compose the Introduction and General Discussion.

In February, Ducasse wrote to say that he had read over everything Stevenson had sent and was much impressed. The investigations had been painstaking. The evidence was presented in an effective manner and with appropriate caution. He thought the monograph deserved to be published by the ASPR and would recommend it to the Board of Trustees at their March meeting. Ducasse used the title *Twenty Cases Suggestive of Reincarnation* in reporting actions of the ASPR Board to Stevenson. Presumably, this was the title Stevenson gave to the manuscript he sent to Ducasse in December 1962.

The Board appointed a special committee of five to "read, evaluate and recommend disposition of Dr. Stevenson's paper." Besides Ducasse, the committee members were Gardner Murphy, Robert Laidlaw, George Hyslop, and Alan MacRobert. George Hyslop, the son of James Hyslop, served as president of the ASPR from April 1941 until January 1962, when Murphy succeeded him. Alan MacRobert was a minor player at the ASPR and in parapsychology about whom nothing is recorded except his brief tenure on the ASPR Board (1961–64). It seems likely that he was related to Russell G. MacRobert either as a brother or son.

Stevenson did not submit the monograph's final chapter, the General Discussion, until the middle of April 1963, and continued to update sections he had already submitted. On April 25, he left for another ten days in Alaska, necessitating revisions to the Tlingit chapter upon his return. He asked Story and Banerjee to read over

the chapters they were coauthoring. He wanted Banerjee to collect more data for the recently added case of Ravi Shankar Gupta. "You must think me a fiend for details, but I do think this pays off," he said. "Without this attention to detail, the attitude of the tough-minded experimentalist to spontaneous cases is justified."

By late April 1963, committee members were reading different parts of the manuscript. Ducasse, as chairman, reviewed everything, but the others saw portions only. Murphy was sent the chapters on India and Brazil, along with the General Discussion; Laidlaw, the chapter on Alaska and the General Discussion; Hyslop, the Jensen Jacoby case, the chapter on Ceylon, and the Introduction; MacRobert, the Jensen Jacoby case, the chapter on Brazil, and the Introduction. Comments were to be sent to Ducasse for forwarding to Stevenson.

Murphy was impressed with the parts he saw, but asked what if some material was acceptable, some not? Psychical research was at a critical juncture, and presentation mattered. It was imperative that they separate themselves from popular writing on similar topics. Stevenson granted that the material was of uneven quality, but the deficiencies of some cases were balanced by the strengths of others. The 20 cases were representative of the genre and ought to be read together. The Indian philosopher C.T.K. Chari had launched a sustained assault on past-life memory claims in a series of recent papers (1962a, 1962b, 1962c, 1962d), but few people could see his distortions; the publication of a large bloc of cases was required as a response and corrective.

Acceptance of the monograph was delayed not only by Murphy's concerns, but by the opposition of Hyslop and MacRobert. James Hyslop had doubted the possibility of reincarnation and George Hyslop thought the ASPR should honor his father's memory by preserving his feelings on the matter. Ducasse expressed the hope that a favorable three-to-two decision would be reached at a June 12 committee meeting, then ratified at a Board meeting later that day, but this did not happen. Stevenson grew increasingly frustrated with the process. Considering that Chari was able to publish widely while never stepping away from his armchair, he was astonished that the ASPR would not accept his report based on field investigations. He wanted to get out his monograph to account for himself at his university and because he believed it would attract funds for future research.

The committee's comments on the manuscript were delivered to Stevenson at the beginning of November 1963. Some of the comments were good and useful, some were captious, but he was going to do his best to accommodate them all, he told Francis Story. He agreed with Murphy about publication standards and was mindful of his own reputation. This was not a matter of satisfying critics outside of parapsychology only, however. "The last year has certainly shaken my rather bland belief that we had fair freedom of investigation and expression in the West. . . . Then too, as Professor Ducasse recently pointed out to me, even in such an unorthodox subject as psychical research, there exists an orthodoxy and an unorthodoxy. And I obviously belong to the unorthodox wing of this unorthodox group!"

## SETBACKS, 1964–1965

Stevenson returned a revised draft of his monograph late in January 1964, incorporating new data on some cases, in addition to addressing the committee's concerns. With the *Twenty Cases* manuscript out of the way, he wanted to get on with a book about psychiatric interviewing he was supposed to be writing on his sabbatical. He was hoping to complete the first draft of this book before returning to Charlottesville in September. Ducasse considered the revised draft of *Twenty Cases* a substantial advance and expected a favorable decision at the March meeting of the evaluation committee and Board. Murphy, however, wanted all committee members to read the full final draft. He thought the Jensen regression case weakened the impact of the spontaneous cases and would prefer to see it withdrawn. He could not follow Stevenson on the need for quick decision. The ASPR ought to put out the best product possible, he contended.

Stevenson felt that the committee was acting unfairly. He had not encountered such obstacles with any of the papers he had published in mainstream journals. The ASPR had run articles about reincarnation before, including one of Chari's recent pieces (1962a). He, Stevenson, had gone to the trouble of investigating the cases in the field, rather than simply accepting accounts that arrived in the mail (as Murphy and Louisa Rhine did). He could not understand the protracted delay, especially after he had made the requested changes.

MacRobert resigned from the evaluation committee when his Board term expired in January 1964. He was replaced by Laura Dale on the committee and, along with another departing member, by Chester Carlson and Gertrude Schmeidler on the Board. On March 17, the reformed committee accepted Stevenson's monograph for publication in the ASPR *Proceedings*, with the proviso that Stevenson be the sole author, the other names being introduced by "with the assistance of" at the head of the appropriate chapters. The motion to send the recommendation to the Board was made by Hyslop, who withdrew his opposition at the last moment. Ducasse presented the recommendation to the Board, which accepted the vol-

ume unanimously. On March 18, Dale telegrammed Stevenson in Zurich with the news.

Dale's telegram reached Zurich while Stevenson was on an 18-day trip to Turkey, Lebanon, Syria, and Israel, researching reincarnation among two heterodox Shia Islamic sects, the Alevi and the Druze. Banerjee had put him in touch with Reşat Bayer of Istanbul, and Bayer had notified him about several Turkish Alevi cases. Additionally, while in Brazil in 1962, Stevenson was given a lead in a Druze case in Lebanon. He also had preliminary information about a case in Israel.

Stevenson wrote to Story about his Middle East tour on March 27, immediately upon his return to Zurich. In southeastern Turkey, he had found a larger concentration of cases than in Asia and Alaska. He had studied two "with rich detail" and a half-dozen others "having less detail." The patterns were those now familiar from elsewhere in the world. There were many cases with announcing dreams and birthmarks, although these were not as common as in Alaska. With Bayer, he had gone back over the Alevi case Banerjee had studied and found that his investigation had been disturbingly superficial. The case appeared stronger than Banerjee had represented it as being, but it was clear that Banerjee had been careless in recording facts and often had not asked pertinent questions, Stevenson confided in Story.

In Lebanon, Stevenson had the good fortune of finding, for the first time, a case in which the previous life had not yet been identified. He was able to record testimony from the subject, Imad Elawar, and his family, then follow up on this information and trace the deceased person to whom Imad's memories referred. The experience taught him much about how past-life memories presented and the difficulties that could arise in the course of their verification. Imad's parents misconstrued some of his statements (inferring that one name he mentioned repeatedly was that of his previous incarnation and another was that of that person's wife) and relayed their assumptions to Stevenson as things Imad had said. This sent Stevenson down blind alleys, and his initial inquiries in the village Imad had indicated were unproductive, but after Stevenson returned to the family and obtained a cleaner list of Imad's statements, he was able to match them to a deceased Ibrahim Bouhamzy. Ibrahim had spent the last month of his life bedridden with tuberculosis, which might help explain why Imad had repeatedly expressed surprise at being able to walk when he was young.

The Imad Elawar case investigation made a strong impression on Stevenson. He had begun to think there was not much more to be done with the reincarnation cases, because it seemed that investigators would invariably arrive on the scene after the main events were over and could never be certain how they had unfolded. The Imad Elawar case showed this assumption to be wrong. If investigators were able to follow cases from the outset, they could be more confident they had missed nothing of relevance, and it would be possible to make closer observations of psychological and behavioral correspondences between the past and present lives. With his renewed enthusiasm for reincarnation studies, he was more than ever determined to get out of his administrative commitments at the University of Virginia and devote his full time to field research.

Stevenson was relieved by the ASPR Board's decision to publish his monograph, but when he learned about the requirement that he be the sole author in letters from Ducasse and Laidlaw, which arrived a few days later, he had a new set of concerns. His association with Banerjee had been a source of difficulty of late and now apparently was having an impact, as he had feared it would. The problem was not only Banerjee's carelessness, as troubling as that was. Banerjee had allowed people to believe he held a Ph.D., when he did not. Stevenson had referred to him as "Dr. Banerjee" for 18 months before discovering the truth (in April 1963). Banerjee had been in a doctoral program, but had not completed the requirements for the degree. Stevenson had encouraged him to go back and finish up, but Banerjee had not done this. Banerjee's duplicity over his degree was one of the reasons for Murphy's reservations about Banerjee's coauthorship, and Murphy had evidently passed on his concerns to other Board members.

Stevenson had no objection to eliminating Banerjee as coauthor and only crediting his assistance in the four Indian cases in which he had been involved, but removing Story and T.E's husband as coauthors of their contributions created problems. Story had investigated one of the Ceylonese cases on his own, and it would be inappropriate to include this case with himself as the sole author, Stevenson felt. The situation with the Jensen case was different and more complicated. Stevenson had researched it independently of T E.'s husband, but as the hypnotist, T.E.'s husband was closely connected to it. For a while, he preferred not to share authorship with Stevenson, but when he learned that he needed to have a professional paper to his credit to gain access to a library he wished to consult, he changed his mind. Stevenson had agreed to have him as coauthor, but for that to happen, the Jensen case would have to be included in the monograph.

While these issues were under discussion, T.E.'s husband introduced a new concern. Although he had not previously objected to the inclusion of the Jensen case in the monograph, he now expressed misgivings about having it associated with a series of children's past-life memo-

ries. That would imply that he accepted a reincarnation interpretation of the Swedish xenoglossy, whereas he believed Jensen was a discarnate spirit who had possessed his wife. Stevenson agreed to remove the report, but it was too long for a journal publication. It would require a *Proceedings* of its own, which meant another financial subsidy.[5] Story's case, on the other hand, could be published independently in *JASPR*. It appeared there in April 1967 (Story & Stevenson, 1967), Stevenson in the interim having had the opportunity to join in its investigation.[6]

The removal of Story's case, along with Jensen Jacoby, left the monograph with eighteen cases. Stevenson suggested adding the Imad Elawar case he had discovered in Lebanon, together with a seventh Indian case (Parmod Sharma) on which he had done sufficient work, to bring the total back to twenty. This proposal was accepted by Ducasse in late April, subject to Board approval, just before another Banerjee-related crisis erupted.

The new issue was another Stevenson had seen coming and tried to avert. Banerjee, it had emerged, had been in J. B. Rhine's employ at the same time Stevenson was sending him money and tasking him with reincarnation-case investigations. From 1958 to 1963, Banerjee had conducted card-guessing tests of ESP between mothers and their school-aged children, experiments which had been showing good results. Neither Rhine nor Stevenson knew the extent to which Banerjee was engaged with the other. Rhine was prepared to tolerate Banerjee's affiliation with Stevenson to a point, but when he realized that Banerjee was more interested in reincarnation case studies than in telepathy experiments, he cut him off financially and, in April 1963, severed all ties to him.

Around the same time, rumors of fraud began to circulate in the parapsychology community. Stevenson never accepted these. He believed that Banerjee was as sloppy in his experimental record-keeping as he was in his field research, and encouraged him to address the rumors privately before they broke into the open and came to the attention of the ASPR trustees. But that did not happen. The April 1964 issue of the *Journal of Parapsychology* carried a review of a five-year report from Banerjee's institute (Rao, 1964), which insinuated that Banerjee had faked his results. The ASPR Board immediately withdrew approval for Stevenson's monograph as it stood.

Ducasse wanted the four cases "contaminated" by Banerjee (Jasbir Singh, Prakash Varshnay, Ravi Shankar Gupta, and Parmod Sharma) removed from the monograph.[7] Because these were among his strongest cases, Stevenson did not want to take them out. He was considering withdrawing the manuscript from the ASPR when it occurred to him that he might return to India and reinvestigate the cases with new assistants. With Carlson's support and the approval of the ASPR Board, he went back to India for four weeks in August and early September, stopping in Lebanon for three days of further research on Imad Elawar. Sami Makarem of the American University, Beirut, assisted him in Lebanon on this occasion. In India, he arranged to have two interpreters on each case. P. Pal and Jamuna Prasad, Deputy Director of Education for the state of Uttar Pradesh, filled this role, except in the case of Ravi Shankar Gupta, for which Prasad had acted as Banerjee's interpreter. Stevenson also had Story come to India to back him up. The two interpreters, Story and Stevenson, kept independent notes, which they compared the day they were made, resolving discrepancies before they left the area. In addition, Stevenson had all documents translated by Banerjee retranslated.

Stevenson expected his reinvestigations to vindicate Banerjee. Instead, although he discerned no evidence of deceit on Banerjee's part, there were manifold indications of carelessness. After his return to Zurich, he wrote Banerjee a blistering letter terminating his affiliation with him, at least until he completed his Ph.D. and gained some appreciation for investigative procedure. His reinvestigation was a turning point for Stevenson in other ways. It made him realize the benefits of reinterviewing witnesses after a period away. The follow-up interviews provided checks on the reliability of memories, furnished the opportunity to fill in gaps in testimony, and permitted him to learn how the children had fared since he had last seen them. From then on, Stevenson employed two interpreters to compare translations and make it more difficult to overlook details of witness testimony.

Back in Zurich, Stevenson set about revising his manuscript once again. He had to update not only the chapter on India but also the Introduction and General Discussion. He sent the updated chapter to Story, Pal, and Prasad for approval, then turned to his book on psychiatric interviewing, determined to spend on it what remained of his sabbatical.[8] He put off the final revisions of *Twenty Cases* until he returned to Charlottesville, forwarding to Ducasse his final draft in May 1965. It was accepted for copyediting without further alteration, and discussions on financing resumed. The 362-page monograph was published in September 1966 with the assistance of Carlson and Garrett, sent out to ASPR members free of charge, and offered to the public at the price of $6.00. The initial issuance of 7,000 copies sold out in twelve months, and the book was reprinted.

## THE RECEPTION OF TWENTY CASES, 1966–1967

*Twenty Cases Suggestive of Reincarnation* is written in the style of psychical research. The emphasis is on

demonstrating the veridicality of statements and behaviors, showing that they cannot be explained by reference to the children's present lives, and arguing that reincarnation is the most satisfactory way to account for them.

The case reports follow a standard format. They open with a summary of a case and its investigation, discuss the geographical relationship of the sites of the past and present lives, consider possible means of communication between the past and present families, list the people interviewed, and treat at greater length the child's statements, recognitions, and behaviors relating to the previous personality, as Stevenson referred to the earlier incarnation. He supplied tables of these items, noting the witnesses for each. The reports conclude with comments on the evidence of the children's "paranormal knowledge."

In his General Discussion, Stevenson considered a range of hypotheses to account for this paranormal knowledge and other features of the cases. He believed his investigations would have uncovered deception, were it a factor. He considered cryptomnesia (source amnesia), the possibility of which Chari (e.g., 1962a) was fond of emphasizing, but could see no evidence for that either. He spent some pages on what he termed "extrasensory perception plus personation." ESP alone could not be responsible for identifications with the previous personality: Information acquired through ESP would have to have been mobilized subconsciously to generate the behavioral and emotional elements of the cases, but nothing like this was known from cases of spontaneous ESP. Skilled behaviors, such as Swarnlata's Bengali songs and dances and the ability of Paulo Lorenz to use his sister's sewing machine, required practice to perfect, and posed an even greater challenge to the ESP hypothesis. The exceptional knowledge and behavior might be attributable to obsession or sporadic possession by a discarnate personality, but the birthmarks could not be. On the whole, Stevenson thought, reincarnation provided the best explanation for the data he had assembled (1966, pp. 291–354).

*Twenty Cases* was reviewed in both mainstream and parapsychology journals. Most of the reviews were written by Stevenson's friends, who walked a tightrope between advocacy and academic respectability. In a sympathetic review for the *American Journal of Psychiatry,* Robert Laidlaw (1967, p. 128) stated, "the question of the survival of part of the individual beyond physical death should be of vital interest to every psychiatrist." Writing in the *Bulletin of the Menninger Clinic*, Gardner Murphy (1967, p. 167) recommended the book "as a broadening study from a socio-cultural and philosophical point of view." In the most reflective review, for the *British Journal of Medical Psychology,* James F. McHarg (1969) speculated that unresolved conflicts at the time of death might have stimulated a transfer of information via ESP and that the question of personal reincarnation depended on the definition of "person."

Armando Favazza (1967) praised the case studies in *Medical Opinion and Review* but cautioned that they were not scientific because they were not laboratory-based. Jan Ehrenwald (1967), in the *Journal of Nervous and Mental Disease,* suggested the possibility of "doctrinal compliance," whereby psychiatrists of different persuasions elicit evidence to match their expectations. Donald West (1967), a British psychiatrist associated with the Society for Psychical Research, said in the *British Journal of Psychiatry* that "Dr. Stevenson concentrates on the issue of evidence for the paranormal; but at the same time he has provided an admirable collection of case studies illustrating the operation of cultural factors in shaping the child's perception of reality." John Beloff (1967), a psychologist at the University of Edinburgh, reviewed the book for the *Journal of the Society for Psychical Research*. He agreed that all alternative interpretations of the case data failed, yet reincarnation faced formidable obstacles to acceptance in Western culture. *Twenty Cases* was, he believed, a work of major importance, but it would be long before it was recognized as such.

C.T.K. Chari reviewed the book twice, first for *Śaiva Siddhānta: A South Indian Quarterly Journal of Philosophy and Religion* (1966) and then for the *International Journal of Parapsychology* (1967). His argument in *Śaiva Siddhānta* was that the "sensational reports" of reincarnation were best understood as instances of spirit possession. In the *International Journal of Parapsychology*, he downplayed this idea and instead introduced a litany of concerns: Stevenson's dependence on interpreters, the possible effect of parental influence on children, and the potential for genetic transmission of physical anomalies. The children's "patchy memories" hinted at pathological states of consciousness, Chari believed; apparent past-life memories might actually be "veridical hallucinations" that incorporated information retrieved via ESP.

The relative paucity of cases in southern as opposed to northern India indicated to Chari a conformance to cultural demands. He devoted special attention to Stevenson's single South Indian case, that of Mallika Aroumougam. Mallika's case was one of the weakest in terms of memory claims, but had interesting behavioral features. When her father moved to Pondicherry for a job, he rented the ground floor of a house. Mallika was not quite four years old when she first visited the landlord's quarters upstairs. There, she noticed chair cushions and announced that she had made them; in fact, they had been crafted by the landlady's deceased sister, Devi. Thereafter, Mallika began to go upstairs regularly, where she responded to

other articles and made other observations suggestive of having been Devi. Her behavior was witnessed only by the landlord's family, yet Mallika's father and grandfather assured Chari that the case was without foundation. From this, Chari concluded that the witnesses interviewed by Stevenson were unreliable and discounted Mallika's recognition of her landlord as Devi's brother-in-law, which was documented in a police report.

Stevenson (1968) responded to Chari's review in a letter to the journal editor. Genetic transmission of physical traits would not account for the appearance of birthmarks in unrelated families or from wounds received at death. True, more cases were reported in some areas than in others, but cultural conditioning was not the only way to account for this uneven distribution; it could be that, for some reason, more cases developed in certain places. Regarding parental influences, although Indian parents might be receptive to past-life memory in general, they tended to be skeptical of claims that appeared in their own families. As to Mallika Aroumougam, since her father and grandfather had not witnessed any of her relevant behavior, their opinions were immaterial in judging the case. Moreover, Chari provided no justification for his rejection of Mallika's recognition of Devi's brother-in-law, as recorded in the police report.

In her review in the *Journal of Parapsychology*, Louisa Rhine (1966) was concerned principally with the parapsychological aspects of Stevenson's work. She noted that reincarnation presumed postmortem survival; however, inasmuch as the survival question was still undecided, research on reincarnation was "strictly speaking, premature" (Rhine, 1966, p. 264). Reincarnation research could be justified only if it promised to provide stronger evidence of survival than other phenomena, but did it? She alleged that Stevenson was only able to solve his case by assuming that Imad Elawar's parents had made some wrong inferences. Rhine accepted that Stevenson had ruled out fraud and cryptomnesia, but thought that he had not given due attention to the possibility of parental influence, nor was she prepared to set aside the involvement of clairvoyant ESP. She faulted Stevenson for employing an "old" understanding of ESP, allotting responsibility to the agent rather than to the percipient. She speculated that physical traits like birthmarks matching wounds might be acquired characteristics in the Lamarckian sense.

Stevenson (1967) responded that it was not true that the identification of Ibrahim Bouhamzy depended on his correcting Imad's parents' mistaken inferences. These had put him on the wrong track initially, but Imad had said enough specific things about Ibrahim to make the identification secure. The mistaken inferences were all about connecting the dots, not the dots themselves. More generally, parental imposition of identity could not explain how the parents obtained the information to shape their children's behavior, nor could it account for the persistence of the children's memories, and it could not be squared with attempts by some parents to suppress their children's memories. Ravi Shakar Gupta's father beat him mercilessly whenever he talked about the previous life, but this succeeded only in making the boy afraid of his father, and he continued relating his memories to others. Clairvoyance would not account for the targeted selection of deceased individuals, nor for behavioral identifications or physical signs. Physical characteristics could not be inherited in most cases because there was no genetic avenue for transmission from the deceased to the child. Rhine was refusing to fairly confront the evidence. Stevenson (1967, p. 154) concluded by quoting a line attributed to Heraclitus of Pontus: "If you expect not the unexpected, you shall not find the truth."

## CONCLUSION

Critics of Stevenson's reincarnation studies have sometimes charged that he was driven by the Theosophy to which he was introduced by his mother to "prove" the reality of reincarnation, but his story does not support this notion. Stevenson indeed became acquainted with reincarnation in the Theosophical texts he read as a child, but because he could see no way to test Blavatsky's claims, they held no appeal for him. His medical career was concerned with a very different set of issues as he moved from one specialty to another, trying to find one that dealt satisfactorily with the relationship between mind and body, particularly the problem he regarded as central to his life—the question of why people developed the particular illnesses they did. Almost certainly, Stevenson's preoccupation with this question was prompted by his own bronchiectasis, for which no satisfactory explanation was provided.

Although he appears never to have stated this openly—certainly, he never speculated about it in print—one must wonder whether Stevenson came to think the answer might lie in reincarnation. As he studied case after case, he was brought to realize that not only memories and behaviors, but also physical traits—including internal diseases—might be carried forward from life to life (Stevenson, 1997). Marta Lorenz, one of the Brazilians about whom he wrote in *Twenty Cases*, recalled having been a woman who intentionally contracted tuberculosis after her father twice forbade her to marry men with whom she was in love. Marta suffered from recurrent upper respiratory infections, much like Stevenson. Stevenson was born during the 1918 influenza pandemic; it would not be

surprising if he wondered whether he might be the reincarnation of someone who succumbed to the disease.[9]

Skeptical critics (e.g., Augustine, 2015; Edwards, 1996) have been merciless in their attacks on Stevenson's interviewing style, his habit of spending only a few days with case subjects, and his use of interpreters, among other things (Matlock, 2022b). Philosopher Stephen Braude (2003) introduced a series of more sophisticated critiques, arguing that Stevenson's inquiries and interpretations were psychologically superficial, and that he betrayed an inadequate grasp of crucial issues concerning language competency, dissociation, and the relevance of studies of savants and prodigies.[10]

In Stevenson's defense, it should be remembered that he was a seasoned psychiatric interviewer who wrote textbooks on proper technique (Stevenson, 1960c, 1969); he did not approach his fieldwork naively. He was aware of potential pitfalls in his practices and did what he could to mitigate them. From the outset, he supplemented his own field research with that of professional colleagues, who sent him information about cases before and after he arrived on the scene and acted as his interpreters while there. After the Banerjee debacle, Stevenson adopted the routine of using two interpreters for each interview, in order to ensure that everything of significance was recorded faithfully. He learned the value of reinterviewing witnesses after a time away, and in his later studies, did this regularly, sometimes following his subjects for years before publishing reports about them (Stevenson, 1975, 1977a, 1980, 1983, 1997, 2003). Moreover, a comparison of Stevenson's investigation and report of Gnanatilleka Baddewithana to an earlier, independent investigation of the case by a Ceylonese team headed by H. S. S. Nissanka not published in English (until 2001: Nissanka, 2001) found that although Stevenson missed considerable detail, he got nothing wrong, despite spending only two days on the case and working partially through interpreters (Matlock et al., in press).

There can be little doubt that the criticisms, nonetheless, have been successful in directing attention away from Stevenson's work. An entrenched commitment to a reductionist view of consciousness as brain-generated surely played its part in this. However, Stevenson's mode of presentation did not help. His parapsychological orientation and emphasis on establishing reincarnation as the most satisfactory interpretation of his cases did not connect well with workers in other disciplines; he did not change his style even when publishing in mainstream journals, as, thanks to his professional background, he was sometimes able to do (Kelly, 2013). Critics like to deride and dismiss Stevenson's case studies as "anecdotal," ignoring the extensive investigative effort behind them. Case studies are widely employed in medicine, so that aspect of Stevenson's method should not have been off-putting, but it may be that because he published the bulk of his cases in books, rather than in peer-reviewed journals, they were overlooked by much of his intended audience.[11] None of his case collections after *Twenty Cases* sold very well, and those from the University Press of Virginia (Stevenson, 1975, 1977a, 1980, 1983) were retired after only a few years.

Stevenson's research met considerably more resistance than he imagined it would, and the funds he hoped would flow after the publication of *Twenty Cases* were never forthcoming. Stevenson applied to the Ittleson Family Foundation and visited the National Institute of Child Health and Development in the autumn of 1966 but was turned down by the former and received no encouragement from the latter. In the Spring of 1967, he submitted an application to the New World Foundation, which purported to finance research on postmortem survival, but that too was rejected summarily. Stevenson submitted grant applications to the National Science Foundation (NSF), the National Institutes of Health (NIH), and the National Institute of Mental Health (NIMH) after the publication of the second edition of *Twenty Cases* (1974) and the first volume of his *Cases of the Reincarnation Type* series (1975), but none were successful.[12]

More generally, Stevenson's hoped-for recognition of reincarnation as an explanatory force for many unanswered problems in medicine (expressed in Stevenson, 1977b, 1997, 2000) has yet to come about. But it may be too soon to render a final judgment on Stevenson's contribution. The research program he initiated has survived him (Matlock, 2019), and he may still have the last say, proving correct John Beloff's (1967) verdict on *Twenty Cases*, that although a work of major importance, it would be long before it was appreciated as such.

## IMPLICATIONS AND APPLICATIONS

Stevenson's story has lessons for reincarnation research going forward. There is little reason to suppose that simply amassing more evidence and reporting it in the same ways that Stevenson reported it will make more headway in reaching the mainstream medical, academic, and scientific communities than he was able to achieve. Priority should be given to publishing in journals as opposed to books, at least initially. Researchers would do well to begin connecting their research to mainstream concerns, moving beyond a strict proof orientation to incorporate process-related variables, directly confronting issues such as those identified by Braude (2003). Many common skeptical complaints can be dealt with effec-

tively by seeking out cases with written records made before verifications are attempted, as with Gnanatilleka. A prospective research program that followed children from birth would both supply information on the prevalence of cases and document their unfolding, furnishing insights into the nature of past-life memory retrieval and the course of its manifestation (Matlock, 2022a).

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## ENDNOTES

1. M.D.C.M. stands for Medicinae Doctorem et Chirurgiae Magistrum, Doctor of Medicine and Master of Surgery.
2. Marquis Who's Who (1968, p. 1609).
3. Stevenson (1989).
4. Stevenson included a longer report of the Weiss-Roos case in *European Cases of the Reincarnation Type* (2003).
5. The Jensen Jacoby case was eventually published after T. E.'s husband's death until the title *Xenoglossy: A Review and Report of a Case*, as Volume 28 of the ASPR *Proceedings*, although never distributed to members. The simultaneous publication by the University Press of Virginia (Stevenson, 1974b) was limited in its sales, and the book was soon taken out of print.
6. Stevenson (1977) later included his own report of the case in the second volume of his *Cases of the Reincarnation Type* series.
7. Stevenson argued successfully that Swarnlata Mishra had not been affected by Banerjee because he had done a complete reinvestigation of the case in English, without interpreters. Pal, not Banerjee, had researched the Bengali songs and dances from Swarnlata's purported intermediate life.
8. This book was published in 1969 as *The Psychiatric Examination* (Stevenson, 1969).
9. We learn from a September 24, 1960, letter to Ducasse that Stevenson had his own past-life memories, although it is not clear whether they included someone who died of the flu in 1918. Nor do they appear, in themselves, to have been a strong motivating factor for him, at least in September 1960: "I have had a couple of apparent memories of previous lives, myself. I must say, however, that though these have been important to me, I cannot consider that they have brought as much conviction to me as the evidence I have studied from the cases of the kind of which we are familiar. The reverse has been true; that is, the evidence acquired from my study of other cases has made me more receptive to the possibility that these apparent memories I have had are in fact just that and not pseudo-memories or fantasies, as I might have been inclined to believe ten years ago."
10. Braude (2003) also criticized Stevenson for having too shallow an appreciation for the possibilities of information acquisition through psi, what is called super-psi or living-agent psi (Braude, 2016), but he has since backed away from this opinion. Braude (2021, pp. 31–32) now considers social construction in its various forms to be more likely than psi as an explanation for the reported case phenomena. This mirrors the progression in Stevenson's thinking. In *Twenty Cases* (1966, pp. 343–373), he gave much attention to the possibility of "ESP plus personation," but in the third volume of his *Cases of the Reincarnation Type* series (1980, p. 343), he wrote that he had come to think that the two most viable alternative explanations for the cases were "normal means of communication of the information attributed to the subject, and reincarnation." Stevenson's research and writing, therefore, emphasized ruling out normal means of information acquisition.
11. Stevenson originally intended to publish the cases collected in *Twenty Cases* in journals, but was persuaded to bring them together in a *Proceedings* instead. Thereafter, he published some of his cases in journals before including them in books (Kelly, 2013), but as his work proceeded, the number of cases quickly exceeded what journals would accept. Also, Stevenson could describe cases at greater length in books. A comparison of the space devoted to cases with birthmarks of the head and neck in periodicals as opposed to *Reincarnation and Biology* (Stevenson, 1997) found a mean of 2.1 pages in the former versus nine pages in the latter. Reincarnation research is unusual among the sciences in its use of books to present much of its data (Matlock, 2024).
12. In July 1976, Stevenson told Beloff that "the federal government now has a completely clean record of having turned down every application for a research grant [in parapsychology] it received during in the last two years." Recently, he had "a long and

painful conversation" with an NSF staffer assigned to summarize the reasons for rejection of a 1975 proposal. He professed to be "astonished at the adamantine rejections of paranormal explanations as at least deserving of consideration in studying cases of the reincarnation type." The man had told him "frankly that he saw no possibility of the National Science Foundation supporting my research in the foreseeable future. . . . He quoted one reviewer as saying that he had no objection to private funding of my research, but could not allow government money to be spent on it."

13. Jim Tucker (personal communication).

## REFERENCES


Alvarado, C. S., Coly, E., Coly, L., & Zingrone, N. L. (2001). Fifty years of supporting parapsychology: The Parapsychology Foundation (1951–2001). *International Journal of Parapsychology, 12*, 1–26.

Augustine, K. (2015). Introduction. In M. Martin & K. Augustine (Eds.), *The myth of an afterlife: The case against life after death* (pp. 1–47). Rowman & Littlefield.

Beloff, J. (1967). Review of *Twenty cases suggestive of reincarnation*, by I. Stevenson. *Journal of the Society for Psychical Research, 44*, 88–95.

Bernstein, M. (1956). *The search for Bridey Murphy*. Doubleday.

Braude, S. E. (2003). *Immortal remains: The evidence for life after death*. Rowman & Littlefield.

Braude, S. E. (2016). The super-psi hypothesis. *Psi Encyclopedia*. https://psi-encyclopedia.spr.ac.uk/articles/super-psi-hypothesis

Braude, S. E. (2021). Farewell missives. *Journal of Scientific Exploration 35*, 725–728. https://doi.org/10.31275/20212369

Chari, C. T. K. (1962a). "Buried memories" in survivalist research. *International Journal of Parapsychology, 4*, 40–61.

Chari, C. T. K. (1962b). How good is the evidence for reincarnation? *Tomorrow, 10*, 89–98.

Chari, C. T. K. (1962c). Paramnesia and reincarnation. *Proceedings of the Society for Psychical Research, 53*, 264–286.

Chari, C. T. K. (1962d). Paranormal cognition, survival, and reincarnation. *Journal of the American Society for Psychical Research, 56*, 158–183.

Chari, C. T. K. (1966). Reincarnation in parapsychological perspectives. *Śaiva Siddhānta: A South Indian Quarterly Journal of Philosophy and Religion, 1*, 318–329.

Chari, C. T. K. (1967). Reincarnation: New light on an old doctrine. (Review of *Twenty cases suggestive of reincarnation* by I. Stevenson.) *International Journal of Parapsychology, 9*, 217–222.

Ducasse, C. J. (1951). *Nature, mind, and death*. Open Court.

Ducasse, C. J. (1956). Review of *The search for Bridey Murphy* by M. Bernstein. *Journal of the American Society for Psychical Research, 50*, 125–128.

Ducasse, C. J. (1960). How the case of *The search for Bridey Murphy* stands today. *Journal of the American Society for Psychical Research, 54*, 3–21.

Ducasse, C. J. (1961). *A critical examination of the belief in a life after death*. Charles C Thomas.

Edwards, P. (1996). *Reincarnation: A critical examination*. Prometheus Books.

Ehrenwald, J. (1948). *Telepathy and medical psychology*. Norton.

Ehrenwald, J. (1967). Review of *Twenty cases suggestive of reincarnation* by I. Stevenson. *Journal of Nervous and Mental Disease, 144*, 330–331. https://doi.org/10.1097/00005053-196704000-00013

Favazza, A. R. (1967). Intimations of other lives. (Review of *Twenty cases suggestive of reincarnation* by I. Stevenson.) *Medical Opinion and Review*, 3 (May), 69, 73.

Fielding [Hall], H. (1898). *The soul of a people*. Bentley & Son.

Huxley, A. (1954). *The doors of perception*. Chatto & Windus.

Kelly, E. W. (Ed.) (2013). *Science, the self, and survival after death: Selected writings of Ian Stevenson*. Rowman & Littlefield.

Kline, M. V. (Ed.) (1956). *A scientific report on "The Search for Bridey Murphy."* Julian Press.

Laidlaw, R. (1967). Review of *Twenty Cases Suggestive of Reincarnation* by I. Stevenson. *American Journal of Psychiatry, 124*, 128

MacRobert, R. G. (1956). Something better than reincarnation. In DeW. Miller, *Reincarnation: The whole startling story* (pp. 99–107). Bantam Books.

Malinowski, B. (1927). *Sex and repression in a savage society*. Routledge & Kegan Paul.

NOT IN DOC Marquis Who's Who. (1968). *World who's who in science: A biographical dictionary of notable scientists from antiquity to the present*. Marquis Who's Who.

Matlock, J. G. (2011). Ian Stevenson's *Twenty Cases Suggestive of Reincarnation*: An historical review and assessment. *Journal of Scientific Exploration, 25*, 789–820.

Matlock, J. G. (2019). *Signs of reincarnation: Exploring beliefs, cases, and theory*. Rowman & Littlefield.

Matlock, J. G. (2022a). Clarifying muddied waters, part 2: What the Sudduth-Tucker exchange concerning James Leininger suggests for reincarnation research. *Journal of Scientific Exploration, 36*, 760–781. https://doi.org/10.31275/20222765

Matlock, J. G. (2022b). Criticisms of reincarnation

case studies. *Psi Encyclopedia*. https://psi-encyclopedia.spr.ac.uk/articles/criticisms-reincarnation-case-studies

Matlock, J. G. (2024). Birthmarks and birth defects in the head and neck region and claims of past-life memories: Cases in Ian Stevenson's *Reincarnation and Biology*. *EXPLORE: The Journal of Science and Healing, 20*, 306 - 312. https://www.sciencedirect.com/science/article/abs/pii/S1550830723002410

Matlock, J. G., Weerasekera, A., & Nahm, M. (in press). The case of Gnanathilaka Baddevithana: An early independent investigation of one of Ian Stevenson's reincarnation cases. *Journal of Scientific Exploration*.

McHarg, J. F. (1969). Review of *Twenty Cases Suggestive of Reincarnation* by I. Stevenson. *British Journal of Medical Psychology, 42*, 84–86.

Murphy, G. (1967). Review of *Twenty Cases Suggestive of Reincarnation* by I. Stevenson. *Bulletin of the Menninger Clinic, 31*, 167.

Nissanka, H. S. S. (2001). *The girl who was reborn: A case study suggestive of reincarnation*. S. Godage Brothers.

Pal, P. (1961–1962). A case suggestive of reincarnation in West Bengal. *Indian Journal of Parapsychology, 3*, 5–21.

Rao, K. R. (1964). Review of *Five Years Report of the Seth Sohan Lal Memorial Institute of Parapsychology*, by S. C. Mukherjee. *Journal of Parapsychology, 28,* 59–62.

Rhine, J. B. (1953). *New world of the mind*. William Sloane.

Rhine, L. E. (1966). Review of *Twenty cases suggestive of reincarnation* by I. Stevenson. *Journal of Parapsychology*, *30,* 263-272.

Stevenson, I. (1957). Review of *A Scientific Report on "The Search for Bridey Murphy,"* ed. M. V. Kline. *Journal of the American Society for Psychical Research, 51*, 35–38.

Stevenson, I. (1960a). The evidence for survival from claimed memories of former incarnations. Part I: Review of the data. *Journal of the American Society for Psychical Research, 54*, 51–71.

Stevenson, I. (1960b). The evidence for survival from claimed memories of former incarnations. Part II: Analysis of the data and suggestions for further investigations. *Journal of the American Society for Psychical Research, 54*, 95–117.

Stevenson, I. (1960c). *Medical history-taking*. Harper & Row.

Stevenson, I. (1961–1962). Introduction to "A case suggestive of reincarnation in West Bengal" by Professor P. Pal. *Indian Journal of Parapsychology, 3*, 1–4.

Stevenson, I. (1966). *Twenty cases suggestive of reincarnation. Proceedings of the American Society for Psychical Research, 26*, 1–361.

Stevenson, I. (1967). Letter to the Editor. *Journal of Parapsychology, 31,* 149–154.

Stevenson, I. (1968). Ian Stevenson's reply to C. T. K. Chari's criticism. *International Journal of Parapsychology*, *10*, 104–107.

Stevenson, I. (1969). *The psychiatric examination*. Harper & Row.

Stevenson, I. (1974a). *Twenty cases suggestive of reincarnation* (2nd ed., rev.). University Press of Virginia.

Stevenson, I. (1974b). *Xenoglossy: A review and report of a case*. University Press of Virginia.

Stevenson, I. (1975). *Cases of the reincarnation type. Volume I: Ten cases in India*. University Press of Virginia.

Stevenson, I. (1977a). *Cases of the reincarnation type, volume II: Ten cases in Sri Lanka*. University Press of Virginia.

Stevenson, I. (1977b). The explanatory value of the idea of reincarnation. *Journal of Nervous and Mental Disease*, *164,* 305–326. https://doi.org/10.1097/00005053-197705000-00002

Stevenson, I. (1980). *Cases of the reincarnation type. Vol. III: Twelve cases in Lebanon and Turkey*. University Press of Virginia.

Stevenson, I. (1983). *Cases of the reincarnation type. Volume IV: Twelve cases in Thailand and Burma*. University Press of Virginia.

Stevenson, I. (1989). Some of my journeys in medicine. (Flora Levy Lecture in the Humanities, 1989, ed. A. W. Fields.) University of Southwestern Louisiana. https://pdfs.semanticscholar.org/1aab/6f3ee79ec8003b0e9ed370a4ae6c5e55297c.pdf

Stevenson, I. (1997). *Reincarnation and biology: A contribution to the etiology of birthmarks and birth defects* (2 vols). Praeger.

Stevenson, I. (2000). The phenomenon of claimed memories of previous lives: Possible interpretations and importance. *Medical Hypotheses, 54,* 652–659. https://doi.org/10.1054/mehy.1999.0920

Stevenson, I. (2003). *European cases of the reincarnation type*. McFarland.

Stevenson, I. (2006). Half a career with the paranormal. *Journal of Scientific Exploration, 20*, 13–21.

Story, F. (1959). *The case for rebirth*. Buddhist Publication Society (Kandy, Ceylon).

Story, F., & Stevenson, I. (1967). A case of the reincarnation type in Ceylon: The case of Warnasiri Adikari. *Journal of the American Society for Psychical Research*, *61*, 130–145.

West, D. J. (1967). Review of *Twenty cases suggestive of reincarnation* by I. Stevenson. *British Journal of Psychiatry*, *113* (June). https://doi.org/10.1192/bjp.113.499.698-a

White, K. L. (2008). Ian Stevenson: Recollections. *Journal of Scientific Exploration, 22*, 11–17.

## APPENDIX: WHO'S WHO

The following list supplies brief identifications of the many individuals, of diverse backgrounds, referenced in this paper.

**Hemendra Nath Banerjee**. Director of the Seth Sohan Lal Memorial Institute of Parapsychology in Sri Ganganagar, Rajasthan, India, and Editor of the *Indian Journal of Parapsychology*.

**Reşat Bayer**. Stevenson's Turkish colleague and research assistant; of Istanbul.

**John Beloff.** 1920–2006. Lecturer and then Professor of Psychology at University of Edinburgh, 1962–85.

**Morey Bernstein.** 1920–99. Colorado businessman; author of *The Search for Bridey Murphy* (1956).

**Helena Blavatsky.** 1831–91. Russian-American author; originator of the occult system Theosophy.

**Frances Payne Bolton.** 1885–1977. Delegate to the US House of Representatives from New York State, 1940–69; co-founder, with Eileen Garett, of the Parapsychology Foundation, 1951

**Chester Carlson**. 1906–68. Physicist; inventor of the Xerox copying process. Philanthropist and major benefactor.

**Dorris Carlson.** 1910-1981. Wife of Chester Carlson.

**C. T. K. Chari.** 1909–93. Philosopher, Madras Christian College, Madras, India

**Laura Dale.** 1918–83. ASPR office manager and Editor of ASPR publications, intermittently, from 1941 onwards.

**C. J. Ducasse.** 1881–1969. French-born analytical philosopher. Professor of Philosophy at Brown University, 1926–58; Member of the ASPR Board of Trustees, 1951–65; chairman, publications committee, 1959–65.

**Jan Ehrenwald.** 1900–88. New York City psychiatrist; member of the ASPR's Medical Section in the 1950s and early 1960s.

**Armando Favazza.** 1941– . American psychiatrist with medical degree from University of Virginia best known for his studies of cultural psychiatry.

**Eileen Garrett**. 1892–1970. Renowned British mental medium who, with the financial assistance of Frances Bolton, founded the Parapsychology Foundation in 1951.

**Aldous Huxley.** 1894–1963. British philosopher and writer, author of *The Doors of Perception* (1954), which described his psychedelic experiences under mescaline.

**George Hyslop.** New York City psychiatrist, son of James Hyslop, President of the ASPR Board of Trustees, 1940–62; First Vice-President, 1962–65.

**James Hervey Hyslop.** 1854–1920. American philosopher; Director of the ASPR, 1907–20.

**Richard and Isabella Ingalese**. American authors, affiliated with the 19th century New Thought movement, similar in some respects to Helena Blavatsky's Theosophy.

**Milton V. Kline.** 1923–2004. Psychiatrist, editor of *A Scientific Report on the Search for Bridey Murphy*.

**Robert Laidlow**. 1929–2014. New York City psychiatrist, founder and chairman of the department of psychiatry at Roosevelt Hospital, 1949–57; member of the ASPR Board of Trustees,

**Alan F. MacRobert**. Member of ASPR Board of Trustees, 1961–64.

**Russell G. MacRobert.** New York City psychiatrist; member of the ASPR's Medical Section in the 1950s and early 1960s

**James F. McHarg.** 1917–2003. Consultant Psychiatrist and Honorary Senior Lecturer in Psychiatry at the University of Dundee, Scotland.

**Gardner Murphy.** 1895–1979. Personality and social psychologist, Director of Research at Meninger Foundation, 1952–68; First Vice-President of ASPR Board of Trustees, 1940–62; President, 1962–72.

**P. Pal.** Professor of Psychology at Itachuna College, West Bengal, India.

**Jamuna Prasad.** Deputy Director of Education for the state of Uttar Pradesh, India / Indian psychologist. Served at Bureau of Psychology, Allahabad, Uttar Pradesh, India, in various capacities from its inception in 1947; as Director from 1959, except for a few months spent as Deputy Director of Education for Uttar Pradesh.

**Octavia Reynolds.** Maiden name of Stevenson's first wife.

**J. B. Rhine**. 1895–1980. American botanist and parapsychologist at Duke University, founder of the Parapsychology Laboratory at Duke University in 1935.

**Louisa Rhine.** 1891–1983. American botanist and parapsychologist, wife of J. B. Rhine.

**Gertrude Schmeidler**.1912–2009. Research psychologist at City College of the City University of New York; member of ASPR Board of Trustees,

**Emil L. Smith.** 1911–2009. American biochemist.

**John Stevenson.** Scottish-born Canadian journalist; Stevenson's father.

**Ruth Stevenson.** Stevenson's mother.

**Francis Story.** 1910–71. Lay monk and Religious Director of Bauddha Dharmadutadhara Sangamaya in Sri Jayewardenepura Kotte.

**T. E.** Pseudonymous initials of subject of Jensen Jacoby responsive xenoglossy case (Stevenson, 1974b).

**Donald West.** 1924–2020. British psychiatrist associated with the Society for Psychical Research.

**Kerr White.** 1917 - 2014. Stevenson's elder brother. He assumed the surname "White" to satisfy a childless maternal uncle who wished to have his surname passed on.[13]

**Stewart Wolf.** 1914–2005. American physician, pioneer in psychosomatic medicine.

**Harold Wolff.** 1898–1962. American physician, along with Steward Wolf pioneer in psychosomatic medicine.

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RESEARCH ARTICLE

# Vapor Phase Electrochemistry 2: Spherical and Spheroidal Air Plasmas

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## HIGHLIGHTS

Rare atmospheric events like lightning balls, earth-lights, and UFOs (now also called UAPs) are probably all plasmas formed by chemical reactions in the air, with the potential to provide carbon-free electrical energy if artificially created.

## ABSTRACT

Among the rare meteorological phenomena that exist are long-lived spheroidal air plasmas. Of these, lightning balls are best characterized. Closely related are earth-lights, tornadic lights and Unpredictable Flying Objects (UFOs). Early physicists took all such phenomena to be plasmas and would refer to them as electric fire or fireballs. Many physicists today do not accept that these light emitting objects are plasmas because they neglect a variety of influences that result from *chemical change*. Stability results mainly from entropy production as an ionized, metastable form of nitrous acid, produced at an air plasma surface, refrigerates the surface through its conversion to the stable acid. It is then oxidized to nitric acid in an aerosol form, which restricts the inflow of air to the plasma surface. This can explain the " surface tension" of lightning balls early, as hypothesized by Stakhanov (1979). Studies of earth-lights (Teodorani, 2004) imply that these are plasma balls held together by the same forces as those providing mechanical stability to lightning balls. Studies of flame balls in space support this view. UFOs and earth-lights are structured similarly but the plasma components of UFOs can be held together by far stronger forces. Potentially, air plasmas have important technological implications since they are all powered by extracting and using chemical energy from the air. Crucially, this energy can only be extracted from air whose temperature is below $15^0$ C. If air plasmas could be prepared artificially, they would prove invaluable in supplying ample carbon-free electrical energy.

## KEYWORDS

Ball lightning; earth-lights; UFOs; air plasmas; tornadoes.

## INTRODUCTION

### Ball Lightning and UFOs

A few physicists have been interested in the seemingly anomalous properties of ball lightning for over two centuries (Arago, 1855; Flammarion, 1888; Brand, 1971; Singer, 1971; Stakhanov, 1979; Smirnov, 1987), but we are still unable to produce the balls artificially (Stenhoff, 1999). This is despite the fact that two small free-floating plasma balls were produced by accident in the mid-18th century (Priestly, 1781; Cavallo, 1782). The phenomenon possesses so many different characteristics that whole

books are needed to describe them. By the 1970s, the wide variety of apparently conflicting evidence was obvious (Singer, 1971; Stakhanov, 1979), and a few physicists concluded that there must be more than one phenomenon. This is now thought unlikely, and by 2002, it seemed clear that the real problem is the number of distinctly different disciplines that are needed to explain all the characteristics (Turner, 2002).

Reconciling all the apparently irreconcilable observations had seemed totally impossible until Stakhanov (1979) offered his tentative and largely qualitative model. It was the earliest model based on electrochemistry. It made use of what little relevant quantitative information was available on the hydration thermodynamics of the two gas phase ions he thought would be present in an air plasma. Before Stakhanov's contributions to understanding the problems there seemed to be very good reasons for doubting the very existence of ball lightning. This was despite much evidence to the contrary.

According to Singer (1971), many scientists, including Kelvin (1872), have concluded that the phenomenon has to be an optical illusion since so many of its claimed attributes clearly violated one or more of the well-established laws of physics. Faraday's conclusions were much less dismissive in that he accepted the empirical evidence but he felt secure in stating that ball lightning cannot possibly be an electrical phenomenon (Faraday, 1839). The simplest way (Turner, 2023) of describing why the conclusions of Faraday were wrong is that it *predated* the formalization of chemical thermodynamics. This was not completed until Gibbs (1878) showed clearly the importance of entropy in all chemical changes.

Stakhanov's (1979) model was based on the idea that, if a plasma containing *hydrated ions* can be held separate from the normal air by some kind of effective surface tension, the lack of buoyancy exhibited in ball lightning reports can be explained by the total weight of the hydrated ions in the plasma. His was the first model that seriously attempted to account for *all of the apparent anomalies* that have long been reported and have continued to confuse physicists. Dozens of these anomalies are known, and Stakhanov's model could account for most of them, including a number of characteristics that no previous model had attempted to explain. However, there were two major limitations. One was that the model could not explain the existence of the most powerful plasma balls that have been reliably reported. The other was that no *explanation* was provided for the *origin* of the "effective surface tension" which is a crucial ingredient of the model. In addition, Stakhanov's choice of one of the ions in the plasma turned out to be incorrect.

A later model, based closely on Stakhanov's, removed all of these limitations by extending the gas phase thermodynamic data *to far higher degrees of hydration* than Stakhanov had considered. An interpolation, between the gas phase hydration data and those for the liquid phase, removed this limitation. The new model (Turner, 1994) explained the apparent surface tension of the balls as a consequence of various chemical and electrochemical changes that seem to be unavoidable *under suitable conditions*. It changed Stakhanov's original picture by showing that the excess weight of a ball is not, as he had assumed, in a spherical volume of lightly hydrated ions; the weight is actually concentrated *at the surface* of a much hotter plasma than his model could explain. This weight of really heavy ion clusters *at the ball's surface* is mainly, though probably never entirely, balanced by the buoyancy of the hot central plasma.

Cooled aerosols need to be produced at the plasma surface, and they restrict the air inflow toward the plasma. In this way, such reported characteristics as bouncing and squeezing through holes smaller than the ball's diameter are explained. Even tendencies to be top-heavy (Stakhanov, 1979) and to be drawn towards hot objects can now be explained - *perfectly naturally though only qualitatively* (Turner, 1998a, 2001, 2002, 2003). One key to the improved model is a relationship for Stakhanov's "effective surface tension" which can be calculated from basic laws of physics together with what seem to be reasonable estimates for the parameters involved (Turner, 2002).

The most obvious relationship comes from an approximate balancing of weight with buoyancy (Turner, 1994). The number of aerosols or droplets surrounding the hot air plasma can be taken as $n_a$, and their mean radii are all assigned a value of $r_a$. The radius of the plasma can be taken as $r_p$, the density of the surrounding aerosols (or droplets) being that of normal water, $n_w$, while the mean density of the hot plasma can be taken as $n_p$ and that of the ambient air as $\tilde{n}_a$. Then, at equilibrium:

$$n_a r_a^3 \rho_w = r_p^3 (n_a - n_p)$$

The model also attempted to assess how the chemically induced air inflow and viscosity will influence a ball's stability, but, in this case, the arguments were far less straightforward. This was partly because of an inadequate understanding of the causes for occasional deviations of the balls' shapes from spherical symmetry (Stakhanov, 1979). Nevertheless, it was very clear that reasonable flow rates had no difficulty whatsoever in accounting for the range of "effective surface tensions" that Stakhanov had found were needed.

Later, it was realized that *the most important fact* im-

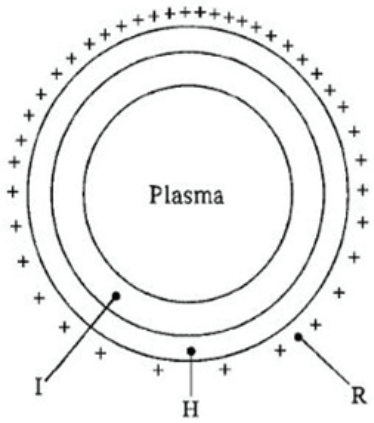


**Figure. 1** Schematic Diagram of a Lightning Ball in the Electric Field of a Thunderstorm (not to scale). R is the refrigeration zone, H is the hydration zone and I is the intermediate zone. The ball is held together by an inflow of air but nitrogen oxidation is only possible as long as the refrigeration zone R is present. There are more positive charges on top of the ball than below it due to charge neutralization resulting from conductance in the electric field of the storm.

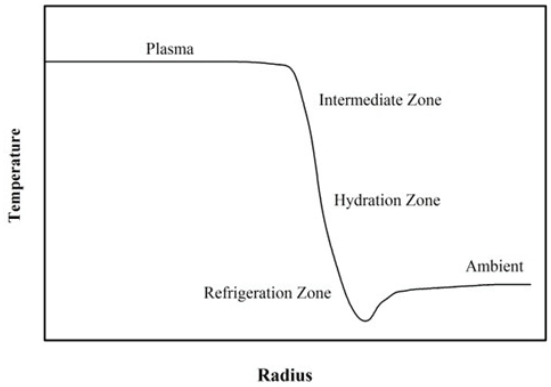


**Figure 2.** Temperature Profile Near the Surface of a Lightning Ball. No value can be quantified.

plied by the above equation (also by another equation - for the pressure difference produced by the droplets) is that it is *always the product* of the droplet sizes and their concentrations that controls one of the forces acting on a plasma ball. The important point is that a local reduction in *either* $r_a$ or $n_a$ will increase the local inflow of air. This means that any such reduction, whether induced by a local flow of current (Turner, 1994), by a local heat source (Turner, 1996a, 2001), or, as we shall see, by the presence of another nearby plasma ball, can produce an unanticipated force of attraction.

Figure 1 is a schematic diagram of a lightning ball according to this model (Turner, 1994). It represents a plasma ball floating in the normal electric field of a thunderstorm. The plasma at the center, once established, acts like a catalyst for the oxidation of nitrogen, first to nitrous acid and then to nitric acid. These reactions extract energy from the air and explain the surprisingly long lives of some plasma balls. We shall see that such balls can combine to form larger and longer-lived assemblages of plasma balls such as earth-lights and unpredictable flying objects (UFOs). The acronym UFO has long been used to stand for Unidentified Flying Objects, but Unpredictable Flying Objects now seems to be a more appropriate name. This acronym has the advantage, over all the more recently used ones, that it has been in constant use for three-quarters of a century (Turner, 2023). The following descriptions of the zones in a lightning ball (some widths greatly exaggerated in the figure) assume that all current interpretations of the relevant observations (Turner, 2002) are correct. No width can currently be quantified because the relevant chemistry cannot be quantified (Turner, 2023).

Figure 2 represents, qualitatively, a radial profile of the temperature near the surface of a lightning ball. Plasma temperatures and diameters of lightning balls are known to vary over many orders of magnitude (e.g., Stenhoff, 1999). The rates of the various chemical reactions occurring outside the plasma will also vary greatly. We possess no valid way of describing these rates of reaction because they involve ions in humid air (Turner, 2023). Because we are equally ignorant of the rates of aerosol growth as a function of distance from the plasma, the size distributions of the particles surrounding it are also unknown. All we know about the particles is that they can be either small enough to appear transparent or so large that they prevent any light from passing through them (e.g., Singer, 1971). In principle, their sizes can vary by factors of millions (Turner, 2023). Since ambient air is being drawn into the plasma, there will be gradients in humidity which it is also impossible to quantify.

In the vital refrigeration zone R (now believed to be very thin), endothermic (heat extracting) reactions occur, and aerosols containing nitrous and nitric acid are produced just outside it. In zone I, the intermediate zone, the identities of the very hot plasma ions change, as they cool, in favor of more stable (lower energy content) ions. In zone H, which is probably the widest zone, the most

**Table 1.** Approximate Temperatures and Key Chemistry near a Plasma Surface

| Zone | Plasma | I | H | R | Exterior Air |
|---|---|---|---|---|---|
| **Temperature** | > ~ 450 | < ~ 450 | 450 to ~ 15 | < ~ 15 | normal range (degrees C) |
| **Significant Chemistry** | hot ions | $NO^+$ and $NO^-$ are formed | hydrated $NO^+$ changes to $H_3O^+.nH_2O$ | molecular $NO_2$ is formed | $HNO_2$ is oxidized to aerosols that contain $HNO_3$ |

stable ions arriving from I (which are $NO^+$ and $NO_2^-$) become increasingly hydrated as they cool further. In this process, the anion remains unchanged (apart from its hydration), but the cation changes to hydrated forms of $H_3O^+$. An electrical double layer exists between the plasma and the intermediate zone, with an excess of electrons lying toward the edge of the plasma.

Table 1 provides a simplified summary of the various processes that are, according to the basic electrochemical model (Turner, 1998a), occurring near the plasma boundary of a lightning ball. All the processes must be occurring at suitable rates for any plasma, or group of plasmas, to have a long life. The need for them all to proceed at near-optimum rates probably explains why air plasmas are all rare phenomena.

## Quantitative Restrictions Concerning the Electrochemical Model of Air Plasmas.

Since the *rates* at which ions react in moist air cannot currently be calculated - assuming that valid conclusions are required - ion *concentrations* cannot be calculated either. This problem results from a sub-discipline of thermodynamics that has never been developed (Turner, 2023). Unfortunately, this is not the only problem that restricts us to qualitative arguments.

The role of aqueous aerosols is clearly vital to the model, but they are very poorly characterized. Mole fractions of any impurities in them can vary widely (by factors of many millions), and they have hardly been explored experimentally at all (Turner, 2023). To make matters worse, where experimental studies on aerosols have been made, there is frequently little agreement on the interpretations of the results. A summary of the problems has recently been provided by Lee et al. (2019). Clearly, any serious attempt to quantify the model is likely to prove misleading. Fortunately, it is possible to glean a little more information on air plasma processes by considering the characteristics of *grouped plasma balls*.

As we shall see later, grouped air plasmas are usually far longer-lived than are lightning balls and single lightning balls have *never* been observed to approach one another close enough to stay in contact. This is because of the positive charges on the outside of each ball. For this reason, the large groups of plasmas observed in earth-lights and UFOs probably need to be produced at the same time. The reason for the longer lives of clustered plasmas is probably that the electrochemical environment established by any single ball, in a group of them, helps stabilize the necessary processes that occur at the surface of all the others. As with normal gas flames, the *ignition* requirements of an air plasma are probably quite different from the requirements for a *long life*. It seems there are close similarities between the two forms of plasma. One obvious difference is that, because of refrigeration at their surfaces, only air plasmas can form into groups of plasma balls.

As is well known, the Earth maintains its negative charge during thunderstorms (e.g., Mason, 1971). Downward-moving negatively charged species, carrying roughly half of this current in the air, move far faster through the plasma than through the air. This is because electrons are carrying the negative current once they are inside the plasma, These electrons then proceed preferentially to neutralize some of the positive charges at the bottom of the ball. This reduces the *number* of charged aerosols below the ball and thus increases the flow of air *into the bottom of it.* An inertial force (a jet engine in reverse) adds to the effective weight of the ball and also explains the phenomenon once called "electrostatic guidance" (Turner, 1994).

Since 1993, a few physicists have acknowledged the merits of this model (see e.g., Anonymous, 1994; Chown, 1993; Corliss, 2001; Matthews, 1994). However, many more seem only to see non-existent weaknesses in it rather than the *real limitations* which result from the absence of any *valid* quantitative theory for ion-ion interactions in compressible fluids (Turner, 1983) and in moist gases (Turner, 1994, 2023). This absence first became clear in failed attempts to quantify the thermodynamic properties of electrolyte solutions in near-critical water and steam (Turner, 1983, 1989, 1990). A problem which would certainly arise in any future attempt to test improved models is sedimentation in the gravitational field of the Earth (also at sharp pipe bends in flowing steam). Some electrolytes, including NaCl, are sufficiently soluble near the critical point of water to permit experimentation (Turner, 1988), but in moist air, the *equilibrium* levels of electrolytes are all so low that comparable experiments would be impossible.

Lightning balls were once fairly commonly witnessed indoors, but such sightings are far less frequent inside modern homes. This is probably because lightning balls are attracted to heat sources (Turner, 1996a, 2001), including those produced by coal or wood fires. The attraction results from smaller, slightly hotter aerosols on one side of the ball. There has long existed an almost unbelievable illustration, from 1886 originally, of a large lightning ball entering a room over what appears to be a burning fire and terrifying the people in the room (Hartwig, 1892). The occurrence is by no means unique (Brand, 1923). Few of the serious books on ball lightning use this image - although recent entries in Wikipedia have shown it. Previous decisions *not* to re-publish it were probably

made because the event seemed impossible. Hence most reputable scientists have not wanted to risk using what could be unreliable evidence. However, once the electrochemical model for air plasmas is accepted, the event can be seen as perfectly natural (Turner, 1996a).

Few houses in the developed world are any longer heated using open fires - so the balls now have fewer ways that favor entry into a room. In and near the top of a chimney, above a burning fire, a lightning ball sufficiently close to it will be unavoidably drawn towards the heat from the fire below it because of the fact that the equilibrium *diameters* of aerosol particles will be smaller on the side of a ball that is warmer than on the other side (Turner 1996a, 1998a. 2001). Presumably, similarly produced forces can attract lightning balls in through open windows. Passage, through closed windows, is an entirely different matter, as it seems that several different types of driving forces must be involved in these cases (Turner, 1998b).

Most crucially, the first form of the basic ball lightning model (Turner, 1994) provides a very powerful cooling mechanism at air plasma surfaces (see later). As a consequence of this cooling, very hot plasmas can be contained. Due to the absence of valid ion interaction theories, however, the model is little better than qualitative - although there are a few quantitative elements. A more recent description of the missing science (Turner, 2023) clarifies *why* it is still quite *impossible to quantify* any relevant ion interaction model that could be valid for the surface of an air plasma. It also explains why the situation is unlikely to change soon - unless attitudes to the support of some kinds of very long-term research change completely.

It seems that once well established, a ball's lifetime need only end when the electric double layer at the plasma surface is somehow destroyed. This can occur for a variety of reasons, including contact being made with a well-earthed object, an unobserved change in the electrical state of the air, or encountering a parcel of air whose impurities disrupt the oxidation processes at the ball's surface. In the model being described, a central plasma is obviously *assumed*. However, the fact that an air plasma's presence can explain so many observations supports the basic assumption. Many physicists, who simply cannot believe in any plasma model, seem unable to accept that *only electrochemical models* can explain *every one* of the well-known peculiarities of lightning balls.

## Further Clues Concerning the Electrochemical Model of Air Plasmas.

Fortunately, Powell and Finkelstein (1969) were prepared to accept (as have many other physicists) that ball lightning must be a plasma. As a result, they obtained emission spectra from brief globules of plasma that were produced by powerful radio frequency discharges on mixtures of nitrogen and oxygen. Their conclusions were essential components in the first version (Turner, 1994) of the basic electrochemical model for ball lightning.

Dozens of distinct ball lightning characteristics have been described over the centuries and many of them, taken together, certainly do imply that the phenomenon is inconsistent with one or more of the known laws of physics. Descriptions go back many centuries, the earliest one recently found in English records apparently dating from 1195 (Gasper & Tanner, 2022). Most of the apparent anomalies are by now very well defined, but no property, except perhaps size *range* (Turner, 2002), can be explained even semi-quantitatively (Turner, 1998a, 2023).

Partly because of these problems, a number of writers on the subject have been unwilling to accept that any kind of self-contained air plasma can exist. There are usually two justifications claimed for this belief. The first is the objection that Faraday (1839) raised. He simply could not accept that ball lightning could possibly be an electrical phenomenon. This was because all of the evidence available to him implied the inevitability of rapid charge neutralization - but this is *not the case* if the two ions concerned are heavily hydrated (Turner, 1989, 1994, 2023). The second claim is that any *self-supporting* plasma violates the so-called virial theorem (Singer, 1971; Collins, 1978). This theorem completely ignores chemical driving forces so that the argument is totally irrelevant if chemical and electrochemical forces contribute to the stability of the plasma. The mere existence of gas flames, where chemical processes provide containment, now makes this criticism seem absurd.

Probably, however, the main factor that limits our understanding of ball lightning is that it has proved impossible to simulate most of its characteristics under controlled conditions. Over the centuries, there have been numerous preparations of short-lived, roughly spheroidal air plasmas that have been claimed to be simulations of ball lightning, but a lifetime of two seconds is about the longest ever claimed over the last two centuries (Barry, 1980). In fact, there seem to have been only two truly realistic simulations ever, and they were both accidental preparations in the mid-18$^{th}$ century.

After providing detailed descriptions of the two experiments, Priestley (1781) commented on one of them as follows: "Could we repeat this experiment, there would not, I think, be any natural phenomenon, in which the electric fluid is concerned, that we could not imitate at pleasure. This circumstance alone makes it a very interesting object of investigation". See Priestley (1781), Caval-

lo (1782), or Turner (2002) for the only detailed description of the experiments we possess. Sadly, Priestley's over-optimistic hope is still unfulfilled.

These early experiments had been performed by a physics teacher, John Arden, and a landowner and Fellow of the Royal Society, William Constable. In two *successive* experiments, with very large and very similar Leyden jars, a 2 cm diameter ball of light was formed (somewhere inside each jar) during the electrical charging of the jar. Each ball survived for several seconds, the first having escaped from the top of the jar in which it had formed and then returned to the inside of the jar for a few more seconds, hugging the chain carrying the charging current as it moved. This ball survived long enough for the very strange behavior to cause an exchange of comments between the two men conducting the experiments.

Both balls ended their lives by cracking circular holes through the glass wall of the jars in similar ways to those by which lightning balls occasionally crack holes in glass windows (Grigor'ev, et al., 1992; Turner, 1997a,b). The conversation during the first experiment had been recalled and it was subsequently recorded in a letter to Priestley. Its duration was used, very much later, to estimate that this ball had lasted outside the jar for at least four seconds. The total lifetime would probably have been at least 10 seconds (Turner, 2002). This is far longer than the duration of any other claimed ball lightning simulation, of which I am aware, *and* it simulated *far more characteristics* than any other. The balls both ended their lives by cracking circular holes in the glass walls of the Leyden jars in which they were produced and, in both cases, the holes apparently had diameters indistinguishable from those of the balls themselves. During most modern cases of this kind of window damage, the actual formation of the holes (due to thermal cracking) was not witnessed, but *whenever the hole cracking was actually observed*, the ball and the hole seemed to have had identical diameters (Grigor'ev et al., 1992).

In the case described by Priestley, a need for precise matching of electrical and chemical forces seems the only rational explanation for the fact that two extremely rare events *immediately followed one another but were never repeated*. The implication of this finding is that (very rarely) a lightning ball can start its life in a similar way to that by which a fire starts its life - i.e., with the help of a spark. However, there is little doubt that lightning balls *can also* begin their lives without the involvement of a visible spark (see e.g., Corliss, 1977, 2001; Singer, 1971). Population inversions (Handel & Leitner, 1994) seem crucial in such cases - and possibly always.

One might have expected that the formal similarity between the plasmas of lightning balls and of gas flames would quickly have led to an agreed picture of how the two phenomena are related. In the event, a very slow and circuitous path was taken. The main reason was certainly that the two accidental Leyden jar preparations were soon forgotten - and for a very good reason: the experiments were *never* replicated. Many of the most famous "electricians" of the day, including Franklin and Priestley, had apparently tried repeatedly to duplicate the findings, but al the attempts failed (Cavallo, 1782; Priestley, 1781).

In 1992, I was able to visit the stately home of William Constable, where his collection of scientific curiosities and equipment was being readied for eventual display to the public. All the hardware (except the broken Leyden jars, of course) that he and Arden must have used appear to have survived. As a consequence of help from Alan Clark, at the time Deputy Librarian of the Royal Society, I had been invited to inspect the collection before it went on public display. One important fact became clear from *simply seeing the actual hardware* that had been used: replacement of a broken Leyden jar would have been a very simple matter (with sufficient care) without causing any disturbance to most of the *very thick* brass chain used to connect the "electric machine" to the inner coatings of the Leyden jars.

The heavy chain could easily have produced a spark between its links, but it was unlikely to have provided exactly the same distribution of poorly conducting contacts between its links if it had been moved, even slightly, between the charging operations that produced the two plasma balls. Following the second experiment, the chain was presumably disturbed and the energy in the spark, that resulted from the charging current through the chain, could no longer exactly match the other required conditions. These could have been space charge distributions, air contamination, the absolute electrical potential and/or gas phase inversions of excited state molecules that might have allowed a ball to form. Such inversions are key elements in the ball lightning model of Handel and Leitner (1994), and they might well be crucial ingredients at the birth of most, if not all, lightning balls.

The similarity in size of the two plasma balls witnessed by Arden and Constable seems very significant. It is now known that reported lightning ball sizes can vary over three orders of magnitude (Stenhoff, 1999). Also, it appears to be generally recognized that even if one could predict that a ball would form somewhere in front of an observer, its diameter would be almost totally unpredictable. On rare occasions, multiple balls have been seen escaping from a dark cloud in the sky (Singer, 1971; Turner, 1996b), and they usually seem to be of fairly similar sizes - though just how similar would usually have been difficult to determine.

## Air Plasmas and Meteorological Conditions

In this context, a truly extraordinary account of multiple ball production from the clouds (Turner, 1996b) was provided to me as a consequence of one of the witnesses (Keith H. Hill) having read a brief historical account of the development of the electrochemical model that had appeared in the New Scientist (Chown, 1993). Like many of the more informative accounts by witnesses of ball lightning, this one had been remembered vividly over many decades, in fact, from late July 1956. The display had been observed by crew members of a fishing vessel who were encountering a very severe storm in the Great Australian Bight (to the south of the continent).

The cloud height seemed very low (about 300 m), and, as Mr Hill described the event, the clouds consisted of "... a solid dark grey rippled even mass from horizon to horizon. The storm began shortly after sunset with large balls of lightning coming from the cloud base, dropping to the sea in 2 to 3 seconds of activity. These rather large balls seemed to be about one metre diameter occurring every 3 to 10 seconds, to within 100 metres (but fortunately not on our vessel !) to some miles away. The display allowed us to dispense with our compass sighting as so many times the sky was alight." Following later correspondence, Mr. Hill investigated some meteorological records for the relevant day, and it seemed clear that the large air mass involved would have passed over a huge area in the southern Australian desert and then through some very humid environments near to the coast. The exact track could not be determined.

In 2006, an extremely detailed account was provided of the nuclear weapons tests that were performed by British and Australian personnel during the 1950s and 1960s (Carter et al., 2006). The motivation for this work was an assessment of the health risks to the individuals involved, but my interest was purely in the dates of the tests. The important point, from the locations of the sites, is that two of them had been almost certainly under the path of the air mass that was responsible for the unique cloud formations and for the other observations made from that fishing boat in 1956. In the 1990s, Mr Hill had gone as far as he could in finding roughly the regions over which the cloud mass responsible must have passed. At that time, it had not occurred to either of us that the locations of old nuclear test sites might be relevant.

In view of the unique nature of what Mr. Hill and his colleagues had observed, I now believe that my original conclusions about the event (Turner, 1996b) are probably largely irrelevant. They would have been quite different if I had known what was revealed in the study of Carter et al. (2006). In 1996, I tried to explain the unusual form of the clouds and the apparently very similar sizes of the lightning balls as consequences of the pickup (in the desert) of dust particles and the subsequent gradual sorting by size as the cloud moved to the south and then over the very humid coastline to the sea. This sorting may well have occurred but it seems much more relevant that what could have made the event unique was that large quantities of *radioactive materials* had been picked up and transported from one of the sites of the nuclear tests.

In fact, the date of Mr Hill's observations places significant restrictions on which of the sites might have been the source of the ionizing radiation that must surely have produced the exceptionally large number of similarly sized lightning balls. The restrictions apply because one of the test-sites (Emu Field) had been used for a few tests before the main site (at Maralinga) had been prepared. All the tests at Maralinga were dated *after* July 1956. Thus, any radioactive material could only have come from the nearby Emu Field site - or far less likely from very much more remote sites.

Clearly, much speculation is involved in the arguments just provided, but at least they can explain why there seem *never* to have been any records remotely similar to those provided ed by Mr Hill. Assuming the validity of most of the arguments used here, it seems clear that it should be *possible*, in principle at least, to provide formation conditions that are far more reproducible than has been believed to be possible in the past.

Unfortunately, we do not know what all these conditions would have been.

## More Recent Evidence

In 2002, the Royal Society published a special "Theme" issue of Phil. Trans. Roy. Soc, on the subject of ball lightning. I, among others, had been asked to contribute to it. As editor, they chose John Abrahamson, who had recently published a brief paper on ball lightning in Nature (Abrahamson & Dinniss, 2000) but who had rather little earlier experience on the subject. In my initial contacts with him, he expressed great enthusiasm for a then recent book on ball lightning (Stenhoff, 1999). This book presents a well-balanced assessment of the very challenging interpretational problems and of the widely divergent views on the nature of ball lightning.

However, it seems Abrahamson subsequently decided to seek advice from two other physicists. Both men were widely acknowledged experts in the field, but, unfortunately, *both* were among the large group of physicists who simply could not accept that ball lightning is a plasma. They were Stanley Singer and Vladimir Bychkov. The coverage of the "Theme" issue clearly reflects their

views. Presumably, this was the reason that my contribution (an invited one) was the *only one in the collection* that advocated a plasma model. The decision to ignore all basically electrical models must have been taken despite the fact that several such models offer good, if partial, explanations for some of the well established properties of ball lightning (see Stenhoff, 1999).

Singer, whose 1971 book first convinced me that ball lightning really exists, provided an introduction to the new collection. Unfortunately, it included a comment that was completely inaccurate. It reads as follows: "Ball lightning has been observed by staff in the Cavendish Laboratory, although its head at the time, Professor B. Pippard (1982), was skeptical of the reality of its existence". In fact, in that 1982 paper, he gave a completely objective description of the event and would not have fought so hard as he subsequently did to ensure the publication of my first paper on ball lightning (Turner, 1994) had he not accepted the reality of the phenomenon. Neither would he have provided me with copies of *all* the correspondence that had resulted from his 1982 description of the Cavendish event in Nature and from a subsequent radio interview on the BBC.

Some of the correspondence he provided may explain Singer's mistaken claim in his Introduction to the special "Theme" issue of *Philosophical Transactions of the Royal Society* (Singer, 2002). Presumably, the views of Singer and Bychkov are the reason the editor *added the words* "with comment" to the original title of my paper. These comments were trivial, but the effect seemed to have been to warn readers that the content of the paper should not be taken seriously. My formal work on the subject had ceased a decade earlier when the laboratory at which I had once worked was closed, and I was "offered" early retirement. By 2002, I was working without formal support. I was thus in no position to object to the addition even had I been warned of it - but this was not the case. Similarly strong views to those of Singer and Bychkov may also account for the fact that some recent entries on ball lightning in Wikipedia did not even mention that plasma models exist.

A newer entry there does refer to a few plasma models, but it still makes no mention of electrochemical models - despite listing 101 references to other work and despite the fact that no other models can explain *all* the well-reported characteristics of the phenomenon. It seems that many people prefer to believe in mysteries rather than accept that reasonable (though largely thermodynamic and qualitative) explanations for them exist.

Fortunately, a new *experimental* study provides strong support for important aspects of the improved electrochemical model (Turner, 1998a). This is because nitric and nitrous acid are *both* produced (at very low levels) in water vapour-saturated air when this is irradiated with high-energy UV (Bartlett and Turner, 2024).

The whole subject of UFOs is controversial - but only, it seems, to those who are unfamiliar with recent progress in understanding ball lightning. In an early book on UFOs, Klass (1968) assembled a wide variety of evidence demonstrating a close connection between UFOs and ball lightning. He assumed that both phenomena are plasmas. It now seems clear that Klass's kind of logical, but largely qualitative, arguments can *only* be seen as realistic once it is acknowledged that vapor phase electrochemistry has *never* been brought to a usable state of development (see Turner, 1983, 2001, 2003, 2023). Most physicists who have studied many UFO accounts find that some characteristics of these objects are so anomalous that the possibility of alien visitations must be taken seriously (Vallee, 1965; Hynek, 1972; Sturrock, 1999).

Although such beliefs are understandable, I believe they are mistaken. In my opinion, the most valid criticism of Klass's conclusions is that, since ball lightning is itself poorly understood (so poorly that it has not been reproduced artificially for over 260 years), any claimed similarity between it and UFOs represents no real advance. It seems that attitudes like this have contributed significantly to the inhibition of research on all of the naturally contained air plasma systems that exist. Specialization has not helped either (Smirnov, 2000; Turner, 2001, 2002, 2023). The tracking of aircraft by UFOs, which makes the credibility problem even worse for most people, will be considered elsewhere (Turner, 2024).

## Flames and Flame Balls

Eighteenth-century scientists, including Benjamin Franklin and Joseph Priestley, had developed only a very crude understanding of electricity, but they easily identified meteors and lightning balls as electrical phenomena. They usually called them fireballs or globes of fire (Bertholon, 1787), but they sometimes referred to them as "electric fire". It seems that most early investigators made little distinction (apart from duration) between the various forms of plasma that had been observed: fire, lightning balls, what we now call meteors, and fireballs (very long-lived meteors).

Flames are very easy to study compared with lightning balls, but despite centuries of study, there are still many unanswered questions (e.g., Gaydon & Wolfhard, 1970; Wu, et al., 1998; Wu, et al., 1999). The most instructive early experiments with flames were those that employed pre-mixed flames, for example, from Bunsen burners. However, experiments with pre-mixed gases

can also be undertaken in a quite different way - if gravity-free experiments are used. Some experiments of this type were designed and conducted specifically to address several outstanding questions (Wu et al., 1999). During one unique set of experiments in an orbiting space Shuttle, two complete surprises were encountered. These surprises are highlighted in the NASA news story, (*A Flame Ball Named Kelly*,' available at https://naturalplasmas.com.) and they will be discussed once some historical aspects of flame study have been briefly described.

During early experiments conducted by "electricians", as these scientists tended to be called at the time, it was discovered that the surfaces of flames are always electrically charged. This is not surprising to us, since we now know that a flame is a plasma and that electrons move much faster than ions. Hence, an electrical double layer is produced at any plasma surface, and such a layer is a *qualitative* prediction at the visible surface of any flame.

Sanduloviciu has long stressed the importance of self organization at a plasma surface in connection with the surfaces of lightning balls (Sanduloviciu, 1991, 1992) and she has subsequently succeeded in producing brief, but remarkably spherical, plasmas in the air (Sanduloviciu & Lozneanu, 2000). However, these balls required an extremely complex combination of fields that seems most unlikely to be provided in Nature.

Since flames require fuel and since the air provides the oxygen to burn it, it seems obvious that chemically induced air inflows (plus electrostatic forces at the plasma surface), when balanced by the thermal energy output from the plasma, can contribute stability to a flame. Laboratory studies on flames quickly led to the discovery that much greater reproducibility of experiments can be achieved by *premixing* the fuel with the oxidant. These flames then revealed how very complicated other aspects of their physics and chemistry really are (Gaydon & Wolfhard, 1970).

There is one very rare kind of feeble flame that has been reported from time to time for well over a century, but it is so rare (and poorly understood) that it almost defies belief, and hardly anyone (including practically all forensic scientists and lawyers) ever takes its possible occurrence seriously. The flames are those involved in the phenomenon of spontaneous human combustion (Randles & Hough, 1992). The only rational explanation for what is (regularly but very rarely) reported seems to be that the flames arising from the bodies observed are a kind of hybrid between a normal flame and a lightning ball (Bauer, 2003; Turner, 2003).

In other words, their stabilities partly result from similar forces to those present in ball lightning. In human combustion cases, some of the required energy could be provided by the oxidation of very small quantities of organic molecules - most plausibly ethanol. The phenomenon is usually associated with excessive alcohol consumption. As with ball lightning, though with much more evidence to go on, we still understand few of the details involved in actually *igniting* the burning process for any flame.

In the 1960s, Barry (1968) prepared some unusual balls of glowing plasma in an attempted simulation of ball lightning. The idea prompting the experiments was that ball lightning is a flame and that an electric spark, resulting from a thunderstorm field, might ignite a localized source of hydrocarbon fuel. Barry assumed that the fuel concentration required might be well below that usually needed for combustion, so these conditions were provided. Some of his experiments employed a large container filled with extremely lean mixtures of propane in air.

When an electric spark was applied between copper electrodes, bright balls were sometimes formed and, when they were, they lasted for up to 2 seconds. The balls were a few cm in diameter, yellow-green in color, and they moved randomly and rapidly about the chamber. The unusual greenish color of the balls was assumed to result from the use of copper to make the spark gap. It is just possible (because three oxidation states for copper are accessible) that copper species in the air can sometimes catalyze specific reaction steps needed in producing a stable gas-plasma interface.

In 2003, NASA published some very unexpected findings with flame balls, which had been obtained on the tragic last flight of the Space Shuttle Columbia. They represented the final experiments in a program of research on low fuel-content flames under so-called "micro-gravity" conditions (Wu et al., 1999). Actually, the gravitational field experienced on a manned spacecraft is usually more like $10^{-4}$ of normal gravity than $10^{-6}$ of it, but the name is used nonetheless. The objectives of the experiments were tests of chemical engineering models in an area where there were known to be inadequately answered questions. The most obviously strange observations from these experiments concerned occasional pairs of flame-balls that *spiraled each other* at a fixed separation once the pair had formed.

The experimental approach was, in principle, the same as Barry's since it also used pre-mixed gas components, low fuel content, and spark gap ignition. Preparatory experiments, in this case, had involved brief tests under low gravity conditions in drop towers plus a set of tests on an earlier Shuttle flight. The latter indicated that the balls had lasted considerably longer than predicted. The final results of the program were obtained during the fatal last flight of the Shuttle in 2003. A summary of

the findings and a discussion of them can be found in the web-published NASA news story.

The new results confirmed that some of the balls had survived for far longer than had been predicted by the theoretical models. These were based on known rates of chemical reaction, the heat produced by these reactions, the quantities of fuel present, and what was expected of the normal means of heat transport. However, the balls also showed completely unexpected behaviours. Multiple balls were sometimes created and on two occasions a single ball underwent a corkscrew-like spiral motion. This probably occurred when the igniter released a small particle of charged metal of appropriate sign. This then produced the attraction causing the spiral motion. If this explanation of the spiral motions is correct, it reveals serious omissions from the standard models that attempt to explain flame behavior."

When a hydrogen containing fuel is burned, the water released is strongly attracted into the electric fields of any ions present in the double layer at the plasma surface. Any combustion products that have thermodynamic properties similar to metastable nitrous acid (see earlier) can cause refrigeration to take place at this surface. If sufficient electrochemical cooling at the surface is taking place, a local structure rather similar to that of a lightning ball will result, the inflow of gas being restricted by the presence of aerosols - just as in the case of ball lightning. Under the lean fuel conditions being studied, heat losses will be unusually small.

On Earth, heat losses due to convection are significant. In zero gravity, there will be essentially no heat loss due to convection and very little due to conduction. These effects had been allowed for in the models used, so there was no obvious cause for an extended life of some flame balls. In ball lightning, heat escape by conduction is prevented by the inflow of air through a spherical array of aerosols and this inhibits conduction - and convection is prevented because of the *evenly* distributed *inflow* of reactants.

In fact, the author's earliest description of ball lightning referred to this "thermal lagging" as a significant part of a ball's role as a "thermochemical heat pump" powered by the electric field of a thunderstorm (Turner, 1994). In 1994, I had yet to realize that the production of nitric acid can feed additional chemical energy to the ball *so long as* efficient refrigeration at the plasma surface is maintained and no adverse chemical changes occur. The role of nitric acid formation in providing energy was only appreciated later (Turner, 1998a).

If, in the studies of flame balls in space, heat loss by conduction had been overestimated or reaction rate estimates had mistakenly assumed the identity of activities and concentrations, the efficiency of the combustion process could have been underestimated and led to an underestimation of the lifetimes of the flame balls. Some of the *organic ions* unavoidably released during burning may well have been able to refrigerate the plasma surface by processes analogous to those in ball lightning. The unavailability of any relevant thermodynamic data means that this is impossible to prove or disprove. However, since the actual concentration of ions around a flame ball will be minuscule in comparison with any uncharged species present, the concentrations of trace impurities inside a flame ball will greatly exceed those of the ions (as is the case with lightning balls).

If nitrogen was one of the impurities, as was probably unavoidable in even the purest gases obtainable, then metastable nitrous acid would have been escaping from the flame balls, and it, alone, could have refrigerated the plasma surface and made the electrochemistry at its surface closely resemble that of a lightning ball. Most important will have been the unanticipated force attracting air to the plasma surface just as it does with ball lightning (Turner, 1994, 1998a, 2002). If another flame is sufficiently close, the unexpected inward force will resemble that of a jet engine but in reverse (because the gas flow is reversed). Thus, qualitative arguments, similar to those used for ball lightning, seem relevant. In the case of a pair of flame balls under micro-gravity conditions, this unquantifiable force of attraction is certain to be present whenever two balls happen to be produced sufficiently close to one another.

This is because the total concentration of water released in the burning process will be fairly small in view of the lean-burn conditions employed. Hence, the ions present *between* the two balls will compete very effectively with each other for the few free (combustion-produced) water molecules present locally. The attracting force between the balls will result from the increased inflow of air where the charged aerosols are reduced in size, producing inter-ball attraction.

The cause of the inter-ball attraction (reduced sizes of the hydrated ions between the balls) is, of course, very similar to that providing what used to be called the electrostatic guidance of lightning balls (Turner, 1998a, 2002), but it is more similar to their attraction to hot objects (Turner, 2001). It should be recalled that an attracting force on a plasma ball can arise through a reduction in either the size or concentration of the aerosols involved (Turner, 1994). With flame balls, only *size* reductions are likely since no current (apart from the sparking current) was presumably present during the experiments.

The extraordinary corkscrew motions observed in the Shuttle experiments must have resulted from this

force of attraction balanced by electrostatic repulsion between the positively charged surfaces of the balls - plus slight gas motion resulting from the creation of any flame balls formed earlier. The unexpectedly long lifetimes of the gravity-free balls can be taken as additional evidence supporting the electrochemical processes that occur in ball lightning. As we shall see later, similar mechanical forces seem to explain the attraction between plasma balls in the much larger structures that are occasionally observed as UFOs.

In the electrochemical model for ball lightning, as with a flame, the central plasma's shape and stability depend on a balance between thermal, electrostatic, gravitational, and chemical forces. These produce electrostatic repulsion between cations at the outer plasma surface, and resistance to the inflow of air by the heavily hydrated ions inevitably formed near the plasma surface. Of course, lightning balls and flame balls display obvious differences, such as the nature of the fuel. In a flame, the fuel can be any substance known to be combustible over a wide range of elevated temperatures, while nitrogen is not normally thought of as a fuel at all. It can only be considered to be a fuel when nitrogen is oxidized *in moist air* by a plasma whose air surface is *below 15⁰ C* (Turner, 1998a). The difference arises simply because normal burning is sustained by the *energy* released during oxidation while, as we shall see in the next Section, the *essential intermediate* process in nitrogen "burning" is *entropy* driven.

## Thermochemical Refrigeration and Energy Supply in Air Plasmas

The electrochemical model for ball lighting (Turner, 1994) is based on the only reliable *quantitative* data that are relevant and available. They are standard state thermodynamic data for the species most likely to be involved in air chemistry (Chase et al., 1985; Wagman et al., 1982) plus data obtained later (Keesee & Castleman, 1986) on the *hydration thermodynamics* of gas phase ions. Unfortunately, the forces between such hydrated ions cannot be calculated validly using any available theory for ion-ion interactions (Turner, 1990, 2023). This fact means that *nothing of value* can be quantified concerning the thermodynamic activities of real ions (at any finite concentration) *or to their rates of reaction*. *Only standard state thermodynamic values are of any practical use at all*. The reasons for this have been re-stated in detail recently (Turner, 2023).

Clearly, this restriction to standard state properties greatly reduces how much quantitative information can be deduced from tabulated data, but by 1994, it had been discovered that the use of *standard state data* (alone) can be surprisingly informative when applied to the problem of ball lightning stability (Turner, 1994). The crucial group of reactions is approximated by the following set of charge neutralization processes:

$$H3O^{+}.nH_2O + NO_2^{-}.nH_2O \rightarrow HNO_2 + (2n+1)\,H_2O \qquad (1)$$

Hydrates of the two stablest known ions likely to be present near the surface of an air plasma are represented as the reactants in Reaction 1. In reality, it is most unlikely that n is the same for both ions. The simplification is necessary because there is no current way of knowing how the thermodynamic activities of the species present are related to their concentrations (Turner, 2023). The estimated thermodynamic properties of Reaction 1 (for standard state conditions at 25°C) are shown in Table 2 (Turner, 1994). The thermodynamics of neutralization for these pairs of ions (referred to here collectively as metastable nitrous acid) resulted in a consistent, though qualitative, explanation for *most of* the strange behaviors associated with ball lightning (Turner, 1994).

$\Delta H^{\circ}$, $\Delta S^{\circ}$, and $\Delta G^{\circ}$, are respectively, the standard enthalpy, entropy, and Gibbs free-energy for the process. They are related by the identity $\Delta G^{\circ} = \Delta H^{\circ} - T\Delta S^{\circ}$. The $\Delta H^{\circ}$ values can be thought of as the energy that would be released as heat, for each participant in Reaction 1, if each component were to be present in its standard state. This is obviously a hypothetical concept, but what matters most in the present context is the sign and magnitude of the free energy and of the enthalpy. Both energies are large in magnitude for n = 0. On the other hand, for n = 15, the *heat taken in from the surroundings* is even larger than that *released* when n = 0. In the latter case, the enthalpy is forcing the reaction to proceed from left to right, while for n = 15, it is pushing the reaction to the left so that it is *the entropy change* that forces the reaction to occur.

The dependence of the free energies of Reaction 1 on n at higher degrees of hydration is shown in Fig 3. Since what determines whether a reaction will go to the left or the right is the free energy, the $\Delta G$ values in Fig.3 show that, for all the listed values of n up to 25, the reaction can proceed to the right because the free energy is negative. As just pointed out, what allows the effect of the unfavorable enthalpy, for, say, n = 15, to be overridden is the positive entropy contribution. A positive entropy is a measure of the extra freedom that the molecules in the system gain when the reaction occurs. The cooling resulting from Reaction 1 resembles that resulting from water evaporation in that both processes have positive enthalpies and positive entropies.

The changes in $\Delta G^{\circ}$ with n imply that, for n much greater than about 25, *any form of nitrous acid in the gas phase* must be considered a *strong acid* as opposed to the

**Table 2.** Standard State Thermodynamics for Reaction 1 (from Turner, 1994)

| n | 0 | 1 | 3 | 5 | 7 | 10 | 15 |
|---|---|---|---|---|---|---|---|
| $\Delta H^0$ / kJ.mol$^{-1}$ | -700 | -487 | -217 | - 11 | 169 | 433 | 872 |
| $\Delta S^0$ / J.mol$^{-1}$K$^{-1}$ | 14 | 241 | 687 | 1174 | 1640 | 2332 | 3521 |
| $\Delta G^0$ / kJ.mol$^{-1}$ | -704 | -559 | -422 | -361 | -320 | -262 | -177 |

weak one that it is in a normal aqueous solution. The data also show clearly that the change from heating to refrigerating processes occurs over a very limited range of n, so this change is only possible in the very *early stages* of hydration of any freshly produced ions.

These will have been produced by UV irradiation from the plasma. This means that the temperature gradient close to an air plasma will inevitably be very high. This is why lightning balls occasionally crack circular holes in glass windows (Turner, 1997b). The very limited range of hydration numbers that can lead to refrigeration is presumably one of the many possible reasons for the rarity of ball lightning and its close relatives.

It should be noted that the values listed in Table 2 (taken from Turner, 1994) are the results of the original estimates, and, as pointed out at the time, the number of digits displayed implies a considerable overestimate of the precision of the actual measurements. These represented remarkable experimental achievements, but the actual precision of the available data, as tabulated by Keesee and Castleman (1986), is not known.

A far more important limitation is that, because of the absence of any theories that apply to the interactions between ions in a compressible fluid or in moist gas (Turner, 1983, 2003, 2023), nothing can be predicted validly about the actual concentrations of the ionic species that surround an air plasma. This also means that *no reaction rates between the ionic species* can be calculated - at least if meaningful conclusions are required. The stablest *dry* cation formed near an air plasma is $NO^+$ (Turner, 1994), but this is rapidly converted to $H_3O^+$ as soon as it encounters water vapor (Puckett & Teague, 1971). This fact supports the importance of Reaction 1 as well as the other main assumptions of the basic electrochemical model for ball lightning (Turner, 1994).

As seen in Fig. 3, if n exceeds about 25, nitrous acid in the vapor phase will become a strong acid in that the two ions cannot annihilate each other's charges (as Faraday assumed they would). There is now some suggestive experimental support for this implication (Bartlett & Turner, 2024). The fact that metastable nitrous acid can be a strong acid in the gas phase is crucial to plasma stability. This is because, as the distance from the plasma increases past the point where n exceeds 15, hydration numbers will increase, and the aerosols will rapidly grow. Lightning balls are sometimes transparent and sometimes very cloudy. A potentially more stable ball might be expected in the latter case. However, even such a ball cannot survive if the *earliest stages* of Reaction 1 are catalyzed in some way so that surface refrigeration becomes impossible.

Reaction 1 alone cannot explain the long lives of many lightning balls (or of their even longer-lived relatives, such as tornadic lights and UFOs). Plasmas surrounded by metastable nitrous acid only possess long lives (in the absence of a thunderstorm field) because of a second overall reaction involving nitrogen oxidation:

$$N_2\,(g) + 2.5\,O_2\,(g) + H_2O\,(g) \rightarrow 2\,HNO_3\,(aq) \qquad (2)$$

Here, the designation (g) means gas phase, and (aq) means aqueous, in the form of aerosols (and/or droplets sometimes) of nitric acid solution. There are three other reactions nominally similar to Reaction 2 - due to the possible presence of water in two phases - but Reaction 2 is the only one that is thermodynamically possible, and even then, it is only possible if the local temperature is less than about $15^0$ C (Turner 1998a, 2023). The other three reactions (all of them thermodynamically impossible) are for reactant water as a liquid and for nitric acid product as a gas. Note the reduction in the number of molecules in Reaction 2, which means that an inflow of air toward the plasma is predicted (according to Le Chatelier's principle) whenever this reaction occurs.

An air plasma itself is usually so hot that it contains numerous different ions and radicals that are of sufficiently high energy to produce either nitrous or nitric acid under appropriate conditions. This implies that the plas-

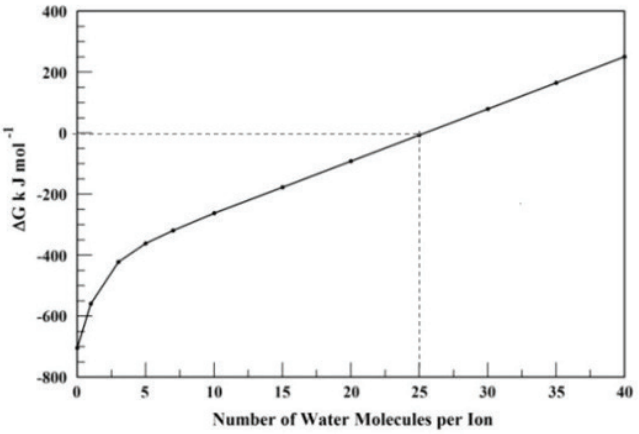


**Figure 3.** Free Energies of Reaction 1. Note that ΔG changes sign where the number of water molecules in each ion cluster (assumed identical for ions of both charges) is near 25. This means that the reaction is thermodynamically *impossible* when the number of water molecules in each ion cluster is greater than about 25.

ma can act as a catalyst for the formation of nitric acid. In the original formulations of the model (Turner, 1994, 1998a, 2002), it was tacitly assumed that the nitrous acid needed to diffuse out of the plasma. However, it has recently been shown that high-energy UV (that produced by electric charge neutralization within a mercury vapor plasma) can produce both nitrous and nitric acids directly from cool, moist air (Bartlett & Turner, 2024). This implies that *no species needs to diffuse out of the plasma* against the air that is flowing in towards it. Once nitrous acid has been produced outside the plasma, it is easily oxidized to nitric acid by, for example, the ozone that is also produced by UV radiation.

A crucial point in the context of air plasma stability is that if the metastable nitrous acid produced just outside the plasma is *hydrated by very little more than, say, six water molecules*, their subsequent neutralization will significantly cool the air close to the plasma-air boundary. The data in Table 2 also seem important in another way since they imply that refrigeration at the surface of a plasma is *impossible* if the water activity close to the plasma becomes sufficiently high. This fact must surely contribute to the rarity of stable air plasmas since processes occurring further away from the plasma need to limit the water content *at its surface* to a very narrow range. The *hemispherical* air plasmas that can be easily produced by sparks in water-saturated air (Turner, 2023) probably have very short lives because the water content of the air near the plasma is far too high.

*Any anion* (for example, formed from an organic contaminant) that happens to have similar thermodynamic properties to the nitrite ion could, in principle, behave in a similar way. However, nitrite is the only known gas phase ion, that could be involved at a plasma surface, *whose thermodynamic properties have been measured*. In fact, it seems quite possible that specific organic ions can replace the role of nitrous acid in refrigerating the surface of such feeble plasmas as those that seem to arise in spontaneous human combustion cases (Bauer, 2003; Randles & Hough, 1992; Turner, 2003) and possibly also in stabilizing the surfaces of will-o'-the-wisps.

In an established air plasma, any liquid water reasonably close to a hot plasma will tend to evaporate, whereas, further away from it, it will have a tendency to condense - as long as the local temperature is sufficiently low. In such cases, aerosols and water droplets (all eventually containing nitric acid) located at specific distances from the plasma surface would be in a state of kinetic equilibrium. If the relevant physical and chemical conditions are maintained within correct limits, which means optimal for plasma stability, these steady-state conditions *could* last indefinitely once they have been established. Demonstrably, they *do not* last indefinitely, and this implies that at least some of these conditions are easily lost once they are optimal.

Unfortunately, we still do not know exactly what these conditions are, although one obvious optimization candidate is the water activity close to the plasma - as just seen. It is also unclear to what extent quite different kinds of energy input are needed in the earliest formation stages of some air plasmas. This fact becomes clear once it is accepted that lightning balls can definitely form in the air without the slightest sign of a spark (e.g., Corliss, 1977; Handel & Leitner, 1994 Singer, 1971).

Several of the books by Corliss (including Corliss, 1977, 2001) catalog the behaviors of numerous kinds of unusual natural light phenomena. Some of them seem to be more or less closely related to ball lightning. It seems possible that information from the larger air plasmas might eventually provide valuable hints as to what the optimizing conditions are and how many of them are crucial.

Optimal conditions for forming a stable lightning ball probably include the following: the ranges of mean space-charge-density in the air, the local (possibly time-dependent) electric field, the relative humidity and local humidity gradient plus the nature and concentration of any contaminant molecules and aerosols in the atmosphere (Turner, 1998a, 2002). Generally, we know none of these parameters, and it seems clear, from the rarity of contained air plasmas, that non-ideal values are very much more likely to be present than optimal ones.

The electrochemical model for air plasmas has, to date, relied mainly on ball lightning reports, the particulars of which have been very well documented. The purpose of the material to be discussed next is to seek any relevant clues that might have been revealed through other observations on natural air plasmas: those that have been studied over a far shorter period of time than has ball lightning.

## Earth-lights, Earthquake Lights, and UFOs

Unlike ball lightning reports, which have been taken seriously by at least a few physicists for centuries, the reports of unusual flying objects are taken seriously by very few scientists. Fortunately, there exist a number of phenomena whose sizes are usually larger than lightning balls but smaller than most UFOs, and these have proved somewhat easier to study fruitfully than have lightning balls. Earth-lights are among the smallest of the poorly understood natural lights that are sometimes grouped together with UFOs. They have been studied for several decades in the Hessdalen region of Norway by Strand

and his collaborators and observed in several other places fairly regularly (Devereux, 1990; Strand, 1985, 2000; Teodorani & Strand, 1998; Teodorani, 2004; Teodorani, 2011). The Hessdalen studies, although still inconclusive in some ways, are very important.

As we shall see, the uniquely detailed studies at Hessdalen provide vital clues to the close connections that exist between ball lightning and earth-light plasmas. It now seems clear that earth-lights, as well as earthquake lights, volcanic lights and all *real* UFOs, are basically groups of interacting, electrochemically contained, air plasmas. I should try to clarify at this point what "real", in the context of earth-lights, means.

Teodorani (2004) listed 30 places in the world where apparently real earth-lights (strange luminous phenomena) are reported repeatedly. However, nothing like the detailed studies carried out in the Hessdalen valley have been carried out elsewhere. It has long been believed by some scientists that most, if not all, of these phenomena, are consequences of remote lights, such as car headlights, seen as a consequence of the mirage effect known as the Fata Morgana (Pettigrew, 2003). Some of these lights may simply be the result of these effects, but others are certainly not.

The main reason for thinking that some of these phenomena differ from those studied at Hessdalen is that many seem to arise in deserts, whereas the phenomena observed in the Hessdalen Valley all seem to occur under conditions of very high relative humidity (Teodorani, 2004). However, even deserts are not completely free of water vapor, and relative humidities can go up considerably as temperatures fall at night - so the apparent distinction might possibly be irrelevant. Most earth-lights are seen only at night. The original studies of earth-lights in Norway's Hessdalen Valley (Strand, 1985, 2000) concentrated on attempts to correlate visual appearances of the lights with the detection of radio waves, the object being to understand the energy source (or sources) of the plasmas. Seismic strains have long been considered as possible contributors to their energy supply (e.g. Devereux, 1990; Finkelstein & Powell, 1970), and this is presumably one reason they are called earth-lights.

Far fewer observations of earthquake lights have been reported. This is probably because, for obvious reasons, they are rarely seen clearly and never at close range. Nevertheless, in his book on earth-lights, Devereux (1900) refers to several studies of them made by J. S. Derr and M. A. Persinger. Earthquake lights seem to be closely related to UFOs, but unlike the latter phenomena, seismically produced radiation is more likely to be involved in their production. UFOs are commonly observed at heights of several thousand meters, so seismic forces seem unlikely to be effective. Many of the lights in the Hessdalen Valley were observed fairly high in the sky (Teodorani, 2004), but *they* could still be within the range of tectonically generated radio sources.

A number of intriguing observations, most awaiting detailed explanations, have been reported from this valley. Unfortunately, despite the very wide range of electromagnetic frequencies that have been used in investigating the phenomena, few questions have yet been answered definitively (Teodorani, 2004). The earth-lights at Hessdalen are nearly always observed at night. They tend to be larger and longer-lived than most lightning balls. Unlike ball lightning, they *never* appear to be associated with thunderstorms. Also, unlike lightning balls, they normally consist of groups of individual plasma balls, all resembling lightning balls (Teodorani, 2004).

During one four-year period, after an automated observation system had been installed in the valley, the number of balls recorded monthly varied between 4 and 18 over a period of 26 observing months (Teodorani, 2004).

Clearly, the observations were sufficiently numerous to be very instructive. While this situation is, in most respects, much more favorable to observation than are reports of ball lightning; the lights are seldom seen at close range, the valley being a large one. More studies in this valley appear to be highly desirable,

It seems clear that the *large DC fields* experienced during a thunderstorm are *not* required for the formation of these air plasmas. One very important property of plasmas is that they can absorb and emit electromagnetic energy over a very wide range of frequency (Stenhoff, 1999). This is basically why seismic strains have been thought to be possible initiators of earth-lights as well as for their fairly long lives. It should be realized, of course, that the precise needs for *igniting* any kind of plasma need not be the same as those that provide it with a long life. In lighting a gas flame, the chemistry of the spark has nothing to do with the nature of the fuel.

It is still uncertain where the energizing radiation in the Hessdalen valley originates. Piezo-electricity formed from quartz crystals in the ground has been proposed as have cosmic rays or solar wind particles decomposing in the air (Teodorani, 2004). If cosmic ray showers represent the crucial source of *ignition*, all the initial component balls of an earth-light might well be created as fairly close neighbors. And if, as seems likely, UFOs possess similar structures to earth-lights, they might well be born inside a single large cloud high in the air. It is clear that electrical energy does not need to be supplied continuously to an air plasma because nitric acid production can supply all the needed energy *once a plasma ball exists* (Turner,

1998a).

In the context of plasma stability, the most important observations on the Hessdalen lights probably come from photographic records made unusually close to a few of the lights. These permitted detailed examinations of their shapes and colours. The images were obtained with high resolution digital cameras - still and video.

Teodorani (2004) has provided a detailed summary of some of the findings but mainly concentrating on newer results - including those obtained during his group's visits to Hessdalen from Italy. Most of the detailed imaging was obtained during three joint Italian-Norwegian observing campaigns known as EMBLA. Most of the individuals studying the phenomena were either physicists or electrical engineers.

Unfortunately, it seems necessary, at this point, to comment on the very common (and perfectly understandable) tendency of scientists and others to ignore observations they do not understand. Ever since science has been considered a profession, a reasonable number of scientists have believed in the existence of ball lightning. This seems to be far less true concerning the existence of UFOs and sometimes even of earth-lights. Scientists who refuse to accept anything they have not seen with their own eyes can have so much faith in the laws they were taught that they refuse to believe there are significant gaps in our knowledge. But there are (Turner, 2023).

Some researchers even manipulate totally irrelevant facts to "prove" their points just as effectively as can politicians. In both cases, the reason is the same: they are utterly convinced they are correct. Presumably, some physicists feel justified in acting like this because of their unshakable faith that all the needed laws of physics are available - but this is not true in systems like those being discussed (Turner, 2023). For such people, evidence is usually explained away as a hoax - or simply ignored. The literature is full of similar dogmatism concerning earth-lights (Devereux, 1990). In such matters, dedicated disbelievers can go to extreme lengths to "prove" that the observed lights were really caused by some manmade light even after triangulation using photography has shown this to be quite impossible. The need to mention these facts is that ill-informed comments of this kind (plus even worse ideas) are readily accessible from some of the websites that discuss the Hessdalen phenomena and similar ones.

The more recent collaboration at Hessdalen was mainly between the Østfold University College in Norway and the Radio Astronomy Institute in Bologna, but other individuals have also been involved. In the present context, the photographic evidence was particularly revealing. In a few cases, triangulation, using images from well-separated cameras, allowed output optical powers to be calculated. One recorded light measured 19 kW of visible light (Teodorani, 2004). Structurally, the most important observations (several of which were recorded with video cameras) were assemblages of half a dozen or so white or multicolored balls, from which occasionally, a single ball would shoot away. Also, a few groups of multicolored balls were examined in detail, using image processing methods so as to provide light-intensity profiles for the individual balls that make up the more complex objects.

A few of the strongly colored balls were examined spectroscopically. All appeared to have smooth spectral emission profiles with broad peaks resembling those of light emitting diodes (LEDs). It was found that, as the emission intensity increased or decreased the spectral *shapes* were unaltered. Teodorani speculated that the spectra might result from mold spores drawn into the plasma and burnt. The suggestion was that such spores, on burning, produce a large quantity of almost mono-disperse nano-crystals of semiconductors (quantum dots).

Such nano-crystals can yield very bright colors when excited by UV radiation, and such radiation is inevitably produced by charge neutralization inside a plasma. A reasonable assumption is that only relatively low power balls are colored, while the white ones are much more powerful. This suggestion seems to be supported by the existing evidence on ball lightning. A more quantitative study might, in the future, be possible on this matter.

Many studies have shown that quantum dots can be produced efficiently by processing appropriate mixtures

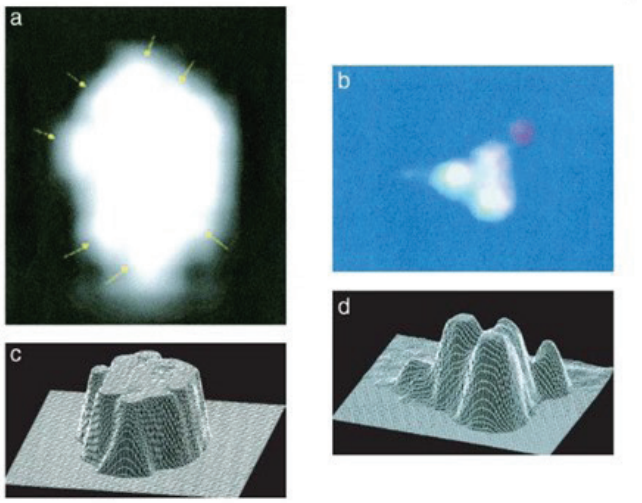


**Figure.4.** Processed Images for Two Earth-Lights. The top figures are the actual images and the bottom two are those processed by the methods described by Teodorani (2004). The left image was obtained by summing 30 sequential frames from a video-camera. The image on the right was obtained as a still photograph and the luminosity was estimated to be about 100 kW.

of chemicals inside plasmas (e.g. Mangolini et al., 2005; Sankaran et al., 2005). It seems that fungal spores can provide ideal mixtures that provide bright colors. Teodorani suggested that a search for the fungi possibly responsible was desirable. This would obviously require tests of whether burning their spores in a plasma does actually produce quantum dots. Such studies should prove very instructive - especially if spore counts at different seasons of the year could also be measured and compared with data on the brightly colored balls. The identities and properties of the various suspended mold spores could presumably be significantly different at different seasons of the year.

The forces that hold a group of spherical plasma balls together in an earth-light seem to have an origin that is closely related to those that provide structural stability in lightning balls. As shown earlier, apparently similar forces can also draw pairs of flame-balls together (under gravity-free conditions). *The forces holding multiple balls together in an earth-light* seem not always to be very large and they are balanced by electrostatic repulsion. The air inflow *between* the individual balls, in a stable assemblage of plasma balls, will be more restricted than it is *into the outward-facing* parts of the balls. Thus, they can be easily drawn together *if initially formed sufficiently close to each other*. The aerosols *between* the individual balls will be smaller than elsewhere because of the extra competition for hydration - causing mutual attraction between the balls.

Teodorani (2004) discussed several different aspects of the unusual structures and behaviours of earth-lights. One is a commonly observed change in size of such lights. Figure 5 (Fig. 6a in Teodorani's paper) shows a selection of low resolution video frames of an earth-light growing in size and then shrinking. Teodorani was able to demonstrate that the simple *expansion* of a *single* plasma sphere seems *never* to be observed.

Instead, there is a sudden appearance of "satellite spheres" around the original ball. He also provides single shots from video images showing the ejection from large white light-balls of small green ones (sometimes appearing rather yellow on a printed page). At least on the basis of the data collected so far at Hessdalen, it seems that the ejected balls are *always* green. As Teodorani points out, this seems to suggest an important role for trace chemical contaminants in the air.

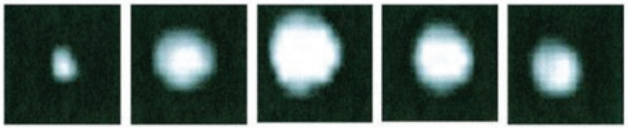

**Figure 5.** Selected Video Images Showing Changes in Size for a Stationary Earth-Light. The light was situated on a hill top and was visible for a total of 60 video frames.

Variations in radiant optical power of the Hessdalen lights tend to be characterized by a pulsation rate, either regular or irregular, whose period is normally less than one second. Teodorani (2004) observed that there would be "several cycles of pulsation, ranging from 1 second up to 3 minutes or more, in alternating 'on' and 'off' phases, each lasting some seconds. 'On' phases most often had a duration of 5 seconds". Occasionally, a few effectively invisible plasma balls were present. When a light phenomenon lasted longer than 3 minutes or so, the "radiant power tended to stabilize at a high value with a much lower-amplitude pulsation".

Figure 6 shows 21 fairly evenly spaced shots from a video record of a typical event that lasted about 3 minutes. One interesting characteristic of a few of the earth-lights observed by Strand and Teodorani in the Hessdalen Valley is that they can sometimes be invisible at optical frequencies *although detectable in the infra-red*.

Possibly the entry of a specific impurity into such a ball causes it to become visible. Presumably these balls are of fairly low energy before one or more key impurities enter the system or the local electrical state changes.

One obvious possibility is that specific insects are drawn into the *outside of one* component ball and that their presence interferes with the local refrigeration processes, thus making some balls (the green ones) sightly less robust than others. The possible consequences of such changes will be referred to shortly. On the evidence available so far, what seems clear is that small balls positioned within a few meters of the main cluster can be either white, red, or blue, but those in the range between 50 and 100 m from the main structure are always green.

These arguments imply that creating an earth-light should be little more surprising than that of a lightning ball. However, it is not obvious why lightning balls never cluster into earth-lights. Their *location at birth* (normally far from cosmic rays or sources of tectonic forces) might well be responsible. It seems significant that lightning balls, once created, never seem to transform into earth-lights. Possibly, a complete explanation would require a *valid quantitative theory* for ion-ion interactions in moist air. However, we know that this does not exist and that it is unlikely to do so in the near future (Turner, 2023).

Once the apparent family resemblance between lightning balls and earth-lights is accepted, earth-light growth, splitting, pulsation, and the overall shape-changes all seem moderately easy to understand. It seems clear that an earth-light is basically an assembly of mutually attracting air plasma balls whose output of light can change

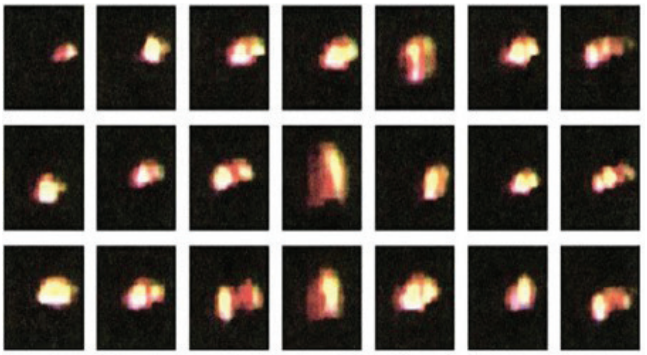

**Figure 6.** Selected Video Images Showing Changes of Shape of an Earth-Light. Only the brightest frames were selected. The light was blinking and its total duration was about 3 minutes.

very rapidly as species of different chemical identity are drawn into the component balls. It should be born in mind that most lightning balls are so much shorter-lived than most earth-lights that changes in colour or brightness are rarely recorded. They are, however, described occasionally (Stakhanov, 1979). From a few measured sizes of earth-lights (via multiple photographs of the same light source), it seems that their large *overall* size can result from both the *number* of component balls and the *individual ball sizes*. All *individual* plasma ball sizes seem to be established at birth (Turner, 2002). They never appear to change size once they have formed.

Extreme temporal variations in light intensity were found to be common with the lights at Hessdalen. Such changes are rarely observed with lightning balls. The difference may be due to the fact that lightning balls seem *never* to be observed very close to one another. Hence, normally, once a lightning ball has ceased to exist and emit UV radiation (perhaps because of the entry of some specific chemical), it can no longer induce nitrogen oxy-acid formation locally, and the self-re-enforcing chemistry ceases. On the other hand, if several plasma balls are present, as in an earth-light, there can still be ample UV locally to assist the refrigeration/oxidation processes - so long as these regions are favorable in other necessary ways (whatever they may be). If this explanation is correct, a possible implication is that igniting a lightning ball and igniting an earth-light require somewhat different conditions. Unfortunately, this does not immediately suggest what these conditions are in either case.

There are several possible reasons for size differences in individual lightning balls. These sizes are consistent with a reasonable initial growth mechanism that is very fast and, in part, is determined by the dust content of the air (Turner, 2002). The calculations imply that very large balls *only* form in rather clean air. However, hardly any of the required input parameters for even this calculation can be defined reliably, and none at all that could provide a credible estimate of a size range for an *assembly* of plasma balls such as an earth-light or UFO.

A noteworthy reported difference between the three types of air plasma is that lightning balls are almost always observed as individual objects while UFOs and earth-lights always seem to consist of multiple balls. The component balls of UFOs normally seem to be in such very close contact that, as seen in the next Section, they often appear to be single metallic-looking structures that can sometimes be decorated with bright lights.

Multiple lightning balls are occasionally seen escaping from dense clouds, but they apparently have no tendency whatsoever to attract one another. It is extremely rare for multiple lightning balls to be seen near the ground although there was one reported exception in 1897 when two ladies reported seeing a group of balls (of various delicate colors) floating around and apparently always evading their grasp (Anonymous, 1930; Corliss, 2001). Not the slightest tendency for mutual attraction was observed in this case. This is probably because of the long range of the forces of repulsion compared with the short-range influence of the inertial forces.

## Some Unique Characteristics of UFOs

There is ample evidence for the existence of atmospheric phenomena that are, in many respects, remarkably similar to ball lightning but are very much larger and longer lived. As pointed out earlier, Klass (1968) stressed these similarities. The full range of diameters attributed to ball lightning is between $2.10^{-2}$ and 2 m (Stenhoff, 1999). However, some authors (e.g., Corliss 1977, 2001) do not distinguish between ball lightning and less common atmospheric plasmas such as Unpredictable Flying Objects or UFOs. These objects have often been reported to have a linear dimension of 20 m or more, but they are usually seen at far greater distances (in such cases predominantly by aircraft pilots) than are normal lightning balls. Hence, the range of their sizes is less reliably defined.

In addition, UFOs can be reported with *significantly different shapes,* while free-floating lightning balls are always close to spherical (except when squeezing through holes smaller than their normal diameter or when they are bouncing or rotating very fast). Few ball lightning witnesses think of reporting their experiences unless they hear broadcast accounts by some eminent physicist, read about a new ball lightning model, or learn of specific requests for new accounts. Unfortunately, many people who have seen UFOs appear to be severely inhibited about reporting their observations. It seems there can be

very real risks of ridicule whenever UFO experiences are described (Hynek, 1972).

Clearly, as is the case with ball lightning, all the evidence for the existence and characteristics of UFOs comes from eyewitness testimony. Thus, it is necessary to comment on some of the adverse consequences that this fact has had on the whole question of belief in the existence of naturally contained air plasmas. The occasional hoax is one obvious difficulty, but hoaxes are usually fairly easy to identify.

There seem to be three far more important problems concerning UFOs. One is the ease with which reliable sightings can be misinterpreted or, particularly by those in authority, simply ignored (Haines, 1994; Hynek, 1972; Kean, 2010). Another problem is a general belief among many physicists that long-lived air plasmas simply cannot possibly exist. The third is the high degree of specialization in science (Turner, 2002, 2023). This has long proved essential if progress is to be made, but it unavoidably leaves all scientists, however objective, ignorant of a truly enormous number of empirical facts. Thorough collections of such facts are *only* feasible in the space provided by a book.

As indicated earlier, several good books on ball lightning have been written, nearly all of them by well-respected physicists. Recent books on ball lightning make reference to thousands of scientific papers on the subject, many of them published by scientists of repute in well-respected journals. The situation regarding UFOs could hardly be more different. The literature on UFOs is also very large, but the fraction of it that is scientifically valuable has always tended to be small (Hynek, 1972). Among the reasons that ball lightning reports are easier to believe than those of UFOs are the following facts: lightning balls are almost always small, rarely being large enough for anyone to think of the cloudy ones as containing even a baby. Also, they are usually fairly transparent. Thus, alien visitors are *never* invoked or even implied in efforts to explain lightning balls.

UFOs are even more unfamiliar objects than lightning balls to most people. Even though rather few individuals have seen a lightning ball themselves, many people seem to know of someone who has, even if it is only through the memory of an elderly relative or a friend. However, there are much more important differences between the two phenomena than this. UFOs are sometimes quite large enough to hold one or more men, and they are usually characterized by strongly reflecting surfaces. A very large number of fairly small plasma balls will emit copious quantities of UV, the output of each ball helping to stabilize its neighbors. This will produce large quantities of aerosols and barely visible droplets (Bartlett & Turner, 2024). In quantity, these can *resemble* metallic surfaces by making them capable of reflecting light from the Sun or Moon. Also, UFOs frequently display illuminated bright patches that are often interpreted as windows.

Book publishers can hardly be blamed for seeing the advantages of sensationalism over the likely boredom of scientific disputes concerning speculations on poorly understood phenomena. Nevertheless, there exist enough detailed descriptions of UFOs and their strange behaviors to clarify their clear similarities to and differences from lightning balls.

Some experienced investigators of UFOs (e.g., Vallee, 1965, 1999) prefer to call them Unexplained Aerial Objects (UAOs) - because the objects will normally have been *identified,* and they are just as likely to be stationary as moving. Others (e.g., Teodorani, 2011) prefer to use a similar acronym but substitute the word "Object" with "Phenomenon." This is presumably because the relationship between the objects and their energy sources is considered one of the most important problems - as it certainly is.

The long-used term UFO is used here mainly because of its familiarity but also because it is an acronym for Unpredictable Flying Objects. Unpredictable Aerial Phenomenon is, in some ways, a more appropriate term, but the objects can fly. It is the *unpredictability* in their behavior that is so strange (Haines, 1994). It has to be admitted that, for those like Strand (1985) and Teodorani (2004) whose interests were mainly in earth-lights, flying is not a word that readily comes to mind since they *usually* remain reasonably stationary - although there are exceptions.

Among the most numerous and mutually consistent reports of UFOs are those provided by pilots (Chester, 2007; Haines, 1994; Hynek, 1972; Kean, 2010; Smith, 1997). These reports frequently describe apparently deliberate tracking of an aircraft by one UFO or a group of them, as well as rapid changes in direction. Although often seen at considerable distances from the observers, the descriptions are surprisingly consistent - even in their seemingly most bizarre movements (Haines, 1994). UFO tracking of aircraft and mutual UFO motions, resulting from long, thin plasmas between them, will be discussed in a future publication.

One of the most respected experts on UFOs was J. Allen Hynek, an astronomer who was originally asked, by the US Air Force, simply to eliminate from its records all those UFO reports that were likely to be misidentifications of astronomical objects. Eventually, however, he spent over 20 years examining all the records held by the Air Force as well as many other descriptions provided by private citizens and policemen from observations at ground level. His general approach has been followed by

several later investigators of the subject.

The ground-level reports can be almost as strange as the reports of pilots. Both kinds occasionally describe sudden bursts of acceleration to speeds much faster than are likely to be achievable by any fighter aircraft (though not faster than the speeds of molecules in gases). In close encounters, witnesses can sense high temperatures on their skin, more rarely actual burning, and at longer ranges it is frequently observed that a UFO has burnt a patch of vegetation while it was close to it. More often than not, if the UFO approaches a moving car at close range, the ignition system malfunctions, and the car stops. The car cannot then be started until the UFO has flown away (Hynek, 1972). It is well known that plasmas can emit radiation over a huge range of frequency (Stenhoff, 1999), so that many observations are readily explicable if the objects are plasmas.

Also, when electrochemical influences are invoked, we can see that the structure and characteristics of UFOs are easily explained as assemblages of single plasma balls that differ in no significant way from lightning balls. One of the most basic problems in understanding UFOs (as well as lightning balls) is that no one has ever been able to prepare *very long-lived* air plasmas that have been *held in place* for long enough to study their full emission spectra in great detail. The only real evidence on this matter is that of Powell and Finkelstein (1969) which was discussed earlier. However, the absence of such evidence does not justify doubting those characteristics of UFOs that could simply be consequences of electromagnetic radiation.

The literature on both ball lightning and UFOs is large, and it demonstrates that viewpoints on both subjects are very diverse. This seems inevitable. Not only do the accounts seem difficult to reconcile with the known laws of physics, but viewpoints inevitably reflect what we already happen to know. Various aspects of the serious problems associated with over-specialization, as it relates to air plasma study, have been raised elsewhere (Turner, 2002, 2003, 2023). Clearly, if relevant valid theories do not exist, all that remains for advancing our knowledge are qualitative arguments. Attention here mostly concerns further evidence that the structures of air plasmas can be stabilized by their interactions with other air plasmas.

In 1999, the solar physicist Peter Sturrock was persuaded by a visiting physicist and UFO expert, Jacques Vallee, to organize a three-day presentation on the subject of UFOs. This was followed, a few weeks later, by a three-day panel discussion among the audience of eminent physicists who (some with difficulty) had been persuaded to attend. They had agreed to attend both meetings even though none had any particular interest in the phenomena. It seems that the most encouraging statement these physicists were prepared to make after the event was this: "Whenever there are unexplained observations, there is the possibility that scientists will learn something new by studying these observations." (Sturrock, 1999).

Vallee had been collecting data on UFOs for many years and has written several books on the subject. At Sturrock's formal meeting, specific examples of the kind of evidence available were provided - one involving a helicopter being so extraordinary as to be almost unbelievable. In all the specific cases described, either multiple witness reports or physical evidence (such as confirmed photographs or, radar records or both) were used. Many of the observations threw light on such points as physical appearance (shape and reflectivity), huge power ranges displayed, and (somewhat smaller) ranges in size. Unsurprisingly, no hopes were raised for research funds to study UFOs through the normal science funding agencies. In any case, it is difficult to suggest what could be done apart from observing earth-lights more thoroughly - for example, by setting up more autonomous recording stations similar to those at Hessdalen. Really adequate levels of support for such studies seem unlikely as long as basic gaps in science remain unrecognized and as long as there are plenty of more easily studied research projects to occupy the attention of scientists (Turner, 2023). However, as mentioned earlier, more studies like those at Hessdalen should prove extremely valuable.

In the present context, it is only necessary to refer to one particularly dramatic occurrence described in Sturrock's (1999) collection. One reason for choosing this example is its possible relationship to the kinds of plasma lights that are, from time to time, reported inside and above tornado funnels (Vonnegut, 1960; Vonnegut & Weyer 1966; Corliss 1977, 2001). Studies of tornadic lights, though their existence is apparently still not accepted by all meteorologists, have occasionally been published in reputable journals, and obtaining support to study these lights might prove possible in the future.

The specific UFO report that seems most relevant to tornadic lights was described by Zeidman (1999) in Sturrock's book. The UFO was witnessed in Mansfield, Ohio. There were nine eyewitnesses to the event which occurred in 1973. Four of the witnesses were army helicopter crew members and five watched from a car.

Some in the latter group eventually left their car to watch as the UFO closed in on the helicopter. This was almost directly above them at the time the two objects were at their closest. Allowing for blocked views, some fairly small differences in estimates of size by the crew, and slightly differing estimates of timings, the accounts agree remarkably well. The initial sightings, by the heli-

copter pilots, closely resembled the kind of UFO report where a conventional aircraft was tracked.

The encounter occurred at night, and the object was first seen as a red light pacing the helicopter on the eastern horizon. Minutes later, it was clearly moving towards the helicopter, and eventually, it approached a distance probably best estimated as 20 to 30 m. By then, it was above and slightly in front of the helicopter. The object appeared to be metallic, and the best estimate of its length was 30 m (Zeidman, 1999). Most of the crew initially saw a well-defined red light in front of the approaching squat, cigar-shaped object. A white light was also seen at the rear of the UFO - but only by the ground-based observers.

As the object closed in on the helicopter, it slowed down and then rapidly changed its direction so as to remain above the helicopter. After this, it parallelled the latter's path (still in front of the helicopter) for about 250 m, and then it returned to its previous bearing. While the UFO was closest to the helicopter, radio communication became impossible, and the magnetic compass was observed to rotate slowly. (The latter observations have been reported on occasion when pilots have close encounters with UFOs). During this period also, a beam of green light shone strongly into the cabin of the helicopter making everything inside appear bright green. According to the witnesses on the ground, there was already a dull green light below the object that brightened significantly (for at least ten seconds) while it was closest to the helicopter. The green light seems (for geometric reasons) to have been impossible to notice from inside the helicopter until the object had moved to a position above it, at which time it suddenly became much brighter.

A point agreed on by all the observers was that, until the UFO was above the helicopter, the brightest light was the red one at the front of the object. This would be consistent with the motion of the UFO being driven by an inflow of air where there is least resistance to it (and presumably, the escaping light would be brightest). During the period of closest approach, the green light increased its brightness dramatically. The apparent opening up of the ball supplying the green light implies a charge neutralization process at the ball's surface similar to that which used to be referred to as the electrostatic guidance of lightning balls (see earlier).

It should be remarked that a simple DC field may not be the only kind that could have led to charge neutralization below the green ball and, thus, an increase in the attracting force between the two objects. If an alternating field was present, possibly in addition to a DC field, a charge neutralization process would occur on the side facing the source of the current - i.e., the helicopter. A force of repulsion, during the opposite phase of the current, need not have been effective if interactions with the many other plasma balls behind it partly neutralized its effects.

The observations of most relevance here were reported by the pilot and co-pilot of the helicopter at the time of closest approach. The pilot had gradually lowered the helicopter from its original height in order to avoid the approaching UFO. Finally he moved the relevant lever to achieve the maximum possible rate of descent.

*Despite this*, the helicopter was found, shortly after the object departed, to be higher than the crew thought remotely possible and still *rising* at a rate of 1,000 feet (300 m) per minute while *the lever was still set for the maximum rate of descent*. It seems obvious that the UFO had been responsible for the dramatic increase in the helicopter's height. The inflow of air, to the plasmas that lifted the helicopter, must have been considerable.

Once it is accepted that tornadic lights could be similar to this particular UFO, the powerful lifting abilities of tornadoes and the well-known atmospheric pressure drops below them (Meteorological Office, 1978; Roberts, 1982) would seem to be assisted by essentially the same processes in both phenomena. Presumably the lifting forces routinely used in tornado models, which arise from rising parcels of humid air, would still be present in the presence of the plasma lifting processes. It is not obvious to me that the former kind of lifting force can be predicted sufficiently precisely to rule out an *additional plasma assisted* role in most tornadoes.

As we have seen, most descriptions of UFOs by pilots suggest that they are very much larger objects than Earth-lights and also are far more robust (see e.g., Haines, 1994; Hynek, 1972; Smith, 1997; Sturrock, 1999). The evidence available confirms that there are two distinct reasons for this size differences: individual component balls can be larger and the *number* of clustered balls is often very much larger in UFOs than in earth-lights.

The first effect is consistent with such UFOs usually having formed at cloud level (most likely inside clouds) in regions where *solid* condensation nuclei happen to be fairly scarce (see Turner, 2002). Large UFOs can certainly travel extremely fast, so their formation high in the atmosphere could easily be responsible for the occasional reports of the sudden appearance of similarly large objects near ground level. If they travel from cloud level to Earth as rapidly as they sometimes do, their chances of being seen are obviously lower than if they were only moving at a few hundred km/hr$^{-1}$. They are thus statistically unlikely to be seen moving from the clouds, and it may be significant that the actual *births* of UFOs seem *never to have been witnessed,* whereas the births of lightning balls and earth-lights are witnessed fairly commonly.

The second effect (increased number of balls) is consistent with the fact that the high-energy UV produced by very energetic plasma balls can produce new aerosols that are associated with local refrigeration (Bartlett & Turner, 2024). Such processes can presumably support the formation of new plasma balls nearby (as observed at Hessdalen).

In any comparison of UFOs with earth-lights, the latter appear to have a relatively non-rigid structure that holds the individual balls together for most of the time - as the result of air inflow through the (relatively) water-starved regions *between* the balls. A reduced water content there will inevitably be produced as the result of strong local competition for water vapor by the ions that face neighboring balls. The evidence for orbiting of pairs of flame-balls, represents a simpler version of this mechanism.

Such mechanisms probably provide a partial explanation for the shapes and properties of all assemblages of air plasmas. A stable geometry, once generated in *less energetic* balls (e.g., in earth-lights), can be easily disrupted by some perturbation. This might be the entry of a large insect through the outer surface of an outer ball, causing the separation of the ball from the other balls. There is no obvious mechanism that can easily cause a ball so ejected to return to its original clump of plasma balls - another *optimally placed opening* in the ejected ball being required for this to happen. This could be why the apparently weaker green balls in the Hessdalen Valley are ejected and then stay briefly in fixed positions, 50 to 100 m distant from their original clump (Teodorani, 2004).

A comment on the possible internal structure of UFOs is warranted here. We have no direct evidence on this matter. However, reasonable arguments seem appropriate. There seems no doubt that at least one layer of plasma balls always surrounds a UFO, and a second layer may also be possible. However, it is more likely that most of the interior consists of one huge plasma ball. The possibility that plasma balls *can* combine (under very unusual circumstances) may be supported by the fact that the *reverse process* is occasionally observed with ball lighting.

A clear example was that illustrated in an observation described by Mattétal (1895). This was made by a man who was watching a severe thunderstorm from an upper-story window and had a clear view of the roof of a house road across the road, During this observation, a single large sphere of plasma formed above the roof of the house opposite. This occurred on top of an iron rod. The ball suddenly released itself from the rod and split into three, similarly sized, smaller balls. These balls rolled down the roof in the form of typical lightning balls and disappeared when they contacted the gutter. There is no obvious reason why the reverse of such a process, through re-minimization of the surface energy, could not take place *inside* a large UFO where they would be held together by the inward directed forces from the outer balls.

If the electrochemical processes occurring at all the surfaces of air plasma clusters are proceeding very effectively (as is to be expected in very powerful UFOs), the reductions in air pressure between the outer balls could be so large that the balls *never separate*. The normally very stable structures of UFOs are known to split up in only about *6% of sightings* - based on 225 cases (Haines, 1994). The far smaller database on the splitting of earth-lights has only been established for a few years, but splitting seems to be considerably more common in earth-lights than it is for UFOs. If earth-light splitting really is more common than UFO splitting, the differences could simply result from differences in the efficiency of the electrochemical processes that hold the plasmas together.

## Supercell Storms

The account of the UFO witnessed in Mansfield and described by Zeidman (1999) was unusually clear and reliable. This is because its description by those in the helicopter was supported by independent witnesses on the ground and because all nine witnesses were unusually close to the UFO when it interacted with the helicopter. There can be little doubt that similar UFOs exist and that they would behave in similar ways if the circumstances were similar. The main significance of this fact to meteorology is that if objects with properties resembling this UFO exist *behind the very thick clouds* of many supercells, they could be responsible for the rare reports of tornadic lights that seem to be associated with supercell storms.

The name "supercell" describes a convective storm system in which complex airflow patterns can become established in a kinetically stable state. It seems that the formation of supercells inhibits the thunderstorm cell replacement processes that otherwise link normal thunderstorm cells together (Browning & Foote, 1976).

Severe storms that produce very large hail are usually of this type. Supercells can exist for several hours and may produce very damaging hail and sometimes spawn tornadoes (e.g., Browning & Ludlam, 1962). By the late 1960s, many characteristics of tornadic storms had been identified and tentatively explained using the concept of a supercell. This was well before any detailed testing of the concept had proved possible.

In 1972, an extensive set of observations permitted an unusually detailed study of a powerful supercell storm that started in Wyoming and ended in Kansas (Browning & Foote, 1976). The largest hailstones fell near the town

of Fleming, Colorado, and the storm was named after this town. The total track length of the storm was roughly 450 km. However, the portion of the track within the range of the ground radar installations employed in the study was all that was studied in detail. In fact, tornadoes were observed in this storm, but not until it had moved beyond the areas being monitored. Despite this, the study proved unusually instructive.

The Fleming storm was a typical super-cell storm in that it produced large hail to the left of its eastward path. Radar observations were made continuously from four fixed locations and from four aircraft flying close to and through the storm. The high-resolution monitoring of this storm provided an unprecedented amount of detail in *three* dimensions. High resolution was required because the objective was better *to understand hailstone production* - as opposed to the more usual objective of severe storm study, which is, of course, improved prediction. Far larger scale (but far less detailed ) models are used in tornado prediction.

The authors introduced their detailed study of this storm by pointing out that one characteristic of many supercell storms, in both the USA and Europe, is the existence of a distinctively shaped volume well hidden inside the clouds. They refer to this as a weak-echo vault (Browning & Foote, 1976). The term refers to a feature of the radar echoes (from the hail and rain) that is associated with these storms. Most of the fast-rising air in a supercell storm passes through these vaults, and hailstone embryos are produced near a feature that Browning and Foote (1976) refer to as the embryo curtain. This is close to where the air's upward flow is greatest - just inside a characteristic portion of the vault wall. The term vault will be used here in preference to the alternative name more frequently used these days (e.g. Snyder et al. 2013), which is a bounded weak echo region or BWER.

The results of Browning and Foote (1976) show that, from near the curtain wall of rising precipitation, the growing embryos are at first carried up and down, eventually forming very large hailstones and gradually moving more horizontally than vertically. When they grow too large to be retained by the inward air flow, they fall out of the storm. In the northern hemisphere, this is always on the left of the eastward-moving storms. The general picture of the storms (though not the hailstone motions) has, since 1976, been confirmed by many others (e.g., Markowski, 2002; Nelson, 1983). If very large hailstones, ejected during these storms, are collected and sliced, they display characteristic bands of differing textures (see e.g., Mason, 1971).

The hailstones whose nitrate distributions were reported in the first paper of this series (Turner, 2023) were of this type. In this case, the supercell responsible for forming the hailstones was called the La Plata storm since La Plata, MD. was where most damage was experienced. When anion dependencies on radius within the hailstones were obtained for three very large hailstones, enormously varying (orders of magnitude) concentrations of chloride and sulfate were found as a function of radius. However, nitrate concentrations only varied slightly - being essentially independent of radius. The only obvious way that such unchanging nitrate analyses could be explained is repeated passage of the hailstones close to a plasma (or plasmas), producing nitric acid within them on every visit.

Many observations on other supercell storms are well-established empirical facts but attempts to obtain *realistic* flow patterns within the storms have failed to produce generally accepted conclusions. Unsurprisingly, all tornado models have implicitly assumed that *hydrodynamic sinks for the air inside them are absent*. If an air plasma were to be present, however, it would inevitably act as a sink for the water vapor and some of the air by transforming moist air into nitric acid-containing hailstones and then expelling them.

This possibility seems to be routinely ignored in models despite the fact that *tornadic lights* have been reported occasionally for over a century (Vonnegut, 1960; Vonnegut & Weyer, 1966). The collection of accounts in the latter study provides convincing evidence that tornadoes are, in some way, electric phenomena and, it seems (Dessens, 1965) that (at least in French tornadoes and not infrequently then) "...the bottom of the tornado 'vomits' balls of fire". These observations all refer to individual tornadoes, rather than the supercells in which they can be produced, but electrical effects seem clearly to be present - just as Vonnegut claimed.

Possibly, large assemblages of air plasmas inside supercells can sometimes lead to the ejection of individual small plasma balls that can enter the top of a tornado funnel and then either remain in a fixed position or, rarely, move down it. The mechanism for movement against a flow of air would be the same as that which explains several other unusual properties of lightning balls, that is, a local reduction in the size of the aerosols that surround the balls in a comparatively dry wind.

On a much larger scale, the presence of hidden plasmas could explain why there is so little consistency in different interpretations of the flow patterns in various more recent storms that have been studied and modeled. Typically, the vault of a supercell is roughly cone-shaped but with the point replaced by a wide arched roof. In the case studied by Browning and Foote (1976), the main curtain of precipitation began about 4 km above the ground,

and the vault extended to a height of about 10 km. The observations implied that hailstone embryos were forming in or near the curtain and growing elsewhere in the cloud. *Inside the vault*, the hailstone production was found to be *extremely inefficient* as measured by radar. This was because only a few large hailstones were present instead of the many much smaller particles that provided the radar echoes elsewhere.

The modeling that attempted to explain the Fleming storm results (Browning & Foote, 1976) was particularly instructive because the motions of clumps of large hailstones were used to define the detailed internal airflow. When these flows were combined with measured wind velocities elsewhere, the patterns resulting appeared to be extraordinarily complex. It seems very likely that if suitably situated air plasmas (acting as sinks for the air) had been incorporated into the models, far more reasonable flow patterns would have been derived.

Presumably, the presence of one or more embedded air plasmas contributed to the larger-scale wind flows, which were, as always, inwards (toward the vault). The presence of hot plasmas would, of course, explain the absence of small hailstones within the vault. Radiant heat would have constantly evaporated any very small ice particles and allowed only large hailstones to grow.

In fact, the observed *motions* of one group of what were taken to be very large hailstones (Browning & Foote, 1976) seem to confirm the above suggestion. The authors referred to these groups of hailstones as radar "hot spots" deep inside the vault. Tracking the paths taken by a few of these grouped objects revealed some very unusual motions. Assuming, with Browning and Foote, that the "hot spots" were groups of large hailstones traveling with the wind, the path of one group, in particular, seemed truly extraordinary. This "hot spot" took a roughly horizontal curved path that had a large velocity component *towards the axis of the low echo vault,* after which it appeared to be heading towards the walls of the vault (see Fig. 16 of Browning and Foote, 1976).

Without the very strange wind motions hypothesized in the authors' model, this motion would have seemed impossible. On the other hand, if appropriately placed sinks for the air had been allowed for, the paths of the hailstones might well have been seen as perfectly natural. The raw data on which the work of Browning and Foote was based may still be available. If it is, a re-analysis of the findings that allows for plasmas in different positions could prove very instructive.

Weak echo vaults similar to the one inside the Fleming storm are sometimes reported in less destructive thunderstorm systems than this, but it seems their structures normally fade away as each storm cell weakens and is replaced in power by the next one of the system. Many studies of hailstone-producing storms have been conducted since the Browning and Foote (1976) investigation (e.g. Nelson, 1983; Wurman, Straka, and Rasmussen,1996; Marquis et al., 2012). The *less detailed* empirical evidence from them usually seems very similar to their findings. However, the various models used to explain the results often lead to divergent conclusions. This situation looks suspiciously similar to the long history of failed attempts to explain the peculiar properties of ball lightning.

## Population Inversions

Two decades ago (Turner, 2002), it was argued that a major reason for our limited understanding of ball lightning is that its various characteristics are unavoidably studied by scientists from widely differing disciplines. There are a few characteristics of lightning balls which have only been mentioned briefly so far. Probably the most important of these concern the unavoidable consequences of population inversions (Handel & Leitner, 1994). The earlier statements made here were directly related only to ball lightning but they would also be expected to apply equally to earth-lights and UFOs.

A population inversion of some kind seems *essential* in all those cases where no mechanism for forming sparks seems likely. There exist a significant number of observations of this kind. One specific manifestation of population inversions seems needed to explain the passage of lightning balls through glass windows *without any apparent damage to the glass* (Turner, 1997b). It is not clear, from the very limited information available, whether this phenomenon is more or less common than are cases where a ball cracks circular holes in the glass. If the glass of a window is undamaged when a lightning ball passes through it, energy from the ball must obviously have been transmitted through the glass. As Handel and Leitner (1994) point out, a population inversion seems the only possible way through which this process could be accomplished.

As Table 1 implies, many different processes occur at different distances from any natural air plasma. They can produce aerosols whose sizes vary widely. Population inversions are likely to be very common in at least some of the hydrated ions present outside a plasma ball. This is because energy level differences in all the clusters of water molecules are very small indeed. Hence, energy level inversions are particularly easy to accomplish in aerosols. Their presence could presumably account for many of the reported interactions between air plasmas and electromagnetic waves of widely differing frequencies.

## Power Obtainable Safely from an Air Plasma

One of the characteristics of air plasmas that have not been considered so far is the wide range of *energy density* for the phenomenon that has been attributed to them. Unfortunately, the range provided for this property is only meaningful if it is *assumed* that the balls are created following a *single* supply of energy. If this is not the case, the important property is the *power* of the plasma which is a quite different property. However, since most lightning balls only last a few seconds, a rough idea of the range of power in these small plasma balls can be obtained by neglecting this fact and simply using the apparent energy density as a proxy for the power an air plasma can deliver. Such an approach was implicitly adopted by Barry (1980), who showed that the effective energy density (assuming a once-off energy input) can be huge, varying between 0.4 and $2.8.10^5$ J.cm $^{-3}$.

From numerous descriptions in the literature of damage (or lack of it) attributed to lightning balls (and from the much smaller database on UFOs), it seems that the *power* associated with UFOs and tornadic plasmas could vary over similar ranges of power (e.g., Corliss, 1997, 2001). Doubts over whether or not ball lightning is fed by some external energy source were of great concern to nearly all pre-1994 investigators of ball lightning. This question, to which a simple and credible answer was first provided in 1998 (Turner, 1998a), illustrates clearly the problems that arise when *chemical* contributions to the stability and lifetime of all air plasmas are neglected.

Earth lights and UFOs are hardly ever associated with severe weather, so until 1998, it must have seemed obvious to most physicists that UFOs, at least, could not possibly be natural phenomena.

That the energy content and power of an assembly of air plasmas can be considerable is demonstrated by the case of the helicopter that was raised high into the air by a UFO near Mansfield, Ohio (see earlier). In this case, it is clear that it is the power and not the energy content of an air plasma that is important. Part of the reason for referring to these facts here is that they are relevant to the possibility that air plasmas might eventually provide a complete solution to the problem of global warming (assuming it is not too late already). If this development were to prove possible, the fuel used (nitrogen ) would produce mainly very dilute nitric acid (Bartlett & Turner, 2024).

There would be no point in trying to extract chemical energy from the air if it could not be accomplished safely. The only safe approach would appear to be to ensure, at least initially, that only single air plasmas could form. If there were to be a risk that new lightning balls could be created from an initial one, this could present a serious problem. However, there does not appear to be a single record of one lightning ball spontaneously creating another one, and, as discussed earlier, the reason for this seems understandable. A minor safety consideration is keeping a single ball in a fixed position. This seems to be easily achieved by providing an array of earthed metal points or rings below the plasma (Turner, 1998a).

## CONCLUSIONS

The various kinds of evidence assembled here leave little doubt that many kinds of electrochemically contained air plasmas exist on Earth and that all of them have been observed repeatedly. They include: ball lightning, earth-lights, tornadic lights, and UFOs. However, all the phenomena are extremely rare. The most obvious reason for their rarity seems to be that the long-term stability of the plasmas depends on the presence of several different kinds of force that need to operate cooperatively and possibly at different times after at least one of the necessary forces has already been optimized Our total lack of progress toward the preparation of air plasmas under controlled conditions is another serious problem. It can be hoped that this difficulty will be minimized in the future since we now seem to have a fairly good idea of how exactly these air plasmas differ. Recent successes in obtaining video images of UFOs are encouraging, but there are already many hundreds of witnessed accounts of UFOs and over ten thousand accounts of the strange properties of ball lightning. Rather, little new information seems likely to be obtainable quickly unless drones can be used to approach earth-lights and tornadic lights more closely than is currently possible.

The missing science of vapor phase electrochemistry (Turner, 2023) means that normal models involving chemical kinetics can only lead to misleading conclusions (because we cannot derive chemical activities from chemical concentrations). Thus, it is quite impossible to produce any valid *quantitative* model that involves chemical kinetics and can address the stability problem. Fortunately, some progress seems possible without the need for detailed modeling of the chemistry involved. Since the containment of an air plasma seems to depend on a near-optimal distribution of aerosols in a temperature gradient, modeling without using invalid kinetic assumptions should prove possible.

Experimentally, there seem to be at least two ways to learn significantly more about naturally contained air plasmas. One would involve new, more detailed studies of thunderstorm supercells similar to that of Browning and Foote (1976). The second would involve detailed observa-

tion of the kinds of plasma that exist and can be located reasonably predictably. The most obvious path would involve more studies of earth-lights similar to those conducted at Hessdalen. A recent proposal (Teodorani, 2024) for new, more detailed studies like these has been outlined. A more radical proposal would be to send drones like those proposed, but fitted with heat seeking devices and video-cameras into tornado-producing supercells.

Such studies might put us on a path to reversing the global warming problem - because of the chemical energy contained in the air which air plasmas do, occasionally, extract. Such developments should hopefully eliminate the need to burn fossil fuels - at least for the production of electricity.

The most economical approach to starting such a research program could involve three steps. First, it would be wise to determine whether *all large hailstones* created in supercells display nitrate concentrations whose distributions within the hailstones are fairly independent of radial position - as was the case with the hailstones from the La Plata storm (Turner, 2023). Secondly, drones fitted with heat-seeking sensors and video cameras could be used to search for air plasmas inside some tornadic supercells. Thirdly, devices for measuring the scattering of electromagnetic radiation near plasma surfaces could be added to the heat-seeking drones in the hope of obtaining more detailed guidance on the distribution of aerosols around natural air plasmas.

Since there has long been funding available for the study of flames, and because of the apparently revealing behavior of flame balls in space, more studies of flame balls under gravity-free conditions could prove valuable. However, such studies would only be really worthwhile if the flame balls were to be *modeled* in a way that accepts the importance of electrostatic fields at air plasma surfaces. Predictions of any reaction rates that involve ions would not be useful until a satisfactory way of obtaining thermodynamic activities from component concentrations is available. This seems likely to be impossible in the foreseeable future (Turner, 2023).

## REFERENCES


Abrahamson, J., & Dinniss, J. (2000). Ball lightning caused by oxidation of nanoparticle networks from normal lightning strikes on soil. *Nature, 403*, 519–21. https://doi.org/10.1038/35000525

Anonymous. (1930). Historic natural events. *Nature, 126*, 262. https://doi.org/10.1038/126262a0

Anonymous. (1994). Ball lightning formation. *Whitaker's Almanac*, 1994, 1184. J. Whitaker & Sons, London.

Arago, F. (1855). Meteorological essays. Longman, Brown, Green and Longmans.

Barry, J. D. (1968). Laboratory ball lightning. *Journal of Atmospheric and Terrestrial Physics, 30*, 313-317. https://doi.org/10.1016/0021-9169(68)90087-1

Barry, J. D. (1980). *Ball lightning and bead lightning*. Plenum Press, New York. https://doi.org/10.1007/978-1-4757-1710-5

Bartlett, V., & Turner, D. J. (in press). Vapour phase electrochemistry 3. Preparation of metastable nitrous acid. *Journal of Scientific Exploration*.

Bauer, H. (2003). Editorial. *Journal of Scientific Exploration, 17*, 387-388.

Bertholon, P. (1787). *De l'électricité des météores*. Bernuset.

Brand, W. (1971) *Ball lightning* (Translation, B. Benny, NASA TT F-13228) in *Problems in cosmic physics, Vol. II & III,* H.Grand, (original work published in German, 1923)

Browning, K. A., & Ludlam, F. H. (1962). Airflow in convective storms. *Quarterly Journal of the Royal Meteorological Society, 88*, 117-135. https://doi.org/10.1002/qj.49708837602

Browning, K. A., & Foote, G. B. (1976). Airflow and hail growth in supercell storms and some implications for hail suppression. *Quarterly Journal of the Royal Meteorological Society, 102*, 499-533. https://doi.org/10.1002/qj.49710243303

Carter, M., Robotham, F., Wise, K., Williams, G., & Crouch, P. (2006). Australian participants in British nuclear tests in Australia. *Vol 1: Dosimetry*. Department of Veterans' Affairs, Canberra. ISBN 1 920720 38 3.

Cavallo, T. (1782). *A complete treatise on electricity in theory and practice*. Dilly and Brown, London.

Chester, K. (2007). *Strange company: Military encounters with UFOs in WWII*. Anomalist Books.

Chase, M. W., Davies, C. A., Downey, J. R., Frurip, D. J., McDonald, R. A., & Syverud, A. N. (1985). JANAF thermochemical tables (3rd ed.). *Journal of Physical and Chemical Reference Data, 14* (Suppl. 1).

Chown, M. (1993). Fire and water: A recipe for ball lightning. *New Scientist, 137* (20 March), 18.

Collins, G. W., II. (1978). *The virial theorem in astrophysics*. Pachart Corp.

Corliss, W. R. (1977). *Handbook of unusual natural phenomena*. Sourcebook Project.

Corliss, W. R. (2001). *Remarkable luminous phenomena in nature*. Sourcebook Project.

Dessens, J., (1965). *Journal de Recherches Atmospheriques*, 2, 91-96.

Devereux, P. (1990). *Earth lights revelation*. Blandford Press.

Faraday, M. (1839). *Experimental researches in electricity (Vol. 1)*. Bernard Quaritch. https://doi.org/10.1098/

rstl.1839.0002
Finkelstein, D., & Powell, J. (1970). Earthquake lightning. *Nature, 228*, 759-760. https://doi.org/10.1038/228759a0
Flammarion, C. (1888). *L'Atmosphère*. Librairie Hachette.
Gasper, G. E. M., & Tanner, B. K. (2022). A marvellous sign and a fiery globe: A medieval English report of ball lightning. *Weather*. https://doi.org/10.1002/wea.4144
Gaydon, A. G., & Wolfhard, H. G. (1970). *Flames: Their structure, radiation, and temperature*. Chapman and Hall Ltd.
Gibbs, J. W. (1878). On the equilibrium of heterogeneous substances. *Transactions of the Connecticut Academy of Arts and Sciences, 3*, 108–248, 343–524.
Grigor'ev, A. I., Grigor'eva, I. D., & Shiryaeava, S. O. (1992). Ball lightning penetration into closed rooms: 43 eyewitness accounts. *Journal of Scientific Exploration, 6*, 261-279.
Haines, R. F. (1994). *Project Delta: A study of multiple UFO*. L. D. A. Press.
Handel, P. H., & Leitner, J-F. (1994). Development of the maser-caviton ball lightning theory. *Journal of Geophysical Research, 99*, 10,689-10,691.
Hartwig, G. (1892). *The aerial world* (p. 267). Longmans, Green and Co., London.
Hynek, J. A. (1972). *The UFO experience: A scientific inquiry*. Henry Regnery Co.
Kean, L. (2010). *UFOs, generals, pilots and government officials go on the record*. Three Rivers Press.
Keesee, R. G., & Castleman, A. W., Jr. (1986). Thermochemical data on gas-phase ion-molecule association and clustering reactions. *Journal of Physical and Chemical Reference Data, 15*, 1011-1071. https://doi.org/10.1063/1.555757
Kelvin, Lord. (1872). Atmospheric electricity. Royal Institution lecture, May 13. *Reprints of papers on electricity and magnetism* (pp. 226-228). Macmillan and Co., London.
Klass, P. J. (1968). *UFOs - Identified*. Random House.
Lee, J. K., Samanta, D., Nam, H. G., & Zare, R. N. (2019). Micrometer-sized water droplets induce spontaneous reduction. *Journal of the American Chemical Society, 141*, 10585-10589. https://doi.org/10.1021/jacs.9b03227
Mangolini, L., Thimsen, E., & Kortshagen, U. (2005). High-yield plasma synthesis of luminescent silicon nanocrystals. *Nano Letters, 5*, 655–659. https://doi.org/10.1021/nl050066y
Markowski, P. M. (2002). Hook echoes and rear flank downdrafts: A review. *Monthly Weather Review, 130*, 852-876.
Marquis, J., Richarson, Y., Markowski, P., Dowell, D., & Wurman, J. (2012). Tornado maintenance investigated with high-resolution Dual-Doppler and EnKF analysis. *Monthly Weather Review, 140*, 3-27. https://doi.org/10.1175/MWR-D-11-00025.1
Mason, B. J. (1971). *The physics of clouds* (2nd ed.). Clarendon Press.
Mattétal, M. (1895). Curiosités de la foudre. *Nature (Paris), 23*, 407.
Matthews, R. (1994). Great balls of fire theory explodes myth. *Sunday Telegraph*, June 12.
Meteorological Office. (1978). *A course in elementary meteorology*. H.M. Stationary Office, London.
Nelson, S. P. (1983). The influence of storm flow structure on hail growth. *Journal of the Atmospheric Sciences, 40*, 1965-1983. https://doi.org/10.1175/1520-0469(1983)040<1965:TIOSFS>2.0.CO;2
Pettigrew, J. D. (2003). The Min Min light and the Fata Morgana. *Clinical and Experimental Optometry, 86*, 109-120. https://doi.org/10.1111/j.1444-0938.2003.tb03069.x
Pippard, B. (1982). Ball of fire? *Nature, 298*, 702. https://doi.org/10.1038/298702b0
Powell, J. R., & Finkelstein, D. (1969). Structure of ball lightning. *Advances in Geophysics, 13*, 141-189. https://doi.org/10.1016/S0065-2687(08)60510-5
Priestley, J. (1781). Experiments and observations relating to various branches of natural philosophy with a concentration on the observations on the air (Vol. 2, pp. 379-383). J. Johnson, London, U.K.
Puckett, L. J., & Teague, M. W. (1971). Production of H3O+. nH2O from NO+ precursor in NO-H2O gas mixtures. *Journal of Chemical Physics, 54*, 2564-2572. https://doi.org/10.1063/1.1675213
Randles, J., & Hough, P. (1992). *Spontaneous human combustion*. Dorset Press.
Roberts, C. W. (1982). *Meteorology*. Thomas Reed Publications Ltd.
Sanduloviciu, M. (1991). On ball lightning phenomenology. In S. Kuhn, K. Schöpf, & R. Schrittwieser (Eds.), *Current research on fusion, laboratory, and astrophysical plasmas* (pp. 235-240). World Scientific, Hackensack, NJ.
Sanduloviciu, M. (1992). The ball lightning as a globular double layer resulting after a self-organization process. In R. W. Schrittwieser (Ed.), *Double layers and other nonlinear potential structures in plasmas* (pp. 268-272). World Scientific, Hackensack, NJ.
Sanduloviciu, M., & Lozneanu, E. (2000). Ball lightning as a self-organization phenomenon. *Journal of Geophysical Research, 105*, 4719-4727. https://doi.org/10.1029/

1999JD900987
Sankaran, R. M., Holunga, D., Flagan, R. C., & Giapis, K. P. (2005). Synthesis of blue luminescent Si nanoparticles using atmospheric-pressure microdischarges. *Nano Letters, 5*, 537–541. https://doi.org/10.1021/nl0480060
Singer, S. (1971). *The nature of ball lightning*. Plenum Press, New York. https://doi.org/10.1007/978-1-4684-1866-8
Singer, S. (2002). Ball lightning - the scientific effort. *Philosophical Transactions of the Royal Society A: Mathematical, Physical and Engineering Sciences, 360*, 5-9. https://doi.org/10.1098/rsta.2001.0916
Smirnov, B. M. (1987). The properties and nature of ball lightning. *Physics Reports, 152*, 177–226. https://doi.org/10.1016/0370-1573(87)90142-6
Smirnov, B. M. (2000). Advances in ball lightning studies. *Journal of Meteorology UK, 25*, 308-318.
Smith, W. (1997). *On pilots and UFOs*. Unicat Project.
Snyder, J. C., Bluestein, H. B., Venkatesh, V., & Frasier, S. J. (2013). Observations of polarimetric signatures in supercells by an X-band mobile Doppler radar. *Monthly Weather Review, 141*, 3-29. https://doi.org/10.1175/MWR-D-12-00068.1
Stakhanov, I. P. (1979). *The physical nature of ball lightning* (in Russian). Atomizdat, Moscow. (English Translation: Central Electricity Generating Board Translation CE 8244).
Stenhoff, M. (1999). *Ball lightning: An unsolved problem in atmospheric physics*. Kluwer Academic/Plenum.
Strand, E. P. (1985). Project Hessdalen 1984 - Final technical report. Project Hessdalen Articles and Reports. http://hessdalen.hiof.no/reports/hpreport84.shtml
Strand, E. P. (2000). Project Hessdalen. Project Hessdalen Articles and Reports. http://hessdalen.hiof.no/reports/Hessdal-article2000.shtml
Sturrock, P. A. (Ed.). (1999). *The UFO enigma: A new review of the physical evidence*. Warner Books.
Teodorani, M. (2004). A long-term scientific survey of the Hessdalen phenomenon. *Journal of Scientific Exploration, 18*, 217-251.
Teodorani, M. (2011). Interview. http://www.unexplained-mysteries.com/column.php?id=200292
Teodorani, M. (2024). Investigating UAP events using astronomical techniques. *LiminaT, 1*, 40-54. https://doi.org/10.59661/001c.92684
Teodorani, M., & Strand, E. P. (1998). Experimental methods for studying the Hessdalen phenomenon in the light of the proposed theories: A comparative overview. ØIH Rapport, 1998(5), 1-93. Høgskolen I Østfold, Norway. https://doi.org/10.1039/f19888402683
Turner, D. J. (1983). Electrolyte solutions near the critical point of water: A review and proposal for studies under microgravity conditions. In *Proceedings of the Fourth European Symposium: Materials Sciences Under Microgravity* (ESA SP-191, pp. 393-400). European Space Agency, Paris.
Turner, D. J. (1988). Salt solutions in high pressure steam: Some preliminary studies on the influence of gravity. *Journal of the Chemical Society, Faraday Transactions 1, 84*, 2683-2695.
Turner, D. J. (1989). On the existence of iodide-borate complexes in saturated steam. In M. Pichal & O. Sifner (Eds.), *Proceedings of the Second Safety of Nuclear Installations (CSNI) Workshop on Iodine Chemistry in Reactor Safety* (pp. 135-150). AECL-9923/CSNI-149, Toronto, Canada.
Turner, D. J. (1990). Ions in steam. In M. Pichal & O. Sifner (Eds.), *Proceedings of the 11th International Conference on the Properties of Steam* (pp. 439-451). Hemisphere Publishing Corp., New York, N.Y.
Turner, D. J. (1994). The structure and stability of ball lightning. *Philosophical Transactions of the Royal Society A: Mathematical, Physical and Engineering Sciences, 347*, 83-111. https://doi.org/10.1098/rsta.1994.0040
Turner, D. J. (1996a). Some characteristic properties of ball lightning. *Journal of Meteorology UK, 21*, 325-338.
Turner, D. J. (1996b). Reproducibility in the formation of lightning balls. *Journal of Meteorology UK, 21*, 397-399.
Turner, D. J. (1997a). The interaction of ball lightning with glass window panes. *Journal of Meteorology UK, 22*, 52-64.
Turner, D. J. (1997b). Ball lightning through window glass? *Journal of Meteorology UK, 22*, 219-221.
Turner, D. J. (1998a). Ball lightning and other meteorological phenomena. *Physics Reports, 293*, 1-60. https://doi.org/10.1016/S0370-1573(97)00043-4
Turner, D. J. (1998b). Ball lightning and window panes. *Journal of Meteorology UK, 23*, 236-238.
Turner, D. J. (2001). Explaining ball lightning. *Journal of Meteorology UK, 26*, 101-104.
Turner, D. J. (2002). The fragmented science of ball lightning. *Philosophical Transactions of the Royal Society A: Mathematical, Physical and Engineering Sciences, 360*, 107-152. https://doi.org/10.1098/rsta.2001.0921
Turner, D. J. (2003). The missing science of ball lightning. *Journal of Scientific Exploration, 17*, 435-496.
Turner, D. J. (2023). Vapor phase electrochemistry 1: The missing science. *Journal of Scientific Exploration, 37*, 390-428. https://doi.org/10.31275/20232707
Turner, D. J. (in preparation). Vapor phase electrochemistry 4: Cylindrical air plasmas.
Vallee, J. (1965). *Anatomy of a phenomenon*. Henry Regnery.

Vallee, J. (1999). Estimates of optical power output for six cases of unexplained aerial objects. In P. A. Sturrock (Ed.), *The UFO enigma: A new review of the physical evidence* (pp. 216-233). Warner Books, New York.

Vonnegut, B. (1960). Electrical theory of tornadoes. *Journal of Geophysical Research, 65*, 203-212. https://doi.org/10.1029/JZ065i001p00203

Vonnegut, B., & Weyer, J. R. (1966). Luminous phenomena in nocturnal tornadoes. *Science, 153*, 1213-1220. https://doi.org/10.1126/science.153.3741.1213

Wagman, D. D., et al. (1982). The NBS tables of chemical thermodynamic properties. *Journal of Physical and Chemical Reference Data, 11* (Suppl. 2).

Wu, M-S., Liu, J-B., & Ronney, P. D. (1998). Numerical simulation of diluent effects on flame balls. In *Proceedings of the Twenty-Seventh Symposium (International) on Combustion* (pp. 2543-2550). Combustion Institute, Pittsburgh. https://doi.org/10.1016/S0082-0784(98)80106-9

Wu, M. S., Ronney, P. D., Colantonio, R., & VanZandt, D. (1999). Detailed numerical simulation of flame ball structure and dynamics. *Combustion and Flame, 116*, 387-397. https://doi.org/10.1016/S0010-2180(97)00356-8

Wurman, J., Straka, J. M., & Rasmussen, E. N. (1996). Fine-scale Doppler radar observations of tornadoes. *Science, 272*, 1774-1777. https://doi.org/10.1126/science.272.5269.1774

Zeidman, J. (1999). The Mansfield, Ohio case. In P. A. Sturrock (Ed.), *The UFO enigma: A new review of the physical evidence* (pp. 298-372). Warner Books.

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RESEARCH ARTICLE

# A Detailed Phenomenology of Poltergeist Events

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## HIGHLIGHTS

A growing database of the key features of 'poltergeist' cases enables researchers to discover important patterns that might hold insights about the nature and dynamics of these events.

## ABSTRACT

The objective of this paper is to propose a reference point in the phenomenology of poltergeists either for people who want to know more about these phenomena or for researchers looking for cases and sources associated with some particular phenomenon. In parallel, an ongoing work is conducted aimed at building a global case repository of poltergeist cases with their phenomenological characteristics and their sources, which will be available soon at www.macropk.org. A historical view of the 50+ qualitative and quantitative studies of the poltergeist phenomenon is presented along with the different authors/researchers and the publications associated. The different types of phenomena observed are studied from four angles: the physical impacts on the environment, the interactions with people, other features such as duration, focus effect, and contagion, and how the phenomena ended. Each type of event is illustrated through different cases extracted from our case repository (about 1250), often with a short extract from (one of) the sources describing some key characteristics. A discussion about the validity of these data is then developed, looking in particular at testimonials, fraud detection, legal impacts, and the similarity of description of unconnected people. These elements tend to give a strong plausibility to the diverse phenomena observed, even the more "bizarre" ones. Considering all these cases and the details associated with them could help to build a more global picture of the phenomenon. This could provide more ideas based on facts to develop current and new hypotheses, as well as new psychophysical models, in order to make progress in comprehending the phenomenon. A list of the 105 cases used in the description of the phenomenology is provided along with their sources and their distribution across historical periods and geographical areas.

## KEYWORDS

Ghost, haunted house, hauntings, phenomenology, poltergeist, poltergeist agent, RSPK.

## INTRODUCTION

In the history of parapsychology and the field of anomalistics, hauntings/poltergeists, more recently sometimes referred to as "RSPK" (recurrent spontaneous psychokinesis) or ghostly episodes, have played an ongoing role since antiquity in many places (Dullin, 2021). Although sometimes it appears difficult to differentiate hauntings from poltergeists, here the focus will be on cases where some physical phenomena are observed: external phenomena versus internal phenomena, as depicted in Figure 1 presented by Wolfgang Fach (2023) from the IGPP at the Parapsychological Convention in Oslo. All the abbreviations used are defined in Appendix B.

Arthur Conan Doyle, through his character Sherlock Holmes, stated:

> It is a capital mistake to theorize before one has data. Insensibly, one begins to twist facts to suit theories, instead of theories to suit facts (Doyle, 2013 p. 65).

Many studies and case collections have been conducted concerning these phenomena. In a first step, researchers conducted some case collections and qualitative analyses as follows:

- Jules E. de Mirville, France, six books on spirits and their manifestation (Mirville, 1863).
- Puls (former court assessor in Berlin), Germany 1880, 70 cases (Puls, 1900).
- William Barrett, UK, founder of SPR, study on six cases (Barrett, 1911).
- Camille Flammarion, France, 100 cases (Flammarion, 1923).
- Otto Piper, Germany, poltergeists mixed with different kinds of anomalous events (Piper, 1917).
- Italian study of Ernest Bozzano in 1929: 12 poltergeist case studies in his book (Bozzano, 2000), extract from his collection of 520 cases of hauntings, 158 of poltergeists (not published).
- Albert Baron Schrenck-Notzing, Germany: seven documented cases such as the one of Hopfgarten (Schrenck-Notzing, 1921).
- Harry Price, U.K., The story of the Borley rectory in Price(1940).
- Fanny Moser, Germany, 1950: detailed study of the Stans case which took place in 1860 (Joller,1862) and 29 other cases, study extract of 300 cases she had collected but not published (Moser, 1977).
- Hereward Carrington & Nandor Fodor, UK: index of 375 cases (Carrington & Fodor, 1951).

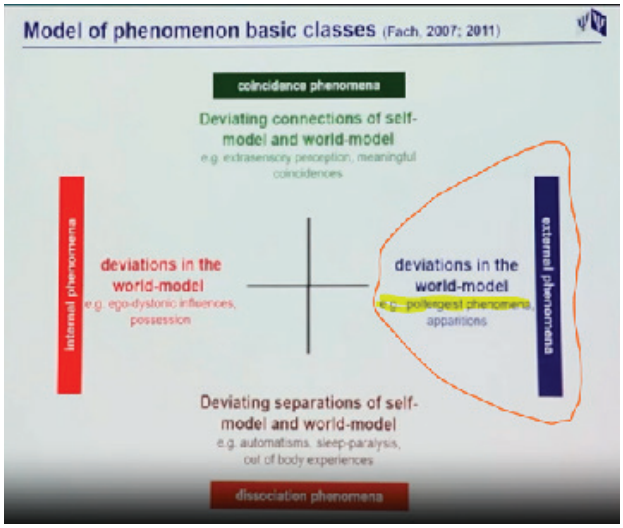


**Figure 1.** External Phenomena.

- Herbert Thurston, UK, 36 cases (Thurston, 1953).
- Lambert, UK, Geophysical theory (Lambert, 1955).
- William G. Roll, USA, proposition of the RSPK (recurrent spontaneous psychokinesis) theory in the Seaford disturbance report (Roll & Pratt, 1958).
- Sacheverell Sitwell published a study in 1959 with detail on 10 main cases (Sitwell, 1988).
- William E. Cox, 46 cases (30 US/UK) comparative analysis (Cox, 1961).
- The reasoned rejection of Lambert's geophysical theory by Antony D. Cornell and Alan Gauld (Cornell & Gauld, 1961) .
- D. Felton book in 1964 focused on ghost stories from antiquity with five cases of poltergeists (Felton, 1999)
- F.A. Volmar, Poltergeist cases and other mysterious phenomena in Switzerland (Volmar, 1969).
- Emile Tizané, France, 1962, 40 new cases in France (Tizané, 1977a).
- Alan R.G. Owen made an open treaty on the poltergeist phenomenon using the Sauchie case and other historical cases (Owen, 1964).
- Raymond Bayless: The enigma of the poltergeist (Bayless, 1967).

Of course, many of the same cases are found in different studies or books. The presidential allocution of Hans Bender at the 12th P.A. convention (1969) presented six cases investigated by the IGPP. This focus on a presidential allocution showed the importance of the topic. Then a second step started with more detailed studies, quantitative studies, and some proposals/hypotheses for explanation, such as:

- Book by William G. Roll, USA: with a review of 47 cases

(Roll, 2004).
- Roll, USA, The rotating beam theory (Roll et al., 1973)
- Guy Lyon Playfair, UK: study of six South American cases (Playfair, 1975).
- Alan R.G. Owen, Canada (with his Toronto group), inspired by the early work of Kenneth J. Batcheldor (1966), presented the results (some raps) of their Philipps experiment (P.K. by Committee) using a fictitious entity (Philipps) as a trigger, feeding in that way the psychokinesis thesis (Owen & Sparrow, 1974).
- William G. Roll, PRF, USA: study with 116 international cases (31 U.S., 26 United Kingdom, 21 Germany-Austria-Switzerland, nine Italy, seven France) (Roll, 1977a).
- Theo Locher/Guido Lauper, Switzerland: Cases of poltergeists and psychokinesis in Switzerland (Locher & Lauper, 1977).
- Alan Gauld and A.D. Cornell, UK: study on 500 cases with a detailed phenomenology and an international view (Gauld & Cornell, 1979).
- Mickaël Goss (1979): 1100 ref articles, books, newspapers on poltergeists (Goss, 1979).
- Scott Rogo reviewed 57 cases, including many American cases in his book (Rogo, 1979).
- Guy Lyon Playfair: book on the Enfield case (Playfair, 2011).
- Colin Wilson (1981): book including an argumentation around possession and black magic and the study of the Pontefract case (Wilson, 1981).
- Kenneth Batcheldor confirmed his P.K. hypothesis with his theory of P.K. induction from the Sitter Group (Batcheldor, 1984).
- Loyd Auerbach, USA: book with, in particular, his proposal to become a good investigator (Auerbach, 1986)
- Walter von Lucadou, Germany, IGPP, proposed an explanation of the poltergeist psychosocial dynamic using systems theory: The Model of Pragmatic Information (Lucadou, 1987).
- Monika Huesmann and Friederike Schriever, Germany, IGPP: study of 54 cases (Huesmann & Schriever , 2022) with comparison to the 116 cases of Roll and the 500 of Gauld (Gauld & Cornell, 1979).
- Massimo Biondi, Italy: study of 260 cases from 1800 to 2000 and their comparison with non-Italian cases (Biondi & Caratelli, 1993).
- Annekarin Puhle, Germany: study of ghosts, apparitions, and poltergeist incidents in Germany between 1700 and 1900 with a list of 50 cases (Puhle, 1999).
- Barrie G. Colvin presented a study of the rapping sounds of 10 poltergeist cases, showing some apparently key differences between the sound profile or raps and manual knocks (Colvin, 2010).
- We could also discuss the Ed and Lorraine Warren cases, studies from 1974 to 2000, even if they are a bit controversial, extracting the more documented cases from Brittle (2013).

Then since 2000, several books have been published:

- either on new specific cases such as Bothell House (Linder, 2018, 2020) or South Shields (Ritson, 2020)
- or case collections such as in Clarkson (2011), or in Lecouteux (2007) with a historical/cultural approach, or in Fraser (2020) with, in particular, the Cage case and a comment by James A. Tacchi on the Barrie G. Colvin study on sounds.
- or specific geographic areas such as Australia (Healy & Cropper, 2014) or Scotland (Holder, 2013).
- or some multidisciplinary studies such as Houran and Lange (2001), Catala (2019), and Delaplace (2021).
- or new identified trends such as the contagion (Ritson, 2021).
- or some more journalistic approach integrating polemics, such as Clément (2020) and Benoit (2021).

In parallel, some papers have been published, in particular:

- discussing the connection between psychology and poltergeists such as Ventola et al. (2019), with in particular the concept of transliminality and psychological and neuropsychological tests of poltergeist agents in Williams (2019).
- looking to the connection between environment and haunting-like experiences, such as in French et al. (2009) and Houran et al. (2023).
- the elusiveness often attached to these phenomena (Evrard, 2019).
- and the development of Von Lucadou's vision, looking at the poltergeist as some sort of entanglement phenomenon between psyche and matter (Lucadou et al., 2007; Lucadou, 2011).

In order to maintain a connection with the concrete events happening in the cases, a repository of more than 1200 cases has been built on which today more than 900 have a detailed phenomenology. The objective of this paper is to describe the detailed phenomenology encountered with the references associated, so researchers can find all the information needed to study a certain aspect of the poltergeist phenomena. A paragraph will complement the credibility of the cases constituting the repository.

## METHOD

The astronomer Camille Flammarion said about the scientific approach using a report by Lavoisier to the Academy of Sciences in France:

> In the field of scientific study, nothing should be scorned. We must always respect this double principle: do not deny anything a priori, and do not affirm anything without evidence. We imagine that to admit the reality of a fact, we must be able to explain it.
> Witnesses saw the stone fall in broad daylight on September 13, 1768, in open country; they picked it up. There she is; it is examined, analyzed, and concluded... that she did not fall from the sky. Preconceptions prevent us from recognizing the truth. The popular opinion associating these stones with thunder was wrong; one does not have the idea to reject the theory and imagine that there can be no other explanation. Human testimony is considered null and void, and even today, a certain school, friend of paradox, continues to teach that witnesses, whoever they are, have no probative value. (Flammarion, 1923, p. 378)

The approach for building this phenomenology study was:

- To identify the main phenomena reported in peer-reviewed articles, books, and possibly journals, both by investigators and by the people living the phenomena.
- To segment the phenomena by categories of events.
- To select cases illustrating the best each category of phenomena, with, if possible, at least one case associated with an investigation and reported in a paper/conference with a lecture committee.

The 105 cases selected from the 1270 cases of the database for illustrating the paper, were also chosen using, the level of testimonial and detail of the case (see below) as a guideline, and their historical position (we have favored the most recent cases). All the cases are referenced by the town where they were located and the year when they started happening, A list of the cases cited is presented in Appendix A at the end of the paper with the reference of the source associated in the standard format (Author(s), date) which are included in the reference part. Their distribution across historical periods and geographical areas is also provided.

For these phenomenology studies, we are sometimes looking for the detail, the point that the witness reported, which could give some input on the psychophysical phenomenon behind them. Some details reported are also some kind of evidence of authenticity because people building a hoax story would have very little chance to think about this very specific fact.

Some of the cases in our repository have in their description a testimonial level (1 to 10) according to the following features:

- Number of witnesses identified.
- Testimonial first-hand or second-hand.
- Delay between the event and the reporting.
- Existence of recorded audio, photos, videos.

And, a level of detail (1 to 5) according to:

- Citation in a book or on the Internet.
- Full article in a daily newspaper.
- Full article in a spirit magazine or newsletter.
- Full article in a parapsychological/scientific journal with reviewing committee.
- Day-to-day reporting of the events (log of events) by the main witness.

These were used as help to select the case in this study and in some statistical evaluations in the section "levels of evidence and plausibility".

So many phenomena are presented below with a lot of testimonials or references. A lot of effort has been made to present coherent facts confirmed by serious investigations.

However, the reader has to make up their own mind about the evidence of these facts and will have the information source at their disposal in order to study it in more detail.

## RESULTS

The phenomena of poltergeists (sometimes called "RSPK," but this will be discussed later) are complex and multifaceted.

### Kinds of Events

Table 1 gives an idea of the diversity of the phenomena observed with their frequency of appearance on the basis of 906 cases with a detailed phenomenology from our study base from antiquity to 2020. This database (still under development) currently consists of 500 cases from the study of Alan Gauld (former president of the SPR) and Antony Cornell (former president of the CUSPR) (Gauld & Cornell, 1979), 257 Italian cases from the study of Massimo Biondi and Giulio Caratelli (Biondi & Caratelli, 1993),

**Table 1.** Types of Events with their Percentage of Occurrences.

| Types of Events | % Occurrence in 906 Cases |
|---|---|
| Moving small objects | 60% |
| Noises other than raps (footsteps, voices, dragged furniture, crash ..) | 46% |
| Raps (beatings in walls, bedpost, ceilings) | 43% |
| High energy (heavy object, structural modification, slamming doors, levitation,..etc.) | 39% |
| Aggression (pulling out of bed, beatings, confinement) | 29% |
| Apparitions, mysterious figures, appearances of hands, feeling presence | 28% |
| Apports/desapports, teleportation, rain of stones, coins apports | 24% |
| Weird trajectories, high-precision shots/throws | 23% |
| Communication/interaction (through rap, writing, object arrangements, sms) | 18% |
| Action on latches, opening/closing doors | 14% |
| Starting fires, spontaneous fires | 12% |
| Spontaneous breakage, candle extinguishing, uprooted plants | 11% |
| Apparent communication with deceased persons or synchronization with death | 10% |
| Light effects, optical phenomena | 8% |
| Electrical, magnetic disturbance, dysfunction of electrical equipment | 6% |
| Ringtones, action on doorbells or entrance bells | 5% |
| Water-related phenomena (appearance of water on the floor/wall/ceiling) | 5% |
| Cold air current | 4% |
| Hot objects on contact | 3% |
| Phenomena suggestive of possession by an evil entity | 3% |
| Smell that spreads or disappears instantly | 3% |

and around 500 cases identified by the author through direct contacts and bibliographic research with an international perspective.

Each poltergeist case may involve one or more effects that are often repeated many times. For example, in Dortmund in Germany in 1713 (Puhle, 2001) , the house of Doctor Barthold Florian Gertsmann was bombarded for 20 days with 760 stones that generated 147 broken windows.

## Type of Places Concerned

Poltergeists can appear within a house and a family unit, but also in a shop, such as in the case of Beuvry (8 km from Béthune) in 1907, in a grocery store (furniture danced the saraband, chairs flew from one room to another, breaking against tables or walls) (Flammarion, 1923, p.246) , or over the period 1965–66 in a German porcelain shop in Bremen (Bender, 1969). These phenomena can also occur in a workshop like a carpenter's shop at Swanland in1849 (Myers, 1891), with pieces of wood that "fly" in the workshop, or a warehouse as in Miami in 1966 (Roll, 1971, 1973), or in a theater like the case followed by Thomas Rabeyron in Lyon in 2007 (Rabeyron, 2010) , or finally in monasteries as in Lyon in 1525 (Montalembert, 1580), presbyteries as in Borley in 1927 (Price, 1940; Hastings, 1969), or schools as in Sauchie in 1960 (Gauld & Cornell, 1979, pp. 79–83).

The following paragraphs describe the main types of events observed, with some case references that illustrate them:

## Physical Impacts of the Phenomenon

### *Sound Creation*

As its name suggests (Poltergeist: "Noisy Spirit"), the poltergeist phenomenon is often surrounded by many noises. This can be blows hitting in the walls (raps) or the bedposts, as in our personal experience in Savoie at Cessens in 1983 (Dullin & Gaudiez,2017). In some cases, these knocks have been used to communicate with the phenomenon (see also communication section).

In his book, William F. Barrett, co-founder of the SPR, recounts a case he investigated in Kingstown near Dublin in 1876. In particular, he sought to locate the sounds:

> Doubts have been suggested as to the possibility of localizing sounds; with some kinds of sounds, this is difficult, but direct experiments that I made for this purpose showed that when blindfolded, most people can pretty accurately locate the position of sounds, such as I heard on this occasion. Sometimes the raps traveled away and were heard in different parts of the room, out of reach of anyone present. On one occasion, I asked for the raps to come on a small table near me, which Florrie was not touching, they did so; I then placed one of my hands on the upper and the other on the undersurface of the table and in

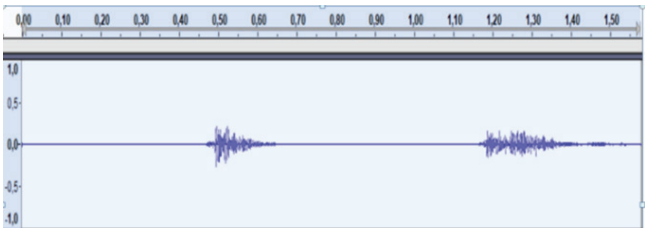

**Figure 2**. Sound Signature of a Rap in Cessens in 1983.

> this position I felt the slight jarring made by the raps on the part of the table enclosed between my hands. It made no difference whether Florrie and I were alone in the room, as was often the case, or other observers were called in. This latter was done occasionally when the raps were going on, to test my hallucination theory, but everyone heard the sounds. (Barrett, 1918, p. 61)

Barrie G. Colvin (2010), in his study of acoustic properties and, in particular, the sound signature of knocking, compared conventional sound signals with sound samples taken from poltergeist cases between 1960 and 2000 (raps, see also paragraph Communication). He found that the sound signatures of raps did not have the rapid initial rise that occurs when a blow is struck manually on metal, wood, rubber, or a wall. The signal recorded by the author at Cessens (Dullin, 2017) with a microphone placed 75 cm from the estimated center of sound emission (so close enough to avoid the signal amortization argued by James A. Tacchi (in Clarkson, 2011)) seems to confirm Barrie G. Colvin's hypothesis, with a weak start to the signal, rather than a brief rise corresponding to a strike.

This is coherent with the fact that Colvin reported that the onset of the vibration appeared to be slightly before the moment when they heard the rapping sound. So the signal seems to emanate from within the wood, like a vibration that builds in strength.

Melchior Joller, in the case of Stans in 1860 (Joller, 1862), in his personal pamphlet published by Fanny Moser (1977), explained that on Wednesday, 20 August 1862, after a pursuit of raps throughout the house, he made this direct observation of raps:

> I placed my hand on the door, variously on the inside and outside, and on the upper half around which the blows were perceptible, yet without feeling anything on my hand, not even a draft or disturbance of air. I also held the door half-open, so as to observe it from both sides; the rapping occurred again without me perceiving any cause. (Joller, 1862, Wednesday 20 August)

But often, these noises do not correspond to a physical phenomenon: for example, a great noise as if a piano had fallen by crashing into the next room was heard, while when entering the room in question witnesses did not see anything particular, or noises sounding as if something heavy such as furniture was being dragged upstairs above while in fact the room was empty, or the sound of attic stairs unfolding while there is no staircase attic as in the case of Bothell in 2012 (Linder, 2018).

Sounds such as footsteps, explosions, doors closing, latches, rustling clothes, a mason at work with their tools (hammer, drill, etc.), men fighting, and falling are also reported as in the case of Tackley in 1905 (Gauld & Cornell, 1979, pp. 183–6).

Also, as reported in (Clément, 2020, pp. 54–67), in 2011, in a school in Compiègne, established in a building corresponding to an old sanatorium, more than ten people (general assembly of the school) heard noises corresponding to kids running, laughing, and moving furniture on the floor above, whilst this place (a previous preventorium) had not been in use for a long time, was locked, and had been confirmed as being empty.

In some cases like the previous one, voices of all kinds, usually rather frightening (sighs, screams), are heard. Sometimes, it seems that a voice answers questions, as in the case of Macon in 1612 where, according to Sieur François Perrault, in his pamphlet "The Antidemon of Mascon," (Perrault, 1615) discussions took place for two months between the villagers and an "entity" described by him as a demon. Also, in the province of Quebec, in Clarendon in 1889 (Thurston, 1953, Chapter XV, pp. 162–170), 17 witnesses signed a report on the poltergeist phenomena that happened over two months on the Canadian farm of George Dagg, and in particular on the voice talking to them and answering them.

Another example of imitative voices is given in West Midlands in 1901 (Stratton, 1914), where a family and their house helps (maids and nurses) experienced all kinds of phenomena over 12 years, with imitative voices calling or answering with the same voices as an occupant or a maid, thereby creating confusion.

In addition, there is often a disconnect between the physical event (breakage of an object on the ground, violent smashing of an object against a wall) and the corresponding noise (e.g., a huge noise from a light strike on a wall or, vice versa, a very weak noise from a huge strike against the wall) as in the case in Iletsky in 1870 (Leaf, 1897), published in Russian by Alexander *Aksakov* translated and reviewed by Walter Leaf in the 12th proceeding of the SPR:

> But the strangest thing was that when they fell on

> the felt-covered floor, they made a sound which did not belong to them; for instance a piece of stuff from the bedclothes fell with a sound like a hard heavy body, whereas hard bodies fell with no sound at all. (Leaf, 1897, p. 324)

### *Movement of Objects Sometimes with Strange Trajectories*

This is one of the typical phenomena of poltergeists: Objects move without any human intervention and without any other apparent physical cause. William E. Cox had looked in detail at these movements during his comparative study in 1961 on 46 cases (Cox, 1961). He had already highlighted many features that we have completed below.

Objects move without human intervention and very often with non-ballistic trajectories showing efforts sometimes to avoid being caught as in Sumatra in 1903 (Grottendieck, 1906). In the English case of Swanland in 1849 (Myers, 1891), the witnesses said: "They deftly evaded all our stratagems to catch them."

Objects are able to bypass obstacles by making 90° turns as in Java in 1950 (Zorab, 1973), or staying suspended in the air as in Portland in 1909 (Gilbert, 1910) , or they land lightly like a feather as in Hartville in 1957 (Clarkson, 2011, pp. 202-204), sometimes without noise as in the pre-cited case of Swanland. In the case of Durweston in 1894 (Podmore, 1896), Frank Podmore recounted:

> I was looking at the door opening into the garden, it was wide open, leaving a space of 15 inches between it and the inner wall, when I saw coming from behind the door a quantity of little shells. They came round the door from a height of about 5 feet. They came one at a time, at intervals varying from half a minute to a minute. They came very slowly, and when they hit me I could hardly feel them. With the shells came two thimbles. They came so slowly that in the ordinary way they would have dropped long before they reached me. (Podmore, 1896, p. 91)

Very slow movements sometimes presented sudden accelerations as in Hartville aforementionned. According to other cases, some objects that were hurtling at high speed towards a person (with obvious fear of injury) suddenly barely touched that person as in Marcinelle in 1913 (Flammarion, 1923) and fell vertically, sometimes without bouncing as the laws of logistics would have predicted as in Los Angeles in 1974 (Rogo, 1979, pp. 112–123).

In the case of Bristol in 1761 (Gauld & Cornell, 1979, pp. 118–24), Henry Durbin, a direct witness, talked in his pamphlet "A narrative of extraordinary things," published in Bristol in 1800, about a glass placed on a chest of drawers:

> It rose gradually about a foot, perpendicularly from the drawers; then the glass seemed to stand, and thereupon inclined backwards, as if a hand had held it; it was then flung with violence about five feet and struck the nurse on the hip a hard blow. (Gauld & Cornell, 1979, p. 120)

In many cases, bedding is regularly thrown out of bed, as in Amherst in 1878 (Carrington, 1913; Hubbell, 1879, pp. 95–124). Sometimes, the objects are thrown with extreme precision, as in the case of Nottingham in 1990 (Cornell, 2002), where small stones passed at high speed through the same hole drilled by the first in a window (not achievable by manual throws or catapults). Also, they seem to move intelligently, avoiding other objects as in Miami in 1966 (Roll, 1971, 1973), where William G. Roll conducted some experiments and reported:

> Susy placed an alligator ashtray as a target object on the second shelf at the north end of Tier3, one of the most active areas in the room. Right in front of it, Julio himself put a cowbell that had been involved in earlier incidents. .... I was looking at Julio (the supposed agent), who was just about to reply to Miss Rambisz when the alligator ashtray crashed to the floor behind him. The cowbell remained in place, so the ashtray must have moved over or around it. I had Julio and the others under observation and had examined the target area myself. No one had been near it since my last examination. (Roll, 2004, pp. 134–5)

In some cases, objects' movements seem to be directed towards a particular area or person as in Indianapolis in 1962 (Roll, 1970, pp. 85-87; Roll, 2004, pp. 56–69).

### *Spontaneous Breaking, Cutting, Tearing, Extinguishing*

During these events, objects can be broken either because they are thrown and smash into a wall or on the ground, or due to a spontaneous breaking or explosion like the lamp bulbs in a lawyer's office in the city of Rosenheim in 1967 (Bender, 1969). In Baltimore in 1960 (Rogo, 1979, p. 257), a family is reported to have seen bottles burst or watched objects explode.

In some cases, clothes are torn as in Cadouin in 1940 (Zorab, 1964, case 19), as described by Goerges Zorab in

his comparative analysis of cases:

> One night all Josiane's clothing was torn to shreds within a few seconds. (Zorab, 1964, p. 122)

It has also happened that plants have been declared uprooted or moved in Bothell in 2012 (Linder, 2018) and candles extinguished as in Folkstone in 1918 (Roll, 2004) or in Tackley in 1905 (Gauld & Cornell, 1979, pp. 183–6), where two candles were extinguished simultaneously by a throw of two clods of earth.

### *Strong Energy Effects*

These phenomena can sometimes develop huge energy, such as moving large pieces of furniture or people, or generating a tremor of the whole house. In the case of Rosenheim aforementioned (Bender, 1969), a 200 kg storage cabinet moved away from the wall. In fact, the cabinet was found placed above the 4 mm of lino that surrounded the cabinet, so there has been also a levitating shift.

In the Sauchie case in 1960 (Gauld & Cornell, 1979, pp. 79–83), some furniture moved both in the family house (such as a heavy sideboard that moved outwards a distance of about 5 inches and returned to its original position, as seen by two witnesses) and at school in the presence of the teacher, Miss Stewart:

> Shortly afterwards, Miss Stewart, happening to look up, saw an empty desk behind Virginia, slowly rise bodily upwards about an inch, and settle down again gently a little way from its original position. Miss Stewart went to it immediately, and made sure that no strings, etc., had been attached to it. (Gauld & Cornell, 1979, p. 81)

In 1905, in Tackley aforementioned, two people witnessed the levitation of a bed on which one of them was lying. A dozen cases of the same type are listed in the study of Gauld and Cornell (1979), such as the Sandfeldt one in 1722 (Gauld & Cornell, 1979, Chapter 6), which benefited from 27 visual witnesses of the different phenomena observed and where a bed shook from bottom to top with two teenagers on it while their two mothers tried to hold it in place.

Also, in the Australian case in Adelong in 1889 (Healy & Cropper, 2014), the bed of Nellie, the little girl targeted by the poltergeist phenomena, rose from the floor with Nellie on it.

Sometimes (rarer) people are directly concerned. At Stratford in 1850 (Thuston, 1953, pp. 10–13) , H.B. Taylor reports:

> In my presence the eldest son was carried across the room by invisible hands and gently placed on the floor. (Carrington & Fodor, 1951, p. 87)

Finally, it is sometimes the house, the structure itself, that is the target of the phenomenon. For example, in Fougères-sur-Bièvre in 1913 (Flammarion, 1923), the whole village came to listen to the noises caused on the partitions of a house and even on the whole house as reported by the investigator M. Boutin de Blois:

> It is now a terrible tremor, which shakes the wall with a force that 10 men could not match. One evening, the noise was such that it was heard distinctly not only from neighboring buildings, but across the street, more than 60 meters away. The house was shaken up and down, the partitions vibrated with intensity,.. The noises that accompanied the tremendous vibrations of the house sounded like rumblings of distant thunder. (Flammarion, 1923, pp. 220–221)

The Anglo-Saxons also speak of "house hiving" because the vibrations sometimes resemble the sound of a beehive as in Midlands in 1967 (Stratton, 1914).

In the case of Andover in 1974 (Colvin, 2008), the banging sound could be heard by the investigator Barrie G. Colvin at least 50 yards from the alleyway and vibration was felt by 20 people on the outside surface of the wall, at ground level in the passageway.

### *Rain of Stones or Other Objects*

Until now we have talked about phenomena related to places (houses, workshops, etc.) and objects contained in those places. However, many poltergeist cases present "rain of objects" effects outside, and sometimes even apported inside while the various openings (doors, windows) are closed, as if they were teleported from the outside (see also next paragraph on teleportation). The objects are mostly stones, but all kinds of projectiles have been encountered, including pieces of tiles, rubble, clods of earth, excrement, or bolts that can be found in the environment of the house or the city.

Although in all these cases, the first thing was to test the intervention of pranksters using manual jets or catapults (slingshots), they could not be unmasked, and a number of factors reject this hypothesis because of the effects described previously: bizarre trajectories, extreme precision in some cases, sudden slowdown, and

people affected without being injured. Moreover, in some cases, the projectiles arrived vertically (like rain), which excludes the possibility that they were launched with a classic parabolic trajectory as reported in Roodeport in 1922 (Bayless, 1967). A well-known case in France is that of a house on the rue des Grès in 1849 (Flammarion, 1923), next to the Sorbonne, reported by Camille Flammarion in an extract from the court gazette of Paris of February 2, 1849:

> Where did these projectiles come from, consisting of cobblestones, fragments of demolished neighboring walls, even whole rubble stones which, by their weight and the distance from which they came, could not be thrown by the hands of a human being? This is what it was impossible to discover. Day and night surveillance was unsuccessfully carried out under the personal direction of the Commissioner of Police and competent persons. It was in vain that the head of security remained constantly on site. It was in vain that guard dogs were released every night in the nearby pens. Nothing could give the explanation of the phenomenon... (Flammarion, 1923, p. 18)

In the case of Neudorf in 1852 investigated by Professor Hans Bender, the latter reports:

> The day before we arrived, seven objects appeared in the kitchen in the space of sixteen minutes. They were observed by five witnesses, some of whom were not part of the household. I had the opportunity to reconstruct this event in great detail according to the descriptions which were collected absolutely independently, and without consultation, from the various witnesses. The objects seemed to shoot out of the wall at high speed. (Bender, 1969)

A more recent case in France in Arcachon in 1963 appraised by Robert Tocquet, IMI investigator, highlights the former Orthopedic Clinic of Arcachon, which from mid-May to early September 1963 was harassed by the projection of pebbles, pieces of rubble, and fragments of bricks whose origin has remained unknown (Tocquet & Cuenot, 1966).

In Lynwood in 1960 in California, in a used car park, 200 stones, some as big as a chicken's egg, followed by nuts and bolts, were thrown over two days; the projectiles arrived horizontally with unpredictable trajectories and sometimes at very high speeds. Thirty police officers searched for the culprit. A trial took place that eventually concluded that it was a "supernatural cause, a cosmic disturbance." (Rogo, 1979)

The cases of Mayanup, Boyup brook, and Pumphrey in southwestern Australia in 1957 (Healy & Cropper, 2014, Chapter 2) are impressive. Hundreds of people witnessed them. For example, at the Mayanup site, hundreds of stones and other objects (cans, potatoes, onions, pieces of steel) arrived from nowhere. Objects landed with a "plop" like a cork stopper, stopping dead in their tracks (some floated quietly to the ground, others changed direction by 90°, rose, and appeared suddenly in the air). Many stones and objects appeared inside. Outside, a stone the size of a pumpkin (15.9 kg) landed smoothly on a steel water tank.

In Narrabri in Northwest Australia in1900]:

> The stone-throwing took place in open daylight, while a party of police and civilians were watching and some mounted men were scouring around to a distance of 200 yards. The most extraordinary thing is that there are no stones in the vicinity, the soil being a level plain, and the nearest neighbor's house is over a mile away, with scrub intervening. To dispel suspicion all the party submitted to a search, and no stones were found on any of them. (Healy & Cropper, 2014, p. 209)

Another example is the case in Tucson, Arizona in 1983 reported by Scott Rogo, where stones rained down outside a house (especially on cars), causing damage of more than $7,000, and where people went out with helmets and came to greet visitors with shields. Several hunts in the surroundings of the house and three helicopter surveillance could not find any culprit (Rogo, 1987).

In the case of San Remo in 1986, in Australia, pebbles, instead of coming from above, levitated from the ground (Healy & Cropper, 2014, Chapter 6).

Finally, sometimes coins appear in a house. A recent experiment on a site in Mexico City in 2021 carried out by Ramses D'Leon's team made it possible to film the materialization of a coin inside a house thanks to six cameras installed inside the house. Overall, 60 coins seem to have "materialized" in this house, most of them old and no longer in use today (D'Leon, 2021).

In the case of South Shields in 2006, a dozen events concerned pieces that fell to the ground, sometimes hot (Ritson, 2020).

In South Wales in 1989, coins from 1912 disappeared and appeared, and rolled-up five-pound notes appeared in different places (making a total of 70 pounds) :

> It seems that these events appeared to be in response to repeated requests to 'Pete' by the four principal witnesses to bring 'some money'. A pen fell beside Jim when he had spoken of writing down the incidents, followed by a piece of headed notepaper, which on investigation turned out to have come, by unknown means, from the office premises on the floor above. Also, coins, most of which appeared to originate from a collection of pennies and halfpennies kept in the office. When Paul asked out loud for a sovereign, a Jubilee crown (which appeared to have come from a drawer in Jim and Ann's house) had dropped beside him. (Fontana,1991, p. 389)

### *Teleportation of Objects or People (Children)*

During these phenomena, objects seem to be able to pass through walls or roofs (like stones falling into houses), or as in Mayanup in 1957:

> Ronald Nicholson watched as stones simply appeared in mid-air, floated down and passed through the table to land on the floor below. (Healy & Cropper, 2014, p. 62)

In a case experienced by the author in France in Cessens in 1983, objects placed in the kitchen landed in the next room (such as a saucer for a coffee cup, an egg cup, a syringe) while the door between the two rooms was closed; stones and a fresh flower from the outside landed on the floor while all the openings to the outside were closed, and the five people present were searched and placed under mutual supervision (Dullin & Gaudiez, 2017).

In the case of Vachendorf in 1948, appraised by the IGPP Institute in Friburg-en-Brisgau, Professor Hans Bender reported his interview with witnesses:

> When we questioned her, the old woman was still deeply impressed by these ordeals. She told me that in the morning she had picked up the tools that were scattered throughout the room. She put them back in their box, and then sat down and said, "Now you'll stay here." She assured us that while she was still sitting on the box, the tools had again been scattered, one by one, in the various corners of the room. My colleague photographed her as she recounted this, and her expression seems to reflect consternation and astonishment. It was the first time that a seemingly credible testimony confronted me with the problem of the penetration of matter through matter, or the sudden appearance of objects from an enclosed place. (Bender, 1969)

In the case of Nickleim in 1968, Professor Bender himself had the following experience:

> My own observations also make this penetration of matter through matter likely. I had the whole family under control in the kitchen. My coat was hanging, not far away, in a small wardrobe. The tape recorder was on. At that moment, Brigitte heard the cat meowing outside the front door. His mother went to open it to him. She ran back and said, "Your coat is outside, carefully resting on the snow next to the stairs." It was very cold, and the door had remained continuously closed. We controlled the times, and according to the tape recorder, the mother was absent from the kitchen for exactly eight and a half seconds. We then controlled how long it would take to rush from the kitchen to the wardrobe, take the coat, walk down the stairs and put the garment on the snow. The most efficient person, after several attempts, manages to carry out these operations in twenty-one seconds. So it seemed that the cloak had been teleported (Bender, 1969).

In the case of Neudorf in 1952, nail rains were repeated 16 times in 45 minutes; the nails came from a locked cabinet in the basement (Bender, 1969).

At South Shields in 2006, this time it was a three-year-old child (Robert) who was moved from his upstairs bedroom to a closet in the parents' room (upstairs too), surrounded in a blanket (Ritson, 2020).

In 1919 in Coimbra in Portugal, two parents (Mr. and Mrs. Homem Christo), after attempting to shoot with a revolver at an invisible entity, found the cradle of their child empty. The mother fainted, and the father, after a search of the house, found the baby one floor below, completely stripped of his swaddling clothes, in the middle of a marble table (Lacombe-Frondoni, 1910).

In the Australian case of Tarcutta in 1949, a milking machine belonging to dairy farmer Laurence Wilkinson malfunctioned in a weird and dramatic way: its metal pulsator plates (368 g) repeatedly and inexplicably vanished and landed up to 250 yards from the shed. On landing, they either buried themselves in the ground or tore two-foot-long, one-inch-deep (60 x 2.5 cm) scars in the earth. More than 20 adults were witnesses. A technician from the company examined the milking machine and found it worked perfectly (Healy & Cropper, 2014, Chapter 3).

In the Indian case of Poona in 1920 reported in the form of a diary by Mrs Kohn and published by Harry Price:

> I went out, leaving on my table a tightly closed screw-top aluminum "safety" inkpot, containing a glass bottle of "Swan" ink. By this elaborate device I had hoped to surpass the cunning of the malicious "spirits." I returned to the house at precisely five pm. The very instant before I entered the house, there had been a crash. The "Swan" glass bottle had broken into innumerable pieces, which I saw scattered over the floor of my room; and the entire floor was a mass of freshly spilt ink. The aluminum outer inkpot was nowhere to be seen! I involuntarily looked upwards, as so many objects have been seen to descend from above during our experiences of the past few months. I called out jokingly: "I do hope the spirit will throw back the pot, it cost me one rupee eight annas!" No sooner had I finished speaking than I saw the missing inkpot appear in mid-air, at a distance of roughly six inches from the ceiling of my room. It fell on to the bed. I rushed to examine it, and found it as tightly screwed as when I had closed it that afternoon. So the bottle of ink had gone from a closed aluminum container. (Price & Kohn, 1930, p. 182)

In the same case, eggs got out one by one from a closed cupboard, with several witnesses :

> Saturday, June 30th. At eight a.m. my sister bought four dozen eggs, which were counted, and put in a basket in the food cupboard in the dining room. Almost immediately one egg shot in our direction from the direction of the (closed) cupboard, and smashed. We took the basket out of the cupboard, and ascertained that one egg was missing. I had no sooner gathered up the eggshell and washed the stain from the floor than a second egg came violently from the opposite direction, i.e. not as if coming from the cupboard, and smashed near the spot where the first egg had smashed. We again counted the remaining eggs, and ascertained that a second egg was missing. My sister D., whom we were closely observing, had not approached the cupboard during this time, and therefore could not possibly have had any hand in the mischief. At eleven a.m., two more eggs were broken in the same manner, and a fifth egg at seven p.m. (Price & Kohn, 1930, p. 182)

Also, three one-rupee pieces fell in rapid succession, apparently from near the celling. Mrs. Kohn examined her handbag, from which this amount was missing.

Finally, this case, among other astonishing events, talks about an event looking like a teleportation of Mrs Kohn's little nephew:

> He was playing in the compound. He chanced to be alone for a moment. After a few minutes he came into the house to my sister, looking dreadfully pale and frightened, and scarcely able to speak. He reported that he had felt himself lifted from the compound into the motorcar which stands in the shed. His eyes had been closed. When he had opened them, he found himself on the front seat of the car. When he came out of the shed, he had to pull aside the "chick" which forms the door to the shed. Though a few minutes before, he had been in the best of health, he was now very sick. He made 10 movements in an hour and a half. A doctor was called at once, who said that the child was completely physically exhausted. His pulse was almost gone, and his eyes were rolling. He was unable to eat for several days, and was quite thin and weak. As he had not previously eaten any over-ripe fruit, or anything else which, according to the doctor, could have produced the condition he was in, the doctor decided that his condition was due purely to the great fright he had felt (Price & Kohn, 1930 , p. 181).

### *Materialization of Liquids*

In materialization phenomena, liquids are also found, mainly water in the form of small puddles on the ground or on furniture, as in the case of Bothell House in 2012 (Linder, 2018).

Hans Bender also reported a case where several houses were concerned by the same phenomena in Scherfede in 1972 :

> Small puddles of water appeared first in the bathroom, then in the kitchen and in other rooms. Mechanics checked the water pipes and the heating tubes but could not find any leakage. Then humid spots showed on the walls, carpets became wet but underlying structures proved to be dry. Pools of water also appeared outside the house and several times the outer walls were wet. (Bender, 1974, p. 138)

But sometimes real rain falls from the ceiling, as with the Gardner family in the Rochdale case in 1995 (Clarkson, 2011), or water jets come out of the walls, as in the case of Laurence and Methuen in 1963 in Massachusetts :

> The family noticed a wet spot on the wall of their T.V. room. A few moments later, they heard a pop, like a firecracker, and water squirted from the wall. After several days, there was so much moisture in the house that the family had to abandon it for a night and move to the home of a relative. Five people have seen the strange phenomena. (Bayless, 1967, p. 99)

The entire apartment was checked by the fire department and the infrastructure services of the building without finding a plausible explanation.

Some cases show other liquids as in Ancona in Italy in 1903 in the house of the Attorney General Mr. Marracino, where all kinds of liquids (milk, wine, coffee) were constantly spread on the floor (Rogo, 1979, p. 184).

### *Paranormal Spontaneous Combustion (PSC)*

In the phenomenology of poltergeists, we also find spontaneous fires that can occur alone or with other phenomena already described.

The fire can be restricted to objects such as Bibles that burn from the inside, as in the case of Bothell in Seattle in 2012 :

> March 31st 2014: ...When I yanked opened the bedroom door there it was a bright orange light. There on the floor, lying inches from our doorway was a book. It was on fire. The light from the flames had illuminated the hallway, talk about an eerie feeling. Instinct alone forced me to lean down and close the book. That put the fire out. All we have left is a hallway filled with smoke and soot. Pages of ash are floating in the air all around me. Wait a minute? I know this book. This is my book. It's the Bible that went missing on October 10th, 2012. The one I left on the lamp table before going to bed. I leaned down to pick up the Bible, and I'm thinking to myself, oh my God, it's come back. And that's when I felt the bulge. Something was inside. I opened it up, and there within the burnt pages was a wooden cross. I know this cross. This was the cross that I bought from Amazon. I couldn't tell what more was frightening: The Bible returning on fire, or the cross inside, which was almost unrecognizable. This cross was not resting in the Bible on the night that it went missing. Hell no. I bought this cross online a few weeks ago. We hung it above our bed as a means of protection. (Linder, 2018, Chapter 14, p. 79)

The case of Calvin Truck and his family in the United States in Talladega in 1959 mobilized firefighters and police; 22 fires started in his cottage in a few days. Fires seemed to start from the ceiling, but sometimes flammable objects went up in flames, and even nonflammable objects burned. After the family moved to a new home, five unexplained blazes broke out on the first day of their occupation (Rogo, 1979, pp. 164–168).

In Clarendon in 1889, according to the report of Mr. Woodcock, in one day, eight spontaneous fires occurred, six inside the house, two outside, in broad daylight whilst the family and neighbors were in the house (Thurston, 1953, Chapter XV, pp. 162–170).

In the case of Amherst in 1878, investigated by Walter Hubbel and then Hereward Carrington, all the members of the household saw a lighted match fall from the ceiling to the bed, having come out of air. During the next 10 minutes, eight to 10 lighted matches fell on the bed and about the room, out of the air.

Three weeks later the invisible power took a dress belonging to Esther Cox that was hanging on a nail in the wall near the door and, after rolling it up and placing it under the bed before their eyes (six people), but so quickly they could not prevent the action, set it on fire (Carrington, 1913; Hubbell, 1879, pp. 95–124).

Alexander Aksakov also presented a case showing a strong component around a spontaneous fire:

> In the small village of Lipsky in 1853, there were a series of events as seen above; despite police investigation and surveillance and several exorcism rituals, perturbation went on. Then, a bed caught fire in the presence of two people (the captain and his wife), and finally, the roof caught fire. So, the captain and his family moved to another house in the village. But the events went on: pillows were sent out of bed, bottles of water were spilled, and the thatched roof began to burn with a first extinction successful. Then, a mattress was found full of smoking points. Finally, the complete roof caught fire, helped by a strong wind, while under the supervision of firemen. With this house, four other neighboring houses were burned completely. Because the damages concerned government property, there were five

> days of investigation, which were transferred to the civil court. Final expertise three years later concluded that there was no explanation (Leaf, 1897).

In Ipiranga, close to Sao Paulo in 1967, a family moved house three times and phenomena were still going on six years later. Any of the three women in the family could be regarded as the epicenter of the phenomena.

In the third house, where the family spent only one month in 1971, loud knocks were heard on the front door that they were sure could not have been made by anybody visible. Nora's husband's pajamas caught fire while he was asleep in them, and finally, the house, which was brand new, set itself on fire when there was nobody at home. Mother and Nora came back from shopping to find the house full of smoke; the fire had started in a bedroom and destroyed two wardrobes and an entire bed, the mattress, wooden frame, sheets, and all.

They remarked that the fire started with burn marks in the shape of small circles, the same as you get when you try to start a fire with a magnifying glass and the sun on a piece of paper. Surprisingly, some highly inflammable objects would not burn themselves out (Playfair, 1975).

This can be compared with the case aforementioned of Talladega in 1959, where, in contrast, nonflammable objects burned.

Guy Lyon Playfair reported on the cases of Enfield (Playfair, 2011) and Holloway (Playfair, 2011, pp. 188–189) in 1977 poltergeists during which, boxes spontaneously combusted inside drawers without igniting the matches, and also spontaneously extinguishing themselves.

Sometimes, a bedspread burns without harming the bedclothes beneath. In the Holloway case:

> Maurice Grosse, who did the investigation with Guy Lyon Playfair, examined a large burn mark on the wall of the bathroom, where the heat must have been intense, for a plastic beaker on a nearby shelf had been melted. He commented: They look more like radiation burns. It's as if a powerful heat source had passed by and then gone away. (Playfair, 2011, p. 189)

More recently, in Turkey, in Siirt, in 2012, the Toprak family moved through six residences, experiencing 300 fires (Healy & Cropper, 2014, pp. 269–274).

### *Self-operating Doors and Locks*

A typical phenomenon of poltergeists is the action on doors (doors open or slam violently). An example reported by Ernest Frantz to Camille Flammarion occurred in Strasbourg in 1855:

> Ernest Frantz, after hearing footsteps on the stairs over several days, decided to lay black wires across the steps. The following night, a huge oak door with a prison lock with a key weighing at least 250g opened violently while all the locks were closed. No wires were broken. (Flammarion, 1923, p. 226)

The locks (which represent more delicate/precise movements) are also concerned: the key turns itself in the lock, or people are sometimes simply locked outside their house as in the case aforementioned of Mr. Homem Christo in Cimbra in 1909. The door closed behind him with a turn of the key and a huge burst of laughter.

More recently, in 2006, in the case of South Shields already mentioned, one of the witnesses, Marianne, experienced several times a front door being unlocked and opened after being locked. Then another day:

> The cupboard door swung open sharply, so the other witness Marc (Marianne's husband) shut it, then it reopened and Marianne shut it and the same sequence repeated a number of times. There was a bolt on the door, but which was rarely used. Marc snapped the bolt shut, hoping that it would prevent the 'polt' playing its trick. No chance. The polt simply slid the bolt out and opened the door again, and continued to do so repeatedly until precisely 9pm. (Ritson, 2020, pp. 289–90)

On another day, in the presence of Marianne, Marc, and Mike (one of the investigators):

> As they descended the stairs, the patio doors, which had been locked, suddenly unlocked themselves. The handle arced downwards, as if pushed invisibly, and the right-hand (facing) door flew outwards violently. (Ritson, 2020, p. 327)

In the case of the Château of Calvados in 1875 reported by Camille Flammarion :

> Mrs. X, hearing noise in the room of the Reverend, went upstairs, followed by the latter. She heard stirring in the room, she moved forward her right hand to take the handle of the door and opened: before she touched it she saw the key that came off, quickly turning in the lock, and

> came to hit her on the left hand. The Reverend was a witness. The blow was strong enough that two days later, the shot was still sensitive and visible (Flammarion, 1923).

In the Enfield case:

> There was no wind or rain outside and no traffic to be heard. Then to his amazement Grosse saw the door of the lavatory open and close on its own. This happened three or four times. At the same time he felt a sudden cold breeze around his legs, and then around his head. (Playfair, 2011, p. 24)

### *Electrical Disturbances, Actions on Doorbells, Phones, Electronic Instruments*

The poltergeists of the nineteenth and early twentieth centuries were particularly fond of doorbells. Camille Flammarion had received 48 testimonials of automatically activated bells. We can mention in particular the case of Douai in 1907, which was the subject of a report to the Universal Society of Psychic Studies by M. Dhuique, a chemist. Overall, 300 witnesses were able to see the doorbell outside shaking, sometimes violently, without anyone around (Flammarion, 1923).

More recently, electrical phenomena have been reported, such as lights that turned off or on, electrical disturbances, and electrical appliances that stopped, or, on the contrary, started to work while they were broken. A very well-documented case already quoted is that of Rosenheim in 1967, followed by Hans Bender, where we have both electrical and phone disturbances:

- Electrical problems: current changes (up to 50 A) that should have blown the fuses; a backup power system to control the electrical flow was set up; the same deviations in amperage were observed.
- Phones: calls are made by themselves and increase the phone bill. Only one phone is left in operation. However, hundreds of calls are recorded, very often to the talking clock. The latter is sometimes called four to five times per minute (40 to 50 times in a row without anyone using the phone in the office).

In addition to Hans Bender of the IGPP, this case was appraised by:

- Herr Bruner, engineer in charge of the electrical energy supplier department in Rosenheim.
- Dr. F. Karger, physicist from the Max Planck Institute and Dr. G. Zicha from the Technical University of Munich, who performed electrical, magnetic, and sound analyses.
- The Criminal Police Department.
- The Municipal Fire Department.
- The Construction/Infrastructure Services.

Their conclusions were:

- No explanation for the 15 strong current deviations observed, and the sound effects are sometimes associated (bangs such as during flashes of discharges).
- No explanation for the mechanical effects: lamp explosions, blowing fuses (while the current is normal), paintings and lamps turning.
- No explanation for the effects on the phone that seem to be carried out by intelligently controlled forces (dixit).

In addition, 40 people witnessed these events (Bender, 1969).

Even more recently, phenomena related to electronic instruments and smartphones have been observed, such as televisions sometimes turning themselves off or on or switching channels to a program not listed in the schedule as in the cases aforementioned of Bothell in 2012 and South Shields in 2006. In the latter case, messages appeared on television about haunted house programs that were imminent. However, the television had not been programmed and the program in question was not found in the programming of the channels.

### *Light Effects, Appearances of People or Hands, Fog Effects, Feeling of Presence*

Some cases of poltergeists include incidents of pure hauntings such as appearances of people, shadows, glows, fog effects, or visible hands even in bright light.

For example, in the Lot et Garonne, in Port Sainte Marie, in 1922, a school and the associated house were the seat of many phenomena:

> One night, Mrs X, director of the school, had seen the curtains of her bed stirring, and then she thought she saw a hand enlarged, passing over the curtains. Fear seized her, she sat down on her bed, her lamp remaining lit, she still saw this hand, which eventually disappeared; but the curtains continued to shake violently. (Flammarion, 1923, p. 246)

More than 50 cases of hands being seen are reported in our case repository.

W.G. Roll reported a case at a house in Clayton, North Carolina, in 1962, occupied by Mrs. Pearl Howell and her two grown children, France (19) and Robert (22):

> Starting from June 1962, the family saw some lights flashing in the house even when all the lights were switched off.
>
> Charles Barden a freelance photographer interested in the case said that he had been at the house the previous evening and seen several flashes. They seemed faster than flashbulbs, he said, having more the speed of electronic flashes. He never saw anything like this before. Nothing had been found which could cause them. He and the police had decided that there would be no further publicity. They would welcome collaboration with parapsychologists.
>
> WG Roll did an investigation with his team, and could not find any explanation except the fact that the phenomenon seems connected to the presence of the daughter France. The family moved from the house by the end of July of the same year (Roll, 2004) Chapter 6 - p 70-87.

In the South Shields case already mentioned, there were several appearances, some of which were visible only to Robert (the three-year-old son of Marianne and Marc) in the form of his invisible companion Sunny. However, Mike and Marianne saw a three-dimensional figure 2 m high, of a rather threatening black color, that crossed the landing. Marianne screamed as soon as she saw it; the apparition continued and stopped at Mike's level and, despite having no eyes, gave the impression of staring at him with an icy look.

In September, Jackie and Mike's father outside the house saw a man trying to chat with Mike, Marc, and Marianne in the dining room. However, when they met, they claimed to have been with no one else.

In the case of South Wales already quoted, a synchronization between an appearance and an external event (throwing of stones) has been observed, the event happening just after the appearance, with one witness (Jim) and one other person present (Paul).

> Paul and Jim were working together on a piece of machinery, kneeling on the floor in the middle of the workshop and illuminated by full electric light. Paul once again caught sight of the apparition, and at once called to Jim to "Look behind you", whereupon the apparition vanished, and simultaneously a large stone struck with great force the machinery on which the two men were working, shaking both of them.
> (Fontana, 1992, p. 228)

According to the JSPR paper, Paul had two other appearances of the same type with a small boy aged about 12 years, 2.5 feet tall, wearing short trousers and a peaked school cap (with no face under the cap) and no outline hands or bare knees.

The case of Stans in 1860, with a detailed diary by Melchior Joller of his destructive experience, presents no fewer than 19 form appearances with often several witnesses (up to three at the same event): a black shape that flees, a white form, hands, a small grey cloud, a silhouette at the windows, a child, a strong feeling of presence, a touch followed by the vision of a form. Two of them caused a person seeing it alone to faint (his son, the maid) (Joller, 1862).

With regard to the Borley Rectory case in 1927, lights were seen inside the Rectory over several days by villagers and seven identified witnesses even though it was closed and unoccupied. Harry Price, in Appendix D of his book, recapitulating the phenomena (or alleged phenomena) with the names of those who observed them, listed 14 identified people who were witnesses of a nun figure in different forms. (Price, 1940, p. 178).

In the Lletsky case cited above :

> On January 8th my wife fainted on seeing a ball of light float from under her bed, small at first, but growing to the size of an India rubber ball as big as a soup plate. (Leaf, 1897, p. 324)

### *Heat of Objects*

"Materialized" objects are sometimes hot or even very hot, as reported by G. Vesan, a priest from Issime in 1909, who witnessed a stones shower with more than 50 other people in a chalet in the Alps:

> These 5 fallen stones were hot. I wanted to examine them all: these five, I had trouble holding them in hand and judging by the touch, they could have 45 to 50 degrees of heat. (Lecouteux, 2007, p. 148)

In Humpty Doo in 1998, Brendan Gowdie, a building maintenance expert, performed some interesting experiments. He first showed that if a person picked up and threw an object, his thermal imaging camera revealed warm spots corresponding to fingerprints on its surface. So he looked at some supposed "polt-propelled" objects (pistol cartridges, glass shards) to see if he could find

some kind of hints. But in fact, he found that they were uniformly warm all over (Healy & Cropper, 2014, Chapter 1, pp. 9–55).

Also, in aforementioned case of Sandfeldt in 1722, an iron ring from a plow wheel was flung at the feet of Haenell's two assistants, the gardener, and the watchman. When the gardener tried to pick it up, it burnt his glove. They carried it to Haenell at the manor house 1500 yards away, and it was still quite warm when he felt it (Gauld & Cornell, 1979, Chapter 6).

In some cases, water starting to boil in a container has been observed, as in the already quoted case of Amherst, in 1878.

### *Odors*

They can be perceived by several people and disappear instantly in an entire room or house. They can be unpleasant or pleasant, like in Nanterre in 2015, where, among other phenomena, smells of jasmine and canella were perceived every evening for one week in a high school lodge (Catala, 2019, pp. 160–163).

Anthony D. Cornell personally experienced this phenomenon in Histon in 1954, with 15 witnesses: The smell would move around the room and be localized in a vertical column from the floor to the ceiling, so concentrated that it could not be detected more than 2 feet away. It could also be like a concentrated ball moving between them under their noses (Cornell, 2002, p. 167-175).

### *Cold Drafts*

Cold drafts are perceived. Sometimes it's the whole room that seems cold, as in Aberdeen in 2002, where a bar's owner experienced a plummeting temperature combined with an object crashing and a passport photograph fluttering down from the ceiling (Holder, 2013, pp. 201–204), (case 123).

In some other cases the cold is perceived only in a specific area or on an individual as in Cambridge in 1967 (Rogo, 1979, pp. 261–269), where a matron of the school in which Matthew Manning was residing suddenly felt cold throughout her body and then saw a shower of pebbles falling from the ceiling.

Or as in the West Midlands in 1901 (Stratton, 1914), where the main occupant of a house, who was strongly skeptical at first, reported:

> Another night, at about the same spot in the hall, I felt an icy cold wind pass over my face. This was not an ordinary draught or anything like it. Of this, I am convinced. No doors or windows were open. (Stratton, 1914, p. 286)

## Interaction with the Phenomena

### *Religious Symbols as Targets*

Many objects moved or burned are related to religious symbols, such as in Sicily in 1890 (Roll, 2004), in Bothell in 2012 (Linder, 2018), or at Thanjavour in India in 1920 (Thurston, 1991), which happened at the center of a conversion from Hinduism to Catholicism.

### *Communication: Raps, Voices, Object Placements, Graffiti, Magic Slate, Text Messages*

Some poltergeists are interactive and appear to communicate with the people concerned. In extreme cases, it is with the voice, as in the case of Macon, where, according to Sieur François Perrault, a Huguenot minister at the time, discussions took place for two months between the villagers and an "entity" described by him as a demon in Macon in 1612 (Perrault, 1615).

Other cases mention the use of a code through knocks on walls or bedposts (e.g., one knock means yes, two shots mean no, or even the number of strokes is used to designate a letter of the alphabet). Some investigators have reported questioning the phenomenon mentally (they knew the answer without pronouncing it, and the answer was right). The physicist William Barrett, co-founder of the SPR and ASPR companies, investigated the Derrygonnelly case in 1877 (Barrett, 1911). First of all, he made sure that no one could be the cause of the beatings, checking that everyone in the room was clearly visible. Then he reported:

> Then I mentally asked it, no words being spoken, to knock a certain number of times, and it did so. To avoid any error or delusion on my part, I put my hands in the side pockets of my overcoat and asked it to knock the number of fingers I had open. It correctly did so. Then, with a different number of fingers open each time, the experiment was repeated four times in a row and four times in succession and four times I obtained absolutely the correct number of raps. (Barrett, 1911, p. 393)

In the case of a poltergeist in Fenland at Wisbech in 1957 (Cornell & Gauld, 1960), A.D. Cornell and Alan Gauld reported:

> All four witnesses agree that the knocks were responses to questions. The longer series came in a steady and quite rapid rhythm and when the

> answer was completed there was a silence until after the next question had been posed. Inspection of the records showed there were at least fifteen series of more than two raps. (Cornell & Gauld, 1960, p. 354)

Also, in Andover in 1974 (Colvin, 2008), Barie G. Colvin used a deck of 40 cards (printed with numbers from one to ten). In the room with him were 3 other people.

He drew cards in 3 scenarios:

- everyone could see the card drawn.
- only he could see the drawn card.
- no one could see the card drawn.

In each scenario, he asked the supposed external agency which named himself Eric Waters, to strike the number of strokes associated with the card. The answers were given by the poltergeist, with a stronger blow at the end, as if to mark the end of the sequence.

The 1st scenario gave five correct results out of 5. The 2nd gave seven correct results out of 7, then 8 out of 10, and 2 results with a deviation of 1 from the number on the card. The 3rd eight results out of 10

However, the information obtained from these communications, specifically concerning the identity of the supposed external intelligent entity, is, in most cases, false or incoherent. In the latter case, despite a very detailed search, no Eric Waters has been found in the area around Andover (even if Eric has announced that his bones were below the house, which was too difficult to check), and no Eric was found in the Waters genealogy back to the 17th century.

A.D. Cornell and Alan Gauld made the following remarks about the case Wisbech cited precedingly :

> The raps were ostensibly the work of an intelligence. In general they did not occur whilst we were asking questions; they came in an even tempo, at a rate of one second or somewhat faster and were appropriate in number to the questions asked, for example two for 'no' or eleven for 'November'. The intelligence, however, was crude and a search of local church records failed to confirm any of the information received. (Cornell & Gauld, 1960, p. 347)

In some cases, communication is opened with one person and blocked with another, as in Enfield in 1977 (Playfair, 2011), where communication was possible with Maurice Grosse but not with Guy L. Playfair.

Sometimes, the potential ESP competency of the phenomena looks quite surprising and is mixed with the anomalous events:

> Tracy Farrar experienced something even weirder. In her spare time, she makes jewelry from seashells, and on the day before the Humpty Doo visit, she'd spent hours collecting a particular type of small, brown shell at a Darwin beach. The next morning, while interviewing Kirsty (who she'd never met before), she watched in amazement as an identical shell sailed over her shoulder and landed on the table between them. Rachelle entered the room moments later to witness more shells falling, apparently from the ceiling.
>
> During her interview with Kirsty, Tracy received several electric shocks from her microphone – something that had never happened before and also saw the much-travelled TV remote lift off a table just a couple of feet to her right and fly up into the air.
>
> Before joining ABC, she was a science technician. Yet, as she told Frank Robson later, "I can't explain it in scientific or any other terms. But I know what I saw ... and it wasn't a hoax." (Healy & Cropper, 2014, p. 45)

With regard to writings, in the case of Stratford in 1850 :

> Dr. Phelps in fact averred that, when writing alone in his study he had for a minute turned his back to the table, and on resuming his task found written in large letters, the ink still wet, upon the sheet before him: 'Very nice paper and very nice ink for the devil.' (Thurston, 1953, p. 12)

Esther Cox in the case of Amherst in 1878 (Carrington, 1913; Hubbell, 1879, pp. 95–124) was the target of aggression with an inscription on a plaster wall (as performed with a big steel point) that was realized under the eyes of several witnesses: "Esther Cox, you are mine to kill." As reported by Hereward Carrington, at another moment with six people in the room, someone asked the polt, "How many people are in the room? Give a knock for each person in the room." Six distinct knocking sounds were instantly made by the power.

In the case of Borley rectory in 1927 (Price, 1940; Hastings, 1969), there were 19 witnesses of the writings globally. Mr. Glanville and his brother-in-law were also fortunate enough to witness the appearance of those famous wall markings:

> Mr Glanville and his brother in law, in August 1937, photographed one of the 'Marianne' messages. Although the house was locked and sealed, another mark appeared by the side of the message an hour or so later. The message was again photographed, and the new mark can be seen in one photograph but not in the other. (Price, 1940, p. 110)

In the case of Bothell House in 2012 (Linder, 2018), reported by its tenant Keith Linder, the walls are marked with the words "DIE KL" (Death to Keith Linder) with a drawing representing a man upside down and a pair of scissors planted at the level of the head and the number 666 (the symbol of the devil in some beliefs).

New technologies are widely used in the case of South Shields in 2006 (Ritson, 2020). In particular, messages such as "JUST GO NOW" are written on a Doodle tablet (graphic slate). These messages then sometimes erase themselves. A study of the writing using graphology and QDE (questioned document examination) has been conducted by different experts. Results pointed out some resemblance with the potential agent's writing (Marc), but no evidence of any hoax has been established (Houran et al., 2022).

At the same time, aggressive messages ) were received via SMS both inside and outside the house. Marianne and Marc (the two main witnesses) were sitting in the kitchen. Then:

> At 6:05 pm, Marianne's mobile phone beeped, indicating that she had a text message waiting to be received. This message had ostensibly been sent from Marc's phone, which was sitting – switched off – on the table between them. Marc's mobile phone showed no sign of life, and the screen was not illuminated. With great apprehension, Marianne opened the message. The words on the screen simply said, "Get you bitch".
>
> Marc, desperate to stop whatever was transpiring, opened the battery case on his phone and took the battery out. He then placed the battery, battery cover and phone back down on the table. Two minutes after the first text message, another one arrived. This one was far more sinister and, incredibly, it was being sent from Marc's mobile phone even though he'd removed the battery. Marianne opened the message, which said, "You're dead" (Ritson, 2020, p. 224).

Some messages took into account what had just been said by those present. Some calls were sent from the landline while the latter was only entitled to incoming calls because the bill had not been paid correctly to the operator, and the latter had blocked the line for outgoing calls.

In the Scottish case of Dundee in 2008 (Holder, 2013, case No. 131), the phrase "You must go" was scrawled on the wall, and "Gonna hurt you" appeared on a notepad.

Finally, in the context of communication, arrangements of objects to convey a message can be observed (in fact, more than 10% of cases present some arrangement of objects with or without a clear message, some looking simply neatly arranged).

In the case of Humpty Doo in 1998 (Healy & Cropper, 2014, Chapter 1, pp. 9–55), for example, small pebbles were used to form letters, the latter arranged in words

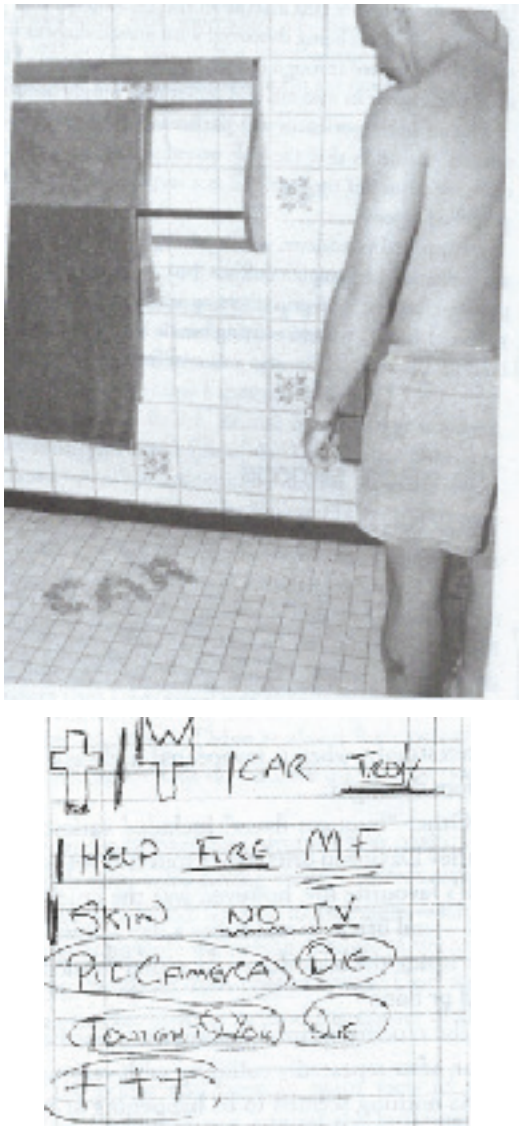

**Figure 3.** Small Pebbles Arrangement to Build Words in the Humpty Doo Case.

with a particular meaning or message (Figure 3). In the photo, we see the word "car," which echoed a car accident that had taken place. In the second photo, we see the different words that appeared: for example, PIG CAMERA when cameras had been installed everywhere by a television crew in the hope of recording some live events.

In the suburbs of Buenos Aires, in Villa Devoto in 1903 (Flammarion, 1923) , a zoologist at the Natural History Museum reported a poltergeist case that included an arrangement of three flower vases and a lamp delicately turned upside down and all placed in a cross in a locked room.

Eventually, in the case of Stratford in 1850 (Thuston, 1953, pp. 10–13), a scene with 11 characters built from clothes (obviously recovered from all over the house) constituting a kind of painting describing a scene of everyday life was built in a short time while no one had been able to enter the room concerned. The figures were arranged in a posture of devotion, and several Bibles were opened before them.

### *Poltergeist Agent*

The phenomena are often related to the presence of a particular person. Here are the facts concerning the human influence on poltergeists with some numbers extracted from our repository of 906 cases with detailed phenomenology:

- One agent can be clearly identified with local influence (when they are outside the place, nothing happens) (38% identified in our case repository)
- In many cases, there is a combination of the agent and the place (when the agent moved out of the place, nothing happened in their new place, and nothing had happened to them before in their previous place)
- However, sometimes the phenomenon follows them to new places, as in Bremen in 1965 (Bender, 1969)

- Following breaking events in a porcelain shop in Bremen, Hans Bender detected that Heiner, a 15-year-old apprentice, was the poltergeist agent. He helped him to find another job in Freiburg as an apprentice with an electrician. In March 1966, cables had to be installed in the basement of a new school. A large number of hooks had to be attached to concrete walls. For each hook, two 8 mm holes were drilled in the wall, and then the hook was secured with two screws in plastic dowels. The foreman noticed that the screws were taking play as soon as the hooks had just been installed. So Hans Bender organized an experiment with the staff of the Freiburg Institute and several workers. They installed two hooks in the concrete wall and made sure they were securely attached. They placed the boy one meter away from the wall, while observing the screws closely. Within two minutes, they were loose. None of them had seen them unscrew.

- In some rare cases, the identified agent displayed a capacity for extended influence, such as Matthew Manning, who reported that when he was in trouble in school, some poltergeist manifestations were triggered in his parents' home at a distance of several km (Manning, 1975). Also reported by Hans Bender in the Rosenheim case :

> Phenomena happened in the lawyer's office when Annemarie Sch., the supposed agent, was 1500 yards away (Bender, 1974, p. 128).

And in the Scherfede case in 1972 (Bender, 1974, pp. 138–141), in which eventually a little girl was identified as a water poltergeist agent for several houses, the last house was about 300 meters away from her residence.

In the Turkish case of Siirt in 2012 (Healy & Cropper, 2014, pp. 269–274), experienced by the Toprak family, fire is reported to have broken out when the supposed poltergeist agent (a young girl, 11 years old) was away from the house.

In the Miami case in 1966 (Roll, 1971, 1973), Jerome Eden, a TV reporter, despite the fact that Julio, the poltergeist agent, and all the other employees had left for the day, and so he was alone in the warehouse, experienced the crash of a bottle 36 feet from the place where he stood.

- 73% of the identified agents are aged less than 20, with historically more females (in our global case repository twice the number of males), but this difference has decreased recently.
- In other rare cases, some events happened in a place without any people in it and without a clearly identified agent, as in the case in New Jersey South in 1973 (Osis & McCormick, 1982), which concerned 24 witnesses, where, among other typical poltergeist phenomena, an alarm system, even after having been replaced three times, continued to be triggered regularly without any people inside or infraction detected.
- The group/family system (emotional) sometimes seems to be an agent: The poltergeist releases when some internal/underground problem is resolved or at least becomes explicit. In the Indianapolis case in 1962 (Roll, 1970, pp. 85-87; Roll, 2004, pp. 56–69), mother and grandmother seem to both be agents (but not the 12-year-old daughter who was often absent when

phenomena occurred). The phenomena were said to have stopped when the grandmother returned to her original country (Germany). The conclusion of William G. Roll on this case was that at the heart of the case was an interpersonal relationship characterized by frustration and anger that could not be adequately expressed within the relationship.

- In some cases, several agents are candidates, and it's difficult to draw a conclusion, as in the Turin case investigated by Dr. Lombroso in Turin in 1900 (Bozzano, 2000, pp. 206–209)
- It appears that some people are more inclined to experience them or trigger them.
- In many cases from our case repository, there was not enough information to identify an agent clearly (if we look at cases with a level of testimonial of five or higher on a scale of 10 we got 50% of agents identified instead of 40% with a level of four or less).
- Often, investigators inhibit the phenomena, but not always.
- Also, some people seem to be able to predict that some poltergeist event is on its way or be physically influenced by it.

For example, in the Enfield case aforementioned, Mrs. Harper (the main agent's mother) was able to predict some future act of the poltergeist or feel that something was on its way:

> Mrs Harper mentioned the strange headache she always felt just before something is going to happen. It varies, she told me. If the thing is hanging out I get a slight throbbing sensation, and if it is going too bad there's a short bright band across the front of my head. And then it'll sort of go. (Playfair, 2011, p. 72)

> Mrs Harper: It looks like it's using all our energy. First girls, then me. (Playfair, 2011, p. 81)

As Janet (main agent) bent a spoon without touching it with a hand on her eyes, Mrs. Harper reported that she saw it and felt that headache come and go just as it bent.

- In Malaysia at Kota Bahru in 2010 (Healy & Cropper, 2014, p. 261-266), 13-year-old Nurfatifa seemed to know when fires were starting and was usually the first to find and extinguish them. She said she "smelled them."
- In a Vietnam case in Ho Chi Minh in 2012 (Healy & Cropper, 2014, pp. 266-268), the poltergeist girl told Paul Cropper that she never sensed beforehand that the fires were about to occur; she said that she sometimes "felt electricity" when they were happening and felt quite tired afterward.

Some poltergeist agents also mentioned a kind of release after the polt events happened, as in Miami, in 1966, precedently cited, where, after a series of anomalous events, W.G. Roll asked Julio, the supposed agent, how he felt:

> "I feel good, I really miss the ghost..." –, he caught himself, "I mean... not the ghost, but I miss it when something doesn't happen." (Roll, 2004, p. 167)

The main current interpretation of this connection between poltergeist events and human, proposed initially by Willam G. Roll (Roll & Pratt, 1958), is that the subconscious part of the agent triggered by some mental state (such as frustration, anger, stress, etc.) would be able to produce all the phenomena described above and is connected to psychokinesis (PK). As there are multiple spontaneous events, it is called "RSPK" (recurrent spontaneous psychokinesis). The energy required would be provided either by the agent body or by some mechanism using, for example, the thermic energy of the room (Mattuck, 1977).

Another approach is the one proposed by Walter von Lucadou with :

- his Model of Pragmatic Information (MPI) (Lucadou, 1987) using systems theory to explain the process of development and extinction of the poltergeist phenomenon with elusiveness and a declining effect.
- and the generalized quantum theory (GQT) (Lucadou & Römer & Walach, 2007), using a metaphor of quantum mechanics theory to explain some kind of entanglement between the psyche of one person or a group of people and the matter.

This will not be developed further in this paper in order to maintain the focus on phenomenology.

### *External Agency*

In other cases, the place looks to be the only trigger (15% identified in our case repository).

For example, in Humpty Doo in 1998 (Healy & Cropper, 2014, pp. 9–55), Brendan Gowdie, a building maintenance expert who was called to do some thermal imaging on flying objects (see also "Heat of Objects" section), reported that anomalous things were happening even when he was alone in the house (no housemate, no other crew).

Different occupants experienced poltergeist phenomena and only in this place. It could be a public place like a bar such as in the case of Croydon in 1960 (Wilson, 1981, pp. 326–332), where three successive managers of the King's Cellar experienced the same poltergeist manifestations (glasses vibrating on the bar or shelves, bottles of wine sailing across the room to shatter against the wall, water springing from the toilet, cold drafts or rooms, spontaneous combustion, noises, apparitions, tills jammed).

In these latter cases, people (witnesses, investigators) experienced things that seemed to have been controlled by an external intelligence agency that was interacting, taking into account things that people in the place were saying or doing to make them afraid, playing tricks, or sending messages.

In some cases, there are apparitions of some kind combined with a strong feeling of presence, as in the case of Pittsburgh in 1971 (Pierce, 1973; Gauld & Cornell, 1979, pp. 356–9), where several witnesses saw a "white, misty figure" combined with a dog barking or a child laughing or giggling, and later seeing and experiencing a rocking chair moving by itself.

The current interpretations of this external agency, which will not be developed here neither, are:

- Elemental entity (such as "little people") typical in the Middle Age tradition (trickster), Djinn in some Middle Eastern folklore or Muslim tradition.
- Death people because some cases seem to be related to some dead people in a place (10% of our case repository of 906 cases), like in Pontefract in 1966 (Wilson, 1981). An excellent discussion can be found in Gauld and Cornell (1979, Chapter 8): Poltergeists and the dead, showing the difficulty of a definitive answer on this topic.
- Evil spirits (3% of our cases) as in Earling in 1928 (Rogo, 1979, pp. 205–209) (see also next section).
- Black magic (3% of our cases) as if people cast spells as in Tidworth in 1662 (Gauld & Cornell, 1979, pp. 43–64), or in some South American cases reported by Guy L. Playfair in Playfair (1975), or in some Australian cases (Healy & Cropper, 2014).

### *Aggression, Stigmata, "Possession"*

Although most poltergeists seem simply to try to scare people by, for example, throwing objects with relatively harmless impacts, some are more virulent and do not hesitate to conduct some real aggression. Many cases report people thrown out of their bed, such as, for example, children in the case aforementioned of Tidworth and Enfield. They give "violent slaps of wind" or real slaps, leaving fingerprints as in the case of Coimbra in 1919 (Lacombe-Frondoni, 1910), punches in the face as the case in Dundee in 2008 (Holder, 2013, case No. 131), and throw objects that sometimes cause injuries as in Issime in 1909 (Lecouteux, 2007, pp. 141–156).

They can injure or cause stigmata on the body. In the case of South Shields, nine people witnessed scratches/cuts appearing "live" on Marc's torso and back (he had had them several times, and each time, they disappeared a few hours later).

In regard to stigmata, we should mention the case of Eleanore Zugun in Bucharest in 1925 ( Mulacz, 1998), who, after repeated poltergeist episodes, had regular stigmata (180 validated) attributed to a demon. They were appraised by Harry Price in his laboratory in London (Price, 1945).

Also, in the case of Montfort-sur-Meu in 1938 (Tizané, 1977a, p. 153), reported by Police Commissioner Tizané, there is an extract from the PV 209 of April 26, 1938, by the gendarmerie brigade of Monfort-sur-Meu:

> On April 8, 1938, between 9 a.m. and 2 p.m., we had adjutant B., Constables M., J., H., and B. found that a few pins, about 8, had sunk into the black blouse worn by Mr. H. at Mrs. widow P.'s., without our knowledge, despite close and continuous surveillance, four times, and after serious control of the clothes capable of preventing any trickery. The pins appeared to us only when they were already stung in the clothes when M.H. had uttered a cry accusing the sting.

Other cases of pins are cited by Eleanore Zugun in the case of Bucarest in 1925 (Mulacz, 1998) and Esther Cox in the case of Amherst in 1898 (Carrington, 1913; Hubbell, 1879, pp. 95–124). Esther Cox also experienced some swelling of her body associated with periods of poltergeist activity (which returned to normal after).

In the Bristol case in 1761 (Gauld & Cornell, 1979, pp. 118–24), Henry Durbin conducted a scrutiny study and also carried out some experiments. Two girls were the target of assaults, such as stinging with a pin or biting: Dobby (eight) and Molly (13). Here is one of his experiments conducted on 15th February:

> I made Molly sit down in a chair in the middle of the parlour: I took a large pin, and marked it at the top with a pair of scissors. I put her hand across, and bid her not to move. I desired the above Gentlemen to watch her narrowly; none were in the room besides ourselves; I then put the marked pin in her pincushion in which the other pin was;

> I put the pincushion that hung at her side into her pocket hole, and pulled her clothes over it. As I moved one hand (my watch being in the other to see the time), she cried out she felt somewhat at her pincushion, and directly was pricked in the neck (her hands being still across). The identical pin that I marked was run through the neck of her shift, and stuck in her skin, crooked very curiously. It was not a minute from the time I put the pin in, to her being pricked in the neck: those two Gentlemen were witnesses of the Fact. We then marked four other pins, and I put them in her pincushion singly, as before; and all of them were crooked, and stuck in her neck. (Gauld & Cornell, 1979, p. 122)

In terms of assaults, here is another one:

> Jan 2, 1762, I went and met there Mr.-, and several other gentlemen. We went into a room called the George, and saw the children pinched with impressions of nails, and the children said they saw the hand that did it .... Dobby cried, the hand was about her sister's throat, and I saw the flesh at the side of her throat pushed in, whitish, as if done with fingers, though I saw none. Her face grew red and blackish presently, as if she was strangled; but without convulsion or contraction of the muscles. (Gauld & Cornell, 1979, p. 123)

In terms of biting:

> I saw Dobby wiping her hand in a towel, while I was talking to her, she cried out she was bitten in the neck. I looked and saw the mark of teeth, about eighteen, and wet with spittle? It was on the top part of the shoulder, close by the neck; therefore it was impossible for her to do it herself, as I was looking on all the time, and nobody was near her but myself. (Gauld & Cornell, 1979, p. 122)

As for cuts, here is Durbin's description of the sort of cuts that were inflicted:

> She (Molly) had above forty cuts on her arms, face and neck, with the blood dried on them, and very sore. They looked very black, and were all about two and a half inches long, and about the thickness of a shilling deep; the skin not jagged, but smooth, as if cut with a penknife. (Gauld & Cornell, 1979, p. 123)

In the case of Indianapolis in 1962 (Roll, 2004, pp. 85-87; Roll, 1972, pp. 56–69), William G. Roll did the investigation and was present for some of the 14 occasions of biting (five to six punctures in a 2.5 cm skin area) that concerned a mother (one occurrence) and a grandmother (13 occurrences) among 110 incidents of poltergeist phenomena (such as knocks, noises, movement of objects, and dragging of furniture).

In the case of Seyssuel in 1930, reported by René Sudre (Sudre, 1931), babies were injured (foreheads swollen and bleeding, faces scratched). No one was seen close to them capable of doing that.

However, to the best of our knowledge, there are no recorded cases of fatal injuries directly caused by a poltergeist (two cases of poisoning are mentioned: in the American Bell Witch case with the assassination of John Bell which was reported in Adams County in 1817 (Carrington & Fodor, 1951) and in the Brazilian case of Jaboticabal in 1965 (Linder, 2020) with the suicide of Maria at the center of events, without being certain in these two cases that the action was carried out directly by the poltergeist.

Cases resembling possessions are reported with the intervention of exorcists, who often have little impact or just a temporary one as in Ooty in 1897 (Flammarion,1923, p.348), or who, on the contrary, reinforce the phenomenon.

When a poltergeist seems connected with "possession phenomena," the physical effects reported are often very strong. In the case of Earling in Iowa in 1928 (Rogo, 1979, pp. 205–209), the chief witnesses were Theophilus Riesinger, a highly regarded priest and exorcist, and Father Steiger, in whose church the rites were performed.

The victim was a 42-year-old woman who had suffered from symptoms of demoniac possession since the age of 14, with an abhorrence of religious objects and blessed items. She was psychologically normal between the periods of the attacks. She presented multi-language understanding in front of several witnesses, used ESP-like competencies in discriminating between blessed and unblessed food, and finally took the personality of a demon that spoke through her in parallel with very strong physical effects, such as:

> With lightning speed the possessed dislodged herself from the bed and from the hands of her guards; and her body, carried through the air, landed high above the door of the room and clung to the wall with a tenacious grip. All present were struck with a trembling fear. Father Theophilus alone kept his peace. "Pull her down.

> She must be brought back to her place upon the bed."
>
> Real force had to be applied to her feet to bring her down from her high position on the wall. The mystery was that she could cling to the wall at all.
>
> Fearsome noises, howling, and voices broke out of the rectory when the exorcism was resumed. (Rogo, 1979, p. 207)

Scott Rogo pointed out that in some cases a conventional poltergeist outbreak gradually leads to the victim becoming, or at least believing themself to be, possessed. In these cases there is no doubt that a more conventional poltergeist was active long before the probable agent began to exhibit possession symptoms. A good example is given by the case in Georgetown in 1949 (Rogo, 1979, pp. 210–214) at the origin of the movie *The Exorcist*.

For this theme of possessions and hauntings, different complementary information and approaches can be found in Brittle (2013), Catala (2017), Rogo (1974), Oesterreich (1930), and Thurston (1953).

### *Response to the Challenge*

Sometimes, people become targets as a result of a form of challenge to the phenomenon (they insult it, claim that they are not afraid, or try to stop it in its way by trying to immobilize an object that has moved previously). For example, in Sicily in 1890 (Roll, 2004), a 25-year-old woman clasped a fan in her hands, which were themselves wedged between her knees, and challenged the "spirits" to take it. In a flash, the fan was twisted and smashed on her head.

> In the south of England at Durweston in 1894 (Podmore, 1896), a dirty boot from a garden was the subject of many movements, including in the house. Mr. Newman reports:
>
> After the boot was thrown out into the garden, I went out and put my foot on it and said, 'I defy anything to move this boot.' Just as I stepped off, it rose up behind me and knocked my hat off. There was no one behind me. The boot and the hat fell down together. (Podmore, 1896, p. 91)

In the case of Issime precedingly cited, within a group of 20 people, three young people were caught in the line of fire because they defied the phenomenon (rockfalls inside a chalet) and, despite their youthful ardor and self-esteem, they had to abandon this living room and take refuge in a nearby cottage (stones still reached them, however, and injured one of them with totally improbable zigzag trajectories).

In the case of South Wales in 1989 (Fontana, 1991; Fontana, 1992, pp. 225–231), one of the witnesses Ann, talking about "Pete," the surname they gave to the supposed polt entity, said "I got more active response from 'Pete' if I called out insults to him."

### *Marking and Returns*

Some have experimented with taking the stones that were sent to the house, marking them, and returning them in the same direction, e.g., in Tucson in 1983 (Rogo, 1987) and in Sumatra in 1903 (Grottendieck, 1906). A number of stones returned, while finding them in the surrounding wilderness around the house seemed very complex.

At Keninup in the case of Mayanup in 1957 (Healy & Cropper, 2014, Chapter 2) :

> ..the Hacks selected exactly 100 stones, coated them with silver paint, and scattered them widely over the general area. After just a few days they had all been pitched back into the Smith's camp. (Healy & Cropper, 2014, p. 86)

In another example in Italy at Boccioletto in 1908 :

> Stones regularly fall inside a closed room, each weighing in general 200 to 300 g, some more than 500 g and one 2 kg; each person was touched but without harm; one evening they marked the stones with coal and threw them away; some have returned (Bozzano, 2000, p. 204).

In some cases, stones are reported to have been launched and come back directly in a reciprocal way. A good example is given by the aforementioned case of South Wales in 1989 reported by the investigator David Fontana:

> The poltergeist appeared able to throw stones back when Jim threw them into the corner (which was at approximately 20 feet). On a number of occasions, I was present when this happened, the stones being thrown by Jim or Ann. In each case, I watched the flight of the stone thrown by the individual concerned, witnessed it land, then a moment later (with the stone thrower in my full view) heard the familiar clatter as it was 'returned' against the wall on either side of the workshop. Perhaps more significantly still, I found I was able to reproduce this phenomenon

**Table 2.** Effects of Outside Witnesses on Number or Severity of Occurrences (From Roll, 1977b, p. 54).

| Period | Total Number (N) | Uncertain (N) | Uncertain (%) | Enhances (N) | Enhances (%) | No Effect (N) | No Effect (%) | Total Enhance & No Effect (N) | Total Enhance & No effect (%) | Inhibits (N) | Inhibits (%) |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 1612-1849 | 16 | 1 | 6 | 7 | 44 | 8 | 50 | 15 | 94 | 0 | 0 |
| 1850-1899 | 24 | 2 | 8 | 3 | 12.5 | 17 | 71 | 20 | 83 | 2 | 8 |
| 1900-1949 | 34 | 4 | 12 | 9 | 26 | 26 | 76 | 31 | 91 | 3 | 9 |
| 1950-1974 | 31 | 3 | 10 | 8 | 26 | 16 | 52 | 24 | 77 | 4 | 13 |
| Totals | 105 | 9 | 9 | 23 | 22 | 67 | 64 | 90 | 86 | 6 | 6 |

myself, and did so on a number of separate occasions. (Fontana, 1991, p. 395)

### *Tendency of the Phenomenon to Hide Itself from Human Observers or a specific Human Observer*

Does the observer have an impact on the phenomena? Generally speaking, people see the result but not necessarily the start of the movement. Very often the phenomena occur at the moment when the people present turn their backs. However, even if very often witnesses reported that they didn't see the objects start moving, there are a number of cases where investigators/witnesses were able to do so.

In the case of Olive Hill in Kentucky in 1969, two parapsychologists (William G. Roll and John P. Stump, research associate at the PRF) saw several objects starting to move (Roll, 2004, pp. 148–157).

In the case of Neudorf in 1852, followed by Hans Bender, "the objects seemed to spring from the wall at high speed" (Bender, 1969). Also, in some cases with apport of water or spontaneous combustion, as seen above, people have seen the start of phenomena. Maybe we can say that, in many cases, people didn't see the start because they were not looking at that spot at that moment. In fact, William. G. Roll conducted an analysis of 105 cases and found that there was very low inhibition by the witnesses in the global number of events: 6% on average, 13% in the last period (1950–1974) (Table 2).

But if there was a very detailed observation trying to see the beginning of movements of objects, then, in 45% of the cases, some inhibition was observed (Table 3).

In many cases studied since these first results, it is reported that the phenomenon tends to hide, not be exposed, especially vis-à-vis the cameras. Several cases report the fact that camera batteries are sometimes drained. In the case of Mexico city in 2021 (D'Leon, 2021), the following events were noted by Ramses D'Leon's team:

- five of the six memory cards were unusable after installation during the first experiment.
- one of the cameras changed its angle without any intervention.
- another camera had a recording problem for 50 minutes.

Similar events are reported in the case of aforementioned of Bothell in 2012.

In the case of Neusatz in 1951 (Bender, 1969), on a German farm where phenomena of curtain theft occurred, Hans Bender's team set up a motion-triggered camera in the room where the curtains were theft and asked the police to seal the room. No new occurrence of the phenomenon was observed.

In Australia in the case of Mayanup precedingly cited, two film attempts (at the Boyup Brook and Pumphrey sites) by television crews failed (Tony Healy and Paul Cropper used the expression "Camera-shy spooks"): at Boyup Brook as soon as they arrived the phenomena stopped, only to resume after their departure, and at Pumphrey, the cameramen from the *Sunday Times* saw the stones but could not catch them on their film.

**Table 3.** Effect of Visual Observation by Outside Witnesses on Beginning of Movements of Objects (From Roll, 1997 May, p. 54).

| Period | Total Number (N) | Uncertain (N) | Uncertain (%) | No Effect (N) | No Effect (%) | Inhibits (N) | Inhibits (%) |
|---|---|---|---|---|---|---|---|
| 1612-1849 | 13 | 2 | 15 | 5 | 38 | 6 | 46 |
| 1850-1899 | 23 | 2 | 9 | 9 | 39 | 12 | 52 |
| 1900-1949 | 32 | 7 | 22 | 13 | 41 | 12 | 37.5 |
| 1950-1974 | 26 | 2 | 8 | 12 | 46 | 12 | 46 |
| Totals | 94 | 13 | 14 | 39 | 41 | 42 | 45 |

But filming does not appear to have been totally excluded since, in the case mentioned above in Mexico city, Ramses D'León seems to have managed to film in 2022 the apport of a coin, thanks to six cameras installed in the house 24 hours a day, seven days a week for 18 months (60 pieces apported globally).

Also, Hans Bender reported that the IGGP team succeeded in videotaping the sudden rotation of a painting in the Rosenheim case, and in the Pursruck case in 1970 (Bender, 1974, pp. 135–38), they succeeded in videotaping while the girls concerned were in their beds and the knockings were appearing in different intensities...

However, in some cases, it appears that there is inhibition of the phenomena by a specific observer (by example in the case of Enfield already mentioned, there was some inhibition of the phenomena when Guy L. Playfair was present and not with the presence of Maurice Grosse).

## Other Features Observed

### *Diurnal or Nocturnal*

Events can take place at night but also in broad daylight depending on the case.

### *Duration*

The duration of the phenomena can range from one day to some years. Sometimes it stops to restart again later as in Calvados, in 1875 (Flammarion, 1923). In our case repository (674 with a duration):

- 18% 1 month and less
- 39% 6 months and less
- 17.5% above 2 years

### *Focusing Effect*

Certain objects or areas are particularly affected. In his paper "Experimenting with poltergeists," William G. Roll highlighted that 66% of the 116 cases studied presented some focus objects (repeated incidents with the same or similar objects), 15.5% some focus areas (repeated incidents taking place in the same area such as a room or a shelf), and 10% both (Roll, 1977 b). Detailed results are presented in Table 4.

A detailed study using this focus feature can be found in the case of Miami precedingly cited, where beer mugs were regularly thrown from some specific shelves in a Florida novelty wholesale store (Roll & Pratt, 1971).

Tony Healer and Paul Cropper, in their Australian study, talk about "frequent flyers" as in the aforementioned case of Humpty Doo, where among other focus objects were a homemade bottle opener and a crucifix. The latter, almost every day, sometimes several times a day, disappeared from the top of a small bureau and, either immediately or somewhat later, dropped or crashed into a wall or the floor.

In the case of Holloway in 1977 (Playfair, 2011) pp. 188–9), a cooking book sailed regularly out of the shelf and lay open on the floor, always opened at the same page, this page containing a couple of recipes for luscious-looking cakes. One of these was "American Devil's Food" (which, according to the owners, was never used).

In the Charlottenburg case in 1929 (Gauld & Cornell, 1979, pp. 148–157), a furry ape doll hanging from a bar was regularly agitated (making dancing movements, all its limbs moving vigorously, and sometimes nodding its head) without any explanation.

### *Animal Impact*

Animals often behave strangely in the presence of the phenomenon. Either they demonstrate terror, like the horse terrified in its box and dogs avoiding some rooms in the case of West Midlands in 1901 (Stratton, 1914), or, on the contrary, they are completely indifferent, as in aforementioned case of South Wales in 1989. Sometimes, this second behavior follows the first, as in Mayanup in 1957 (cited earlier), where a kangaroo dog was terrified, howled, broke its chain, and ran away, and then for the two years that the events lasted, did not react at all.

**Table 4.** Objects and Areas Focusing Effects (Roll, 1977 b, p. 59).

| Period | Total number (N) | No Focus. (N) | No Focus. (%) | Object Focus. (N) | Object Focus. (%) | Area Focus. (N) | Area Focus. (%) | Object & Area Focus. (N) | Object & Area Focus. (%) | Total Cases With Focusing (N) | Total Cases With Focusing (%) |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 1612-1849 | 19 | 2 | 10.5 | 11 | 58 | 4 | 21 | 2 | 10.5 | 17 | 89 |
| 1850-1899 | 25 | 4 | 16 | 16 | 72 | 3 | 12 | 2 | 8 | 21 | 84 |
| 1900-1949 | 38 | 2 | 5 | 26 | 68 | 6 | 16 | 4 | 11 | 36 | 100 |
| 1950-1974 | 34 | 3 | 9 | 22 | 65 | 8 | 23.5 | 1 | 3 | 31 | 91 |
| Totals | 116 | 9 | 8 | 77 | 66 | 18 | 15.5 | 10 | 10 | 107 | 92 |

### *Performing Complex Tasks, Starting Complex Devices (Requiring Multiple Steps)*

The phenomenon can show structured intelligence (as previously presented in the object arrangement events), managing processes with multiple steps, including technical knowledge: for example, in the South Wales case, which occurred in a small mechanical engineering repair workshop with a related adjoined retailed shop situated in a suburban shopping street in South Wales:

> The engine of a powerful commercial petrol mower was found running in a fume-filled workshop on Paul's opening up the premises on a Monday morning. I later checked that this mower required no less than three separate operations in order to start it, the final one involving a pull-start achievable only by repeated brute physical force. The loudness of the engine ruled out the possibility of it having been accidentally left running, and the petrol in a full tank would have been insufficient to keep the motor operating for 36 hours from Saturday evening to Monday morning.
>
> A Rubik's cube, placed on a shelf in the workshop, was found regularly 'rearranged' overnight, and once moved to a different shelf. (Fontana, 1992, , p. 226)

In the case of New Jersey South in 1973, analyzed by Karlis Osis and Donna McCormick, where many electrical apparatus were triggered (alarm, light, music box), Mrs. Marge Byron and her daughter Denise Howards were alone in the shop at about 7 pm:

> They heard a loud noise in the workroom, and when they entered the room to check it out, they found the sewing machine operating by itself. Going over to turn it off, they discovered that the switch was already in the "off" position; yet the machine continued to operate. They had to pull out the plug to stop it. Mrs Byron told us that "it was very strange" – we left right away. We inspected the sewing machine, which has a triple switching safety mechanism: before it will operate, it has first to be switched on, then the appropriate button pushed for stitch density, and finally, the foot pedal depressed. The machine will not function if any one of these three switches is not activated. We were told that the machine had not been in use just prior to the incident, and no customer has ever been known to enter the workroom. Denise remarked: "Even if someone did go in there, how would they start [the machine] without using the switches?" There have been no reports of the machine malfunctioning since this incident. (Osis & McCormick, 1982, pp. 30–31)

### *Contagion*

The phenomenon sometimes seems to be given characteristics of contagion by people (Ritson, 2021) or supposed haunted objects, as in the case of the Annabelle doll (Brittle, 2013). It seems that the poltergeist cited precedingly of South Shields, which occurred in 2006, was able to temporarily infect some people who, directly or indirectly, had been in contact with the family at the center of the disorders. Keith Linder, at the center of the case of Bothell in 2012 (cited earlier), reports the same type of experiments. In the Australian case of Mayanup in 1957, already mentioned, two houses 700 m apart, belonging to two brothers, used by two families of employees with a family connection, experienced poltergeist phenomena one after the other. Then, another connection was reported as follows:

> On thing that makes the case uniquely interesting is the way the polt didn't simply focus on one particular family or residence, as is usually the case. At one stage it was pestering, simultaneously, the Smiths at 'Keninup', the Krakouers at 'Lynford Hill' and the Pennys 150 km away at 'Carabin'. Later, when Cyril Penny was again targeted at Borden, the Jannick was still operating at Mayanup, 160 km to the west. Later, as we have seen, it took a shine to young Harvey Dickson and moved to Boyup Brook. (Healy & Cropper, 2014, p. 83)

In the case of South Wales, where no agent could be established, Jim and Ann, and Paul and Yvonne experienced events outside the shops or in their home:

> Frequent telephone calls to Jim and Ann's house during the day and night (on one occasion every few minutes throughout an entire afternoon), but the line dead on answering. British telecoms engineers had been asked to check but had found no fault to account for this. (Fontana, 1991, p. 391)

And for Paul:

> On two occasions, however, the money arrived in a different manner, once when a 10-pound note was found pasted on the wet windscreen of Paul's car at the end of the afternoon, and once when five one-pound coins violently struck the front door of his house while he was crossing the darkened hallway. (In his account he reported the impression that they flung 'through' the door from outside, rather than simply rebounding from inside). (Fontana, 1992, p. 227)

A.D. Cornell reported in the case that he investigated in Histon in 1954, close to Cambridge, on an odor appearing in the rear part of his parents' house, which also appeared in one of the cars he and his friends used on their way back to Cambridge. The smell also materialized in the bedroom of his friend Betty, lodging in Southend, 70 miles away (Cornell, 2002, pp. 167-175).

Also, Guy L. Playfair produced a hypothesis about possible contagion between the Holloway case and Enfield case (similarity of events as the book opened at a page, and closeness in time) and also with Maurice Grosse, who experienced different typical poltergeist events in his own house and was investigating both cases.

There was also some form of contagion in the aforementioned case of Earling, as Father Steiger and some other priests, after having participated in the exorcism ritual, experienced in their homes gnawing sounds, pounding on the walls, eerie noises, and rooms shaking.

Further information on this subject can be found in Ritson (2021) and McCue (2022).

### End of a Poltergeist

Very often, a poltergeist seems to end itself (for no apparent reason). However, the departure of the identified "poltergeist agent" triggers the ending, at least in that place.

Example: In the case of Fougères-sur-Bièvre in 1913 (Flammarion, 1923), the departure of the grandson stops all phenomena.

Or the stopping may be thanks to the support of an organization that accompanies the agent or the "agent group", as in the advisory approaches of the CIRCEE in France or the IGPP in Germany. Typically, the approach is centered around the following:

- acceptance of the phenomena.
- the knowledge that these phenomena happen to other people, other groups.
- the acceptance that these phenomena may be related to a person's or group's own emotional context (they are no longer completely external to them).
- taking into account the poltergeist as a message in the life of the person or group.
- the verbalization of possible emotions (fear, frustration, guilt) not said until now.

Sometimes this verbalization/sharing is enough to reduce the phenomena, or even to stop them. The stopping can be done following a change in the family (indeed some poltergeists seem to be linked to a particular family situation and the departure of one of the members stops the phenomena). Sometimes the phenomena stop when the investigation begins. Sometimes it is a ritual that triggers the cessation of phenomena (at least temporarily).

Finally, the hypothesis of Darren Ritson in his books on the South Shields case (Ritson, 2020) and on contagions (Ritson, 2021) would be a possible "feeding" of the phenomenon by electrical/electronic systems (especially those left on standby at night). By stopping these systems, the poltergeist would have ceased its activity.

## DISCUSSION: LEVEL OF EVIDENCE AND PLAUSIBILITY

### Investigations and Testimonials

One could say that all these stories are myths, or legends. However, in the cases cited above, it can be seen that most were based on numerous simultaneous testimonies, had been the subject of a detailed investigation by different agencies (police, firefighters, infrastructure,..), and for some had been the subject of a detailed analysis by a parapsychologist with the publication of an article in a peer-reviewed journal (globally, 309 out of our 1250 cases have benefited from an investigation).

To give an example of investigation, close to Wisbech in Fenland, in 1957, Alan Gauld and A.D. Cornell conducted a scrutinized investigation on the Hannath Hall case with 12 visits, sometimes accompanied by other members of the SPR and CUSRP (Table 5). Raps were coming from the floor and walls without any explanation (Cornell & Gauld, 1960).

Some cases benefit from a large number of witnesses who have declared on their honor by notarial act the veracity of their testimony: for example the Sandfledt case in 1722 with 27 witnesses (Gauld & Cornell, 1979, Chapter 6).

### Levels of Testimonials and Details Increase over Time

Some cases were scored with a level of testimonies from 1 to 10 (which is based on answers to questions such

**Table 5.** Investigations Done for the Hannath Hall Case. ATTRIBUTION NEEDED

| Date of Visit | Investigators |
|---|---|
| 1957 Nov. 16-17th | A. D. Cornell, A. O. Gauld, J. M. Brotherton, D. J. Murray. |
| 1957 Nov. 21-22nd | A.D.C., A.O.G., D.J.M., I. Hacking, A. Hickling. |
| 1958 Feb. 6-7th | A.D.C., A.O.G. |
| 1958 Aug. 30th | A.D.C., A.O.G., Mrs T. Turner |
| 1958 Oct. 17-18th | A.D.C., A.O.G., Dr I. Fletcher. |
| 1958 Nov. 15-16th | A.D.C., A.O.G., J.M.B., D.J.M. |
| 1959 Apr. 24th | A.D.C., A.O.G., Hon. A. P. Leith. |
| 1959 Apr. 25-26th | A.D.C., A.O.G., Hon. A. P. Leith, Mr and Mrs R. Copley. |
| 1959 June 15th | A.D.C. |
| 1959 June 25th | A.D.C. |
| 1959 Sept. 24th | A.D.C., A.O.G. |
| 1960 Jan. 26th | A.D.C., A.O.G. |

as: Do we just have a second-hand testimony or direct experience? How many simultaneous witnesses of the events are there?) and a level of detail from 1 to 5 (from a low level of detail to a written day-to-day follow-up of all events).

These levels of testimony and detail have progressed over the different historical periods as investigations have developed further, as can be seen in the attached graph, which used the 608 cases about which we currently have information regarding testimonials and details (Figure 4). In recent periods, some cases have been the subject of an entire book (e.g., Enfield, Borley rectory, South Shields, Bothell house).

G.W. Roll, in his study based on 116 cases, had evaluated that 105 cases involved witnesses outside the family, including 73 with professional training (parapsychologists, police, government officials, doctors, scientists, lawyers, psychologists, clergy, teachers, naval officers, firefighters).

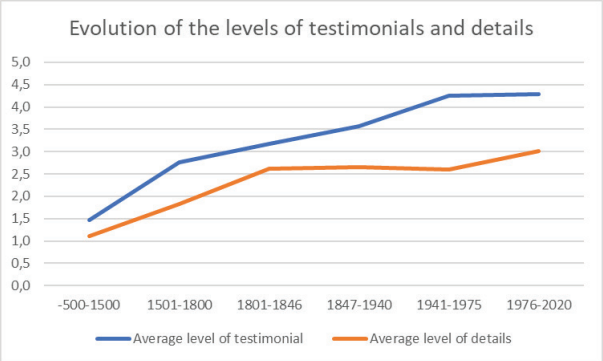


**Figure 4.** Evolution of the Levels of Testimonials and Details.

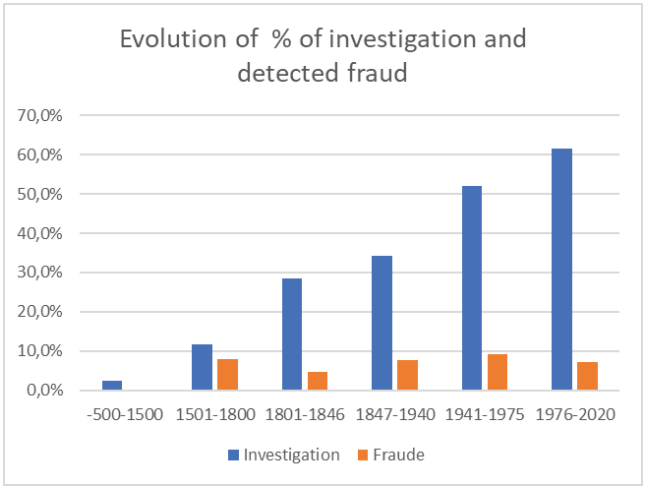


**Figure 5.** Evolution of Percentage of Investigation and Detected Fraud.

## Percentage of Fraud Detected and Level of Investigation

If a majority of cases had been fraudulent, further investigation should have highlighted more and more cases of fraud. This is not what is found in the graph in Figure 5. Indeed, the analysis highlights that the level of investigation of cases, which increased sharply between the 1500s and 2000s, had little impact on the level of fraud detected, which remains at 10% or less.

## Lawsuits, Insurance Reimbursements, and Property Tax Credits Induced by Poltergeists

Many cases have given rise to lawsuits such as that of Lynwood in 1960 (Rogo, 1979), cancellations of lease agreements such as that of Grès in 1849 (Flammarion, 1923), or at least gendarmerie minutes such as those published by Commissioner Tizané (Tizané, 1977b).

There is also a case of compensation for English army officer R.W. Jelf by his insurance following seven spontaneous fire starts in his house in the county of Sussex, which was unsuccessfully appraised by the fire brigade, the police, and the laboratories of Scotland Yard (Mail London Office, 1952).

A $4,000 property tax was apparently granted to an American citizen in the 1910s on the basis that his house was haunted, and therefore, drove away his successive tenants (Journal of the debates, politics, and litterers of 19th August 1912 in France).

All these elements contribute to giving some credibility to the existence of the phenomenon.

## At All Times, Distant, Unconnected People Have

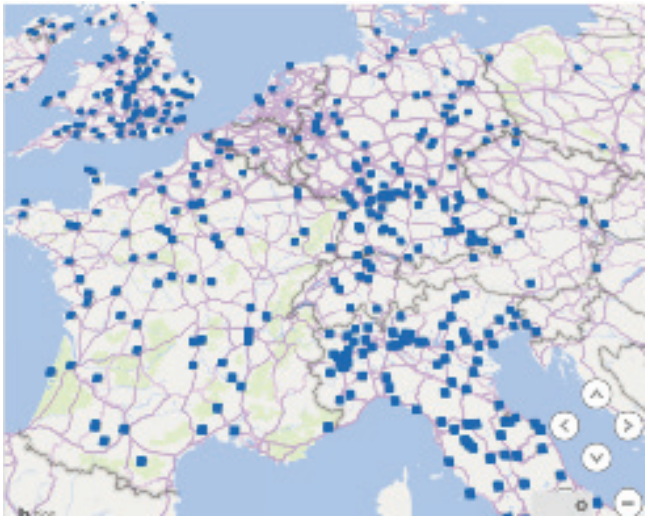

**Figure 6.** Map of Europe with the Distribution of the Cases Listed.

### Described the Same Phenomena

As presented in Dullin (2021) and Dullin (2022), there is similarity in the components of the poltergeist phenomena in different countries. Also, these events are reported in a wide distribution over the territories as shown in this map of Europe (Figure 6) with the distribution of the cases listed.

In the nineteenth century, for example, the means of communication were not those of today. Many cases are described in isolated dwellings, at a time when the inhabitants could not have known about the phenomena observed in other cases of poltergeists. So the similarity of description of the latter is an element of plausibility of the statements.

### Strong Validation of Bizarre Effects

One could say, "Very well, in regard to poltergeists, I am willing to give credit to types of events such as raps, objects moved or broken, and strange noises, but on the other hand, events such as movements of heavy cabinets, non-ballistic trajectories, and teleportation/apports do not seem credible to me."

To test this argument, an analysis of cases taking into account those with a level of testimony higher than four compared to those with four and below was conducted. If these "exotic" phenomena were chimeras, their percentage of citations in well-documented cases with strong testimonies should have decreased. In fact, the opposite is the case, as can be seen in the table below (Table 6).

For example, the phenomena of apports/materializations are cited in 31% of cases with the best testimonies, compared to 23% for those with a lower level of testimony. It's the same for the movement of large objects and non-ballistic trajectories (29% instead of 14%).

William G. Roll also produced an interesting argument pointing out that if you perform a fraud or trick (like a magician), you avoid doing the same trick twice, which is exactly the opposite of what happens in the focusing effect described previously.

## IMPLICATIONS AND APPLICATIONS

The repository of cases supporting this study is an ongoing project. So the few statistics presented could evolve to some extent. However, the main kind of events will stay the same.

Also, the multiple testimonies on strong cases (with a high level of testimonies) open the door to considering cases with a lower level of testimonies with the same kind of events. This could provide some details on the phenomenology, details that haven't appeared in the strong cases. The fact that some exotic features (such as teleportation and non-ballistic trajectories) are more reported in strong cases (with high testimonies) than weaker ones is good evidence of their importance.

Some reported details look so "authentic" (if people were making a story, how could they think to put this in their report or testimonial?) that they induce a degree of evidence on the whole case, even if the level of testimonial is low.

So, considering all these cases and the details could help to build a more global picture of the phenomenon.

**Table 6.** Percentage of Occurrences of the Different Categories of Poltergeist Events Compared to the Level of Testimonial of the Case.

| | No Cases | Raps % | Small Objects % | Large Objects % | Nonballistic Trajectory % | Apports % | Communication % |
|---|---|---|---|---|---|---|---|
| Level of Testimonal > 4 | 221 | 54 | 69 | 51 | 29 | 31 | 19 |
| Level of Testimonal < = 4 | 386 | 40 | 56 | 26 | 14 | 23 | 13 |

For example, we can foster events around the teleportation features, the non-ballistic trajectories, the spontaneous fires, or spontaneous water apparitions through multiple cases. This could provide more ideas based on facts to develop current and new hypotheses and new psychophysical models in order to make progress in the comprehension of this phenomenon.

## REFERENCES


Auerbach, L. (1986). *ESP, hauntings and poltergeists – A parapsychologist's handbook*. Warner Books.

Barrett, W. F. (1911). Poltergeists old and new. *Proceedings of the Society for Psychical Research, 25*, 381–416.

Barrett, W. F. (1918). *On the threshold of the unseen* (p. 40). E. P. Dutton & Co.

Batcheldor, K. J. (1966). Report on a case of table levitation and associated phenomena. *Journal of the Society for Psychical Research, 43*, 339–356.

Batcheldor, K. (1984). Contributions to the theory of pk induction from sitter-group work. *Journal of the American Society for Psychical Research, 78*, 105–122.

Bayless, R. (1967). *The enigma of the poltergeist*. Parker Publishing.

Bender, H. (1969). Poltergeist–presidential allocution. *Paper presented at the Parapsychological Association 30th Annual Convention, New York*.

Bender, H. (1974). Modern poltergeist research: A plea for an unprejudiced approach. In J. Beloff (Ed.), *New directions in parapsychology* (pp. 122-143). Elek Science.

Benoit, M. (2021). *In Tenebris*. Editions Payot.

Biondi, M., & Caratelli, G. (1993). La natura del poltergeist. *Quaderni di Parapsicologia, XXIV*, 12–36.

Bozzano, E. (2000). *Les phénomènes de hantise*. Editions Exergue. (Original work published in 1932).

Brittle, G. (2013). *The demonologist: the extraordinary career of Ed & Lorraine Warren*. Graymalkin Media.

Carrington, H. (1913). *Personal experiences in spiritualism*. T. W. Laurie Ltd.

Carrington, H., & Fodor, N. (1951). *Haunted people: The story of the poltergeist down the centuries*. E. P. Dutton & Co., Inc.

Catala, P. (2017). Possession et hantises. In J.-M. Brohm & G. Bertin (Eds.), *Possessions* (pp. 305–315). Cosmogone.

Catala, P. (2019). *Apparitions et maisons hantées: Mythes et réalités*. Editions de l'IMI.

Clarkson, M. (2011). *The poltergeist phenomena: an in-depth investigation into floating beds, smashing glass, and other unexplained disturbances*. The Career Press Inc.

Clément, M. (2020). *Hantises et apparitions: Un enquêteur témoigne*. JMG Editions.

Colvin, B. G. (2008). The Andover case: a responsive rapping poltergeist. *Journal of the Society for Psychical Research, 890*, 1–20.

Colvin, B. G. (2010). The acoustic properties of unexplained rapping sounds. *Journal of the Society for Psychical Research, 74*, 65–93.

Doyle, C. A. (2013). *A scandal in Bohemia*. Ebook designed by Chelsea Hana Cohen (Original work published 1891).

Cornell, A. D. (2002). *Investigating the paranormal*. Helix Press.

Cornell, A. D., & Gauld, A. (1960). A Fenland poltergeist. *Journal of the Society for Psychical Research, 40*, 348–363.

Cornell, A. D., & Gauld, A. (1961). The geophysical theory of poltergeists. *Journal of the Society for Psychical Research, 41*, 129–146.

Cox, W. E. (1961). Introductory comparative analysis of some poltergeist cases. *Journal of the American Society for Psychical Research, 55*, 47–72.

Delaplace, G. (2021). *Les intelligences particulières: enquête dans les maisons hantées*. Vues de l'esprit.

D'Leon, R. (2021, July 27). Coin-based apports: A methodological approach to study non-mediumistic recurrent physical anomalies. Paper presented at the SSE-PA Connexion Annual Convention 2021, Online Airmeet platform. https://www.youtube.com/watch?v=nySR-QIbR-A

Dullin, E. (2021, July 26). Poltergeist/RSPK quantitative and qualitative study from antiquity until now. Paper presented at the SSE-PA Connexion Annual Convention 2021, Online Airmeet platform. https://www.youtube.com/watch?v=8Exfi_t6Vpg

Dullin, E. (2022, June 20). Trying to validate and get some clues about poltergeist activities using quantitative and qualitative studies: An historical overview. Lecture presented at a SPR monthly event, Online Zoom platform. https://www.youtube.com/watch?v=D8f-HEajsm2o

Dullin, E. & Gaudiez, B. (2017). Témoignage. Nous avons vécu un poltergeist en Savoie l'été 1983. *Métapsychique, 1*, 118-121

Evrard, R. (2019). The Amnéville RSPK case: An illustration of social elusiveness. In G. Mayer (Ed.), *N equals 1: Single case studies in anomalistics*. (pp. 377-393) Lit Verlag.

Fach, W. (2023, August 4). Research with the questionnaire on the phenomenology of exceptional experiences (ExE): from PAgE-R to PExE-II. Conference presented at the Parapsychological Association 65th annual convention, Oslo.

Felton, D. (1999). *Haunted Greece and Rome: Ghost stories from classical antiquity*. University of Texas Press.

Flammarion, C. (1923). *Les maisons hantées: En marge de la mort et de son mystère*. Editions Flammarion.

Fontana, D. (1991). A responsive poltergeist: A case from South Wales. *Journal of the Society for Psychical Research, 57*, 385–401.

Fontana, D. (1992). The responsive South Wales poltergeist: a follow-up report. *Journal of the Society for Psychical Research, 58*, 225–231.

Fraser, J. (2020). *Poltergeist: A new investigation into destructive haunting*. 6th Books.

French, C. C., Haque, U., Bunton-Stasyshyn, R., & Davis, R. (2009). The "Haunt" project: An attempt to build a "haunted" room by manipulating complex electromagnetic fields and infrasound. *Cortex, 45*, 619–629. https://doi.org/10.1016/j.cortex.2007.10.011

Gauld, A., & Cornell, A. D. (1979). *Poltergeists*. White Crow Books.

Gilbert, J.A. (1910). The Elwin March case of poltergeist. *Journal of the American Society for Psychical Research, 4*, 465-524 ; 561-636.

Goss, M. (1979). *Poltergeists: An annotated bibliography of works in English, circa 1880–1975*. Scarecrow Press, Inc.

Grottendieck, W. G. (1906). A poltergeist case. *Journal of the Society for Psychical Research, 12,* 260–6, 278–84, 294–7, 328–332.

Hastings, R. J. (1969). An examination of the 'Borley report'. *Proceedings of the Society for Psychical Research, 55, 226-338*

Healy, T., & Cropper, P. (2014). *Australian poltergeist*. Brio Books.

Holder, G. (2013). *Poltergeist over Scotland*. The History Press.

Houran, J., & Lange, R. (2001). *Hauntings and poltergeists: Multidisciplinary perspectives*. McFarland.

Houran, J., Laythe, B., Lange, R., Hanks, M., & Ironside, R. (2023). Immersive study of Gestalt variables in uncanny geographies. *Journal of the Society for Psychical Research, 87*, 65–100.

Houran, J., Little, C., Laythe, B., & Ritson, D. (2022). Uncharted features and dynamics of the South Shields poltergeist. *Journal of the Society for Psychical Research, 86*, 129–164.

Hubbell, W. (1879). *The great Amherst mystery: A true narrative of the supernatural*. (Reprint, 2016). CreateSpace Independent Publishing Platform.

Huesmann, M., & Schriever, F. (2022). Wanted: The poltergeist. Description and discussion of a collection of 54 RSPK reports of the years 1947–1986, kept at the Friburg Institute for Frontier Areas of Psychology and Mental Health [1989]. *Journal of Anomalistics, 22*, 76–135. https://doi.org/10.23793/ZFA.2022.76

Joller, M. (1862). *Depiction of self-experienced mystical apparitions*. Personal pamphlet.

Lacombe-Frondoni, M. (1910) . La Curieuse Histoire d'une Maison hantée en Portugal. *Annales des Sciences psychiques, v20, Mars*, 90–92.

Lambert, G. W. (1955). Poltergeists: A physical theory. *Journal of the Society for Psychical Research, 38*, 49–71.

Leaf, W. (1897). Review of "The precursors of spiritism" from Alex Aksakoff. *Proceedings of the Society for Psychical Research, 12*, 319–330.

Lecouteux, C. (2007). *La maison hantée: Histoire des poltergeists*. Editions Imago.

Linder, K. (2018). *The Bothell house: Poltergeist of Washington State 2.0*. CreateSpace.

Linder, K. (2020). *Poltergeist: The night side of physics*. Paul Ryan.

Locher, T., & Lauper, G. (1977). *Schweizer spuk und psychokinese*. Aurum Verlag.

Lucadou, W. v. (1987). The model of pragmatic information (MPI). *Proceedings of the Parapsychological Association 30th Annual Convention*, 236–254.

Lucadou, W. v., Römer, H., & Walach, H. (2007). Synchronistic phenomena as entanglement correlations in generalized quantum theory. *Journal of Consciousness Studies, 14*, 50–74.

Lucadou, W. v. (2011). Complex environmental reactions, as a new concept to describe spontaneous "paranormal" experiences. *Axiomathes, 21*, 263–285. https://doi.org/10.1007/s10516-010-9138-4

Mail London Office. (1952, July 12). Insurance paid for noisy spook's fire damage. *The Mail (Adelaide, South Australia)*

Manning, M. (1975). *D'où me viennent ces pouvoirs ? Le récit extraordinaire d'un phénomène paranormal* (French translation of *The link*). Albin Michel.

Mattuck, R. D. (1977). Random fluctuation theory of psychokinesis: Thermal noise model. In J. Morris, W. Roll, & R. Morris (Eds.), *Research in parapsychology 1976* (pp. 191-195). Scarecrow Press.

McCue, P. A. (2022). Paranormal contagion: A potential hazard for case investigators. *Journal of the Society for Psychical Research, 86*, 193–208.

Mirville, J. E. (1963). *Des esprits et de leurs manifestations diverses*. H. Vrayet de Surcy.

Montalembert, A. (1580). *Wonderful story of Sister Allis de Thesieux*. Gallica-BnF.

Moser, F. (1977). *Spuk, ein rätsel der Menscheit*. Walter-Verlag.

Mulacz, P. (1998). Eleonore Zugun - The re-evaluation of an historic rspk case. *Proceeding of presented papers*

*at the Parapsychological Association Conference,1998,* 94-96

Myers, F. W. H. (1891). On alleged movements of objects, without contact, occurring not in the presence of a paid medium. *Proceedings of the Society for Psychical Research, 7, 1982, 384-94*

Oesterreich, T. K. (1930). *Possession: Demoniacal and other among primitive races, in antiquity, the middle ages, and modern times*. Kegan Paul, Trench, Trubner & Co., Ltd.

Osis, K., & McCormick, D. (1982). A poltergeist case without an identifiable living agent. *Journal of the American Society for Psychical Research, 76*, 23–51.

Owen, A. R. G. (1964). *Can we explain the poltergeist?* Garrett Publications.

Owen, A. R. G., & Sparrow, M. (1974). Generation of paranormal phenomena in connection with an imaginary communicator. *Journal of the New Horizon Research Foundation, 1*, 6–13.

Perrault, F. (1615). *L'antidémon de Mascon ou la relation pure et simple des principales choses qui ont été faites & dites par un démon il y'a quelques années dans la ville de Mascon en la maison du Sr. Perreaud* . Gallica-BnF.

Pierce, H. W. (1973). RSPK phenomena observed independently by two families. *Journal of the American Society for Psychical Research, 67*, 86-101.

Piper, O. (1917). *Der spuk: 250 geschehnisse aller arten und zeiten aus der welt des uebersinnlichen*. Sarastro Gmbh.

Playfair, G. L. (1975). *The flying cow: Exploring the psychic world of Brazil*. White Crow Books.

Playfair, G. L. (2011). *This house is haunted: The amazing story of the Enfield poltergeist*. White Crow Books (Original work published 1980).

Podmore, F. (1896, April 24). Poltergeists, case VII: The Durweston case. *Proceedings of the Society for Psychical Research*, *12*, 90–95.

Price, H., & Kohn, H. (1930). An Indian poltergeist. *Journal of the American Society for Psychical Research, 24*, 180-186.

Price, H. (1940). *Borley rectory: The most haunted house in England. Ten years of investigation of Borley rectory*. P. Wolfe.

Price, H. (1945). *Poltergeist over England: Three centuries of mischievous ghosts*. Country Life Ltd.

Puhle, A. (1999). Ghosts, apparitions and poltergeist incidents in Germany between 1700 and 1900. *Journal of the Society for Psychical Research, 63*, 292–305.

Puhle, A. (2001). Learning from historical cases: Six selected poltergeist cases from the 1700s in Germany. *European Journal of Parapsychology, 16*, 61–72.

Puls. (1900). *Spuk-Geschichten: Der Spuk von Resau*. self-published.

Rabeyron, T. (2010). Psychodynamic and cognitive approach to exceptional experiences *[Doctoral dissertation, Université Lumière de Lyon & University of Edinburgh].*

Ritson, D. W. (2020). *The South Shields poltergeist*. History Press Limited.

Ritson, D. W. (2021). *Poltergeist parallels and contagion*. White Crow Books.

Rogo, S. (1974). Psychotherapy and the poltergeist. *Journal of the Society for Psychical Research, 47*, 433–446.

Rogo, S. (1979). *The poltergeist experience*. Penguin Books.

Rogo, S. (1987). A case of mysterious stone-throwing in Arizona. *Journal of the Society for Psychical Research, 54*, 16-37.

Roll, W.G. (1970). Poltergeist phenomena and interpersonal relations. *Journal of the American Society for Psychical Research, 64*, 66-99.

Roll, W. G. (1977a). Poltergeist. In B. B. Wolman (Ed.), *Handbook of parapsychology*, (pp. 382–413). Van Nostrand.

Roll, W. G. (1977b). Experimenting with the poltergeist. *European Journal of Parapsychology, 1*, 47-71.

Roll, W. G. (2004). *The poltergeist*. Paraview. (Original work published 1972)

Roll, W. G., Burdick, D., & Joines, W. T. (1973). Radial and tangential forces in the Miami poltergeist. *Journal of the American Society for Psychical Research, 67*, 267–281.

Roll, W. G., & Pratt, J. G. (1958). The Seaford disturbances. *Journal of Parapsychology, 22*, 79–124.

Roll, W. G., & Pratt, J. G. (1971). The Miami disturbances. *Journal of the American Society for Psychical Research, 65*, 409–454.

Schrenck-Notzing, A. v. (1921). Der spuk in Hopfgarten: eine gerichtliche feststellung telekinetischer phänomene. *Psychische Studien, 48*, 529–552.

Sitwell, S. (1988). *Poltergeists: Fact or fancy*. Dorset Press.

Stratton, F. J. M. (1914). The haunting of ---- house. *Journal of the Society for Psychical Research, 16*, 274–298.

Sudre, R. (1931). A case of Thorybism in France. *Journal of the American Society for Psychical Research, 25*, 24–29.

Thurston, H. (1953). *Ghosts and poltergeists*. Burns Oates.

Tizané, E. (1977a). *L'hôte inconnu dans le crime sans cause*. Editions Tchou.

Tizané, E. (1977b). *Le mystère des maisons hantées*. Editions Tchou.

Tocquet. R., & Cuenot, A. (1966). Jets de pierres, hantise ou simulation? *Revue Métapsychique, 2*, 75-96.

Ventola, A., Houran, J., Laythe, B., Storm, L., Parra, A., Dixon, J., & Kruth, J. G. (2019). A transliminal 'dis-ease' model of "poltergeist agents". *Journal of the Society for Psychical Research, 83*, 144–171.

Volmar, F. A. (1969). *Berner spuk und mysteriöses aus dem*

*Wallis*. Francke Verlag.
Williams, B. (2019). Psychological aspects in poltergeist cases. *Psi encyclopedia*. The Society for Psychical Research. https://psi-encyclopedia.spr.ac.uk/articles/psychological-aspects-poltergeist-cases
Wilson, C. (1981). *Poltergeist! A study in destructive haunting*. Caxton Editions.
Zorab, G. (1964). A further comparative analysis of some poltergeist phenomena: Cases from continental Europe. *Journal of the American Society for Psychical Research, 58*, 105–127.
Zorab, G. (1973). The Siteoebondo poltergeist : a firsthand account written soon after the events – the Sarabaja case (east Java, 1950) - *Journal of the American Society for Psychical Research, 67*, 401-403.

## APPENDIX A

### List of the 105 Cases Cited and Their Sources

Aberdeen, 2002: (Holder, 2013) case 123, pp. 201–204
Adams County, 1817: (Carrington & Fodor, 1951)
Adelong, 1889: (Healy & Cropper, 2014)
Amherst, 1878: (Carrington, 1913; Hubbell, 1879, pp. 95–124)
Ancona, 1903: (Rogo, 1979, p. 184)
Andover, 1974 : (Colvin, 2008).
Arcachon, 1963: (Tocquet & Cuenot, 1966)
Baltimore, 1960: (Rogo, 1979, p. 257)
Beuvry, 1907: (Flammarion, 1923, p. 246)
Boccioletto, 1908: (Bozzano, 2000, p. 204)
Borley, 1927: (Price, 1940; Hastings, 1969)
Bothell 2012: (Linder, 2018)
Bremen, 1965: (Bender, 1969)
Bristol, 1761: (Gauld & Cornell, 1979, pp. 118–24)
Bucarest, 1925: (Mulacz, 1998)
Cadouin, 1940: (Zorab, 1964, case 19)
Calvados, 1875: (Flammarion, 1923)
Cambridge, 1967: (Rogo, 1979, pp. 261–269)
Cessens, 1983: (Dullin & Cuenot, 2017)
Charlottenburg, 1929: (Gauld & Cornell, 1979, pp. 148–157)
Clayton, 1962 : (Roll, 2004, pp. 70-87)
Clarendon, 1889: (Thurston, 1953, Chapter XV, pp. 162–170)
Coimbra, 1919: (Lacombe-Frondoni, 1910)
Compiègne, 2011: (Clément, 2020, pp. 54–67)
Croydon, 1960: (Wilson, 1981, pp. 326–332)
Derrigonnely, 1877: (Barrett, 1911)
Dortmund, 1713: (Puhle, 2001)
Douai, 1907: (Flammarion, 1923)
Dundee, 2008: (Holder, 2013, case No. 131)
Durweston, 1894: (Podmore, 1896)
Earling, 1928: (Rogo, 1979, pp. 205–209)
Enfield, 1977: (Playfair, 2011)
Folkstone, 1918: (Roll, 1972)
Fougères-sur-Bièvre, 1913: (Flammarion, 1923)
Georgetown, 1949: (Rogo, 1979, pp. 210–214)
Grès, 1849: (Flammarion, 1923)
Hartville, 1957: (Clarkson, 2011, pp. 202-204)
Histon, 1954: (Cornell, 2002, p. 167-175)
Ho Chi Minh, 2012 : (Healy & Cropper, 2014, pp. 266-268)
Holloway, 1977 : (Playfair, 1980, pp. 188–9)
Humpty Doo, 1998: (Healy & Cropper, 2014, pp. 9–55)
Indianapolis, 1962: (Roll, 2004, pp. 85-87; Roll, 1972, pp. 56–69)
Ipiranga, 1967: (Playfair, 1975)
Issime, 1909: (Lecouteux, 2007, pp. 141–156)
Jaboticabal, 1965: (Linder, 2020)
Java, 1950: (Zorab, 1973)
Kingstown, 1876: (Barrett, 1918)
Kota Bahru in 2010 (Healy & Cropper, 2014, p. 261-266),
Laurence and Methuen, 1963: (Bayless, 1967, pp. 99–107)
Lipsky, 1853: (Leaf, 1897)
Lletsky, 1870: (Leaf, 1897)
Los Angeles, 1974: (Rogo, 1979, pp. 112–123)
Lynwood, 1960: (Rogo, 1979)
Lyon, 2007: (Rabeyron, 2010)
Lyon, 1525: (Montalembert, 1580)
Macon, 1612: (Perrault, 1615)
Marcinelle, 1913: (Flammarion, 1923)
Mayanup, 1957: (Healy & Cropper, 2014, Chapter 2)
Mexico City, 2021: (D'Leon, 2021)
Miami 1966: (Roll, 1971, 1973)
Midlands, 1967: (Stratton, 1914)
Montfort, 1938: (Tizané, 1977a, pp. 51–52)
Nanterre, 2015: (Catala, 2019, pp. 160–163)
Narrabri, 1900: (Healy & Cropper, 2014, case 21)
Neusatz, 1951: (Bender, 1969)
Neudorf, 1952: (Bender, 1969)
New Jersey South, 1973: (Osis & McCormick, 1982))
Nickleim, 1968: (Bender, 1969)
Nottingham, 1990: (Cornell, 2002)
Olive Hill, 1969: (Roll, 1972, pp. 148–157)
Ooty, 1897: (Flammarion,1923, p.348)
Pittsburgh, 1971: (Pierce, 1973; Gauld & Cornell, 1979, pp. 356–9)
Pontrefact, 1966: (Wilson, 1981)
Poona, 1920: (Price, H., & Kohn, H.,1930).
Port Sainte Marie, 1922: (Flammarion, 1923)
Portland, 1909: (Gilbert, 1910)
Pursruck, 1970: (Bender, 1974, pp. 135–38)
Rochdale, 1995: (Clarkson, 2011)
Roodeport, 1922: (Bayless, 1967)
Rosenheim, 1967: (Bender, 1969)
Sandfeldt, 1722: (Gauld & Cornell, 1979, Chapter 6)
San Remo, 1986: (Healy & Cropper, 2014, Chapter 6)
Sauchie, 1960: (Gauld & Cornell, 1979, pp. 79–83)
Scherfede, 1972: (Bender, 1974, pp. 138–141)

Seaford, 1958: (Roll & Pratt, 1958)
Seyssuel, 1930: (Sudre, 1931)
Sicily, 1890: (Roll, 1972)
Siirt, 2012: (Healy & Cropper, 2014, Appendix A, pp. 269–274)
South Shields, 2006: (Ritson, 2020)
South Wales, 1989: (Fontana, 1991) & (Fontana, 1992, pp. 225–231)
Stans, 1860: (Joller, 1862)
Strasbourg, 1855: (Flammarion, 1923)
Stratford, 1850: (Thuston, 1953, pp. 10–13)
Sumatra, 1903: (Grottendieck, 1906)
Swanland, 1849: (Myers, 1891)
Tackley, 1905: (Gauld & Cornell, 1979, pp. 183–6)
Talladega, 1959: (Rogo, 1979, pp. 164–68)
Tanjore district, 1920: (Thurston, 1991)
Tarcutta, 1949: (Healy & Cropper, 2014, Chapter 3)
Tidworth, 1662: (Gauld & Cornell, 1979, pp. 43–64)
Tucson, 1983: (Rogo, 1987)
Turin, 1900: (Bozzano, 2000, Cas XXII, pp. 206–209)
Vachendorf, 1948: (Bender, 1969)
Villa Devoto, 1903: (Flammarion, 1923)
West Midlands, 1901: (Stratton, 1914)
Wisbech, 1957: (Cornell & Gauld, 1960)

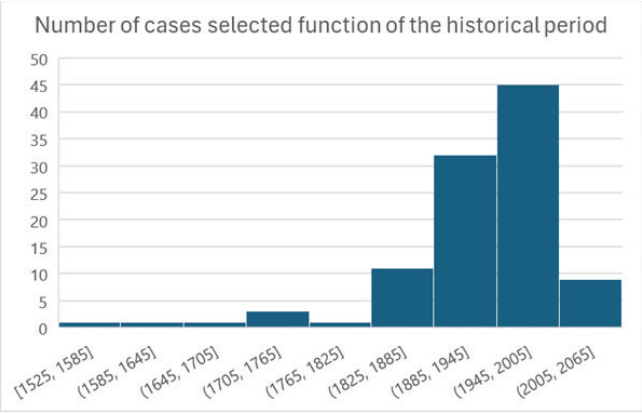


**Figure 7**. Number of Cases Selected as a Function of the Historical Period.


**Figure 8**. Countries Containing at Least One Selected Case.

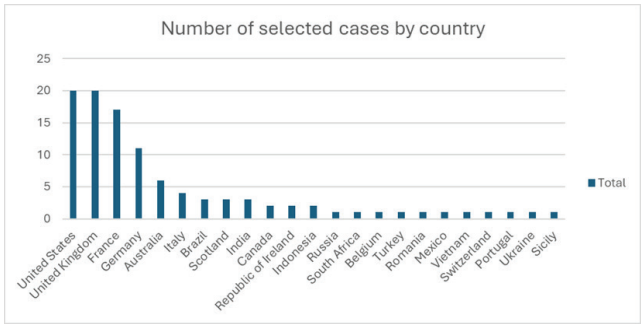


**Figure 9**. Number of Selected Cases by Country.

## APPENDIX B: SUPPORTING FIGURES

60 % of the cases selected for this study are less than a century old compared to 43 % in our global database (Figure 7). About the geographics, Figure 8 presents the countries concerned by the cases selected. and Figure 9 gives a graphic representation for each country of the number of selected cases.

## APPENDIX C: ABBREVIATIONS USED

ASPR: American Society for Psychical Research
CIRCEE: Center for Information, Research and Consultation on Exceptional Experiences
CUSPR: Cambridge University Society for Psychical Research
AI: Artificial intelligence
IGP: General Institute of Psychology (1900–1933)
IGPP: Institut für Grenzgebiete der Psychologie und Psychohygiene (parapsychology research center in Freiburg-en-Grisbau, Germany)
IMI: International Metapsychic Institute
JSPR: Journal of the SPR
JASPR: Journal of the ASPR
OBE: Out-of-body experience
PA: Parapsychological Association
PPA: Proceedings of Parapsychological Association annual convention
PRF: Psychical Research Foundation
PSPR: Proceeding of Society for Psychical Research annual convention
RNG: Random numbers generator used in micro-PK experiments
RSPK: Recurrent spontaneous pyschokinesis
SPR: Society for Psychical Research
SSE: Society for Scientific Exploration

Journal of Scientific Exploration
Anomalistics and Frontier Science

BRIEF REPORT

# The Myth of the Decline Effect in Psi Research: The Empirical Evidence

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## HIGHLIGHTS

A robust analysis of five types of research on extrasensory perception found that only 'remote viewing' studies showed a small but statistically significant decline in effect size over time.

## ABSTRACT

The decline effect (DE) has been discussed in "psi" research since the early times of experimental investigations, and many causes have been advanced from: individual psychology, social attitudes, electromagnetic fields, experimental artifacts, or physical properties often related to quantum physics. Bierman (2001) found small and statistically significant decline slopes in all experimental protocols, but the mind-matter interaction with random number generators, where he found a positive quadratic polynomial slope. This study aimed to update Bierman's results, taking into account all studies completed up to 2023 and analyzed in different meta-analyses. Five experimental protocols were analyzed: including anomalous perception in a Ganzfeld condition, remote viewing, forced-choice design in extra-sensory perception, predictive physiological anticipation, and dream extra-sensory perception studies. The results showed that only one slope coefficient out of the five examined was statistically significant, indicating that there was no evidence of a general DE across the different experimental protocols.

## KEYWORDS

Decline effect; extra-sensory perception; meta-analysis; replication.

## INTRODUCTION

In the field of experimental investigation of extra-sensory perception (ESP) and mind-matter interaction, also broadly defined as "psi", a short-hand term for psychic abilities, there is a myth widely shared among scientists interested in such phenomena even if a formal survey has never been conducted. This myth is about the decline effect (DE). The decline effect is the disappearance of a phenomenon with the repetition of experiments aimed at identifying its characteristics or, more simply, to confirm its reality. This decline is very different from the decline in participants' performance who are requested to repeat an identical task many times on the same or different days. The former is referred to as "*chronological declines*" (Irwin, 1999, cited in Colborn, 2007, p. 2), and the latter is referred to as "*episodic declines*", which are "within an experimental run or within a session" (Colborn, 2007, p. 2).

Thalbourne's (2003) definition of DE identifies these two kinds of decline as '*within* study' and '*between* studies'.

In Schooler's (2011) seminal essay, the DE was taken as a solid fact, and the author discussed what could be the underlying causes, for example, statistical self-correction of initially exaggerated outcomes, also known as regression to the mean, or for the generalizability limitations of the initial findings, for example, to different samples of participants or changes in the experimental designs as it is typical in all so-called conceptual experimental replications (for a distinction between exact and conceptual replications, see Derksen and Morawski, 2022). Deliberate changes in experimental designs (from simple and fun, to complex and tedious) are introduced by theoretically oriented experimenters who want to understand the psi *process* rather than merely *prove* psi's existence, and these improvements in study quality over the years might account for DEs.

In psi research, DE has been discussed since the early times of experimental investigations (Bierman, 2001; Colborn, 2007), and since then, many causes have been advanced from individual psychology, social attitudes, electromagnetic (EM) fields, experimental artifacts, or physical properties often related to quantum physics. Bierman (2001) supported this claim using empirical data. Analyzing the regression line of the effect sizes observed in all experiments of six experimental protocols (e.g., ESP in a Ganzfeld condition, forced-choice precognitive ESP, mind-matter interaction with random number generators, etc.), Bierman found small and statistically significant decline slopes in all six experimental protocols, but for the mind-matter interaction with random number generators, (RNGs), where he found a positive quadratic polynomial slope.

After Bierman (2001), many other experiments have been carried out to test ESP and mind-matter interactions using different experimental protocols. The aim of this short report was to update Bierman's results, taking into account all studies completed up to 2023 and analyzed in different meta-analyses.

## METHOD

### Experimental Protocols Retrieval

All meta-analyses related to ESP and mind-matter interactions included in the database are available for open access at https://tidy.ws/cjnaxX were analyzed for inclusion.

### Inclusion Criteria

The meta-analyses should include: (a) studies that had publication dates ranging over a span of at least 20 years, which we considered a robust time window to check for DEs, and b) databases to estimate the slope regressions.

Regarding the inclusion criteria (a), most meta-analyses spanned greater periods with respect to the inclusion criteria of 20 years as a minimum. Regarding (b), the regression slope can provide information about the rate of change over time. In our case, the *x*-axis indicates time in years, and the *y*-axis indicates effect size. A negative slope (a downward-sloping curve to the right) indicates a decline.

### Included Meta-Analyses

The following meta-analyses were included in this study:

- Stage 2 Registered Report: Anomalous perception in a Ganzfeld condition-A meta-analysis of more than 40 years of investigation (Tressoldi & Storm, 2024).
-Remote Viewing: A 1974-2022 Systematic Review and Meta-Analysis (Tressoldi & Katz, 2023).
- Assessing 36 Years of the Forced Choice Design in Extra Sensory Perception Research: A Meta-Analysis, 1987 to 2022 (Storm & Tressoldi, 2023).
- Predictive physiological anticipation preceding seemingly unpredictable stimuli: A meta-analysis (Mossbridge et al., 2012) collapsing the data with "Predictive physiological anticipatory activity preceding seemingly unpredictable stimuli: An update of Mossbridge et al.'s meta-analysis' (Duggan & Tressoldi, 2018).
- On the correspondence between dream content and target material under laboratory conditions: a meta-analysis of dream-ESP studies, 1966-2016 (Storm et al., 2017).

### Decline Effect Estimates

The DE of each of the five experimental protocols was estimated by the meta-regression linear function included in the metafor package (Viechtbauer, 2010) with year of study publication as a covariate and the standardized effect size of each study as the dependent variable. The five databases and the syntax code are available for open access at https://doi.org/10.6084/m9.figshare.24922620.v4 for independent analyses.

## RESULTS

The slope coefficients with their 95% confidence interval and *p* values of each of the five experimental protocols, are presented in Table 1.

**Table 1:** Slope Coefficients With 95% Confidence Intervals (CI) and p-values of Each of the Five Experimental Protocols.

| Experimental protocol | Years range | Year span | *N* of studies | Slope | 95% CI | *p* |
|---|---|---|---|---|---|---|
| Anomalous perception in a Ganzfeld condition | 1974 (incl.) - 2020 | 47 | 113 | .002 | [-.002, .006] | .26 |
| Remote Viewing | 1974 (incl.) - 2022 | 49 | 40 | -0.009 | [-.016, -.003] | .004 |
| Forced Choice Design in Extra Sensory Perception | 1987 (incl.) - 2022 | 36 | 141 | 0.00045 | [-.0001, .0010] | .088 |
| Predictive physiological anticipation | 1997 (incl.) - 2017 | 21 | 62 | 0.0035 | [-.007, .014] | .50 |
| Dream-ESP studies | 1966 (incl.) –2014 | 49 | 50 | -0.005 | [-0.01, .0001] | .054 |

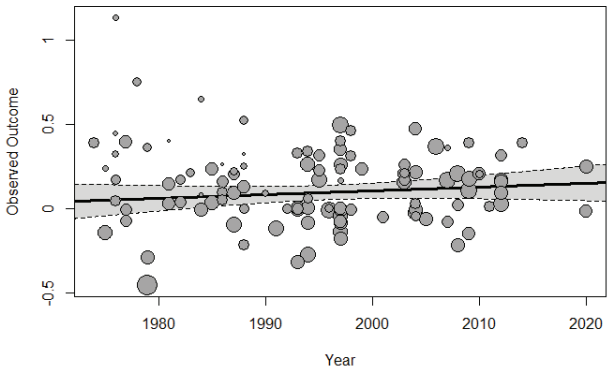



**Figure 1a**: Linear slope regression (dark line) with corresponding 95% CI (grey area) of the ESP in a Ganzfeld condition.

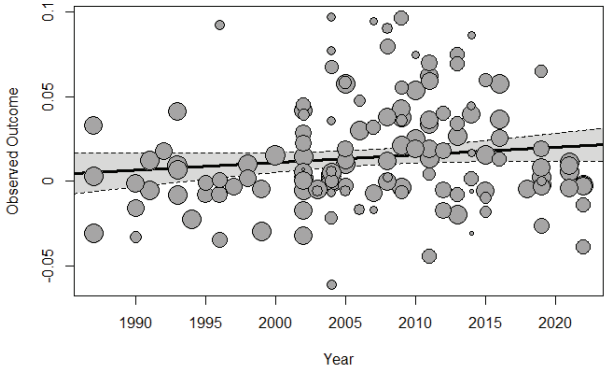



**Figure 1c**: Linear slope regression (dark line) with corresponding 95% CI (grey area) of the ESP with Forced-Choice protocols.

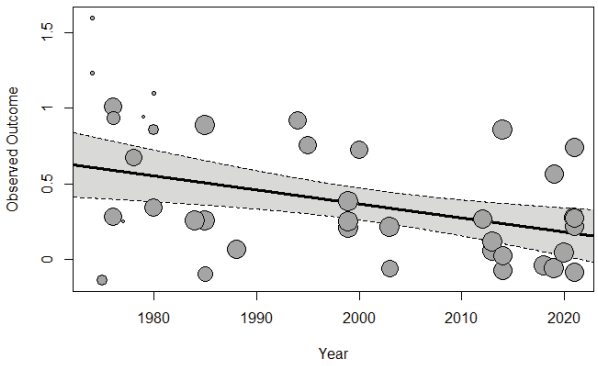



**Figure 1b**: Linear slope regression (dark line) with corresponding 95% CI (grey area) of the ESP with Remote Viewing protocols.

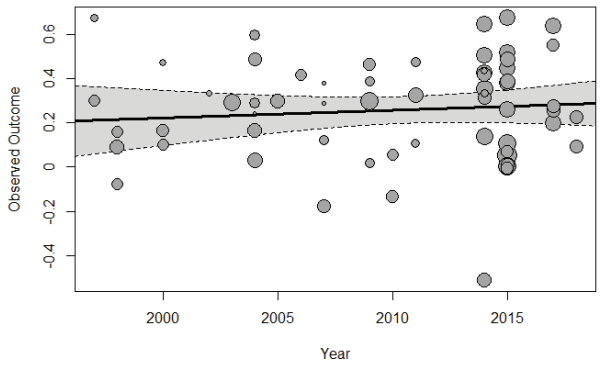



**Figure 1d**: Linear slope regression (dark line) with corresponding 95% CI (grey area) of the Predictive physiological anticipation experiments.

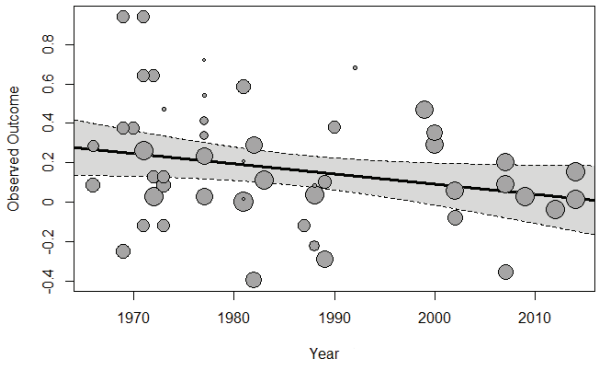



**Figure 1e**: Linear slope regression (dark line) with corresponding 95% CI (grey area) of the ESP in dreams.

## DISCUSSION

The aim of this short report was to provide an update on the empirical support to DE in the field of ESP, estimating the regression linear slope of standardized effect sizes and taking the year of experiment publication as a covariate. To support the reality of DE, statistically significant negative slopes should be observed. Among the five selected databases, those related to Remote Viewing and ESP in dreams showed a statistically significant negative DE, whereas the remaining three databases showed statistically non-significant slopes.

However, if we consider not only the *p* values, but also the size of the slopes, it is observed that for both databases showing a DE, their values are very low, -0.009 and -0.005, for Remote Viewing and ESP in dreams, respectively. If we consider the average effect size observed in the Remote Viewing meta-analysis (Tressoldi & Katz, 2023), corresponding to .34; CI (95%) [.22, .45], and apply a DE of -0.009 each year, we should expect to reach an almost null effect size of .05 in approximately 32 years assuming no improvements in the experimental protocols.

For the ESP in dreams database, the authors of the meta-analysis (Storm et al., 2017) discussed the relevance of the methodological differences between the Maimonides Dream Lab and those carried out by other labs. Among the main differences, the participants in the Maimonides Dream Lab experiments dreamed in a dedicated lab and were continuously monitored during their sleep time to be woken up when reaching the sleep REM phase to identify the target stimuli. In contrast, most participants in the non-Maimonides experiments dreamed at home and sent their dream recalls related to the target stimuli to the experimenters.

If we estimated the DE of only the 36 non-Maimonides experiments, we obtained the following results: slope = -0.0024; CI (95%) [-0.008, 0.003]; $p$ = .40 (see Figure S1 in the Supplementary Material). Furthermore, the database of 50 studies in Table 1 combines two dream-ESP databases (Maimonides Dream Laboratory [MDL] studies and post-MDL studies), which are not significantly different from each other. Storm (2023) has shown that the DEs for both are not significant: MDL studies, $r(12)$ = -0.02, $p$ = .943 (two-tailed); post-MDL studies, $r(34)$ = -0.20, $p$ = .238 (two-tailed).

In summary, only one of the five databases related to different categories of experimental protocols related to ESP showed minimal DE. These results cannot be generalized to different experimental protocols; for example, to mind-matter interaction (psychokinesis) protocols. Consequently, we cannot exclude DEs in other domains.

## REFERENCES

Bierman, D. J. (2001). On the nature of anomalous phenomena: Another reality between the world of subjective consciousness and the objective world of physics. In P. Van Loocke (Ed.), *The physical nature of consciousness* (pp. 269-292). John Benjamins Publishing Company. https://doi.org/10.1075/aicr.29.12bie

Colborn, M. (2007). The decline effect in spontaneous and experimental psychical research. *Journal of the Society for Psychical Research, 71*, 1-22.

Derksen, M., & Morawski, J. (2022). Kinds of replication: Examining the meanings of "conceptual replication" and "direct replication". *Perspectives on Psychological Science, 17*, 1490-1505. https://doi.org/10.1177/174569162110411

Duggan, M., & Tressoldi, P. (2018). Predictive physiological anticipatory activity preceding seemingly unpredictable stimuli: An update of Mossbridge et al.'s meta-analysis [version 2; referees: 2 approved]. *F1000Research, 7*, Article 407. https://doi.org/10.12688/f1000research.14330.2

Mossbridge, J., Tressoldi, P. E., & Utts, J. (2012). Predictive physiological anticipation preceding seemingly unpredictable stimuli: A meta-analysis. *Frontiers in Psychology, 3*, 390. https://doi.org/10.3389/fpsyg.2012.00390

Schooler, J. (2011). Unpublished results hide the decline effect. *Nature, 470*, Article 437. https://doi.org/10.1038/470437a

Storm, L. (2023). The dark spirit of the trickster archetype in parapsychology. *Journal of Scientific Exploration, 37*, 665-682. https://doi.org/10.31275/20232715

Storm, L., Sherwood, S. J., Roe, C. A., Tressoldi, P. E., Rock, A. J., & Di Risio, L. (2017). On the correspondence between dream content and target material under

laboratory conditions: A meta-analysis of dream-ESP studies, 1966-2016. *International Journal of Dream Research, 10*, 120-140.

Storm, L., & Tressoldi, P. (2023). Assessing 36 years of the forced choice design in extra sensory perception research: A meta-analysis, 1987 to 2022. *Journal of Scientific Exploration, 37*, 517-535. https://doi.org/10.31275/20232967

Thalbourne, M. A. (2003). *A glossary of terms used in parapsychology*. Puente.

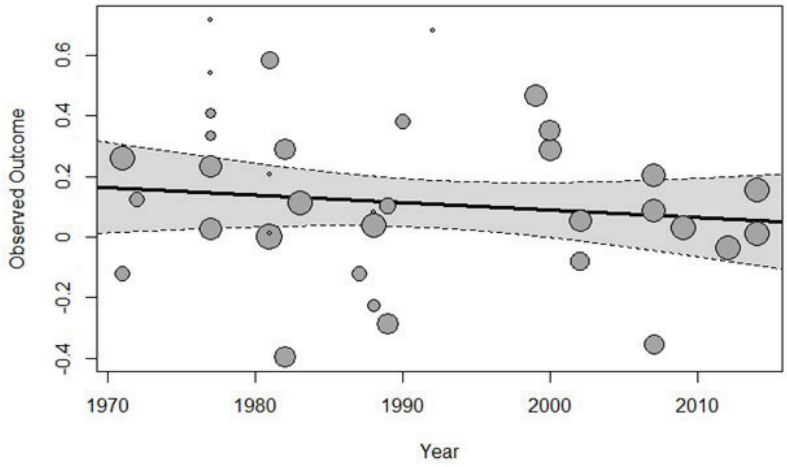
Tressoldi, P., & Katz, D. L. (2023). Remote viewing: A 1974-2022 systematic review and meta-analysis. *Journal of Scientific Exploration, 37*, 467-489. https://doi.org/10.31275/20232931

Tressoldi, P. E., & Storm, L. (2024). Stage 2 registered report: Anomalous perception in a Ganzfeld condition—A meta-analysis of more than 40 years investigation. *F1000Research, 10*, Article 234. https://doi.org/10.12688/f1000research.51746.3

Viechtbauer, W. (2010). Conducting meta-analyses in R with the metafor package. *Journal of Statistical Software, 36*, 1-48. https://doi.org/10.18637/jss.v036.i03

## SUPPLEMENTARY MATERIAL

**Figure** S1.



Journal of Scientific Exploration
Anomalistics and Frontier Science

SPECIAL ESSAY PREFACE

# Reader Advisory for Sudduth's BICS Treatise: Preface to Long Essay Section

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A single, peer-reviewed essay appears in this issue by design—a de facto special subsection featuring a formidable treatise that took approximately a year to prepare and at my request. Readers may criticize me for allotting considerable *Journal* space to this work, but consistent with the actions of other journals (e.g., Kuhn, 2024), my editorial policy has always emphasized generous flexibility as appropriate (Houran, 2023). Dr. Michael Sudduth's paper is a warranted case, in my judgment, as the complicated topic of postmortem survival of physical death has arguably stagnated from a research perspective. Indeed, heated and polarized positions on survival often stifle discussions, thwart adversarial collaborations, and hinder advancements in this domain. *JSE*'s Summer 2022 issue (https://shorturl.at/qX056) therefore attempted to motivate progress via a brokered exchange between ostensible skeptics and advocates (i.e., Keith Augustine et al. and Stephen Braude et al.) who debated the outcomes and lessons from the BICS essay contest on the best available evidence for survival (Kelleher & Bigelow, 2022). No sea-change on either side of the conversation occurred, but at least a more constructive dialogue about the key issues and confounds took root.

However, that published exchange undoubtedly holds many observations and insights that are still waiting to be mined, shared, and contemplated. In this spirit of exploration, Sudduth accepted my invitation to conduct a comprehensive "forensic-type audit" of the assumptions and approaches underlying the positions of Augustine versus Braude. He is especially well-suited for this task given his background teaching and publishing in the areas of critical thinking and epistemology, with a focus on theories of evidence and the justification of belief across different domains of inquiry, including general and legal epistemology (e.g., Sudduth's entry on "Defeaters in Epistemology" in the *Internet Encyclopedia of Philosophy*: https://iep.utm.edu/defeaters-in-epistemology/), topics in the philosophy of religion (Sudduth, 2009), and more recently postmortem survival (Sudduth, 2016). This exercise thus aimed to unstick the apparent stalemate by identifying previously unacknowledged or unexamined points of agreement and sources of divergence in the two camps' respective arguments. Given the scope and depth of the original material, Sudduth's extensive commentary still manages to condense a rather vast territory of issues. It is important to note that his assessment sought neither to declare an ultimate "winner" of *JSE*'s BICS debate nor to defend or indict anyone on a personal level. Rather, the goal was to apply precise, logical analysis to uncover new learnings that can help to foster dispassionate thinking and fresh studies on the survival question, irrespective of any researcher's ideological leanings.

*But be warned*—Sudduth's examination is not for casual readers, offhand thinkers, or the faint of heart. His paper should be tackled only by those who are seriously committed to wrestling with thorny conceptual and empirical issues surrounding the question

of survival. Even so, many pitfalls and traps await readers, including the length, density, and complexity of the core material under scrutiny. Without a careful reading, these factors make it easy to overlook or mistake Sudduth's main points, critical nuances, notable caveats, or even specific references to illustrations in other works. Therefore, *JSE*'s editorial team recommends some steps to engage best with this treatise: (a) Familiarize yourself with the key background literature that anchors Sudduth's approach (i.e., Augustine, 2022a, 2022b; 2022c; Braude et al., 2022; Nahm, 2022); (b) Slowly read Sudduth's analysis at least twice, with a gap in between readings to reflect thoughtfully on the content and perhaps also to consult other works on survival from diverse ideological viewpoints (for a suggested reading list, see Houran et al., 2023: Appendix); and (c) Discuss the end-products of your homework with others who have followed suit.

This exercise might help to cultivate a dedicated and rejuvenated group of maverick pioneers poised to systematically confront humanity's truly final frontier. The BICS contest successfully popularized past and present academic studies on the survival hypothesis. Yet the extant literature is not an endpoint but merely a launching pad for future studies. Sudduth likewise offers no definitive solution at this time. Instead, his treatise encourages researchers to take a strategic step back in an effort to better specify and understand the fundamental questions that must be asked and answered before any substantive progress can be made in the first place.

## REFERENCES


Augustine, K. (2022a). How *not* to do survival research: Reflections on the Bigelow Institute essay competition. *Journal of Scientific Exploration*, *36*, 366–398. https://doi.org/10.31275/20222581

Augustine, K. (2022b). Final reply: When will survival researchers move past defending the indefensible? *Journal of Scientific Exploration*, *36*, 412–435. https://doi.org/10.31275/20222695

Augustine, K. (2022c). Answering more of the same: A reply to Nahm. *Journal of Scientific Exploration*, *36*, 794–808. https://doi.org/10.31275/20222801

Braude, S., Barušs, I., Delorme, A., Radin, D., & Wahbeh, H. (2022). *Not* so fast: A response to Augustine's critique of the BICS contest. *Journal of Scientific Exploration*, *36*, 399–411. https://doi.org/10.31275/20222649

Houran, J. (2023). To engage, inform, and inspire: The many facets of scientific writing [editoral]. *Journal of Scientific Exploration, 37*, 308–311. https://doi.org/10.31275/20233131

Houran, J., Rock, A. J., Laythe, B., & Tressoldi, P. E. (2023). Dead reckoning: A multiteam system approach to commentaries on the *Drake-S* equation for survival. *International Journal of Transpersonal Studies*, *42,* 80–105. https://doi.org/10.24972/ijts.2023.42.1.80

Kelleher, C., & Bigelow, R. (2022). The 2021 Bigelow Institute for Consciousness Studies (BICS) essay contest. *Journal of Scientific Exploration*, *36*, 350–365. https://doi.org/10.31275/20222693

Kuhn, R. L. (2024). A landscape of consciousness: Toward a taxonomy of explanations and implications. *Progress in Biophysics and Molecular Biology*. Advance online publication. https://doi.org/10.1016/j.pbiomolbio.2023.12.003

Nahm, M. (2022). A guardian angel gone astray: How *not* to engage in scientific debates on survival. *Journal of Scientific Exploration*, *36*, 783–793. https://doi.org/10.31275/20222779

Sudduth, M. (2009). *The reformed objection to natural theology*. Ashgate. [2016 softcover: Routledge]. https://doi.org/10.24204/ejpr.v1i2.340

Sudduth, M. (2016). *A philosophical critique of empirical arguments for postmortem survival*. Palgrave Macmillan. https://doi.org/10.1057/9781137440945

Journal of Scientific Exploration
Anomalistics and Frontier Science

ESSAY

# The Augustine-Braude Bigelow Survival Debate: A Postmortem and Prospects for Future Directions

**Michael Sudduth**

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## HIGHLIGHTS

Independent analysis of a published debate on the survival question shows that future research must address certain conceptual and methodological issues to meet robust standards of evidence and reasoning.

## ABSTRACT

In 2021, the Bigelow Institute for Consciousness Studies (hereafter, BICS) sponsored an essay competition designed to solicit the best evidence for the hypothesis that human consciousness survives bodily death, and more specifically, evidence that would prove this hypothesis beyond a reasonable doubt. The summer 2022 issue of *the Journal of Scientific Exploration* featured a special subsection on the BICS contest and its winning essays. Robert Bigelow and Colm Kelleher outlined the motivation, design, and judging criteria for the competition. Keith Augustine provided an extensive critical commentary on the contest design and eight of its prominent winning essays. Stephen Braude and several coauthors[1] responded to Augustine's criticisms, and Augustine provided a reply to Braude and his collaborators. Finally, the subsection concluded with a collaborative paper in which Etienne LeBel, Adam Rock, and Keith Augustine proposed a more rigorous experimental design for testing the survival hypothesis.[2]

## KEYWORDS

Bayesianism, Bigelow competition, evidence for survival, inference to best explanation, the survival hypothesis, theories of evidence.

In this invited paper, my primary goal is to offer a critical evaluation of the Augustine–Braude et al. exchange concerning the BICS competition (hereafter, the Contest). Augustine presented important criticisms of the Contest and several of its prominent winning essays. Moreover, many of his criticisms apply to survival literature in general and so are instructive for the wider survival debate. And Stephen Braude, one of the most important contributors to the survival debate since C.D. Broad and C.J. Ducasse, was at the helm of the reply to Augustine. So, I was looking forward to a fine-tuned, surgical response to Augustine, which would give the devil his due whilst also offering a more conscientious and nuanced case for survival. Regrettably, this was not the case. Although the response to Augustine raised important concerns about skeptical assessments of the evidence for survival, it was hamstrung with several defects:

- **Inadequate calibration:** the Braude et al. reply is inadequately calibrated to address Augustine's actual arguments.
- **Marginalized common ground:** Braude et al. pay insufficient attention to important points on which Braude (at least sans et al.) and Augustine actually agree.
- **Opaqueness:** Braude et al. are unclear about the specific (or even approximate) favorable evidential claim and argument about survival they wish to support or defend against Augustine and his critique of the Contest's papers.
- **Epistemological neglect:** Braude et al. do not discuss the kind of epistemic principles or criteria of evidential support they wish to enlist on behalf of the survival hypothesis or in defense of the essays in Augustine's crosshairs.

I will explore each of these in detail.

However, my paper is more than an audit of the exchange between Augustine and Braude et al. I extend their discussion in a few ways. For example, Braude et al. accused Augustine of dodging a number of important issues. These issues were not essential to Augustine's arguments, but I will address them since they are relevant to the wider survival debate, and I intend this paper to make a positive contribution to the larger debate. Also, I will reiterate and further develop several of Augustine's poignant criticisms of the competition and its essays. This is important because the errors in reasoning that characterize the BICS essays are commonplace in the wider body of survival literature. I have elsewhere documented and discussed these defects (Sudduth, 2009, 2013a, 2013b, 2016, 2021a, 2021b, 2022a, 2022b), but they are worth restating here in the context of Augustine's critique. Logical fallacies in survival literature tend to spread like dandelions on lawns or adverbs in poorly written fiction. They need to be kept in check. Finally, in the interest of offering insights that might advance the survival debate, I use the negative tier of the paper to frame a variety of constructive suggestions for how survivalists ought to approach the logic and epistemology of survival arguments.

I will have a lot to say about principles and criteria of evidential support throughout the paper, but one idea will be especially important – the *comparative expectedness of data* under contrasting hypotheses. Roughly stated, an observational datum is evidence for hypothesis $H_1$ instead of an alternative hypothesis $H_2$ when the observation is more expected given $H_1$ than it is given $H_2$. This principle is baked into inferences to best explanation, as well as various theories of hypothesis confirmation. It is relied on across the natural and social sciences, including forensic science and legal reasoning. Survivalists, too, have relied on it, even if only tacitly – for example, in their attempts to argue that the survival hypothesis is the best explanation of the data. Augustine's critique makes significant use of the idea in the form of the Surprise Principle, and Braude et al. also appear to accept it. The principle is unavoidable if we wish to have a serious discussion about evidence. And, as I have argued elsewhere (Sudduth, 2016), the comparative expectedness of data under contrasting hypotheses plays a crucial role in diagnosing deeply entrenched problems that vitiate traditional empirical survival arguments.

Disclaimer: this paper will not be an easy read. It is lengthy, extensive in scope, and involves considerable analytical detail. Of course, the source material and its history are equally dense. A systematic analysis is warranted but requires conceptual detail and what some readers are likely to see as a daunting, technical discussion of issues in logic and epistemology. However, the wider body of literature has consistently ignored the more complex conceptual issues that underlie survivalist efforts to leverage facts in support of the survival hypothesis. Survivalists have often been guilty of a kind of naïve empiricism which eschews addressing the fundamental philosophical issues on which the cogency of survival arguments depends. Furthermore, a technical treatment of issues in logic and epistemology is unavoidable if we wish to properly diagnose the exchange between Augustine and Braude et al. Since my essay presupposes the content of the BICS essays which Augustine discussed, it would be best if the reader were familiar with some of those essays. My assigned task was to comment on the exchange between Augustine and Braude et al., not remake the meals the BICS essayists served up.

## Index to Paper Sections

Due to the density of the paper, a brief outline of the content by section will be helpful.

In the first half of the paper (**§1–§9**), I provide a critical analysis of prominent issues in the Contest and in the exchange between Augustine and Braude et al. After exploring areas of agreement and disagreement between Augustine and his respondents, I evaluate and reply to the main objections leveled against Augustine's critique.

- **§1** and **§2** identify important conceptual flaws in the Contest and introduce several of my general criticisms of the Braude et al. reply to Augustine.
- **§3** outlines different epistemological questions lurking in the Contest and compares how Braude and Au-

gustine answer these questions.

- **§4** critically discusses inference to best explanation (IBE) survival arguments as the potential locus of genuine disagreement between Augustine and Braude et al.
- **§5** focuses on likelihoodist and Bayesian concepts of evidence in confirmation theory and roadmaps different criteria of evidential support and the role they ought to have in the logic and epistemology of survival arguments.
- **§6** analyzes Braude's reasoning about survival (outside the reply to Augustine), and I argue that Braude's arguments are best construed as a conceptual merger between traditional IBE arguments and a likelihoodist approach to evidential support.
- **§7** clarifies different forms of skepticism, all of which are operative in Augustine's critique, and how these different forms of skepticism impact the dialectical structure of debates between survivalists and their critics.
- **§8** and **§9** examine Braude et al.'s main objections to Augustine's critique, and I provide detailed critical responses to Braude and his collaborators.

The second half of the paper takes a deeper dive into epistemology and philosophy of science. More specifically, I will focus on issues in confirmation theory,[3] which concerns the logic by which scientific or empirical hypotheses are confirmed or disconfirmed by empirical data. This includes evaluating the degree to which evidence supports or confirms a particular hypothesis, as well as the degree to which evidence counts against or disconfirms a hypothesis.

- **§10** responds to Braude et al.'s appeal to the well-worn survivalist trope that non-paranormal counterexplanations of the mediumistic data – for example, fraud – are improbable.
- **§11** examines confirmation-theory-related issues baked into Augustine's comments on mediumship but which Braude et al. did not adequately navigate.
- **§12** clarifies and defends Augustine's argument concerning the significance of failed tests for survival.
- **§13**, **§14**, and **§15** analyze several confirmation-theory-related flaws that undermine survival arguments but which survivalists have failed to address.

Although my commentary has a substantial negative tier, I use my critical remarks as a springboard for constructive analysis and suggestions. My overriding interest is to remedy long-standing and deeply entrenched defects in the logic and epistemology of survival arguments. I hope this will raise the level of discourse in the survival debate in ways that mirror advancements in other (scientific and non-scientific) areas of inquiry.

Among other things, I will argue that survivalists ought to:

- give significant attention to the logical architecture of survival arguments and skeptical counterarguments, paying particular attention to using recognized argument forms to present arguments, with the premises and conclusion(s) of the main argument clearly laid out, and main arguments clearly distinguished from sub-arguments,
- apply statistician Richard Royall's important distinction between two evidence-related questions – *What does the evidence presently say? What should we believe?*
- formulate the survivalist conclusion(s) with greater conceptual clarity – for example, being clear about the difference between favorable *evidential* and *explanatory* claims, as well as the qualitative and quantitative aspects of evidential support,
- deploy fundamental non-domain-specific criteria of evidence assessment – for example, Bayesianism and likelihoodism – and calibrate them in ways that are appropriate to the survival debate, and which may be analogous to their successful use in nearby areas of inquiry such as psychology and philosophy of religion,
- adopt a probabilistic conception of evidence and use it to bulk up explanatory arguments which are in themselves insufficiently truth-conducive.

**Acknowledgements**

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## 1. The Contest and the Augustine-Braude et al. Exchange: Preliminaries

### *The Contest*

The BICS Contest was designed to solicit the best evidence for the hypothesis that human consciousness survives bodily death. The attempt to show that there are data, facts, or observations that are the "best evidence" for a hypothesis raises two kinds of evaluative issues. First, there is the *quality of the observational data* stipulated as evidence – for example, the reliability of testimony, test protocols, investigative procedures, or methodologies, and hence the reliability of data derived from such sources. Second, there is the *quality of the inferences from the data*. The BICS essayists attempted to address both evaluative issues, though oftentimes conflating the two. Some subsequent critiques of the Contest's essays focused heavily on data-quality issues – for example, by ranking the methodologies used to obtain data in comparison with those used in the various sciences (Tressoldi et al., 2022). By contrast, the exchange between Augustine and Braude et al. brought into sharp focus conceptual issues surrounding the quality of the inferences from the data.

My focus will be on the second evaluative question. Important conceptual questions underlie the Contest's design and its winning essays. Both raise important questions about the kind of favorable claims survivalists wish to make about the survival hypothesis, the logical structure of the arguments offered in support of those claims, and the principles or criteria of evidential support on which the cogency of survival arguments depends. These are questions in the logic and epistemology of belief in survival, and they are fundamental to the empirical survival debate. What kind of evidential claim do survivalists wish to make on behalf of the survival hypothesis? What are the relevant epistemic principles or evidential criteria that would clarify and justify the belief that there is evidence for the truth of the survival hypothesis? How strong is the evidence? And what does the argument for survival look like once we have conscientiously answered these questions, if only tentatively?

Regrettably, the Contest's design and many of its winning essays were defective at this juncture. At times, egregiously so. Many of the essays were conceptually opaque and superficial in argumentation, frequently offering little more than narratives vitiated with an assortment of garden-variety logical fallacies. Arguments were sometimes only suggested, not presented. These are hardly exemplars of lucid and rigorous thinking, much less scientific reasoning. Beyond remedial logical mistakes, a particularly salient recurring flaw was the failure to identify and critically apply evidential principles that would be appropriate, if not required, to underwrite what survivalists wish to say about the data. This is by no means a defect uniquely characteristic of the Contest and its winning essays. It is a longstanding and widespread problem in survival literature in general.

Here it is important to invoke a crucial observation made by Stephen Braude:

> ... there's no such thing as a purely empirical inquiry. Even the most apparently straightforward or innocent empirical claims rest on underlying abstract presuppositions, both metaphysical and methodological.... In most areas of science, fundamental philosophical assumptions form part of the working scientist's conceptual background. However, in survival research, abstract and deep philosophical issues often dominate the foreground. (Braude, 2003, p. 2)

Braude is a philosopher. So, it is not surprising that he should offer this particular insight. Nor was he the first to do so. Other prominent philosophers who have written on the topic of survival have made similar points – for example, C.D. Broad, H.H. Price, and C.J. Ducasse. Survival researchers today pay little regard to the cautionary and instructive wisdom of the philosophers from Cambridge, Oxford, and Brown. They remain wedded to a kind of naïve empiricism that eschews engaging the conceptual and abstract assumptions that underlie their ostensible inquiries into the physical world and the inferences they wish to draw from facts. If it is the job of empirically minded researchers to remind philosophers of the facts, it is the business of philosophers to keep such researchers honest about the interpretation of the facts.

### *The Contest's Implausible Legal Evidentiary Standards*

Apart from the cacophony of logical errors to which Augustine drew attention – I will revisit some of these in due course – the Contest's design exhibits several crucial conceptual errors that are ubiquitous in contemporary survival literature. One of the more consequential missteps is the Contest's implausible, if not incoherent, appropriation of legal evidentiary standards, specifically the criminal standard of "proof beyond a reasonable

doubt." Many of the prize-winning essays claimed to have established the truth of the survival hypothesis beyond a reasonable doubt, and many of them deployed various auxiliary legal concepts and analogies as part of their conceptual scaffolding. This might pass for "hip" survival research – it certainly makes for good marketing – but it is bad science, bad jurisprudence, and especially bad philosophy. Happily, Augustine (2022a, pp. 367–368) and Braude et al. (2022, p. 399, 401) agree that this aspect of the Contest was at least contentious, if not altogether dubious.

But more needs to be said.

Legal evidentiary standards presuppose evidence that has been shaped by legal rules. Some of these rules are not governed by the epistemic point of view, roughly, the goal of reliably getting at the truth. For example, *Federal Rules of Evidence*, Rule 403, calls for the exclusion of evidence that has probative (= epistemic) value if the probative value is "substantially outweighed by the danger of unfair prejudice, confusion of the issues, or misleading the jury, or by considerations of undue delay, waste of time, or needless presentation of cumulative evidence" (*Federal Rules of Evidence*, 2015, p. 6; cf. Haack 2014, pp. 39–52, 78–103; Strong, 1992, pp. 340–341). These non-epistemic considerations have no parallel in survival research, but they constrain the application of legal evidentiary standards in both civil and criminal law. Moreover, many other rules further constrain what counts as evidence for fact finders. For example, testimony, to which survivalists often appeal, is subject to many constraints in legal proceedings – for example, admissibility rules, the hearsay rule, and the requirement of cross-examination. There is no parallel in survival research to these or other rules that govern procedures aimed at judicial outcomes. Hence, survivalist appeals to standards that presuppose such rules are implausible, if not incoherent.

Bigelow and Kelleher (2022) offered a justification for the Contest's reliance on the beyond-reasonable-doubt standard, but they failed to acknowledge the longstanding debate in jurisprudence concerning what the standard actually measures – for example, the fact-finder's degree of belief, mathematical probability, or degree of warrant (Haack, 2014, pp. 16–23, 50–77). It is counterproductive to rely on an unclear standard to make a clear case for survival, especially when the obscurity surrounding the standard in its legal context is resolved by protocols – for example, jury instructions – for which there is no analog in survival research. Bigelow claimed to have selected the beyond-reasonable-doubt standard for the Contest since people are familiar with that phrase, and he wanted essay submissions to at least aspire to "the highest standards of evidence possible" (Bigelow & Kelleher, 2022, p. 354). People might be familiar with the phrase, but what matters is what the phrase *means*. The general public is not better qualified to answer this question than legal scholars. Since there is no consensus among the latter, the standard is not transparently the highest epistemic standard available.

What is true and important here is that there is an epistemic *dimension* to legal evidentiary standards and rules. The probative aspect of legal relevance in the *Federal Rules of Evidence,* Rule 401, implies this:

> "Relevant evidence" means evidence having any tendency to make the existence of any fact that is of consequence to the determination of the action more probable or less probable than it would be without the evidence. (*Federal Rules of Evidence*, 2015, p. 6; Strong, 1992, p. 339)

In legal scholarship and practice, relevance and the assessment of the (individual and cumulative) weight of evidence are commonly explicated using more fundamental concepts of reasoning and criteria of evidential support – for example, classical statistics, Bayesianism, likelihoodism, and inference to best explanation (Aitken et al., 2022; Bex & Walton, 2012; Dahlman et al., 2021; Dawid, 2002; Fenton et al., 2016; Haack, 2014; Kaye, 1988; Pardo & Allen, 2007; Strnad, 2007; Tillers & Green, 1988). This reliance on generalizable principles of reasoning is necessary. As Haack once aptly noted, "the law is up to its neck in epistemology" (Haack, 2014, p. 4). And just as "mistaken epistemology can only obscure, and not illuminate, legal issues" (Ibid., p. 29), so also epistemological confusions can only obscure and not illuminate matters related to the inquiry into the truth of the survival hypothesis. In jurisprudence, there has been an enormous amount of literature and healthy debate concerning epistemology. No parallel exists in connection with survival research in general or the Contest in particular. In the latter case, this is ironic. The designers of the Contest and many of its prize-winning essayists pretended to deploy a legal evidentiary standard, but they failed to understand that they were *ipso facto* neck deep in epistemology. This requires giving at least as much attention to well-established general theories of evidence as we routinely see in jurisprudence and other areas of scientific and non-scientific inquiry (see endnote no. 59). But there was precious little of this in the Contest or its winning essays. Some essayists even derided such efforts as overly academic, abstract, and subjective (Nahm, 2021, pp. 59–60; cf. Kelly, 2016, p. 593).

The survival debate is, to repeat Haack's phrase, "up to its neck in epistemology." The Contest had the poten-

tial to advance the survival debate at this juncture. This might have even proven to be the only sensible parallel to jurisprudence and scientific reasoning. But the Contest and its essays failed. More broadly, the failure of contemporary survivalists and their nearest sympathizers to redress their conceptual errors remains one of the more disappointing characteristics of the contemporary survival debate, as well as one of the more formidable obstacles to advancing that debate. Against this background, the debate between Keith Augustine and Stephen Braude and his coauthors was an important opportunity for paving a new path.

### *The Augustine-Braude et al. Exchange*

In the introduction to the subsection of the summer 2022 issue of the *JSE*, Editor-in-Chief James Houran explained that he commissioned Keith Augustine to provide a critical analysis of prize-winning essays in the Contest, specifically to "evaluate their quality of reasoning and consistency of evidence" (Houran, 2022, p. 349). He did not ask him to comment on the broader survival debate or to provide arguments against the survival hypothesis. Braude and his coauthors were supposed to "provide counterarguments to Augustine in their Commentary" (Ibid., p. 349). Although Braude et al. conceded that Augustine offered some fair criticisms of the Contest, for the most part, they were unimpressed with Augustine's critique. They dismissed it as a conceptually unsophisticated and empirically uninformed recycling of old skeptical arguments. In his reply, Augustine accused Braude et al. of losing sight of the central question he was addressing, which was "whether the critiqued essays met their directive to provide 'hard evidence beyond a reasonable doubt' of the survival of human consciousness" (Augustine, 2022b, p. 412). So, Braude et al. "failed to confront the critique with counterpoints (or concessions) responsive to its general criticisms" (Ibid., p. 413).

We might suppose that this outcome is unsurprising. Augustine has been a sharp critic of empirical survival arguments for many years, whereas Braude has long been sympathetic to the case for survival – for example, Braude has emphasized the difficulty of explaining away the best demonstrations of mental mediumship, whether through conventional or exotic counterexplanations. However, Braude has also offered poignant criticisms of survival arguments (Braude, 2003, pp. 1–30; Braude, 2021a), including those he presented in his own prize-winning BICS essay (Braude, 2021b). So, the exchange between Augustine and Braude et al. had the potential to advance the survival debate in some interesting ways. Regrettably, it did not.

Augustine provided a very thorough and lucid critique of several of the prominent prize-winning BICS essays. He showed why they failed to meet their objective, and he did this with transparency concerning his own epistemological assumptions – for example, his appeal to the Surprise Principle as a widely accepted standard of evidential support. In their reply to Augustine, Braude and his cohorts provide familiar survivalist rejoinders to prosaic skeptical assessments of the ostensible evidence for survival. While these rejoinders have some merit in the broader survival debate, their relevance to Augustine's critique is questionable at best.

There are four general problems in the Braude et al. reply.

First, there is a **calibration** problem. While Braude et al. correctly identified problematic assumptions and inferences that have historically characterized certain forms of skepticism about survival and the paranormal, their response was not adequately calibrated to address Augustine's *specific arguments*. Augustine's primary objective, which he repeatedly stated, was to show why the arguments for survival in the BICS essays he examined did not succeed in proving what they claimed to prove. Braude et al. repeatedly lost sight of this specific goal and the arguments Augustine was presenting. In places, they recontextualized the discussion as a debate about the evidence for the paranormal, whereas Augustine's critique was focused specifically on alleged evidence for survival. And when Braude et al. focused on survival, they framed their points in a way that was not adequately sensitive to Augustine's arguments or the survival arguments he was evaluating.[1] As a result, they saddled Augustine with extraneous assumptions, as well as made inappropriate demands that he should provide evidence for claims that he either did not make or which would, at best, be tangential to his main arguments. Augustine correctly identified the calibration problem in his reply to Braude et al. (Augustine, 2022b, pp. 412–415, 429).

Second, there is the problem of **marginalized common ground**. There is agreement between Augustine and Braude (sans et al.) concerning the implausibly extravagant claims and remedial errors made in the BICS essays. Also, Augustine and Braude are both skeptical about the alleged "scientific" character of the case for survival, at least in the sense intended by the Contest's design and many of its prize-winning essays. The reply to Augustine, which Braude largely authored, does not acknowledge this common ground, though it was a significant part of Augustine's critique. For example, Augustine took issue with the contention in some of the BICS essays that they were presenting good, if not compelling, *scientific evidence* for survival (Augustine, 2022a, pp. 366–367, 371, 374). He

also took issue with the essayists' contention that they had proved survival beyond a reasonable doubt (Ibid., pp. 367–368, 376), and Braude et al. at least expressed a shared concern about a reliance on this legal standard (Braude et al., p. 399, 401). It is not clear why Braude et al. did not say more about the extent and nature of their agreement with Augustine. Transparency about areas of agreement can help properly dial in areas of substantive disagreement.

Third, there is an **opaqueness** problem with respect to the evidential claim about survival that Braude and his coauthors were trying to support or defend against Augustine. They indicated that there is evidence *in some sense* for survival – that is, there are facts that actually support the survival hypothesis and so provide some reason for thinking that the hypothesis is true. But they are not clear about the nature of this support or how strong they think it is. This opaqueness derives from a lack of clarity about their understanding of evidence – for example, whether it should be understood probabilistically. Braude (sans et al.) has elsewhere argued that, in at least the best cases, the survival hypothesis offers a better explanation of the data than do the usual suspects to which skeptics often appeal. But I find their position on evidence, explanation, and the relationship between them murky. And if this is opaque, it will be hard to identify any points of serious disagreement with Augustine.

Finally, there is the problem of **epistemological neglect**. Braude et al. frequently introduced metaphysical considerations to reinforce points they intended to make against Augustine.[2] However, the crucial issues in Augustine's arguments are epistemological – for example, the principles of evidence on which we must rely to make assessments of the weight of the total evidence. Augustine appealed to the Surprise Principle to determine which of two competing hypotheses the evidence strongly favors. No similar account of evidential principles is found in Braude et al. Like many survivalists, they suggest some kind of connection between explanatory power and evidential support, but the connection remains unclear. Also, silence at this juncture puts Braude et al. at a disadvantage in offering any kind of salient response to Augustine's arguments. His arguments involve a probabilistic understanding of evidence and purport to show, among other things, that "the overall evidence doesn't even make personal survival more probable than not" (Augustine, 2022a, p. 366, 390; cf. Augustine, 2022b, p. 412).[3]

## 2. Problematic Aspects of the Reply to Augustine

We need to take a closer look at the Braude et al. reply to Augustine.

### *Identifying Augustine's Basic Argument*

An initial problem is that **Braude et al. did not provide even a terse summary of what they take Augustine's main argument(s) to be.** For example, what conclusions did Augustine claim to be supporting? What were the premises of his arguments? What kind of support did Augustine claim his premises offer for his conclusion(s)? Regrettably, survivalists have frequently ignored these important contextual, expository matters, as I have previously pointed out in connection with Jim Matlock (Sudduth, 2021a) and Jim Tucker (Sudduth, 2022b). Nonetheless, I was surprised to see this oversight in the Braude et al. reply to Augustine. They immediately launched into a variety of criticisms. This was a premature excoriation. They offered no tie-in between their criticisms and specific aspects of Augustine's argument. The criticisms were offered in a dialectical vacuum. But without the structure of Augustine's arguments in view, we do not know what part of Augustine's arguments the criticisms are intended to target, whether Augustine is really guilty of making the assumptions Braude et al. saddle him with, or how consequential any of their criticisms would be to Augustine's main arguments. So, even if Braude et al. have undermined Augustine's arguments, it is not clear how.

In the opening of their reply, Braude et al. explain the design of their reply:

> Augustine offers many criticisms of the winning BICS entries he selected for discussion, and we cannot assess them all. In fact, we prefer to shelve discussion of the messy particulars in Augustine's selection of essays, thereby sparing the reader from being drenched in minutiae. Besides, there are bigger concerns that take priority. We need to examine major and pervasive deficiencies in Augustine's discussion—for example, his reliance on straw-man or other notoriously unacceptable tactics, his refusal even to *mention* positive evidence, and his failure to realize that there is nothing privileged about the many assumptions he brings to the table. (Braude et al., 2022, pp. 399–400)

Responding to *all* of Augustine's criticisms would be an unreasonable demand, but it is reasonable to expect a critical engagement with Augustine's *main arguments*. But then we need some exposition of those arguments, however abbreviated. Without this expository framework, Braude et al. can only assert, not show, that Augustine's arguments require the "many assumptions" they reference. While we can appreciate the intention to focus on

"bigger concerns," given the task Houran commissioned Braude et al. with, a clear account of Augustine's main arguments ought to have taken priority, especially given how frequently survivalists overlook this fundamental feature of critical analysis. Had Braude et al. provided such, they might have more carefully distinguished between claims Augustine made that were essential to his arguments and those that were tangential. Unfortunately, Braude et al. too frequently focused on Augustine's side remarks. The red herrings prevented a serious engagement with his actual arguments. In fact, they altogether obscure Augustine's arguments.

Augustine presents his main argument in a lengthy section of the paper under the heading *What Does the Total Available Relevant Evidence Tell Us?* (Augustine, 2022a, pp. 371–375). The title alone should make it clear that Augustine's concern is about how the total evidence is weighed, but is there a succinct way of stating his argument? Yes:

[A1] If belief in the survival hypothesis is well-supported,[4] then it is proportioned to all of the available relevant evidence. (Augustine, 2022a, p. 371)

[A2] Belief in the survival hypothesis is not proportioned to all of the available relevant evidence. (Ibid., pp. 371–384, especially pp. 374-375)

Therefore:

[A3] Belief in the survival hypothesis is not well-supported. (Ibid., pp. 365, 390).

I will refer to this as **Augustine's basic argument**. He offers ramified versions of the argument as he adapts it to the "messy particulars" of the BICS essays and their extravagant conclusions. For example, many of the BICS essayists assert that the evidence supports the survival hypothesis *beyond a reasonable doubt*. If subject to normative constraints, this standard entails a *very* well-supported hypothesis (see endnote no. 8 for discussion on the epistemic core to the legal standard). The bulk of Augustine's critique is focused on presenting detailed support for premise [A2] in the light of the very specific claims made in the BICS essays. As I will show, one of Augustine's prominent concerns is that the BICS essayists either ignore or mishandle ostensible counterevidence in a way that undermines the conclusions they wish to draw. Therefore, the BICS essayists have failed to justify the survival hypothesis or the strong evidential claims they make on behalf of it.

Braude et al. do not sketch Augustine's basic argument, nor do they otherwise address his well-advertised concern about how survivalists weigh (or fail to weigh) the total evidence. They criticize Augustine for *his* failing to address certain strands of positive evidence which allegedly provide the strongest support for the survival hypothesis – for example, the amount and consistency of intimate information about the deceased conveyed in some of Mrs. Piper's sittings, and the protocols implemented to detect or obviate fraud. "[Augustine] is mute on the significance of the many times Mrs. Piper got intimate hits with anonymous sitters she was meeting for the first time—including proxy sitters and people who, during the medium's visit to England, happened to be travelling through Cambridge" (Braude et al., 2022, pp. 400–401).

The Braude et al. reply illustrates rather than dislodges the very problem Augustine's arguments are intended to diagnose. Survivalists disproportionately focus on the apparent evidence in *support* of their position and criticize skeptics for failing to do so, especially by propping up outlier cases that seem very difficult to explain away. Augustine's challenge to the survivalist is clear: whatever facts the survivalist wishes to adduce as alleged evidence for the survival hypothesis, the net plausibility of the survival hypothesis requires considering potentially contravening or undermining facts, so-called *negative evidence*. Since survivalists are the ones making the affirmative claim, they must explain how *they* weigh the total (confirming and disconfirming) evidence. The issue is not whether some fact or other is by itself strong positive evidence for survival, but whether the total evidence strongly supports the survival hypothesis. It is the cumulative weight of the facts that matters. Hence, one cannot successfully argue a robust case for survival without a conscientious handling of ostensibly negative evidence.

The issue of how disconfirming or defeating evidence impacts assessments of the total evidence is especially relevant to the Contest's essays. Many of them attribute to the survival hypothesis an extremely high net plausibility, but disconfirming evidence might undermine such a strong inference while leaving more modest inferences intact. Braude et al. do not acknowledge this nuanced but crucial point. Although I will later comment on the "positive evidence" to which Braude et al. allude, the idea that Augustine *needed* to mention or address the "positive evidence" either misconstrues his argument or imposes an unnecessary requirement for its cogency. First, his argument assumes the evidence for survival presented in the BICS papers, and this includes Braude's own BICS essay, which includes the positive evidence in question. Second, the strength of the best evidence for survival depends in part on the comparative force of the negative evidence. Misunderstanding Augustine's argument at this juncture results in illicitly shifting the burden of proof. As a result, Braude et al. do not critically engage Augustine's basic ar-

gument or any of the sub-arguments he presents in support of premise [A2].

**One of the more serious consequences of the Braude et al. failure to consider Augustine's basic argument is that the reader receives no survival-friendly account of the kinds of criteria that bear on net plausibility assessments.** This, of course, was one of Augustine's criticisms of the BICS essays. After all, it would be important to know whether the survival debate is stovepiped at this juncture because (i) survivalists and skeptics have different criteria of evidence assessment which underwrite their respective net evaluations or (ii) they differently apply the same evidential criteria. Moreover, there are important questions we can ask about Augustine's basic argument. How should we best understand the idea of proportioning a belief to all the relevant available evidence? What are the appropriate criteria for weighing different strands of evidence? How should a cumulative case survival argument be formulated? I would like to have seen Braude et al. address these issues. They did not do so. To this extent, the reply to Augustine exemplified the same epistemic blind spot to which Augustine drew attention in his critique of the BICS essays.

### *Missed Opportunities and Conceptual Opacity*

While Braude et al. suggested that the BICS essays may not represent the best that contemporary survival research has to offer, **it is unfortunate that they did not more firmly acknowledge and clearly identify what they regard as the significant flaws in the essays.** They give Augustine a begrudging nod in the opening of their paper for identifying "some areas of concern" (Braude et al., 2022, p. 399), but most of what they tersely mention is low-hanging fruit and concern the design of the Contest rather than the content of its prize-winning essays. To be fair, they did offer a few tepid cautions to survivalists (Ibid., p. 403, 407), including the concession that the Contest "did not discover or create an authoritative consensus about what the 'best' evidence is, much less clarify the principles by which ostensible survival evidence should be evaluated" (Ibid., p. 399). This is an understatement. The crucial epistemological issues were not even on the radar of most of the winning essays. Worse, dubious substitutes created the illusory appearance to the contrary – for example, reliance on purported legal evidentiary standards, assigning schoolish letter-grades or scorecards to index unconstrained subjective impressions about the quality of data, unwarranted inferences based on contentious models of statistical significance, and opaque, underdeveloped, and question-begging deployments of inference to the best explanation. I would like to have seen a more honest, survival-friendly concession to the failures of the Contest's essays, something similar to what Braude has offered in previous publications, including his own prize-winning BICS essay (Braude, 2021b, pp. 4–11, 29–32).

**Braude et al. also did not redress the defects in the BICS essays that they themselves tepidly acknowledge.** But a salient response to Augustine required this, either by shoring up the *specific* survival arguments Augustine was critiquing or by offering new arguments that would be immune to his criticisms – that is, if they were interested in showing that there is a case for survival better than the ones presented in the BICS essays. For example, Augustine's critique often targeted the survivalist's contention to be offering good *scientific* evidence for survival. Braude and his cohorts conceded that parapsychological phenomena are not susceptible to ordinary empirical testing (Braude et al., 2022, p. 405), and Braude has elsewhere argued that neither psi nor survival are open to the kind of falsification that characterizes scientific hypotheses (Braude, 2003, pp. 16–19, 300). So, Braude appears not to agree with the more extravagant claims made in many of the BICS essays. What then is the survival argument Braude et al. envision that is an improvement on the arguments presented in the BICS essays under examination but also immune to Augustine's criticisms, including the criticisms Braude et al. regarded as "reasonable" (Braude et al., 2022, p. 399)? They do not say, they do not show, and consequently, the reader does not know. This is especially odd since Braude's own prize-winning BICS essay provided resources for outlining such a case. I shall explore this in due course.

**Braude et al. exacerbate the above problem by failing to clarify the favorable evidential claim they wish to endorse on behalf of the survival hypothesis and then contrast it with the evidential claims Augustine doubts or denies.** Braude et al. suggest that the BICS essays do not represent the best that survival research has to offer, but they seem to think *there is some sort of a case for survival that is better than what Augustine is willing to concede*. Of course, there being such a case is consistent with the Contest's essayists failing to make that case. This is true even if, contrary to the contentions made in many of the prize-winning essays, survival is not a scientific hypothesis, or the evidence is not as strong as the BICS essayists claim. So, what evidential claim about survival is weaker than what the overly ambitious BICS essayists assert but stronger than what Augustine is willing to concede? This Goldilocks evidential threshold is not transparent in reading Braude et al., but I will later try to identify it by looking at Braude's own work on survival. Nonetheless, it is a shortcoming of the reply to Augustine

that Braude et al. did not clarify the evidential claim they wish to defend nor state the evidential principle(s) that would justify that claim. Without this kind of clarity, we cannot say with any reasonable assurance whether the case for survival is better than Augustine thinks it is.

The previous point is especially important because Augustine clearly stated an important principle of evidential support – the Surprise Principle – on which he relied for his evaluation. He also made it clear why net evaluations require a conscientious handling of negative evidence, and why the BICS essays failed at this juncture. In his view *the evidence as presented in the BICS essays does not make the survival hypothesis more probable than not, much less highly probable, and still less proven to be true beyond a reasonable doubt*. As far as I can see, Braude et al. neither say nor imply anything to the contrary. Nor do they say anything that undermines Augustine's probabilistic assessment. In fact, Braude et al. make no attempt to critically engage Augustine's probabilistic reasoning. I will later provide several examples of how their criticisms miss the mark on account of this omission.

### *Dialing in the Braude et al. Position*

The opaqueness problem and the closely allied problem of epistemological neglect hamstring the assessment of Augustine's BICS critique. To see why this is the case, we need to map out the wider conceptual territory in which the BICS essays and the Augustine-Braude et al. exchange are embedded. We need to consider the main claims survivalists have made. This will also help dial in the potential area of genuine disagreement between Augustine and Braude et al.

Survivalists have made at least seven different claims based on the kinds of observational data – ostensibly paranormal phenomena – that are the focus of the BICS essays:

(1) The observational data logically demonstrate the survival hypothesis.
(2) The observational data prove the survival hypothesis beyond a reasonable doubt.[5]
(3) The observational data prove the survival hypothesis by a preponderance of the evidence.
(4) The observational data show that the survival hypothesis is probably true.[6]
(5) The observational data are evidence[7] that the survival hypothesis is true.
(6) The observational data favor the survival hypothesis over alternative hypotheses.[8]
(7) The survival hypothesis is the best explanation of the observational data.

(1)-(7) are distinct though potentially related claims. Survivalists have frequently failed to distinguish them. Indeed, they often uncritically conflate them – for example, survivalists routinely conflate (5) and (7). But depending on which of the above claims one intends to justify, different principles of evidential support will be relevant, and the corresponding supporting arguments will also differ. This is also true of the arguments deployed to justify *denying* any of the above claims, as well as arguments that purport to show that survivalist arguments in support of these claims lack cogency. The matter is further complicated by the potential to combine or logically connect some of the claims above – for example, (4), (5), and (6) are often combined with or connected to (7). Sadly, survivalists often exhibit little more than a remedial grasp of these important conceptual distinctions. Consequently, their responses to skeptics are vitiated by the same lack of clarity and flawed reasoning which characterizes their attempts to argue in favor of the survival hypothesis.

It seems that Braude (at least sans et al.) does not affirm (1), (2), or (3). It looks like he shares Augustine's skepticism about these stronger claims. But Braude et al. do accept (5). Of course, without further elucidation that claim is exceedingly modest. It is unsurprising that Augustine does not deny (5). It is less clear what Braude et al. would say about (4). They do not claim or argue that survival is more probable than not, the lower bound for (4), nor do they attempt to defend such a claim. So, I am inclined to think they are not committed to (4), unless the term "probable" just means subjective credence. But this would be an unremarkable and uninteresting claim in the context of the BICS papers and the survival debate in general. No one denies how firmly survivalists believe the survival hypothesis or how firmly they believe the data support it. What is at issue is the actual probative value of the data and how it *ought* to be assessed as evidence.

That leaves us with (6) and (7). Although Braude et al. do not explicitly affirm it in their reply, Braude (sans et al.) has elsewhere argued that, with respect to data in the better cases, the survival hypothesis offers a better explanation than do rival hypotheses (Braude, 2003, 2021b). So Braude has argued something like (7), partly on the basis of (6), at least where the data are narrowly circumscribed to facts which are allegedly most resistant to non-survival counterexplanations. I will subsequently look at Braude's own reasoning in greater detail, but it is worth noting here that Braude et al. frequently refer to the deficiencies of alternative non-survival explanations of the data, specifically Augustine's alleged failure to address these shortcomings. Since ruling out alternative explanations is a crucial step in inference-to-best-ex-

planation arguments, Braude et al. at least appear to be defending a nuanced version of (7), and probably (6). This potentially puts Braude in opposition to Augustine, who is clear why the survival hypothesis fails as a serious explanation of anything. Unfortunately, since Braude et al. do not calibrate their answers in a way that addresses Augustine's very specific, probabilistic arguments, the precise nature of the disagreement between them remains obscure. In the next two sections, I will explain where I *think* the disagreement lies.

## 3. What is the Question? Honing in on the Actual Disagreement

To better diagnose the above problems, we need to clearly distinguish between different kinds of questions lurking in the Contest and in the wider survival debate. This can help clarify the plausible point of genuine disagreement suggested above. But the discussion is of broader significance. One of the ubiquitous problems in survival literature is the failure to distinguish between different kinds of epistemological questions we can ask about the survival hypothesis.

Here is one such question:

(I) Is it rational (or reasonable) to believe in survival on the basis of the kinds of phenomena discussed in the BICS essays?

Braude has provided a compelling argument in his corpus of publications that the answer to question (I) is *yes*, at least if we have the strongest cases in mind. For example, in his prize-winning BICS essay, Braude proposed to answer the question "whether there's sufficient evidence for, and a rational basis for belief in, the survival of bodily death," or whether there can be a "rationally defensible basis" for belief in survival, and he is confident that there is (Braude, 2021b, pp. 1–2; cf. Gauld, 1982, p. 263; Stevenson, 1969). Similarly, in his book *Immortal Remains*, Braude concluded that "the evidence provides a reasonable basis for believing in personal postmortem survival" (Braude, 2003, p. 306). But question (I) clearly is not the question in dispute in the present context. Augustine agrees that there is a sense in which belief in survival can be rational (Augustine, 2022a, p. 390).[9] Moreover, the Contest's essayists, other than Braude, make much stronger claims, and the stronger claims are the focus of Augustine's critique.

We can also ask:

(II). Do the kinds of phenomena presented in the BICS essays provide evidence for survival?

One might suppose that if the answer to (I) is yes, the answer to (II) should also be yes. If evidence is a constraint on rational belief, this is correct. But an affirmative answer to (II) is otherwise plausible. Some accounts of evidence are liberal enough to permit there to be evidence for empirical propositions, however improbable the propositions are. For example, according to Bayesian incremental confirmation, O is observational evidence for a hypothesis H just if O raises the probability of H. But an increase in probability need not make a hypothesis probable – that is, at least more probable than not; the hypothesis could still be improbable. In §5 I will explore Bayesian confirmation and other accounts of evidence in greater detail. But the thing to note here is that (II) is not the question in dispute. To their credit, neither the Contest nor the majority of the Contest's essays are interested in the low-hanging fruit of *mere evidence* for survival.

A third question:

(III) What ostensible evidence is the best evidence for survival?

(III) was built into the design of the Contest: "The question that contest authors attempted to answer in no more than 25,000 words was: What is the Best Evidence for Survival of Human Consciousness After Permanent Bodily Death?" (Bigelow & Kelleher, 2021, p. 351). Braude's BICS essay was a response to this question. He identified a subset of data from mediumship – for example, the mediumship of Mrs. Piper – as the best evidence for survival. Of Mrs. Piper's mediumship, Braude said: "I consider it the strongest case we have for survival, and I'd say no other body of evidence comes close" (Braude, 2021b, p. 29). He concluded that the evidence here provides a "rational basis for belief in the survival of bodily death" (Ibid., p. 3). But perhaps not a very firm belief. He ends his essay by saying, "even if the best actual evidence doesn't warrant a reassuring confidence in the reality of survival, at the very least it encourages optimism on the matter" (Ibid., p. 52). This is much weaker than survivalist claims in the other BICS essays.

To be clear, Braude's arguments in his BICS essay exhibit a level of conceptual sophistication absent from much of the literature, including the majority of the other winning BICS essays. But his conclusions about the evidence are modest in comparison to claims made by more strident survivalists. The best evidence, like the best explanation, is often the best of a bad lot. And given just how weak the rest of the evidence is, even by Braude's own lights, it seems premature to pop a celebratory evidential cork. Even a non-survivalist can accept that there

is some evidence for survival, that some of the evidence is much better than all the rest, and that the best evidence provides a reasonable basis for belief in survival. Owing to the modesty of Braude's claim, it is not surprising that even Augustine can accept it (Augustine, 2022a, p. 390).

By contrast, **Augustine's critique is focused on stronger claims made on behalf of the survival hypothesis and the evidence adduced in support of it** – for example, that the evidence makes the survival hypothesis more probable than not, highly probable, that the evidence is good scientific evidence, or that it meets the legal standard of proof beyond a reasonable doubt. Although these claims differ in important ways, they each entail that the evidence for survival confers a *strong* positive epistemic status on belief in survival. These stronger claims litter the field of pro-survival literature, as well as the BICS essays. These claims and the arguments for them are the main target of Augustine's critique.

So, we can also ask the following two related questions:

(IV). Does any of the ostensible evidence for survival presented in the BICS essays confer a strong positive epistemic status on belief in the survival hypothesis?

and

(V). Do the BICS essays successfully *show* that the evidence they present confers a strong positive epistemic status on belief in the survival hypothesis?

The answer to (V) can be no, while the answer to (IV) is yes. Augustine's main argument supports a negative answer to question (V). But in places he uses this argument as a springboard to talk about the survival evidence as a whole, independent of the BICS essays and their authors. After all, if the Contest represents the best that survivalists have on the evidential tap, or something approximating it, there is some justification for supposing that a negative answer to (V) provides at least modest grounds for a negative answer to (IV). This can also be independently argued, as I will later show.

I do not see that Braude et al. offer any reasons for an affirmative answer to (IV) or (V), so they are not obviously denying Augustine's conclusion. And as I will subsequently show, the issues they do raise are not properly calibrated to address Augustine's reasons for supposing that the answer to (IV) or (V) is no, so they are also not undercutting Augustine's argument. For these reasons, it is difficult to see how they are offering anything to counter Augustine's arguments.

That said, Braude makes an atypically stronger statement at the outset of his prize-winning BICS essay. After denying that we have a *proof* of survival, Braude writes:

> But empirical claims never enjoy that degree of certitude, and yet we can still have good reasons for believing many things that nevertheless remain vulnerable to possible revision or subsequent rejection. So what participants in the survival debate need to consider is something more modest than a slam-dunk proof—namely, whether there's sufficient evidence for, and a rational basis for belief in, the survival of bodily death. (Braude, 2021b, p. 1)

Braude concludes the same essay by saying:

> So, we've seen that one can have legitimate and defensible reasons for concluding that some form of postmortem existence can occur... So even if the best actual evidence doesn't warrant a reassuring confidence in the reality of survival, at the very least it encourages optimism on the matter. Confidence will have to come later, if it comes at all. (Ibid., p. 52)

Whatever Braude means by "legitimate and defensible reasons", the phrase injects considerable modesty into what Braude says here. Most skeptics would agree that one *can* have defensible reasons for concluding that some form of postmortem existence *can* occur. Similarly, someone *can* have defensible reasons for concluding that God *could* exist, that the universe *could* be a simulation, or that Oumuamua *could* be debris from an alien spacecraft. Skeptics can also agree that the best evidence does not warrant a reassuring confidence in the reality of survival. But these concessions are considerably more modest than the sufficiency-of-evidence target Braude affirms at the beginning of his essay. It is unclear whether Braude's concluding comments are intended to be an intellectual settlement which falls short of that target, or if he thinks that his explanatory considerations are sufficient evidence for survival. If the latter, then we have a point of substantial disagreement with Augustine.

In reflecting on Braude's comments above, I think it would be helpful to invoke an important distinction statistician Richard Royall has made. With respect to evidence and hypothesis testing, he distinguished between three questions:

1. What do I believe, now that I have this observation?
2. What should I do, now that I have this observation?

3. What does this observation tell me about A versus B? (How should I interpret this observation as evidence regarding A versus B?) (Royall, 1997, p. 4).

Questions 1 and 3 are the relevant ones for my purposes. It is easy to conflate the *belief question* and the *evidence question*, perhaps because the former presupposes the latter if we desire our belief states to be informed by what the evidence says. However, when it comes to assessing criteria of evidential support, we should be prepared to acknowledge criteria that are very useful for answering question 3 but which are not intended to answer question 1. Braude is correct that participants in the survival debate need to consider something more modest than a slam-dunk proof. However, they also need to consider something more modest than whether there is evidence sufficient to warrant *belief* in survival or whether the evidence makes it reasonable *to believe* in survival. These questions are important, but it may be useful for participants in the survival debate to temporarily sideline questions about belief and focus instead on what the evidence says. After all, the evidence may have something important to say, even if it does not tell us enough to answer the belief question. I will further develop this in §5 and §6.

## 4. Inference to Best Explanation

As previously indicated, Braude has argued that the survival hypothesis can be shown to have an explanatory advantage over alternative explanations if we provide a proper analysis of important features of the better cases. For example, the amount and consistency of veridical claims that emerged in many of Mrs. Piper's mediumistic sittings, as well as her extended and accurate trance personae. Augustine demurs. So, the most plausible point of disagreement between Augustine and Braude et al. seems to lie in their respective assessments of the explanatory power of the survival hypothesis and the evidential cash value of its alleged explanatory merits.

To unpack this, we need to ask a different question than the previous ones:

(VI). Does the survival hypothesis provide the best explanation of data drawn from the ostensibly paranormal phenomena discussed in the BICS essays?

Reasons for an affirmative answer to question (VI) constitute an explanatory or inference-to-best-explanation (IBE) survival argument. Not only is this kind of argument prominent in the BICS essays, it is ubiquitous in the wider body of survival literature, both historically and among contemporary writers. IBE survival arguments are typically deployed to underwrite the claim that data from mediumship, cases of the reincarnation type, etc., provide evidence for the truth of the survival hypothesis.[10] This is because many, if not most, survivalists who have construed the case for survival as an IBE argument have been *explanationists*. They have believed that a hypothesis' providing the best explanation of some data constitutes *evidence* that the hypothesis is true, or that this otherwise provides an epistemic justification for belief in the hypothesis (Almeder, 1992, pp. 61–62; Griffin, 1997, pp. 263–268; Lund, 2009, pp. 215–218; Paterson, 1995, pp. 189–190). So, an affirmative answer to (VI) is often the basis for an affirmative answer to questions (II)–(V) in §3. And it appears that at least some survivalists also think that an affirmative answer to (VI) provides an answer to Royall's first question – what should we believe?

A brief digression on IBE survival arguments is warranted before looking more closely at Braude's view. I have offered a variety of criticisms of these arguments over the years (Sudduth, 2009, 2013a, 2013b, 2016), and the criticisms are worth restating here. Also, several of the flaws in the BICS essays are linked to their mishandling of IBE arguments and implausible attempts to leap from the presumed explanatory power of the survival hypothesis to strong evidential claims. Finally, an analysis of the issues associated with IBE survival arguments might better illuminate where Augustine and Braude et al. are in genuine disagreement.

The generic form of the IBE survival argument can be represented as follows:

(1) $O_1$, $O_2$, ..., $O_n$ are observations in need of explanation.

(2) The survival hypothesis S explains $O_1$, $O_2$, ..., $O_n$.

(3) No available competing hypothesis R explains $O_1$, $O_2$, ..., $O_n$ as well as S does.

Therefore:

(4) The survival hypothesis S is the best available explanation of $O_1$, $O_2$, ..., $O_n$.

So (probably):

(5) The survival hypothesis S is true.

{$O_1$, $O_2$, ..., $O_n$} are placeholders for the relevant observational data, whether culled from mediumship, cases of the reincarnation type, out-of-body and near-death experiences, haunting and poltergeist phenomena, or some other ostensibly paranormal phenomena. Premise (2) affirms the explanatory power of the survival hypothesis over such data, and premise (3) denies that any of the available rival hypotheses does at least as well as the

survival hypothesis in explaining the data. Although (4) is the intermediate explanatory conclusion, most survivalists who endorse the IBE survival argument infer (5) from (4) as the final conclusion of the argument. So, it is natural that survivalists should also regard IBE arguments as providing grounds for believing in survival.

I have elsewhere discussed several intractable problems that infect IBE survival arguments (Sudduth, 2013a, 2013b, 2016). Since I will return to this in connection with Augustine's criticisms of the BICS essays, let me lay my skeptical cards on the table. To date, no IBE survival argument has succeeded, and the prospects for future success or advancing the survival debate look pretty bleak. Simply stated, traditional IBE survival arguments are self-defeating (Sudduth, 2016, chapter 11, especially pp. 286–289). The reasons survivalists routinely offer to justify premise (3) undermine the justification for premise (2). Fixing this problem undercuts the traditional survivalist justification for premise (3). But if the reasons for accepting (3) undermine the justification for accepting (2), and conversely, then we are not presently justified in accepting both (2) and (3). Therefore, we are not justified in accepting (4) on the basis of (2) and (3).[11] That is my central argument against IBE survival arguments succinctly stated in five sentences.

My other reservations about IBE survival arguments stem from very general considerations about the difficulty of inferring the (probable) truth of a hypothesis from its explanatory merit (Lipton, 2004, pp. 151–163). This blocks the inference from (4) to (5). The best explanation may be the best of a bad lot of explanations (van Frassen, 1989, p. 143). To circumvent this difficulty, we must suppose that the true explanation is among the candidate explanations, but that is what the inference to the best explanation was designed to do in the first place. Also, we must assume that explanatory considerations convert to evidential cash value and probabilify the target hypothesis. Even so, the best explanation might still be an improbable one. The best explanation would be more probable than the alternatives, but if each of the alternatives has a very low probability, the best explanation is only more probable than improbable alternatives, which is consistent with the best explanation itself being improbable. Moreover, even if our set of hypotheses $\{H_1, H_2, H_3\}$ is exhaustive and so includes the true explanation, we cannot conclude that the best explanation $H_3$ is true because, though $H_3$ may be more probable than $H_1$ and more probable than $H_2$, it is not more probable than the disjunction *either* $H_2$ *or* $H_3$ (McCain & Poston, 2024).

There are different ways of troubleshooting the above problems. The point here is that survivalists who deploy IBE survival arguments do not even acknowledge these problems. Consequently, they are ill-positioned to formulate IBE survival arguments that have some degree of immunity to these criticisms. For example, one can mitigate the general philosophical difficulties above by adding premises to the generic IBE argument or by merging IBE and Bayesian probability (see §5). As I have argued elsewhere (Sudduth, 2009, 2013a, 2013b, 2016), I doubt that survivalists can successfully leverage these maneuvers to defend IBE survival arguments. But if survivalists do not understand and acknowledge the problems, they are unlikely to advance the debate with new ideas and improved arguments. In fact, they may altogether fail to grasp criticisms of survival arguments that presuppose this conceptual territory. This hamstrings their ability to offer informed responses to skeptical objections. Unfortunately, like people who ignore safety recall notices for their cars, survivalists continue to press the accelerator on arguments that have been recalled.

### *Where are Braude et al. in This Landscape?*

Braude (sans et al.) has argued that the survival hypothesis has explanatory advantages over non-survival alternatives, at least when it comes to the best cases. In Braude's view, this is because the abundance and consistency of verified information in such cases, specifically mediumship and cases of the reincarnation type, is more difficult to reconcile with non-survival explanations (Braude, 2003, pp. 216–222; 2021b). The alleged explanatory power of the survival hypothesis underwrites Braude's claim that there is a reasonable basis for belief in survival (Braude, 2003, p. 306).

Some selections from Braude (2003) highlight the connection he has drawn between evidence, explanatory mileage, and a reasonable basis for belief in survival:

> Of course, it's philosophically momentous to conclude that there's satisfactory evidence for some sort of postmortem survival.... My aim, here, is to examine carefully the best types of evidence for survival and to see how successfully they resist explanation in terms of unusual (and possibly paranormal) capacities of the living. (Braude, 2003, p. xiv)

> My case selection was guided by my primary objective in this book: to determine whether there's any reason for preferring a survivalist explanation of the evidence over explanations positing exotic (including paranormal) activities among the living.... We need to examine good cases *very* carefully to decide whether the sur-

> vival hypothesis succeeds where its rivals fail. (Ibid., p. xv)
>
> And I think we can say, with little assurance but with some justification, that the evidence provides a reasonable basis for believing in personal postmortem survival. It doesn't clearly support the belief that everyone survives death; it more clearly supports the belief that some do. And it doesn't support the belief that we survive eternally; at best it justifies the belief that some individuals survive for a limited time. (Ibid., p. 306)

As these passages show, Braude thinks the survival hypothesis is *explanatorily successful*, even if marginally so, at least when it comes to the best cases. In his view, the better cases have features, previously noted, that are difficult to explain away either by conventional or exotic counterexplanations. Moreover, the explanatory merits of the survival hypothesis over rival hypotheses imply that the data it explains should be regarded as *evidence* that provides a basis for a *reasonable* or *justified* belief in survival.

Where S = the survival hypothesis and O = the salient observational evidence, we have the following inferential schema:

S is *explanatorily successful* over O. → O is *evidence* for S. → It is *reasonable* to believe S.

Braude should be regarded as an explanationist. However, to his credit, he does not endorse the following implausible inference:

(4) The survival hypothesis S is the best available explanation of $O_1$, $O_2$, ..., $O_n$.

So (probably):

(5) The survival hypothesis S is true.

Instead, he seems to endorse an inference from (4) to

(5′) The explanatory success of the survival hypothesis is *evidence* for the truth of the survival hypothesis.

Then a subsequent inference from (5′) to

(5″) The explanatory success of the survival hypothesis is a *reasonable* basis for belief in postmortem survival.

While (5′) and (5″) are more modest than (5), they still raise several issues that need addressing. I am particularly interested in (5′) since it is an evidential claim inferred from an explanatory claim. This inference is unclear and potentially contentious. It needs further unpacking. We need to review some general points related to the concept of evidence and outline some well-established and widely deployed criteria of evidential support. This will illuminate why Braude's inference from (4) to (5′) is implausible unless augmented with principles from confirmation theory. But it is otherwise important to map out different criteria of evidential support and how they can function in a robust epistemology of survival arguments. These issues will also play an important role in my subsequent analysis of the Augustine-Braude exchange.

## 5. Confirmation Approaches to Evidential Support

I previously noted Richard Royall's distinction between two important evidence-related questions: *What should you believe? What does the present evidence say?* It is time to look at this distinction more closely in relation to different ways of thinking about evidential support, as well as tie them to the questions canvassed in §3. This is something of an outline on salient concepts in epistemology relating to criteria of evidential support.

### *Evidential Justification*

Evidence plays an important role in justifying beliefs, but not all evidence is sufficient to justify a belief. A female student enters the Philosophy Department student lounge carrying a copy of Descartes's *Meditations on First Philosophy*. This is plausibly evidence that the student is a Philosophy major, but by itself it is not strong enough to warrant believing that she is. Several witnesses report seeing a man matching Brian's description near the location of a homicide about the time of the murder, another witness describes seeing a car similar to Brian's parked near the location of the homicide around the same time, and Brian has no alibi covering the time of the murder. Here we plausibly have evidence that Brian committed the murder, but many people would have the (I think correct) intuition that the evidence is not strong enough to justify believing that he committed the murder.

What degree of evidence is strong enough to justify believing a proposition? Philosophers have given two general answers. First, the evidence should make the proposition at least *more probable than not*. Second, the evidence must make the proposition *highly probable*.[12] A justified belief exhibits a kind of goodness vis-à-vis the epistemic point of view – roughly, the goal of believing what is true and not believing what is false. Where evidence justifies a belief, the evidence must track truth in a particular way. It must put us in a strong position to believe what is true and not believe what is false. This

suggests that for evidence to be good enough to justify a belief that H the evidence must be sufficiently *indicative* of the truth of H and sufficiently *discriminative* between H and not-H. This is a robust account of evidential justification. In the above examples, the evidence is plausibly indicative of the truth of the belief. After all, if H were true, we would expect O. But it is not discriminating evidence because it seems implausible in those cases to suppose that if H were not true, then we would not expect O. But it is easy to consider how additional observations would alter this. For example, another Philosophy major might say the student is in two of his upper-division Philosophy classes, classes typically reserved for Philosophy majors, and that she has been attending Philosophy Club meetings since the beginning of the semester.

Although having evidence that makes a hypothesis more probable than not is an important epistemic desideratum, having evidence below this threshold can also be epistemically significant. For example, evidence can still raise the probability of a hypothesis, even if it does not confer a very high probability on it, and the cumulative effect of multiple instances of "raising the probability of H" may eventually raise the probability of H high enough to justify believing H. But evidential support can be construed otherwise. Instead of evidential support involving observations that raise the probability of a hypothesis, observations may discriminate between two competing hypotheses by favoring one of them over the other. My impression is that survival literature has not properly distinguished between the latter contrastive view of evidential support as a favoring relation and the prior non-contrastive view which interprets evidential support as boosting the probability of a hypothesis, ideally above some threshold value.

We can draw on confirmation theory to develop the distinctions introduced above and see how they bear on Royall's distinction.[13] I will subsequently show how they are woven into the fabric of the debate between Augustine and Braude et al., as well as how they bear on the wider survival debate. To reiterate what I said earlier in the paper, the theories and principles of evidence I outline below are non-domain specific and widely deployed across scientific and non-scientific disciplines. They also crop up, often opaquely, in survival literature.

### *When Does an Observation Favor One Hypothesis Over Another Hypothesis? The Law of Likelihood*

(LL) Observation O favors hypothesis $H_1$ over hypothesis $H_2$ if and only if $Pr(O \mid H_1) > Pr(O \mid H_2)$.

(LL) states the law of likelihood (Edwards, 1972; Royall, 1997; Sober, 2008). It parses evidential support in terms of a *favoring* relation between some observation(s) and two contrasting hypotheses. It tells us that observation O favors $H_1$ over $H_2$ just if $H_1$ confers a greater probability on the observation than does $H_2$. In other words, $H_1$ leads us to expect O more than $H_2$ does. $Pr(O \mid H)$ formally expresses **the likelihood of H**. I use the word "likelihood" here (and throughout) in the technical sense coined by R.A. Fisher to refer to the probability *of the observation* given the hypothesis. This should be distinguished from the probability *of the hypothesis* given the observation, formally $Pr(H \mid O)$, also called the posterior probability of H. (LL) uses likelihood inequalities to establish when an observation favors one hypothesis over another.

Technically, (LL) is the first part of the law of likelihood. The second part tells us:

(*) The degree to which O favors $H_1$ over $H_2$ is given by the likelihood ratio $Pr(O \mid H_1)/Pr(O \mid H_2)$.[14]

Since some survivalists are likely to misunderstand (LL) or its application to the survival hypothesis, let me offer a few clarifications.

First, (LL) does not require that we assign numerical values to $Pr(O \mid H_1)$ or $Pr(O \mid H_2)$. It only requires that the hypotheses be sufficiently contentful to say that some observation is more probable/expected under one hypothesis than it is under another (Sober, 2019, p. 34).[15] Jimmy experienced a sudden onset of intense vomiting and explosive diarrhea a few hours after eating a bacon cheeseburger he purchased from a college food vendor. We do not need to assign numerical values to see that the observation is more probable under the hypothesis that the bacon cheeseburger was contaminated with Salmonella than it is under the hypothesis that Jimmy is upset about receiving a C+ on his Physics exam earlier in the day.

Second, (LL) does not require that either of the contrasting hypotheses *predicts* the observational evidence, either in the sense of making the evidence at least more probable than not or the evidence being novel or previously unobserved. The presence of an accelerant in a house fire is more probable given the hypothesis of arson than it is given the hypothesis of an electrical malfunction, but the arson hypothesis does not predict the presence of an accelerant in either of the previous senses. After all, there are many ways for an arsonist to start a fire without an accelerant. In more extreme cases, an observation can favor one of two contrasting hypotheses, even if the observation is highly improbable under each of the hypotheses. A forensic scientist might observe that two individuals have a particular genetic profile. If the two

individuals are full siblings, the probability of the profile might be $(0.0005)^{20}$, whereas if the two are unrelated, the probability of the profile might be $(0.000001)^{20}$. In this case, the genetic data favor the siblings hypothesis over the hypothesis that the two are unrelated. Although the genetic data are hugely improbable under each hypothesis, they are $500^{20}$ times larger (and hence less improbable) under the siblings hypothesis (Sober, 2008, p. 52; Sober, 2012, pp. 360–361).

Third, neither (LL) nor (*) (when its value is high) tells us anything about the *probability* or *plausibility* of either of the contrasting hypotheses. (LL) only tells us that an observation discriminates between two competing hypotheses – that is, O is evidence in support of $H_1$ as opposed to $H_2$, and O is evidence against $H_2$ in relation to $H_1$ (Royall, 1997, pp. 8–11, 14–15). And notice that the conception of evidence here is *relative*. O is evidence for or against a particular hypothesis $H_1$ only in relation to some other hypothesis $H_2$.

Finally, (LL) does not tell us that we should *believe* $H_1$ or disbelieve $H_2$. After all, even if $Pr(O \mid H_1)$ is much greater than $Pr(O \mid H_2)$, $Pr(H_1 \mid O)$ might be very low, even lower than $Pr(H_2 \mid O)$. Evelyn Marie Adams won the New Jersey State lottery twice in four months (Hand, 2014, p. 86). This observation is much more to be expected given $(H_1)$ God wanted Adams to have money to pay her bills, invest, and pay the educational expenses for her family members than it is given $(H_2)$, the lottery was fair, but $Pr(H_1 \mid O)$ is very low, much lower than $Pr(H_2 \mid O)$. Although the observation favors $H_1$ over $H_2$, it would not be reasonable to believe $H_1$ nor disbelieve $H_2$ on the sole basis of the observation.

### *When Should We Up Our Confidence in a Hypothesis? Incremental Confirmation*

(IC) O confirms H if and only if $Pr(H \mid O) > Pr(H)$.

(IC) codifies a Bayesian view of evidential support called *incremental* confirmation (Fitelson, 2007, 2011; Lin, 2023; Sober, 2002, 2008). Pr(H) refers to the prior probability of the hypothesis – its probability independent of the observation. (IC) tells us that an observation O confirms H just if O raises the prior probability of H. For the Bayesian, confirmation is probability-raising, and disconfirmation is probability-lowering. Hence, O disconfirms H just if $Pr(H \mid O) < Pr(H)$. Bayes' theorem (see below) allows us to extract equivalent definitions of (IC). For example, O confirms H just if $Pr(O \mid H) > Pr(O \mid \sim H)$ – that is, the probability of O given H is greater than the probability of O given not-H. Like (LL), incremental confirmation embeds a likelihood inequality: $Pr(O \mid H)$ and $Pr(O \mid \sim H)$. However, ~H is not a particular alternative hypothesis that contradicts H but the full logical complement of H, that is, the disjunction of all logically possible alternatives to H.[16] Also, as with (LL), specific numerical values are not needed. Incremental confirmation works regardless of the value one assigns to Pr(H), as long as that value is neither 0 (H is impossible) nor 1 (H is certain). Similarly, specific numerical values are not needed for the likelihood inequality $Pr(O \mid H) > Pr(O \mid \sim H)$.

My traffic app says it will take 20 minutes to drive to PetSmart. Suppose I have a low degree of confidence in this since I know PetSmart is only a few miles away. But after I leave my house, I hit heavy traffic on the route due to road construction. This observation raises the probability that the traffic app is correct. What I observe in route to PetSmart is much more to be expected given that the traffic app is correct than it would be if the traffic app were incorrect. My degree of confidence in the traffic app's route time, whatever it was initially, ought to increase in the light of the observational data. In this example, the observation raises the probability of the hypothesis, and it does so without needing to assign specific numerical values to either the prior probability of the hypothesis or the likelihood of H or ~H.

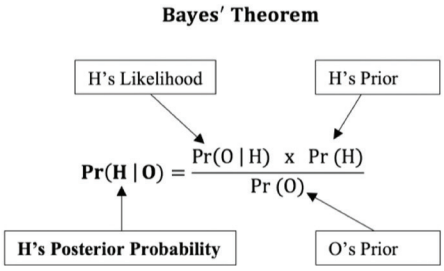
Clearly then, incremental confirmation does not mean *proving* that a hypothesis is true, and disconfirmation does not mean *proving* that a hypothesis is false. Confirmation simply tells us we should increase our confidence in H, and disconfirmation tells us that we should decrease our confidence in H. Moreover, even if O confirms H, $Pr(H \mid O)$ can be low. This is perhaps not true in the traffic app case above. But suppose you hear a rolling, rumbling sound in the attic. The hypothesis that there are gremlins in the attic bowling guarantees the observation, so $Pr(O \mid H) = 1$. But the negation of the gremlin hypothesis does not entail the observation, so $Pr(O \mid H) > Pr(O \mid \sim H)$. Here the observation incrementally confirms the gremlin hypothesis, but the probability of that hypothesis, given the observation, remains very low (Sober, 2008, pp. 10, 22, 37–38).

(LL) answers Royall's question about what the evidence says, whereas (IC) answers Royall's belief question. More specifically, (IC) tells us when we ought to increase or decrease our confidence in a particular hypothesis, or – in the event $Pr(H \mid O) = Pr(H)$ – that we ought to make no changes to our degree of confidence in H. This is because (LL) informs us that evidence discriminates between two competing hypotheses and supports one over the other, whereas (IC) tells us what we need to do with our confidence in a specific hypothesis.[17]

### *How Can We Determine the Net Plausibility of a Hypothesis? Bayes' Theorem and Posterior Probabilities*

(IC) does not tell us whether the probability of H is greater than some alternative hypothesis given the same evidence, nor does it tell us what the overall probability of H is given the evidence. But, incremental confirmation is derived from Bayes' theorem, which does give us the resources for answering further questions about the net plausibility of a hypothesis, technically, the posterior probability of a hypothesis.

#### Bayes' Theorem




Bayes' theorem follows from the axioms of the mathematical calculus of probability. It tells us that Pr(H | O) – the probability of H given O (posterior probability of H) – depends on three values:

- Pr(O | H): the extent to which the hypothesis leads us to expect the observation (H's likelihood)
- Pr(H): the probability of H before O is considered (H's prior probability).
- Pr(O): the extent to which we would expect O whether or not H is true. (O's prior or marginal probability)

As a first approximation, *Bayes' theorem tells us that the probability of a hypothesis depends on the extent to which it leads us to expect those observations which are otherwise not expected, and where the prior probability of H is a weight.* Roughly stated, Pr(H | O) will be high to the extent that the product of the likelihood and the prior (the numerator) is large relative to the marginal probability of the observation (the denominator). Since Pr(O) is shorthand for Pr(O)Pr(O | H) + Pr(~H)Pr(O | ~H), a crucial element in Bayes' theorem is Pr(O | H)/Pr(O | ~H) – the *Bayesian likelihood ratio*. Recall that ~H here refers to the disjunction of all alternatives to H, not to a single hypothesis that contradicts H. So, the Bayesian likelihood ratio differs from the "likelihoodist" likelihood ratio which contrasts a hypothesis and a single alternative hypothesis. The likelihood ratio in the Bayesian context compares how probable the observed datum O is under the hypothesis H relative to the probability of O under all alternative hypotheses (collectively designated by the catchall ~H).

So, the posterior probability of a hypothesis requires that we determine whether O is more expected under H than it is under ~H. If so, how much more? Precise numerical values are not necessary to answer either of these questions, but we must be able to say something about the comparative expectedness of the observations under H and under the catchall ~H.[18] If the likelihood ratio is greater than 1, it implies that the observed evidence O is more likely under hypothesis H than under the catchall ~H. In which case, O confirms H. This leads to an increase in the posterior probability of H relative to ~H. The higher the likelihood ratio, the stronger the evidence supports hypothesis H over ~H, and the more it pushes up the posterior probability of H. Conversely, if the likelihood ratio is less than 1, the evidence is more expected or better explained by ~H than by H. This leads to a decrease in the posterior probability of H. The likelihood ratio is central to Bayesian reasoning.

From Bayes' theorem, we can derive several important criteria, in addition to (IC), for assessing the posterior probability of a hypothesis.

### *Which of Two Hypotheses is More Probable than the Other? Principle of Contrastive Posterior Probabilities*

(CP) $Pr(H_1 \mid O) > Pr(H_2 \mid O)$ if and only if $Pr(H_1)Pr(O \mid H_1) > Pr(H_2)Pr(O \mid H_2)$.

(CP) allows us to contrast the posterior probabilities of two competing hypotheses. It tells us that the posterior probability of one hypothesis ($H_1$) is greater than the posterior probability of another hypothesis ($H_2$) just if the product of the prior probability of $H_1$ and its likelihood is greater than the product of the prior probability of $H_2$ and its likelihood. If the priors of the hypotheses are equal, then the hypothesis with the higher likelihood will have the higher posterior probability. If the likelihoods of the hypotheses are equal, then the hypothesis with the higher prior probability will have the higher posterior probability. Notice that (CP) differs from the contrastive or differential support articulated in (LL). (LL) tells us that likelihood inequalities alone are evidentially salient for discriminating between $H_1$ and $H_2$. (LL) tells us about the contrastive probabilities of the observational evidence given each of the hypotheses, not the probabilities of the hypotheses themselves. (LL) is about what the observa-

tion tells us about two competing hypotheses – which of the two the observation favors.

It is worth noting that IBE arguments can be merged with (CP). A Bayesian explanationist takes it that the best explanation is the one with the highest posterior probability (Lipton, 2004, pp. 103–120; McCain & Poston, 2024; Niiniluoto, 2004). Similarly, the *better* of two explanations, which need not be mutually exclusive or jointly exhaustive, will be the one with the higher posterior probability. Given (CP), an explanation $H_1$ will be superior to a rival explanation $H_2$ either with respect to H's likelihood or H's prior probability (or both). And, the greater the difference between $(H_1)Pr(O \mid H_1)$ and $Pr(H_2)Pr(O \mid H_2)$, the greater the difference in their posterior probabilities and hence the greater the explanatory power of one over the other. While it is possible to merge (LL) and IBE, typically, more goes into explanatory power than likelihoods – for example, simplicity, coherence, scope, and fit with background knowledge. The Bayesian typically rolls these into the prior probability of a hypothesis (Roche & Sober, 2013; Sober, 2002).

### *When Does Evidence Justify Believing a Hypothesis? Principle of Absolute Confirmation*

However, perhaps we want to determine, not simply whether a hypothesis has a *higher* posterior probability than one or more alternative hypotheses, but whether its posterior probability is *high* or *very high*. Bayes' theorem also gives us the resources for inferences about the overall or net probability of a hypothesis.

(AC) O confirms H if and only if $Pr(H \mid O) > ½$.

(AC) captures a stronger sense of confirmation, where the observations or evidence make the posterior probability of H high to very high. This kind of Bayesian confirmation is usually called *absolute* confirmation. The term "absolute" here does not mean the absolute value of H, nor any kind of conclusive confirmation. It refers to the kind of confirmation that occurs when O raises the probability of H above a particular threshold value. Since that threshold value is typically ½, I have built it into the formulation. Given (AC), an observation strongly supports H. Of course, it may also be the case that $Pr(H \mid O) >> ½$ – that is, the probability of H given O is *much greater* than ½. In both cases, O confers a high probability on H; it does not merely raise H's probability (IC), nor does it mean that the posterior of H is merely higher than the posterior of some particular rival hypothesis (CP). (AC) is usually what is required if the evidence needs to be strong enough to justify a belief in a truth-conducive sense of justification.

"Bayesian confirmation theory," as Augustine has aptly stated, "is merely probabilized hypothesis-testing" (Augustine, 2022c, p. 805n17). It is not surprising then that, in the history of psychical research, prominent commentators have baked Bayesian elements into their explanatory reasoning about survival (Almeder, 1992; Broad, 1919, 1925/1960; Dodds, 1934; Ducasse, 1961; Griffin, 1997; Lund, 2009; Paterson, 1995).[19] They have relied on prior probabilities and contrastive likelihoods to determine whether the survival hypothesis is explanatorily superior to non-survival alternatives. I do not mean that these writers have *formally* utilized Bayesian criteria. With only a few exceptions (Augustine & Fishman, 2015; Sudduth, 2016), the reliance on Bayesian ideas has been largely informal and often inchoate. But survivalists and their critics have long debated the antecedent or prior probability of the survival hypothesis, as well as the expectedness of the data, but for the survival hypothesis. Based on such considerations, writers have inferred which of the competing hypotheses (survival vs. some alternative) has the higher net plausibility. And the survivalists among these writers have also concluded that the cumulative weight of the evidence confers a favorable net probability on the survival hypothesis, usually greater than ½. So (CP) and (AC) have at least been informally relied on in the survival debate since writers as early as Broad and Dodds, and typically these Bayesian elements have been merged with explanatory reasoning.

Bayesian epistemology deserves a more extensive treatment than there is space for here, though Augustine and I have elsewhere discussed it at length (Augustine & Fishman, 2015, pp. 256–271; Sudduth 2016, pp. 160–187). Here I note two things. First, even when survivalists have tacitly relied on one or more elements of Bayesian reasoning, their arguments have only informally or loosely incorporated such elements. Augustine and I each advocate a more robust use of Bayesianism. Second, and more concerning, is the extent to which survivalists push back against Bayesian analyses, and for transparently bad reasons. So, before moving on, it is necessary to address a couple of frequently encountered survivalist criticisms of Bayesian analyses. The more virulent objections concern prior probabilities.

### *Survivalist Confusions about Bayesian Analyses*

Some survivalists have said or otherwise suggested that skeptics such as Augustine rig their Bayesian arguments by assigning a low prior probability to the survival hypothesis so that no amount of accumulated evidence can tip the scales in favor of survival. Jim Matlock (2016b, 2016c, 2019)[20] and Ed Kelly (2016)[21] have each presented

variations of this objection, and Nahm (2021, pp. 59–60) seems to give it a nod of approval while citing Kelly (2016).

Matlock, the more strident advocate of this objection, wrote:

> Augustine and Fishman naturally believe that the dependence thesis is the winner of the contest with the independence thesis, because they assume that the mind cannot affect the brain and body and that the physical realm is causally closed. These starting assumptions constrain the estimation of prior probabilities and guarantee that the dependence thesis comes out ahead. If we reject the notions that the brain always acts antecedent to mental events and that the physical realm is causally closed, the calculus changes so that the dependence and independence theses are more equal in their prior probabilities; and when we take into account all of the data relating to mind/body relations, not just those which conform to the expectations of the dependence thesis, our background knowledge changes enough to tilt the balance in favor of the independence thesis. (Matlock, 2016b, p. 200)

Augustine (2016, pp. 216–218) and I (Sudduth, 2021a, pp. 193–195) have each shown that this is a misrepresentation of the Bayesian analysis in Augustine and Fishman (2015). In fact, we get multiple misrepresentations for the price of one. Apart from the fact that Augustine and Fishman do not claim (or imply) that "the mind *cannot* affect the brain and body" (emphasis mine), they are quite clear that their Bayesian analysis relies on the principle of indifference with respect to prior probability. They initially assign *equal* prior probabilities to the dependence and independence theses. They write, "we will charitably assign equal prior probabilities of 0.5 to the dependence and independence theses" (Ibid., p. 260). This is the same prior probability survivalists such as Ducasse (1961) have relied on (cf. Broad, 1919; 1925/1960, pp. 519–532). Since the sum of the priors (H and ~H) must equal 1, assigning the survival hypothesis a prior of 0.5 means that we are initially assuming that survival is *as probable as not*. We are assigning it the *same* initial probability as its negation. This probability is not low, and the value assignment is not rigged.

The prior is a weight in Bayes' theorem, but the likelihood ratio is the engine that drives the posterior probability. Assigning a prior of 0.5 highlights this. Begin with a prior of 0.5. If, once we consider the evidence, the likelihood ratio is less than 1, then the posterior probability of the survival hypothesis will drop below 0.5. Hence, the survival hypothesis will *now* be improbable to some degree. In the article in question, Augustine and Fishman argue that relevant likelihoods are less than 1. This is why they say, "if we charitably assumed equal priors for the dependence and independence theses, Bayes' theorem would [still] yield a vastly lower posterior probability for the independence thesis" (Augustine & Fishman, 2015, p. 270). But in this situation, the critic is not *assigning* a low posterior probability for the survival hypothesis. The low posterior probability is the outcome of a properly conducted data-driven empirical assessment. If survivalists such as Matlock and Kelly think otherwise, they must address the arguments for the unfavorable likelihood ratio and/or present a wider data set that gives the survival hypothesis a compensatory favorable likelihood. In other words, they must show that there is something wrong with either the data that Augustine and Fishman use or their inferences from that data. To date, they have done neither.

### *The Washing Out of Priors*

The previous point draws attention to the *diachronic* aspect of Bayesianism, also ignored by survivalist critics of Bayesian analyses. Bayes' theorem tells us how different quantities in the equation are related to each other and how the quantities in the numerator and denominator contribute to the posterior probability of a hypothesis. The theorem itself is synchronic. It informs us of the posterior probability at a particular time. But Bayesianism is also diachronic. It gives a rule for updating one's degree of belief as new evidence comes in.

The rule for updating is a simple one:

> The rule of updating by strict conditionalization says that if *O* is the totality of the new information you have acquired, your new probability for *H* should be equal to your old value for $Pr(H \mid O)$. In other words, $Pr_{now}(H) = Pr_{then}(H \mid O)$, if *O* is all the evidence you have acquired between then and now. (Sober, 2008, p. 11).

As Bayesians say, today's priors are often yesterday's posteriors. This is especially relevant when considering how new evidence or the accumulation of evidence *ought* to change the initial prior probability of a hypothesis. I pointed out above how updating can lower an initially neutral prior probability, but this would also be true if we initially assigned a high prior probability to the survival hypothesis. To illustrate, assume Pr(survival) = 0.8 (highly probable). If Pr(O | survival) = 0.2 and Pr(O | ~survival) = 0.9, the posterior probability of survival will be

0.47, which is less than ½. The exact numerical values do not matter here. They simply illustrate this crucial point: what matters, especially when considering the effects of the accumulation of independent pieces of observational data, is rational updating, which is dependent on the Bayesian likelihood ratio.

Notice, though, that the point about rational updating can, in principle, work in the other direction, which is favorable to the case for survival. If – at a given time – we assign the survival hypothesis a sufficiently low prior probability, this can prevent its posterior probability from being greater than ½ *at that time*. But updating will raise the probability if the likelihood ratio is greater than 1. In this way updating can wash out an initially low prior probability for the survival hypothesis, pushing its posterior probability above ½ – more probable than not (Augustine & Fishman, 2015, p. 258; Sudduth, 2016, pp. 202–205).[22]

In Sudduth (2016, pp. 204–205), I illustrated the above point with a formalized version of R.W.K Paterson's informal Bayesian-style cumulative argument for survival. I showed that one could initially assign the survival hypothesis a low prior probability – I selected 0.125 *to illustrate* – and by successive updating using several categories of evidence conclude that the survival hypothesis is more probable than not. But the point here is generically easy to otherwise illustrate. Assume a likelihood ratio of 2 for the evidence, so the evidence is twice as likely if the survival hypothesis is true than if it is false. Using different low priors as our starting points, we can easily calculate how many independent items of evidence it would take to push the posterior probability of survival above ½.

- Assume Pr(survival) = 0.10 = *very* improbable. If the likelihood ratio = 2 for each of **four** independent items of evidence, then the posterior probability of the survival hypothesis will increase to 0.64 (more probable than not) after successive updating.
- Assume Pr(survival) = 0.010 = *highly* improbable. If the likelihood ratio = 2 for each of **seven** independent items of evidence, then the posterior probability of the survival hypothesis will increase to 0.56 (more probable than not) after successive updating. **Eight** pieces of evidence pushes the posterior probability to 0.72.
- Assume Pr(survival) = 0.001 = *extremely* improbable. If the likelihood ratio = 2 for each of **eleven** independent items of evidence, then the posterior probability of the survival hypothesis will increase to 0.65 after successive updating.[23]

Theoretically, then, two people could initially disagree about the prior probability of survival, but they could come to an agreement over time as the result of updating. The initial disparity of priors is washed out with the accumulation of evidence over time. It also follows that if one's priors are subjectively skewed, updating over time can correct this. This shows that *the likelihood ratio matters more than any initial prior probability*. So, if survivalists wish to attack the unfavorable outcome of Augustine's Bayesian analyses, they need to show that his treatment of the likelihood ratio is flawed. To date, they have not done this. Ironically, survivalists who reject Bayesianism close off a well-established and widely deployed path to handling the cumulative weight of evidence. And they offer no lucid alternative, indeed no alternative at all.

### *The Subjectivism Objection*

Bayesian reasoning is also allegedly "subjective" because nothing "objective" constrains prior probabilities.

First, this is a peculiar objection coming from those survivalists whose reasoning about survival is seemingly unconstrained by *anything*, including the standard forms of argument one encounters in an introductory course in critical thinking. It is also an odd sort of objection given the tendency of more sophisticated survivalists to appropriate (Fisher or Neyman-Pearson) significance tests to leverage facts in favor of survival or against non-survival counterexplanations. Some of the motivation of adopting these methodologies is a pretense to achieve a kind of objectivity that is not attainable in any domain of inquiry. As Howson and Urbach have noted, "virtually none of those [frequentist] methods can be applied without a generous helping of personal judgment and arbitrary assumption" (Howson & Urbach, 2006, p. 9). For example, the hypothesis one ends up accepting or rejecting depends on which hypothesis one initially accepts as the null hypothesis, but the null hypothesis is selected from a wider set of competing hypotheses. Selection here is pragmatic, if not arbitrary. Stopping rules (i.e., the criteria used to determine when to stop data collecting or a statistical experiment) and p-values (used to quantify the statistical significance of a test result) are equally subjective and arbitrary, guided by the researcher's preferences and goals, or simply a matter of convention (Ibid., pp.131-182).[24]

Second, it is not generally true that nothing "objective" constrains prior probabilities. Priors are often empirically defensible (Sober, 2008, pp. 24–26). Of course, priors are *sometimes* not empirically defensible. In that case, a rational agent should use Bayes' theorem to rationally update their beliefs, which is one of the goals of Bayesian epistemology. Subjective Bayesianism – the interpretation of Bayesianism in which there are no empirical constraints on initial prior probabilities – is concerned with rules for how to properly regulate one's subjectivity

(Sober, 2008, pp. 11–12, 26, 31–32). Bayesian reasoning is analogous to deductive reasoning in this respect. Deductive logic gives rules to guide what inferences you ought or ought not to make given that you begin with whatever premises you begin with; it does not give you advice on what premises to begin with, nor does it tell you whether your premises are rationally acceptable (Howson and Urbach, 2006, pp. 265–266, 301–302). Ideally, we would like Bayesian posterior probabilities to have probative force, and this is more plausible when there are empirical constraints on priors. But I see no reason why empirical facts are unable to inform judgments about the prior probability of the survival hypothesis. And there is good reason to think they do, though in a way unfavorable to survival (Augustine, 2022a, pp. 371–374; Augustine & Fishman, 2015).

The survivalist preoccupation with priors is a distraction from the more salient issue of the comparative expectedness of the data under competing hypotheses. As a result, it distracts from the many interconnected problems associated with generating the required (Bayesian or likelihoodist) likelihood inequalities to be discussed in subsequent sections of the paper. But sufficient for the moment are the criticisms thereof. Survivalist objections to prior probabilities are overrated, confused, and implausible.[25] To the extent that survivalist criticisms of Bayesian analyses rest on such objections, they carry no force.

### *Summing Up Insights from Confirmation Theory*

As in the wider corpus of survival literature, the BICS essays make frequent use of phrases that express ideas of confirmation and evidential support, but on the whole the essayists poorly navigate the conceptual territory they parachute themselves into with the use of such language. Consequently, operative phrases like "____ is *evidence* for survival," "____ *supports* the survival hypothesis," "____ *favors* the survival hypothesis over alternative explanations," and "____ better *fits* the survival hypothesis than the alternatives" are not disambiguated and sufficiently clarified. But these and similar phrases pick out different epistemologically important concepts that need to be carefully distinguished. And these must also be sharply distinguished from rules designed to guide decision-making procedures. As Elliott Sober has explained, the value of likelihoodism and Bayesianism is their ability to provide formal proposals that shed light on informal concepts. "A formal proposal that describes how an informal concept should be understood is to be judged by the light it throws on the informal concept, but it also should be judged by the light it throws, period" (Sober, 2008, p. 35).

To sum up the key points of the Bayesian and likelihoodist viewpoints:

- Each viewpoint tells us that likelihood inequalities are a crucial determinant of evidential support, which can be captured by their respective likelihood ratios.
  - O evidentially supports $H_1$ over $H_2$ just if $Pr(O \mid H_1)/Pr(O \mid H_2) > 1$.
  - O evidentially supports H (full stop) just if $Pr(O \mid H)/Pr(O \mid \sim H) > 1$.
- Each viewpoint tells us that if an observation O is evidence *for* a hypothesis H, then O is evidence *against* some alternative to H. In Bayesianism, the alternative is H's negation; in likelihoodism the alternative is some specific competing hypothesis. Bayesianism and likelihoodism are both contrastive in this broad sense.

Bayesian confirmation tells us what to believe. (IC) tells us when we should increase our degree of confidence in a hypothesis. (CP) tells us how to determine which of two specific competing hypotheses has the greater posterior probability. (AC) provides the criterion for showing that a hypothesis has a favorable posterior probability (above ½) and, therefore, would provide a robust justification for believing a hypothesis.

- Likelihoodism captures an important intuition about evidential support via the favoring relation, and thereby tells us which of two competing hypotheses the evidence favors.
- Bayesian explanationists hold that the best explanation is the hypothesis with the highest posterior probability, and the better of two rival explanations is the one with the higher posterior probability. (CP) provides the criterion for the latter, and (AC) provides the criterion for the former. A likelihoodist explanationist can use (LL) to at least partially parse explanatory power.

In later sections of the paper, I will show how likelihoodist and Bayesian approaches help identify important errors in survival arguments which informal approaches to evidence evaluation mask. Hence, confirmation theory has an important heuristic value. This will be a further development of what I have argued in previous publications (Sudduth, 2013a, 2013b, 2014, 2016), namely that the comparative expectedness of the data captured by the likelihood ratio highlights the crucial yet problematic role

of auxiliary assumptions in survival arguments. The likelihood ratio depends on likelihoods, and the latter, in turn, depend on auxiliary assumptions. However, if the likelihood function depends on *unwarranted* auxiliary assumptions, it cannot be effectively used to test hypotheses or generate evidential support for a hypothesis.

## 6. Back to Braude

We previously saw (in §4) that Braude infers the following two claims from the explanatory power of the survival hypothesis:

(5′) The explanatory success of the survival hypothesis is *evidence* for the truth of the survival hypothesis.

and

(5″) The explanatory success of the survival hypothesis is a *reasonable* basis for belief in postmortem survival.

Based on the discussion in §5, we can assess why Braude's (5′) and (5″), though initially modest, are nonetheless problematic. Subsequently, I will show how the previous digression into theories of evidence sheds light on some of the errors encountered in the Augustine-Braude et al. exchange and the wider survival debate.

First, it is not clear what Braude means by "evidence" or how he is construing its relationship to "explanation." We need more clarity here. He speaks of certain features of the best cases *tilting* the scales in favor of survival (Braude, 2003, p. 216). But this is ambiguous between the principle of contrastive posteriors (CP) and the law of likelihood (LL) – that is, an ambiguity between (a) the *posterior probability of the survival hypothesis is higher*, given the evidence, than the posterior probability of the best rival explanation conditioned by the same evidence and (b) the evidence *favors the survival hypothesis* over the best rival hypothesis.

I think Braude's reasoning is better explicated using (LL). First, he is skeptical or at least cautious about enlisting prior probabilities to do epistemic work (Ibid., pp. 302–303), and he does not contrast the survival hypothesis with its negation (the Bayesian catchall likelihood). Second, he repeatedly appeals to what we would *expect* given a rival hypothesis (Ibid., pp. 48, 88, 95) and what we would *expect* given the survival hypothesis (Ibid., pp. 6, 52, 72, 94–95, 179, 304–305). It is true that Braude issues caution about our expectations for what the evidence should look like if survival is true. For example, he says, "we're unable to predict with any confidence at all what the data *should* look like" (Ibid., p. 19; cf. p. 222). But (LL) does not require that the survival hypothesis predict the data, nor does it require a high degree of confidence in what the survival hypothesis would lead us to expect. It only requires that we have reasons supporting contrastive expectations that favor the survival hypothesis, however tentative or qualified these may be.

So, I think it is relatively clear that Braude is not offering a Bayesian-style survival argument of either the (IC), (CP), or (AC) variety covered in the previous section. It is at least tempting to regard Braude as a covert likelihoodist who relies on (LL) to explicate explanatory efficacy.[26] At any rate, the survivalist's explanatory advantage, as Braude understands it, *can* be codified in terms of likelihood inequalities between survival and particular competing hypotheses, the strongest of which is, in Braude's view, the appeal to living-agent psychic functioning.

One illustration of Braude's covert reliance on (LL) or something close to it is worth noting. It is his argument from crippling complexity, which he says gives the survival hypothesis a "slight explanatory advantage" over the motivated living-agent psi hypothesis (Ibid., pp. 86–95, 99, 218, 305–306). His argument is framed entirely in terms of implicit likelihood inequalities between the survival hypothesis and the alternative explanation. The context is mediumship, and the relevant observational data are "the *amount* of veridical material revealed during sittings" and "the consistency with which subjects provide it" (Ibid., p. 91), where this includes sustained trance personae in mediumship. Among other things, Braude argues that "the more super we allow psychic functioning to be, the less likely it becomes that a medium's ESP could produce an extended and accurate trance persona" (Ibid., p. 94; cf. p. 90). He also says, "If psi cannot overcome the problems of task complexity and multiple sources of information, then it will be too weak to account for the best actual cases" (Ibid., p. 94). The core of Braude's argument from crippling complexity is an attempt to justify these likelihood inequalities. So, it is plausible to interpret Braude's evidential claims in terms of (LL)'s favoring relation.[27]

Of course, a (LL) styled survival argument would not tell us what we should believe, nor how probable or plausible the survival hypothesis is. At the most, it would tell us that the features of the best cases that Braude identifies (strongly) favor survival over the motivated living-agent psi hypothesis (or some other rival hypothesis). This does not justify *belief* in survival, at least not in any robust sense. But it is no less significant for reasons canvassed earlier in connection with Royall's question – what does the evidence say? Moreover, if likelihood inequalities are baked into explanatory power, then (LL) gives us a reason to believe (5′), but it does not give us a good rea-

son to accept (5″). (LL) does not answer the question of what we should believe or whether we ought to increase our degree of confidence in a hypothesis.[28]

That said, I think Braude's (5′) and (5″) are in themselves fairly innocuous. I suspect Augustine would agree. At all events, neither (5′) nor (5″) is incompatible with Augustine's criticisms of the BICS essays. The potential problem lies elsewhere.

Braude's endorsement of (5′) and (5″) seems to depend on:

(4) The survival hypothesis S is the best available explanation of $O_1$, $O_2$, ..., $O_n$,

which in turn is partly based on the premise:

(3) No available competing hypothesis R explains $O_1$, $O_2$, ..., $O_n$ as well as S does.

Since Augustine provides several reasons for doubting, even denying, (3) and (4), we have here a plausible point of actual disagreement between Augustine and Braude et al. As I read the exchange, **the heart of the debate between Augustine and Braude et al. concerns a dispute about whether the survival hypothesis has an explanatory advantage vis-à-vis a subset of the data which has an alleged immunity against being explained away by non-paranormal hypotheses.**

I will subsequently discuss several issues that further illuminate this apparent point of disagreement. Here I simply note two points.

First, on a Bayesian view, the best explanation is the one with the highest posterior probability. While Augustine has elsewhere provided a Bayesian analysis of the survival hypothesis (Augustine & Fishman, 2015), I have given several reasons for supposing that Braude's positive argument for survival is better interpreted as a likelihoodist argument. But doubting or denying that the survival hypothesis is the best explanation *in the Bayesian sense* is logically consistent with affirming that the survival hypothesis is the best explanation of certain data *in the likelihoodist sense*. On the Bayesian view, prior probabilities matter, as does the marginal probability of the data Pr(O), the denominator in Bayes' theorem. Unlike a likelihoodist IBE argument, a Bayesian IBE requires that we consider how probable the evidence is if the survival hypothesis is *not true* – that is, we need to determine how probable the evidence is given the disjunction of all possible alternatives to the survival hypothesis. On this view, simply determining that the survival hypothesis better leads us to expect some data than does this or that rival hypothesis is inadequate.

Second, even if Augustine and Braude disagree about (3) and (4), this is tangential to Augustine's critique of the BICS essays. His critique focuses on strong epistemic claims – for example, absolute confirmation claims – and the reasons survivalists adduce in support of such claims. Braude et al. neither support nor defend such strong claims. Nor does the endorsement of IBE survival arguments do the job here, unless they are robustly formulated to permit the otherwise contentious inference from "best explanation" to "highly probable explanation." Absent that, and especially in the light of the gap between Bayesian and likelihoodist IBEs, it is unclear how Braude, et al. can use mere explanatory considerations to undercut Augustine's critique of the BICS essays or his argument against the survival hypothesis being more probable than not given the totality of the evidence. I will subsequently show how this derails the Braude et al. critique in connection with the evidential implications of neurophysiological data suggestive of mind-brain dependence and facts surrounding mediumship that seem to significantly reduce the evidential force of even the better demonstrations of mediumship.

## 7. Varieties of Skepticism

Before looking more closely at the reply to Augustine, we should consider another recurring theme that Braude and his coauthors make use of in their criticisms of Augustine. They refer numerous times to Augustine's "skeptical position" and his "skeptical arguments" (Braude et al., 2022, pp. 400, 409, 404–405). And no less than nine times, they align his skeptical stance with the "anti-survivalist" position, five of which occur in the space of eight consecutive paragraphs (Ibid., pp. 403, 405, 408–409). Unfortunately, this language fails to properly characterize the position Augustine takes in his BICS critique. It also resembles the undisciplined use of such language in the Contest's essays, where *skeptic* and *skepticism* are pejorative words used to dismiss any position that disagrees with the preferred survivalist viewpoint. Survival research has long been committed to this kind polemically charged advocacy language (Hart, 1959, pp. 252–263). What is needed, though, is more nuance to adequately capture different critical stances or attitudes toward the survival hypothesis and the arguments made on behalf of it.

Consider a vanilla form of the survival hypothesis:

S: The consciousness of at least some persons persists after biological death.

What is a skeptical position toward S? For any propo-

sition p, one can *affirm* p, *deny* p, or *withhold* p. So, there are two basic ways to characterize skepticism. A skeptic could be someone who denies S, that is, believes that S is false. Perhaps they think S is self-contradictory or are convinced that there is a sound deductive argument for supposing that S is false. Or maybe they think there are good reasons for supposing that S is probably false. Call this **denial skepticism** (hereafter, D-skepticism). Alternatively, someone might withhold the belief that S – they consider the proposition S and neither believe S nor believe not-S. Maybe they believe there is no sufficiently good reason either to affirm S or to deny S. Call this **withholding skepticism** (hereafter, W-skepticism).[29]

There is also **argument skepticism** (hereafter, A-skepticism). This is skepticism about *the cogency of an argument* for some proposition. If you think an argument lacks cogency or you are unconvinced of its cogency, then you are an A-skeptic with respect to that argument. So, you are an A-skeptic concerning the survival hypothesis if you deny or doubt the cogency of some argument(s) for survival.[30] A-skepticism typically informs D- or W-skepticism, but it does not entail being a D- or W-skeptic with respect to the survival hypothesis itself. Similarly, rejecting the cogency of arguments for the existence of God does not make someone an atheist.

Skepticism is clearly Janus-faced. But what kind of skeptic is Augustine?

Augustine made it clear that, given the totality of the relevant evidence, personal discarnate survival is possible but highly unlikely. For example, he said, "The totality of the evidence renders discarnate survival highly unlikely" (Augustine, 2022b, p. 412). In the body of his reply to Braude et al., he said, "I've always characterized this [neuroscientific] evidence as rendering personal survival highly unlikely, not impossible" (Ibid., p. 423). So, Augustine's skeptical position includes D-skepticism. Braude et al. correctly picked up on this skeptical dimension to Augustine's wider viewpoint and body of work, though they regrettably mischaracterized it in places – for example, attributing to Augustine the view that survival is impossible (Ibid., p. 407; cf. p. 404).[31]

However, there is more going on in Augustine's essay. While it is fine to acknowledge what Augustine *thinks* about the survival hypothesis all things considered, it is more important to focus on what Augustine *intends to argue* in his essay. Unfortunately, preoccupation with the former question hampered a properly calibrated response to the latter question.

- In his reply to Braude et al., Augustine says his respondents lost sight of "whether the critiqued [BICS] essays met their directive to provide 'hard evidence beyond a reasonable doubt' of the survival of human consciousness" (Augustine, 2022b, p. 412). He later reiterates that his "directive was to critically evaluate the arguments for discarnate personal survival" (Ibid., p. 415). He criticizes his respondents for shifting the focus away from this and leaving "the adequacy of the arguments found in the BICS essay competition unresolved" (Ibid., p. 429).
- Consider the bookends to his initial essay: "The overall evidence doesn't even make personal survival more probable than not" (Augustine, 2022a, p. 366) and "The evidence doesn't even make personal survival more probable than not" (Ibid., p. 390). He repeats this at the end of his reply: "I thus stand by my original conclusion: given the evidence as a whole, discarnate personal survival is not even minimally more probable than not" (Augustine, 2022b, p. 429). I am inclined to take Augustine's point here to be that the overall evidence presented *in the BICS essays* fails to be sufficiently indicative of a marginal probability in favor of survival. But to the extent that the BICS essays are the best arguments on offer, we are right to draw a broader conclusion: the type of phenomena at the center of the BICS essays do not make the survival hypothesis at least more probable than not.
- Nearly all the fallacies Augustine puts in bold in his initial critique and reply – for example, cherry-picking, begging the question, confirmation bias, stacking the deck, hasty conclusion – concern the arguments *for* survival lacking cogency. These critical considerations are designed to *undercut* the survivalist inference, not *rebut* it. Undercutting an argument involves losing the reasons for accepting the argument's conclusion, whereas rebutting an argument involves acquiring reasons to deny the argument's conclusion. Arguing that the survivalist has not presented good reasons to accept that the survival hypothesis is more probable than not is distinct from arguing that there are good reasons to deny the survivalist's conclusion.
- Augustine is cognizant of W-skepticism and links it with perceived defects in arguments for psi and survival. In referring to the failures of tests for psi, Augustine says, "Their failure gives the scientific community good reason to doubt the existence of extrasensory perception (ESP)" (Augustine, 2022a, p. 371). "Many are thus skeptics of discarnate personal survival simply because the evidence in its favor is hardly compelling" (Ibid, p. 389). "Some skeptics may simply point out that empirical survivalists have not made their case for personal survival without committing to a position on the survival question" (Augustine, 2022b, p. 415).

While Augustine *thinks* discarnate survival is improbable, the bulk of his critique concerns a negative assessment of *the arguments for survival* in the BICS essays. Of course, most of the BICS essayists also make claims about how they see the evidence as a whole. Augustine is arguing that their arguments for a strong favorable net assessment do not succeed. He is challenging the cogency of the arguments presented in the essays, not presenting an all-things-considered argument to support D-skepticism, though many of his points would be relevant to such an argument, which he has elsewhere developed in detail (Augustine, 2016; Augustine & Fishman, 2015). So, although Augustine is a denial skeptic with respect to discarnate survival, in his critique, he mostly deployed argument- and withholding-skeptical arguments. As I will show in the subsequent discussion, Braude et al. lost sight of the Janus-faced character of Augustine's skepticism at crucial points and misconstrued the kind of skeptical argument he was presenting.

## 8. Neurophysiological Evidence and the Survival Hypothesis

Several of Braude et al.'s specific complaints against Augustine emerge in connection with questions in the philosophy of mind and the interpretation of neurophysiological data. They present two general criticisms of Augustine's critique. They say he "carefully avoids discussing two matters of great importance: (1) not simply the strongest reasons but any reasons for challenging his negative appraisal of particular cases and (2) arguments exposing how unverified assumptions and hasty inferences pollute the received view of the relevant physiological data" (Braude et al., 2022, p. 400). As they see it, this allows Augustine to make "his skeptical position seem more substantive than it really is" (Ibid., p. 400).

It is unclear what any of this actually means in the context of Augustine's actual arguments. What are the "two matters" supposed to be of great importance *to*? Presumably, the plausibility of Augustine's skeptical position. But *which* skeptical position? This question matters because the force of the criticism here depends on the kind of skeptical position Augustine is arguing for in his paper. As I have shown, **Augustine was targeting the cogency of the arguments presented in the BICS essays and the collateral contention that the arguments represent a scientific approach to survival.** So, we would need to consider how the two matters Braude et al. introduce bear on Augustine's reasons for claiming that the arguments for survival in the Contest's essays lack cogency and scientific validity, as opposed to how the two matters bear on other kinds of skeptical arguments – for example, arguments which purport to show that survival is all things considered improbable.

### *Augustine's Surprise Principle Argument*

Consider the argument to which Braude et al. are ostensibly responding, though regrettably, they only tersely and opaquely reference it (Braude et al., 2022, p. 409). If we look at the section of Augustine's paper in which he examines the evidential implications of neurophysiological and related scientific data about consciousness and brain functioning (Augustine, 2022a, pp. 371–375), his main point was that **the BICS survivalists either ignore or mishandle data/facts that ostensibly disconfirm the survival hypothesis**. Recall that his wider argument concerns the alleged failure of survivalists to properly weigh the force of all the relevant evidence, partly because they mishandle ostensible counterevidence. He raises a similar criticism in connection with the survivalist responses to apparent experimental failures regarding psi and survival – I will discuss this later. In the present context, he argues that there are scientific facts which strongly confirm the mind-brain dependence thesis, and therefore, strongly count against the independence thesis, and hence, against the hypothesis of discarnate survival (Ibid., p. 371).

To understand why Augustine thinks survivalists ignore or mishandle the alleged counterevidence to survival at this juncture, we have to first consider why *he* thinks the scientific facts he identifies are counterevidence to the survival hypothesis. Counterevidence in this context is any proposition that "constitutes evidence against discarnate personal survival" (Ibid., p. 371) or, more technically, which "lowers the probability of discarnate survival" (Ibid., p. 374).[32] Augustine relies on the *Surprise Principle*, an important principle of evidential support which codifies "the basic idea behind inference to the best explanation" (Ibid., p. 374).

Augustine quotes philosopher of science Elliott Sober on the Surprise Principle:

> The Surprise Principle describes when an observation O strongly favors one hypothesis ($H_1$) over another ($H_2$). There are two requirements:
>
> (1) If $H_1$ were true, then you would expect O to be true.
>
> (2) If $H_2$ were true, then you would expect O to be false.
>
> That is, (1) if $H_1$ were true, O would be unsurprising; (2) if $H_2$ were true, O would be surprising.

(Sober, 2012, p. 30)

The Surprise Principle (hereafter, SP) is an informal expression of one of the important confirmation-theory insights discussed at length in §5, namely the evidential significance of likelihood inequalities.[33] It covers instances of the law of likelihood where Pr(O | $H_1$) = high and Pr(O | $H_2$) = low. However, unlike the law of likelihood, it also covers instances where the contrasting hypotheses are H and ~H (Bayesian catch-all) and Pr(O | H) = high and Pr(O | ~H) = low. We can also think of the SP as the qualitative expression of the (Bayesian or likelihoodist) likelihood ratio when it is (much) greater than 1.

Augustine argues that the scientific facts he lists (Augustine, 2022a, p. 371) strongly favor the dependence hypothesis (*consciousness depends on a functioning brain*) over the independence hypothesis (*consciousness is independent of a functioning brain*) because the facts are what we would expect if the dependence thesis were true, but they are not what we would expect if the independence thesis were true. The independence thesis is the negation of the dependence thesis (and conversely), so the contrasting hypotheses are mutually exclusive and jointly exhaustive.

Let F = Augustine's list of scientific facts, D = the dependence hypothesis, and ~D = the independence hypothesis. In standard form, Augustine's argument looks like this:

(1) If D were true, then we would expect F to be true.
(2) If ~D were true, then we would expect F to be false.
(3) F

Therefore:

(4) F strongly favors D over ~D.[34]

The crucial premises are (1) and (2). Augustine deploys a mail bin thought-experiment to show why we would be strongly inclined to regard these premises as true (Ibid., pp. 371–372).[35] Given SP, (4) follows necessarily from (1), (2), and (3). Since D and ~D are mutually exclusive and jointly exhaustive, evidence that strongly supports D is evidence against ~D. So, F are strong evidence against the independence thesis. Since discarnate survival entails ~D, evidence against ~D is evidence against discarnate survival.

Since Augustine is clear about when a fact would be evidence *for* a hypothesis, he is also clear about when facts would be evidence *against* a hypothesis. Part of Augustine's wider interest is to show that survivalists are not clear about either of these two vital points. Consequently, several of the Contest's essayists fail to properly address salient ways in which neurophysiological and other empirical data provide even *potential* evidence against discarnate survival, much less how to weigh it against the evidence which ostensibly supports the survival hypothesis. More precisely, **survivalists fail to acknowledge how scientific data at least potentially undermine their reasons for supposing that the survival hypothesis is beyond reasonable doubt or has some other extremely high epistemic credential.**

### *The Surprise Principle and Posterior Probability*

Before looking at the Braude et al. reply to Augustine on the neurophysiological evidence, some clarifications on Augustine's use of SP are necessary.

The conclusion of the SP argument means that the observational evidence under consideration strongly counts for D and against ~D. This does not mean that D is (highly) probable or that ~D is (highly) improbable. This is a consequence of the concept of contrastive support. Likelihoods alone do not yield conclusions about the probability of hypotheses, and the SP argument relies only on the contrasting likelihoods of D and ~D. As Bayesians say, no probabilities in, no probabilities out. Likelihoods must be combined with priors to yield posterior probabilities. The SP argument is not any less weighty on account of this, as it informally expresses an important concept of evidential support. Remember the summary point in §5 – whenever an observation is evidence for a hypothesis, it is also evidence against some alternative hypothesis.

Moreover, it is relatively easy to modify the SP argument by introducing priors so that it yields a favorable posterior probability for the mind-brain dependence thesis. This is not the focus of Augustine's arguments in his BICS critique, but it is important to note, especially in light of the survivalist errors discussed in §5 concerning Bayesian analyses. For example, if we supplement SP with the principle of indifference, then we would assign equal prior probabilities to D and ~D. Each would be *as probable as not*. But if Pr(D) = 0.5 and Pr(~D) = 0.5, then the likelihoods expressed by premises (1) and (2), which entail a Bayesian likelihood ratio of greater than 1, automatically result in the dependence thesis being probable and the independence thesis being improbable.[36] We can arrive at the same conclusion by initially assigning the independence thesis a very *high* probability. Assume Pr(~D) = 0.9 and so Pr(D) = 0.1 and suppose that the likelihood ratio = 2. Now apply SP iteratively to four independent facts, updating the prior with each iteration. It would only take four independent pieces of evidence to cumulatively raise the probability of the dependence thesis above ½ and so render the independence thesis improbable.

The previous point is important for two reasons. First, Augustine elsewhere invokes the principle of indifference to make judgments about the net plausibility of the survival hypothesis (Augustine & Fishman, 2015). This partially explains why, in his BICS critique, he says that the survival hypothesis is not even more probable than not. Second, the analysis once again shows that, in the context of the survival debate, prior probabilities are considerably less important in Bayesian analyses than likelihoods. The likelihood ratio, not prior probabilities, are doing the real work of evidential support.

### *The Complaint Against Augustine*

Having seen how Augustine leverages the neurophysiological data, as well as how it could be leveraged by expanding Augustine's argument with the introduction of priors, we can examine what Braude et al. argued in their response to Augustine's appeal to such data.

Braude et al. claim that Augustine avoids discussing "arguments exposing how unverified assumptions and hasty inferences pollute the received view of the relevant physiological data... there are serious reasons for relaxing our commitments to standard interpretations of the neurophysiological data and entertaining possibly radical alternatives" (Braude et al., 2022, pp. 400–401). They illustrate this by adducing evidence against the claim that memories are located in the brain. When they address mind-brain correlational data which Augustine made use of, they say, "But what are they evidence of? Augustine's anti-survivalist position is only an option, and probably it seems compelling primarily to *those antecedently committed to, or caught in the grip of*, a prevailing conventional scientific view of the world" (Ibid., p. 407).

What strikes me here is what Braude et al. do not say. Despite the length of their discussion on the neurophysiological data, they do not comment on Augustine's SP argument, nor his reasons for claiming that the Contest's essayists fail to properly engage the evidential issues the SP argument brings into sharp focus. They do not argue that the premises of the SP argument are false or otherwise unwarranted. They do not argue against the inference from the premises to the conclusion. Nor do they argue that SP is an incorrect principle for assessing when facts evidentially support one hypothesis over another. And nothing they say provides a defense of the Contest's essays against Augustine's reasoned criticism that the essays fail to properly handle ostensible counterevidence and so fail to show that the survival hypothesis has the epistemic credentials they attribute to it.

Moreover, Braude et al. miss a crucial point. Even if we thought that the neuroscientific facts to which Augustine appeals in his argument are insufficient to support the "anti-survivalist position" or outweigh considerations favorable to the survival hypothesis, those considerations would still potentially undermine the BICS essayists' exaggerated claims on behalf of the survival hypothesis, especially when we examine the reasons they offer for their claims. No antecedent commitment to a prevailing scientific view of the world is required to understand how Augustine is leveraging the neurophysiological facts against the arguments to which he is responding, nor is such a commitment needed to see that his argument is cogent. And since Braude et al. do not acknowledge the argument Augustine actually presented, they are unable to show how the considerations they adduce are even relevant. **What is at issue is whether the SP argument undercuts the survivalist arguments to which Augustine is responding and supports premise [A2] in Augustine's (previously outlined) basic argument.**

But we should consider this further.

First, Braude et al. introduce considerations against Augustine's mind-brain dependence *interpretation* of the facts. They try to raise doubt about the hypothesis that a functioning brain is necessary for consciousness. This is a perplexing dialectical strategy for three reasons.

- Augustine made it quite clear *what* the correlational data are evidence for and *why* they are evidence for it. His SP argument shows in a straightforward way why the correlational data *are* evidence for mind-brain dependence and thus support that particular "interpretation" of the data. It is necessary to address that argument since, if cogent, it justifies the very interpretation of the data with which Braude et al. wish to take issue. Instead, they introduced their own reasons for doubting or denying the mind-brain dependency hypothesis. This is not properly responsive to Augustine's reasons for affirming that conclusion. An argument is not addressed, much less defeated, simply because one adduces reasons for the opposite conclusion. One must show that such reasons outweigh the reasons in support of the original conclusion.[37] So, to counter Augustine's argument, Braude et al. would have to have shown that their reasons for doubting or denying the dependence thesis are stronger than Augustine's stated reasons for supposing that the data he cites strongly support it over the independence thesis.
- "Unverified assumptions and hasty inferences" may very well "pollute the received view of the relevant physiological data," but what matters is whether they pollute *Augustine's* argument. Braude et al. did not show that they do.
- The cogency of Augustine's SP argument is compatible

> with there being *some reason* to doubt the dependence thesis, just as it is compatible with unverified assumptions and hasty inferences polluting the received view. This is because Augustine presented a specific argument for a particular conclusion about the empirical data. In the absence of evidence to the contrary, his argument is no more saddled with the defects of other physicalist arguments than Braude's arguments for survival in his winning BICS paper are saddled with the unverified assumptions and hasty inferences that pollute much of the received view of the relevant data for survival.

Second, in reference to the facts that Augustine says are expected given the dependence hypothesis, Braude et al. (Ibid., p. 409) say these data are also expected given the brain-is-an-instrument version of the independence hypothesis. This appears to be a *sotto voce* concession to the validity of the Surprise Principle, the only time they acknowledge it. However, what is required is (i) *showing* that the data are expected given the brain-is-an-instrument version of the independence hypothesis and (ii) *showing* that the data are more expected given their preferred hypothesis than the alternative. Recall the point (in §5) that a hypothesis must be tested against an alternative. Without such an argument, we have no reason to believe that the data favor their hypothesis. And, unless they can show that the data are just as likely given their preferred hypothesis, they do not succeed in neutralizing the evidential support that the data lend to the dependence thesis and against discarnate survival.

Third, even if Braude et al. could show that Augustine's scientific facts are at least as probable given the brain-as-instrument independence hypothesis as they are given the dependence hypothesis, a problem remains. Augustine's SP argument contrasts the dependence and independence theses, where these are comparably simple versions of the hypotheses. He argued that given *these* two contrasting hypotheses, the former better leads us to expect the data. Braude et al. claim that the brain-as-instrument view which McTaggart proposed can accommodate these facts. Perhaps, but this amounts to bulking up the vanilla independence hypothesis to accommodate the facts, but now we are contrasting a robust version of the independence thesis and a simple version of the dependence thesis. This is the same kind of conceptual sleight-of-hand Braude accused survivalists of in his Contest essay (Braude, 2021b, p. 8) and which he cautions against in his collaborative response to Augustine (Braude et al., 2022, p. 403). It is always possible to bulk up predictively impotent hypotheses with additional assumptions so that the hypothesis becomes robust enough to lead us to expect the observational data, or any data for that matter.

Augustine drew attention to this fallacy:

> [O]ne can always contort any hypothesis to fit any facts, just as one can hammer at a square peg to force it into a round hole. The key to assessing the degree of evidential support is to start with what the most basic version of each hypothesis predicts. What do their *simpliciter* versions – the hypotheses unamended with auxiliary assumptions, or at most only amended with agreed-upon/confirmed auxiliaries – lead us to expect? (Augustine, 2022a, p. 372; cf. 2022b, p. 424)

Regardless of which version of the independence hypothesis Braude et al. wish to adopt, they must show in a non-question-begging way that their hypothesis accommodates the data in question. Simply asserting it is insufficient. We must see the assumptions that have been added to the hypothesis to permit this accommodation, compare the competing hypotheses in their equally robust forms to see whether the bulked-up independence hypothesis better leads us to expect the data, and then ask whether the accommodation has a price tag that would bankrupt the case for survival.

To his credit, Augustine pointed out the minimum requirement:

> [T]hey should at least *try* to show (not merely assert) that (1) the dependence thesis does not predict this evidence, or else that (2) the independence thesis would lead us to expect the same evidence just as much. (Augustine, 2022a, p. 374)

Their failure to meet this challenge invited Augustine's poignant rejoinder:

> To show that the dependence and independence theses are evidentially on par, Braude et al. (2022) would have had to have *shown* (not merely asserted) that either the dependence thesis would not lead us to expect my bulleted agreed-upon facts, or else that the independence thesis would lead us to expect them *just as much*. But they did *neither*. (Augustine, 2022b, p. 427)

Augustine dialed in a fatal flaw, not only in the Braude et al. reply, but in the bulk of survival literature. **What is ultimately at issue – I will subsequently discuss it in greater detail – is the epistemic status of the auxiliary assumptions we must employ to tightly or even loosely**

**connect survivalist conjectures to observational data.** This is the needed conversation if the survival debate is to have enough lucidity to merit any further serious consideration or exploration.

## 9. The Data From Mediumship

Another general criticism Braude et al. raise is that Augustine "carefully avoids discussing... not simply the strongest reasons, but any reasons for challenging his negative appraisal of particular cases" (Braude et al., 2022, p. 400). One particular species of cases they have in mind is from mental mediumship, specifically the mediumship of Mrs. Piper.

### *Augustine on BICS Survivalists on the Data of Mediumship*

Before looking at Braude et al.'s criticisms, we should get clear on the context of Augustine's discussion of mediumship and the conclusion he aimed to support. His discussion occurs in two sections of his paper: the failures of positive results in experiments designed to test the survival hypothesis (Augustine, 2022a, pp. 368–371) and the Contest's essayists ranking of the survival evidence, including mediumship (Ibid., pp. 376–379). In each case, **Augustine argues that the BICS essayists make important mistakes in their reasoning in support of a favorable conclusion about mediumship being good evidence for survival.** I will subsequently discuss the former, but with respect to the latter, Augustine argues that the survivalist mistakes include not properly weighing, in some cases ignoring, features of mediumship that potentially undercut their strongly favorable net assessments of mediumistic data. Augustine discusses four such concerns: (a) the mixture of twaddle and accurate information in Mrs. Piper's sittings, (b) her demonstrably fictitious controls, (c) the specter of fraud, and (d) the possibility that Mrs. Piper had access to information via ordinary channels of knowledge. I will consider these in due course.

### *Braude et al. on Augustine's Criticisms*

After providing a block quote from Augustine on the relevance of (a)–(d) to overall plausibility assessments of the data of mediumship as evidence for survival, Braude et al. respond with several interesting counterclaims (Braude et al., 2022, pp. 400–401). They claim that Augustine did not provide positive evidence in support of the hypothesis of fraud or the hypothesis that Mrs. Piper relied on ordinary sources of information. Instead, they claim, Augustine appealed only to the *possible* truth of such hypotheses. They also claim that Augustine ignored the reasons why many survivalists think these counter-explanations of Mrs. Piper's mediumship are improbable.

Braude et al. have raised some interesting general issues here. But whether their points undermine Augustine's critique will depend on what Augustine intended to argue and how he argued it, as well as what Braude et al. mean by Augustine's "negative appraisal."

A negative appraisal of Mrs. Piper's mediumship could mean any one of the following:

- The data provide no evidence for survival.
- A person could not be rational and regard the data as evidence for survival.
- The data do not make survival more probable than not.
- The data do not make the survival hypothesis highly probable.
- The data do not prove survival beyond a reasonable doubt.
- The survivalist has not shown that the data from mediumship are evidence for survival, good evidence for survival, make the survival hypothesis more probable than not, make the survival hypothesis highly probable, prove survival beyond a reasonable doubt, etc.

These different kinds of negative appraisal have different consequences for what can sensibly be required of a skeptic. In Augustine's case, one of his arguments concerning mediumship involves a negative appraisal of the reasoning of Delorme, Radin, and Wahbeh (2021) – hereafter, DRW – as well as Michael Nahm (2021). Augustine's conclusion is that *their arguments* lack cogency because *they* mishandle potentially defeating evidence in *their* favorable net plausibility assessments – for example, in the high letter grade (B+) they assign to such cases.[38] The main negative appraisal Augustine gives at this juncture is the final item on the list above. Braude et al. obscure this important dialectical point. Consequently, it looks as if they are picking out some of Augustine's *claims* as a foil for refuting something Augustine is not actually arguing. Since Braude et al. do not provide a clear statement about what sort of evidential claim on behalf of survival they think is justified, the extent to which they disagree with Augustine is not clear.

What is perspicuously missing from Braude et al. are claims that counter a clearly identified premise in Augustine's argument, challenge the inferential link between his premises and conclusion, or which appropriately counter Augustine's conclusion. For example, Braude et al. did not argue that DRW's argument for assigning the letter grade of B+ to the totality of the data from mediumship is warranted, nor do they show that Augustine's argument, which was intended to undercut DRW's favorable conclusion, is fallacious or otherwise weak. And this

is particularly odd, seeing as DRW were among Braude's coauthors. Instead, Braude et al. criticize Augustine for failing to provide positive evidence for his alleged suggestion that Mrs. Piper engaged in fraud or mined information from ordinary sources. But this demand is rooted in a misunderstanding of what Augustine is actually arguing and illicitly shifts the burden of proof. **Augustine's argument does not require providing evidence for the alternative hypotheses, because he is not arguing *for* such counterexplanations. He is arguing that survivalists have failed to adequately rule out such counterexplanations** (Augustine, 2022b, p. 415).

### *In Defense of Augustine*

It is tempting to suppose that Braude et al. have simply conflated

(1) Mrs. Piper's achievements were the result of fraud or the acquisition of information through ordinary sources.

and

(2) Survivalists have failed to adequately rule out (1).

As already indicated, Augustine is arguing against the cogency of survival arguments as presented in the BICS essays. Some of the essays appeal to the mediumship of Mrs. Piper. In those cases, it is sufficient to argue (2), and it is not necessary to argue for (1) in order to support (2).

Braude is familiar with this strategy of argument. He has argued at length that survivalists have failed to rule out certain recalcitrant counterexplanations of the data, principally the motivated living-agent psi hypothesis (Braude, 2003, pp. 10–29). For example, he has argued that survivalist efforts to rule out this particular counterexplanation depend on false or unwarranted assumptions (Ibid, pp. 12–14), and he has elsewhere appealed to epistemic possibilities to counter survivalist criticisms of the appeal to motivated living-agent psi (Ibid., pp. 14, 16). Braude's arguments here do not depend on his providing positive evidence that the persons under consideration actually exhibited psi functioning, had particular motivations, etc. This is because, in the context in question, Braude was attempting to *undercut* specific survival arguments, not offer a positive argument *for* his motivated living-agent psi hypothesis.

But there is more going on here that needs to be fleshed out.

First, Braude et al. suggest that Augustine shows only the possibility of fraud or the possibility of the medium acquiring information from ordinary sources. But survivalists and their sympathizers have often claimed of such-and-such a medium, or of some particular sitting, that fraud was *impossible* or *inconceivable*, or that the medium *could not* have acquired certain information through ordinary sources (Hart, 1959, pp. 52–69).[39] Prominent early reports on Mrs. Piper's sittings routinely make such claims, rarely supported by any kind of argument – for example, Hodgson (1898, p. 285), Lodge (1890, pp. 446–447), and Myers (1890, pp. 438–440). William James, too. He claimed that Mrs. Piper "showed a most startling intimacy" with details of the private lives of sitters, "talking of many matters known to no one outside, and which gossip *could not possibly* have conveyed to her ears" (James, 1886, pp. 15–16, emphasis mine). Braude et al. quote this very passage from James, and they do so adjacent to their complaint against Augustine (Braude et al., 2022, p. 400). This makes their criticism of Augustine look implausible on the face of it. When researchers claim that a medium *could not possibly* have engaged in fraud or *could not possibly* have acquired information through ordinary means, gossip or otherwise, such claims are refuted by showing that fraud or the ordinary acquisition of information *was possible*, or that these exaggerated claims are otherwise unwarranted.

Second, mediumistic fraud is not merely (logically or epistemically) possible. Fraud is known to have frequently occurred in ostensible displays of physical and mental mediumship. Survivalists have long acknowledged this, which is why *the Proceedings* of the British and American Societies for Psychical Research are replete with efforts to address this problem and mitigate its impact on the case for survival. Augustine cites additional literature regarding this (Augustine, 2022a, p. 378), including investigations into the physical mediumship of Kai Mügge by Braude and Nahm (Braude, 2014, 2016; Mulacz, 2015; Nahm, 2014, 2015, 2016). The latter case also illustrates how seasoned investigators can fail to prove the extent of fraud in cases involving fraud despite the investigators implementing experimental controls over many sittings spanning several years. Mediumistic fraud exists. It has always existed since the early days of psychical research. And it has been significant enough to merit considerable attention by researchers. The sensible dispute is how we should weigh the frequency of known instances of fraud in our general assessments of the evidence from mediumship, as well as how it should bear on our assessment of particular cases. The latter is especially true in cases where a medium has not been caught engaging in fraud, even though investigators trained in trickery implemented strategies for uncovering fraud but were unable to confirm it.

On this matter, Braude et al. wrote:

> What matters is not whether fraud is possible, but whether it is actual, and whether (or to what extent) the evidence for a properly conducted experiment or investigation outweighs the evidence for fraud. Moreover, although there is a clear and rich history of mediumistic fraud... one cannot generalize from tainted cases to impugn the entire body of mediumistic evidence. (Braude et al., 2022, p. 406)

Whether the generalization in view here is warranted will depend largely on the nature of the impugning. The known widespread occurrence of mediumistic fraud is one of several factors that ought to inform our initial plausibility assessments of particular mediums, including Mrs. Piper. This does not require the logically fallacious inference *some mediums have been shown to be fraudulent, therefore all mediums are frauds*. Survivalists are fond of attributing this strawman argument to skeptics (Hart, 1959, p. 52, 255).[40] Braude et al. come close to doing the same in the quote above.

What looks more promising is their reference to *weighing* the evidence for fraud against the evidence for a properly conducted investigation or experiment.[41] I would like to have seen Braude et al. develop their point in greater detail, especially since Augustine argues that survivalists – for example, Nahm and DRW – do not properly weigh considerations of fraud and other contravening factors in *their* net assessments. But also, had Braude et al. developed their point a bit more, we might have a better understanding of how they propose to weigh the evidentially salient aspects of mediumship. We would presumably have a better idea of their criteria of evidential support. As far as I can see, Augustine is the one who offered an evidential marker here. Braude et al. did not. Consequently, an important conversation about how to weigh the evidentially salient aspects of a case did not occur.

My own view is that the "clear and rich history of mediumistic fraud" ought to inform initial plausibility assessments, or at least be an important *constraint* on favorable conclusions we draw about the evidential force of mediumship in general. This is what we observe in our wider doxastic practices.[42] Moreover, whether the failure to uncover fraud in particular cases ought to override initial skeptical assessments will depend on the particulars of the situation – for example, the reliability of the strategies deployed to obviate fraud in its different manifestations, overt and covert. In the case of Mrs. Piper, there is some reason to doubt the adequacy of Hodgson's protocols, as well as his less than unimpeachable documentation of Mrs. Piper's sittings (Gauld, 2022, pp. 92–93, 99; Munves, 1997).

Braude et al. also ignore Augustine's more nuanced point that a medium's seemingly impressive display of veridical information, including the kind that impressed William James, can be created whole cloth by the *combination* of undetected exposure to ordinary sources of information and various strands of the improbability principle – for example, the law of sufficiently large numbers, the probability lever, and the law of near enough (Hand, 2014). Nor is this scenario a mere (logical or epistemic) possibility. I have previously shown (Sudduth, 2021b, pp. 1006–1009) and Augustine references (Augustine, 2022b, p. 414) how such a scenario easily generates the misleading appearance of survival, *even without intentional fraud*. The protocols of past researchers – James and Hodgson, for example – were not sufficiently fine-grained to screen for these more subtle scenarios.

Furthermore, dark data at least complicate the evaluation of mediumship, even in the absence of conscious fraud. No investigator can reasonably claim omniscience, so there will be facts that did not register on the investigator's radar. In some cases, these overlooked or unnoticed facts can significantly impact how we should interpret the case. I have elsewhere shown this in connection with the James Leininger reincarnation case (Sudduth, 2021b, 2022a),[43] but it also applies to mediumship. And the older a case is, the more difficult it is to mitigate this problem. Mrs. Piper's mediumship took place over a century ago. It is doubtful that we can *now* know the kind of salient facts which, had they been known *then*, would have dissolved the convincing appearance of survival. And the methods of her investigators did little then to ameliorate the dark data problem for us now.

Nothing I have said implies that Mrs. Piper's mediumship has no evidential value. What is at issue is how strong that evidence is. **While the above skeptical considerations – mine and Augustine's – may not be strong enough in themselves to altogether undercut Mrs. Piper's mediumship as evidence *simpliciter* for survival, they do pose a more serious challenge to the notion that Mrs. Piper's mediumship *strongly* supports the survival hypothesis.** Skeptical considerations need not be deployed to show that certain data provide no evidence for survival at all. They can and often are deployed to deflate the extravagant assertions of survivalists and the bloated nature of their arguments. This is Augustine's primary target in his BICS critique. Augustine's so-called "negative appraisal" of mediumship is a negative appraisal of the extravagant, unwarranted conclusions prominent survivalists have drawn from insufficient, albeit interesting, data.

But Braude et al.'s demand that Augustine should produce positive evidence for mediumistic fraud or dependence on ordinary sources of information is otherwise mistaken. In the absence of a clear statement concerning what survivalists would accept as positive evidence, it would be premature for Augustine to attempt to meet the demand. After all, Braude et al. run roughshod over Augustine's effort to provide positive evidence for mind-brain dependence, even though he grounded his argument in a clear and widely deployed principle of evidential support. Until survivalists and their sympathizers are clear about *their* criteria of evidential support, the prospects for constructive dialogue with their critics remains bleak, and rightfully so.

To further illustrate the need for a more developed survivalist epistemology at this juncture, consider the following: survivalists tend to disregard the prior probability of fraud as positive evidence of fraud. Indeed, as shown in §5, they seem to disregard prior probabilities altogether, and for transparently bad reasons. Moreover, survivalists routinely claim that fraud can co-exist with genuine paranormal abilities or communications from the deceased. Survival literature is littered with such concessions to mixed mediumship, even in connection with Mrs. Piper. Survivalists have accused Mrs. Piper, or at least her secondary personality Phinuit, of fishing, deception, and other illicit techniques of information acquisition. So, even if skeptics were to adduce positive evidence that a particular medium engaged in fraud on some occasion(s), what reason is there to believe that survivalists would regard such facts as evidence against the medium's alleged extraordinary abilities? What non-question-begging reason is there to suppose that a mixed medium is a genuine medium who engages in fraud half the time as opposed to a complete fraud who has only been half found out? Survivalists, not skeptics, have the burden to explain how we can separate the mediumistic wheat from the mediumistic chaff.[44]

Finally, to return to my earlier point, Augustine was not arguing that fraud or gossip is a sufficient rival explanation of the data. This is a misreading of Augustine's otherwise lucid argument. Claiming that his "dismissal" of Mrs. Piper's mediumship requires that he "demonstrate that his gossip hypothesis has some evidence in its favor, and also that it is adequate to a wide range of facts" (Braude et al., 2022, p. 400) is a plausible requirement only if he were arguing that the gossip hypothesis provides an at least equally good explanation of Mrs. Piper's mediumship. That may or may not be true, but it requires a different kind of argument, not the one Augustine is making. **He was arguing that survivalists overstate the evidential force of mediumistic data in part because they ignore the subtle ways information can be aggregated into a narrative, giving the misleading appearance of survival.** This is precisely the context of the Augustine block quote Braude et al. give us (Ibid., p. 400).

Elsewhere in their reply, Braude et al. make the same logical mistake when speaking more broadly of anti-survivalists:

> We have seen that anti-survivalists must do more than assert that evidence suggesting survival can be accounted for by appealing to the possibility of fraud or other Usual Suspects. They must wallow in the grubby details and show that fraud (or whatever) is either likely or actual. (Ibid. p. 403)

By parity of reasoning, a survivalist could argue against Braude as follows: *Braude must do more than assert that evidence suggesting survival can be accounted for by appealing to the possibility of motivated living-agent psi; he must wallow in the grubby details and show that motivated living-agent psi is either likely or actual in said cases.* Such an objection misses the nuanced nature of Braude's criticisms of traditional survival arguments, namely their being directed "to show just how daunting of a task it is to rule out super-psi explanations" (Braude, 2003, p. 23). Augustine's point is that something similar is true in the case of mediumship when it comes to ruling out conventional explanations or failing to let such explanations constrain the net assessment of the evidence. In Augustine's view, the BICS essays fail at this point. And nothing Braude et al. argue comes remotely close to showing that this is not the case.

## 10. The Alleged Improbability of Fraud and Other Counterexplanations

But have survivalists not shown that fraud, chance-coincidence, the influence of ordinary sources of information, and other non-paranormal alternatives to survival *are* improbable, at least in the better cases of mediumship? Braude et al. accuse Augustine of ignoring such improbabilities:

> Granted, Augustine mentions that private detectives tailing Mrs. Piper never found anything suspicious. But he is mute on the significance of the many times Mrs. Piper got intimate hits with anonymous sitters she was meeting for the first time—including proxy sitters and people who, during the medium's visit to England, happened to be traveling through Cambridge. So although

> it is certainly relevant that Mrs. Piper was never caught cheating, survivalists do not need to rely on a never-caught-cheating card. Augustine simply ignores the strongest reasons for thinking that cheating is highly improbable. (Braude et al., 2022, pp. 400–401)

I agree with Braude et al. that the survivalist's "strongest reasons" for thinking that cheating is highly improbable deserve attention. In fact, they deserve more attention than survivalists themselves have given them. I will scrutinize these alleged reasons below. But it is important to first appreciate why Augustine did not address these "strongest reasons." As he explained in his reply to Braude et al., the criticism that he ignored the strongest reasons for supposing that cheating is highly improbable is based on a misunderstanding of his argument (Augustine, 2022b, p. 414).

### *Augustine and Mrs. Piper's Mediumship*

First, the relevant part of Augustine's discussion is his assessment of how Nahm (2021) and Delorme, Radin, and Wahbeh (DRW) (2021) rank the evidence from mediumship in their essays. The latter argue that the evidence merits a letter grade of B+, and Nahm offers a similar favorable score-card assessment. Augustine argues that neither DRW nor Nahm offers cogent arguments for their respective conclusions. He offers several considerations, most of which have nothing to do specifically with Mrs. Piper, but which have everything to do with net assessments.[45] Augustine is arguing that these survivalists have not provided a good enough reason to accept *their* conclusion(s) about the strength of mediumistic evidence. And this is because they have poorly handled potentially contravening evidence in their net assessments. If we shift attention specifically to Mrs. Piper, we have the circumstances in which Mrs. Piper demonstrated "intimate hits" (positive evidence). What we need to ask about this positive evidence is whether it is as strong once contravening factors are introduced. Otherwise put, **the issue is how positive evidence and contravening factors are weighted against each other in our net assessments of the evidence**.

Second, since Augustine was responding to Nahm and DRW, he selected features of Mrs. Piper's mediumship which they, principally Nahm, had mentioned in their papers. If Augustine was mute on the features of Mrs. Piper's mediumship which Braude et al. mention, it was only because the survivalists to whom he was responding were mute on this matter. Three of the contributors to Braude et al. were Delorme, Radin, and Wahbeh, and the above criticism of Augustine seems more appropriately directed to Braude's coauthors, who neglected to give prominence to these aspects of Mrs. Piper's mediumship, merely listing her name as one among several "historically well-documented cases of accurate mediums" (Delorme, Radin, & Wahbeh, 2021, p. 13).

### *Moving Beyond Impressionistic Reasoning*

Nonetheless, Braude et al. have raised an issue that bears on the wider survival debate, specifically in connection with how survivalists purport to rule out counterexplanations. It has been common for survivalists to claim that certain counterexplanations are implausible or improbable, and so must be rejected. This plays an important role in IBE survival arguments since such arguments must "rule out" rival explanations. Since Braude et al. raised this specifically in connection with the mediumship of Mrs. Piper, I will consider it largely in that context.

First, survivalists from Hodgson forward typically do not *argue* that cheating is improbable, at least not explicitly. They *assert* its improbability as a matter of personal impression, belief, or opinion. True, they cite reasons why they *regard* the fraud hypothesis as improbable, but they do not show that such reasons *make* the fraud hypothesis improbable. Consequently, it looks like "the improbability of fraud" is merely a subjective probability embedded in a personal narrative. It is a report of the survivalists' own degree of incredulity at the suggestion that fraud was at work. Unsurprisingly, commentators such as Hart (1959, ch. 4) have done little more than make an appeal to the authority of investigators such as Hodgson, James, Myers, Tyrrell, and Drayton Thomas, who were confident that fraud was improbable. But what is required is an argument that shows that those reasons are good reasons for supposing that fraud is improbable.

Second, Braude et al. suggest that the strongest reasons are not that Mrs. Piper was never caught cheating, even though detectives shadowed her at various times. The lion's share of improbability seems to be based on "the significance of the many times Mrs. Piper got intimate hits with anonymous sitters she was meeting for the first time—including proxy sitters and people who, during the medium's visit to England, happened to be travelling through Cambridge" (Braude et al. 2022, pp. 400–401). This is a start, but what we need to know is why anyone not antecedently committed to the truth of the survival hypothesis ought to regard the fraud hypothesis as improbable *given such facts*. We need an argument from these facts to the conclusion that fraud is improbable. Braude et al. do not present such an argument, nor source anyone who does.

So, let me suggest one.

Consider the following argument. (i) *Mrs. Piper's mediumship had certain features O,* (ii) *if Mrs. Piper were cheating, O would be quite surprising – that is, O would be improbable. So, we should conclude that (iii) the fraud hypothesis is improbable*. The same kind of argument can be run for the chance-coincidence hypothesis and influence from ordinary sources of information, or any combination of non-paranormal alternatives to survival. The argument relies on the observation O and a likelihood – **Pr(O | fraud) = very low** – and concludes that **Pr(fraud | O) = very low**. Therefore, we can rule out the fraud hypothesis.

Let me flesh out the argument. What kind of observations are such that they would allegedly be highly improbable given the fraud hypothesis? If Mrs. Piper cheated, then it would be improbable that she would be able to convey the quantity and quality of veridical data that she did, especially given the introduction of sitters under pseudonyms, removing her from her native locale (Boston) and placing her in an unfamiliar social environment (England), etc. Perhaps this is what Braude et al. are suggesting when they appeal to such positive evidence. But it applies to the never-caught-cheating card, too. After all, one can argue that it is improbable that Mrs. Piper would have never been caught cheating if she had been cheating, given the use of spies and the efforts of skeptics to ferret out deception. Michael Sage wrote, "during the fifteen years the experiments [with Mrs. Piper] have continued, all the suggestions made by sceptical and sometimes violent objectors have been kept in view, that the fraud might be discovered, if fraud there were. All has been in vain" (Sage, 1904/2007, p. 38).[46]

### *The Argument of the Sophisticates*

I did not invent the above argument *de novo*. It actually originates from an early phase in the history of parapsychology and survival research. I refer to it as the argument of the sophisticates because it at least has the veneer of being logically rigorous. Unlike the impressionistic reasoning of many survivalists, it makes explicit use of probabilistic reasoning in the form of arguments relying on statistical data.

One good example is John Thomas (1937). He argued that the chance and fraud hypotheses were each improbable as explanations of experimental results with mediums because these hypotheses confer extremely low probabilities on the data collected. He provided a detailed description of the arrangements and circumstances of various sittings with different sensitives and mediums, including Mrs. Osborne Leonard, with attention to protocols designed to obviate fraud. Thomas rejected the idea that a mere high percentage of hits is evidence of the absence of fraud. "Indeed," he says, "definiteness, high veridicality, and striking accuracy in a series of records might be expected from effective fraudulent practices" (Ibid., p. 129). Instead, he emphasized experimental protocols that would make it improbable, though not impossible, that the quantity of hits could be fraudulently produced – for example, the anonymity of sitters, switching out of stenographic recorders, no advance notice of the sittings, a large number of sittings over many years, and the use of diverse locations. He also considered different go-betweens to assist in fraud (Ibid., pp. 132–148) and paid particular attention to the content of sittings, including facts remote in time, obscure in nature, or only naturally accessible at remote locations. If the fraud hypothesis were true, then many improbable things would also have to be true. "Fraud," he concluded, "is improbable in the highest degree" (Ibid., p. 129). And, "The fair conclusion, then, is that the fraud explanation, while not absolutely impossible, is fantastically incredible" (Ibid., p. 148).

This type of reasoning is clearer in connection with the examination of the chance-coincidence hypothesis, which is more amiable to mathematical calculations of probability. Saltmarsh and Soal (1930) presented a method for estimating the value of evidence for paranormal knowledge as compared to chance in the sittings of Mrs. Warren Elliott. Saltmarsh, with the assistance of statistician R.A. Fisher, calculated that the recorded hits in a particular sitting with Mrs. Warren had a probability of one in a thousand million given chance. "I submit," Saltmarsh said, "that this result is such that the hypothesis of chance alone could have produced this amount of veridicality is definitely excluded" (Ibid., p. 271). Similarly, Thomas (1937) claimed, in connection with other experiments, that the statistical analysis showed that given chance, the probability of the various results ranged from 5 in $10^7$ to 4 in $10^{39}$ to 10 in $10^{40}$. He concluded, "one may definitely exclude the hypothesis that chance alone can account for the degree of veridicality in these data" (Ibid., p. 163).

### *The Fallacy of Probabilistic Modus Tollens*

The above examples suffice to show that parapsychologists and survivalists *have* presented arguments to show that, with respect to mediumship, the fraud and chance hypotheses are improbable. Unfortunately, the form of argument on which they have relied to show this is bogus. The purported inference is based on a commonly encountered fallacy in probabilistic reasoning. I suspect that parapsychologists and survivalists who commit this mistake do so because of an incorrect use of R.A. Fischer's

problematic statistical significance tests.

Happily, I am not the first one to identify this fallacy. C.D. Broad discussed it in his critical remarks on the statistical analyses of results in parapsychological experiments:

> Suppose that a certain hypothesis would, if accepted, render *extremely improbable* certain propositions which are found on observation to be *true*. Then, that extreme improbability is reflected back on the hypothesis, and it becomes unreasonable to accept it. This may be compared with the following principle, which is certainly valid. Suppose that a certain hypothesis would logically entail the *falsity* of certain propositions which are found on observation to be *true*. Then the hypothesis must be rejected as *false*. (Broad, 1962, pp. 74–75.)

Broad here compares the purported probabilistic inference to a perfectly valid form of deductive inference known as *modus tollens*: for any propositions, p and q, *if p then q, not q, therefore not p*. As Broad says, it looks like the suggested inference is a probabilistic version of the valid inference: *if p, then probably not q, q, therefore (probably) not p*. Sober has referred to this as **probabilistic modus tollens**. Sober and Royall have shown why the inference is a flaw in Fisher's significance tests (Royall, 1997, pp. 65–68; Sober, 2008, pp. 48–58). Broad also showed why the argument was unacceptable (Broad, 1962, p. 79). One salient point raised by Broad, Royall, and Sober is that **hypotheses must be tested against an alternative**. As the earlier DNA match example illustrated, we need to know whether O is more probable under $H_2$ than it is under $H_1$, not simply whether O is improbable given $H_1$. In the case of mediumship, the survivalist needs to show that the survival hypothesis confers a greater probability on the observational evidence than does the fraud hypothesis. The observations will then favor survival over fraud.

If one does not find the fallacious nature of the inference in question intuitively obvious, it is very easy to find examples of hypotheses that confer hugely low probabilities on an observation without the hypotheses themselves plausibly being regarded as having (hugely) low probabilities. Twenty-six consecutive black numbers came up on the roulette wheel at Monte Carlo in 1913, with odds of about 1 in 137 million (Hand, 2014, p. 83). This outcome was hugely improbable given that the roulette game was fair, but it is clearly implausible to infer that a fair roulette game was improbable merely because that hypothesis confers a hugely low probability on the outcome. Evelyn Marie Adams won the New Jersey lottery twice in four months in the 1980s, with odds of one in a trillion (Ibid, p. 86). This outcome was also hugely improbable given that it was a fair lottery. It is more impressive than Mrs. Warren Elliott's mediumship, the results of which were one in a billion by comparison. So, if Saltmarsh's reasoning was correct in the case of Elliott, *a fortiori* the chance hypothesis should be excluded in the case of Evelyn Marie Adams. But this is absurd. We rightly do not conclude that the hypothesis of a fair lottery was improbable, and so must be rejected. We also should not regard the observation as evidence against the hypothesis. In both the Monte Carlo and lottery example, $Pr(O \mid H) \neq Pr(H \mid O)$; the respective probabilities are not even close.

What is the source of this error among survivalists? It may be a simple conflation of two kinds of conditional probabilities – $Pr(H \mid O)$ and $Pr(O \mid H)$. Hence, they think that since $Pr(O \mid H)$ = low, it must be that $Pr(H \mid O)$ = low. Survivalists who pay little attention to the rules of probabilistic reasoning are especially vulnerable to being duped by such fallacies. But I suspect that the more widespread cause of the error lies in the parapsychological and survivalist appropriation and misapplication of frequentist statistical theories. Survivalists often rely on statistical significance tests to draw conclusions. Several of the prize-winning BICS papers did exactly this (Beischel, 2021; Long, 2021; Neppe, 2021).[47] On a prevalent interpretation of such tests, we should reject a hypothesis when it makes the probability of some observation sufficiently low, for example, when $p < .05$ or $p < .01$. However, as the previous examples show, when a hypothesis confers a low probability on an observation, it is not reasonable to conclude that the hypothesis is improbable, that the observation is evidence against the hypothesis, or that we should reject the hypothesis. As previously illustrated, sometimes it is actually evidence *for* the hypothesis because the alternative hypothesis confers an even lower probability on the observation. Significance tests as a rule for epistemic evaluation and rejecting hypotheses on evidential grounds are simply incorrect (Royall, 1996, pp. 65–68; Sober 2008, pp. 48–58). Survivalists who rely on such reasoning are underwriting their survivalist claims with dubious inferences.

Braude et al. chided Augustine for not addressing the strongest reasons survivalists have for regarding fraud as improbable. Those strongest reasons appear to be either mere subjective credence or involve a fallacy in probabilistic reasoning. So, the reasons are either irrelevant or unreasonable. But since Braude et al. decided to raise the issue, I chose to put it to rest. Here we see another illustration of how the survivalist's "strongest reasons" for believing something turn out on further scrutiny to

be no good reason to believe it. And we can invoke Fisher against the survivalists who rely on his significance tests to draw unwarranted conclusions. As Fisher correctly pointed out, if a hypothesis H says an observation O is improbable and O occurs, then one of two things is true, either H is false or *something very improbable has happened* (Sober, 2008, p. 56). Survivalists have not offered any good reason to prefer the former possibility to the latter. Indeed, they seem oblivious to the options in the conceptual space.

## 11. Mediumship and the Logic of Confirmation

In the previous sections, I looked at Augustine and Braude et al. on some perennial issues related to mediumship, specifically the specter of fraud and the possibility that Mrs. Piper relied on ordinary channels of information. What is most relevant there, I have argued, is how Braude et al. consistently miss the structure of Augustine's arguments and leave crucial questions in the logic and epistemology of survival arguments unanswered.

In this section, I address other issues related to mediumship and the logic of confirmation covered in Augustine's critique and to which Braude et al. offer various criticisms.

Augustine wrote, and Braude et al. reproduce in their essay, the following:

> . . . the fact that historical trance mediums' accurate statements must be fished out of reams of twaddle (James, 1909, p. 115) is surely relevant to any plausibility assessments here, as is the agreed-upon fact that a significant proportion of the entities that they claimed to contact were undeniably fictitious constructions of the mediums' own minds. Certainly, the latter more than offsets any gain provided by appealing to the "never caught cheating" card, which is hardly conclusive in any case since Mrs. Piper had access to gossip within a large web of her community connections. (Augustine, 2022a, p. 377)

Before looking at the criticisms, Braude et al. offer in response to this passage we need to clarify how Augustine intends to leverage his points. He is responding to the essays by DRW and Nahm, both of whom comment favorably on the total force of the evidence from mediumship. As indicated earlier, DRW assign mediumship the letter grade of B+ (in between good and strong evidence), and where B+ implies that the data are "implausibly explained by conventional science" (DRW, 2021, p. 10) and that there is "no plausible materialistic (psychology or neuroscience) explanation" (Ibid., p. 11). Nahm refers to the "astonishing quantity and quality of accurate information" as among the "most compelling" facets of mediumship, adding that in the case of Mrs. Piper, the medium was shadowed by private detectives to determine whether she was acquiring information through ordinary means (Nahm, 2021, p. 11).

Augustine's criticism is that Nahm and DRW ignore or fail to properly weigh salient counter considerations – (i) the significant number of ostensible spirits being fictitious constructions of the medium's own mind, (ii) the mixture of accurate statements and twaddle, and (iii) Mrs. Piper's access to gossip as an ordinary source of information. (i) and (ii) are relevant to net plausibility assessments. They more than *offset* any gain the "never caught cheating card" provides, which cannot be *conclusive* on account of (iii). These considerations require that Nahm and DRW downgrade their highly favorable assessment of the evidence from mediumship or explain why such data make no difference to their favorable assessment.

### *The Evidential Relevance of Fictitious Controls*

In response to Augustine's argument concerning fictitious controls, Braude et al. said, "as far as clearly fictitious mediumistic control personalities are concerned, even if one grants the reality of survival, the existence of these controls would not be surprising. They might even be exactly what many survivalists expect" (Braude et al., 2022, p. 400). This is an interesting point and potentially relevant to Augustine's argument. Augustine's suggestion is that the data on fictitious controls and twaddle count against or lower the probability of the survival hypothesis in a way not acknowledged or anticipated by Nahm and DRW. I would have liked Braude et al. to have better dialed in their point to Augustine's actual argument by adding, for example, *that while Nahm and DRW failed to mention the phenomenon of fictitious controls, it would not downgrade their favorable assessments because....* That would properly contextualize Augustine's argument.

The question remains, though, as to whether Braude et al. *can* successfully leverage the points they make at this juncture to dislodge Augustine's criticisms or otherwise reinforce the arguments Nahm and DRW present. I think this is a formidable task, and it reveals even deeper flaws in survival arguments. To see this, we need to address pertinent issues in the logic of confirmation.

Braude et al. say that the existence of Mrs. Piper's controls would not be surprising given survival, and they might even be what many survivalists expect. Of course, what survivalists might or might not expect as a matter of their psychology is irrelevant. What matters is wheth-

er the expectation is warranted, given the *content* of the survival hypothesis. Unfortunately Braude et al. only assert this; they do not show it. But showing it would be crucial to address Augustine's argument. Why exactly, in point of logic, are Piper's fictitious controls *not surprising* given survival? Two possibilities: (i) the hypothesis leads us to expect the data or (ii) the hypothesis does not lead us to expect the absence of the data. My vegetable garden may have tomato hornworms and blite. This may be unsurprising given the hypothesis that there is an invisible gardener who oversees it. Why? Because my hypothesis is *precise* enough to lead me to expect these data or it is *vague* enough to accommodate the data by not predicting that we should not observe the data. The same is true for fictitious controls and the survival hypothesis. The former may be unsurprising given the latter either because the survival hypothesis can be forced to fit any observational data – fictitious controls, fraud, alien abduction experiences – or because it has been bulked up enough to make predictions.

The Braude et al. reply hedges at this juncture. The first sentence (in the quote above) is compatible with both the survival hypothesis leading us to expect fictitious controls and the hypothesis not leading us to expect the absence of fictitious controls. The second sentence says the first scenario *might* be true. Braude et al. say "might" because they know that whether the survival hypothesis leads us to expect fictitious controls depends on auxiliary assumptions. Apparently, they do not wish to adjudicate this issue in their reply to Augustine. But there are problems here that undermine the attempt to neutralize Augustine's criticisms of Nahm and DRW.

First, suppose we have a very bold survivalist who says that *the survival hypothesis leads us to expect the existence and/or nature of fictitious controls*. This is insufficient. Remember, hypotheses must be tested against alternatives, in this case, either the negation of the survival hypothesis or some specific naturalistic, non-survival hypothesis. What matters is whether fictitious controls are *more* to be expected if survival is true than if survival is false (Bayesian likelihood), or whether they are *more* to be expected under the survival hypothesis than they are under some alternative naturalistic hypothesis (likelihoodist likelihood).

- If Pr(fictitious controls | some naturalistic hypothesis) > Pr(fictitious controls | the survival hypothesis), then the existence/nature of fictitious controls will favor the naturalistic hypothesis over the survival hypothesis, *even if the survival hypothesis leads us to expect such entities*. Similarly, under the Bayesian view, if Pr(fictitious controls | ~ the survival hypothesis) > Pr(fictitious controls | the survival hypothesis), then the existence/nature of fictitious controls will lower the probability of the survival hypothesis, *even if the survival hypothesis leads us to expect such entities*.
- To neutralize the above counterarguments, the survivalist must show, not that the survival hypothesis leads us to expect fictitious controls, but at a minimum that Pr(fictitious controls | naturalistic hypothesis) = Pr(fictitious controls | the survival hypothesis), or to counter the Bayesian view, that Pr(fictitious controls | ~ the survival hypothesis) = Pr(fictitious controls | the survival hypothesis). That is, the survivalist needs at least to show that fictitious controls are *just as* expected given the survival hypothesis as they are given a naturalistic alternative or given the negation of the survival hypothesis.
- It is unclear how survivalists can successfully mount the neutralizing argument above.
    - They would have to enlist auxiliary assumptions to generate well-defined likelihoods and show that such likelihoods are approximately equal to the likelihood of a proposed naturalistic alternative or (more ambitiously) approximately equal to the catchall likelihood of the negation of the survival hypothesis.
    - The required auxiliary assumptions are likely to be at least as contentious as the survival hypothesis itself. They may be ad hoc or lack independent justification. (I will explore the problem of auxiliary assumptions in greater detail below.)
    - By contrast, given the well-understood human motivations that underlie fraud (mediumistic and otherwise) and the varied phenomena of abnormal psychology – for example, dissociative phenomena and savant syndrome – neither the existence nor the nature of fictitious controls is surprising if the survival hypothesis is false. And, unlike the survival hypothesis, no extravagant assumptions are required.

Perhaps this is why Braude et al. do not attempt to show that the survival hypothesis leads us to expect fictitious controls as much as naturalistic hypotheses do. Given their own comments about auxiliary assumptions, they are wise not to step on that landmine. But in that case, they cannot get sufficient leverage against Augustine's argument at this juncture.

Second, suppose we take the more modest view that the survival hypothesis does not lead us to expect that there should be no fictitious mediumistic controls. In that case, hasn't the survivalist successfully also blocked the inference to a disconfirming observation? No. If the con-

tent of the survival hypothesis does not lead us to expect anything one way or the other relative to the existence/nature of fictitious controls, why is this exactly? In the space of possibly true auxiliary assumptions that can be deployed to bulk up the survival hypothesis, there will be some that, when conjoined to the survival hypothesis, will lead us to expect fictitious controls and some that will lead us to expect no fictitious controls. How shall we choose? Prima facie, there is a problem here. There is no independent reason to favor one over the other. But this is arguably true for many auxiliary assumptions without which the survival hypothesis would lead us to expect precious little at all (Sudduth, 2016, pp. 214–218, 238–240). To that extent, there will be precious little in the way of observational evidence to support the survival hypothesis as well. Predictively impotent hypotheses may be shielded from empirical disconfirmation, but this comes with a steep cost: the loss of empirical confirmation. So, this move offers no help to the empirical survivalist.[48]

### *Twaddle and Truth*

The above points also apply to Augustine's appeal to mediumistic twaddle as a salient fact which survivalists poorly handle in their favorable net assessments of mediumship.

It is quite clear why, if mediumistic communications are *not* actually originating from deceased persons, we would expect considerable triviality, falsehoods, and unverifiable claims and extended discourses about the afterlife and mundane matters, especially where these reflect religious conceptions of the afterlife that were antecedently part of the cultural milieu. For example, it would be clear why Mrs. Piper's G.P. control would, unlike the living G.P., be incompetent in philosophy and literature. Like knowing French, philosophy is both a knowledge and a skill not easily reproducible by the medium who has little more than a passing acquaintance with such subjects. We would also expect communications to be a mixture of true and false statements, especially where (general and specific) true statements are contextually dependent on fishing, physical cues, rational inferences, the content of previous sittings, and exploiting aspects of the improbability principle such as the probability lever and the law of near enough.

From the perspective of the survivalist, though, things are not so clear. This is partly due to the survivalist's own assumptions. Survivalists wish to count accurate information conveyed to the medium as a confirmation of the survival hypothesis. The same holds for the medium's exhibition of personality traits and skills which resemble the deceased. This is entirely reasonable, of course, if we are postulating the persistence of a person's psychological profile, especially their memories. Our ordinary, everyday judgments about personal identity depend to varying degrees on the recognizable psychological continuity of persons. The challenge for the survivalist is to sensibly explain how all that can count as a confirmation that the deceased is communicating through the medium, but the failure to observe such outcomes in any given sitting, or observing anything that conflicts with them, does not count as a disconfirmation of the hypothesis that a deceased person is communicating.

Let me clarify the problem here. One *can* invent a "theory" (hypothesis plus auxiliary assumptions) that will accommodate deviations from the survivalist's default expectations. Alan Gauld's "overshading" theory is one such example, perhaps the best on offer. And long before Gauld, thinkers such as Hodgson, Hart, and Ducasse toyed with tweaking the survival hypothesis to account for such deviations from default expectations. But this kind of reasoning does not succeed at immunizing survivalist arguments from skeptical criticisms. First, the effort to bulk up the survival hypothesis to accommodate apparently contrary data depends on ad hoc or otherwise epistemically suspect auxiliary assumptions. Second, the result is a survival model that can, in principle, accommodate pretty much any datum. But *a theory that accommodates everything predicts nothing*. Such a model is evidentially useless within a logically rigorous framework such as confirmation theory. Indeed, it seems useless given fairly prosaic standards of reasoning. This problem needs to be addressed if survivalists are to present something more substantial than a narrative that exhibits the illusion of evidential support.

### *Returning to the Braude et al. Reply*

Braude et al. did not comment on the salient issues in the logic of confirmation, which are baked into Augustine's entire critique. This is unfortunate. We need a serious conversation about the justification of the kinds of auxiliary assumptions required for the survival hypothesis to generate even approximate or general expectations concerning how the empirical world should look if survival is true, as well as how it should look if survival is false. Again, it is interesting that Braude discusses this crucial issue in his prize-winning BICS essay, but he does not make use of those resources in response to Augustine, where they would have been most useful given Augustine's central criticisms. Instead, Braude et al. refer to Augustine's "cursory dismissal of Mrs. Piper's mediumship" (Braude et al., 2022, p. 400). They criticize him for failing

to consider positive evidence of her paranormal abilities, and they contrast it with the superior kinds of critical assessment found in Alan Gauld's work on mediumship.

These criticisms of Augustine are not calibrated to address his actual arguments. Unlike Gauld, Augustine was not offering an assessment of Mrs. Piper's mediumship *per se*, nor was he dismissing her mediumship *per se*. As the relevant passage from Augustine shows, he was offering a critical assessment of *survivalist assessments* of Mrs. Piper's mediumship. He was arguing that they ignore or mishandle facts – that pesky negative evidence – that are relevant to net assessments of mediumship, and he was focused particularly on this flaw in Nahm and DRW. To the extent that the arguments Augustine is critiquing incorporate the kind of positive evidence Braude et al. demand, his argument presupposes that evidence. To the extent that the arguments he is critiquing do not incorporate such evidence, it is not Augustine's oversight. Nor would it be relevant to Augustine's specific criticisms. He was not asked to improve on the arguments for survival in the BICS essays. If anything, that would have been a task for Braude et al. But the central issue here is not the implications of unstated positive evidence. It is the implications of survivalists not properly handling any of the stated counterevidence, and this remains an issue regardless of the survivalist's stock of positive evidence.

Since Braude et al. do not address Augustine's argument, there is no engagement with the crucial issue of how we ought to properly weigh ostensible counterevidence in the wider data set. Moreover, the need for survivalist transparency concerning the structure and cogency of their intended arguments goes unaddressed. Set aside the goal of advancing the survival debate. This is a lost opportunity to simply have a lucid debate.

## 12. The Significance of Failed Tests

The other confirmation-related issue concerns the implications of failed tests for survival. Here we need to distinguish between the implications of what survivalists routinely assume and the implications of adopting other kinds of assumptions. Augustine is primarily concerned with the former. He considers the importance of failed tests for the survivalist in connection with survivalist arguments concerning data collected from mediumship (Augustine, 2022a, pp. 368–370), OBEs/NDEs (Ibid., pp. 369–370), and cases of the reincarnation type (Ibid., p. 381).

Recall the wider context here, specifically **Augustine's basic argument** presented earlier:

[A1] If belief in the survival hypothesis is well-supported, then it is proportioned to all of the available relevant evidence. (Ibid., p. 371)

[A2] Belief in the survival hypothesis is not proportioned to all of the available relevant evidence. (Ibid., pp. 371–384, especially pp. 374-375)

Therefore:

[A3] Belief in the survival hypothesis is not well-supported. (Ibid., pp. 365, 390).

We saw earlier that neurophysiological data provide one kind of potential counterevidence to the survival hypothesis. According to Augustine, survivalists have not properly weighed this counterevidence in their strongly favorable assessments of the alleged evidence for survival. But they have also failed to properly weigh the counterevidence provided by their own failed experimental results. Augustine contends that this provides additional support for premise [A2] of his basic argument. He surveys a variety of unsuccessful survival tests. For example, mediums have consistently failed to decipher encrypted messages or open user-set combination locks in tests arranged by living persons to be executed posthumously by the formerly living person communicating keywords or phrases through the medium. And despite several decades of attempts to have OBE and NDE subjects identify visual targets, including in various controlled experiments, there have been no consistent positive results.

It is important to be clear about the conceptual framework of Augustine's argument. As he explains, survivalists have proposed empirical tests for mediumship. These tests assume that the survival hypothesis can be tested by the observational outcomes of experiments with mediums. More specifically, they assume that if the survival hypothesis is true, we would expect some observational datum. This is baked into IBE survival arguments. On this view, the survival hypothesis must *account* for the data, where this *accounting* requires that the survival hypothesis leads us to expect the observational data or leads us to expect the data more than alternatives do – for example, it leads us to expect that the medium would possess knowledge or exhibit personality traits or skills characteristic of the deceased. When these features occur in mediumistic sittings and alternative explanations are ruled out, survivalists attribute explanatory merit to the survival hypothesis, and they regard the data from the sitting as evidence for the survival hypothesis.[49]

Here is the problem**.** Bayesian confirmation is *symmetric*. O confirms H just if not-O disconfirms H. As noted earlier, O confirms (= raises the probability of) H just if $Pr(H \mid O) > Pr(H)$, but this is equivalent to $Pr(O \mid H) > Pr(O \mid \sim H)$. An erratic EKG is more probable if someone is having

a heart attack than if they are *not* having a heart attack. So, O raising the probability of H entails that O is more likely to occur under H than it is under ~H. But in that case, a normal EKG disconfirms (= lowers the probability of) the hypothesis that a person is having a heart attack. Hence, Pr(H | O) > Pr(H) just if Pr(H | ~O) < Pr(H). So, for a hypothesis H to genuinely lead us to expect an observation O in the sense of *predicting* O, H cannot also lead us to expect ~O or anything incompatible with O. Otherwise stated, if an observation O raises the probability of H, ~O lowers the probability of H. By contrast, according to strict Popperianism some propositions are verifiable but not falsifiable – for example, there exists a black swan. Neither Bayesian nor likelihoodist models permit this with reference to confirmation/disconfirmation.

Augustine's criticism concerning the significance of failed experiments is a straightforward implication of the logic of hypothesis testing outlined above. He is criticizing survivalists for painting over outcomes that are not what *survivalists* would expect given the kind of observational data which *they* regard or would regard as confirmatory of the survival hypothesis. We have plenty of examples: communicators conveying twaddle, inaccurate statements, or their failure to give requested information the deceased would be uniquely positioned to know. Similarly, we have OBE and NDE subjects who fail to accurately report visual targets or children who make false claims about an alleged previous personality. And while it is easy to find claims made by children that correspond to autobiographical facts of a previous personality – this is supposed to be evidence for the reincarnation hypothesis – the challenge is to provide a net assessment that acknowledges and shows how disconfirming facts are being factored into the net assessment. This goes straight to premise [A2] of Augustine's basic argument.

A counterfactual scenario might help underscore Augustine's point. Suppose that the G.P. communicator had exhibited considerable fluency in philosophy, literature, and Greek and Latin. Would the survivalist not count such data as a confirmation of the hypothesis that G.P. was the surviving George Pellew? But if the survivalist would treat such a scenario as confirmatory of the survival hypothesis, then it is a crippling inconsistency to suppose that G.P.'s actual deviations from the knowledge and personality of George Pellew are not disconfirmations of the hypothesis that G.P. is the surviving personality of Pellew. Similarly, if an OBE or NDE subject's successful identification of a visual target counts as evidence for mind-brain independence, the inability to do this should count against that hypothesis. If a child's veridical information about a previous personality confirms the reincarnation hypothesis, a child's false claims about a previous personality should count as a disconfirmation of the reincarnation hypothesis. Not decisively, of course, because neither disconfirmation nor confirmation is conclusive.

To be clear, Augustine is not leveraging experimental failures to support the claim that the survival hypothesis is (probably) false.[50] He is arguing that the failure of survivalists to acknowledge and show how they weigh experimental failures in their net assessment provides good reason to believe [A2]. This also undermines the favorable net assessments of the evidence in the essays Augustine was targeting. Since coherent testability criteria are essential to scientific reasoning, the essays do not represent a scientific approach to the alleged evidence for survival.

## 13. Survivalist Rescues and Contrastive Confirmation

Some survivalists have acknowledged the general problem in the previous section and have attempted to engage it. Unfortunately, their responses have been implausible. Instead of acknowledging the existence of data which count against the survival hypothesis and which therefore requires appropriately downgrading the weight of the total evidence, they try to neutralize the disconfirming implications of the data. Richard Hodgson provides an early example of this with respect to mediumship. He offered several conjectures designed to explain why mediumistic communications should have "obscurities and deficiencies" (Hodgson, 1898, p. 366) and that therefore these are features not bugs of the survival hypothesis.

Hodgson's complete discussion on the topic (Ibid., pp. 366–392) is worth reading for context, but I will limit myself to a particularly apt portion of his discussion:

> [I]f the "spirits" of our "deceased" friends do communicate as alleged through the organisms of still incarnate persons, we are *not* justified in expecting them to manifest themselves with the same fulness of clear consciousness that they exhibited during life. We should on the contrary expect even the best communicators to fall short of this for the two main reasons: (1) loss of familiarity with the conditions of using a gross material organism at all – we should expect them to be like fishes out of water or birds immersed in it; (2) inability to govern precisely and completely the particular gross material organism which they are compelled to use.... [T]he confusion and failure which we find in Mrs. Piper's trance communications, are so far from being what we

> should *not* expect, that they are exactly what we *should* expect, if the alleged spirits are communicating. (Ibid., pp. 366–367)

There are at least three problems with Hodgson's reasoning.

First, we need not suppose that discarnate spirits will manifest themselves with the *same* fullness of clear consciousness they exhibited during life. This overstates the expectation and thereby suggests a strawman. The issue is whether the "obscurities and deficiencies" are what we would expect given the kind of consciousness required for (the same) discarnate spirits to convey the quality and quantity of veridical information they are assumed to communicate on other occasions. Or are the "obscurities and deficiencies" more probable given one or more non-survival hypotheses? Of course, if we have no independent reason to suppose, even approximately, what consciousness would be like if it should survive bodily death, then it might be hard to say for any datum whether it is more to be expected given the survival hypothesis than it is given some alternative hypothesis. But under Hodgson's suppositions, everything is permitted, or at least nothing is forbidden. His survival hypothesis has the virtue of accommodating anything. Unfortunately, this is indistinguishable from the vice of explaining nothing.

Second, Hodgson cites two reasons to support the claim that we should expect communicators not to exhibit the same fullness of clear consciousness they exhibited in life, but his supporting reasons are not more obviously true than the conclusion he wishes to derive. His (1) and (2) are *possibly* true, but in the absence of any independent reason to think that they are actually true, his reasoning begs the question and commits the error Braude et al. noted concerning the appeal to possibly true propositions as auxiliary assumptions (Braude et al., 2022, p. 403). C.D. Broad would later show why the kinds of assumptions Hodgson thought were natural are a small subset of a larger set of possible states of postmortem consciousness, including various models of impersonal survival or Broad's "psychic factor," namely the persistence of the dispositional basis of the personality (Broad, 1962, pp. 387–430; Sudduth, 2016, pp. 33–46, 165–175). Hodgson provides no good reason to privilege his assumptions about survival over any number of the other assumptions we can make about the postmortem persistence of consciousness, but which would result in the data of mediumship *not* providing evidence for personal discarnate survival.

Third, a careful reading of Hodgson's wider discussion shows that the potentially disconfirming data – obscurities, confusions, and deficiencies in the content of mediumistic communications – are allegedly what we would expect **not** if the survival hypothesis (simpliciter) is true,[51] but *if the alleged spirits are speaking in the actual sittings Hodgson is describing*.[52] The problem here is that the latter conjecture packs the observational data into the survival hypothesis, which is akin to already assuming one's conclusion in one's premises. Obviously, **if** the communicators are who they say they are, then the spirits exhibit obscurities and deficiencies. But notice that it is now a survival+ hypothesis – the survival hypothesis modified – that is doing the work for him. But the Pr(a communicator's obscurities and deficiencies | survival+) = 1. And for the same reason the Pr(cosmic fine-tuning | God caused the universe to be fine-tuned) = 1, and the Pr(pepper plants thriving in inhospitable temperatures | an invisible garden fairy is causing the pepper plants to have immunity to inhospitable temperatures) = 1.

It should be immediately apparent that packing observations into one's hypothesis is a fatal flaw. Unless we are epistemic chauvinists, Hodgson's jerry-rigging is a maneuver a skeptic can equally exploit to undercut the survival inference. After all, *if the alleged spirits are **not** speaking in the actual sittings Hodgson is describing*, then the probability that a communicator would exhibit obscurities and deficiencies also equals 1. However, favoring requires likelihood inequalities. So, if we pack the observations into each of our competing hypotheses, the observations will favor neither hypothesis. This neutralizes the survival inference. Of course, it also makes hypothesis testing altogether impossible (Sober, 2019, pp. 34–35).

**Here we come to a crucial point concerning the survival hypothesis and the contrastive nature of confirmation**. The salient kind of experimental failures are observational outcomes that are contrary to what the survival hypothesis would otherwise lead us to expect. When confronted with such failures, survivalists should not ask how they can tweak the survival hypothesis to accommodate such data by fattening the hypothesis with some possibly true auxiliary assumption(s). They should ask whether and to what extent non-survival alternatives make such observations more probable than the survival hypothesis does and with far less contentious assumptions. Then, they should provide a clear account of how they factor such disconfirming observational data into their net assessments of the evidence in favor of the survival hypothesis. Hodgson did not do this, and subsequent survivalists have not advanced beyond Hodgson's fallacious reasoning.

Braude et al. understand the general problem here. They concede that survivalists cannot simply appeal to

mere *possibilities* to fatten the survivalist hypothesis and insulate it from critique:

> In order to explain away or dismiss experimental failures, they [survivalists] must do more than appeal to the mere possibility of psi-inhibitory conditions. They must also provide reasons for thinking that those conditions were actually or probably obtained. And if they fail to mount that defense, then critics can justifiably complain that survivalists do not take experimental failures as seriously as they would take successes. (Braude et al., 2022, p. 403)

This is an important concession, but they should have made more of it, especially since it was prominent in Augustine's arguments. Also, Braude et al. speak generally of survivalists, but they say nothing about the survivalists whose essays were the focus of Augustine's critique. The question is, did *those* survivalists fail to amount the defense to which Braude et al. allude? Moreover, seeing as Braude et al. invoke *mere possibilities* in their counterarguments to Augustine (Ibid., pp. 400, 404–405), they should have provided reasons for supposing that such possibilities are actual in particular cases. This would have allowed them to illustrate or model the kind of defense they suggest above. As it stands, I agree with Augustine that Braude et al.'s comments at this juncture do not undermine his arguments (Augustine, 2022b, pp. 420–421, 427, 431n14).

## 14. Evidential Support without Predictions

In the preceding two sections, I have assumed the widely held view that the survival hypothesis (allegedly) *predicts* observations. Expressed as a likelihood, prediction requires that $Pr(O \mid H) > ½$. This formally codifies what is often meant by the expectation or the expectedness of an observation – that is, given the hypothesis, the observation is more likely to occur than not occur. Prominent survivalists and survival researchers have asserted or implied that the survival hypothesis makes predictions (Almeder, 1996, pp. 498, 504–505; Roll, 2006, pp. 167–170; Schmeidler, 1977; cf. Braude, 2021b, pp. 8–9). These same writers have tried to leverage this fact as evidential support for survival. However, it is possible to claim that psychical phenomena are evidence for the survival hypothesis even if the phenomena are not predictions of the survival hypothesis. This view is defensible given the contrastive model of evidential support codified under (LL), and this is especially important to the Augustine-Braude et al. exchange since (a) Braude is reluctant to say that the survival hypothesis makes predictions (Braude, 2003, pp. 16–19) and (b) I have argued that (LL) can plausibly be interpreted as a criterion which underwrites some of Braude's survival-friendly claims.

Recall that according to (LL) an observation O favors $H_1$ over $H_2$ just if $Pr(O \mid H_1) > Pr(O \mid H_2)$, but this does not require that $Pr(O \mid H_1) > ½$. So evidential favoring does not require that either of the contrasting hypotheses predicts the observation. The accelerant that was present in a house fire is more probable given the arson hypothesis than the electrical malfunction hypothesis, but the arson hypothesis does not predict the accelerant. But, if an observation O can evidentially favor $H_1$ over $H_2$ without $H_1$ predicting O, then clearly observations could favor the survival hypothesis over some competing hypothesis, even if the survival hypothesis did not predict the observation. In that case, not observing O (or observing something inconsistent with O) would not disconfirm $H_1$. This seems to defang the criticism in the previous two sections.

Another example. You know Corbin smokes Cohiba Cuban cigars, and Jeremy does not smoke at all. A Cohiba wrapper and remains of a recently smoked cigar were found near each other on the living room floor of a house that was broken into a few blocks from where Corbin and Jeremy live. Pr(Cohiba Cuban cigar | Corbin broke into the house) > Pr(Cohiba Cuban cigar | Jeremy broke into the house), but Pr(Cohiba Cuban cigar | Corbin broke into the house) is not high, not even greater than ½. The hypothesis that Corbin is the thief does not *predict* that we should find the remains of a Cuban cigar at the crime scene, though it is certainly less surprising that we would find it if Corbin is the person who broke into the house rather than Jeremy. Had there been no cigar remains left behind, we would not say *that fact* disconfirms the hypothesis that Corbin broke into the house, nor would we say *that fact* favors the Jeremy-broke-into-the-house hypothesis over the Corbin-broke-into-the-house hypothesis. Similarly, if there were a subsequent break-in at another house in the neighborhood but no Cuban cigar remains were found, this would not disconfirm the hypothesis that Corbin was the person who broke into the second house.

By parity of reasoning, a survivalist could adopt (LL) and maintain that data from mediumship, cases of the reincarnation type, near-death experiences, etc., favor the survival hypothesis over some conventional or exotic non-survival alternative – for example, cold reading, fraud, coincidence, or more exotic alternatives such as a motivated living-agent psi hypothesis. In which case, the survivalist only needs to argue that the survival hypothesis confers a greater probability on these data than does the alternative hypothesis. But the survivalist could quite

sensibly deny that the survival hypothesis predicts these data, that is, the survival hypothesis need not confer a probability > ½ on the data.

To illustrate, consider Mrs. Piper's mediumship. Take the better G.P. hits to which Braude et al. refer. The survivalist can argue that these observations discriminate between the George Pellew survival hypothesis and an alternative, say, the cold reading hypothesis. The survivalist can argue as follows:

Pr(Mrs. Piper's better G.P. hits | George Pellew is the communicator) > Pr(Mrs. Piper's better G.P. hits | cold reading),

but not that

Pr(Mrs. Piper's better G.P. hits | George Pellew is the communicator) > ½.

In this situation, the observational data would favor the hypothesis that George Pellew is the communicator over the cold reading hypothesis. As with the evidence in the Cohiba Cuban cigar example, the survival hypothesis here does not *predict* the better G.P. hits, either the specific content or the more general fact that Mrs. Piper conveyed such detailed veridical communications about the life of Pellew. So, Mrs. Piper's errors and confusions about Pellew in various sittings would not be a disconfirmation of the hypothesis that George Pellew is the communicator. It would not lower the probability of that hypothesis. In fact, the entire idea of confirming and disconfirming a particular hypothesis misses the contrastive nature of evidential support (LL) codifies. (LL) only tells us which of two competing hypotheses some observation favors; by itself, it is insufficient to show that an observation *raises* or *lowers* the probability of a particular hypothesis.

(LL) holds another advantage for survivalists. It may resolve the ambivalence of survivalists who are reluctant to say that the survival hypothesis predicts the data, but who still maintain that the hypothesis *accounts for*, *fits,* or *leads us to expect* the data (Gauld, 1982, pp. 73, 77, 110, 138–139; Lund, 2009, pp. 101–103, 152; cf. Hodgson, 1898, pp. 361–367). Of course, survivalists who adopt (LL) should emphasize the contrastive nature of the expectedness. The survival hypothesis does not lead us to expect the data full-stop; rather, it leads us to expect the data *more than* some competing (non-exhaustive) hypothesis.

**I think this is the best response a survivalist can pull together in the landscape of well-established, widely deployed criteria of evidential support. Unfortunately for the survivalist, it is a hollow victory.**

First, adopting (LL) means that survivalists will have to soften what they claim on behalf of the survival hypothesis. They will not be able to make non-contrastive claims about the favorable plausibility or probability of the survival hypothesis, nor that they have proved the survival hypothesis by a preponderance of the evidence (i.e., that survival is more probable than not), much less beyond reasonable doubt (i.e., highly probable). They will only be able to say that some observation(s) favor the survival hypothesis over some but not all alternative hypotheses. Finally, (LL) does not tell us what we should believe, other than the belief that some observation favors the survival hypothesis over some competing hypothesis.

Second, while there may be some observations that favor the survival hypothesis over the competitors taken individually, there will also be other observations that favor conventional alternative hypotheses. For example:

Pr(Mrs. Piper's G.P. errors | cold reading) > Pr(Mrs. Piper's G.P. errors | George Pellew is the communicator)

Pr(Mrs. Piper's G.P. lacking the philosophical and classical knowledge characteristic of Pellew | cold reading) > Pr(Mrs. Piper's G.P. lacking the philosophical and classical knowledge characteristic of Pellew | George Pellew is the communicator)

Pr(Mrs. Piper's G.P. weaker hits | cold reading) > Pr(Mrs. Piper's G.P. weaker hits | George Pellew is the communicator)

For each of the above, the "cold reading" likelihood would be very high, perhaps even 1, since it is very much to be expected that, if cold reading is the source of the G.P. persona's knowledge and demonstrated abilities, this would produce the cocktail of remedial truths, significant errors, and confusions Hodgson tried to rationalize. And no contentious assumptions are needed. But the survivalist, having adopted the idea that the survival hypothesis is not predicting anything, will automatically lose every round in which rival hypotheses in fairly simple forms confer probabilities of greater than ½ on the observations. So, under (LL) some observations will (strongly) favor non-survival hypotheses over the survival hypothesis. Such observations are, in a clear sense (strongly), *unfavorable* to the survival hypothesis. This functions as a disconfirmation of the survival hypothesis, even if only relationally or contrastively.

Third, the demand for net assessment is just as appropriate under (LL) as it is under Bayesian confirmation criteria. So, Augustine can recalibrate his important point about the failure of survivalists to properly weigh disconfirming observations. This does not require that the

survival hypothesis make predictions, only that all relevant observations and their corresponding likelihoods are weighed. What must be considered are (i) the individual observations that favor survival over competing conventional hypotheses and (ii) the individual observations that favor one or more competing conventional hypotheses over the survival hypothesis. These must then be weighed so that something can be said about what the *total observations* favor. The survivalist would need to show that the total relevant observations O* are such that for each hypothesis $H_i$ from the set of competing alternative hypotheses $\{H_1, H_2, H_3 \ldots H_n\}$, it is true that $Pr(O^* \mid \text{survival hypothesis}) > Pr(O^* \mid \text{conventional hypothesis } H_i)$.[53]

Finally, although auxiliary assumptions are needed for hypotheses to make predictions, they are also needed to apply (LL) when the contrasting likelihoods are both less than ½. This is because the expectation of the observation, however weak, typically depends on auxiliary assumptions. I refer back to the Cuban cigar/house break-in example. We needed auxiliary assumptions about Corbin in that example, even though they did not generate a prediction about the specific item of evidence. And this brings us to a fundamental criticism. **Survival arguments, whether likelihoodist, Bayesian, or IBE, all depend on likelihood inequalities. The evidential and/or explanatory salience of likelihood inequalities is the one point of agreement between all three of these approaches. But this is the Achilles' Heel of survival arguments.** I turn to this in the final section as it relates to one of Augustine's important criticisms of the BICS essays and the reply from Braude et al.

## 15. The Testability Problem

Braude and his cohorts concede that parapsychological phenomena are not susceptible to ordinary empirical testing (Braude et al., 2022, p. 405). Braude has elsewhere argued that neither psi nor survival are open to the kind of falsification that characterizes scientific hypotheses, and so it is difficult to say what the evidence for survival should look like (Braude, 2003, pp. 16–20, 300). Braude et al. may be correct here, but this cannot plausibly be leveraged against Augustine's argument or even against a recalibrated, non-predictive (LL) version of his argument. Quite the contrary. If tests for survival or psi phenomena are not susceptible to ordinary empirical testing, if we cannot say with any reasonable confidence what the evidence for survival should look like, then so much the worse for the BICS essayists who assume otherwise. They, not Augustine, are the ones proposing that they have good scientific evidence for survival. It is the survivalists in Augustine's crosshairs who are forced to adopt auxiliary assumptions which are no more than possibly true in order to shield the survival hypothesis from disconfirmation. This is why survival arguments only create the illusion of being scientific and empirically testable.

Instead of acknowledging the above, Braude et al. manage to turn their observations into a criticism of Augustine:

> Lurking below the surface is an interesting and serious problem which Augustine does not consider at all – namely, whether we can ever confidently assess success or failure in *any* parapsychological test... most (or perhaps all) of the time, we have no idea what is really going on in a parapsychological experiment. (Braude et al., 2022, p. 405)

Two things are worth reiterating here. First, Augustine was addressing the testability of the survival hypothesis as this topic arises in the BICS essays in question. He was not discussing the broader category of *parapsychological* tests. Second, the interesting and serious problem Braude et al. have noted is precisely the one Augustine has diagnosed, at least with reference to proposed tests for survival. As I previously demonstrated, Augustine's argument concerning the significance of failed tests is materially conditioned by *survivalist* assumptions, not his own. The survivalists Augustine is responding to have proposed tests for survival on the assumption that we *can* assess success in such tests, whether hits in mediumship or pre-assigned targets in NDE tests. His point is that survivalists who regard apparent successes as evidence in favor of the survival hypothesis ought to take experimental failures as facts that weaken the purported inference to the survival.

Braude et al. also claim that experimental failures, whether in mediumship or near-death experiences, would only "disconfirm a particular *model* of personal survival" (Ibid., p. 405). And why is that? Because all such experimental tests – for example, the encrypted message or combination lock tests in mediumship – rely on various assumptions about what it would be like to survive death. Braude et al. appear to be relying on the assumption that the survival hypothesis has no predictive consequences or well-defined likelihoods (and so no explanatory power) unless it is bulked up with auxiliary assumptions. This is correct (Sudduth, 2016, ch. 9). This is a consequence of the Duhem-Quine thesis: we test statements in bundles (Sober, 2008, pp. 144–147). As previously noted, it is typically a hypothesis *plus auxiliary assumptions* that generates predictions or well-defined likelihoods.

But it is unclear why Braude et al. think the above

observation is an effective counterpoint to Augustine. Perhaps they think Augustine was claiming that a failed test for survival disconfirms the survival hypothesis and is therefore evidence against the hypothesis. But it should be clear from Augustine's response to this particular objection (Augustine, 2022b, p. 421) that he was not leveraging "experimental failure" and "disconfirmation" against the truth of the survival hypothesis. He was attempting to undermine the survivalist's appeal to ostensible successes as a confirmation/strong evidence for the truth of survival. To do this, he adopted, for the sake of argument, the assumptions survivalists must adopt to lay claim to ostensible successes as confirmations. He said as much in his reply (Ibid., p. 431n15). **The wider point Augustine is arguing is that these survivalists badly mishandle disconfirming or defeating evidence. Consequently, they fail to cogently reach conclusions about the net assessment of the evidence for survival. So, their strong claims about the survival hypothesis are not justified.**

Moreover, while it is true that we test statements in bundles (core hypothesis plus auxiliaries), it does not follow that failed tests for survival would *only* disconfirm a particular model of survival. In cases of apparent disconfirmation, it is hypothesis H plus auxiliary assumptions A that leads us to expect what we do not observe. *Absent further considerations,* this situation can count either against H or A, or both. All we can say is that we have disconfirmed the conjunction {H & A}, but we do not know whether the actual observation discredits or counts against H or A or both. So, we cannot say it *only* disconfirms the model (hypothesis plus auxiliaries). It *might* only disconfirm the model. But if there are independent reasons to doubt H, then this would give us a reason to view H as the culprit.

The latter point is important. Augustine's Surprise Principle argument from the neurophysiological data (see §8) provides evidence against discarnate survival. This argument is independent of his argument concerning the significance of failed survival tests. The former argument gives us reasons to suppose that the survival hypothesis is false, and the latter argument (at least) gives us reason to doubt whether the data to which survivalists appeal confirm the survival hypothesis.

**This brings us to a fundamental problem baked into the entire empirical survivalist's program.** It looks like the survival hypothesis is not empirically testable at all. On the one hand, it is not testable *without* auxiliary assumptions since auxiliaries are needed to generate sufficiently defined likelihoods, which are in turn necessary if any of the kinds of data in question are to confirm the survival hypothesis. On the other hand, it looks like the survival hypothesis is not testable *with* auxiliaries because the auxiliary assumptions themselves cannot be independently justified. This is an important implication of Augustine's discussion of the significance of failed tests for survival, which I have elsewhere argued vitiates all extant empirical survival arguments (Sudduth, 2013a, 2013b, 2014, 2016).

Sober writes:

> Hypothesis $H_1$ can now be tested against hypothesis $H_2$ if and only if there exist true auxiliary assumptions *A* and an observation statement *O* such that (i) $Pr(O \mid H_1\&A) \neq Pr(O \mid H_2\&A)$, (ii) we now are justified in believing *A*, and (iii) the justification we now have for believing *A* does not depend on believing that $H_1$ is true or that $H_2$ is true and also does not depend on believing that *O* is true (or that it is false). (Sober, 2008, p. 152)

Sober's account of testability reiterates the broadly contrastive nature of hypothesis testing and the need for hypotheses to be joined with auxiliary assumptions to generate likelihood inequalities. More specifically, the formulation underscores that we must be justified in believing the required assumptions and that this justification be independent of believing either the hypothesis or believing that the observation is either true or false.[54]

Sober's point reinforces why Hodgson and other survivalists have been mistaken. The error is not the idea that the survival hypothesis leads us to expect the observational evidence; it is the belief that the survival hypothesis is empirically testable in a way that leads to a victory for the survivalist. **This goes right to Augustine's central criticism concerning experimental failures. It is not that survivalists refuse to acknowledge that the survival hypothesis has been disconfirmed. It is that their maneuvering reveals why its disconfirmation, given survivalist assumptions, would be as illusory as its confirmation given those same assumptions.** Unlike ordinary and extraordinary empirical hypotheses, the survival hypothesis is an untestable hypothesis. It is a metaphysical conjecture seeking a point of connection with the empirical world. Far from this not occurring to Augustine, he has laid the groundwork for rational doubt concerning whether – to quote Braude et al. – "we can ever confidently assess success or failure" when it comes to tests for survival or the paranormal. At present, we cannot. Therefore, arguments that assume otherwise are flawed. Braude et al. seem to agree.

There are some illuminating parallels between the logical flaws in survival arguments canvassed above and widely discussed criticisms of intelligent design argu-

ments (Sudduth, 2014, 2016, pp. 303–304). Sober's observations are worth quoting at length:

> The problem with the hypothesis of intelligent design is not that it makes inaccurate predictions but that it doesn't predict much of anything. Rather, the design hypothesis merely allows our observations – whatever they turn out to be – to be folded inside a simple formula...
>
> I have argued that the design argument is unsuccessful because we have no way to evaluate
>
> *Pr*(the eye has features $F_1 \ldots F_n$ | the eye was made by an intelligent designer).
>
> My point is not that we don't know what the point value is of this probability but that we can't even judge whether it is greater or less than
>
> *Pr*(the eye has features $F_1 \ldots F_n$ | the eye was the result of a mindless random process).
>
> The value of this second probability is very low, but it is not zero. As we have seen, auxiliary propositions can be invented about the putative designer's goals and abilities that ensure that the likelihood of the intelligent-design hypothesis is very high, but it is equally true that auxiliary propositions can be invented that ensure that the likelihood of the intelligent-design hypothesis is zero. What is needed is not the invention of auxiliary propositions (whether they help or hurt the design hypothesis) but the identification of auxiliary information that is independently supported. Paley did not provide this information, and the same is true of modern defenders of the design argument. (Sober, 2008, pp. 154, 167–168)

*Mutatis mutandis*, Sober has concisely summed up the plight of the survival debate. The fundamental flaw in survival arguments is not that the survival hypothesis makes inaccurate predictions, but that it makes no (soft or hard) predictions at all. At least it makes no predictions until it is bulked up with auxiliary assumptions. But those assumptions, lacking any independent justification, are simply part of a just-so story that allows survivalists to accommodate any possible observation. What is needed is independent support for whatever auxiliary information survivalists wish to enlist for the purpose of laying claim to evidence for survival. Hodgson did not provide the information, and the same is true of modern defenders of the survival argument, especially as those defenders are paradigmatically represented by the winners of the BICS Contest.

If I may anticipate a possible Braude et al. styled rejoinder, *not so fast*.

The conscientious survivalist may have picked up on something in the preceding argument that looks like it might be used as leverage against Augustine's conclusion that the neurophysiological data strongly supports the dependence thesis over the independence thesis. If the survival hypothesis makes no soft or hard predictions at all (sans contestable auxiliaries), how can any of the neurophysiological data disconfirm it in Augustine's sense? If the hypothesis is so predictively impotent as to not generate any likelihoods at all without supplementation with contestable auxiliaries, is not the skeptic barred from appealing to neurophysiological data as evidence against survival? In a word, *no*.

In saying that the survival hypothesis is an untestable hypothesis, I meant specifically a *robust* survival hypothesis or survival *theory* which is bulked up with untestable or non-independently justified auxiliary assumptions. Without auxiliaries, it is not possible for the kinds of data in question to confirm the survival hypothesis, nor for failed tests to disconfirm such a survival theory. If observations incrementally or absolutely *confirm* the survival hypothesis (i.e., raise its all-things-considered probability a bit, or raise it above ½, respectively), then the hypothesis must be bulked up with a variety of suspect auxiliary assumptions. If those observations are to *favor* the survival hypothesis over a non-survival competing hypothesis, then the survival hypothesis will need to be bulked up with various suspect auxiliary assumptions. Moreover, any disconfirmation resulting from failed tests for survival would also presuppose such auxiliaries. And, as we saw with Hodgson, if the survivalist is to immunize the survival hypothesis (or theory) from disconfirmation arising from the *survivalist's* idiosyncratic assumptions, then even more suspect assumptions are required.

However, it does not follow from any of this that such bulking up is required for any observation to count against a *simple* survival hypothesis, at least if that hypothesis *entails* the mind-brain independence thesis. As previously indicated, Augustine was careful to note in his deployment of the Surprise Principle that he was applying it to the dependence and independence theses in their vanilla forms, with as little supplementation as possible. Whichever way one bulks up the hypothesis of discarnate survival, it will always entail the persistence of consciousness in the absence of a functioning brain or some other suitable physical substrate. To the extent that there are observations much more expected given mind-brain dependence than the generic supposition of mind-brain independence, then observations *can* disconfirm the core simple hypothesis of discarnate survival.

In connection with this point, Augustine and Fishman

(2015, pp. 227–246) provide a more detailed discussion of observations that disconfirm a simple survival hypothesis. There is no need to relitigate that here. The present point is a conceptual one. What matters is the logical relationship between the description of the observation and the content of the survival hypothesis. As that stands, it looks like the hypothesis of discarnate survival can be disconfirmed without the survivalist having to say much at all, but unless the survivalist is gregarious, his hypothesis cannot be confirmed. The survivalist is thus caught between the Scylla of a straightforward empirical disconfirmation and the Charybdis of an elusive, if not illusory, confirmation.

## Concluding Remarks

Augustine's central criticism of the BICS essays is that they failed to accomplish what they claimed to have accomplished. The essays failed to prove the truth of the survival hypothesis beyond a reasonable doubt or otherwise show that the alleged evidence confers any strong positive epistemic status on belief in survival. Moreover, as Augustine sees it, the considerations adduced by the BICS essayists do not even make discarnate survival more probable than not, a much lower evidential threshold than what most of the essayists claim to have shown. If the essays represent the best evidence for survival, the best is not very good at all. The essayists have failed to show that the best evidence for life after death is anything more than the best of a bad lot.

According to Augustine, the main problem is that **the survivalists in his crosshairs fail to properly weigh the total evidence**. **Consequently, their conclusions are unwarranted, and belief in survival (based on their alleged evidence) is not justified.** Augustine shows an alarming trend among the BICS essayists. They are supposed to represent the cream of the crop in the field of survival research, but their reasoning is logically and epistemologically defective.

- They ignore or mishandle salient facts – for example, neurophysiological data or facts concerning their own failed experimental tests – which are potential evidence against the survival hypothesis, or which would otherwise weaken the inferences they wish to draw.
- They provide very superficial analyses of rival explanations or how the space of rival explanations impacts the net assessment of evidence for the survival hypothesis – for example, they treat alternative explanations in their least plausible forms and ignore more nuanced ways facts can deceptively give the appearance of survival.
- They lack sufficient clarity about why the data, individually or jointly considered, should be regarded as evidence, good or otherwise, for the survival hypothesis, or they rely on inappropriate criteria of evidential support to justify their claims.
- They commit a large number of garden variety logical fallacies en route to their preferred conclusions.

### *The Braude et al. Reply*

The Braude et al. reply to Augustine's criticisms is perplexing in several ways. It is not adequately calibrated to address Augustine's arguments, ignores salient common ground, neglects epistemological issues central to Augustine's critique and the survival debate, and is opaque with respect to the positive survival claim they wish to defend against Augustine's criticisms. They seem to think that there is a better case for survival than Augustine is willing to concede, but that case is not presented or even outlined anywhere in their reply. They say, "some evidence seems much stronger than what skeptics assume or conclude" (Braude et al., 2022, p. 399). *Seems* to whom exactly? By virtue of what *standard*? Given what range of *assumptions*? And *which* skeptics? The Braude et al. claim needs substantial unpacking before we can take it seriously as anything more than a declaration of personal credulity. Presumably, a skeptic is just as entitled to any degree of incredulity. Consequently, the sentence that Braude et al. wish to offer as a criticism of Augustine involves the same kind of vagueness and potentially question-begging insinuations that vitiate the wider field of survival literature.

In §4 and §6, I explored Braude's assessment of the evidence for survival in his previous publications. His evaluation of the evidence is modest and compatible with Augustine's conclusion that the BICS essays are unsuccessful at showing that the survival hypothesis has been proven beyond a reasonable doubt (= highly probable), as well as unsuccessful at showing that the evidence even makes survival more probable than not. In fact, Braude's assessment of the evidence for survival is compatible with *denying* that the evidence makes survival more probable than not. And we have also seen that Augustine agrees with Braude that belief in survival can be reasonable. To be sure, Braude does contend that there are some data for which the survival hypothesis apparently provides the best (or at least marginally better) explanation. But according to Braude, the best explanation is not, as it is for Augustine, the explanation with the highest posterior probability. On my reading, Braude is not a Bayesian explanationist. He does not use Bayes' theorem to bridge the gap between explanatory power and evidential sup-

port.

The area of genuine disagreement between Augustine and Braude seems more narrowly concentrated on the extent to which the better cases – Mrs. Piper's mediumship, for example – are susceptible to non-paranormal counterexplanations. Braude thinks such cases resist being explained away by fraud, coincidence, and other usual suspects. By contrast, Augustine argues that survivalist attempts at ruling out such counterexplanations are inadequate. These efforts, paradigmatically represented in the BICS essays, involve a variety of mistakes in logic and epistemology. Among these are (i) not considering more nuanced ways counterexplanations lower the probability of the survival hypothesis or weaken the inference to survival, (ii) relying on fallacious inferences, like probabilistic modus tollens, to rule out rival hypotheses, and (iii) failing to show that the survival hypothesis makes the observational data more probable than do non-survival alternatives.

A few points are worth reiterating here. Even if naturalistic or conventional hypotheses do not provide a satisfactory explanation of some data, this is insufficient to rule out such explanations. Survivalists must do better than assert that there are some cases or specific phenomena which are not easily explained away. This is lazy testing. Hypotheses must be tested against one or more alternatives. Do not ask whether some observation is improbable given a non-survival hypothesis; rather, ask whether the survival hypothesis makes the observation *more probable* than some non-survival alternative. Therefore, ask what the survivalist must assume for some observational datum to be more expected given the survival hypothesis than it would be given the alternative hypotheses. **The case for survival depends on justified likelihood inequalities.** Ruling out counterexplanations depends on *ruling in* the survival hypothesis. The survivalist preoccupation with the former has often distracted them from accomplishing the latter. **Consequently, survivalists have failed to successfully show that there are observational data that are genuinely surprising but for the survival hypothesis.**

Braude et al. concluded their reply by saying, "Augustine has squandered an opportunity to advance the debate over survival. What's needed are novel propositions, not the tired and transparently defective skeptical arguments on which he often relies" (Braude et al., 2022, p. 409). This assessment rests on the assumption that Braude et al. have adequately understood Augustine's arguments. They have not. So, the latter assertion is a toothless bite on the skin of Augustine's critique. The accusation also rests on the assumption that survivalists and their critics have the same conception of what it means to advance the survival debate. I am not convinced this is true. As for novel propositions, Augustine provided several; Braude et al. engaged none of them. Of course, even if Augustine failed to advance the debate, he has done no worse than the best survivalists have had to offer for the past 140 years. In one crucial respect, though, he has actually done better. His critique of the Contest and its essays, as well his reply to Braude et al., has at least illuminated why the survival debate has failed to advance much during its century-long nascence.

### *Eight Constructive Suggestions*

So much for the negative summary. Here are eight constructive suggestions for survivalists.

***First*, survivalists need to pay far more attention to the logical architecture of survival arguments.** What are the premises and conclusion(s) of the main survival argument they wish to present? What sub-arguments are being invoked to justify potentially contentious premises in the main argument? Survivalists have a tendency to present narratives in which they stack facts and then assert their preferred survivalist conclusion, often including a perfunctory dismissal of rival hypotheses. At the remedial level, this can be addressed if survivalists would state their argument(s) using recognizable argument forms, with their premises and conclusion(s) explicitly stated and sub-arguments carefully distinguished from their main argument. Just as a pile of wood does not make a house, a heap of facts does not make an argument.

***Second*, survivalists need to exhibit greater care in how they represent and engage in critical assessments of their arguments.** For example, it is important to distinguish between the contention that the survival hypothesis is false and the claim that survivalists have failed to provide sufficient reason to suppose that the survival hypothesis is true. These are very different kinds of claims, and the arguments offered in support of them are significantly different. The distinction between three broad types of skepticism outlined earlier provides a helpful template for mapping out salient forms of skepticism.

***Third*, survivalists need to do logical remediation and clean up the fallacious nature of their suggested inferences.** Augustine provided an extensive catalog of logical mistakes in the BICS essays, and I have canvassed several in this paper. For example, the suggestion that non-survival hypotheses are improbable or must be rejected because they confer low probabilities on the observational data – that is, do not account for, do not lead us to expect, etc., the relevant data. Or concluding that the survival hypothesis is at least more probable than not because it is the best explanation of the data. These fre-

quently encountered inferences are simply not cogent.

***Fourth*, survivalists need to apply Royall's important insight and distinguish between the following two questions – What does the evidence presently say? What should we believe?** Which question do survivalists propose to answer? This is important because it is possible for evidence to tell us something significant – which of two competing hypotheses do the observations favor? – without telling us which hypothesis we should believe or how strongly we should believe it.

***Fifth*, survivalists need to express their survivalist conclusion(s) with greater conceptual clarity.** This includes being clear about the favorable evidential claim they wish to make on behalf of the survival hypothesis – for example, what they mean by evidence, how strong they think the evidence is, and what kinds of criteria they are relying on to make such judgments. And, in the case of IBE survival arguments, survivalists need to clearly state how they construe the relationship between evidential support and explanatory power as outlined in section §5.

***Sixth*, survivalists need to avoid relying on inappropriate criteria of evidential support.** One of the more egregious examples, discussed in §1, is the reliance on legal evidentiary standards – for example, the standard of proof beyond reasonable doubt. Although there is a distinctly epistemic dimension to legal evidentiary standards, even in jurisprudence, this aspect of legal standards of proof is explicated in terms of more fundamental, non-domain-specific criteria of evidence assessment – for example, Bayesianism, likelihoodism, or bulked-up inferences to the best explanation. Otherwise stated, in their epistemic dimension, legal evidentiary standards are instantiations of broader theories of evidence. In this sense, "beyond reasonable doubt" is not an alternative to the kinds of criteria discussed throughout this paper; rather, in its epistemic dimension, it is grounded in such principles. If we stripped away the judicial features of legal evidentiary standards or extracted their salient epistemic elements, we would not be relying on anything distinctly *legal*. Consequently, survivalists who profess to be relying on legal standards of proof are either engaged in nonsense or relying on broader epistemic/evidential standards. The latter renders the use of legalese, legal analogies, and so forth, unnecessary and misleading.

It follows from the previous points that survivalists ought to reject the first three of the seven survivalist claims listed in §2.

- The observational data logically demonstrate the survival hypothesis.
- The observational data prove the survival hypothesis beyond a reasonable doubt.
- The observational data prove the survival hypothesis by a preponderance of the evidence.

***Seventh*, survivalists should retool their deployment of inference to best explanation survival arguments or refrain from using such arguments.** (i) Stronger formulations of IBE survival arguments would be needed to circumvent the philosophical objections outlined in §4. (ii) There are many different explanatory virtues and so different IBE arguments, but no clear way of choosing between these rival accounts or weighting explanatory virtues. (iii) As a special case of the former, there is no probability cash value in explanatory merit unless IBE is merged with Bayesian probability. (iv) There are good reasons for thinking that explanatoriness is evidentially irrelevant (Roche & Sober, 2013). (v) For reasons noted in §4, traditional survival IBE arguments are self-defeating.

It follows from the seventh point that, unless survivalists are prepared to substantially retool IBE survival arguments – for example, by merging such arguments with Bayesian probability – they ought also to reject the following claim:

- The survival hypothesis is the best explanation of the observational data.

This leaves us with only three kinds of generic claims that ought to be the focus of inquiry into whether there are facts that provide evidential support for the survival hypothesis, and assuming there are such facts, in what way and to what degree the facts evidentially support the survival hypothesis:

- *The observational data favor the survival hypothesis over alternative hypotheses.*
- *The observational data are evidence that the survival hypothesis is true.*
- *The observational data show that the survival hypothesis is probably true.*

***Finally*, survivalists need to adopt and deploy epistemic principles and evidential criteria that are relevant to or would be required to justify the three claims above, and they need to correctly deploy such criteria.** As I argued in §5, the law of likelihood is sufficient as a framework for justifying the first and second claims above, though without saying anything about the plausibility or posterior probability of the survival hypothesis or what we should be believing regarding its truth. By contrast, Bayesian incremental confirmation provides an alternative framework for justifying the second claim,

and in a way that also tells us something about belief in survival, namely that we should increase our confidence in that hypothesis. And, it is Bayesian reasoning, specifically Bayes' theorem, that provides a framework for justifying the third claim above, where this implies a net assessment of the survival hypothesis being at least more probable than not given the evidence.[55] This also addresses Royall's belief question.

One of the prominent themes in this paper has been that evidence should be understood *probabilistically*, either the probability *of a hypothesis* given the evidence (Roush, 2005, pp. 154–178) or contrasting probabilities *of the observations* given two competing hypotheses (Sober, 2019, pp. 32–41). This is no place for a full-blown defense of these probabilistic views of evidence which Bayesian and likelihoodist views formally codify, nor for the Surprise Principle which informally expresses the widespread intuition that likelihood inequalities are evidentially significant and essential to hypothesis testing and inference to the best explanation. Given the fundamental nature of Bayesian and likelihoodist views of evidential support, it is hardly surprising to see them applied across multiple areas of inquiry, including philosophy of religion, psychology, sociology, and jurisprudence.[56] It is only recalcitrant survivalists who wish to insulate the survival hypothesis from probabilistic reasoning and its epistemic guardrails. Survivalists who do not care for established theories of evidence as the scaffolding of survival arguments are free to propose and defend their own. What is not an option is, as Braude has rightly called it, "more sloppy reasoning about survival" (Braude, 2021a).

Of course, there is no guarantee that survival arguments developed along the lines I have proposed will be successful. I, for one, am skeptical of such an outcome. But even in the worst-case scenario, survivalists at least have an opportunity to produce the "novel proposals" to which Braude et al. refer in their final words. Perhaps lucidity also. Even if this does not warrant a reassuring confidence in the reality of survival, it at least encourages optimism that survivalists are capable of advancing the debate in a sensible and perhaps rigorous way. In the words of Stephen Braude, "Confidence will have to come later, if it comes at all."[57]

## ENDNOTES

1. Imants Barušs, Arnaud Delorme, Dean Radin, and Helané Wahbeh.
2. Augustine subsequently published (Augustine, 2022c) a response to Michael Nahm's reply to Augustine's BICS critique (Nahm, 2022). He also published an essay (Augustine, 2023) in which he shows "striking similarities" between the arguments of survival researchers and the fallacious reasoning of fundamentalist Christian apologists.
3. Bayesianism and likelihoodism are the two dominant approaches to confirmation (Chalmers, 2013; Curd, Cover, & Pincock, 2013; Fitelson, 2007, 2011; Hawthorne, 2011, 2018; Lin, 2023; McGrew, T., 2000; Sober, 2008, pp. 1–108; Swinburne, 1973). Each makes use of probability to provide qualitative and quantitative criteria for reasoning about evidential support. For detailed applications of confirmation theory to the survival debate, see Augustine and Fishman (2015) and Sudduth (2016). I discuss Bayesianism and likelihoodism in the present paper beginning in §5.
4. For example, Braude et al. (2022, pp. 401–402) objected to Augustine's criticisms of the survivalist reliance on testimony, specifically his use of Loftus's work. But Augustine's argument here was directed at Michael Nahm's reliance on testimony and other legal concepts for proving survival beyond a reasonable doubt. The issue is not the general reliability of testimony. It is the reliability of testimony in the context of legal rules and evidentiary standards (Augustine, 2022a, p. 368). Augustine only said of Loftus that she provides "all sorts of reasons to hesitate to rely upon it [testimony] so heavily (as survival research typically does)" (Ibid., p. 368). His brief reference to Loftus is only one of several considerations designed to undercut the degree to which survivalists rely on testimony and its independent adequacy to justify the attribution of strong positive epistemic status to the survival hypothesis. Our ordinary reliance on testimony may provide a *prima facie* justification for testimonial beliefs, but this kind of justification is defeasible and would not be sufficient to ground extravagant epistemic claims. Augustine also quoted Braude's coauthors to support his position. Braude et al. ignored how Augustine framed his points on testimony, and nothing they said about the general reliability of testimony (and memory) rescues it from the grip of the specific problems that arise in the legal context Nahm adopted for his survival argument.
5. Braude tends to invoke the skeptic's alleged reliance on various metaphysical assumptions. But survivalists are hoist by their own petard. They depend on an enormous amount of unsupported and untestable assumptions – for example, assumptions about the nature and capacities of postmortem consciousness. Also, Braude's frequent redirects to issues in the philosophy of mind are not responsive to Augustine's lengthy and novel argument, which shows why no position in the philosophy of mind inoculates survivalist arguments

from the kind of criticisms Augustine has offered (Augustine, 2022a, pp. 384–388).

6. The term "probability" is used in different ways. **Inductive probability** refers to the degree to which the premises of an argument provide (non-conclusive) evidential support for the argument's conclusion. **Epistemic probability** refers to the degree to which a statement or belief is supported or made plausible by some other statement(s) for a particular person at a particular time. Epistemic probability can be viewed as parasitic on inductive probability. "The *epistemic probability* of a statement is the inductive probability of that argument which has the statement in question as its conclusion and whose premises contain all of our relevant factual knowledge" (Skyrms, 1966, p. 15). As Skyrms notes, the inductive probability of an argument is not person- or time-relative, whereas the epistemic probability of a statement is since it depends on "the stock of relevant knowledge possessed by a person at a given time" (Ibid., p. 18). On epistemic probability, also see Swinburne (1973, pp. 1–10), and (2001, pp. 56–73). Epistemic (and inductive) probability should be distinguished from **factual probability** (including "physical" and "statistical" probability), which is a function of objective features of the physical world (e.g., its laws and structure). For example, the factual probability of drawing a black ball from a sealed box containing nine black balls and one white ball is .9 (almost certain), whereas its epistemic probability will vary depending on the evidence one has about the color and number of the balls in the box. In this paper, I am primarily concerned with inductive and epistemic probability.

7. "Well supported" here means that the evidence, codified in an argument, makes the survival hypothesis at least more probable than not (see notes no. 6, 8, and 9). If Augustine's basic argument is sound (valid with true premises), then it will be sound *a fortiori* for arguments that purport to show that the survival hypothesis is highly probable, beyond reasonable doubt, etc.

8. As indicated in §1, "beyond reasonable doubt" and "preponderance of the evidence" are *legal* evidentiary standards and inappropriate as evidential criteria in the survival debate. I include them here because survivalists make such claims. What is relevant, of course, is the epistemic *dimension* to such standards. This involves the calibration and application of non-domain-specific criteria of reasoning and evidential support – for example, Bayesian reasoning. The epistemic or probative dimension to "preponderance of the evidence" is often expressed probabilistically as a(n) (inductive or epistemic) probability above the threshold value of 0.5 or ½ – that is, the evidence should make the hypothesis at least *more probable than not*. When expressed probabilistically, "beyond reasonable doubt" requires surpassing a threshold value typically assumed to be above 0.9 – that is, the evidence should make the hypothesis *highly probable.*

9. I take "probably" in (4) in the broad sense, such that the survival hypothesis is *at least* more probable than not given the relevant observational evidence. Where H = the hypothesis and O = observational evidence, Pr(H | O) > ½ formally expresses this idea. I list (3) and (4) as distinct claims. Survivalists sometimes assert (3) but refuse to parse it in terms of probability. Also, some survivalists assert (4) but do not parse it using the legal evidentiary standard in (3). Finally, "probably" in (4) includes probabilities much greater than ½.

10. By *evidence* here, I mean evidence in the non-stipulative sense. It is common to refer to data, facts, observations, information, etc., as evidence regardless of whether the former *actually* supports a claim. This stipulative use of the term *evidence* is common in jurisprudence to refer to information that can be used to support claims in the legal context. In the philosophy of science, *evidence* is used to refer to observational data in contrast to the hypotheses that are adduced to explain them, especially when *evidence* is parsed probabilistically. For example, Pr(hypothesis | evidence) or Pr(H | E) is a way of referring to the probability of the hypothesis given the relevant observational data. I use the term *evidence* in the stipulative sense in places where convention dictates it, but it should be clear that the central question is whether the data, facts, and information adduced on behalf of survival actually make the survival hypothesis *evident* to some significant degree, that is, whether the facts *are* evidence for survival, and if so, how strong the evidence is.

11. Survivalists who make this claim usually contrast the survival hypothesis with one or more specific competing hypotheses – for example, usual suspects such as fraud, malobservation, cryptomnesia, and more exotic hypotheses such as living-agent psychic functioning. However, it is important to distinguish between observations that favor the survival hypothesis over (i) a *single* alternative hypothesis, (ii) *more than one* alternative hypothesis, and (iii) *all* alternative hypotheses. These distinctions play out in different ways depending on one's theory and criteria of evidence. Bayesian analyses, for example, require considering (iii). This is because, according to Bayes' theorem, the overall probability of a hypothesis H depends on the prior probability of H's negation – Pr(~H) – which is the probability of the disjunction of all logically possi-

ble alternatives to H, and the extent to which the evidence is to be expected given all alternative hypotheses – Pr(O | ~H). Pr(~H) is often referred to as a catchall prior, and Pr(O | ~H) as a catchall likelihood. As I have argued elsewhere (Sudduth, 2016, pp. 289–300), the catchall probabilities can be high, even if the probabilities for each of the alternative hypotheses subsumed under the catchalls are low when considered individually. See §5 for a discussion on Bayesianism.

12. Rationality is a Janus-faced positive epistemic status. It can mean being within one's intellectual rights in believing a proposition (deontological rationality) or forming/holding a belief that is the product of properly functioning cognitive faculties (proper function rationality). It can also refer to any species of subjective rationality – for example, believing a proposition because it seems to be true and one knows of no overriding evidence to the contrary, or it can mean updating one's credence consistent with Bayes' theorem (Bayesian rationality). None of these statuses implies the strong claims made in the BICS essays about the probative value of the evidence.

13. I am treating IBE arguments as a way to *justify* the truth of a hypothesis – that is, an argument form in which the truth of a hypothesis H is inferred from the fact that H provides the best explanation of some data. In this case, H's explaining the data in question provides evidence (to some degree) that H is true (Harman, 1965; Lipton, 2004, 2007; McCain & Poston, 2024). It is important to distinguish this commonly deployed *epistemic* version of IBE from the *heuristic* version of IBE where explanatory considerations guide inquiry and lead to the discovery or generation of hypotheses (Iranzo, 2007). The term *abduction* has often been used for both heuristic and epistemic IBEs.

14. To justify premise (3), survivalists adduce reasons to rule out more recalcitrant alternative explanations. These reasons concern the alleged dependence of such explanations on assumptions that are ad hoc, lacking independent support, or which suffer from some other kind of epistemic defect. But the survival hypothesis is no less dependent on assumptions characterized by the same kind of epistemic defects, if it is to lead us to expect any data. The survival hypothesis explains nothing unless we bulk it up with a variety of untestable auxiliary assumptions (the Duhem-Quine thesis). But if epistemically defective assumptions justify ruling out counterexplanations, they also justify ruling out the survival hypothesis itself. So, the (traditional) justification for (3) defeats the justification for (2). IBE survival arguments are hoisted by their own explanatory petard. See Sudduth (2016, pp. 214–245, 258–270, 286–307).

15. For different views on the appropriate threshold here, see Achinstein (2001) and Roush (2005). Roush distinguishes between *some evidence* and *good evidence* (Roush, 2005, p. 158). When Pr(H | O) > ½, O is *some* evidence for H, and when Pr(H | O) = high, O is *good* evidence for H.

16. For discussions on different theories of evidence, including those discussed in the present paper, see Achinstein (2001), Fitelson (2011), Hawthorne (2018), Roush (2005), Sober (2002, 2008).

17. "Law of likelihood: The observations *O* favor hypothesis $H_1$ over hypothesis $H_2$ if and only if $Pr(O \mid H_1) > Pr(O \mid H_2)$. And the degree to which *O* favors $H_1$ over $H_2$ is given by the likelihood ratio $Pr(O \mid H_1)/Pr(O \mid H_2)$" (Sober, 2008, p. 32).

18. Typically, it is a hypothesis plus auxiliary assumptions that confers a probability on an observation (Sober, 2008, pp. 141–154). This is often referred to as the Duhem-Quine thesis (Gillies, 1993, pp. 98–116). Roughly, the idea is that statements must be tested in bundles. I assume this throughout, though in the interest of presentational simplicity, I avoid the more cumbersome formalisms Pr(O | H & A) – "A" for auxiliaries – or Pr(O | H & K) – "K" for background knowledge. I discuss the significance of auxiliary assumptions in the latter part of the paper (§8, §11, §13, §14, and §15).

19. "The likelihoodist concept of favoring describes what the evidence says about the competition between any two hypotheses that both probabilify the data at hand. The Bayesian concept of confirmation addresses a special case; it describes what the evidence says about the competition between a hypothesis and its own negation" (Sober, 2008, pp. 34–45).

20. The Bayesian replaces the dichotomous concept belief – one believes p or does not believe p – with the idea that one has different degrees of belief. This results in a more fine-grained interpretation of Royall's belief question. The question is not about what we should believe or not believe full stop, but the level of confidence we should have based on the evidence and whether we ought to increase or decrease our degree of confidence (or do neither) given new evidence.

21. For an account of Bayes' theorem and Bayesian epistemology, see Chalmers (2013), Hawthorne (2011, 2018), Howson and Urbach (2006), Lin (2023), McGrew, T. (2000), Sober (2002, 2008, pp. 8–32), Swinburne (1973, 2002).

22. In Anglo-American philosophy of religion Bayes' theorem has been used to parse explanatory arguments for the existence of God. See Dawes (2009), McGrew, L. (2004), Oppy (2006), Sobel (2004), and Swinburne

(2004).

23. Matlock writes: "Augustine and Fishman (2015) maintain that the materialist position has so much going for it that it should be given the presumption of truth. They introduce a Bayesian analysis in which they assign much more weight to the brain/identity thesis than to the possibility of mind/brain interaction. The outcome of a Bayesian analysis is heavily dependent on how one weights the factors that go into it. By assigning the weights as they do, Augustine and Fishman ensure that the mind/brain identity thesis emerges the winner. However, the mere fact that there are serious questions about the mind/brain identity thesis reduces the weight that may in fairness be allotted to it, and if all the evidence in favor of mind/ brain interaction is taken into account as well, the outcome of the Bayesian analysis looks very different (Matlock 2016b, 2016c). Sudduth (2016) undertakes a similar Bayesian analysis that fails for the same reason (Matlock 2016a)" (Matlock, 2019, p. 246). Matlock here repeats his misrepresentation of Augustine and Fishman, as well as Sudduth (2016), despite Augustine correcting him three years earlier (Augustine, 2016, pp. 216–218).

24. Kelly writes: "Survival-deniers Martin and Augustine (2015) make that negligible prior probability a cornerstone of their own quasi-Bayesian approach to the survival question, devoting a large part of their book simply to repetition of the familiar standard arguments supporting the prevailing physicalist account of brain/mind relations. (Schiller [1927] clearly anticipates this strategy, by the way, and more generally the deliberate use of low priors as a means of preventing accumulation of evidence favoring any opinion one happens not to like)" (Kelly, 2016, p. 593). Kelly is incorrect about Schiller, whom he carelessly references in support of his uninformed and misguided polemic against Bayesianism. In the referenced article, Schiller offered criticisms of *a priori* prejudices that would, in principle, prevent the accumulation of evidence in support of the survival hypothesis (Schiller, 1927, p. 218). Among the prejudices he notes is the (now long outdated) skeptical demand that there be a conclusive proof of survival. On that view, it is easy to dismiss any ostensible evidence for survival on the grounds of inconclusiveness. On Schiller's view, (i) the evidence for survival is cumulative and involves a growing probability, and (ii) inconclusive cases should be permitted to acquire collective weight (Ibid., p. 219). Other than referring to the Baconian inductive method, Schiller does not provide any details as to how such a cumulative argument for survival can be made, what it would actually look like, or whether it would actually be successful. Nor does Kelly. But Bayesianism is the most prominent and well-justified framework for making such a cumulative argument. And nothing Schiller says is evidence against the use of Bayes' theorem for arriving at conclusions about the posterior probability of the survival hypothesis. More to the point, neither I nor Augustine demand a conclusive proof for any hypothesis, and we have said nothing that would prevent the accumulation of evidence favoring survival. All our arguments have consistently assumed Schiller's (i) and (ii). But unlike Schiller and Kelly, we have actually shown our work.

25. It is particularly disappointing that Matlock raises the rigging-of-the-priors objection in his review of Augustine's book, and Kelly does so in his review of my book (see note no. 24). Augustine and I each explain in detail why the rigging objection would be untrue even if we assigned a low prior to the survival hypothesis. Neither Matlock nor Kelly seems aware of how cumulative case reasoning works when constrained by Bayesian updating, despite the fact that Augustine and I discuss this in the very books Matlock and Kelly were reviewing.

26. To illustrate, take the first scenario, where the initial prior probability = 0.10.

| Observation | Pr(H) | Pr(O \| H) | Pr(O \| ~H) | Pr(H \| O) |
|---|---|---|---|---|
| 1 | 0.10 | 0.80 | 0.40 | 0.1818 |
| 2 | 0.1818 | 0.80 | 0.40 | 0.3077 |
| 3 | 0.3077 | 0.80 | 0.40 | 0.4705 |
| 4 | 0.4705 | 0.80 | 0.40 | 0.6399 |

The chart is a streamlined illustration (based on Bayes' theorem) of how an initial prior probability of 0.10 is successively updated with four independent observational data, each of which has a likelihood ratio of 2. Notice that the specific values assigned to Pr(O | H) and Pr(O | ~H) – I chose 0.80 and 0.40 – do not matter, only that the ratio equals 2. The values could have been 0.60 and 0.30 or 0.40 and 0.20. The posterior probability, after the first observation, is 0.1818. This posterior probability becomes the new prior probability, which has increased from 0.10 to 0.1818. The process gets repeated iteratively three more times, resulting in H's final posterior probability = 0.6399 (more probable than not). If survivalists would like to "do the math" and explore probabilistic outcomes with adjustments in the values of priors and likelihoods, they can use a Bayesian calculator. Many online calculators are available: https://bayesian-calculator.greenleafimaging.com or https://www.richardcarrier.info/bayescalculator.html.

27. In another sense, these frequentist methodologies are worse than subjective. They are *epistemically irrelevant*. They are not informative about evidential support or the epistemic status of propositions. Answering the question *"how should we act?"* is a matter of prudential decision-making in which we act *as if* a hypothesis were true or false. This is not the same thing as acquiring evidence that a hypothesis is true or false. Neyman and Pearson admit as much, indicating that a significance test "tells us nothing as to whether in a particular case h is true" (Neyman & Pearson, 1933, p. 142).

28. There are additional arguments that could be offered. Roush (2005, pp. 166–167) argues that Pr(O), Pr(O | H), and Pr(O | ~H) are sufficient to determine the posterior probability of a hypothesis. Roush maintains that Pr(H) and Pr(~H) are better treated as weights, and that we can solve for their values on the basis Pr(O) and the likelihood ratio Pr(O | H)/Pr(O | ~H). She further shows how under particular circumstances we can determine Pr(O) without having first determined Pr(H) and Pr(~H). If Roush is correct, we have another reason to dismiss survivalist complaints about prior probability.

29. This is consistent with Braude's endorsement of the *cumulative* force of the total evidence (Braude, 2003, p. 301; 2021b, p. 19). I have explored Bayesian cumulative case survival arguments (Sudduth, 2016, pp. 202–213, 297–299), mainly because survivalists have often construed the case for survival as a cumulative case argument, and they have been doing so for over a century. See Schiller (1927) and note no. 24. But it is possible to develop a cumulative case argument for survival within the likelihoodist framework. Likelihoodism not only tells us when O favors $H_1$ over $H_2$, but also the *degree* to which O favors $H_1$ over $H_2$, which is given by the likelihood ratio $Pr(O \mid H_1)/Pr(O \mid H_2)$. This allows independent pieces of evidence to strengthen or weaken the degree to which accumulating evidence favors $H_1$ over $H_2$. For any set of independent pieces of evidence $\{O_1, O_2, O_3..., O_n\}$ and contrasting hypothesis $H_1$ and $H_2$, the likelihood ratios can be multiplied to determine the degree to which the total evidence favors $H_1$ over $H_2$. This does not tell us what the probability of the survival hypothesis is, but only the degree to which the total evidence favors the survival hypothesis over a rival hypothesis.

30. Braude makes appeals to parsimony (Braude, 2003, pp. 86–95, 216–222; Braude, 2021b, pp. 25–29), but I do not see what sort of *epistemic* work it is supposed to be doing. For example, it is not informing the prior probability of the survival hypothesis as a Bayesian might say. The survival hypothesis (conjoined with various assumptions about the causal nexus and crippling complexity) entails a model that is allegedly simpler than the model implied by the living-agent psi hypothesis, but his main conclusion is that **the observations are more likely given the simpler model than the alternative.** For example, "(8) *Therefore*, the more potentially wide-ranging and virtuosic we take psi to be, the less likely it becomes that a person's psi could produce an extended and accurate trance persona, or provide all the detailed, intimate information found in the most astonishing survival cases—and even more so, to do these things consistently" (Ibid., 2021b, p. 27). The relevant observational evidence is the "extended and accurate trance persona" which provides "all the detailed, intimate information found in the most astonishing survival cases." Braude is concluding that these features of mediumship are less to be expected given the living-agent psi than they are given the survival hypothesis. His conclusion implies a likelihood inequality. (LL) tells us that in this situation, the survival hypothesis enjoys contrastive evidential support in relation to the alternative. Also, simplicity is typically invoked as a criterion of choice when competing theories equally predict the data, but that is not the case here. Braude also relies heavily on explanatory reasoning. Although likelihood inequalities play a significant role in Braude's explanatory reasoning, he also refers to an array of ostensible explanatory virtues such as "empirical adequacy, explanatory simplicity, and conceptual cost" (Braude, 2003, p. 220). But it is not clear how these are functioning in his arguments.

31. I do not know what Braude means by a "reasonable basis" for belief in survival. I agree that the evidence is such that a person who adopted various assumptions could, upon considering the various strands of evidence, repeatedly update his beliefs in accordance with Bayes' theorem and eventually end up assigning the survival hypothesis a subjective probability (much) greater than ½ (Bayesian rationality). Alternatively, some survivalists could be within their intellectual rights in believing in survival after considering the evidence (deontological rationality) or not be cognitively askew (proper function rationality). But we could say the same thing about the evidence that God exists, that Jesus Christ rose from the dead, that the universe is a computer simulation, that Oumuamua is an artifact from an alien spacecraft or, to insert a more mundane example, that Arthur Lee Allen was the Zodiac killer. There is evidence that provides a reasonable basis for all these beliefs, but not in any robust or

truth-conducive sense of evidential justification.

32. In the philosophy of religion, it is common to see a distinction between (hard) atheists who deny that God exists and (soft) atheists who neither believe that God exists nor believe that God does not exist. This is parallel to the distinction between D-skepticism and W-skepticism. In each case, the latter may also be described as a form of agnosticism about the hypothesis.

33. I understand this broadly so that it includes *doubting* or *denying* that an argument shows that survival is the best explanation of the data, survival is probable, or some set of facts is evidence for survival.

34. The extreme interpretation of Augustine occurs elsewhere in the Braude et al. reply. "Augustine seems to infer not simply that nothing psychic could be happening during the tests for OBErs and NDErs, but more likely, given his broad skepticism about things paranormal, that nothing psychic *could occur*" (Braude et al., 2022, p. 404). This is unwarranted, both as an interpretation of Augustine's arguments and as a way of characterizing skepticism in general.

35. "Counterevidence" here does not mean evidence that survival is impossible, nor does the counterevidence logically entail that survival is false, nor is Augustine leveraging this particular counterevidence as sufficient to prove that the survival hypothesis is highly improbable.

36. As Augustine rightly notes (2022a, p. 374), the Surprise Principle is typically baked into accounts of explanatory power. In Sudduth (2016) I argued that this is because survivalists think explanatory merit requires that the survival hypothesis S leads us to expect the observation O more than rival hypotheses ($R_1$..., $R_n$) do. Regardless of how strict or loose survivalists regard such expectations, they are nonetheless committed to the explanatory salience of likelihood inequalities, either between the survival hypothesis and some specific rival hypothesis – Pr(O | S) > Pr(O | $R_i$) – or between the survival hypothesis and its negation – Pr(O | S) / Pr(O | ~S), where the catchall ~S refers to all logically possible alternative hypotheses. This understanding of explanatory power in connection with survival arguments goes back at least as far as C.D. Broad (1925/1960, ch.12).

37. In the interest of presentational simplicity, the likelihoods are formulated with the scientific facts treated collectively as *F*, but the ten scientific facts Augustine lists can also be treated individually in a cumulative case argument. There are potential advantages to this alternate formulation, especially if the argument is expanded into a full Bayesian cumulative case argument including initial prior probabilities and successive updating. See below in main text.

38. Augustine's support for premises (1) and (2) does not depend *solely* on the mail bin analogy. For example, the reasons he offers to believe that "brain development is the engine pulling the train" (Augustine, 2022a, p. 372) explain why the first three facts in the bullet point list support (1) and (2). His response to Ducasse on the proportional correlation between brain activity and mental complexity supports the fourth and fifth item in the list. And he presents further support with a passage from Henry Stapp (Ibid., p. 373) and a "disrupted hardware" analogy (Ibid., pp. 390–391n6). Thanks to Augustine for pointing this out to me.

39. If 0.5 is the threshold for expectedness/prediction, then the formal rendering of premises (1) and (2) would be Pr(F | D) > 0.5 and Pr(F | ~D) < 0.5. Therefore, the likelihood ratio – Pr(F | D)/ Pr(F | ~D) – is greater than 1. But whenever the prior of a hypothesis is 0.5, and the likelihood ratio is greater than 1, the posterior probability of the hypothesis will be greater than 0.5 – it will be more probable than not. So, if we rely on the principle of indifference and assign Pr(D) = 0.5, then given premises (1) and (2), it follows that Pr(D | F) > 0.5 – more probable than not. We can get to the same conclusion even if we initially assign the independence thesis a higher prior probability than the dependence thesis. For example, if Pr(~D) = 0.6 (probable) and Pr(D) = 0.4 (improbable), but we specify that the likelihood ratio = 2 (the evidence is twice as expected given D than given ~D), it follows that Pr(D | F) > 0.5. If Pr(~D) = 0.7 (probable) and Pr(D) = 0.3 (improbable) and the likelihood ratio = 3, it also follows that Pr(D | F) > 0.5. Likelihoods, not priors, do the heavy evidential lifting.

40. An argument is a good one just if the premises and inferential connection between the premises and conclusion are appropriately credentialized. This means the premises should be rationally acceptable (or have some other positive epistemic status) and strongly relevant to the conclusion. Given that what makes for a good argument has these narrow parameters, there are a limited number of ways to critically respond to an argument. One can challenge the rational acceptability of the premises or the strength of the inferential connection between the premises and the conclusion. One can also show that there is some rationally acceptable proposition which would weaken the inferential connection if we added it to the arguer's set of premises. However, merely adducing reasons to deny the conclusion of the arguer's argument is not a proper dialectical maneuver. One would also have to show that one's reasons to deny the arguer's conclusion

outweigh the reasons the arguer appealed to in support of it.

41. Even Augustine acknowledges that they seem to have missed what is plausibly required of him given what he intends to argue (Augustine, 2022b, pp. 413–415).

42. Hornell Hart says, "Some mediums have received honestly information which they *could not* have obtained normally, and which *cannot* be explained as due to lucky chance" (1959, p. 73, emphasis mine).

43. Hart is an egregious offender in this regard. First, his analogies are atrociously implausible. "The existence of counterfeit money certainly does not *disprove* the existence of genuine money" (Hart, 1959, p. 52) and "If we follow the logic of the anti-survivalists we should deduce from this case of a pseudo doctor that all doctors are frauds" (Ibid., p. 255). Unlike survival, there is no antecedent dispute about whether actual money or actual doctors exist, nor is there any antecedent dispute about what counts as evidence for their existence. Second, the sources Hart cites to illustrate the "anti-survivalist" view that *all mediums are frauds* do not actually support the attribution. For example, Hart quotes Joseph Rinn: "I must take the position that no evidence exists tending to prove survival or spirit communication. . . . During my investigations I never found anything but fraud and never met even one person with supernormal or supernatural power" (Ibid., p. 52). Hart takes this as support for his claim that some investigators say that "psychic claims are ALL fraudulent" (Ibid., p. 52). The quote does not support such a conclusion, which Hart fallaciously drew from it. It does, however, support the more modest view that Rinn has no good reason to accept the claims of any medium. A stronger conclusion – there are no genuine mediums – would be warranted if there were good reason to suppose that (i) if genuine mediumship exists, then an adequate investigation should have produced conspicuous evidence of it, (ii) adequate investigations have been conducted, and (iii) none of the investigations have discovered conspicuous evidence for genuine mediumship.

44. Braude et al. refer to the evidence for a properly conducted investigation or experiment, but I should think there is considerable dispute about what such a thing would actually look like, especially if "we have no idea what is really going on in a parapsychological experiment" (Braude et al., YR p. 405). Also, why suppose that a properly conducted experiment or investigation would sufficiently ferret out fraud?

45. I may have no positive evidence that a particular used car salesman is going to swindle me, but his doing so is not a mere (logical or epistemic) possibility. Our background knowledge influences our initial degree of credence in particular situations. The same holds for mediumship. And, it is even more perspicuous in cases where we know that a particular medium has previously engaged in fraud, fishing, or other nefarious behavior, such as was the case with Mrs. Piper's Phinuit control. While fraud *can* co-exist with genuine abilities, this is evidentially irrelevant to the kinds of probabilistic assessments involved in Augustine's arguments. Even a shady used car salesman *can* make a fair offer on occasion despite a track-record of swindling customers. The problem is that knowing the latter gives one a defeater for believing the former.

46. Consider the James Leininger reincarnation case (Leininger, 2021), relatively recent compared to Mrs. Piper's sittings. The Leininger case is considerably less impressive once we include facts only disclosed many years after researchers had repeatedly applied their extensive skills to the case – for example, the video James watched contained imagery of a plane being shot down, the flight museum he visited on at least two occasions displayed images containing World War II information baked into James's early veridical statements, and other allegedly unique claims attributed to James are found on other videos the boy watched. In addition to a hornet's nest of logical fallacies, the case illustrates how dark data amplify what is otherwise a simulation of evidence for survival. See Sudduth (2021b, 2022a, 2022b).

47. Nor will it do to say we have isolated ostensibly genuine phenomena merely because no chicanery was detected, though the same methods detected fraud in other instances. We *might* draw that conclusion. We might also conclude that the methods for detecting fraud are not always properly calibrated to detect more subtle forms of deception. We are at the mercy of the limits of the investigator/researcher's imagination, which has on various occasions proven to be less extensive than the cunningness of tricksters. Kai Mügge's physical mediumship illustrates this. See Braude (2014, 2016), Mulacz (2015), and Nahm (2014, 2015, 2016).

48. Among Augustine's considerations are mediumship being an exemplar of known fraud, DRW's superficial treatment of the history of fraud, errors in their discussion of the Scole sittings in the 1990s, overstating the implications of researchers' failure to detect fraud in some cases of mediumship, rationalizing instances where mediums have been caught cheating as of little relevance, and the subjective nature of assigning letter grades.

49. Similarly, Sage considered improbable scenarios that

we should expect if Mrs. Piper acquired information through spies. For example, "If Mrs. Piper obtained the information through spies in her employment, these spies would be obliged to send her private details about all the families in the United States and Europe…" (1904/2007, p. 36).

50. We can single out Julie Beischel here to further illustrate. In her prize-winning BICS essay on mediumship, she wrote: "For example, the threshold level of probability used by scientists to determine whether or not to reject a null hypothesis (usually $p < .05$) can be equated to the 'standard of proof' threshold used in a court system to determine whether or not proof beyond a reasonable doubt has been established" (Beischel, 2021, p. 60). Beischel commits three mistakes here. **First**, as noted in the text, $p < .05$ (or other p values) is an insufficient guide to evidence assessment. It cannot reasonably be used to justify the claim that there is strong evidence against the null hypothesis or for rejecting the null hypothesis. Even the Neyman-Pearson alternative on which one of Beischel's cited sources relies is unsound when used in this way; it may be adequate for *decision procedures*, but not for assessments of *evidential weight*. As Royall emphasized, we should distinguish questions about how we should interpret observations as evidence for hypotheses and the question of what we should *do* now that we have some observations (Royall, 1997, pp. 3-4). **Second**, Beischel overgeneralizes about scientists – Bayesians or likelihoodists do not accept significance tests, and some advocates of significance tests (e.g., Fisher) and the Neyman-Pearson alternative would not accept Beischel's suggested use of the p-value. **Third**, the suggested legal application of the p-value implies the well-known Prosecutor's Fallacy. Some item(s) of evidence may be highly improbable ($p < .05$) given the non-guilt of the defendant. This does not justify inferring that the non-guilt of the defendant is highly improbable (= guilt is highly probable), nor does it otherwise justify the rejection of the null hypothesis (non-guilt) and so rendering a guilty verdict.

51. An empirical hypothesis is one that can be tested against observable phenomena, but this requires a logical connection between the hypothesis and features of the observable world. Survival, if it is an empirical hypothesis, must have such a connection, even if it means nothing more than an observation is more probable under the survival hypothesis than under some alternative. Compromising this logical connection compromises the status of survival as an empirical hypothesis. If this is what it means for survival to be a part of "frontier areas of science" (Nahm, 2021, p. 59), then survival research looks like metaphysics with a scientific veneer and indistinguishable from faith-based survivalism. Religious survivalists shield their survival beliefs from empirical disconfirmation, but, unlike empirical survivalists, they do so consistently and consciously since they acknowledge the metaphysical character of their afterlife beliefs. Thanks to Keith Augustine for raising this point in response to an earlier draft of this paper.

52. Predictive power is not the only explanatory virtue which has been leveraged in survival arguments. If it is a component of explanatory merit – it typically is – what matters is whether it is leveraged consistently (see below in main text). Moreover, as a component of explanatory power, predictive power can be downgraded to contrastive expectedness in accordance with (LL), as I suggested earlier in connection with Braude. See §13 in the main text for a discussion of this survivalist maneuver.

53. Although this corrective should be clear from my exposition, Augustine has elsewhere made the same nuanced point. "The failure to secure replicable positive results in NDE target-identification experiments does not establish the nonexistence of any spiritual realms, but it does serve to substantially challenge positive arguments in favor of the existence of spiritual realms from NDE reports" (Augustine, 2019, p. 595). Failed experiments provide data that are undercutting defeaters, not rebutting defeaters.

54. If the probability of Hodgson's auxiliaries (1) and (2) were high given the simple survival hypothesis – human consciousness persists after biological death – then they might not be obviously *ad hoc*. But the conditional probability of Hodgson's auxiliaries is not very high given the simple survival hypothesis. Hodgson's model is only one of at least a dozen possibly true models of what consciousness would be like should it survive death (Sudduth, 2016, pp. 33-46). Absent any further evidence, Pr(Hodgson's auxiliaries | simple survival) < .10, which is quite low, and conservatively so. But this is a problem for the survivalist. Pr(H & A) cannot be greater than Pr(A). From a Bayesian perspective, the survival hypothesis, even if it is assigned a very high prior probability, will have a very low prior probability when it is conjoined to improbable auxiliary assumptions. See Chalmers (2013, p. 573).

55. When discussing confusions even the best communicators exhibit, Hodgson draws attention to how these are more prominent in initial communications from adult communicators than they are in their later communications (Hodgson, 1898, pp. 391-392). In this context, Hodgson appeals to (1) "loss of familiarity with

the conditions of using a gross material organism" and (2) "inability to govern precisely and completely the particular gross material organism which they are compelled to use" (Ibid., pp. 366–367). Hodgson's justification for (1) and (2) depends on the justification for supposing that the communicators really are who they say they are. This is epistemically circular reasoning.

56. It is easy to see why this may not have an outcome favorable to survivalists. If the survivalist asserts *favoring without predictive likelihoods*, then for every relevant observation $O_i$ from the set of total observations ($O_1$, $O_2$, $O_3$, ... $O_n$), Pr($O_i$ | survival hypothesis) $\leq$ 0.5. By contrast, for many observations, conventional hypotheses do strongly predict the observation. Otherwise put, supporting evidence will only weakly support survival, but counterevidence will strongly count against survival. This does not bode well for a cumulative case (LL) survival argument, but I will leave it to survivalists to, in the words of Braude, "wallow in the grubby details" (Braude et al., 2022, p. 403).

57. See Sober (2008, pp. 145, 148–154) for the supporting argument for his account of testability. I have discussed it elsewhere (Sudduth, 2016, pp. 232–234).

58. What is important here is a probative concept of probability, not subjective probability. Nonetheless, whatever subjective degree of belief a person has in the survival hypothesis, they should use Bayes' theorem to update their degree of belief when they acquire new evidence. So, skeptics and survivalists should follow Bayesian updating with their respect to their personal beliefs about survival.

59. Confirmation theory, especially Bayesianism, is widely deployed outside the hard sciences. **Philosophy of Religion:** Chandler & Harrison, 2012; Collins, 2009; Dawes, 2009; McGrew, L., 2004; Oppy, 2006; Sobel, 2003; Swinburne, 2004. **Jurisprudence:** Aitken, Taroni, and Bozza, 2022; Bex and Walton, 2012; Dahlman, Stein, & Allen, 2021; Dahlman & Mackor, 2019; Dawid, 2002; Faigman & Baglioni Jr., 1988; Fenton, Neil, and Berger, 2016; Fienberg, 1997; Finkelstein & Fairley, 1970; Fischoff & Beyth-Marom, 1983; Friedman, 1997a, 1997b; Gastwirth, 2020; Haack, 2014; Jellema, 2021; Kaye, 1988; Pardo & Allen, 2007; Strnad, 2007; Tillers & Green, 1988. **Psychology:** Etz & Vandekerckhove, 2018; Kruschke, 2014; Lee & Wagenmakers, 2014; Rouder, Speckman, Sun, Morey, & Iverson, 2009; Wagenmakers, Morey, & Lee, 2016. **Social Sciences:** Fairfield & Charman, 2019; Gill, 2014; Kaplan, 2014; Russo, 2020. **Sociology:** Jackman, 2009; Western & Jackman, 1994. **Political Science:** Gill, 1999; Gill, & Wasif, 2020; Jackman, 2004; Martin, 2008. **Economics:** Koop, 2003; Koop, & Tole, 2004. **Archeology:** Buck, Cavanagh, & Litton, 1996. **Medicine and Epidemiology:** Goodman, 1999; Greenland, 2006. **Health and Nutrition:** Gleason, & Harris, 2019. **Environmental Sciences:** Annan, 2010; Lee, Zwiers, Hegerl, Zhang, & Tsao, 2005; McCarthy, 2007; O'Hagan, 2019.

60. Braude concluded his winning BICS essay by saying, "So even if the best actual evidence doesn't warrant a reassuring confidence in the reality of survival, at the very least it encourages optimism on the matter. Confidence will have to come later, if it comes at all" (Braude, 2021b, p. 52).

## REFERENCES


About BICS. (2021, January 21). *Bigelow Institute for Consciousness Studies*. https://www.bigelowinstitute.org/about.php

Achinstein, P. (2001). *The book of evidence*. Oxford University Press. https://doi.org/10.1093/0195143892.001.0001

Aitken, C., Taroni, F., & Bozza, S. (2022). Evidence, probability and relative plausibility. *The International Journal of Evidence & Proof, 26*(4), 309–334. https://doi.org/10.1177/13657127221114508

Almeder, R. (1992). *Death and personal survival*. Rowman and Littlefield. https://doi.org/10.1016/S1521-6942(96)80010-6

Almeder, R. (1996). Recent responses to survival research. *Journal of Scientific Exploration, 10*(4), 495–517.

Annan, J.D. (2010). Bayesian approaches to detection and attribution. *Wiley Interdisciplinary Reviews: Climate Change, 1*(4), 486–489. https://doi.org/10.1002/wcc.47

Augustine, K. (2016). Evidence or prejudice? A reply to Matlock. *Journal of Parapsychology, 80*, 203–231.

Augustine, K. (2019). Near-death experiences are not evidence for either atheism or theism. In J.W. Kotereski & G. Oppy (Eds.), *Theism and atheism: Opposing arguments in philosophy* (pp. 594–596). Macmillan Reference.

Augustine, K. (2022a). How 'not' to do survival research: Reflections on the Bigelow Institute essay competition. *Journal of Scientific Exploration, 36*(2), 366–398. https://doi.org/10.31275/20222581

Augustine, K. (2022b). Final reply: When will survival researchers move past defending the indefensible? *Journal of Scientific Exploration, 36*(2), 412–435. https://doi.org/10.31275/20222695

Augustine, K. (2022c). Answering more of the same: a reply to Nahm. *Journal of Scientific Exploration, 36*(4), 794–808. https://doi.org/10.31275/20222801

Augustine, K. (2023, May 1). Christian vs. survivalist apologetics. *The Secular Web*. Retrieved from https://infidels.org/library/modern/survivalist-apologetics

Augustine, K., & Fishman, Y. I. (2015). The dualist's dilemma: The high cost of reconciling neuroscience with a soul. In M. Martin & K. Augustine (Eds.), *The myth of an afterlife: The case against life after death* (pp. 203–292). Rowman & Littlefield.

Beischel, J. (2021, November 24). Beyond reasonable: Scientific evidence for survival. *Bigelow Institute for Consciousness Studies*. Retrieved from https://www.bigelowinstitute.org/wp-content/uploads/2022/10/beischel-scientific-evidence-survival.pdf

Bex, F.J., & Walton, D.N. (2012). Burdens and standards of proof for inference to the best explanation: three case studies. *Law, Probability and Risk, 11*(2–3), 113–133. https://doi.org/10.1093/lpr/mgs003

Bigelow, R.T., & Kelleher, C.A. (2021). The 2021 Bigelow institute for consciousness studies (BICS) essay contest. *Journal of Scientific Exploration, 36*(2), 350–365. https://doi.org/10.31275/20222693

Braude, S. E. (2003). *Immortal remains: The evidence for life after death*. Rowman & Littlefield.

Braude, S.E. (2014). Investigations of the Felix experimental group: 2010–2013. *Journal of Scientific Exploration, 28*(2), 285–343.

Braude, S.E. (2016). Follow-up investigation of the Felix Circle. *Journal of Scientific Exploration, 30*(1), 27–55.

Braude, S.E. (2021a). More sloppy reasoning about survival. *Journal of Scientific Exploration, 35*(3), 477–484. https://doi.org/10.31275/20212251

Braude, S. E. (2021b, November 24). A rational guide to the best evidence of postmortem survival. *Bigelow Institute for Consciousness Studies*. Retrieved from https://www.bigelowinstitute.org/wp-content/uploads/2022/10/braude-guide-postmortem-survival.pdf

Braude, S. E., Barušs, I., Delorme, A., Radin, D., & Wahbeh, H. (2022). Not so fast: A response to Augustine's critique of the BICS contest. *Journal of Scientific Exploration, 36*(2), 399–411. https://doi.org/10.31275/20222649.

Broad, C.D. (1919). The antecedent probability of survival. *The Hibbert Journal, 17*, 561–578.

Broad, C.D. (1925/1960). *The mind and its place in nature*. Littlefield, Adams & Co.

Broad, C. D. (1962). *Lectures on psychical research*. Routledge & Kegan Paul.

Buck, C. E., Cavanagh, W. G., & Litton, C. D. (1996). *Bayesian approach to interpreting Archaeological Data*. John Wiley & Sons.

Chalmers, A. (2013). The Bayesian approach. In M. Curd, J.A. Cover, & C. Pincock (Eds.), *Philosophy of science: The central issues* (2nd ed., pp. 565–578). W.W. Norton & Company.

Chandler, J., & Harrison, V.S. (2012). Probability in the philosophy of religion. In J. Chandler & V. S. Harrison (Eds.), *Probability in the philosophy of religion* (pp. 1–24). Oxford University Press. https://doi.org/10.1093/acprof:oso/9780199604760.003.0001

Collins, R. (2009). The teleological argument: an exploration of the fine-tuning of the universe. In W.L. Craig & J.P. Moreland (Eds.), *The Blackwell companion to natural theology* (pp. 202–281). Wiley-Blackwell. https://doi.org/10.1002/9781444308334.ch4

Curd, M., Cover, J.A., & Pincock, P. (2013). Commentary. In M. Curd, J.A. Cover, & C. Pincock (Eds.), *Philosophy of science: The central issues* (2nd ed., pp. 597–646). W.W. Norton & Company.

Dahlman, C., Stein, A., & Allen, R.J. (2021). *Philosophical Foundations of evidence law*. Oxford University Press. https://doi.org/10.1093/oso/9780198859307.001.0001

Dahlman, C., & Mackor, A.R. (2019). Coherence and probability in legal evidence. *Law, Probability and Risk, 18*(4), 275–294. https://doi.org/10.1093/lpr/mgz016

Dawes, G. (2009). *Theism and explanation*. Routledge.

Dawid, A.P. (2002). Bayes's theorem and the weighing of evidence by juries. In R. Swinburne (Ed.), *Bayes's theorem (Proceedings of the British Academy, 113)* (pp. 71–90). Oxford University Press.

Delorme, A., Radin, D., & Wahbeh, H. (2021, November 24). Advancing the evidence for survival of consciousness. *Bigelow Institute for Consciousness Studies*. Retrieved from https://www.bigelowinstitute.org/Winning_Essays/3_Dean_Radin_et_al.pdf

Dodds, E. R. (1934). Why I do not believe in survival. *Proceedings of the Society for Psychical Research, 42*, 147–172.

Ducasse, C. J. (1961). *A critical examination of the belief in a life after death*. Charles C. Thomas.

Edwards, A.W.F. (1972). *Likelihood*. Cambridge University Press.

Etz, A., & Vandekerckhove, J. (2018). Introduction to Bayesian inference for psychology. *Psychonomic Bulletin & Review, 25*, 5–34. https://doi.org/10.3758/s13423-017-1262-3

Faigman, D., & Baglioni Jr., A.J. (1988). Bayes' theorem in the trial process: Instructing jurors on the value of statistical evidence. *Law and Human Behavior, 12*(1), 1–17.

Fairfield, T., & Charman, A. (2019). A dialogue with the data: The Bayesian foundations of iterative research in qualitative social science. *Perspectives*

*on Politics, 17*(1), 154–167. https://doi.org/10.1017/S1537592718002177

Federal Rules of Evidence. (2015). Rule 401: Relevance and its Limits. In *Federal Rules of Evidence, 2015 edition* (pp. 6–12). Michigan Legal Publishing Ltd.

Federal Rules of Evidence. (2015). Rule 403: Relevance and its Limits. In *Federal Rules of Evidence, 2015 edition* (pp. 6–12). Michigan Legal Publishing Ltd.

Fenton, N., Neil, M., & Berger, D. (2016). Bayes and the law. *Annual Review of Statistics and Its Applications, 3*, 51–77. https://doi.org/10.1146/annurev-statistics-041715-033428

Fienberg, S. E. (1997). Theories of legal evidence: what properties should they ideally possess and when are they informative? *International Journal of Evidence and Proof, 1*(Special Issue), 309–312. https://doi.org/10.1177/1365712797001special07

Finkelstein, M., & Fairley, W. B. (1970). A Bayesian approach to identification evidence. *Harvard Law Review, 83*, 489–517. https://doi.org/10.2307/1339656

Fischoff, B., & Beyth-Marom, R. (1983). Hypothesis evaluation from a Bayesian perspective. *Psychological Review, 90*, 239–260. https://doi.org/10.1037/0033-295X.90.3.239

Fitelson, B. (2007). Likelihoodism, Bayesianism, and relational confirmation. *Synthese, 156*, 473–489. https://doi.org/10.1007/s11229-006-9134-9

Fitelson, B. (2011). Favoring, likelihoodism, and Bayesianism. *Philosophy and Phenomenological Research, 83*(3), 666–672. https://doi.org/10.1111/j.1933-1592.2011.00536.x

Friedman, R. D. (1997a). Answering the bayesioskeptical challenge. *International Journal of Evidence and Proof, 1*(Special Issue), 276–291. https://doi.org/10.1177/1365712797001special02

Friedman, R.D. (1997b). Towards a (Bayesian) convergence? *International Journal of Evidence and Proof, 1*(Special Issue), 348–353. https://doi.org/10.1177/1365712797001special16

Gastwirth, J.L. (2020). The role of statistical evidence in civil cases. *Annual Review of Statistics and Its Application, 7*, 39–60. https://doi.org/10.1146/annurev-statistics-031219-041238

Gauld, A. (1982). *Mediumship and survival: A century of investigations*. Heinemann.

Gauld, A. (2022). Heyday of mental mediumship: 1880s-1930s. In Z. Weaver (Ed.), *Investigators, Mediums, and Communicators* (pp. 215–230). White Crow Books.

Gill, J. (1999). The insignificance of null hypothesis significance testing. *Political Research Quarterly, 52*(3), 647–674. https://doi.org/10.1177/106591299905200309

Gill, J. (2014). *Bayesian methods: A social and behavioral sciences approach* (3rd ed.). Chapman and Hall/CRC.

Gill, J., & Wasif, K. (2020). Bayesian analyses of political decision making. In D. P. Redlawsk (Ed.), *The Oxford encyclopedia of political decision making*. Oxford University Press. https://doi.org/10.1093/acrefore/9780190228637.013.1002

Gillies, D. (1993). *Philosophy of science in the twentieth century*. Blackwell Publishers.

Gleason, P.M., & Harris, J.E. (2019). The Bayesian approach to decision making and analysis in nutrition research and practice. *Journal of the Academy of Nutrition and Dietetics, 119*(12), 1993–2003. https://doi.org/10.1016/j.jand.2019.07.009

Goodman, S. N. (1999). Toward evidence-based medical statistics. 1: The P value fallacy. *Annals of Internal Medicine, 130*(12), 995–1004. https://doi.org/10.7326/0003-4819-130-12-199906150-00008

Greenland, S. (2006). Bayesian perspectives for epidemiological research: I. Foundations and basic methods. *International Journal of Epidemiology, 35*(3), 765–775. https://doi.org/10.1093/ije/dyi312

Griffin, D.R. (1997). *Parapsychology, philosophy, and spirituality: A postmodern exploration*. State University of New York Press.

Haack, S. (2014). *Evidence matters: Science, proof, and truth in the law*. Cambridge University Press. https://doi.org/10.1017/CBO9781139626866

Hand, D. (2014). *The improbability principle: Why coincidences, miracles, and rare events happen every day*. Scientific American/Farrar, Straus and Giroux.

Harman, G.H. (1965). The inference to the best explanation. *Philosophical Review, 74*(1), 88–95. https://doi.org/10.2307/2183532

Hart, H. (1959). *The enigma of survival: The case for and against an afterlife*. Charles C. Thomas.

Hawthorne, J. (2011). Confirmation theory. In P.S. Bandyopadhyay & M.R. Forster (Eds.), *Philosophy of statistics (Handbook of the philosophy of science, 7)* (pp. 333–389). Elsevier. https://doi.org/10.1016/B978-0-444-51862-0.50010-1

Hawthorne, J. (2018, Spring). Inductive logic. In E. N. Zalta (Ed.), *Stanford encyclopedia of philosophy*. Stanford University. https://plato.stanford.edu/archives/spr2018/entries/logic-inductive/

Hodgson, R. (1898). A further record of observations of certain phenomena of trance. *Proceedings of the Society for Psychical Research, 13*, 284–582.

Howson, C., & Urbach, P. (2006). *Scientific reasoning: The Bayesian approach* (3rd ed.). Open Court.

Houran, J. (2022). Introduction to the special subsection: contemplating the BICS essay contest on sur-

vival. *Journal of Scientific Exploration, 36*(2), 348–349. https://doi.org/10.31275/20222659

Iranzo, V. (2007). Abduction and inference to best explanation. *Theoria, 60*, 339–346. https://doi.org/10.1387/theoria.455

Jackman, S. (2004). Bayesian analysis for political research. *Annual Review of Political Science, 7*, 483–505. https://doi.org/10.1146/annurev.polisci.7.012003.104706

Jackman, S. (2009). *Bayesian analysis for the social sciences*. Wiley. https://doi.org/10.1002/9780470686621

James, W. (1886). Report on the committee on mediumistic phenomena. In F. Burkhardt & F. Bowers (Eds.), *The works of William James, Vol. 16: Essays in psychical research* (pp. 14–18). Harvard University Press.

James, W. (1909). Report on Mrs. Piper's Hodgson-control. *Proceedings of the Society for Psychical Research, 23*, 2–121.

Jellema, H. (2021). The reasonable doubt standard as inference to the best explanation. *Synthese, 199*, 949–973. https://doi.org/10.1007/s11229-020-02743-8

Kaplan, D. (2014). *Bayesian statistics for the social sciences*. The Guilford Press.

Kaye, D.H. (1988). What is Bayesianism: A guide for the perplexed. *Jurimetrics Journal, 28*, 161–177.

Kelly, E. (2016). Review of *A philosophical critique of empirical arguments for postmortem survival*, by M. Sudduth. *Journal of Scientific Exploration, 30*, 586–595.

Kelly, T. (2016, Winter). Evidence. In E.N. Zalta (Ed.), *Stanford Encyclopedia of Philosophy*. Stanford University. https://plato.stanford.edu/archives/win2016/entries/evidence/

Koop, G. (2003). *Bayesian econometrics*. John Wiley & Sons.

Koop, G., & Tole, L. (2004). An investigation of thresholds in air pollution-mortality effects. *Environmental Health Perspectives, 112*(12), 1183–1187.

Kruschke, J. K. (2014). *Doing Bayesian data analysis: A tutorial with R, JAGS, and Stan*. Academic Press. https://doi.org/10.1016/B978-0-12-405888-0.00008-8

Lee, M., & Wagenmakers, E.J. (2014). *Bayesian psychological modeling: A practical guide*. Cambridge University Press.

Lee, T.K., Zwiers, F., Hegerl, G., Zhang, X., & Tsao, M. (2005). A Bayesian approach to climate change detection and attribution. *Journal of Climate, 18*(13), 2429–2440. https://doi.org/10.1175/JCLI3402.1

Leininger, B. (2021, November 24). Consciousness survives physical death: Definitive proof of reincarnation. *Bigelow Institute for Consciousness Studies*. Retrieved from https://www.bigelowinstitute.org/contest_winners3.php

Lin, H. (2023, Winter). Bayesian epistemology. In E. N. Zalta (Ed.), *Stanford encyclopedia of philosophy*. Stanford University. https://plato.stanford.edu/archives/win2023/entries/epistemology-bayesian/

Lipton, P. (2004). *Inference to best explanation* (2nd ed.). Routledge.

Lipton, P. (2007). Is the best good enough? In D. Papineau (Ed.), *The philosophy of science* (pp. 93–106). Clarendon Press.

Lodge, O. (1890). A record of observations of certain phenomena of trance (2). *Proceedings of the Society for Psychical Research, 6*, 443–557.

Long, J. (2021, November 24). Evidence for survival of consciousness in near-death experiences: Decades of science and new insights. *Bigelow Institute for Consciousness Studies*. Retrieved from https://www.bigelowinstitute.org/wp-content/uploads/2022/10/long-survival-consciousness.pdf.

Lund, D. (2009). *Persons, souls and death: A philosophical investigation of an afterlife*. McFarland.

Martin, A.D. (2008). Bayesian analysis. In J.M. Box-Steffensmeier, H.E. Brady, & D. Collier (Eds.), *The Oxford handbook of political methodology* (pp. 494–510). Oxford University Press. https://doi.org/10.1093/oxfordhb/9780199286546.003.0021

Matlock, J. G. (2016a, November). Review of *Sudduth (2016), A philosophical critique of empirical arguments for postmortem survival*, *Journal of Parapsychology, 80*, 107–110. http://jamesgmatlock.com/reviews-2/review-of-sudduth-2016/.

Matlock, J. G. (2016b). The myth of mortality: Comments on Martin and Augustine's *The Myth of an Afterlife. Journal of Parapsychology, 80*(2), 190–203.

Matlock, J. (2016c). Whose prejudice? A response to the replies of Augustine, Smythe, and Larsen. *Journal of Parapsychology, 80*, 235–250.

Matlock, J. (2019). *Signs of reincarnation: Exploring beliefs, cases, and theory*. Rowman and Littlefield.

McCarthy, M. A. (2007). *Bayesian methods for ecology*. Cambridge University Press. https://doi.org/10.1017/CBO9780511802454

McCain, K., & Poston, T. (2024). Explanation and evidence. In M. Lasonen-Aarnio & C. Littlejohn (Eds.), *The Routledge handbook of the philosophy of evidence*. Routledge. https://doi.org/10.4324/9781315672687-30

McGrew, L. (2004). Testability, likelihoods, and design. *Philo, 7*(1), 5–21. https://doi.org/10.5840/philo2004711

McGrew, T. (2000). Probabilistic confirmation theory and Bayesian reasoning: An annotated bibliography. Retrieved from https://timothymcgrew.com/wp-content/uploads/2020/06/ConfirmationTheoryAnnBib.

pdf
Mulacz, P. (2015). Fall of the house of Felix: An investigation into the mediumship of Kai Mügge and the Felix circle. *Paranormal Review, 74*(Spring), 16–22.
Munves, J. (1997). Richard Hodgson, Mrs. Piper and 'George Pelham': A centennial reassessment. *Journal of the Society for Psychical Research, 62*, 138–154. https://doi.org/10.1111/j.1540-5834.1997.tb01252.x
Myers, F.W.H. (1890). A record of observations of certain phenomena of trance (1). Introduction. *Proceedings of the Society for Psychical Research, 6*, 436–442.
Nahm, M. (2014). The development and phenomena of a circle for physical mediumship. *Journal of Scientific Exploration, 28*(2), 229–283.
Nahm, M. (2015). Promissory mediumship. *Paranormal Review, 74*, 15.
Nahm, M. (2016). Further comments about Kai Mügge's alleged mediumship and recent developments. *Journal of Scientific Exploration, 30*(1), 56–62.
Nahm, M. (2021, November 24). Climbing mount evidence: A strategic assessment of the best available evidence for the survival of human consciousness after permanent bodily death. *Bigelow Institute for Consciousness Studies*. https://www.bigelowinstitute.org/Winning_Essays/Michael_Nahm.pdf
Nahm, M. (2022). A Guardian angel gone astray: How not to engage in scientific debates. *Journal of Scientific Exploration, 36*(4), 783–793. https://doi.org/10.31275/20222779
Neppe, V. (2021, November 24). What is the best available evidence for the survival of human consciousness after permanent bodily death? *Bigelow Institute for Consciousness Studies*. Retrieved from https://www.bigelowinstitute.org/wp-content/uploads/2022/10/neppe-best-evidence-survival.pdf
Neyman, J., & Pearson, E. S. (1933). On the problem of the most efficient tests of statistical hypotheses. *Philosophical Transactions of the Royal Society A, 231*, 289–337. https://doi.org/10.1098/rsta.1933.0009
O'Hagan, A. (2019). Expert knowledge elicitation: Subjective but scientific. *American Statistician, 73*(sup1), 69–81. https://doi.org/10.1080/00031305.2018.1518265
Oppy, G. (2006). *Arguing about gods*. Cambridge University Press. https://doi.org/10.1017/CBO9780511498978
Pardo, M.S., & Allen, R.J. (2007). Juridical proof and the best explanation. *Law and Philosophy, 27*, 223–268.
Paterson, R.W.K. (1995). *Philosophy and the belief in a life after death*. Saint Martin's Press. https://doi.org/10.1057/9780230389885
Roche, W., & Sober, E. (2013). Explanatoriness is evidentially irrelevant, or inference to best explanation meets Bayesian confirmation theory. *Analysis, 73*, 659–668. https://doi.org/10.1093/analys/ant079
Roll, W. (2006). On apparitions and mediumship: An examination of the evidence that personal consciousness persists after death. In L. Storm & M.A. Thalbourne (Eds.), *The survival of human consciousness: Essays on the possibility of life after death* (pp. 142–173). McFarland.
Rouder, J. N., Speckman, P. L., Sun, D., Morey, R. D., & Iverson, G. (2009). Bayesian t tests for accepting and rejecting the null hypothesis. *Psychonomic Bulletin & Review, 16*(2), 225–237. https://doi.org/10.3758/PBR.16.2.225
Royall, R. (1997). *Statistical evidence: A likelihood paradigm*. Chapman and Hall.
Roush, S. (2005). *Tracking truth: Knowledge, evidence, and science*. Clarendon Press. https://doi.org/10.1093/0199274738.001.0001
Russo, R. (2020). *Statistics for the behavioural sciences: An introduction to frequentist and bayesian approaches* (2nd ed.). Routledge. https://doi.org/10.4324/9781315200415
Sage, M. (1904/2007). *Mrs. Piper and the society for psychical research* (N. Robertson, Trans.). Bibliobazaar.
Saltmarsh, H.F., & Soal, S.G. (1930). A method of estimating the supernormal content of mediumistic communications. *Proceedings of the Society for Psychical Research, 39*, 266–271.
Schiller, F. C. S. (1927). Some logical aspects of psychical research. In C. Murchison (Ed.), *The case for and against psychical research* (pp. 215–226). Clark University.
Schmeidler, G. (1977). Looking ahead: A method for research on survival. *Theta, 5*, 2–6.
Skyrms, B. (1966). *Choice and chance: An introduction to inductive logic*. Dickenson Publishing Company.
Sobel, J.H. (2004). *Logic and theism: Arguments for and against beliefs in god*. Cambridge University Press. https://doi.org/10.1017/CBO9780511497988
Sober, E. (2002). Bayesianism – Its scope and limits. In R. Swinburne (Ed.), *Bayes's theorem (Proceedings of the British Academy, 113)* (pp. 21–38). Oxford University Press.
Sober, E. (2008). *Evidence and evolution: The logic behind the science*. Cambridge University Press. https://doi.org/10.1017/CBO9780511806285
Sober, E. (2012). *Core questions in philosophy* (6th ed.). Pearson Education.
Sober, E. (2019). *The design argument*. Clarendon Press. https://doi.org/10.1017/9781108558068
Stevenson, I. (1969). Some implications of parapsychological research on survival after death. *Proceedings of the American Society for Psychical Research, 28*, 18–35.

Strnad, J. (2007). Should legal empiricists go Bayesian? *American Law and Economics Review, 9*(1), 195–304. https://doi.org/10.1093/aler/ahm007

Strong, J.W. (Ed.). (1992). *McCormick on evidence* (4th ed.). West Publishing Co.

Sudduth, M. (2009). Super-psi and the survivalist interpretation of mediumship. *Journal of Scientific Exploration, 23*(2), 167–193.

Sudduth, M. (2013a). A critical response to David Lund's argument for postmortem survival. *Journal of Scientific Exploration, 27*(2), 283–322.

Sudduth, M. (2013b). Is postmortem survival the best explanation of the data of mediumship? In A.J. Rock (Ed.), *The survival hypothesis: Essays on mediumship* (pp. 40–64). McFarland & Co.

Sudduth, M. (2014, January 19). Getting sober about survival (part 1 of 3) [Blog post]. *Cup of Nirvana*. Retrieved from http://michaelsudduth.com/getting-sober-about-survival-i/

Sudduth, M. (2016). *A philosophical critique of empirical arguments for post-mortem survival*. Palgrave Macmillan. https://doi.org/10.1057/9781137440945

Sudduth, M. (2021a). Review of the book *Signs of reincarnation: Exploring beliefs, cases, and theory* by J. Matlock. *Journal of Scientific Exploration, 35*, 183–208. https://doi.org/10.31275/20212089

Sudduth, M. (2021b). The James Leininger case re-examined. *Journal of Scientific Exploration, 35*, 933–1026. https://doi.org/10.31275/20212361

Sudduth, M. (2022a, January 17). Bruce Leininger's "Definitive" proof of reincarnation. *Cup of Nirvana*. Retrieved from http://michaelsudduth.com/bruce-leiningers-definitive-proof-reincarnation/

Sudduth, M. (2022b). Response to Jim Tucker. *Journal of Scientific Exploration, 36*(1), 91–99. https://doi.org/10.31275/20222515

Swinburne, R. (1973). *An introduction to confirmation theory*. Methuen.

Swinburne, R. (Ed.). (2002). *Bayes's theorem (Proceedings of the British Academy, 113)*. Oxford University Press.

Swinburne, R. (2001). *Epistemic justification*. Clarendon Press. https://doi.org/10.1093/0199243794.001.0001

Swinburne, R. (2004). *The existence of god* (2nd ed.). Oxford University Press.

Thomas, J.F. (1937). *Beyond normal cognition*. Boston Society for Psychical Research.

Tillers, P., & Green, E. (Eds.). (1988). *Probability and inference in the law of evidence: The uses and limits of Bayesianism*. Springer. https://doi.org/10.1007/978-94-009-2931-9

Tressoldi, P., Rock, A., Pederzoli, L., & Houran, J. (2022). The case for postmortem survival from the winners of the Bigelow Institute for Consciousness Studies essay contest: A level of evidence analysis. *Australian Journal of Parapsychology, 22*(1), 7–29. https://doi.org/10.31234/osf.io/pr364

van Fraassen, B.C. (1989). *Laws and symmetry*. Oxford University Press. https://doi.org/10.1093/0198248601.001.0001

Wagenmakers, E. J., Morey, R. D., & Lee, M. D. (2016). Bayesian benefits for the pragmatic researcher. *Current Directions in Psychological Science, 25*(3), 169–176. https://doi.org/10.1177/0963721416643289

Western, B., & Jackman, S. (1994). Bayesian inference for comparative research. *American Political Science Review, 88*(2), 412–423. https://doi.org/10.2307/2944713

Journal of Scientific Exploration
Anomalistics and Frontier Science

# Dominon Lost

## BOOK AND MULTIMEDIA REVIEW

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Rapuano, B. E. (2023). *Dominion lost*. Amazon.

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The author of this substantial book holds a BA in Neurobiology and Psychology from the University of Pennsylvania, a PhD in Neuropharmacology from the University of Connecticut, and a JD from New York Law School. Dr. Rapuano has conducted independent biomedical research as a cell biologist at several internationally recognized medical institutions and authored or co-authored over thirty scientific articles in peer-reviewed medical journals. His more notable published work includes an examination of the role of membrane phospholipids in nerve function, the skeletal effects of cancer metastases, metabolic bone diseases, and the coordinate surface electrical and biological properties of titanium alloy implant materials.

*Dominion Lost* details the author's narrative of his own alien abduction experiences. More than that, the text extrapolates from current scientific theories and understandings about how various reported aspects of the phenomenon—missing time, nasal implants, 'mind control' and the implantation of false memories, and the proven intergenerational nature of the phenomenon—might be explained by what we know from cutting-edge medical practice and current understandings of molecular biology.

The text runs to 14 chapters of varying length, most examining the alien abduction phenomenon from different perspectives or discoursing on the author's personal memories of his experiences. The reader quickly comes to realize that this author is no run-of-the-mill UFO enthusiast with a tenuous grasp of the STEM sciences: he's the 'real deal,' a serious, credentialed scientific professional, so a comparative rarity as a 'confessional abductee' writer in this field.

Early chapters recalling personal childhood abduction experiences alternate with those reporting historic waves of UFO sightings in the national press throughout the mid-20th century. Chapter Three, for example, details the infamous 1965 'northeast blackout' incident, discoursing on EMP and other documented electromagnetic phenomena reported concurrently with mass UFO sightings as the author speculates on how exactly these disruptive electromagnetic effects might have been initiated by the presence of UFO propulsion systems. It is here that the author, at an early point in the narrative, demonstrates his mastery of physics and is able to explain the technical complexity of a regional electrical grid system and exactly where its vulnerabilities to such unplanned-for, large-scale electromagnetic intrusions might be found.

The middle chapters of the book take a deep dive into the oft-reported 'alien nasal implant' procedures and their medical consequences, with the author referencing his own personal memories of having suffered these from childhood. Some of the book's very few illustrations are presented on pp.146-148, where anatomical diagrams illustrate how the ostia of the sphenoidal sinuses may be accessed by human practitioners in the medical disciplines of otolaryngology and ophthalmology when treating cavern-

ous sinus thrombosis (CST). From abductee testimony (Betty Andreasson and others), Dr. Rapuano speculates that the abductors may utilize this method of access and, by so doing, demonstrate their deep knowledge of human anatomy and high level of surgical competence with these procedures.

(Of special note here: attention should be paid to the obvious truth that all these reportedly super-fine instruments deployed to access areas such as the sphenoidal sinus ostia and the cribriform plate of the ethmoid bone have obviously been manufactured precisely and exclusively for use on human anatomy, so presenting further evidence that the serial abduction of human subjects by the alien abductors is indeed likely to be a pre-planned program characterized by very precise and detailed preparation in the design and manufacture of such instrumentation.)

Chapter Ten is titled *Alien Use of Implants and Related Technologies to Study Human Brain Function,* and Chapter Eleven *Alien Control of the Mind and Human Society.* Here the author really gets into his stride as he utilizes his professional and vocational expertise to discuss convolutional neural networks in the context of the 'staring procedures' so frequently reported by abductees and the various ways these might be utilized to control human brain functions. This long section might be a struggle for some readers unfamiliar with the development of brain-computer interface devices and their application to cognitive mapping in memory studies. This reviewer certainly struggled with this section, though doubtless many SSC members will possess the appropriate medical and scientific skillset to fully engage with Dr. Rapuano's expertise in these highly specialist areas.

The penultimate chapters explore zero-point energy and quantum entanglement and how cosmic portals, wormholes, and 'gravity drive' might work in an attempt to address the question, 'How do they get here?' (To this reviewer, the evidence that they are obviously 'here' means that 'how do they get here?' is the wrong question – for the moment, anyway. They obviously got here somehow, so the pertinent questions should be 'what are they doing here, and what do they want?').

In his final numbered Chapter (the fourteenth), Dr. Rapuano returns to recount his own visceral experiences and so brings the reader back to 'what started it all', rounding off the narrative.

An *Afterword* discusses the Pentagon's Office of the Director of National Intelligence's January 2023-released study of Unidentified Aerial Phenomena, which admits that 171 separate encounters were reported by Air Force and Navy pilots in recent years with UAPs which demonstrated flight characteristics outside any known - or planned - terrestrial aviation technologies. The author then emphasizes the *physical* nature of the abductors and speculates on the bipedal humanoid alien morphology always described, asking the question: 'Why do these aliens always look like us?' In attempting to answer this question, the author speculates that the ubiquitously reported 'grey aliens' may well have been genetically engineered from homo sapiens. The biological science and currently existing genetic technologies (in the human world) that might help understand how this was carried out are explored – though he doesn't speculate on *who* might have done this or *why*.

The concluding paragraphs speculate on the ultimate intentions of the abductors and express a rather bleak perspective which does not differ radically from my own, less scientifically informed 2022 treatise, *Out of Time: The Intergenerational Abduction Program Explored.*

The hardcover volume of *Dominion Lost* is a weighty tome: 11.2 inches/28.4cm x 8.5 inches/21.4 cm, weight = 2.73 pounds, with large-print double-line-spaced text running to 449 pages printed on what appears to be >100gsm quality paper, so you're going to need one-and-a-half inches of shelf space to accommodate it and take care you never inadvertently drop it on your foot. The copyright page displays a single ISBN for the hardcover, but the book is also available in paperback and eBook formats and in an 'abridged' version. Printing, and presumably distribution, are by Amazon - the hardcover binding is Amazon's usual case laminate offering rather than the (more 'classy') cloth-bound hardback with a separate detachable dust jacket, which Amazon does not offer - and the author's contact email address is printed on the copyright page.

Observations on some content and stylistic aspects of *Dominion Lost* follow below.

1. The author seems not to understand the difference between a *Foreword* (which is always a written recommendation by *someone other than the author*) and an *Introduction*, which should always be *written by* the author; ditto an *Afterword* vs an *Epilogue*. This error speaks merely of inexperience with bibliographic conventions when publishing this type of book designed for a general readership.
2. Dr. Rapuano chooses to list numbered references directly after each chapter, rather than the more usual practice of listing the notes and references in a single section in the final pages of the book. This works well, as the relevant references are easy to find when reading each chapter. Many chapters contain upwards of one hundred references to published and peer-re-

viewed scientific papers - for example, Chapter Nine alone has 160 separate detailed references spanning 13 pages.

3. The large print and double-line spacing of text may be more common in scientific or medical textbooks but is unusual in a work of this kind. However, this decision makes the book easy to read, even if it makes it heavier to hold due to the resulting 449-page count and >100gsm paper stock.
4. The author cites the published work of each of the more authoritative and well-known abduction researchers, such as Budd Hopkins, Dr. David Jacobs, Dr. John Mack, and others, literally hundreds of times.
5. The author finds *The Day After Roswell* by the late Lt. Col. Philip Corso an entirely credible work and similarly cites this book hundreds of times in the text. Not all readers are likely to share his unqualified enthusiasm for the veracity of Corso's tale.
6. Ditto the claims of Bob Lazar, but here he might be admittedly standing on firmer ground.
7. Editing is good but not exemplary, and occasional minor grammatical errors may be found in the text: I counted around 20 obvious ones, but this is no big deal in a book of this length and serious ambition.

Despite these minor gripes, I would strongly endorse this work and recommend it unreservedly to the membership of the SSE. This recommendation would be particularly strengthened for any potential reader professionally qualified or accredited in the STEM disciplines, especially those in microbiology, the medical sciences, or (conversely) astrophysics. I would, however, caution those less qualified or experienced in these disciplines that they may find at least parts of *Dominion Lost* a challenging read.

Journal of Scientific Exploration
Anomalistics and Frontier Science

BOOK AND MULTIMEDIA REVIEW

# American Vikings: How the Norse Sailed Into the Lands and Imaginations of America

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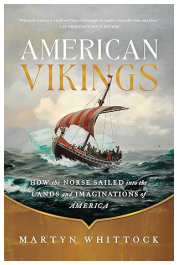


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Among the Norse Scandinavians of earlier medieval times were maritime raiders known as "Vikings." However, although originally descriptive of only a minority of the Nordic populace, the term "Viking" has, in modern times, vernacularly become a designation for the Norse in general. Author Whittock, a prolific British independent popular historian and theologian, especially of the Middle Ages, adopts this popular nomenclature. I will follow the more scholarly usage.

Whittock's book has three aims: to synopsize the ascertainable reality of eleventh-century and almost certainly later Norse activity in North America (see also, Enterline 1972, 2002, neither cited), to limn the notion of Vikings in America in popular and political culture, and to identify the differences between reality and myth—and even fraud.

The author notes the high degree of mobility of Norse traders and raiders, their purview extending from at least Atlantic-coastal Canada to the Caspian Sea, and including Baghdad in Iraq. During the ninth century, substantial numbers of settlers left Norway in favor of Iceland, to escape the domination of King Harald Finehair (Fairhair). Further influx to Iceland occurred when the Irish reconquered the previously Norse-occupied Dublin area; Irish females were in the majority in early Iceland (Irish monks already on the island seem to have fled when the fierce Norse first arrived). From Iceland, certain Norse individuals and some Irish ones among them went on to settle in southern Greenland (geographically, part of North America), whence some, under Leifr Eiríksson (Leif Erikson), ultimately traveled farther westward to Canada (pp. 27–30, 37). For about 300 years, the Medieval Warming Period diminished the extent of storm activity and pack-ice in the North Atlantic, creating relatively favorable climatic conditions for exploration and trade (p. 39).

We moderns first knew of the circa-A.D. 1000 Norse presence in Canada from two Icelandic sagas and spotty other medieval records; Whittock accepts the basic accuracy of the originally oral sagas, which were written down during the later Middle Ages. He also rightly notes that the visits recorded in these accounts may not have been the earliest actually undertaken to America on the part of Norsemen.

Beginning in 1960, the Scandian presence in the New World was finally amply verified materially by the excavation of the Norse L'Anse aux Meadows site at the northern end of Newfoundland's Great Northern Peninsula. There, archaeologists found remains of four Icelandic-style longhouses and appurtenant structures, including an occasionally used iron smithy (Native North Americans did not smelt metals). Telling artifacts were also unearthed. Of 11 red-jasper fire-strikers found, only two were from Newfoundland sources; the others were of materials originating in Greenland and Iceland. As Whittock does not mention, a pig-bone fragment was also discovered there (Ingstad

& Ingstad, 2001, p. 147).

A boat plank at L'Anse had pegs of Scotch pine, common in Norway but not native to the New World. Also recovered were a Scandinavian-style pin, a fragment of a gilded ring, a whetstone, a spindle-whorl, possible stone loom weights, and weaving tools (the natives did not weave; pp. 61–74; but see below). So, the site is clearly Norse; whether it is one actually mentioned in the Icelandic sagas (e.g., Straumfjordr) is ambiguous (it was probably *not* Leifsbudir and certainly not Hóp; the former may have been on southernmost New Brunswick's Passamaquoddy Bay).

The relative paucity of artifacts at the site suggests a total time of occupance of only a decade or so, and the conclusion has been that the place served not as a "permanent" colony but as a sporadically utilized base or staging site for exploration and resource-procurement farther to the west and south. The dwellings at L'Anse had a combined capacity of 70 to 90 individuals, so it seems that a relatively huge workforce was present, at least at the outset; the entire Norse population of Greenland had reached only some 400 souls. Accordingly, the place must have been perceived as highly important (pp. 66–67). It would seem that any alleged association with Leifr himself is dubious, however, because the latter's only known visit to the region occurred two decades earlier than L'Anse's inception (Jett, 2000).

The original radiocarbon dates taken at the site spanned a significant period and, accordingly, were not at all exact. Whittock recognizes this and summarizes the very latest dating, based on tree-rings of human-worked local wood scraps. All three that were tested displayed cutting dates of A.D. 1021, thereby establishing the time of the inception of construction there (pp. 70–72). (Note that the *Annals of the Kings of Iceland* declare that "in 1021 Bishop Erik [Gnupson] of Greenland went to look for *Vinland*" [p. 56]).

Old maps associate the location with "Vínland," whose name has always been thought to derive from the discovery of an abundance of wild (fox) grapes.[1] However, since such grapes' range does not currently extend northward to near L'Anse, the Gulf of St Lawrence's shores are concluded to be the probable heart of Vínland. L'Anse did yield specimens of American basswood and butternut, trees whose contemporary ranges run no farther northward than New Brunswick (p. 68).

New World products procured for export probably included fur, grapes, "walnuts" (apparently, "white walnuts," i.e., butternuts), and, particularly importantly, timber (Gudmundsdóttir, 2023). In treeless Greenland, chests made of wood of the American larch (tamarack) are known, as are ship parts made from American larch and spruce (pp. 67–68); many house beams of elites—since, rotted away—no doubt came from the western continent. Bog iron was likely also an important North American product.

Accepting McCrone's early (but dubious) findings, Whittock (very probably correctly) labels Yale's Vinland Map a fake but fails to cite the book that makes that almost certain (Floyd, 2018).

Although the Norse (including Leif) are the best-known of proposed pre-Columbian European visitors to the New World, there are a few additional individual candidates; Whittock provides modest discussions of three. The most plausible, he declares, is that of the Irish anchorite monk (St.) Brendan of Clonfert, of Galway, plus his crew, during the sixth century. The surviving accounts of his voyages are detailed, and their geography seems to match real places such as the Faroe Islands, Rockall, and Iceland; Brendan is also said to have reached the "The Promised Land for Saints," a lush country that some have supposed to have been North America. Tim Severin's 1976–1977 experimental "Brendan voyage" in a replica skin-covered curragh demonstrated the feasibility of a transatlantic traverse in the kind of craft attributed to Brendan (pp. 76–82).

The Welsh (bastard) prince Madoc ap Owain Gwynedd is also alleged to have sailed to America in, A.D. 1170. Whittock finds no reason to believe that the voyage described in the old literature has any correspondence with historical reality. He dismisses the many (and probably at least mostly fanciful) accounts of Welsh-speaking American Indians (pp. 82–89). He does not cite Richard Deacon's 1966 *Madoc and the Discovery of America*.

Henry I Sinclair (Saint Clair), Earl of Orkney, has been contended also to have voyaged to coastal Canada and New England, circa-1380. "This claim has no basis in fact," says Whittock (p. 89). The notion rests on the allegedly baseless identification of Sinclair with a "Prince Zichmni" mentioned in a 1588 publication by the Venetian Zeno family, founded on alleged letters of about 1480 that describe a traverse by two of the family's forebears, the brothers Nicolò and Antonio. In any case, "the claim is clearly a hoax by the Zeno brothers or their publishers," since the record shows that the Zenos were in Venice at the time of the alleged ocean traverse and since mainland North America is not mentioned in the letters. "Even the original hoax makes no such claim. It clearly states that Zichmni landed in Greenland (or *Engrouelandia*). It was later . . . writers who tried to make a connection with the New World" (p. 90). Here, Whittock relies on Websites and the online *Dictionary of Canadian Biography*. He does not cite the extensive (and, admittedly, sometimes fantastic) print literature on the topic. Most notable (and

quite sober) is de Robilant 2011, which reports, among other things, that at least Nicolò was in reality *not* in Venice at the critical time, and that philological studies have shown much of the relevant text to be, in fact medieval (p. 194; see also, Enterline, 2002, pp. 277–280). Since the book presently under review is largely about Vikings, I do not further elaborate here.

In Chapter 7, Whittock switches from a consideration of attested Norse history in the New World to the topic of the ascendance over time of Vikings in the United States national-origin mythology. Although Cristoforo Columbo (Christopher Columbus) never set foot on the North American mainland to the north of Central America and slaughtered the indigenous Taínos of Hispaniola, he had been adopted as a U.S. hero over the Italian voyager Giovanni Caboto (John Cabot), official discoverer of the northern continent in 1497. Cabot had suffered in the post-Revolutionary U.S. psyche from his association with the rejected British crown. With the latter nineteenth-century influx of hoards of Catholic Italian immigrants, perceptions of Italian (?) Columbus's worthiness became diminished among the majority Anglo-Saxon Protestant Americans, and a second origin-story, that of Massachusetts's 1620 *Mayflower* "pilgrims" who fled religious persecution in England, gained increased luster; from 1816 onward, the celebration of Thanksgiving gained ever more ground as a holiday nationwide. The ordinary-family-farmer nature of the Massachusetts Bay Colony carried more appeal than did the earlier 1607 settlement of entrepreneurs representing the commencement at Jamestown, VA, of an influx of mostly male tobacco-plantation-founding "Cavaliers."

Meanwhile, in 1837, the Danish historian Carl Christian Rafn published Danish and Latin translations of the Icelandic Vínland sagas, with an English summary and "claimed to have identified Viking-Age artifacts along America's eastern seaboard. . . (pp. 100–101). This furthered a "Viking revival" in the U.S., which was added to by 1874's *America Not Discovered by Columbus*, by Rasmus Bjørn Anderson, Professor of Scandinavian Studies at the University of Wisconsin, which contemplated repeated Norse visits to New England from the tenth through the fourteenth century. This book had considerable popularity and impact in the country (pp. 100–102; in 1890, Middleton Reeves translated and edited the sagas for an English-speaking audience, countering some of Rafn's overenthusiastic speculations). Even though some of the Norse had been pagans and the rest Catholic, this was mitigated in the WASP public mind by the fact that these folks' descendants had become Protestants. Too, these explorers had been Germanic Northwest Europeans as had the Anglo-Saxons, and their individualistic freedom-loving restlessness, bravery, optimism, and ambition were sometimes seen as personifying American cultural values; in the consciousnesses of many, Leif and Vikinghood were certainly preferable as icons to the looked-down-upon Southern European Catholics represented by Columbus and Cabot (and Amerigo Vespucci; pp. 102–105)—never mind that the Vikings had been cruel and rapacious pillagers, slavers, murderers, torchers, and torturers.

These pro-Viking notions resonated particularly with Scandinavian-Americans of the Upper Great Lakes region. Columbus's slippage in public sentiment helped to set the stage for the rise of awareness of Leif Erikson and the *pre*-Columbian Norse "discovery." In 1893, the World's Columbian Exposition was held in Chicago. Norway's Capt. Magnus Andersen provocatively sailed *Viking*, a replica of the ninth-century Norse Gokstad ship, across the Atlantic and on up the Hudson River, the Erie Canal, and the Great Lakes to the Exposition named for Leif's cultural rival (p. 120).

In light of the fact that subsequent claims concerning a Norse presence included surprising areas deep in the interior of the U.S., Whittock makes a stab at defining what we can say for certain about their real rovings in the continent. Although they were certainly capable of penetrating deeply into interiors using the rivers, says the author, we don't have firm evidence that they did. We do know, he asserts, that they must have operated to as far southward as New Brunswick in order to have encountered grapes and butternuts (he does not consider the possibility that these plants' ranges extended farther northward during the three-centuries-long Medieval Warming Period that he mentions).

The writer then turns to potential signs of Norse activity beyond historic Vínland, starting with the Kensington Runestone, unearthed by a farmer in 1898 in the roots of an aspen near an eponymous town in Minnesota (note that, at this writing, the *Wikipedia.com* entry "Kensington Runestone" is helpful in drawing upon Scandinavian-language sources).[2] The "KR" is a slab of greywacke largely covered with a runic inscription describing a Norse exploration party that had been attacked by hostile natives in the year 1362. Immediately upon the stone's (ostensible) discovery (and enduring to the present), debate as to its authenticity arose. Looking briefly at the historical, linguistic, and circumstantial pros and cons as he knew them, Whittock wrote, "While the jury is still out regarding the final verdict on the Kensington Runestone, the overall view among most archaeologists and historians is that it is probably a fake," and the writer admits to being "very skeptical," as well (pp. 121, 144, also, 166; Whittock cites and has contributed to *The Skeptic Encyclopedia of*

*Pseudoscience*, Shermer 2002—rendered "Sherman" in the book being reviewed). In his work on the stone, the author depends quite a bit on *Hoax Springs Eternal: The Psychology of Cognitive Deception* (Hancock, 2015), which carries a long, skeptical chapter on the Runestone. One may note that *Wikipedia* (accessed 5 Dec. 2023) labels the object as nineteenth-century in manufacture and points to a local contemporary of Öhman's having possessed a Futhark (alphabet) of runes said to resemble the odd ones of the stele.

Since this object is one of such central potential importance to Euroamerican history and to the subject of the author's book, and although he has admirably unearthed a number of obscure relevant references during his research, it seems strange that he neglects to cite much at all the numerous (perhaps, overwhelming number of) relevant books out there and depends largely on unrefereed Websites for his information and argument—reflecting a troubling trend of our times. Most particularly, he (like *Wikipedia.com*) has ignored the seminal efforts of the late Danish-American engineer Richard Nielsen (1933–2016), who obtained his Ph.D. in ship structures in Denmark (disclosure: I provided some editorial assistance to Nielsen at an early stage of his investigation and followed his research all along; I possess his last, never-published manuscript).

Nielsen, beginning with the premise that the object looks like a genuine Norse document, for decades studied the inscription's purportedly anachronistic runes, foreign usages, lexical issues, and so forth that are still almost universally accepted as belying the stone's bona fides. Nielsen eventually found that essentially all of these "anomalies" *are*, in fact, attested in the runic writings of the period in question, permitting the carvers to be traced to Sweden's Gottland. The fact that he discovered much that was unknown to experts of the time of the accused perpetrator (Swedish-immigrant farmer Olof Öhman) demonstrates the *genuineness* of the inscription, since any faker would have been ignorant of these usages as well. Too, the stone's dialect was not that of Öhman or his wife.

Nielsen and the forensic petrographer Scott Fred Wolter (2006) conducted a microscopic study and other tests on the stone, which revealed that the "too-fresh-looking" runes had, following discovery, been scraped out with a steel nail to enhance visibility but that vestiges of considerable age-patination still survived here and there in the grooves. The pair also did historical research on the circumstances of the find and did not conclude for any dishonesty.

I examined the object (and the find site) in 2021 and can attest that what appear to be root marks on the slab are actually present. My overall conclusion has become that the Kensington stone is an authentic Norse object, manufactured on-site but ultimately overtaken by the growth of the tree and hidden from view until Öhman felled the aspen while clearing land near his house.

Whittock stresses what could be called the "Viking-nationalist" tenor of the time and region, which may help account for the small stir that the find first precipitated and the enthusiasm with which it came to be embraced in modern Minnesota and beyond but which hardly demonstrates fraud. When plausible circumstantial context and hard evidence are in conflict, the hard evidence must prevail (actually, the *local* circumstantial evidence supports a lack of fraud). Too, as Whittock chose not to mention, some of many's *dismissal*s of authenticity could have come from loyalty to Columbus as the discoverer.

Nielsen's published work, which commenced in the mid-1980s, was mentioned in the semipopular literature at least as early as 1992 (Huyghe, 1992, pp. 158–159, 247; also, Nielsestuen, 1994, Ch. 4). The prominent anthropological archaeologist Alice Beck Kehoe (2005), acquainted with Nielsen, drew upon his and Wolter's work in preparing her small but authoritative and synthetical book *The Kensington Runestone: Approaching a Research Question Holistically* (although Nielsen and Wolter's magnum opus was not released until a year later than Kehoe's, in 2006). Kehoe's volume (from a mainline press) objectively considers the question, scientifically and historically, from all sides and as a whole—including, uniquely, the fourteenth-century Scandinavian context—and concludes for genuineness (p. 86). Whittock cites this work once (p. 235), but—very oddly, indeed—not in connection with the Kensington stone, Kehoe's topic. The Nielsen and Wolter book *was* issued by an obscure press, but Internet searching could have called it up, and it is available on *Amazon.com*.; in any case, it and other Nielsen titles are cited in Hancock (2015), which Whittock draws upon. Also earlier available were plural serious books arguing for authenticity (e.g., Hall, 1995; Nilsestuen, 1994), most importantly chemist Barry Hanson's 2002 pregnant self-published two-volume *Kensington Runestone: A Defense of Olof Ohman the Accused Forger* (listed in *WorldCat* and *Amazon.com*). One hesitates to conclude that Whittock has shied away from sources that did not contribute to his theory that the era's cultural-context led to all sorts of hoaxes, hallucinations, half-baked hypotheses, and ethnic hype—including, most likely he thinks, in the form of the Kensington stone. One may note, however, that his context chapter follows consideration of the accepted L'Anse site but *precedes* the discussion of the non-L'Anse purported evidences of a Norse presence, all of which he ends up rejecting as showing that "the Norse were here."

Whittock does recognize the genuineness and implications of Norse-related finds in indigenous sites on some of Canada's eastern-Arctic islands across Baffin Bay from Greenland. In Native sites on Ellesmere Island have been found Scandinavian-style cloth, bits of mail, fragments of iron and copper, and part of a bronze balance. These date to as early as the twelfth century (p. 124). Devon Island has yielded part of a cast-iron bowl and some smelted iron, from the fifteenth century (p. 125). Baffin Island has produced a small twelfth–thirteenth-century wooden figurine of a seeming Norse cleric, and comparable figurines, of a century later, come from Greenland's little Kingiktorsaug (sic; Kingittosuaq) Island, on whose summit a perhaps-early-fourteenth-century runestone was also found, in 1824; six undeciphered runes follow the main text—whose stated date is ambiguous.

Spun cordage and other artifacts have been found both on Baffin Island and in northern Labrador (pp. 125–126). These were all attributed to the Norse, as Whittock observes. However, recently some of the cordage and cloth has been dated to around the time of Christ, thus substantially prior to any acknowledged Scandinavian presence in the region (but see Peterborough, below), which has caused some scholars to attribute independent invention of textile technology to the Native population (Hayeur, Smith, Smith, & Nilsen 2018). This strikes me as very implausible in light of its complete absence in historic times; it most likely speaks to earlier European (and not necessarily Norse) contacts, from established weaving cultures.

Whittock's survey of other claimed Norse objects and inscriptions in America is unusually broad. He first tackles those in the continent's northeast: "Do they *really* constitute evidence of North American Vikings moving down the eastern coast of North America? Or do they tell us more about the grip of Vikings on later imaginations?" (p. 127).

Yarmouth Rock in Nova Scotia is a quartzite slab that came to the fore in 1812. It carries 13 markings that may be runes. We may never know for certain, because, seemingly, someone "improved" the characters with a hammer and chisel, and before-and-after "translations" differ. Still, points out Whittock, the site is not all that far from Newfoundland (pp. 127–132).

Three portable stones with runic inscription and an etched map were reported from Spirit Pond in Maine, in 1971. The map shows "Vínland" and "Hóp," toponyms found in the sagas. Whereas the (largely amateur) New England Antiquities Research Association (NEARA) has favored authenticity, Whittock says "hoax" (pp. 130–132). I do not at this time have a firm opinion but do harbor some reservations.

Rafn identified what he saw as a Norse text on Massachusetts's petroglyph-covered tidal Deighton Rock (pp. 133–135). However, although there are a handful of letter-like characters among many other marks, they are scattered and not explicitly rune-like, and nothing suggests a true text to me.

The poet Longfellow romanticized Rhode Island's circular Newport Tower (which I have inspected) as a Norse-associated structure, but in his 1677 will, land-owner Benedict Arnold, Sr., referred to it as "my stone built Windmill," and Whittock accepts it as such and therefore as Colonial. Although he cites NEARA in connection with Spirit Pond, he does not cite NEARA's extensive studies of the Tower, which include the observation that the erection is of a European style earlier than the seventeenth century, that it was seemingly mentioned in a document of 1630, that the site is shown as Norman Villa or Tolovilla on sixteenth-century maps, and that the structure displays astronomical alignments (Carlson & Dranchak, 2006; see also, Nilsestuen, 1994, Ch. 11). It would be problematic to operate as a windmill as built; its top is not a perfect circle and the building contains a fireplace. On the other hand, a fireplace would have been a rarity in the 1300s, the building's proposed time of erection. Mortared stone masonry—especially, involving arches—is not attributable to Native New Englanders.

Then, there is the Narragansett Runestone in Rhode Island, consisting of eight runes on one line and two on a second line; three characters are unclear, possibly owing to weathering in the rock's original low-tidal location. Attempted translations differ; Whittock, who believes the inscription likely to be modern, scoffs at Sue Carlson's interpretation as *skraumli* 'screaming river' but notes that one gloss of the term *Skraeling* for an indigenous person is 'screamer'. Whittock does look more tolerantly on this and the Yarmouth stone than he does on the distant Kensington stone.

Whittock considers the genuine 1080 Norse penny that avocational archaeologist Guy Mellgren in 1957 (initially, privately) reported from the Native American Goddard site (A.D. 1086–1235) near coastal Brooklin, ME, probably a true archaeological find but likely traded southward to this locale, which appears to have been a hub of indigenous long-distance exchange (I have been told that Mellgren did not publicize the discovery during his lifetime because he feared being accused of having salted the site; note his Swedish surname).

Not mentioned are the bedrock inscriptions at Canada's Peterborough, Ont. Some of these have, controversially, been attributed to Scandinavian-speakers of an age long prior to the Viking Age (Fell, 1980; Kelley, 1998; Vastokas, 2004).

Chapter 10 treats "Viking" objects and texts that "are clearly, forgeries and hoaxes; or possibly Native American monuments which have been culturally highjacked in the search for evidence of Vikings." This, he says, "reveals penetration of the minds and imaginations of later Americans." Relevant circumstances include increased consciousness of the Icelandic sagas, particularly after 1850. "Heightened awareness led to a search for corroborative evidence. . . ." (pp. 144–145). This and the Kensington stone should also be seen in the context of Scandinavian immigrants to the Middle West, who sought to legitimize their land claims, he continues. "This explains several 'Viking' finds that were later made there" (p. 147). Further, these claims incentivized the reimagining of any sophisticated "monuments" as having been authored not by Native Americans but, instead, by Northern Europeans—and Scandinavians, at that (pp 145–146, 159–160). One must ask, is Whittock really saying here that items like the Kensington runestone were actually American Indian-made? Probably not; he seems to have architectural monuments in mind. However, other than l'Anse and the Newport Tower, no pre-Columbian American architectural works have ever been attributed to the Norse.

Returning to the "hoaxes," although he refers to the AVM Stone near Kensington as clearly a prima-facie fake, he seems not to have noticed that in 2001, some UM graduate students actually confessed to having created it in 1985 (Kehoe, 2005, p. 14; Powell, 2002). Note that although some have textual and/or runic issues that inspire dubiety, the AVM is the only U.S. runic rock inscription that is demonstrably fake on the basis of science, witness, or confession; no faker has ever legitimately been identified.

Whittock does show that earlier-alleged Norse "mooring stones" in the area were not such.

In eastern Oklahoma are the Heavener Runestone, a cliff carving first noticed in 1923. The mixture of runes is wrong, he says, and "We may safely conclude that it is a modern fake" (p. 153); he does not mention the hypothesis that it was created by a nineteenth-century Norwegian farmer as a boundary marker rather than as a fraud. The portable Poteau Stone was discovered by children in 1967; for Whittock, it falls into the same category as the Heavener (pronounced "*Hee*vuhner"), which I have also visited in the field. Children also found the Shawnee Stone, which is of a kind. In his discussions, Whittock fails to speak of the local dynamo behind consideration, Gloria Farley (see her 1994 book, listed in *WorldCat* and *Amazon.com*).

The writer refers to what he terms West Viginia's "Braxton County Runestone" and "Grave Creek Runestone" (pp. 155–157); why, I am not sure, since the writing on these look Semitic rather than runic and I have never before heard of them being called "runestones."

Altogether, Whittock provides a generally reasonably comprehensive, cogent, and up-to-date—if conservative and incomplete—sketch of the Norse expansion into North America a thousand years ago, stressing Newfoundland's Norse L'Anse aux Meadows site but also looking at a number of lesser-known and less-unambiguous Norse objects/inscriptions as well as lesser-known proposals for non-Norse medieval transatlantic European contacts. He does lean heavily on Websites and secondary sources. There are some repetitions and a few minor inconsistencies through the text, as though the book was put together over a considerable period of time. More seriously, the treatment of the Kensington Runestone is seriously deficient in not reporting on Nielsen's crucial findings (and linguist Robert Hall's before him) and on Kehoe's exemplary comprehensive synthesis of not just the stone but of the entire Norse-in-America picture.

The author's interest in this book is focused not only on true Norse history, however, but—in something less than half of the book—also on the cultural embrace, popularization, transmogrification, and "weaponizing" of cultural symbols like Leif Erikson and Vikings in general for contemporary ethnosociopolitical, entertainment, and economic purposes. Intrinsically intriguing though it may be, I do not here review Whittock's observations on the contemporary cultural appropriations of Viking character and history other than to mention that he examines topics such as Vikings as represented in comic books, the cinema, and television, as well as as an icon of QAnon-influenced White-supremacy culture as manifested during the January 6, 2021, attack on the U.S. Capitol building. He also reviews Vikings as a theme in merchandising. I take note of books displaying similar themes but preceding Whittock's that he does not draw upon (e.g., Herman, 2022; Krueger, 2015; Machan & Helgason 2020).

A virtue of the book is the writer's understanding of the complexity of history and that cascades of consequences can be set off by any event (pp. 33–34).[3]

## ENDNOTES

1 Alice Kehoe (personal communication) has forwarded the idea that "Vínland" is the Old Norse form of the Latinized Gaulish Vindolanda 'White Field', a possible reference to the "white beaches" of Labrador.

2 Note that the stone's current home, the Runestone Museum in Alexandria, MN, holds a whetstone whose label indicates that it was unearthed just beneath where the runestone had been extracted, and a short while later.

[3] This review will also appear in *Pre-Columbiana: A Journal of Long-Distance Contacts*.

## REFERENCES


Deacon, R. (1966). *Madoc and the discovery of America: Some new light on an old controversy*. George Braziller.

di Robilant, A. (2011). *Irresistible north: From Venice to Greenland on the trail of the Zeno brothers*. Alfred A. Knopf.

Carlson, S. (Comp.), & Dranchak, J. (Ed.). (2006). *The Newport Tower Arnold to Zeno*. New England Antiquities Research Association.

Enterline, J. R. (n.d.). *Viking America*. Doubleday.

Enterline, J. R. (2002). *Erikson, Eskimos & Columbus: Medieval European knowledge of America*. Johns Hopkins University Press.

Farley, G. (1994). *In plain sight: Old World records in ancient America*. ISAAC Press. (Reprinted in 2007 by Gloria Farley Publications, Golden, CO).

Fell, B. (1980). *Saga America*. Quadrangle/New York Times Book Co.

Floyd, J. P. (2018). *A sorry saga: Theft, forgery, scholarship . . . and the Vinland map*. Independently published.

Gudmundsdóttir, L. (2023). Timber imports to Norse Greenland: Lifeline or luxury? *Nature: International Journal of Science, 97*(392), 162–174. https://doi.org/10.15184/aqy.2023.13

Hall, R. A., Jr. (1995). *The Kensington Rune-Stone: Authentic and important, a critical edition*. Edward Sapir Monograph Series in Language, Culture, and Cognition 19. Jupiter Press.

Hancock, P. (2015). *Hoax springs eternal: The psychology of cognitive deception*. Cambridge University Press. https://doi.org/10.1017/CBO9781107785311

Hanson, B. J. (2002). *Kensington Runestone: A defense of Olof Ohman the accused forger* (Vols. 1–2). Archaeology ITM.

Hayeur, S. M., Smith, K. P., & Nilsen, G. (2018). Dorset, Norse, or Thule? Technological transfers, marine mammal contamination, and AMS dating of spun yarn and textiles from the Eastern Canadian Arctic. *Journal of Archaeological Science, 96*, 162–174. https://doi.org/10.1016/j.jas.2018.06.005

Herman, A. (2022). *The Viking heart: How Scandinavians conquered the world*. Houghton Mifflin Harcourt.

Huyghe, P. (1992). *Columbus was last: From 200,000 B.C. to 1492, a heretical history of who was first*. Hyperion.

Ingstad, H., & Ingstad, A. S. (2001). *The Viking discovery of America: The excavation of a Norse settlement in L'Anse aux Meadows, Newfoundland* (E. S. Seeberg, Trans.). Checkmark Books.

Jett, S. C. (2000). The Norse in the North Atlantic: An overview. *Pre-Columbiana: A Journal of Long-Distance Contacts, 2*(1), 2–21.

Kehoe, A. B. (2005). *The Kensington Runestone: Approaching a research question holistically*. Waveland Press. https://doi.org/10.1525/ac.2006.17.2.106

Kelley, D. H. (1998). Writing in the Americas. *Journal of the Southwest, 37*(4), 25–30.

Krueger, D. M. (2015). *Myths of the Rune Stone: Viking martyrs and the birthplace of America*. University of Minnesota Press. https://doi.org/10.5749/minnesota/9780816696918.001.0001

Machan, T. W., & Helgason, J. K. (Eds.). (2020). *From Iceland to the Americas: Vinland and historical imagination*. Manchester University Press. https://doi.org/10.7765/9781526128768

Nielsen, R., & Wolter, S. F. (2006). *The Kensington Rune Stone: Compelling new evidence*. Lake Superior Agate Pub.

Nilsestuen, R. M. (1994). *The Kensington Runestone vindicated*. University Press of America.

Powell, E. A. (2002). Runestone fakery. *Archaeology, 55*(1), 9.

Reeves, M. (Ed. & Trans.). (1890). *The finding of Wineland the Good: The history of the Icelandic discovery of America*. Henry Frowde.

Shermer, M. (Ed.). (2002). *The skeptic encyclopedia of pseudoscience*. ABC Clio. https://doi.org/10.5040/9798216015123

Vastokas, J. M. (2004). The Peterborough Petroglyphs: Native or Norse? In C. Diaz-Granados & J. R. Duncan (Eds.), *The rock art of eastern North America: A new world view* (pp. 277–289). The University of Alabama Press.

Journal of Scientific Exploration
Anomalistics and Frontier Science

BOOK AND MULTIMEDIA REVIEW

# Are Quanta Real?: A Galilean Dialogue

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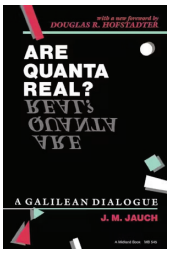


Jauch, J. M. (1990). *Are quanta real?: A galilean dialogue*. Indiana University Press.

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The subtitle of the book is "A Galilean Dialogue" and is based on Galileo's "Dialogue Concerning the Two Chief World Systems," written circa 1632 AD. In it, three imaginary characters, *Simplicio, Sagredo,* and *Salvietti,* are having a profound conversation about the nature of reality and how it is perceived via experiments and meaningfully discussed via the theories of the day. Their dialogue was divided into four days; each day addressed a different area of concern: Was the sun or the earth at the center of the universe? This was Galileo's final book and was a scientific testament covering what later became classical physics (the only physics of his time). Jauch brings the characters forward in time to circa 1970.

Their up-to-date conversations take place again over four days, with each day becoming more and more difficult to comprehend than the previous day's contemplation. *Are Quanta Real?* explores the "new" physics known by 1970, namely quantum physics, and raises questions which were profound then and even today remain (for some scientists) not sufficiently well-answered—perhaps even mysterious.

To tell this story basing it on our present-day (circa 2024) understanding of quantum physics will require me to add another fictitious character—a time traveler from the present time—who goes back to the Fall of 1970 to the same villa situated on the shores of Lake Geneva, Switzerland and meets with the trio.

Call our time traveler (from our time around 2024), *fugitio,* who overhears their dialogue and adds his own futuristic (from their points of view) quantum-physical comments.

## Day One

On day one, our trio explores the quantum physical question of realism brought into question by Niels Bohr's so-called Copenhagen interpretation: Accordingly, things "out there" only come into existence when (and only when) they are *observed*. Prior to their observations, these "things" only existed as ghost-like waves of possibility.

Thus, there seem to be hidden factors or variables that render our actual things apparently invisible until an observation. Our trio believes that before our observation or measurement, these things still existed "out there". Thus, Bohr's thesis leaves our trio all a bit confused since their commonsense view of the world and all its contents seemingly exist, regardless of whether any of them observe it.

Suddenly appears *Fugito* waving a flag of truce: Well, gentles all, we in the future, some fifty years hence, have added some light to this dark dilemma, for now, we have several new ideas which might help disperse the confusion, but I caution you all, for it also introduces some perhaps even stranger ideas.

The trio exclaims: "Speak, we can't wait to hear you!"

*Fugito* reports: Let me summarize your pondering: Albert Einstein, with whom you all are cognizant, had, during the quantum theory's inception, many discussions with Niels Bohr, Werner Heisenberg, and several other luminaries of this *new* physics, pointed out that we all previously believed that observed phenomena—attributes of real things brought forward by experiments—simply pre-existed "out there" regardless of what idea or theory we had about them. Einstein upset this point of view when he declared: "It is the theory that determines what we can observe." In other words, without any idea or concept of what is "out there"—we cannot really know what we are observing. We take it for granted that there must be real things or particles that exist and we expect our theory to tell us how these particles behave. But quantum theory doesn't describe that picture.

To this, they all reply nearly in unison, "Well then we need a better theory, one that can perhaps grapple with hidden controlling factors or variables that render the results of our observations."

*Fugito* responded: Albeit it turns out that our current theory of quantum physics does produce, in my time (2024), some new and very interesting proposals concerning reality especially those pesky hidden variables.

This left our trio hopeful but nervous and curious. So, they adjourned, planning to meet the next day.

## Day Two

On day two, they again wonder whether the concept that there are real objects "out there" in the universe was true—an idea that seemed so successful in describing the everyday world they all perceived around themselves. Why, they ask, is it so elusive to describe real objects when they are very small—atomic-sized? Aren't large objects made up of smaller ones? Classical physics, as seen by Newton and others, seems to work very well when dealing with large objects. Even throwing Einstein's special theory of relativity into the pot, although a somewhat mysterious concept that changes our commonsense view of time and space, does seem to still grasp that objects are "real" and "out there."

*Fugito*: Yes, that raises a most profound question and observation. You all seem to think that we just need a new addition to our classical theory, one that contains such hidden variables—even Einstein thought the quantum theory was incomplete. You point out how difficult it is to make a consistent "hidden variable theory." How should such variables act? We all might agree that if "real" particles are really "out there" and are locally controlled by such variables, and if two such particles interact and then widely separate, their properties should be independent of each other.

An attempt towards such a classical hidden variable theory was given by David Bohm. He reinterpreted standard quantum physics such that the apparently ghost-like wave of possibility mentioned earlier was theorized to be a real wave and "out there" and, as such, was able to influence all real particles just as a magnetic field influences current-carrying wires and magnets. Later, Bohm's interpretation was revisited by physicist John Bell, in his famous no-go theorem (in essence, there cannot be hidden variables), who showed that such a "real" wave describing two quantum-entangled (meaning having interacted and thus influencing each other) and separated particles could not produce such independence. This meant a measurement made on one particle at one spacetime location could suddenly change the measurement result made on the other particle at a distant (spacelike—faster than light could travel from one to the other) spacetime location simultaneously. This is called *quantum entanglement* and resulted in 2022 three Nobel prizes to Alain Aspect, John F. Clauser, and Anton Zeilinger. Working independently, each of the three researchers forged new experiments demonstrating and investigating this seemingly magical connection.

This led our trio to question whether classical physics could ever be the ground for the ultimate theory sought for. So, the three adjourned to look forward to the next day.

## Day Three

Our still somewhat befuddled trio began to wonder if any theory would suffice being that experimental results were so uncertain, as if God were throwing dice, producing results that were seemingly chancy, yet at times quite close to what was predicted. "Could the future vision given to us by you, *Fugito*, be of any help?"

*Fugito* replied: When we consider joint measurements (of two or more variables) based within quantum physics, something called *contextuality* of our observations comes into question, and with it, so does *classicality*—the notion that underlying the world are objects that behave just like large objects of our everyday world behave. We have already discussed classicality (that there are real particles) during the previous two days. The new notion of contextuality probably first came into quantum physics in 1968, so you may have already read about it. In brief, any observed result will depend on the context with which it was observed—the other variables that are also observed before, at the same time, or after.

Two physicists, Simon Bernhard Kochen and Ernst Paul Specker (KS), came up with a rather perhaps complex but nevertheless surprising proof, a mathematical inequality, dealing with such apparent classical hidden

variables, specifically what we assume to be real and "out there," even if we don't actually look to see them, turns out to be an illusion. KS concluded that classical hidden variables cannot represent "elements of physical reality."

Later, Israeli physicist Asher Peres showed using a simple exercise that what we call "the result of a measurement of a variable **A**" cannot depend only on **A** provided that other allowed variables, such as, e.g., **B**, are also measured. Thus, the result for **A** depends on the choice of other quantum measurements like **B** that may possibly be performed—at any time—in the so-called context of **A**'s measurement.

*Simplicio* then replied, "That would mean if I flipped two coins and one came up heads while the other came up tails, the observation of the first coin's result would depend on whether or not the second coin came up heads or tails no matter when I flip the second coin?" *Fugito* replied, yes, that is correct, even though the coins may be spatially completely out of range of each other.

## Day Four

*Fugito* decided to continue his commentary the next day while the trio was perhaps waking up while still reflecting about contextuality. He went on: Yes, some new ideas may be helpful in this regard, but they may require you to give up some precious ideas about the nature of time. For example, consider a paper by Yakir Aharonov, Eliahu Cohen, Doron Grossman, and Avshalom C. Elitzur (ACDE), written circa 2015, entitled *Can a Future Choice Affect a Past Measurement's Outcome*? Here, the idea of two kinds of measurement is introduced: weak and strong measurement. Whether a measurement is weak (WM) or strong (SM) depends on the measuring instrument. SMs are produced when the measuring instrument is sharply tuned, while WMs are produced when the instrument is not sharply tuned. Surprisingly, WMs are able to yield significant results *when they are made before* an SM. One such result produced the outcome that the WM made at the earlier time was actually determined by the SM made at the later time. The reciprocal, however, does not hold for a combination of measurements of which the latter one is weak and the first one strong. *The latter SM affects the former WM, never vice versa*. Therefore, when a weak measurement precedes a strong one, the only possible direction for the causal effect seems to be from the future to the past.

Surprisingly, even though the experimenter did not recognize that this WM result would be determined by what would be done in the future, the relationship between the later SM and the earlier WM result was indeed as predicted. The most reasonable resolution seems to be that the experimenter's choice has been encrypted within the WM's outcomes, even before the experimenter knew what their choice would be.

Our trio was quite upset that such results could be the truth. *Fugito* continued: Finally, this work of ACDE sheds new light on the age-old question of free will. One would tend to believe that the anticipation of the choice of a measurement by a human being to be made much later renders that choice fully deterministic and only bound by earlier causes. The profound result of ACDE, however, shows that this is not the case. The choice anticipated by the WM outcomes can become known only after that choice is actually made. This inaccessibility, which prevents causal paradoxes like "killing one's grandfather," secures human choice full freedom from both past and future constraints. The earlier choice is fully deterministic, seemingly but erroneously bound by even earlier causes. The choice anticipated by the weak outcomes can become known only after that later SM choice is actually made, even though what earlier choice is made depends on what will be chosen later. But our earlier experimenter seemingly doesn't know what will be observed later. He will think his earlier choice is freely made—even though it will be determined by what he will choose to do in the future. This inaccessibility thus secures human choice and full freedom from both past and future constraints, even though they are connected.

*Simplicio* said: I am totally at sea with this extension of quantum physics theory. I am still wondering about how we are able to measure anything at all. I go back to the old dichotomy: are things waves or particles? Could this be resolved by what you have told us so far?

What shall we do when the theory gives two contradicting points of view? This dichotomy led us to the idea that whatever was accurately revealed in a measurement always contained a hidden complementary reality. (Observe the momentum of a particle and its position cannot be seen with any accuracy and vice versa.)

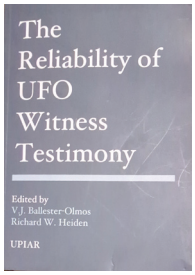
*Fugito* responds: That old conundrum still remains, but it is now pictured quite differently. Quantum physics has not only re-pictured matter, it has also made us rethink what is meant by space and time. Our everyday view has it that things or particles exist as separate things appearing at unique times, always such that what occurs now can only have an influence on what happens later but never before. Quantum physics has now changed that. Measurements of things occurring now must depend on what else is being observed now or, before, or even after. So, the old picture of wave or particle duality is replaced by a whole new ballgame. Not only does matter take on a new meaning but so do time and space. Perhaps what is still missing is the role human minds play in the arena we call the universe.

Journal of Scientific Exploration
Anomalistics and Frontier Science

BOOK AND MULTIMEDIA REVIEW

# The Reliability of UFO Witness Testimony

**Kevin Randle**




Ballester Olmos, V.-J., & Heiden, R. W. (2023). The reliability of UFO witness testimony. UPIAR.

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Dr. Jacques Vallee, in a book review in the Fall 2001 issue of the *Journal of Scientific Exploration* (15,3), wrote, "In his introduction Jacobs (David Jacobs, *UFOs and Abductions: Challenging the Borders of Knowledge*) proposes a statement of dual problems of contemporary UFO research: (1) all the work done by ufologists over the last 50 years, 'has not solved the problem of building bridges between them and the scientific community,' and (2) the key issue is to decide whether people 'are accurately recalling real events, or are they generating psychological based accounts?'"

The major complaint about UFO research and UFO researchers was that a great deal of the evidence is in the form of testimony, which science often claims is anecdotal in nature. Science suggests that evidence for alien visitation lacks reproducibility, that it can't be taken into the lab to be examined, and it can't be replicated in the lab. We are left with the observations of people, some highly trained and some who never finished the most basic of education, as the witnesses.

Ballester-Olmos and Heiden have put together a book of 711 pages in an 8½ by 11 format that explores this problem. It is filled with photographs and charts, which reduces some of the reading time, but it does take time to work through most of the scientific papers. Some of the papers, rather than looking as if they were prepared for a peer-reviewed journal, look as if they were written for a popular magazine. While there is a comprehensive table of contents, there is no index. Each of the entries provides a bibliography of source material for those who wish to assess the value of those sources and that entry.

The book is divided into seven sections, beginning with case studies of various UFO events and ending with epistemological issues, including "On the Fallacy of Residue," which suggests there will always be an unresolved residue of cases for reasons that have nothing to do with the alien nature of the events but because there will always be cases in which critical information will have been overlooked or left out. And, of course, this is an accurate statement.

About the first thing I noticed about the case studies was that most of the entries were written by those who resided in the skeptic's community. That's not necessarily a bad thing though that bias might have infected the thinking of some of the authors of the various papers. Although I try to maintain a dispassionate view in my investigations, I sometimes find that my biases creep into my books and articles. It is one of those things that many of us work to avoid but frequently fail to do so completely.

The book does not have to be read in sequence to understand the points of view. I turned to the case histories with which I had some intimate personal knowledge. The first segment I read was the analysis of the abduction of Charles Hickson and Calvin Parker, which was provided by Joe Nickell of CSI. There are portions that seem disingen-

uous; however, he does move from the position that the case is either a hoax or an alien abduction by providing a third theory. He postulates that it was the result of a hypnogogic hallucination. Although hypnogogic hallucinations are always associated with sleep, Nickell suggests that Hickson had been drinking prior to the abduction, and this might have induced both the necessary sleep and another explanation. There is no evidence that Hickson had been drinking prior to the event and none that he had fallen asleep. This aspect of the theory is invented by Nickell.

Nickell wrote, "Although the UFO reported by the men had apparently not been seen by people on the heavily traveled nearby highway, there had been sightings in the area, including on the night in question." That quote is attributed to me, but Nickell had to know the original source was Philip Klass, and it was later documented to be false.

The area in question was a highway bridge that did have a view of the abduction site, but the terrain, the structure of the bridge, and the vegetation in the area obscured that site so that only a fleeting glimpse was available. More importantly, however, is a document created by high-level Air Force officers at Keesler Air Force Base the day after the abduction. It provides the names of two witnesses who saw the UFO and links to two others who were on the bridge at the time. This information was readily available when Nickell wrote his analysis because he cites Ralph and Judy Blum's *Beyond Earth: Man's Contact with UFOs*, published months after the sighting, which contained the witness information.

In a discussion I had with Calvin Parker, I asked about the claim that he had passed out and had no real memory of the event. He said that he hadn't wanted to be involved, and it was Hickson who suggested that he say that he had passed out. Parker had a clear memory of what happened and later described his examination onboard the alien craft. This, it seems, renders Nickell's hypothesis moot.

In the discussion of the Phoenix Lights by Tim Callahan, a solution, that is flares dropped by military aircraft during an exercise, is suggested as the solution for all the sightings. The evidence is persuasive. The lights filmed and spotted over Phoenix were the flares, contrary to what a few UFO researchers have claimed.

In his discussion about the case, Callahan noted there were three Air Force bases in the general area, but none of them responded to the lights. Davis-Monthan AFB is in Tucson, but there is no air defense capability there. The 355 TFW was a training unit in 1999 but was equipped with the A-10 Warthog, which is a ground support fighter and not an interceptor. The Air National Guard Papago Park Military Reservation had no air defense mission and had no capability of intercepting the intruder. Finally, Luke Air Force Base was a training facility in 1999 and had no air defense responsibility. In answer to Callahan's question, none of those bases had the equipment nor the mission to provide intercept of any intruder. His question about that is irrelevant.

In his analysis, Callahan cites Dr. Elizabeth Loftus, the psychologist who is a leading authority on memory and perception, to explain that witnesses can often be subtly led during interviews. Sometimes their memories are colored by what they have heard about a situation or by discussing it with other witnesses or family members. In Phoenix, the sightings were important news that was reported almost immediately. This is an obvious source of contamination.

Loftus' studies are often cited when dealing with eyewitness testimony, and they certainly suggest that those gathering the data should be careful when interviewing witnesses. With the Phoenix Lights, it seems that the sources of contamination are ignored by the UFO investigators.

However, Callahan has assumed that the Phoenix Lights and the sighting of a large triangular-shaped object were also reported that night as two components of the same event. Witnesses I have interviewed who were not in the Phoenix area but did see the triangular object said that it was solid, based not on it blocking out the stars but because they could see the actual shape. This sighting was not explained by the flares.

The discussion about perception and memory are important factors in dealing with an event, especially when the interviews are conducted weeks, months and years later. Loftus is cited in many of the subsequent sections of the book as well.

Wim van Utrecht's report on "Lunar Terror in Poland: A Doctor's Dilemma, provided another problem. While it seems that the solution of the sighting is correct, there was one aspect of this that was worrisome. On page 208, while discussing the possible solution, he discovered a discrepancy with the date. It had been widely reported by UFO researchers that the sighting date was September 5, 1980, but using astronomical records, the moon was not in a position that could be seen given the directions and times. However, on September 5, 1979, the moon was right where the witnesses had said they saw the circular UFO. He found a reference to the sighting that did confirm the earlier date, but that source had cited another source. He didn't follow up on that.

Tim Printy, in his discussion of expert witnesses, mentioned a sighting from Stockton, California, on August 15, 1975, that, according to Printy, had "been thoroughly investigated and used several independent sourc-

es of information." This suggests a solid case and one that deserves scrutiny.

But Printy, here, does what I think of as "chasing footnotes." He wrote, "However, as one pulls on the string, the entire garment unravels. It appears that all of these individuals obtained their material from one single news story that was missing a lot of pertinent data."

That is the real point to made in these investigations. The leads must be followed to the end to ensure that the best information is recovered. In van Utrecht's analysis, he did not check the primary source, which had no real impact, but that extra step would have made the analysis stronger. Printy took that step, which made his analysis strong and weakened the importance of the case.

The real importance of the book comes in the sections following the case studies, which might be thought of as the scientific papers. Here is where the book shines. It provides the current research on various psychological, anthropological and eyewitness testimony as well as related other issues that are important to understanding the status of UFO study.

Thomas D. Albright reported "On Eyewitness Reports of Extraterrestrial Life." He provides a definition of the various kinds of evidence, rating the importance of them and there is no complaint about that. For proper investigation, proper definitions are necessary. The problem here seems to be a lack of understanding of the history of UFO reports and UFO evidence. Instead of reporting on Close Encounters of the Third Kind, referencing reports of alien beings, he examines Our Lady of Fatima, bringing a religious element into the discussion.

But, he does examine the importance of gathering testimony and the biases that are built into such reports. He looks at "The Eyewitness: Expertise of Everyman," which provides insight into the problems with eyewitness testimony. He offers some methods of improving the gathering of testimony and that the investigator must be aware of some of these problems.

I do want to note that there are articles that are at opposite ends of the spectrum. Tim Printy analyzed the idea of expert testimony, suggesting that pilots, while highly skilled and highly trained, were not experts in what was in the air around them. He suggested, rightly, that even pilots with thousands of hours of flight time could be fooled by the unusual. The term an expert witness, while enhancing the credibility of a pilot testimony, might be misleading.

Richard Haines, in "Witness Reliability: Accuracy – Reliability of Pilots – Personal Honor," suggested that a pilot's skill and training, along with thousands of hours of flight experience, did, in fact, provide them with a different perspective. Their observations from the cockpit are often corroborated by hard sensor data, as seen in the discussion of aviation accident analysis and "near miss" incidents. In other words, pilot eyewitness testimony isn't nearly as unreliable as other forms of eyewitness testimony.

What makes Haines' paper interesting is that it challenges some of the conclusions drawn by Printy in the case study section. Printy suggested that labeling pilots and law enforcement officers as expert witnesses might be something of a misnomer. While they are highly trained, that does not necessarily translate into expertise when observing ambiguous objects under unusual conditions. Haines suggests that, because of their training and experience, they are more careful in their observations than the general public. I think of them as more credible witnesses, which does not mean that they can't be in error, only that they are better at these sorts of observations than the average citizen.

That makes for an interesting discussion of the relevance of such testimonies and the weight that should be given to them. Both Printy and Haines make the case from their personal perspectives.

There are instances in which various papers support one another. Robert Young examines the Kecksburg UFO crash, while Dr. Jean-Pierre Rospars, in "Abilities and Limitations of Eyewitness Assessed on Atmospheric Entries of Meteoroids and Artificial Satellites," supports much of Young's thesis.

I agree with Young, that the Kecksburg UFO crash is a misidentified meteor fall based on the research that I have conducted. The physical evidence and the photographs of the smoke train lead to that conclusion.

Before this gets completely out of hand, let me say this. I found the first section of the book, that is, the case studies, to be slanted toward the skeptical. I'm not sure that this point of view matters in assessing the overall importance of those cases specifically or the book generally. The heart of it, most of it, deals with the ongoing research into various arenas that directly affect UFO research. Sleep paralysis, for example, as an explanation for many tales of alien abduction, is an area of research that wasn't understood in the 1970s and 1980s. David Hufford's book, *The Terror that Comes in the Night*, examines what we now think of as hypnogogic hallucinations. Many of the abduction tales mimic the illusions from an episode of what Hufford called sleep paralysis. Hufford's book helps us understand this latest book.

There are many of the scientific papers that should be required reading for those who wish to engage in serious UFO research. There is a cluster of papers that deal with alien abduction that provide many of the terrestrial explanations for the abductions. I have advocated for years

that a protocol be developed to distinguish between alien abduction and sleep paralysis. Although some have told me that they were working on that, I have seen nothing being used in the world today.

In the end, this book is worth the effort to study it because it addresses one of the major flaws in UFO research. Too many of us ignore the scientific method and the scientific literature that would benefit us as we investigate UFOs. Those who see themselves as investigators and researchers should be required to read the book because of the comprehensive nature of the work.

While this is a worthwhile effort, the sad thing is that this book comes to us twenty years after Jacob's made his comments. UFO research has not advanced very far since then, but this book should begin the process of moving into the scientific arena. At the beginning of a scientific project, one of the basic requirements is to complete a literature search. This book provides the basis for that literature search. Now, the rest of us must build on that process with this book as the first of those steps, and I highly recommend it for anyone interested in UFOs and UFO research.

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COMMENTARY

# JSE Special Issue Editor Commentary on Rubinstein (2024)

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I am delighted that Prof. Rubinstein — like many scholars around the world who contacted me after the appearance of *JSE's* 2023 special issue (37:2) on the Shakespeare Authorship Question — found the issue of sufficient interest to respond in such depth. As that issue argued, the leading candidate by a wide margin is Edward de Vere, the 17th Earl of Oxford. Yet the debate goes on. Whether one wants to argue for Christopher Marlowe or Mary Sidney or even Sir Henry Neville [as Prof. Rubinstein (2024, *38*, 258-272) did] the claims for all of the candidates remain essentially circumstantial with the Oxfordian circumstances somewhat more equal than most of the others. The bottom line here is important though: even when scholars advocate for other candidates, they all seem to agree that the historical man from Stratford was certainly *not* the true author for the many good reasons argued in the special authorship issue and which Prof. Rubinstein simply adds to in his own essay.

My own response to Prof. Rubinstein's most articulate article is that there is just too much surmise in it for evidentiary comfort, far more surmise than one finds, for example, in the Oxford argument. And even he seems astounded by his final suggestion: that the man from Stratford regularly stopped into the city of Oxford to meet with Neville at the home of his friend Sir Henry Savile. Indeed, Prof. Rubinstein himself writes of this totally unsupported claim that "there is nothing whatever to link Shakespeare and Savile...and there is nothing whatever to link Savile with the London theatre...." So why, one asks, does Rubinstein even suggest so improbable a connection at the conclusion of his essay. *It could have been*? *It might have been*? As they say in the old canard, if the Queen had alternative plumbing, she would have been King.

That said, some of Prof. Rubinstein's points do need to be answered even in this very-open-to-debate context. Specifically – and roughly in the order presented by the good professor in his essay:

- *The Hand D argument*. This argument suggests that Shakespeare -- whoever he, she, or they were --wrote a portion of the extant play, *Sir Thomas More*. The argument says that of the various handwritings that make up this text, Hand D is the man from Stratford's. Unfortunately, all we have from the Stratford man's entire life are six barely legible and often differently drafted signatures on four different legal documents. Forensic specialists in the handwriting field have concluded more than once that these signatures – possibly made by law clerks -- are not enough to make any sort of comparison with Hand D. Of course, if Hand D turns out to truly be by the author using the pseudonym Shakespeare than maybe Hand D has some value in this context. But at the moment, Hand D in and of itself has no intrinsic value in the authorship debate. A very red herring.

- *Dates of composition*. Prof. Rubinstein says without any proof whatsoever that the plays *Hamlet, Othello, King Lear, and Macbeth* were *written*, respectively, in the years 1601, 1602, and 1605. The fact is, no one knows when any of these plays were actually written. All we know is when particular plays were first produced or published. The standard chronology of the plays simply assumes that all were written within the birth and death dates of the Stratford man (1564-1616). Eliminate the Stratford man as author, and the supposed dates of writing quickly fade away. Oxfordians suggest that many of the plays were actually written a decade or two earlier than the standard chronology and were first produced for court performances before the Queen under different titles. It was years later that they appeared in often significantly revised versions in the public theatres.

- *The Sonnets and the words "our ever-living poet."* Most scholars accept the notion that "ever-living" in any dedication suggests clearly that the dedicatee is no longer living. Edward de Vere died in 1604, and the Sonnets, probably dating from more than a decade earlier, were not published until 1609. It makes perfect sense then that such a posthumous publication might well use the term "ever-living" in a dedication to the poet. Neville was still very much alive.

- *The Strachey letter*. This document, published in 1625, is often cited as a source for a reference to "the Bermudas" in *The Tempest*. The reference, however, dates much earlier and connects to a dubious section of London known as "the Bermoothes." If you do not like that reference, others have suggested it is a reference to wormwood used to make absinthe. Connecting the Strachey letter to a dating of *The Tempest* was quite clearly put to rest in 2013 by scholars Roger Stritmatter and Lynne Kositsky in their study of the play, *On the Date, Sources and Design of Shakespeare's The Tempest*.

- *Early authorship doubts*. Prof. Rubinstein suggests that no one ever questioned the Stratford man's authorship or offered up clearly an alternative author until the 19th century. In fact, questions and hints that the Stratford man was *not* the true author began to appear as early as the 16th century. With respect, Rubinstein needs to read some non-Neville research on the subject, such as Bryan Wildenthal's *Early Shakespeare Authorship Doubts* (2019), and to take note of Prof. Stritmatter and Alexander Waugh's upcoming two-volume set of even more early allusions to the authorship question. The research on this issue is already strong and getting stronger.

- *Why not Oxford?* The idea that Oxford's early plays were produced with great success at the court and then later published and/or produced in revised versions, often with different titles for the public theatre, is dismissed by Prof. Rubinstein as "improbable." I disagree strongly and suggest that he look at the impressive research by independent scholar Ramon Jimenez about this important subject in *Shakespeare's Apprenticeship* (2018), which clearly makes the argument that these plays are Shakespeare's lost juvenilia. The fact is, early plays such as *Taming of a Shrew* and *King Leir* surely fit the dating for this pro- Oxford argument.

  Indeed, much of what Rubinstein argues for Neville (his familiarity with the French Court and Italy) is the same as the arguments for Oxford's candidacy. Only the names are changed to protect the chosen candidate. Need I add here that the Oxford argument has been tested now for more than a century. By comparison, the Neville argument is only about twenty years old, and relatively few authorship doubters have lined up behind it.

- *The handwriting at Audley's End*. Prof. Rubinstein argues that the large collection of books owned by Neville, and which today are at Audley's End, are filled with clear connections to Shakespeare's plays *in Neville's own hand*. In the last year or two, however, Prof. Stritmatter has been studying the same material, and he comes to a much different conclusion. In the Winter 2024 issue of the *Shakespeare Oxford Newsletter* (pp. 6-7), Stritmatter writes:

  "Prof. Rubinstein and his late colleague John Casson deserve gratitude for having been responsible for first bringing before the public a revealing look at the wonders of the Audley End volumes. In their 2016 book, *Sir Henry Neville Was Shakespeare: The Evidence,* Rubinstein and Casson showed, beyond any doubt, a pattern of evidence that deserved, and indeed, required, further study.... [However], these annotations are not by Sir Henry Neville...they are by Edward de Vere, Earl of Oxford.... The formal demonstration of this conclusion is forthcoming...in the *Journal of Forensic Document Examination.* The article first debunks the belief that the annotations are by Neville and then, using the same standards, shows that Oxford was the annotator....

  "Beyond this general response to Professor Rubinstein's several arguments by innuendo and imaginative reconstructions of hypothetical scenarios of

book provenance.... Rubinstein's belief that provenance trumps forensic inquiry is mistaken.... Neville outlived Oxford by a dozen years. It is not difficult to see how books from Oxford's collection might have found their way into Neville's collection; being objects of value, they had to go somewhere after his death. It shouldn't require an advanced degree to realize that book collectors own, and some even collect, books annotated by past owners."

Again, sincere thanks to Prof. Rubinstein for continuing the important debate about the Shakespeare Author Question and to *JSE* for publishing it.

## EDITORIAL NOTE: OBITUARY

**William Rubinstein**

Shortly after writing the above comments about Prof. Rubinstein's response to the special *JSE* issue on the Shakespeare Authorship Question, news reached me of his sudden death at the age of 77 in Australia, where he lived. I would like to express my genuine condolences to his family and many friends in the Shakespeare authorship community.

Prof. Rubinstein and I never met personally but we certainly knew one another's work and, I believe, we shared mutual respect for one another's positions on various issues even in disagreement. The fact is, who wrote Shakespeare was just one of this historian's many causes.

Born in New York City and educated at Swarthmore College and John Hopkins University, he moved to Australia in the 1970s, where he taught history at the Australian National University in Canberra from 1976–1978, at Deakin University in Victoria from 1978 to 1995, and from 1995 to 2011 at Aberystwyth University in Wales. He returned to Australia after his Welsh experiences and became an adjunct professor at Monash University in Melbourne from 2013 to 2015.

A Fellow of the Australian Academy of the Humanities, the Australian Academy of the Social Sciences and of the Australian Royal Historical Society, he also was an indefatigable supporter of Jewish causes. One of the founders of the Australian Association for Jewish Studies, he served as its president from 1989–1991. From 2002-2004, he served as President of the Jewish Historical Society of England.

Widely published, his many essays on a variety of social and historical subjects, Judaism, and even the Shakespeare authorship question appeared in numerous scholarly publications worldwide. His writings were translated into Finnish, Russian, French, Hebrew, Italian, Chinese, and Japanese. His books on modern Jewish history include *A History of the Jews in the English-Speaking World: Great Britain* (1996) and *The Myth of Rescue* (1997).

An obituary for Prof. Rubinstein in the *Australian Jewish News* on 11 July 2024 noted that "Beyond academia, Rubinstein was a powerful voice in public discourse. A regular contributor to both Jewish and mainstream media, he fearlessly advocated for Jewish causes, courting controversy with his conservative political views. His intellectual curiosity, for Jewish history and culture, made him a uniquely influential figure in Australian Jewish life."

His major work on the Shakespeare authorship question was called *The Truth Will Out*, which was published in 2005 and co-authored by Brenda James. It was in that book that he argued most clearly for Henry Neville as the real author of Shakespeare's works. That said, at his death, Prof. Rubinstein's position was still very much a minority view, even within the authorship community. Suffice it to say here, his passionate advocacy on this subject– as with so many other issues -- will clearly be missed

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# Response to Don Rubin

COMMENTARY

**William Rubinstein**

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There are two other matters to raise before responding to the specific points made by Professor Don Rubin (2024). The first concerns 'Hand D' (although I also respond to other points he raised about Hand D below.) I attach photographs made by John O'Donnell, an excellent Neville researcher, of words in the manuscript of Hand D, and the same exact words in letters written by Sir Henry Neville. (When I wrote my original article for this *Journal* (Rubinstein, 2024), I had not yet secured the permission of Mr. O'Donnell to reproduce them, as I have since then.) As will be seen, the two sets of words are identical, and were obviously written by the same man. To reiterate, these are photos of the same exact words, not merely evidence of apparently similar writing styles. The idiosyncratic features of this handwriting were also very likely to have been accentuated by the pens, ink, and paper used in Elizabethan times. The photos here also supplement the photos of Neville's handwriting reproduced in my original article and, more fully, in the book I co-authored with the late Dr. John Casson (Casson & Rubinstein, 2014). The identical nature of the handwriting constitutes powerful, if not irrefutable, evidence that Neville wrote Shakespeare's works.


**Figure 1.** The Handwriting of 'Hand D' (Left Column) in *Sir Thomas More* (Jowett, 2011) Compared to Sir Henry Neville's Handwriting.

Secondly, Don Rubin is an advocate of the view that Edward de Vere, 17th Earl of Oxford (1550–1604), wrote the works of William Shakespeare. When, many years ago, I first became interested in the Authorship question, I read everything available on de Vere as the real author and have read most works published subsequently, but I was not convinced, then or since. Although the Oxford theory has now been around for over a century, not a single example of what might reasonably be termed convincing evidence in support of the theory has ever been found. There is, in addition, the inconvenient fact that de Vere lived from 1550 until 1604, although all mainstream accounts of Shakespeare's career assert that he wrote his works between about 1590 (when de Vere was 40 years old) and 1613 (when de Vere had been dead for nine years), necessitating the creation by Oxfordians of a new chronology of the life and writing career of 'William Shakespeare,' for which no evidence whatever exists, to say nothing of the fact (as outlined in my original essay) that Shakespeare's plays contain references to events that occurred after de Vere's death.

The conclusion that the Oxfordian theory is false is also the conclusion of all academic scholars who have studied it, including those who have examined its claims in detail. A prime example of such a scholar is Emeritus Professor Alan H. Nelson, formerly of the University of California at Berkeley. Nelson (2003) was the author of the standard scholarly biography of de Vere, *Monstrous Adversary: The Life of Edward de Vere, 17th Earl of Oxford*, a 527-page work which includes hundreds of footnote references, many from unpublished manuscript sources. Nelson was well aware of the Oxfordian theory and devoted several chapters in his book to examining de Vere's career as a poet and playwright. He was also the author of the biographical entry on Oxford in the *Oxford Dictionary of National Biography* (the ODNB, first published in printed form in 2004), the standard biographical compilation of the lives of notable British people from Roman times to a few years ago, containing over 60,000 entries, written by specialist experts, and continuously revised as required. Nelson (2004) concluded his online entry on de Vere by stating that:

> claims by literary and historical amateurs, beginning with J. Thomas Looney in 1920 and embraced by Oxford's otherwise worthy biographer B.M. Ward, that Oxford wrote the poems and plays attributed by contemporaries to William Shakespeare are without merit.

In *Monstrous Adversary,* Nelson (2003, p. 386) also quoted Francis Meres's famous 1598 passage about the best writers of his time of comedy:

> so the best for Comedy amongst us be, Edward Earle of Oxforde, Doctor Gager of Oxforde... John Lilly, Lodge, Gascoyne, Greene, Shakespeare, Thomas Nash, Thomas Heywood, Anthony Mundye [sic] our best plotter...

After citing this passage, Nelson (2003) concluded that "Meres (for one) knew that Oxford and Shakespeare were not the same man" (p. 387). This raises a central question about the Oxfordian theory: if de Vere wrote the works attributed to William Shakespeare, why did he write 'comedy' plays under his own name, while also being the author of 37 plays, 154 sonnets, and several long poems using the pen name 'William Shakespeare'? Possibly, he might have feared that writing controversial politically sensitive works about the overthrow of dynasties under his own name might have landed him in trouble, but, as 'William Shakespeare,' also wrote the Falstaff plays, light Comedies, and the non-political sonnets. Writing under two different names makes no sense and is good evidence that de Vere was *not* William Shakespeare. Nelson (2003) also assessed de Vere's poetry, much of which – unlike his plays – still survives, and concluded that "Oxford's poems are, above all, astonishingly uneven. The best, though few, are fine indeed, while the worst, including "The labouring man that tilles the fertile soyle", are execrable" (p. 387).

To turn now to the specific points raised by Don Rubin (2024):

1. *Hand D*. Above, and in my first article (Rubinstein, 2024), I presented striking photographic evidence that the handwriting in Hand D is that of Neville. It is certainly true that William Shakespeare's (i.e., the historical man from Stratford-Upon-Avon) handwriting only consists of six signatures on legal documents, two of which might have been written by lawyers' clerks, but this is evidence that Shakespeare was not an author, not that Neville did not write Hand D. That Hand D was written by 'William Shakespeare', whoever he was, is argued at length and most persuasively by John Jowett (2011, pp. 437–453), editor of the Arden edition of *Sir Thomas More*, who concluded his discussion by saying that "Currently, the case for Shakespeare [as the author of Hand D] looks more secure than ever" (p. 452). Apart from the, by now, almost unanimous opinion of scholars, two other key points should be made: if Sir Henry Neville was *not* 'William Shakespeare', but was – as is clear from the handwriting – the author of Hand D, it is a complete mystery why he should have been asked to write this portion of the play *Sir Thomas More,* alongside four other well-known playwrights of the

day who are believed to have written the rest of the play, namely Anthony Munday, Henry Chettle, Thomas Dekker, and Thomas Haywood, when Neville wrote no other literary works of any kind under his own name. Secondly, if Hand D was written by de Vere, it is curious that, so far as I am aware, no Oxfordian has ever taken the simple step of producing a comparison of his known handwriting with that of Hand D. In fact, the samples of handwriting unquestionably by de Vere which I have seen are nothing whatever like the handwriting of Hand D.

2. *Dating the Plays*. Nevillians fully accept the standard dating of the plays and poems by 'William Shakespeare,' which are supported by much evidence apart from simply Shakespeare's dates. To take one example. *Hamlet,* probably Shakespeare's most famous play, was first published in two separate quarto editions (i.e., a 'quarto' is a play published separately and by itself, and not in a volume of plays), known as Q1 and Q2. Q1 was entered in the "Stationer's Register" – where all plays and other published works had to be recorded – in 1602, and then actually printed in the summer or autumn of 1603 (works written but not yet printed were often listed in the Stationer's Register sometime before their actual publication). Q2, a longer version of the play, similar in content to the *Hamlet* on stage today, was published in 1604. Later, *Hamlet* appeared in the *First Folio* edition of all of Shakespeare's works, published in 1623. It seems obviously likely from this that *Hamlet,* a world-renowned work, was written in 1600–1602, just before the play was registered, rather than having been written many years earlier and, for no reason, kept gathering dust in some drawer or storage box.

Asserting that a work was written many years earlier than its conventional date because its accepted date does not accord with the Oxfordian chronology is a typical claim made by Oxfordians, who regularly invent facts to fit their theories, and understanding this is especially important when considering the dates of those plays by Shakespeare which are agreed by scholars to have been written after de Vere's death in 1604, a long list that includes *King Lear* (1605– 1606); *Timon of Athens* (1605–1606); *Macbeth* (1606); *Anthony and Cleopatra* (1606); *Pericles, Prince of Tyre* (1607–1608); *Coriolanus* (1608); *The Winter's Tale* (1609–1611); *Cymbeline* (1610); *The Tempest* (1610–1611); *Cardenio* (1612–1616, a lost play whose title is known); *Henry VIII* (1612– 1613); and *The Two Noble Kinsmen* (1613–1614), to say nothing of the volume of Shakespeare's Sonnets that appeared in 1609. If the author of Shakespeare's works died in 1604, where were the manuscripts of these 12 plays hiding prior to their apparent dates of authorship? Why were they not published long before? Who authorized their publication, and why then? Common sense alone tells us that these plays were written in the lifetime of their author; the list and the dates are clearly consistent with an active playwright, producing a new play every year or two for his acting company to put on.

3. *The Sonnets*. Our view is that the famous and mysterious dedication to *Shake-speares Sonnets* was written by Neville himself, and that the dedicatee, "Mr. W.H.," was Henry Wriothesley, Earl of Southampton, Neville's close friend, with his initials reversed, almost certainly to recall their time together, from 1601 until 1603, as prisoners in the Tower of London following the Essex rebellion, where they probably joked about being reduced to "Mr." when they were stripped of their titles; the reversal of their initials was almost certainly a private joke, just as was Neville's use of "our ever-living poet." The *Sonnets* were almost certainly published when they were because Neville was then in an upbeat mood. The work's publication coincided with the marriage of his eldest son to an heiress the month before (the first nineteen Sonnets are addressed to a young man, advising him to marry and have children, and had almost certainly been written for and sent to his son). These Sonnets have nothing whatever relevant to anything known about the life of William Shakespeare.

The *Sonnets* were also published when they were to mark the official launch of the London Virginia Company on almost the same day as the work was published, upon whose success Neville was pinning his financial hopes. "T.T.," the initials of the man who signed the dedication, was almost certainly Thomas Thorpe, the volume's publisher. Neville almost certainly did not have Thorpe's permission to use his full name in print, so he used just his initials; any other alleged explanation makes no sense. As the 154 Sonnets were certainly written at different times and addressed to different people, only their author would have had copies of all 154 Sonnets; the fact that the volume was titled *Shake-speares Sonnets,* rather than *The Sonnets of William Shakespeare* strongly suggests that their publication was the work of someone besides the Stratford man. As de Vere had been dead for five years when the work was published, Oxfordians have to explain just who had possession of *all* of the 154 Sonnets, who brought about their publication and why, and why at that particular time, to say nothing of having to explain the meaning of the mysterious dedication, just who "Mr. W.H." might be, and many other questions about its mysteries. They haven't done this – and neither, it should be noted, has anyone who believed that their author was indeed William Shakespeare, despite several hundred years of trying.

4. *The Strachey Letter*. The assertion by Don Rubin that the references in *The Tempest* do not relate to Bermuda,

but to a neighborhood in London, and that the play was not based on the Strachey Letter of 1610, which described the shipwreck of 1609, strikes me as sheer nonsense. Line after line in the play was clearly drawn from the Strachey letter. The most accessible recent work to provide evidence for this is McCrea (2005, pp. 203–205). An older but lengthier article providing extensive evidence for this is Cawley (1926); see also Kuhl (1962), and Gayley's book (originally 1917, recently reprinted, but without a republication date, pp. 40–80.) The shipwreck of the *Sea Venture* occurred in 1609; the Strachey Letter, describing these events, was written in 1610, and the play was first performed on 1 November 1611. The causal connection between these events, which occurred at least five years after de Vere died, could not be clearer. One must again ask that if the play was written years earlier, where it was hiding all those long years, and why was it not performed in de Vere's lifetime? As well, it would certainly be a most remarkable coincidence that a play about a similar shipwreck had been written by de Vere years earlier but was first performed in 1611 and has been thought by every commentator since to be drawn in part from the Strachey Letter. It might also be noted that the Strachey Letter could only be read by directors of the London Virginia Company, who had to swear an oath not to reveal its contents to anyone else. Although Sir Henry Neville was certainly a director of the Company, William Shakespeare had no connection with it of any kind, and was obviously not one of its directors. Therefore, he could not have read the Strachey Letter.

5. *Early Authorship Doubts*. I have no quarrel with this, and pointed out that friends of Neville regarded him in his lifetime as an excellent literary writer. I have a copy of Wildenthal's book, but many of these doubts seem 'vague,' and no one was specifically named as the real author until Sir Francis Bacon, much later.

6. *Why Not Oxford?* This has been explained above and throughout my response. Given the glaring weaknesses in the case for de Vere as the real author, it seems very surprising that anyone could still maintain that he wrote the works attributed to 'William Shakespeare.' It also seems abundantly clear that the case for Sir Henry Neville as the real author is 20 times stronger than the case for de Vere, and I can only hope that anyone with an interest in this great mystery will examine the evidence with objectivity and with open eyes.

7. *The Handwriting at Audley End*. This is one of the strangest claims about the Authorship question of which I have ever heard. Professor Stritmatter is quoted as claiming that the handwriting of the marginalia in some of the books formerly held at Billingbear, Neville's country house in Berkshire, and, since the 19th century, at Audley End in Essex, were not written by Neville but by the 17th Earl of Oxford. In the book I co-authored with the late Dr. John Casson, we printed photographs of literally dozens of examples of the handwriting in Neville's books at Audley End, together with examples of his handwriting in letters and other documents that he wrote and show that they are identical and clearly written by the same man. For his claim to be even remotely accurate, Dr. Stritmatter would have to show the provenance of these books and how they came to be at Audley End. This would be rather difficult, as Neville and de Vere had no personal or intellectual connections or associations of any kind.

Indeed, it is very likely that they never spoke to each other, unless they discussed the weather for 30 seconds while waiting to enter Parliament. De Vere was the foreman of the jury that condemned the Earl of Essex to death following the 'Essex rebellion,' while Neville was sent to the Tower, barely escaping his own execution, for his role in that event. When Edward de Vere, the 17th Earl of Oxford, died in 1604, his books and other effects would have been inherited either by his widow, Elizabeth *née* Trentham, who died around 1612, or by his son and heir, Henry de Vere, eighteenth Earl of Oxford (1593–1625). Billingbear House, where Neville's books were held, was situated about six miles from Windsor in Berkshire, and about 35 miles from London. Edward de Vere had a house in London, as well as his family's ancestral home, Hedingham Castle, in Essex, on the other side of London, nearly 100 miles from Billingbear. Transporting them could only have been by some kind of primitive carriage, over unpaved roads, in English weather. If Neville wanted to buy books, he would have purchased them from booksellers in London, or, more likely, from booksellers in Oxford, where he was a graduate of Merton College, and a close friend of its head, Sir Henry Savile. Even if Neville purchased books from the heirs of de Vere after his death in 1604 – for which zero evidence exists – this proves nothing whatever about de Vere being the author of *Hamlet* or any other play by 'Shakespeare,' and is also irrelevant to the Authorship question after that date, when, as noted, 12 of the plays by 'Shakespeare' were certainly written.

Point 12 of my original article (Rubinstein, 2024), about Shakespeare visiting Sir Henry Savile in Oxford, is of the utmost importance – it has been described to me as "mind-blowing" and similar terms – and, to reiterate, I would very much like to hear from anyone in a position to add anything to the validity or otherwise of this claim. *Because* it is so implausible and has been asserted nowhere else besides in one edition of a book published in the 1890s, it deserves careful consideration. If, indeed, Shakespeare and Savile actually met, their purpose being

to discuss 'Shakespeare's' next play with Neville, its importance cannot be exaggerated.

## REFERENCES


Casson, J., & Rubinstein, W. D. (2016). *Sir Henry Neville was Shakespeare: The evidence*. Amberley Publishing.

Cawley, R. R. (1926). Shakespere's [sic] use of the voyagers in *The Tempest*. *Proceedings of the Modern Language Association*, *41*(3), 688–726. https://doi.org/10.2307/457623

Gayley, C. M. (1917). *Shakespeare and the founders of liberty in America*. Kessinger Publishing. (Original work published 1917).

Jowett, J. (Ed.). (2011). *Sir Thomas More*. The Arden Shakespeare, Series 3. Methuen Drama.

Kuhl, E. P. (1962). Shakespeare and the founders of America: *The Tempest*. *Philological Quarterly*, *41*(1), 123–146.

McCrae, S. (2005). *The case for Shakespeare: The end of the authorship question*. Praeger. https://doi.org/10.5040/9798400623653

Nelson, A. H. (2003). *Monstrous adversary: The life of Edward de Vere, 17th Earl of Oxford*. Liverpool University Press. https://doi.org/10.5949/UPO9781846313592

Nelson, A. H. (2004). Vere, Edward de, seventeenth Earl of Oxford (1550–1604), courtier and poet. In *Oxford dictionary of national biography* (online). https://doi.org/10.1093/ref:odnb/28208.

Rubin, D. (2024). JSE special issue commentary on Rubinstein (2024). Journal of Scientific Exploration, 38,3 549-551. https://doi.org/10.31275/20243529

Rubinstein, W. D. (2024). Why William Shakespeare did not write the works attributed to him, and why Sir Henry Neville did. *Journal of Scientific Exploration*, *38*(2), 258–272. https://doi.org/10.31275/20243243

## IN MEMORIAM: ROBERT (BOB) M. WOOD

James Houran

Many other people could have—or perhaps should have—prepared this remembrance, but I selfishly wanted to do it. You see, the late 1990s brought the opportunity for me as a young frontier scientist to research new aspects of the Roswell UFO incident of 1947. This led to a paper that later appeared in *JSE*. One of the firm but fair reviewers of that early work later opened a dialogue with me. We disagreed on some points related to this famous case, but he encouraged me nonetheless not to be afraid to engage with controversial topics or to face any aftermaths that might accompany it. This was my heartfelt introduction to Bob Wood, and little did I know about his strong presence and influence within the UFO community. Over time I came to fully understand the extent of his passion and the sincerity with which he held his beliefs. Likewise, Bob was widely known and respected even by his critics as a supreme gentleman. His son, Ryan S. Wood, informed the SSE that Bob, aged 96 years old, died on August 26, 2024, from cardiopulmonary arrest. Although I had not corresponded with Bob for 25 years or more, the news of his passing was still deeply felt.

Ryan graciously shared details about his life and career that were new and eye-opening to me. Born on April 4, 1928, in Ithica, New York, Bob's journey through life was characterized by a relentless pursuit of knowledge and a deep commitment to uncovering the truths that lie beyond our immediate understanding. His academic journey began with a B.S. in Aeronautical Engineering from the University of Colorado in 1949. He furthered his education by earning a Ph.D. in Physics from Cornell University in 1953. His early career saw him working for General Electric Aeronautics and Ordnance, followed by a two-year stint in the U.S. Army at Aberdeen Proving Ground. In 1956, he joined Douglas Aircraft, which later became McDonnell Douglas, and eventually Boeing, where he spent 43 years. During his tenure in the aerospace industry, Bob was involved in numerous ground-breaking projects. His work included the thermodynamics of missile cooling, managing independent research and development projects, antigravity research and investigations, designing radars to discriminate between Soviet ballistic missiles and their decoys, and contributing to the Space Station's development. He also played a pivotal role in promoting the Delta launch vehicle as NASA's workhorse for orbital payloads.

Bob's interest in UFOs began in the late 1960s when he led a proprietary project aimed at understanding how UFOs "worked." This *Boys in the Back Room* (BITBR) project employed the late Stanton Friedman and had funding that equated to $4.5 million in today's currency. Ultimately, this blossomed into a lifelong passion, and upon his retirement in 1993, he became extremely involved in the forensics of authenticating the "Majestic-12" UFO trove of documents. Moreover, Bob was a long-time Director of Research for the Mutual UFO Network (MUFON) and served as a physics consultant for the Aerial Phenomena Research Organization (APRO). He was also a counselor to the Society for Scientific Exploration and a member of the American Institute of Aeronautics and Astronautics since 1947. His scholarly contributions included authoring numerous articles on UFOs and the ground-breaking 1968 AIAA talk "Giant Discoveries of Future Science." He authored, edited, and contributed to several books—*Alien Viruses* (2013), *Selected by Extraterrestrials* (2015) by fellow Douglas Aircraft employee Bill Tompkins, and the *Encyclopedia of Flying Saucers* (2023) by Vernon Bowen.

Many leading ufologists disagree with the perspectives and conclusions that Bob advocated later in his life, and I also tend to sympathize with his critics. But his many personal and professional qualities should inspire future generations of researchers and enthusiasts—namely, curiosity, boldness, dedication, integrity, and passion. His relentless pursuit of knowledge and eagerness to debate provocative ideas were consonant with the founding goals of the SSE and an exemplar for the UFO community. Simply put, Bob Wood exemplified for me and many others the attitude of a true "maverick scientist." Rest in peace, our SSE comrade in arms.

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